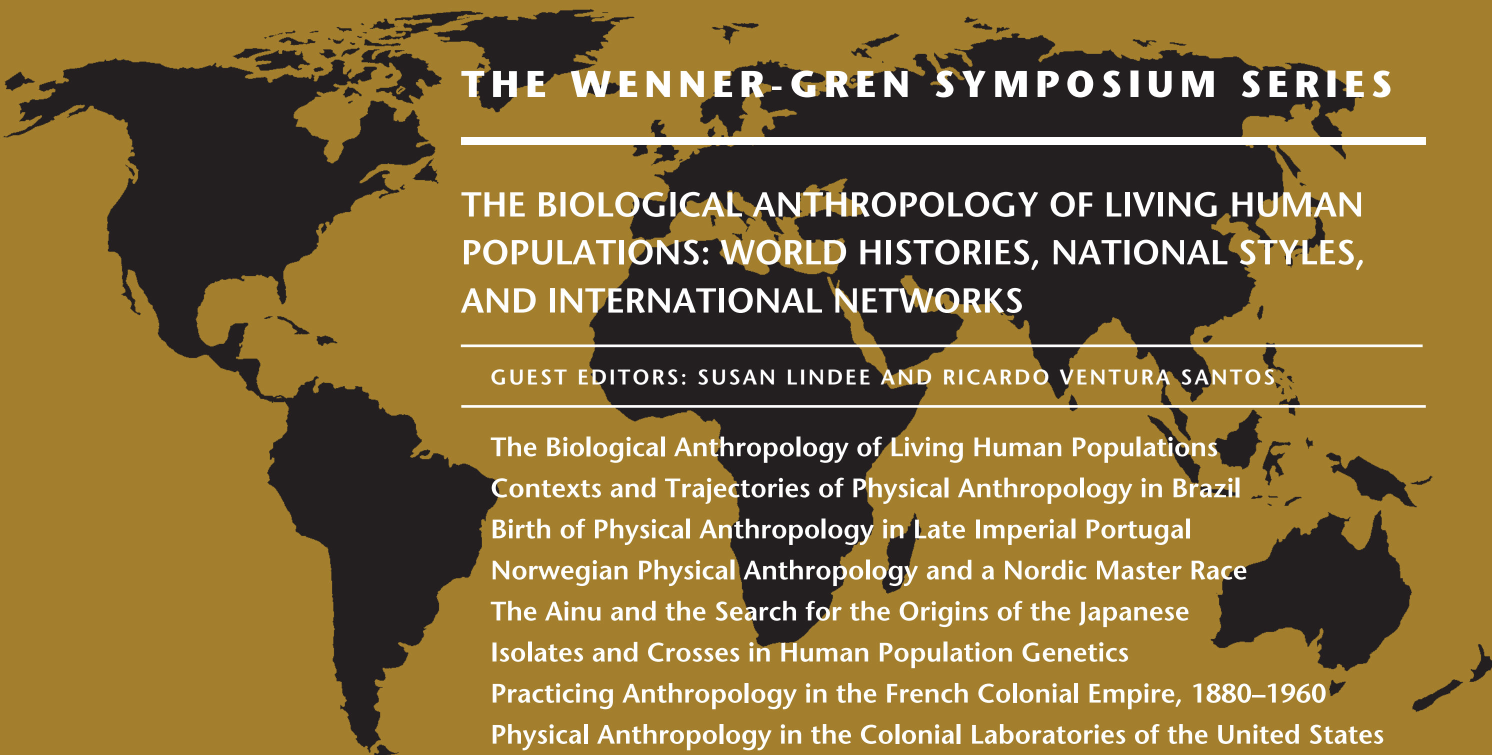
Current Anthropology

VOLUME 53 SUPPLEMENT 5 APRIL 2012



THE WENNER-GREN SYMPOSIUM SERIES

THE BIOLOGICAL ANTHROPOLOGY OF LIVING HUMAN POPULATIONS: WORLD HISTORIES, NATIONAL STYLES, AND INTERNATIONAL NETWORKS

GUEST EDITORS: SUSAN LINDEE AND RICARDO VENTURA SANTOS

Sponsored by the Wenner-Gren Foundation for Anthropological Research

THE UNIVERSITY OF CHICAGO PRESS

Please send all editorial correspondence to
Mark Aldenderfer
School of Social Sciences, Humanities, and Arts
University of California, Merced
5200 North Lake Road
Merced, CA 95343, U.S.A.
(fax: 209-228-4007; e-mail: maldenderfer@ucmerced.edu)

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 Current Anthropology (ISSN 0011-3204) is published bimonthly in February, April, June, August, October, and December by The University of Chicago Press, 1427 East 60th Street, Chicago, IL 60637-2954. Periodicals postage paid at Chicago, IL, and at additional mailing offices. **Postmaster:** Send address changes to *Current Anthropology*, P.O. Box 37005, Chicago, IL 60637.

Current Anthropology

Volume 53 Supplement 5 April 2012

The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks

http://www.journals.uchicago.edu/CA

Letter from the President of the Wenner-Gren Foundation

We apologize to Dr. Martha Macintyre (University of Melbourne) regarding a recent article in the Wenner-Gren Symposium Supplement 3, included with the April 2011 issue of *Current Anthropology*, that examined the role of anthropologists and other actors in the conflicts between indigenous Ipili and the Porgera Joint Venture gold mine in the highlands of Papua New Guinea (Coumans 2011). The article included errors about the work of Dr. Macintyre in relation to those conflicts.

It is appropriate to note that Dr. Macintyre has written extensively on questions of human rights in Papua New Guinea, is the past president of the Australian Anthropology Society, and is the current editor of its journal, *The Australian Journal of Anthropology*. It is also appropriate to note that Dr. Macintyre has informed us that she acted in full compliance with ethical and professional standards in all her work, including her work in relation to Porgera. Any suggestion to the contrary was not the view of *Current Anthropology*.

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Leslie C. Aiello
President, Wenner-Gren Foundation, and
Wenner-Gren Symposium Series Editor

DOI: 10.1086/663289

The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks

Wenner-Gren Symposium Supplement 5

by Leslie C. Aiello

The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks is the fifth Wenner-Gren Symposium to be published as an open-access supplement of *Current Anthropology*. The symposium was organized by M. Susan Lindee (University of Pennsylvania, U.S.A.) and Ricardo Ventura Santos (Museu Nacional & Fundação Oswaldo Cruz, Brazil) and was held March 5–12, 2010, at the Hotel Rosa dos Ventos, Teresópolis, Brazil (fig. 1).

There are interrelated and compelling reasons for Wenner-Gren interest in this symposium. As Lindee and Santos emphasize in their introduction (Lindee and Santos 2012), modern biological anthropologists, and particularly human biologists, are generally embarrassed by the history of their discipline and rarely have an interest in delving into it. Rather, most see themselves as the scientific wing of anthropology, with little to learn from the past or from research less than a decade or so old, leaving the history to historians of science. Accompanying this lack of interest is an apparent absence of appreciation for the rich diversity in international biological anthropology and the development of these varied orientations and approaches.

The Biological Anthropology of Living Human Populations is intended to provide an easily accessible resource to help remedy this situation. It builds on the prior success of *World Anthropologies: Disciplinary Transformations within Systems of Power* (Ribeiro and Escobar 2006), which grew out of a 2003 Wenner-Gren Symposium of the same name, looking at the interconnected global historical trajectories in aspects of social anthropology. Papers in the current supplementary issue are written by anthropologists, historians of science, and scholars of science studies and address the international development of the discipline as well as its contemporary condition and potential future development.

Papers included in this collection cover the development of the field in its "core" areas of France, Germany, and the United States, where it first appeared in the nineteenth century. There are also case studies in areas to which it subsequently spread: Brazil, Portugal, Norway, Japan, Iceland, and South Africa. Crosscutting topics include racism and the changing concept of race, the relationship between colonialism, imperialism, and physical anthropology, and the tension between biological and social adaptation as applied to humans. A major theme is the collection of human biological materials and the changing and evolving quandaries surrounding these collections and their repatriation over time, an issue that has been exacerbated by the "molecularization" of biological anthropology. Lindee and Santos (2012) describe biological anthropological collections as flash points for understanding the discipline, arguing that they play a pivotal role in the construction of modern ethnic, national, and global identities and at the same time are shaping what it means to be a biological anthropologist today.

Past Wenner-Gren symoposia have addressed human biology (e.g., Baker and Weiner 1966), and particularly the interrelationships between biological and cultural adaptation (e.g., Goodman and Leatherman 1998; Harrison and Boyce 1972; Swedlund and Armelagos 1990), and the rise of genetic approaches to the discipline (e.g., Goodman, Heath, and Lindee 2003; Spuhler 1967). However, the current collection is a unique initiative in addressing both the past and the present of international biological anthropology. The strong message emerging from these papers is that biological anthropology has been entwined with politics throughout its history but has evolved in profound ways over the past century and continues to do so. A clear knowledge of its varied international histories is essential to understanding the dilemmas confronting the modern field and its potential future trajectories.

The Wenner-Gren Foundation is always looking for innovative new directions for future Foundation-sponsored and -organized symposium meetings and eventual *CA* publication. We encourage anthropologists to contact us with their ideas for future meetings. Information about the Wenner-Gren Foundation and the Symposium program can be

Leslie C. Aiello is President of the Wenner-Gren Foundation for Anthropological Research (470 Park Avenue South, 8th Floor North, New York, New York 10016, U.S.A.).

 0011-3204/2012/53S5-0001$10.00. DOI: 10.1086/663328

Figure 1. Participants in the symposium "The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks." Seated, from left: Joanna Radin, Mike Little, Rachel Watkins, Ricardo Ventura Santos, Susan Lindee, Leslie Aiello, Laurie Obbink (Wenner-Gren staff). Standing, from left: Morris Low, Clark Larsen, Gonçalo Santos, Vassiliki Betty Smocovitis, Ann Kakaliouras, Warwick Anderson, Jenny Reardon, Gísli Pálsson, Jon Kyllingstad, Trudy Turner, Perrin Selcer, Jonathan Marks, Alan Morris, Jean-François Véran, Noel Cameron. Not pictured: Veronika Lipphardt. A color version of this photo appears in the online edition of *Current Anthropology*.

found on the Foundation's Web site (http://wennergren.org/programs/international-symposia).

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The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks

An Introduction to Supplement 5

by Susan Lindee and Ricardo Ventura Santos

We introduce a special issue of *Current Anthropology* developed from a Wenner-Gren symposium held in Teresópolis, Brazil, in 2010 that was about the past, present, and future of biological anthropology. Our goal was to understand from a comparative international perspective the contexts of genesis and development of physical/biological anthropology around the world. While biological anthropology today can encompass paleoanthropology, primatology, and skeletal biology, our symposium focused on the field's engagement with living human populations. Bringing together scholars in the history of science, science studies, and anthropology, the participants examined the discipline's past in different contexts but also reflected on its contemporary and future conditions. Our contributors explore national histories, collections, and scientific field practice with the goal of developing a broader understanding of the discipline's history. Our work tracks a global, uneven transition from a typological and essentialist physical anthropology, predominating until the first decades of the twentieth century, to a biological anthropology informed by postsynthesis evolutionism and the rise of molecular biology, a shift that was labeled "new physical anthropology." We place biological anthropology in a broad historical context and suggest how the histories we document can inform its future.

We open with a consideration of being both embarrassed and pregnant.[1] As any awkward speaker of a less-familiar language knows, some words that seem the same across languages are in fact amusingly (embarrassingly?) different. One example is the Spanish word *embarazada*, which means "pregnant." Wikipedia calls the word "a false friend for English-speaking students of Spanish who may attempt to say 'I'm embarrassed' by saying '*estoy embarazada*.'" We began to think about embarrassment—its ironies and its productivity—after the biological-anthropologist-turned-historian Michael A. Little, one of the key participants in our symposium, observed candidly during one session that when he first started teaching, "I never talked about the history of my field, because I was embarrassed about it." That history is a history, at least partly, of ideas about racial difference, which as his embarrassment suggested produced an emotional state that silenced or negated certain questions.[2]

Graduate students of social anthropology are generally expected to read the classics of anthropological thought—Tylor, Morgan, Malinowski, Durkheim, Boas, Rivers, and Radcliffe Brown—even if some of the ideas promoted by these thinkers have ceased to be seen as central to the field (Ingold 2002; Kuklick 2008; Stocking 1968). Social anthropologists are expected to know the history of their own discipline. But PhD students in biological anthropology today are unlikely to read

Susan Lindee is Professor in the Department of History and Sociology of Science at the University of Pennsylvania (Suite 303, Cohen Hall, 249 South 36th Street, Philadelphia, Pennsylvania 19104, U.S.A. [mlindee@sas.upenn.edu]). **Ricardo Ventura Santos** is Associate Professor in the Department of Anthropology at the National Museum, Federal University of Rio de Janeiro, and Senior Researcher at the National School of Public Health, Oswaldo Cruz Foundation (Escola Nacional de Saúde Pública/FIOCRUZ, Rua Leopoldo Bulhões 1480, Rio de Janeiro, RJ 21041-210, Brazil). This paper was submitted 27 X 10, accepted 22 IX 11, and electronically published 28 II 12.

1. *Embaras* also has an evocative archaic meaning relating to a blocked river, a passage prevented by debris, or a point at which one is forced to slow down to navigate the water. It was an American term for places where the navigation of rivers is rendered difficult by the accumulation of driftwood. Like the nineteenth-century blocked river, the *embaras* that barred the passage of navigation of rivers, embarrassment is perhaps a point at which one is forced to slow down, navigate, and think carefully about how to move forward.

2. Perhaps embarrassment is a common experience in anthropology: Clyde Kluckhohn confessed to a "feeling of embarrassment" when he read the field notes of his student David Schneider, who was working on the islands of Yap in the 1940s, because they were so personal and confessional (and because Schneider was a remarkably reflexive fieldworker; see Bashkow 1991).

 0011-3204/2012/53S5-0002$10.00. DOI: 10.1086/663335

the works of nineteenth-century leaders in the field—such as Samuel George Morton, Paul Broca, Geoffrey Saint-Hilaire, or Rudolph Virchow—or even to read twentieth-century physical anthropologists who were influential—such as Aleš Hrdlička, E. A. Hooton, Eugen Fischer, Arthur Keith, Leonce Manouvrier, or Rudolf Martin.[3] Indeed, a new graduate student today in biological anthropology is more likely to start with technical training in skeletal biology, molecular genetics, or forensic science—the laboratory specialties grounded in experimental technique that have become so central.

In the United States in recent years, several departments of biological anthropology (including Harvard's) have been reconstructed as freestanding departments of human evolutionary biology not tied to social anthropology, linguistics, or archaeology—that is, to any forms of humanistic analysis (although many others continue to maintain the four-field approach with varying levels of success and with mixed consequences for hiring and training; Borofsky 2005; Calcagno 2003; Segal and Yanagisako 2005). The training of biological anthropologists seems to often involve historical forgetting and little contact with past ideas of the discipline they are entering. It has been a discipline with a history that is often purposively disappeared, forgotten for a reason. As one of us, Ricardo Ventura Santos, has recalled, at some point seeing a photo of himself taking head measurements in the early 1990s with the technologies so long associated with racial narratives of difference and pathology became for him, again, "embarrassing."[4] Even in the arc of his own career, that of a biological anthropologist who went to work in a natural history museum centrally concerned with history and who has become deeply interested in the history of the field over the past decades, these simple technologies of human measurement came to carry a conflicted and charged meaning.

Of course, just as the same bones and bloods can move through different contexts, their meaning varying, their power changing, so too the same actions can mean different things: Noel Cameron's uses of human measurement in a birth cohort study in postapartheid South Africa (explored in the oral history that closes this volume) demonstrate the point. Sequencers and calipers coexist as tools of the discipline today, and even questions about group differences work differently in an age of Internal Review Boards (IRBs), the recalibration of scientific race, repatriation rights, and massive global biobanking systems.

Thinking about disciplinary embarrassment, we propose here, can lead to a productive awareness of complexity, timescales, and the legacies of social and political order: Little, once embarrassed by the history of his field, is now a skilled historian of biological anthropology (Little and Kennedy 2010). And the mistranslation at the English-Spanish intersection, of embarrassment in one language and pregnancy in the other, calls to mind a state of both confusion and incipient birth. We suggest here that the seed of something new is growing, in this case new ways of seeing a history that has vexed both historians and practitioners. We hope in this volume to begin to reconfigure the history of biological anthropology as a resource for moving the field forward.

The papers collected in this special issue of *Current Anthropology* were developed for a Wenner-Gren symposium that was about the past, present, and future of biological anthropology—"The Biological Anthropology of Living Human Populations: World Histories, National Styles, and International Networks"—held in Teresópolis, Brazil, in March 2010. Our goal was to understand from a comparative international perspective the contexts of genesis and development of physical/biological anthropology around the world. While biological anthropology today can encompass paleoanthropology, primatology, and skeletal biology, our symposium focused on the field's engagement with living human populations.

Bringing together scholars in history of science, science studies, and anthropology, we structured our discussions not only to examine the discipline's past in different contexts but also to reflect on its contemporary and future conditions. Our contributors have been guided throughout by a nexus of key questions about national histories, collections, and scientific field practice. Particularly relevant to us was the development of a broader understanding of the discipline's global, uneven transition from a typological and essentialist physical anthropology, predominating until the first decades of the twentieth century, to a biological anthropology informed by postsynthesis evolutionism and the rise of molecular biology, a shift that was labeled "new physical anthropology" in a famous 1951 manifesto by Sherwood Washburn (Washburn 1951). Washburn proposed that physical anthropology could now link the evolutionary synthesis to comparative functional anatomy. He presented the changes as revolutionary, a break with an unfortunate past tainted by typological racism. Physical anthropology, he said, had to become evolutionary, and adaptation, selection, and population biology should become its central problematic (Haraway 1989).

If this transition to a new physical anthropology has been relatively well described in the cases of North America and of certain European contexts, the same could not be said for other regions of the world. In some countries, such as the United States, this "new physical anthropology" continued to be practiced in anthropology departments, while in other

3. This conclusion is based on a somewhat informal survey of English-language graduate syllabi in physical/biological anthropology posted on the Web since about 2000. One thing is clear: what counts as physical anthropology varies a good deal, with some programs built entirely around archaeology, others focused on forensic training, and many on human evolution. It is not unusual for George Stocking or Stephen Jay Gould to be included as assigned reading in graduate training, but reading the primary sources in their original form, with the exception of Charles Darwin, is less common.

4. The measurements were part of a restudy of the Xavante Indians from Central Brazil (see Coimbra et al. 2002). The investigation attempted to collect some of the same bioanthropological variables collected by James Neel and Francisco Salzano in 1962 in the same population (Neel et al. 1964), aiming at studying long-term changes in human biology and health.

countries, such as Brazil, it moved into biology departments (and genetics departments in particular) and in some cases into museums. In natural history museums, the transitions to the new physical anthropology were generally slower and more incomplete, with typological perspectives on human biological variability persisting far longer (Maio and Santos 1996, 2010).

Many scholars have pointed out in passing that physical anthropology has taken varying forms in different national contexts. Our suggestion in this volume is that a deeper understanding of the development of physical/biological anthropology across a broader range of national contexts can be both instructive and productive. We are not merely suggesting that the stories of Brazil, Norway, or Japan need to be "added" to the stories of Germany, France, and the United States; we are proposing that the entire enterprise looks different when the picture broadens. Recent discussions within anthropological circles in the United States, in which the fragmentation of anthropology is seen as a major problem, often fail to consider that in other countries with distinct anthropological traditions, biological anthropology has been practiced for many decades in isolation from other areas of anthropology.[5]

There are also some suggestive consistencies in the historical trajectory of the field around the world. The strong parallels in the development of physical anthropology in geographically and politically diverse contexts seem to provide evidence of a shared internationalistic agenda. The fact that key ethical issues have overlapped seems to point to some consistent ways in which objects and biological materials (things) configure relationships. Anthropologists as the overseers of identity (biological, national, ethnic, racial) appeared in our papers again and again in accounts of politics, professionalization, and biological theory. The movement of globalized knowledge (which changes as it moves) was inflected in explorations of journals, international agreements about race, borrowed methods, and ideas that reflected national relationships. The special status of the human animal was everywhere relevant. We found both tremendous heterogeneity and intriguing convergences in places as diverse as Portugal, Japan, Brazil, France, Iceland, and South Africa.

In our discussions, Gísli Pálsson proposed that anthropology as currently organized around two radically separated domains (biological and social) that are often in tension borders on being out-of-date and ethnocentric in its assumptions—humans are neither social nor biological, he suggests, but always both, and a discipline proposing to study human beings should be both as well. This remains a compelling argument in many departments of anthropology in the United States. From this perspective, biological anthropology, with its emphasis on understanding human biology in social terms, seems to occupy the privileged epistemic position in relation to social anthropology: all animals are biological, and there is no animal in which biology does not matter. Pálsson, himself a social anthropologist, calls into question the radical rejection of biology that is common in the anthropological narratives that explain human life and society. It is a radical rejection, of course, that mirrors a historical problem—roughly, the problem of race.

The word "race" is highly charged in ways that make it difficult to use without sounding as though one is engaged in an accusation, and much of the historical literature does sound a bit like exorcism. As one of our participants (Jean-François Véran) put it during discussions, in some circles today the word "race" can only be used to condemn racism. Yet the term also refers to something readily understood in many social settings all over the world. Race is not arcane or technically sophisticated, and it is not obsolete in the sense that race continues to play roles in real estate, education, political rights, criminal prosecution, the courts, the census, and the data collection of the World Health Organization. The vernacular wordplay "driving while black" used in the United States captures the social immediacy of race on any highway in the United States. The phrase mimics "driving while intoxicated" and is used as shorthand for a form of racial profiling: that is, the tendency for police in the United States to pull over and question drivers interpreted as African American (darker skinned) at a disproportionate rate.[6]

If the use of the word "race" is scientifically suspect (now replaced by ethnicity or population), it is nonetheless an instantly legible human category whether biologists talk about it or not. It was never simply an idea dreamed up by physical anthropology. Indeed, in medical terms, race is as relevant to health risks in societies around the world as sex or age.[7] Denying that it has any transcendent biological genetic justification that can be linked to a hierarchy of values is appropriate, but such denials cannot be expected to eliminate the force and power of the idea in the everyday world. In this larger, wide-angle frame, biological anthropology is part of a legitimate and even pressing concern with the biological correlates of human social difference.

As our discussions developed, two key themes emerged. First, collecting materials and bringing them into relationship to each other in settings often distant from the point of collection has been and remains central to biological anthropology. Second, biological anthropology has played a pivotal

5. For example, Segal and Yanagisako (2005) do not draw on the practices and institutional structure of anthropology in other cultural contexts, in which the four-field model is not dominant, as a way of understanding and undermining its dominance in the United States (which is their goal).

6. The ACLU account is at http://www.aclu.org/racial-justice/driving-while-black-racial-profiling-our-nations-highways (accessed October 12, 2010).

7. World Health Organization data are organized in these terms. See http://www.who.int/topics/womens_health/en/ (accessed October 12, 2010) and data on various ethnic groups, e.g., Hispanics at http://www.paho.org/English/DD/PIN/ePersp001_news02.htm (accessed October 12, 2010).

role in national identities, and national identities are still shaping what it means to be a biological anthropologist today. We want to turn now to these two themes and the questions they raise.

One of the results of the great voyages of discovery of the fifteenth and sixteenth centuries was a diffusion of materials and ideas across the globe. Naturalists from Europe sent back plants, animals, and artifacts that provided testimony to the unseen distant worlds they visited. Visitors who came to Europe from these distant places sent back their own stories of European capitals, kings, and technologies, and in some cases they rapidly and eagerly adopted these technologies, such as guns, as efficiently as they could. The surging movement of goods, people, and ideas across the globe, spanning the five centuries from about 1500 to 2000, is one of the great events in human history. Physical anthropology participated in this process, partly by bringing back to scientific centers materials and bodily objects, collections of bones, bloods, remains, and measurements that are truly impressive for their size, scope, and broad utility.

Physical anthropologists and the geneticists who increasingly worked with them in the field after World War II collected things that were saturated with meaning and overloaded with emotion and desire, things that spoke of death, relationships, power, and immortality. In papers here by Ann M. Kakaliouras, Trudy R. Turner, Jenny Reardon and Kim TallBear, and Pálsson, the quandaries of collection are illuminated across time and place. These objects did not have a consistent or stable purpose, but all parties coming in contact with them tended to view them as meaningful and powerful. This may be why we find ourselves interrogating such objects anew in the twenty-first century. It is genuinely unclear what to make of them or where they belong.

Beginning in the 1970s, many anthropological collections became the focus of repatriation debates with complex protocols and training regimens. Biological anthropologists now often become specialists in returning collected materials to populations from which they were originally drawn and in assessing which materials should be sent to which groups (Beisaw 2010; Fforde, Hubert, and Turnbull 2002; Rose, Green, Gree 1996).[8] The old expertise—how to collect in the field—is now supplemented with new expertise—how to redistribute those same things to a field that is no longer the same. Repatriation has become a sort of "subfield of a subfield" of biological anthropology (of bioarchaeology), and the collections themselves have opened new problems as they require new kinds of professional identity.

Today, biocuration teams are called in to make decisions and redistribute goods. Repatriation ceremonies are performed before laboratory freezers, ancient remains are claimed by more than one constituency, and scientific researchers cannot assume that their interests will always come first. In addition, collections have a visibility they never had before as standards for IRB approval and consenting make the process both more open and more visible to those from whom materials are taken (Turner 2005). The molecularization of biology has also affected what anthropologists collect in the field. While DNA studies have not replaced studies of entire human beings, the analysis of DNA does play a growing role in biological anthropology.[9] Compared with collecting whole blood, collecting DNA is in some ways "easier," requiring only a cheek swab, and in some ways harder because it now occurs in an ethical climate that requires complex decisions about future use and storage. Biological anthropologists trained to use reflectometers (which measure reflected light from a surface), spirometers (which measure expired air as a test of lung function), and thermometers may now find themselves analyzing things that require new kinds of technological translation and that they can no longer experience directly through vision or hearing.

The locations in which such materials are housed have also changed. Physical/biological anthropologists contributed to natural history museums for most of the history of the field, but to a large extent their collections today go to molecular laboratories at universities and other institutions. Perhaps most telling, such collections today have a commercial value (sometimes significant) that was less important in earlier periods.[10] The management of collected materials, as Turner suggests here, may be the next great challenge for biological anthropology. The history of physical/biological anthropology can thus be seen as both a history of collecting and a history of redistributing. Collections are a flash point for understanding the discipline. These shifting contexts also resituate the role of biological anthropology in the construction of modern national and global identities, our second broad theme. A very complex picture is emerging in situations where the national interests that originally stimulated the work led to the oppression of some groups, and the same collections have now become resources to validate the rights of those who were originally disenfranchised. As Morris Low notes here, the Ainu people of the northern island of Hokkaido in Japan have recently (2008) been declared an indigenous people of Japan (although not *the* indigenous people). This classification overturned a century of research that was intended to show

8. Many fascinating details relating to the repatriation process are posted at http://www.nps.gov/nagpra/ (accessed October 12, 2010). See also the various databases listing and characterizing materials held in museums at the NAGPRA site.

9. These topics are drawn from a summary of Categories of Papers from the Human Biology Association Meetings (abstracts published in the *American Journal of Human Biology*) and the Society for the Study of Human Biology (abstracts published in the *Annals of Human Biology*) 2008 (*American Journal of Human Biology* 20[2]:213–241) and from a plenary session on evolutionary endocrinology with papers on early pregnancy (Pearl Lecture), placental hormones, inflammation, cortisol, ovarian function, emotion regulation, and several primate studies. The list was compiled for us by Michael A. Little.

10. For a helpful if incomplete overview of the biobanking system, see https://brd.nci.nih.gov/BRN/brnHome.seam (accessed October 12, 2010).

that they were, biologically, not a part of the Japanese lineage at all. But the research intended to exclude them ended up validating their status: physical anthropologists seeking to demonstrate the inferiority of the Ainu collected the materials and records that later established the Ainu's geographical and political claims. Similarly, in Norway, as Jon Røyne Kyllingstad's study suggests, the Sami were construed as "latecomers" with no particular claims on their territories. But skulls excavated by the Sami teacher and activist Isak Saba became crucial to scientific arguments in the 1930s that suggested that the Sami had an ancient history in northern Scandinavia (and therefore had ancestral rights), and these skulls were later used in public debates as evidence for the indigenousness of the Sami. The Havasupai—a group in Arizona who recently succeeded in a legal effort to reclaim their own DNA—explored in Reardon and TallBear's paper on the new "civilizing mission" of genomic collection, are only the most recent iteration of this traffic between science, entangled objects, land rights, and the construction of history where the living and the dead are both implicated, even conflated, in bones, frozen blood, and hair (Thomas 1991).

Whatever else it might be, physical/biological anthropology is also a part of the history of colonialism. It is one of a cluster of technical disciplines focused on studying places and people considered exotic that came into being in tandem with the engagement of European experts in an extra-European world of extreme natural and social diversity. Indigenous and native groups marked as living in a different "time" and lacking history (Wolf 1982)—and primate bodies that functioned as time machines in their own ways—became resources for a highly technical science of measurement, comparison, blood groups, and theoretical analysis after 1800. Like so many other scientific disciplines, physical anthropology developed networks of intellectual exchange that crossed national boundaries and ideas and that also reflected sometimes narrow nationalist sympathies and concerns.

At the same time, and less transparently, the enterprise began to engage with what might be called indigenous or subject intellectuals, people who were both studied by physical/biological anthropology—as persons who fit racial categories of one kind or another—and who participated in the enterprise of constructing these racial categories—sometimes as subject scientists, sometimes as scholars at the core, and sometimes as scholars at the periphery who had to deal with European- and U.S.-derived theories about "admixed" countries (where they lived and worked; Bastos 2007; Stepan 1991). Veronika Lipphardt's paper here looks at prominent Jewish scientists in Germany such as Felix Bernstein and Arthur Weinberg who played active roles in scientific fields that construed Jews as pathological. In the process, these scientists struggled to make their peace with both their colleagues and their roles in a conflictual scientific field. In a related way, as Warwick Anderson suggests here, the Maori biological anthropologist Te Rangi Hiroa, who also used the name Peter Henry Buck, operated with a dual identity, as a scientist and a subject. Hiroa worked on Polynesian groups from about 1879 to 1954 and embraced an identity as racially mixed while actively studying Maori subjects and celebrating his ethnicity as a part of his methodology by turning "mongrel" into his own honorific (Allen 1994; see also Anderson 2005). Similarly, Caroline Bond Day, whose anthropological work is explored in Rachel J. Watkins's paper, understood herself as a mulatto, something she publicly explored in her fictional work and published essays, and as a scientist she worked on the impact of race-crossing—or, as her adviser Earnest Hooton termed it in his notes on her thesis, "miscegenation,[11] a term that may sound benign in some contexts but in the United States evokes laws that criminalized marriages between European Americans and African Americans.

These stories ask us to notice who participates in making categories and collecting material and to notice their stakes in the enterprise. The polygenist and monogenist debates, exploring the question of the origins of different races, engaged elite naturalists and physicians who were coming to terms with the colonial encounter between people who were self-consciously modern and involved with science and "progress" and people who were not (yet). But the stories of evolution and racial hierarchy also engaged and interested those whose bodily traits placed them inside the scientific narratives, those people marked by biology who were, as our conference monitor and doctoral student in the history of science Joanna Radin has put it, biological enough to become the focus of comparative scientific interest.[12] The frequently uninterrogated notion that some people are "more" biological, whatever that might mean, has played a role in scientific constructions of race, gender, class, and ethnicity for at least two centuries.

In 1972, Jack Kelso complained that physical anthropologists were not reaping the boon of the postwar funding explosion in the United States because they looked too much like biologists to the social scientists and too much like social scientists to the biologists (Kelso 1972). Echoing his concerns, a 2003 special issue of *American Anthropologist* featured the painful reconstruction by biological anthropologists of their relationship to other (social) anthropologists at meetings of the American Anthropological Association, where many reported feeling "abandoned" by the broader professional group and shut out from the pages of traditional anthropology journals (Calcagno 2003). Some of the essays in this 2003 issue explored the missed communication between social and biological anthropology. Defending the study of human biol-

11. See the biographical sketch in her archival collections at Harvard at http://oasis.lib.harvard.edu/oasis/deliver/p̃ea00032 (accessed October 12, 2010; see also Alexander 1993; Williamson 1980).

12. Radin is completing a doctoral dissertation at the University of Pennsylvania in the Department of the History and Sociology of Science, working with Susan Lindee as her adviser. Her dissertation has a working title of "Life on Ice: Frozen Blood, Human History, and Biodiversity in a Genomic Age, 1950–2010." See also her interview with Jonathan Friedlaender (Friedlaender and Radin 2009).

ogy—apparently convinced that such defense was necessary—James M. Calcagno said "name one species today for which the biology of that species is considered unimportant." But of course the issue is not that human biology is unimportant. Rather, it is that thinking about human biology in the ways that biological anthropologists have historically taken as central, in terms of variation and difference, calls up histories that remain unresolved. Are these histories that remain embarrassing?

Like the planners of the "World Anthropologies" Wenner-Gren conference in 2003, who did not seek to construct an abstract model of what world anthropologies should be, we have not sought to construct an abstract model of what biological anthropology can and should be (Ribeiro and Escobar 2006). They found that although social anthropologists around the globe have clearly shared some theoretical and methodological concerns, they have also varied dramatically in their assumptions, methods, and field practices. These differences reflect specific local and national contexts in which they originated as well as particular links to the international networks in which they participated and their experiences in their field sites. The World Anthropologies group wanted to suggest the opportunities opened by pluralized power, which they identified as the central emerging global force of the last half century. Our own explorations illuminate the global historical path of physical/biological anthropology and suggest new opportunities that this history makes possible. Our contributors document how biological knowledge about human populations has been taken up in legislative spheres, used in political claims about rights, and "repurposed" in indigenous networks. They suggest that "center" and "periphery" are fluid categories that can be deployed as experts of different kinds negotiate authority. Our work in this volume looks at race as both a problematic and an opportunity. The biological details can document and challenge injustice rather than legitimate it, and the studied subjects of the past can be allies, experts, and scientists who have a profound stake in the technical knowledge that draws on their histories.

The Origins of Physical/Biological Anthropology

The subject of human biological variation and difference has attracted keen observers for as far back as our historical records take us. The late Frank Spencer's remarkable 1997 encyclopedia of physical anthropology includes references to the ideas of Anaximander, Albertus Magnus, Tocqueville, and even Mark Twain as well as entries on many prominent figures in the history of biology since 1700, from Linnaeus to Robert Chambers to Ernst Mayr, many of whom would not have called themselves physical anthropologists (Spencer 1997). Relevant ancestral fields of science could be construed to include anatomy (a key domain around the world in terms of training), medicine, all forms of natural history and field collecting, and all kinds of racially oriented sciences. Biological anthropology addresses questions with a long, complex lineage.[13]

The disciplinary configuration that looks roughly like modern physical/biological anthropology, however, has its origins in the nineteenth century in the United States and Europe, especially Germany and France. Most practitioners were trained in medicine and were concerned in one way or another with human variation, anatomy, difference, racial classification, and evolution. Training and research programs began to thrive in these centers and then spread out, first to other European nations—to Italy, Scandinavian countries, Czech and Slovak regions of Eastern Europe, Poland, Belgium, the Netherlands, Great Britain—and then beyond to Israel, Latin America (Mexico, Brazil, Argentina, Peru), Russia, South Africa, Japan, China, Australia, and New Zealand. The nascent field seemed to find receptive audiences everywhere in the nineteenth century, often mediated by someone trained in Europe or the United States—someone who brought the methods and language and theories and training standards of physical anthropology to a new setting and applied it there, sometimes in only a short visit of a few weeks. Physical anthropology was easy to plant and easy to grow, which raises intriguing questions about what the consistent cross-cultural appeal might have been. Studies of the history of physical/biological anthropology have been dominated by practitioner-scientist historians, some very skilled, some with a limited understanding of what would count as a historical argument. One of the more interesting resources we found was the International Association of Human Biologists Publications on Histories of Physical Anthropology, a series of papers written by anthropologists about their own national experiences and covering the development of physical anthropology in many nations (see Roberts 1997).[14] These are all relatively short and often autobiographical, but together they reveal some trends and they are rich testimonials for any historian interested in understanding the global development of physical anthropology. Practitioners continue to play a key role in assessing and making sense of the history of the field, but they are now joined by a growing cadre of scholars trained in the history

13. Some of the entries in Spencer's two-volume encyclopedia constitute very helpful starting points for any historian interested in the field. The treatment of interdisciplinary field research in several entries provides a critical guide to the key field projects in the twentieth century; entries on particular national contexts (including Finland, Cuba, New Zealand, and many others) give a quick guide to institutions and leading scientists around the world; the discussion of paleoanthropology attends to theories and collections and includes many helpful references. Like all encyclopedias, the work is long on facts and short on analysis, but it is unquestionably a crucial resource (Spencer 1997).

14. A partial list of the occasional papers on the history of physical anthropology in different countries written by members of the International Association of Human Biologists, including Great Britain, the Netherlands, Belgium, Brazil, Italy, Czechoslovakia, Germany, Italy, Poland, and former Soviet Union, may be found at http://www.worldcat.org/identities/lccn-n89-222553 (accessed September 15, 2011).

of science, technology, or medicine or in political or cultural history who have begun to recognize the centrality of physical/biological anthropology to a range of traditional historical concerns with global knowledge systems, the management of the modern state, and colonialism and its legacies. Social anthropologists have often written about biological anthropology as well.

Historian of anthropology George Stocking played a key role in calling attention to the history of physical anthropology and bringing professional historians into the discussion of its past (Stocking 1988). Beginning with his 1968 study of race, culture, and evolution, Stocking has explored the history of biological and cultural anthropology with an emphasis on the ways that anthropologists themselves have responded to decolonization (Stocking 1968, 1991). In the mid-1970s, as the internal professional crisis over the roles of anthropology in colonization waned, "the assumption that anthropology was linked to Western colonialism became as much a commonplace of disciplinary discourse as the ignoring (or compartmentalizing) of that relationship had once been" (Stocking 1991:4). Historians such as Stocking and historically informed social anthropologists as well as historians began to critically interrogate the questions raised by this assumption and began to place the practice of physical/biological anthropology within national and imperial histories (see, e.g., Anderson 2005 on Australia; Blanckaert 1989 and Dias 1991 on France; Bronfman 2004 on Cuba; Cunha 2002 on Brazil; Dikötter 1997 on China and Japan; Hirsch 2005 on the Soviet Union; Philip 2004 on Southern Indian; Pordgorny 1999 on Argentina; Stepan 1991 on Latin America).

Biological anthropologists with historical expertise, such as Michael Little and Kenneth Kennedy (2010) and Jonathan Marks (1995), have tracked the theoretical and intellectual history of their field—the ideas and theories that made sense of human biology across time and place. Little's paper at our conference sketched out a definition of the field listing paleoanthropology, skeletal biology, primatology, molecular anthropology, population genetics, and human population biology as all a part of biological anthropology. Biological anthropology, he said, was "the study of human evolution and human variation," proposing that the historical threads leading to contemporary biological anthropology included Ancel Keys's wartime starvation studies, the Human Fatigue laboratories, studies of populations in extreme environments, and studies of the stresses experienced in global conflict when the human body is placed under difficult circumstances. The biology of human tolerance became a practical and politically relevant technical problem for physical anthropology, which moved beyond the focus on populations or race. Marks explores the persistence of ideas about race and human diversity through a century of mixed professional and institutional approaches and practices in which either blood or bodies could take precedence depending on the professional or political stakes in play.

Our papers here join an increasingly sophisticated literature on the history of physical/biological anthropology. They are inflected by our interdisciplinary discussions and our collective commitment to the development of a more profound understanding of how past concerns continue to matter to the field. We recognize the high human stakes in both historical and contemporary claims about the sciences of human biology. Those stakes animate our interest and guide our approaches and questions.

Physical Anthropology and National Identity

We start with four case studies of the development and role of physical/biological anthropology in Brazil, Portugal, Norway, and Japan, each of which demonstrates how the scientific practices of the field intersected with issues of national identity and colonial power. Building nations and empires was one of the things that biological facts about bodily difference seemed to be able to do, and in many different places. Human biology was a way of thinking about the nation and the state.

In Brazil, the nationalist agenda may also have reflected concerns about European power. In his paper, R. V. Santos reassesses the influence of Franz Boas on physical anthropology in Brazil, suggesting that the antideterministic postures of prominent Brazilian physical anthropologists such as Edgard Roquette-Pinto drew less on Boas and more on the Brazilian experience of nation building at the end of the imperial period and during the rise of the republic. At the National Museum in Rio de Janeiro, one of the most influential anthropological research centers from 1870 to 1930, anthropologists distanced themselves from theories that disqualified the mestizos and placed non-Europeans lower on the hierarchy of human races. Seen as "guardian angels" of the Brazilian people, the physical anthropologists working in this museum participated in forging national identity, and in the rejection of determinism they rejected what was a part of the nationalist ideal. As R. V. Santos proposes, Roquette-Pinto interpreted the problems of Brazilian populations as social, political, and medical rather than racial, and by the time he visited Boas in New York in 1926, he had already rejected racial types as explanations for national status. R. V. Santos's case study helps us understand how specific contexts intersect with dominant ideas and how nationalism can shape scientific conclusions about populations and racial hierarchies.

Gonçalo Santos explores the development of physical anthropology in Portugal in the mid- to late nineteenth century. Portuguese anthropologists were particularly concerned with the question of the "antiquity of man in Portugal." Some focused on the past (including the prehistoric period), others focused on aspects of the present deemed to be archaic survivals, but both groups were interested in the study of the origins and specificities of the Portuguese as a "race" and a "nation." The country had vast overseas possessions and imperial ambitions, both of which shaped the development of physical anthropology. Many observers have commented on the role of anthropology in the colonial enterprise, but G.

Santos emphasizes the ways that Portugal's own internal sense of political fragility shaped anthropological thinking. Shrinking imperial returns combined with continuing domestic underdevelopment and growing economic dependence on other European powers (especially Britain) to produce a general fear that Portugal could be annexed by Spain. By seeking to demonstrate the "antiquity" and the "unity" of the Portuguese as a superior European "race" and "nation," early anthropologists were not just seeking to define themselves in opposition to colonial subalterns, G. Santos proposes; they were also seeking to strengthen their country's claims to sovereignty while reaffirming its position within European hegemony.

A resonant case study is Kyllingstad's consideration of Norwegian physical anthropology, where the Nordic race concept was abandoned in the 1930s despite pressure and criticism from German physical anthropologists. Physical anthropology in Norway developed with close ties to physical anthropology in Germany, but as Norwegian scientists from the late 1920s began to question concepts of racialized hierarchies, Norway began to "lag behind" in racial consciousness, according to the criticism of a Norwegian Nazi anthropologist. Kyllingstad suggests that the relatively small community of physical anthropologists in Norway (three individuals) was responding to a specifically Norwegian scholarly community engaged with archaeological, linguistic, and historical study of national prehistory and history. This community developed scholarly ideas about humankind, culture, and nationhood that were rather different from the *völkisch* ideas that gained increasing support among German academics in the same period. Racial explanations for national development existed among Norwegian scholars, but archaeologists, philologists, and historians were far more influenced by a cultural evolutionary notion of nationhood in which the nation was seen as a product of the cultural adaptation to a certain environment. This perspective was based implicitly or explicitly on a basic principle of the psychic unity of humankind that became part of Norwegian national identity.

National identity played a key role in the search by physical anthropologists in Japan for the origins of the Japanese, as Low suggests in his paper. He considers how Japanese physical anthropologists studied and assessed the Ainu people of Hokkaido and shows that they often had an explicit nationalist agenda over the last century. For Japanese physical anthropology, the Ainu complicated the ongoing debate about the origins of the Japanese, which has drawn on human DNA, rice genetics, and historical linguistics to suggest that immigrants (from somewhere else) were responsible for the transformation of Japan from a hunter-gatherer to an agricultural society starting in roughly 300 BCE. Physical/biological anthropology was far more advanced in Japan than social anthropology, Low suggests, and the historical narratives he reconstructs matter for the Ainu today. Stories that attend to questions of power and national identity, Low proposes, "bring home to us the importance for anthropologists to use research practices based on prior consultation, cooperation and collaboration with local communities."[15] His case study provides critical perspective on a group that is, like so many others, both "assimilated" and discriminated against and understood to be both modern and ancient simultaneously.

Shifting Cores: Germany, France, and the United States

With the next six papers, we turn to a consideration of the development of physical anthropology in three widely recognized "centers"—Germany, France, and by the mid-twentieth century, the United States. By 1982, G. Ainsworth Harrison could claim that "North America occupies a central position" in physical anthropology and that the population studies then underway by James V. Neel constituted the "main development" in physical anthropology at the time (Harrison 1982). Certainly Neel's interdisciplinary field programs in South and Central America were important for biological anthropology, forging links between geneticists and anthropologists and promoting the use of new laboratory technologies in studies of living human populations. But Neel's approaches reflected theories that had their origins in research programs supported by both German and French anthropologists, and the intellectual traffic between these "centers" was significant.

Lipphardt's exploration of German physical anthropology compares the scientific work of German race hygiene theorists such as Eugen Fischer and Fritz Lenz with the work of the postwar American geneticist L. C. Dunn. She suggests some striking continuities between typological and population-based race concepts both before and after the Second World War. There were population-based evolutionary concepts in German race science before World War II, and there were typologies and typological aspects in human population genetics after World War II. She suggests that all of this work should be assessed in a symmetrical way, within a comparative frame that includes the work of other scientists at the time and later. An overemphasis on the problems of Nordic supremacy, she suggests, has led too many historians to fail to notice both the dissent within Germany—sometimes by extremely effective Jewish scientists who recognized the logical and methodological weaknesses of Nazi race science—and the peculiarities of the scientific community that did emphasize the Nordic race. Her attention to Dunn, who not only carried out fascinating work in Rome but who was also a key player in the UNESCO race statements, permits her to excavate the ways that populational thinking was supposed to resolve the problem of race in genetics.

Like Lipphardt, Emmanuelle Sibeud presents a deep fundamental challenge to the existing historiography. She calls into question any simple connection between colonialism, imperialism, and physical anthropology after 1880 and pro-

15. Morris Low's comments at the symposium reconstructed in an e-mail to the editors, July 2010.

vides insight into practices of what might be called "leisure time" anthropology. These practices reflected the surprisingly limited legitimacy of French physical anthropology at the very moment when its influence should and could have been escalating as the French empire expanded in the late nineteenth century. The old tale of anthropology as an uninflected tool of colonialism does not hold up to historical scrutiny, and practicing anthropology in the French Empire was tenuous, insecure, and not incorporated into the structure of the colonial administration. Securing support for anthropological projects was extraordinarily difficult, and schisms within anthropology undercut the institutional legitimacy of the field. Data on colonial bodies played a relatively small quantitative role in the databases of physical anthropology as a whole in France, and the legacy of Paul Broca, who favored laboratory over fieldwork, had far-reaching consequences for the discipline. By 1900, Sibeud shows, the rise of republican universities shifted anthropology to the academic fringes. Her account underscores the complexity of assessing the roles of anthropologists in any colonial enterprise.

In the colony of Hawaii, Anderson shows that scientific ideas about a natural race-crossing laboratory reflected social and political pressures in the United States. He described his paper in a conversation at our meeting as a consideration of "why Barack Obama is not our first Hawaiian president" in light of the ways that the political and scientific ideas he considers shaped the social experience of race that molded the young Obama in the 1960s. Physical anthropologists from the American Museum of Natural History and Harvard University saw Hawaii in the 1920s as a "racial laboratory" of hybridization, but instead of validating the biological force of race, their research in the Pacific came to reflect the ideas of Franz Boas about human plasticity and the importance of culture. This occurred at the very moment when a mainland notion of blood quantum in racial identity was being institutionalized in Hawaii by the haole-dominated territorial government. The racial technology of the blood quantum was, ironically, imported from colonial management practices in American Indian reservations. While scientists were praising human hybridity and enjoying what he calls "their modernist biological moment," mainland classifications and social rules moved to the islands uninflected by anthropological theory.

Midcentury theorizing about evolution was similarly uninflected by anthropology, as Vassiliki Betty Smocovitis proposes here. The relative absence of anthropologists from the early development of the evolutionary synthesis raises central questions about the field today. The synthesis as it was understood made possible the reduction of the social sciences to the biological sciences and ultimately to physics and chemistry. Within that positivist ordering, social sciences such as anthropology would be reducible to biological sciences. The role of this tension in anthropology today cannot be overstated. As a category of scientific knowledge, biological anthropology is the discipline in life sciences devoted to the study of a single species. She asks, "What would an equivalent biological scientific category devoted to fruit flies look like?" (Smocovitis 2012). And what is the logic of the field of primatology, which boomed in the wake of Washburn's influence in the 1950s, as a category of scientific study devoted to primates but which not only excludes humans but is also an autonomous subset of the larger category, anthropology? Biological logic would dictate that humans be studied by primatologists. Instead, she observes, anthropology preserves and instantiates the special status of humans.

Capturing in a different way some of the interdisciplinary forces shaping biological anthropology, Little here suggests that the field "came of age" during the second half of the twentieth century, particularly after 1945. This period saw a revitalization of the profession, with expanded studies of living populations that focused on body composition, child growth, nutrition, environmental physiology, epidemiology, and demography. The International Biological Programme and its Human Adaptability Component provided a range of new insights about populations around the world and a model for multidisciplinary field research. New technical capabilities in human genetics and the shift from "phenotypic inference" to a more sophisticated mode of "direct DNA" or "molecular genetics" analysis made it possible to ask new questions. New fields of investigation from the 1980s through to the end of the century included reproductive ecology, behavioral evolution, Darwinian medicine, psychoneuro-physiological stress, and biomedical and health research. Little's account captures the many institutional and technological changes that reshaped biological anthropology after the war. Echoing Smocovitis, he tracks the unification of three subfields of anthropology in the 1960s as a theoretical appeal of ecological explanation began to attract those trained in social anthropology, archaeology, and biological anthropology. Little's account captures the diversity and interdisciplinarity that has shaped biological anthropology since 1945.

Clark Spencer Larsen and Leslie Lea Williams provide a focused account of a different and crucial institution, the *American Journal of Physical Anthropology*, one of the leading journals within the field. They show that today about 30% of the contents of the *American Journal of Physical Anthropology* relates to living human variation (human biology) and that international submissions have increased in recent years. Their analysis helps us place the study of living human populations in the broader context of biological anthropology as a whole. The time period chosen was Larsen's editorship of the journal, from 2001 to 2007, but it also proved to be a time when international submissions increased and attention to the study of living populations, especially human genetics, continued to grow. More multinational and collaborative research and non-U.S. authorship could reflect stronger ties in biological anthropology programs across national boundaries, but it also almost certainly reflects increasing electronic access to the submission process. By considering patterns of submission and publication in this key journal, Larsen and Williams illuminate the increasingly international networks of biological anthropology after 2000.

A Global Form of Reason

With the next group of papers, we turn to the transnational negotiation of human biological diversity research and the global forms of reason that shaped it. Race plays some role in every paper here, but this group of papers provides perspectives on its meanings and negotiation transnationally over a century of scientific change.

In his consideration of the roles of European ideas in South African concepts of race and difference, Alan G. Morris proposes that the rise of apartheid in the 1940s was not rooted in the country's physical anthropology of the previous decades. Rather, the engineers of apartheid were for the most part Afrikaans-speaking ethnologists operating out of the Afrikaans-medium universities where little or no physical anthropology was taught, he suggests. Although none of the early practitioners of physical anthropology in South Africa were directly involved in the implementation of the apartheid policy, Morris notes, their strict typological approach to human variation did provide a solid growth medium in which the government policies could develop without credible scientific opposition. Later, some gave testimony before the notorious Race Classification Board set up to hear appeals from individuals seeking to change their race status. The implementation of apartheid after 1948 was a political process that was out of step with most of the post–World War II world, and the ideology that supported it came out of the central European theory of "ethnos." It also drew on the more general physical anthropological concept of typology, but it was not, Morris suggests, strictly homegrown. European ideas played a central role in South African apartheid.

Marks provides a critical perspective on the relationships between genetics and physical anthropology in the first half of the twentieth century, considering how a global community negotiated the relevance (or irrelevance) of genetics to anthropology. His account challenges a commonly repeated story of this relationship—in which anthropologists were unable to recognize the value of human genetics because they were poorly trained—and shows instead that the field of racial serology, as it gained momentum in the 1920s, produced entities that were not recognizably racial and fundamentally therefore not of interest to physical anthropology. Marks notes that the field of racial serology effectively ended in 1963 with a review in *Science* that identified 13 serological races: one African, two Asian, five European, one American, and four Oceanic. As Marks shows here, it was not until the rise of population genetics after 1945 that genetic data began to seem more relevant to physical anthropology, and something called anthropological genetics emerged in the 1960s. Marks explores the crossroads of genetics and anthropology over the last century, the ways that different kinds of data were taken as primary by scientists with different disciplinary and national identities, and the common valorization of genetic explanations and DNA into the present.

Providing an equally interdisciplinary perspective on related issues, Perrin Selcer looks at how the UNESCO statements on race illuminate the consolidation of the postwar liberal racial orthodoxy. Persons unmarked by race, Selcer notes, gained authority on the question by virtue of their racelessness—whites (one might argue the *most* "interested" category in the power dynamics of the racial system) were presumed to be capable of producing science that was less biased than that to be expected by those marked and colored. Because race structured so much of twentieth-century society, from international politics to playground etiquette, Selcer notes, whatever the UNESCO statements said had to play well in many venues, including the popular media and scientific journals, at the UN, in the United States, and in newly independent nations. It is unsurprising under these circumstances that producing the statements was tortuous and that the statements themselves often carried multiple meanings so that different audiences could discover congenial interpretations. They were also controversial, although Selcer shows that the controversies are not always exactly what they seemed. By attention to the three UNESCO race statements, he provides a portrait of uncertain scientific authority and shifting social expectations, exploring how the same data could be used to argue opposite points.

In Pálsson's work on the Icelandic biomedical company deCODE, we see different kinds of global truth and different forms of race in action. With the advancement of genomic research, the issue of human variation has been redefined through new engagements between experts and laypersons. Consumers have become active collaborators in personal genomics, participants who work on themselves and who make their way into membership in a new biosocial community. But they have also become implicated in networks over which they do not have control. In Pálsson's own experience as a consumer of genomic testing, recounted here, he is reduced to comparing his DNA to that of James Watson when his family members—his own social kin—are reluctant to join him in the new world of consumer genomics. In a global network of increasing sophistication, anthropologists should be able to participate and collaborate with at least the guiding assumption that *Homo sapiens* is an undivided being and that decoding it—to the extent that the language of "decoding" is the appropriate one—requires integrative perspectives that in the absence of a better nondualistic language resonate with our biosocial nature/culture. This will not be easy, he says, but it is the only meaningful way to go.

Collecting and Contested Ownership

Our next group of papers considers material objects and their ownership, a topic that, as mentioned above, came to seem central to our project. The human materials and remains that provide evidence in science also provide evidence of history (Lindee 1998).

The community of biological anthropologists in the United States had a particularly active role in the negotiation of race in light of the legacy of slavery, and Watkins considers the emergence of "the American Negro." Echoing Anderson's questions, Watkins's paper might be parsed as explaining "why Barack Obama was not the U.S.'s first *African* president."[16] She looks at the idea—as it was elaborated in a range of scientific research programs—that "American Negroes" constituted a racialized hybrid product of biohistorical forces. American physical anthropologists described the special properties of the American Negro, and black bodies played a role in the establishment of racial and scientific authority. Her elucidation of the simultaneous construction of the American Negro as both a hybrid and racially distinct suggests that explanations of difference in early twentieth-century bioanthropological research cannot be easily distinguished as racialist and nonracialist, the standard categories. Her close reading of studies of American Negro skeletal and living populations dating between 1924 and 1950 takes ideas about admixture as a historical and technical problem and asks questions about scientific methodology, collections, and social entanglements.

In Kakaliouras's study of repatriation, she suggests that in the 20 years since the passage of the Native American Graves and Repatriation Act (NAGPRA), the cultural context for the practice of archaeology and bioarchaeology has been transformed. Disciplines that have traditionally studied material remains in the absence of their makers (archaeology) or biological remains in the absence of their descendants (osteo/bioarchaeology) now manage to live with repatriation as a professional reality. Repatriation, she suggests, changes the world of things (or thing-worlds) for both Native North American people and biological anthropologists. Repatriation has also opened the possibility for Native ancestral remains to occupy a whole different set of spaces and places: to be in transit across large geographic regions, to be in new tribally run curation facilities, or to simply be set apart from other bones, perhaps waiting for a repatriation claim to be made or settled. Repatriated remains also perform time travel, forming an uneasy bridge between the "prehistoric" and contemporary. Thus she shows that repatriation has produced a new category of archaeological and contemporary material culture—the "repatriatable." Repatriatables as such have significant power in the present and have stirred a whole set of complex and long-standing cultural and historical sentiments toward them, from Native people and anthropologists alike.

In a related way, Turner, in her exploration of the philosophical and institutional history of bioethics, is attuned to the practices that collections can produce. Like Kakaliouras, she notes that there are examples of repatriation efforts that have been successfully accomplished, and the repatriation of native material has been in process for years. But there are many unresolved questions. Some materials are clearly subject to the legal requirements of repatriation, but other materials, such as DNA samples, are currently being collected with the explicit attempt to preclude any possibility of return or destruction. She calls attention to the many roles of Neel, who was the primary author for a World Health Organization working group that produced two reports, in 1964 and 1968, that detailed the obligations of researchers to study populations. All of his proposals were in line with standards elucidated at both Nuremberg and Helsinki and could even been seen as farsighted at the time, Turner suggests, yet his work in the field with Napoleon Chagnon in 1968 became the focus of a remarkable controversy in anthropological circles in 2000, and the controversy itself suggests the validity of Turner's perspectives: standards are continually shifting, and holding relationships "still" in some dependable way into the future is extremely difficult.

Reardon and TallBear explore the assumptions of privilege that shape all interactions between scientific experts and studied groups. They propose that in the name of being against "race," contemporary scientists continue to make claims to control Native peoples and to own their resources in the name of "whiteness." Reardon and TallBear argue that while biological anthropologists and geneticists commonly state desires to build an antiracist future, often they do so on conceptual and material terrains that leave intact old links between whiteness and property. Exploring the deeper histories of the relationships between whiteness, property, and the human sciences, they consider how scientists and courts make sense of bodily materials. While indigenous peoples explicitly assert their right to narrate their own histories and identities, Euro-American nation-states and scientists usually need not do so, as these histories and identities are recognized and upheld in dominant systems of law and science. It is an example of the common power of things that do not need to be said. Dominant legal and regulatory mechanisms are shaped by histories of racism and colonialism, and it is these relations that must be addressed in order to respond to the problems created by the constitution of whiteness as property by both the law and the life sciences.

New Powers

Just as Pálsson proposes that the biological and the social cannot logically be separated, Véran proposes that the past and present are working together in biological anthropology and are very difficult to tease apart. His paper points to the vexed status of the anthropologist today—biological or social—who testifies to the legitimacy of categories of historical oppression for the benefit of the oppressed. Remains stored as museum collections or objects of scientific study, he suggests, keep the old anthropology in play, facilitating a new balance of power where the "hard evidences" of yesterday—

16. Promoters of the birther movement in the United States proclaim that he is in fact African and not a U.S. citizen.

the bones, skulls, and blood samples—perform the circumstances of their original collection in the field in new ways. In the repatriation process, Véran notes, they are resignified dialectically, and this dense resignification is the reason that despite many museums' strong voluntarism and deep commitment to repatriation, tensions and conflicts persist. Thus the paradox of contemporary biological anthropology: the anthropologists have never been more committed to ethically aware practices as a guarantee that past mistakes can finally be left behind, and groups demanding the return of materials have never been more committed to keeping the past in play, present, and relevant to their own grievances. Some of the turmoil within anthropology itself, he suggests, reflects this redefinition of power.

Our final "paper" is a nuanced and engaging interview of Noel Cameron conducted by Joanna Radin. His input during the conference came to crystallize our concerns in ways that surprised even Cameron. The formal paper contributed to the conference was an excellent account of his research. But in our discussions it was Cameron's perspectives on his own intellectual trajectory that came to seem most relevant to the themes of the conference, and we therefore proposed to him (at a luncheon meeting in Philadelphia while he was visiting Princeton) that an interview might be a way to capture these perspectives. Radin, who has worked with us on this project in critically supportive ways and whose own doctoral research engages with relevant questions, conducted the interviews and worked to edit them, while Cameron was a full participant in editing and amending the oral history. The resulting text is both an individual life story and a window into the evolution of a community in biological anthropology. His interview shows how radical were the transformations in the practice of physical/biological anthropology in South Africa during the twentieth century. We would suggest that Cameron's experiences reflect more general changes by the end of the twentieth century. After 1945, and in some ways because of the political events of the two world wars and the resulting global political realities, those practicing biological anthropology changed their ways of work and their ways of thought. Carleton Coon remained a full professor at the University of Pennsylvania—and an unrepentant racist—but a new group of practitioners in the United States and elsewhere was sensitized to the strange legacy of racial thought in the field and were determined to draw on new technical and mathematical tools to illuminate human biology in ways that recalibrated what politics meant. In the old order, political priorities seemed to distort thinking about human biology. In the new order, as in Cameron's work, political priorities (including repression, limited access to resources, health care, etc.) could be seen to shape biology itself in human growth, a measurable phenomenon.

We can thus track a general shift to a recognition of politics as inside biology, inside the skin, in body fat, physiology, reproductive rates, disease—in other words, physical/biological anthropology moving toward a science of human biology that could take into account racialized human experience and its biological consequences without construing the resulting group differences as justifying inequality or as grounded in heredity. Indeed, increasingly, racial group differences could provide evidence of inequalities that needed to be eliminated.

Conclusion: A New Look at Biological Anthropology

Many of the people who were historically the focus of field research in physical anthropology were viewed as living in some way outside of time. The modern industrialized world changed rapidly, but the worlds of those studied were often seen as stable, timeless, "without history" (Wolf 1982). The notion of timelessness plays a role even in more recent initiatives, including the Human Adaptability Project of the International Biological Program in the 1960s, the Human Genome Diversity Project in the 1990s, the contemporary ongoing DNA collection of the Genographic Project, and the creation of a dazzling array of new DNA databases in recent years for medical and entertainment purposes. We could therefore be seen as telling a time-inflected story of the meanings of timelessness. Selcer proposes that the archival record of the UNESCO race statements keeps the focus on race: "In the act of debunking scientific racism, the antiracist intellectual inadvertently keeps the focus on the very biological facts he insists are insignificant" (Selcer 2012) focus on a past that we hope can become a resource for moving forward.

Watkins proposed, however, that the embarrassment may not be shared by biological anthropologists who are marked by the forms of race that once constituted the central technical subject of the discipline. Speaking of the experiences of African American biological anthropologists today, she noted that "we don't have the privilege to avoid the history." In thoughtful comments after the meeting, she observed that the contributors to our volume do indeed reflect a broader shift toward merging political and intellectual priorities in research in biological anthropology. However, this shift has occurred primarily among "nonraced" scholars who have had the privilege of deciding whether or not to notice the politics of their work. For scholars affiliated with groups that historically entered the field only as research subjects, ignoring the history was not possible, and there may be no shared sense of embarrassment now.

The purpose of any Wenner-Gren symposium is to create an environment for discussion, and the papers we invited to the table were intended to build a space for thinking and talking. Indeed, at a Wenner-Gren symposium, participants do not traditionally formally discuss the papers in great detail. Rather, the precirculated texts become stepping off points, points of departure, and are left in the background as the conference takes form. To return to pregnancy, Gregory Bateson in the 1960s invoked the metaphor when he suggested that a Wenner-Gren symposium was like "a beast," something that could come alive after a long gestation and a long planning period and was only given "its collective birth when the participants come together." The anecdote is recounted in Sydel Silverman's compelling history of these meetings (Silverman 2002). "When a conference jells," she proposed, "the beast comes to life; it settles down at the center of the table, growing and growling, only to slink away when the conference ends, never to return." When the editors and authors return to their papers and their ideas, having flown home and (in our case) left the remarkable hummingbirds and marmosets behind, they have all been changed.[17] We present here these changed papers, written by people who were also changed by our joint discussions and by shifts in our collective perspective. In the context of the conference and in the process of reviewing their papers in the following months, our participants came to terms with questions of being both "embarrassed" and *embarazada* (pregnant) with new ideas.

Acknowledgments

We thank all of the participants at our meeting in Teresópolis and the gracious staff of the Hotel Rosa dos Ventos. We also owe special thanks to Leslie Aiello, Victoria Malkin, and Laurie Obbink from the Wenner-Gren Foundation, which has supported this project from the very beginning; to the anonymous reviewers who helped make the final volume much stronger; and to Alan H. Goodman, who many years ago inspired us to undertake this project.

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17. It is true that Ricardo Ventura Santos and Jean-François Véran live in Rio de Janeiro, 2 hours southeast of Teresópolis, so perhaps they did not leave the hummingbirds and the marmosets behind, but they did at least leave the conference changed.

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Guardian Angel on a Nation's Path

Contexts and Trajectories of Physical Anthropology in Brazil in the Late Nineteenth and Early Twentieth Centuries

by Ricardo Ventura Santos

In this paper I analyze the trajectory of Brazilian physical anthropology from the late nineteenth century to the early decades of the twentieth century, framing it within the prevailing historical and sociopolitical context. The focus will be on the research and reflections of anthropologists at the Museu Nacional (National Museum) in Rio de Janeiro, one of Brazil's most influential anthropological research centers, from 1870 to 1930. The main aim is to understand why these anthropologists distanced themselves from explanatory approaches that placed mestizos and other non-Europeans on inferior levels in the hierarchy of human races. I argue that the position taken by physical anthropology at the National Museum was the result of far-reaching intellectual and political dynamics operating well beyond academic borders. Anthropologists from the National Museum—and Edgard Roquette-Pinto in particular—shared the nationalist ideals defended by a portion of the early twentieth-century Brazilian intelligentsia. It is also argued that, contrary to some historical interpretations, the antideterministic position of physical anthropology at the National Museum was independent of the Boasian influence. Although there were superficial similarities between the Boasian ideas and those of a segment of the physical anthropology produced in Brazil, the most significant influences came from other, locally produced sources.

Introduction

In *The Hour of Eugenics*, one of the most influential books on the history of eugenics in Latin America between the late nineteenth and early twentieth centuries, science historian Nancy Stepan describes the implications of Brazilian anthropologist Edgard Roquette-Pinto's visit with Franz Boas in the 1920s.

> The Mendelian anthropologist Edgard Roquette-Pinto played an even more public role in keeping eugenics from being identified with strident racism at the First Brazilian Eugenics Congress in 1929. His contacts with the antiracist anthropologist Franz Boas in New York in 1926 helped to make him a defender of the value of ordinary Brazilians of all racial types. (Stepan 1991:160)

In the mid-1920s, Roquette-Pinto was a consecrated intellectual in Brazil. A renowned anthropologist, director of the country's main museum of natural history (the Museu Nacional [National Museum] in Rio de Janeiro), member of the Brazilian Academy of Letters, and one of the founders of the Brazilian Academy of Sciences, he was a famous public figure. Roquette-Pinto's work in physical anthropology was so influential in the 1920s and 1930s that it was cited by Gilberto Freyre in his *Casa-grande & Senzala* (*The Masters and the Slaves*), one of the most important sociological and historical essays on the formation of Brazilian society, first published in 1933. Coincidentally, in an allusion to New York and also to the Brazilian Eugenics Congress, in the preface to *Casa-grande & Senzala*, Freyre recalled an episode he witnessed in the 1920s when he was studying at Columbia University.

> And of all the problems confronting Brazil there was none that gave me so much anxiety as that of miscegenation. Once . . . I caught sight of a group of Brazilian seamen—mulattoes and *cafusos*—crossing [the] Brooklyn Bridge. I no longer remember whether they were from [the ships] *São Paulo* or *Minas*, but I know that they impressed me as being the caricatures of men, and there came to mind a phrase from a book on Brazil written by an American traveler: "the fearfully mongrel aspect of the population." That was the sort of thing to which miscegenation led. I ought to have had some one to tell me then what Roquette-Pinto had told the Aryanizers of the Brazilian Eugenics Congress in 1929: that these individuals whom I looked upon as representatives of

Ricardo Ventura Santos is Associate Professor in the Department of Anthropology of the National Museum, Federal University of Rio de Janeiro, and Senior Researcher at the National School of Public Health, Oswaldo Cruz Foundation (Escola Nacional de Saúde Pública/FIOCRUZ, Rua Leopoldo Bulhões 1480, Rio de Janeiro, RJ 21041-210, Brazil [santos@ensp.fiocruz.br]). This paper was submitted 27 X 10, accepted 7 IX 11, and electronically published 9 II 12.

 0011-3204/2012/53S5-0003$10.00. DOI: 10.1086/662660

Brazil were not simply mulattoes or *cafusos* but *sickly* ones. (Freyre 1946:xx–xxi)

In this passage, Freyre alludes to the paper "Nota sobre os typos anthropologicos do Brasil" (Note on the anthropological types of Brazil) that Roquette-Pinto presented at the Brazilian Eugenics Congress in 1929. Based on a study of physical anthropology, Roquette-Pinto concluded that "none of the types in the Brazilian population presents any stigma [mark] of degeneration" (1929:145). In a sense, this was a dissident position for the time, as Freyre himself suggests.[1] After all, the history of physical anthropology in the late nineteenth and early twentieth centuries provides abundant examples of widespread explanations that fueled convictions on the inequality of the races; the dominance of the biological over the cultural, the intellectual, and the moral; and the negative consequences of racial mixing.

The principal objective of this essay is to analyze the trajectory of Brazilian physical anthropology from the late nineteenth century to the early decades of the twentieth century, framing it within the prevailing historical and sociopolitical context. The focus will be on the research and reflections of anthropologists at the National Museum, one of Brazil's most influential anthropological research centers, from 1870 to 1930.[2] The main aim is to understand why these anthropologists distanced themselves from explanatory approaches that placed mestizos and other non-Europeans on inferior levels in the hierarchy of human races.[3] The suggestion is that the self-ascribed role of anthropology as "guardian angel" of the Brazilian people—to quote an expression by Roquette-Pinto himself (in Ribas 1990:81)—was the result of far-reaching intellectual and political dynamics operating well beyond academic borders. Anthropologists from the National Museum—and Roquette-Pinto in particular—shared the nationalist ideals defended by a portion of the early twentieth century Brazilian intelligentsia to the point of displaying, according to historian Thomas Skidmore, "impressive scientific credentials to the growing campaign to rescue the native Brazilian from the deterministic trap" (1976:188–189).

A second objective is to problematize Stepan's argument. I suggest that Roquette-Pinto's antideterministic position was evident in his writings well before interacting with Boas.[4] In other Latin American countries such as Mexico, Boas carried great influence (Godoy 1977; Rutsch 1996, 2007). In Brazil, however, although there were superficial similarities between the Boasian ideas and those of a segment of the physical anthropology produced in Brazil, including Roquette-Pinto, the most significant influences came from other, locally produced sources.

Anthropology at the National Museum from 1870 to 1930

Founded in 1818, when the Portuguese court had been transferred to Brazil following Napoleon's invasion of Portugal, the National Museum was conceived "to spread the knowledge and study of the natural sciences in the Kingdom of Brazil" (Lacerda 1905:3; fig. 1). Research in physical anthropology gained its first impetus at the institution in the 1870s. The year 1876 witnessed the creation of the section "On anthropology, general and applied zoology, comparative anatomy, and animal paleontology" (Lacerda 1905:38), with physician-anthropologist João Baptista de Lacerda named as department director. The section's name illustrates the close associations between anthropological research and natural history.

Closely linked to the latest theories and techniques produced in European centers, the history of physical anthropology at the National Museum from 1870 to 1930 can be divided into two periods (Castro-Faria 1946, 1952). The first included the work of Lacerda, mainly covering the last decades of the nineteenth century, when the principal focus was on the craniology of the "indigenous races." The second, from 1910 to 1930, was associated with Roquette-Pinto's research at the institution. Having initially conducted physical anthropological studies of indigenous populations, his interests gradually shifted to the issue of miscegenation in Brazil.[5]

1. On the relations between Freyre and Roquette-Pinto, see the recent works by Pallares-Burke (2005) and Burke and Pallares-Burke (2008). See also Souza (2011).

2. During this period there were other research centers working in physical anthropology in Brazil. I am not using the example of the National Museum as representative of the Brazilian context because as we will see over the course of this article, on the contrary, it was an exceptional case. For more on physical anthropology in Brazil at the time, see Castro-Faria (1952), Santos (1998), Schwarcz (1993), Seyferth (1985), Skidmore (1976), and Stepan (1991), among others.

3. Corrêa (1982:213–217), Schwarcz (1993:95–98), Skidmore (1976: 185–190), and Stepan (1991:160–162) are some of the authors who also called attention to the "exceptional" nature of anthropology at the National Museum in the early decades of the twentieth century.

4. Lesser (1995:50) also argued that the interactions between Roquette-Pinto and Boas were decisive in his positions toward the influences of racial determinism in Brazil.

5. Throughout this paper, Lacerda and Roquette-Pinto are referred to as physical anthropologists. In a sense, this designation is a simplification of the two intellectuals' careers and output, because they circulated in various fields. Lacerda published work in the areas of "anthropology, physiology, pathology, tropical diseases, prophylaxis, veterinary medicine, and projects related to the Museum itself" (Lopes 1993:251). Concerning Lacerda's nonanthropological work, see especially the excellent study by Benchimol (1999). Roquette-Pinto, a self-proclaimed "naturalist" (Roquette-Pinto 1933:24), was no less diverse in his pursuits, with interests encompassing physical anthropology, ethnology, education, and other fields (Barbosa 1996; Castro-Faria 1952, 1956/1958; Ribas 1990; Seyferth 2008). Anthropologist Marisa Corrêa (1982) uses the term "physician-anthropologists," which is perhaps more appropriate for describing these intellectuals.

Figure 1. View of the Museu Nacional (National Museum), Rio de Janeiro, in the early twentieth century (from Lacerda 1905).

Craniology, Hierarchy, and the "Indigenous Races"

Physical anthropology at the National Museum was highly productive in the 1870s and 1880s, involving research and teaching activities and participation in events with wide public repercussions, such as the Brazilian Anthropological Exposition hosted in Rio de Janeiro in 1882. In 1876, Lacerda had explained that one of his aims was to disseminate anthropological studies, which had still not "found enthused followers among men of science" in Brazil (Lacerda and Peixoto 1876:47).

In the first volume of the *Archivos do Museu Nacional*, Lacerda published a study on craniology (Lacerda and Peixoto 1876) and another on dental characteristics (Lacerda 1876), both under the general title *Contribuições para o estudo anthropológico das raças indígenas Brasil* (Contribution to the anthropological study of the indigenous races of Brazil). At the time, the main theoretical and methodological reference for this and other research at the National Museum was "Broca's school" (Lacerda and Peixoto 1876:48), thus reflecting the strong influence from the French tradition.[6] During this period, physical anthropology in Europe (France in particular) flourished tremendously with the publication of numerous treatises and the development of a plethora of tools for the morphological characterization and evaluation of the human body (Blanckaert 1989; Gould 1996; Harvey 1983; Stocking 1968; fig. 2). The work by Lacerda and colleagues was based on detailed descriptions of bone morphology and measurements with the collective aim of constructing "a history of fossil man in Brazil," as he wrote in an article published in the *Mémoires de la Societé d'Anthropologie de Paris* (Lacerda 1875). Some of the key questions related to the number of "indigenous races," their antiquity, specific anatomical characteristics, whether dolichocephalic or brachycephalic, whether autochthonous to the New World, and so on.

As was common in the late nineteenth-century anthropological tradition, it was considered possible to make inferences on individuals' intellectual and moral attributes based on the study of their physical characteristics.[7] According to this approach, while an intense debate was raging on labor in Brazil because of the imminent demise of African slavery (in 1888, Brazil was the last country of the Americas to abolish it), Lacerda's studies led him to pronounce an unfavorable verdict on Indians' position in the hierarchy of the races and

6. The year 1859 witnessed the creation of the Societé d'Anthropologie de Paris, which had a pronounced influence in Brazil and other countries of the Americas (see Blanckaert 1989; Castro-Faria 1973; Gould 1996; Harvey 1983). Sá et al. (2008) analyze the formation of the collection of scientific instruments utilized in physical anthropology research at the National Museum in the late nineteenth century and early twentieth century, many of them brought from France.

7. Gould (1996) presents a series of case studies focusing on French and North American physical anthropology from the second half of the nineteenth century in terms of their attempts to correlate physical, mental, and moral attributes. See also Stocking (1968:13–41) and Blanckaert (1989).

Figure 2. Dynamometer, Physical Anthropology Instrument Collection, Museu Nacional (National Museum), Rio de Janeiro. (A color version of this figure appears in the online edition.)

their future potential for effectively participating in Brazilian national life. According to Lacerda, the cranial characteristics ("the organ's thinking portion reaches tiny proportions"; Lacerda 1882*b*:23) and dental traits ("a stamp of animality imprinted in the dentition"; 1876:82) already displayed the intrinsic biological conditions of inferiority, and the study of "three well-built adult male individuals belonging to the Xerente tribe and two Botocudos" (Lacerda 1882*a*:6) during the anthropological exposition of 1882 (fig. 3),[8] held at the National Museum, confirmed the pessimism that his bone research had already anticipated.

> They are ferocious, with no art whatsoever, and with no propensity towards progress or civilization. . . . As a manual laborer, the Indian is unquestionably inferior to the Negro. . . . We used a dynamometer to measure the muscle strength of adult individuals . . . and the instrument recorded a force below what is generally observed in White or Negro individuals. . . . In their graphic representation of length and distances, they proved to be devoid of any sense of comparison. . . . Their finest and keenest sense is their hearing. Nevertheless, combined sounds, whether of minor variations or simple melodic phrases, are rarely captured by the indigenous ear." (Lacerda 1905:100–101)

The Indian, a constant presence in nineteenth-century Brazilian thought, received increasing scientific treatment (largely pessimistic in nature) in the latter half of the century. At a time when most of the Brazilian population was concentrated on the coast—with vast areas of the hinterlands unknown but with various frontiers for demographic expansion—"the comparison between the historical Indian, a matrix of the [Brazilian] nationality, Tupi [speaking] par excellence, preferably extinct, and the contemporary Indian, member of the 'savage hoards' wandering the uncultivated backlands" (Monteiro 1996:15) acquired scientific legitimacy. Physical anthropology at the National Museum tempered its racial analyses with evolutionist notions. Placing the Indians on the lowest rungs of the racial hierarchy, it evoked the ideas of racial determinism commonly espoused at the time by such influential intellectuals as Henry Buckle, Arthur de Gobineau, and Louis Agassiz, among others, who wrote texts on Brazil's racial composition that were far from enthusiastic (Skidmore 1976: 27–32). By incorporating dictates concerning the inferiority of the "indigenous races"—"from the moral and intellectual point of view, the Botocudos are the expression of the human race at its most extreme degree of inferiority" (Lacerda 1882*c*: 2)—and drawing on the "the highly modern studies of Broca, Pruner-Bey, Quatrefages, Wirchow, Topinard, and others" (Lacerda and Peixoto 1876:47), the anthropology practiced at the National Museum in the late nineteenth century produced interpretative schemes that were well aligned with widespread prestigious scientific currents of the time.

In 1911, after a long period without publishing any work on anthropological themes—absorbed as he was by research in other areas and his activities as director of the National

8. For further information on the Anthropological Exposition, see Andermann (2004), Castro-Faria (1993:67–70), Lopes (1993:187–189), and Sanchez-Arteaga and Nino El-Hani (2010).

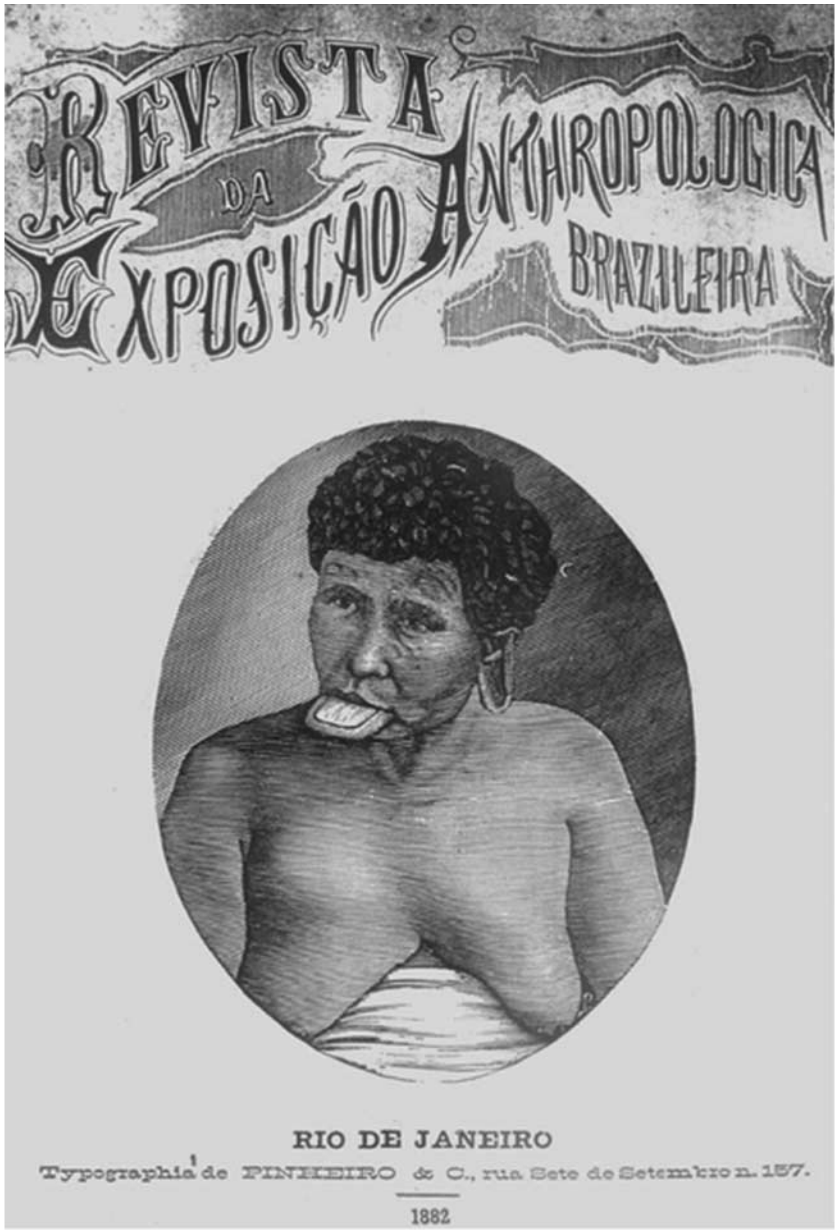

Figure 3. Cover of the magazine of the Brazilian Anthropological Exposition, showing a Botocudo woman (*Revista da Exposição Anthropologica Brasileira*), Rio de Janeiro, 1882.

Museum, a position he held for 20 years (1895–1915)[9]—Lacerda participated in the first Universal Congress of the Races held in London.[10] He attended as official representative of Brazil to present the paper *Sur les métis au Brésil* (On the

9. Lacerda did most of his work in physical anthropology in the 1870s and 1880s (Castro-Faria 1952:79–80). In 1880, the Laboratory of Experimental Physiology was founded, were Lacerda conducted his research on toxins and diseases, topics that became the main focus of his studies (see Benchimol 1999; Lopes 1993:191–194).

10. There is a recent historiography on the first Universal Congress of the Races, as can be seen in the forum organized by Ian Fletcher (2005). W. E. B. Du Bois played an important role in the organization of the congress, and according to Fletcher (2005:99), participants "came from all over the world, from Africa, Asia, North and South America, and Europe, more or less agreed on the biological unity and racial intermixture of humankind, and were committed to ideals of interracial and international understanding and civilizational parity and progress."

mestizos of Brazil; Lacerda 1911). According to the theory of whitening, as this work came to be known, Brazil was a racially viable country because its population was on the way to becoming a "white race." To achieve this, it would have to overcome certain obstacles. The first related to the fate of the Indians and Negros, and of the latter in particular, whose vices "were inoculated into the White race and the mestizos" (1911:12).

According to Lacerda, the Indians and Negros, because of their inherent racial inferiority, were fated to steadily disappear because of the process of "ethnic reduction." According to the paper, the second obstacle, and the one most difficult to remove, was the huge contingent of mestizos. Lacerda considered Brazilian mestizos physically inferior to the Negros in addition to being morally unstable; intellectually, however, they were comparable to whites. Not that the mestizos in general were naturally more viable in terms of intelligence; rather, the "generous slave owners," through a process of "intellectual selection," had encouraged certain individuals to participate in social life, thus producing a differentiated population of mestizos. According to Lacerda, the country was on the path to whitening because the mestizos, in addition to not forming a "firmly constituted race," tended by "sexual selection" to have children with whites, especially in Brazil, where "racial crossing does not obey precise social rules, such that the mestizos have all the freedom to mix with Whites" (1911:8). Together with the internal dynamics of racial transformation, Lacerda called attention to the role of immigration as a factor in accelerating the whitening process, through the infusion of "European/Aryan blood."

One could say that *Sur les métis au Brésil* is an exercise in reconciling the mestizo reality of Brazilian society with the scientific theories that disqualified the mestizo. As highlighted by Seyferth (1985:96), "the theory of whitening reflects the concern among part of the early twentieth century Republican elite with the problem of miscegenation and its meaning in the wider context of Brazil's history" (see also Cunha 2002: 271–275; Skidmore 1976:64–65).

Somatology, Degeneration, and the "Anthropological Types"

Lacerda had already stated during the Anthropological Exposition of 1882 that "anthropology is not merely speculative science, but is fitting for practical and useful applications" (1882*b*:6). Roquette-Pinto, his successor in anthropology at the National Museum, also insisted on this perspective. In a speech in 1944, he recalled his intellectual career.

> During more than thirty years in my modest career as naturalist and professor, I devoted my unswerving enthusiasm to the study of the race, the people, and the types of Brazil. . . . The objective scientific data, free of any sentimental influence, convinced me that in Brazil, the human problems do not result from noxious influences of crossings or biological atavisms, but are exclusively issues of the environment, of social legacy, and of culture. . . . It was my old anthropology that opened this avenue for me, in the desire to be useful, the only vehement ambition of my Brazilian soul. . . . I deemed to have found in science and technique the two "guardian angels" that should guide the way for our people. (In Ribas 1990:81)

The "thirty years" refer to the period from 1905 to 1935, when he was affiliated with the National Museum. Imbued with scientism and nationalism, the quote reflects Roquette-Pinto's perception that through anthropology it was possible not only to conceive of Brazil's path as a nation but also to act directly through science in the quest for solutions to important national problems.

In his early career, Roquette-Pinto approached various topics pertaining to the indigenous issue.[11] One of his main works was *Rondonia: anthropologia, ethnografia* (Roquette-Pinto 1917), originally published in 1917 and resulting from his participation in one of the expeditions by the Rondon Commission across the great tablelands of Mato Grosso in central Brazil in 1912 (fig. 4).[12] Under Lacerda, nineteenth-century anthropology at the National Museum was limited mainly to "armchair research," but Roquette-Pinto established direct intellectual contact with the field itself; with the *sertões* (backlands), he encountered the targets of anthropological research in their own environment. As indicated by the subtitle of the first edition, the book combines "anthropology" (i.e., physical anthropology) with "ethnography" (description of the customs, material culture, language, etc.) in an attempt "to take a snapshot of the social, anthropological, and ethnographic situation" (Roquette-Pinto 1917:xiv) of the Indians in Mato Grosso.

In relation to physical anthropology, *Rondonia* displays a persistent theoretical and methodological treatment based on race, where according to the author, "race is not a verbal expression without value or function; it always marks relations between a group of organisms and the environment in which they live. . . . According to modern anthropology, the dynamic observation of races, types, and individuals themselves has

11. As indicated by Castro-Faria (1952:32–36), Roquette-Pinto's MD thesis (1905) discussed the practice of medicine among the indigenous peoples from Brazil. Later, like Lacerda, he published several studies on archeological issues and bone analysis. For details on Roquette-Pinto's scientific work, see also Barbosa (1996), Castro-Faria (1956/1958), Lima and Sá (2008), and Ribas (1990). The recent PhD dissertation by Souza (2011) is a rich and detailed analysis of Roquette-Pinto's anthropological trajectory.

12. Roquette-Pinto's work is now largely unknown. However, it was read and used in important ethnological work produced subsequently, as for example in *Tristes tropiques* by Claude Lévi-Strauss. In his journey through the Brazilian hinterlands in the 1930s, the French anthropologist followed the same basic itinerary as the Rondon Commission in which Roquette-Pinto participated in 1912. Lévi-Strauss refers to the "charming book by the deceased Roquette-Pinto . . . [providing] brief indications of the very primitive populations discovered in these areas" (1982 [1955]: 284).

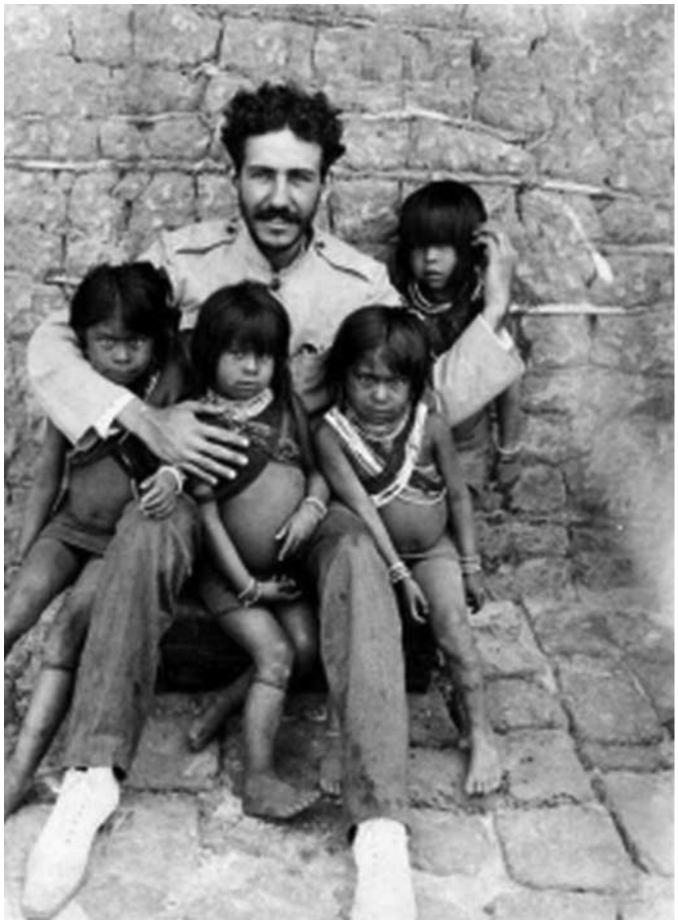

Figure 4. Roquette-Pinto with indigenous children during the expedition by the Rondon Commission in central Brazil in 1912 (from Roquette-Pinto 1917).

gradually become the only alternative for those that study with the purpose of finding the path to progress" (Roquette-Pinto 1917:126). The methodological approach in *Rondonia* and in the upcoming physical anthropology of Roquette-Pinto is linked to the French tradition—not so much to Broca himself but to followers of his school such as Adolphe Bertillon and Léonce-Pierre Manouvrier—as well as in the increasingly influential work of German physical anthropologists such as Félix von Luschan, Rudolf Martin, and Eugen Fischer, whose work featured somatology, that is, techniques for the qualitative description (hair texture, skin color, shape of the breasts, fingerprints, etc.) and quantitative measurement of the human body.[13]

At first glance, Roquette-Pinto's *Rondonia*, including the title itself, appears to be based on the analysis of a specific socioregional context. That is indeed the tone. However, a closer look reveals a universalist perspective. This dimension refers to the anthropological project whereby based on the study of the "primitive" (in the sense of the first, the non-Western, removed from civilization; in short, of "the other"), one comes to understand deeper issues linked to the essence of the human experience. The study of the "primitive" has been one of the traditional pillars in the history of anthropology (Diamond 1993; Kuper 1988). This universalist project allows one to more deeply appreciate various passages from *Rondonia*, including its introduction. According to Roquette-Pinto, "science transforms the world," producing changes that extend to a wide range of daily dimensions (life, death, etc.), even subverting the natural order. However, even in the face of all "progress," the human being "remains, after all, nearly the same primitive being, often feeling, thinking, and acting like his Stone Age ancestors" (Roquette-Pinto 1917:xi). This opposition between civilization and primitiveness not only evokes a broader anthropological project; in the case of *Rondonia*, it is associated with Roquette-Pinto's own personal career in the 1910s. In 1911, as he reports in the book (Ro-

13. Roquette-Pinto accompanied Lacerda as his assistant at the first Universal Congress of the Races held in London in 1911. On the occasion, according to Souza (2011:89), he took courses in physical anthropology and ethnography in Europe, when he interacted with influential physical anthropologists such as Félix von Luschan (see also Cunha 2002:271–275).

quette-Pinto 1917:32), he spent several months in Europe. The contrast between "the Stone Age man isolated in the heart of Brazil" and the impressions of the scientist who "had just arrived from Europe, with his brain still full of the world's refinery" (Roquette-Pinto 1917:108) is structurally essential to the author's worldview.[14]

The "primitive" reflecting the "modern" and vice versa is also evident in another highly significant passage from the book: "the cultured men of the planet prove to be white-skinned Indians, covered with a more or less thick coat of glossy varnish" (Roquette-Pinto 1917:xi). This excerpt characterizes an aspect that received great attention by Roquette-Pinto in *Rondonia* as well as in his later work. According to the anthropologist, under a "varnish" of culture—whether "backward" or "advanced"—is a human being, essentially equal in potential, whether a European or an Indian from Serra do Norte. If Roquette-Pinto draws on racialized models in his anthropological analyses of the Parési and Nambikwára, his emphasis is not on the existence of hierarchies in terms of potentialities. In this poetic tone he expresses the notion that the differences lie less in racial/biological constitution than in factors linked to culture and civilization, represented metaphorically by the "coat of glossy varnish." On this point, *Rondonia* differs to a major extent from other analyses and views of indigenous peoples in late nineteenth- and early twentieth-century Brazil.

Although race is an underlying concept in *Rondonia*, its ascendancy over other aspects of physical and social life is not so hegemonic. In Roquette-Pinto's view, the Indians' biological characteristics are not insurmountable barriers to their incorporation into "civilization." However, what he called an "inferior, primitive, backward" culture was indeed an obstacle to effective participation in a nationality.

> We should not be concerned with making them citizens of Brazil. Everyone understands that an Indian is an Indian, and a Brazilian is a Brazilian. The nation should protect them, or even support them, just as it accepts without reluctance the burden of maintaining abandoned children or indigents, the ill, the insane. Abandoned children and even the insane work; but society does not support them in order to exploit their labor. . . . The program will be to protect [the Indians] without directing, in order not to disturb their spontaneous evolution." (Roquette-Pinto 1917:200–201)

This logic is profoundly influenced by French-inspired positivism so present in the late nineteenth- and early twentieth-century Brazilian scientific and intellectual milieu, including in the work of Rondon and his followers, such as Roquette-Pinto. According to Ferreira (2008), positivism inspired by the French philosopher Auguste Comte played an important role in the institutionalization of Brazilian science from 1870 to 1930. According to the author, "the positivist ethos spread among intellectuals and scientists, spawning an understanding of the social role of science that conceived material progress and social modernization as the result of applying scientific knowledge and techniques to solving the country's problems" (Ferreira 2008:93). The positivists prized the proposal of a social function for technical and scientific knowledge through such strategies as objectivity with a view toward revealing society's problems and potentialities with the goal of devising and implementing practical solutions.

One of the pillars of positivism was the understanding that societies are in different stages of evolution. The positivists argued that if the Indians were given the proper conditions, they would evolve "naturally" to more advanced stages of the human condition. From this vantage point, one can understand Roquette-Pinto's dictum to "protect without directing, in order not to disturb their spontaneous evolution" (Roquette-Pinto 1935:201).[15] This positivist vision, sustaining the notions of "relative incapacity" and "tutorship," was profoundly influential in shaping Brazil's indigenous policy throughout the twentieth century (Diacon 2004; Skidmore 1976; Souza-Lima 1995).

Beginning in the 1920s there was a decreasing emphasis on indigenous studies by anthropology at the National Museum and an emerging interest in the "types" that made up the Brazilian people. During this phase, the most relevant publication by Roquette-Pinto was definitely "Nota sobre os typos anthropologicos do Brasil" (Note on the anthropological types of Brazil; Roquette-Pinto 1929), which was a detailed physical anthropology study based on data collected from young soldiers from all over the country living in military bases located in Rio de Janeiro, then the country's capital (fig. 5).[16] Nationalistic even in its conception (see Castro-Faria 1952:36), the publication was planned as a contribution by the National Museum to the events celebrating the centennial of the country's declaration of independence (1922). The text proposed

14. It is possible to read between the lines of Roquette-Pinto's commentary, which refers to a state of "primitiveness" even in human societies that he considered more advanced, and find an allusion to the violence raging in Europe during World War I, from 1914 to 1918.

15. Roquette-Pinto's scientific practice and social action were heavily influenced by positivism. He even swore an oath to positivism, published in the 1930s: "I believe that man and nature are ruled exclusively by immutable laws, above any will; I believe that science, by integrating man with the universe, created in his mindset both an infinite modesty and a sublime sympathy for all beings; . . . I believe that science, art, and industry will turn Earth into the Paradise that our grandparents believed . . . was in the other World; . . . I believe in the laws of positive Sociology . . . ; I believe that the noble mission of intellectuals . . . is the schooling and culture of the Proletarians . . . ; I believe that since it is very difficult to reconcile the often antagonistic interests of Order and Progress, there is only one way to avoid turmoil and misfortune: to resolve everything in light of altruism and especially fraternity" (Roquette-Pinto 1935:196–197).

16. According to Souza (2011:149–204), this investigation on the "anthropological types" of Brazilians included the collection of physical anthropology data from a much broader and diversified sample, including subjects from several regions of Brazil. The research also included the collection of data from children and women, for which a special team of female anthropometrics was trained. In the 1929 publication, Roquette-Pinto chose to present only the data collected from the recruits.

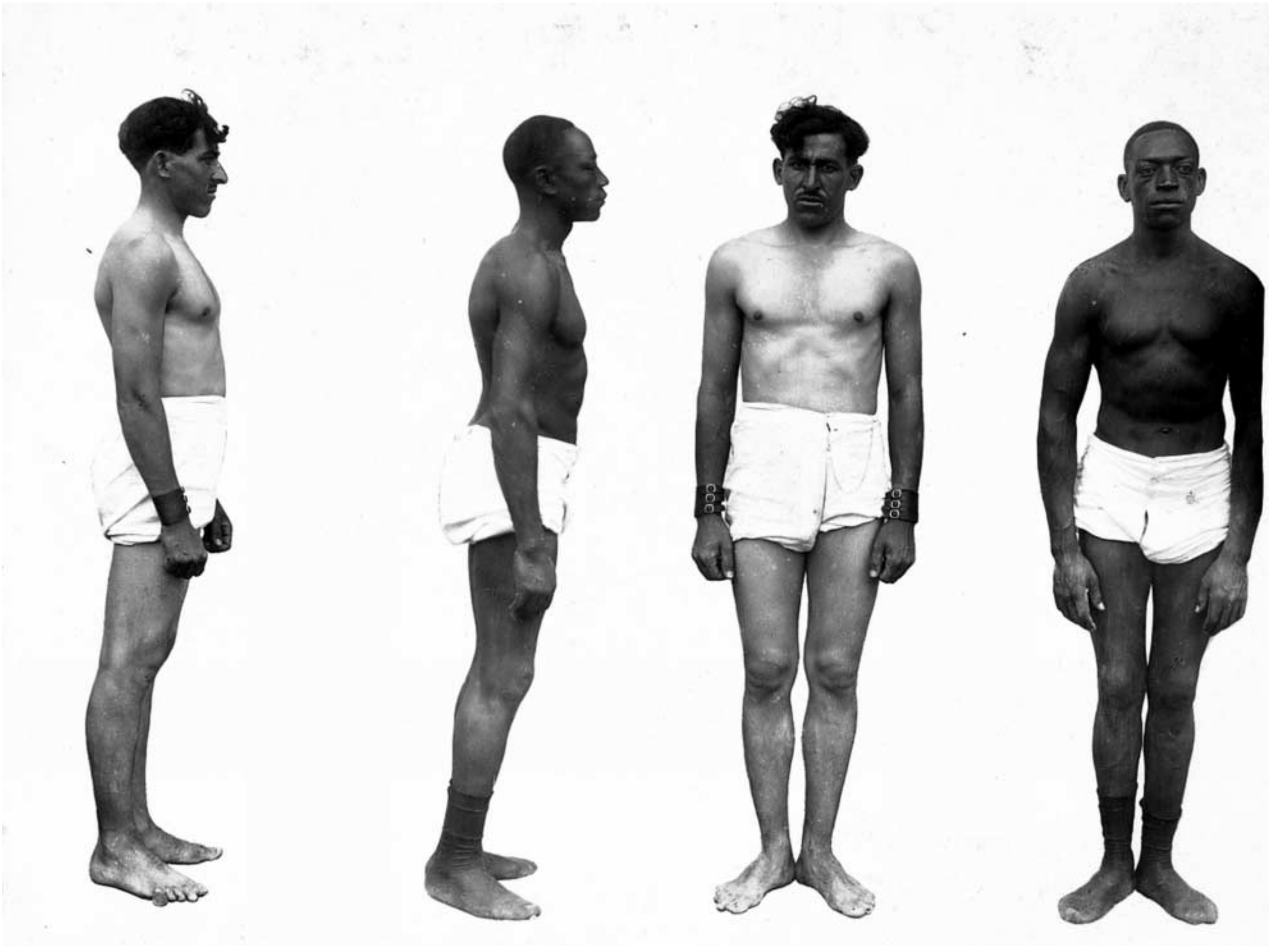

Figure 5. Images from Roquette-Pinto's research on the "anthropological types of Brazil" (Physical Anthropology Document Collection, Museu Nacional, Rio de Janeiro).

to evaluate "whether the . . . anthropological characteristics [of the mestizos] show signs of anatomical or physiological degeneration" (Roquette-Pinto 1929:123–124). Presented at the First Brazilian Eugenics Congress in 1929 (see Hochman, Lima, and Maio 2010; Souza et al. 2009), it became a manifesto in defense of the national types, an attempt to prove that "Brazil's anthropology belies and demoralizes the pessimists" (Roquette-Pinto 1929:147).[17]

Roquette-Pinto's argument consists of two parts. First, that Brazil has a large and underpopulated territory; second, that there is a question as to how many and which "souls" are needed to "take advantage of the country's wealth" (Roquette-Pinto 1929:119). The study's backdrop was immigration policy, where anthropologists had historically prioritized "recruiting White people with their weight in gold, with no restrictions or inspection" (Roquette-Pinto 1929:123). Roquette-Pinto saw things differently, thinking the "best national elements [abandoned to] indigence" (Roquette-Pinto 1929: 123). "Nota sobre os typos anthropologicos do Brasil" was his attempt to show that the Brazilian (mestizo) people themselves were capable of settling the land and exploring the country's resources and that what was lacking was not an adequate biological substrate—"the deficiency is not in the race" (Roquette-Pinto 1929:123)—but rather national organization.

"Nota sobre os typos anthropologicos do Brasil" aims to extend beyond a refutation of "pessimistic rhetorical fantasies" (Roquette-Pinto 1929:147). The argument derived its scientific authority from analysis of a large mass of data complemented by graphs, tables, and mathematical formulas. Analyzing the physical, physiological, and psychological/mental characteristics of "young men from all the States, sons and grandsons of Brazilians, 20 to 22 years of age, all healthy and subject to the same living conditions" (Roquette-Pinto 1929:

17. Among his many remarks on the "pessimists," Roquette-Pinto (1927) referred to Agassiz as "a professor from the United States, where they lynched a Negro with the same ease . . . with which one kills a mosquito" (287). On Gobineau, see Roquette-Pinto (1942:159–173).

124), Roquette-Pinto concluded that "none of the types in the Brazilian population presents any stigma [mark] of anthropological degeneration. On the contrary, all their characteristics are the best one could wish for" (Roquette-Pinto 1929:145). Roquette-Pinto's statement also applied to the highly stigmatized "mulattoes," because "none of the characteristics that were studied . . . allow one to classify them as involuted types" (Roquette-Pinto 1929:129). According to Roquette-Pinto, the solution to Brazil's problems lay in creating the conditions (education and health) for all of the "Brazilian types"—including the "phaeoderms" and "xanthoderms," to use the terms from his racial classification[18]—to display their full potential: "anthropology proves that man, in Brazil, needs to be educated rather than replaced" (Roquette-Pinto 1929:147).

At about the same time in which Roquette-Pinto was collecting the data that resulted in "Nota sobre os typos anthropologicos do Brasil," studies on similar themes were being conducted in Europe and the United States. As in the Brazilian study, the aim was to verify the biological and intellectual viability of mestizos. According to Provine (1986), in the first quarter of the twentieth century, the prevailing notion was that the human races differed both mentally and physically from the hereditary point of view and that miscegenation between individuals of very distant races was biologically harmful (see also Barkan 1992; Gould 1996; Provine 1973). This does not appear to differ greatly from the anthropological tradition of the second half of the nineteenth century; however, it was based on a new modality of biological explanation, namely Mendelianism.

With the rediscovery of Mendel's laws in 1900, research on human heredity entered a new phase (Bowler 1989:270–281; Mayr 1982:727–731). According to the Mendelians of the time, hereditary traits were transmitted by particles—the genes. The early twentieth century witnessed a widespread trend to interpret the most diverse physical and behavioral characteristics as resulting directly from genetic action. For example, in 1911 the influential North American geneticist Charles Davenport (with whom Roquette-Pinto corresponded) published a book in which he claimed that not only such characteristics as hair and eye color, arm span and leg length, and the size of internal organs, etc., but also nomadism, criminality, and musical skills followed the laws of Mendelian transmission (Provine 1973:791, 1986:866–867). As for mixing between individuals of different races, theories were proposed that flaunted the dangers of children being born with "large frames and inadequate viscera," "short stature with too large circulatory apparatus," or "union of a large-jawed, large-toothed race and a small-jawed, small-toothed race," resulting from "disharmonious combinations confined to physical characters" (Provine 1973:791). The "disharmonious combinations" were not limited to physical characteristics: "One often sees in mulattos an ambition and push combined with intellectual inadequacy which makes the unhappy hybrid dissatisfied with his lot and a nuisance to others" (Davenport 1917 in Provine 1973:791).

Another prominent figure in the discussions on the harmony and disharmony of interracial mixing was the Norwegian biologist Jon Alfred Mjoen, who argued that the mestizos' problems were due to hereditary physiological disorders: "it may well be that the glands in a descendant of parents from different races, thanks to the new combination of genes, are more or less disharmoniously adapted to each other" (1931:4). Mjoen (1931:3) mentioned the frequent occurrence of mestizos with disproportional extremities, abnormal body length, lung volume and muscular strength beyond the normal limits, a high diabetes rate, loss of organic equilibrium, and decreased resistance to tuberculosis, among others. Attention was called to the social implications of "uncontrolled" racial crossing. Mjoen said that "I am increasingly convinced that the tenants of our prisons and asylums are mostly recruited from mixed-race types, the number of whom is constantly growing, accompanying the increase in crossing among populations from the entire world" (1931:4).

Roquette-Pinto sympathized with the Mendelian schemes for explaining heredity. His book *Seixos rolados* (Pebbles) contains a long digression on "Mendel's genetics" (Roquette-Pinto 1927:176–179). In "Nota sobre os typos anthropologicos do Brasil," he takes a Mendelian perspective to interpret a genealogy featuring a white child with mulatto and black siblings, mulatto parents, and white grandfathers and black grandmothers: "by a simple game of Mendelian inheritance, one can see, like I have often seen, a White child . . . in the arms of a Black woman, his grandmother" (1929:138–139). Still, his appreciation for Mendelian schemes did not lead him to share the pessimism contained in the conclusions of such authors as Davenport and Mjoen in relation to the hybrids' physical and mental unfeasibility.[19] Mjoen's comments on the purportedly higher frequency of diabetes in mestizos were received with skepticism; the mortality statistics did not support an increased diabetes rate in "half-blood" individuals (Roquette-Pinto 1933:83–84).

Physical Anthropology and "Militant Nationalism"

The years during and especially following World War I (1914–1918) witnessed the emergence of a strong nationalist movement in Brazil (Oliveira 1990:145–158; Skidmore 1976:145–172; Stepan 1991:105–106). According to Skidmore (1976), warfare in Europe was a reminder that nationalism was ob-

18. According to Roquette-Pinto (1929), the most numerous racial types in Brazil were the "leukoderms" (whites), "melanoderms" (Negros), "phaeoderms" (descendents of mixing between whites and Negros), and "xanthoderms" (descendents of mixing between whites and Indians).

19. For a critique of Davenport concerning his interpretations of the effects of racial crossings, see Roquette-Pinto (1933:60–62).

solete and that a country's strength stemmed from its capacity to mobilize resources: its people, land, and industry. What were Brazil's prospects for becoming an important nation on the world stage? The racial issue permeated the Brazilian debate over national identity. From the view of the prevailing determinisms, Brazil's feasibility as a nation was limited; one of its foundations—its people—was perceived as having a weak (racial) constitution, lacking "biological coherency" because it was made up of "'raceless masses'—radical and terrifying heterogeneities instead of biological unities" (Stepan 1991:105). The nationalism of the 1910s and 1920s was a search for ideological liberation from the shackles imposed by racist ideals (Skidmore 1976:146). Oliveira (1990) refers to a "militant" current in this nationalism that "involved the search for a new identity and [whose] parameter was the refusal of the biological models underlying racist thinking" (145).

An example of the materialization of "militant nationalism" was the so-called Prosanitation League, a political-intellectual movement that from 1916 to 1920 "proclaimed disease as the country's main problem and the greatest obstacle to civilization" (Lima 2007; Lima and Hochman 1996:23). The intellectuals participating in the movement were opposed to racial and climatic determinism and considered rural endemic diseases the main obstacle to Brazil's project for redemption: the Brazilian people's indolence, laziness, and lack of productivity were due to diseases and abandonment (Kropf, Azevedo, and Ferreira 2003; Lima 2007; Lima and Hochman 1996:29–30; Santos 1998). These intellectuals were returning from expeditions to Brazil's rural areas, which they portrayed as isolated and abandoned by the country's elites.[20] The intellectuals of the prosanitation movement emphasized the image of Brazil as a sickly country, attributing to the sciences, and more specifically to medicine, an important role in the national reorganization process: "science would allow to unveil the truth and 'shed light' on the national problems, thus representing a crucial instrument for intervention by the state" (Lima and Britto 1996:138). In addition to the excellent analysis by Lima and Hochman (1996), recent historical studies have documented the intellectual and political climate that motivated and surrounded the prosanitation movement as well as its broader repercussions (Castro-Santos 1985; Lima 2007; Lima and Britto 1996; Stepan 1991; Thielen et al. 1991). One of the main results was the creation of the National Department of Public Health (focused on public health reform, one of the objectives behind the formation of the league), a federal agency that held considerable influence in shaping Brazil's health policy.

As an intellectual, Roquette-Pinto was heavily involved in Brazil's project for national redemption in the early twentieth century, and its interpretations of the anthropology of the Brazilian people were linked to him. His writings display an intense concern for health and education; the reasons for the purported inferiority of Brazilian national types were to be found at the environmental and social levels and were not biological or racial (Roquette-Pinto 1927, 1933, 1942). According to him, "The number of somatically deficient individuals in some regions of the country is really considerable. However, this is not due to any racial factor; it results from pathological causes, the elimination of which in most cases is independent of anthropology. It is a matter of health and educational policy" (Roquette-Pinto 1929:146). He had also been in contact with Brazil's rural reality because of his participation as a naturalist in the Rondon Commission in 1912 and portrayed in *Rondonia*, as we have already discussed.

Various facts can be mentioned that attest to Roquette-Pinto's degree of involvement in the nationalist scientific project to save the Brazilian people. For example, he was not only involved in the creation of the Prosanitation League but also participated actively in implementing its activities, including editorial collaboration in the journal *Saúde* (Health), which published the ideas of league members (Lima and Britto 1996; Lima and Hochman 1996). The reformist proposal of which he was a part also emphasized the issue of popular education, the field that he focused on increasingly beginning in the 1920s and especially after he left the National Museum in 1935.[21]

A Shift from Race/Biology to Environment/Culture?

Analyses concerning scientists' involvement in the nationalist issue in early twentieth-century Brazil frequently highlight a discursive shift from racial/biological explanations to those centered on the role of the cultural, economic, and social context (Castro-Santos 1985; Lima and Britto 1996; Lima and Hochman 1996; Skidmore 1976; Stepan 1991). Although the trajectory of (physical) anthropology at the National Museum and that of Roquette-Pinto in particular unquestionably fits this perspective, we should qualify the interpretation that racial/biological notions were completely abandoned in the interpretations of Brazilian social dynamics.

Mendelian genetics, which underwent significant development in the early twentieth century, introduced a series of new ideas for the description and interpretation of human and nonhuman biology (Bowler 1989:270–281; Mayr 1982: 727–776). Examples include the concepts of genotype and phenotype, which relate to an organism's intrinsic genetic characteristics (encoded in its genetic heredity) and the product of interaction between this heredity and the surrounding environment, respectively. These and related notions were al-

20. Thielen et al. (1991) present a set of these images.

21. In the 1920s, Roquette-Pinto was involved in founding educational radio stations, and in the 1930s he created the Instituto Nacional de Cinema Educativo (National Institute of Educational Cinema), of which he was the first director. See Barbosa (1996), Lima and Sá (2008), and Ribas (1990) for more information on Roquette-Pinto's work beginning in the 1930s.

ready being used in scientific/biological discourse in Brazil in the 1920s (e.g., Dreyfus 1929). While in *Rondonia* (Roquette-Pinto 1917) Roquette-Pinto does not take a Mendelian approach, it is quite evident in his later work, such as *Seixos rolados* (Roquette-Pinto 1927:163–205) and *Ensaios de antropologia brasiliana* (Roquette-Pinto 1933:55–62, 117–172). Such writings contain repeated references to concepts such as "germplasm"—"the nuclear substance of reproductive cells" (Roquette-Pinto 1927:173), "carrier of biological heredity, of the particle destined to orient development . . . genes" (Roquette-Pinto 1933:57)—and "somatoplasm"—"the part of the living being that does not enter into heredity" (Roquette-Pinto 1933:59), "the part of the individual capable of being modified by influences from the environment" (Roquette-Pinto 1927:173). Concerning the possibilities for interaction between the environment, germplasm, and somatoplasm, Roquette-Pinto stated that "the general opinion is that the environment influences the 'soma'; however, it respects the 'germ'" (Roquette-Pinto 1933:59).

According to Roquette-Pinto, distinct approaches were needed to deal with issues pertaining to the "soma" and the "germ" (Roquette-Pinto 1927:174; 1933:69–75). While "hygiene [health] attempts to improve the 'environment' and the 'individual,'" "'eugenics' seeks to improve the 'stock,' the 'race,' the 'descendancy'" (Roquette-Pinto 1933:70). Through education and medicine, hygiene could combat the deficiencies and illnesses of the somatoplasm; meanwhile, the weaknesses of the race, the germplasm, purportedly belonged to eugenics' sphere of action, given that "hygiene doesn't go there" (Roquette-Pinto 1933:71). The makeup of the germplasm, where the essence of racial characteristics was encoded, could only be altered in the long term, thus necessitating "the artificial selection of the good seed, facilitating its widespread propagation and thus hindering, if not stanching, the bad [seed]" (Roquette-Pinto 1933:71). Biological heredity was thus "the true domain of eugenics" (Roquette-Pinto 1933:71).

Roquette-Pinto's writings particularly reveal an intellectual who largely opposed the pessimism concerning the various races' biological attributes. For him, Brazil's anthropological problem had less to do with the "germ" than with the environment's deficiencies, which were negatively influencing the "soma." However, this does not mean that he believed in either the complete equality of biological attributes between the human races (at the ultimate level of the "germ's" characteristics) or in the absence of biological predisposition to certain behaviors. The following passage from his book *Seixos rolados* is revealing.

> The human races are indeed diversified in their bodily attributes and brain types, which does not allow considering them in the same degree of similarity. Everyone agrees on that. However, the disagreement begins and the errors accumulate . . . because the truth is that they are unequal on the same level. The races are unequal like the wavelengths in the spectrum. From red to purple, all the rays occupy the same plane. It is undeniable that some races are more intelligent, others more sentimental, and still others more obstinate. The spectrum also contains heat waves, light rays, and actinic rays. Go ask the ultraviolet rays for heat, and if they refuse to give it to you, will you call them inferior? . . . No. The races cannot be placed on planes of different heights, just as zones in the spectrum cannot be forwarded or delayed, except in intensity. If your fancy leads you to demand of the Negro the intelligence that is not the main endowment of his psychological disposition; or of the White, the patience of the Yellow, and of the latter the sentimentality of [the Negro], you will paint various portraits, in which the ethnic inferiority of them all will be brilliantly documented, each in turn." (Roquette-Pinto 1927:287–288)

This quote from Roquette-Pinto clearly emphasizes the existence of purportedly inherent inequalities among the human races. Nevertheless, one notes the coexistence of a logic that emphasizes inequalities with another that denies the possibility of establishing hierarchies. This is expressed in the ambiguity of the expression "unequal on the same level."

Analyses focusing on the trajectory of turn-of-the-century eugenics have emphasized the differences between a "negative" and "positive" current of thought in the movement (Hochman, Lima, and Maio 2010; Kevles 1985; Souza et al. 2009; Stepan 1991). The first, more common in Europe and the United States, was heavily informed by Mendelianism and postulated the implementation of coercive measures (e.g., sterilization) and policies for racial improvement. The second, more present in France and Latin America, was based on a neo-Lamarckian approach, contending that racial constitution could be implemented by improving environmental conditions, especially through health care and education. Roquette-Pinto's positions combined elements from eugenics' negative current (especially Mendelianism) and its positive one (the importance of the environment).[22]

Final Remarks

The historical archive of the National Museum contains an interesting map from the 1920s (fig. 6). It is a depiction of South America with signatures by some of the distinguished participants in the International Congress of Americanists held in 1924 in Göteborg, Sweden, including the names of Roquette-Pinto and Franz Boas (see Lowie 1925). This was

22. Cunha (2002:275–279) provides enlightening thoughts on the combination of Mendelian eugenics and early twentieth-century German anthropological tradition (as in Eugen Fischer) in Roquette-Pinto's physical anthropology even as he was a critic of those that condemned miscegenation in Brazil.

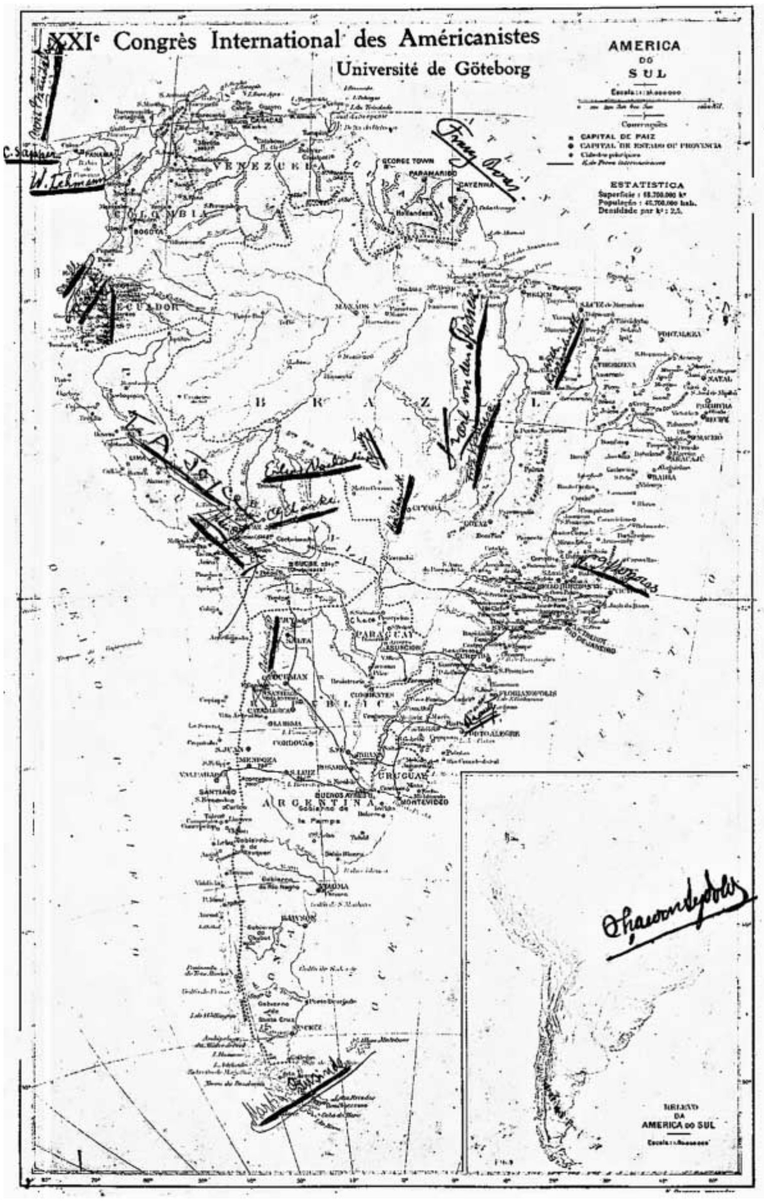

Figure 6. Map of South America with signatures of the participants in the XXI International Congress of Americanists, held in Göteborg, Sweden, in 1924. Includes signatures of Edgard Roquette-Pinto, Franz Boas, and Karl von den Steinen (Historical Archive Collection, Museu Nacional, Rio de Janeiro, Box 17, folder 25).

the first time the two met. The second time was two years later, in 1926, in New York, as mentioned above.[23]

Although Boas's work was known to Brazilian intellectuals in the 1910s and 1920s,[24] it is somewhat surprising that his ideas were not explicitly evident in Roquette-Pinto's anthropological interpretations despite the two scholars' apparent interpretative proximity, particularly the extensive defense of the environment's influence on the expression of sociocultural characteristics. I would argue that by the time Roquette-Pinto

23. Lima (2010) mentions a comment by Roquette-Pinto on Boas, published in his column in *Jornal do Brasil* in 1953: "Boas was always decidedly against racism. I met the old master personally in 1924, in Sweden. We became friends. At the Museum, I received numerous introductory letters from him brought by young disciples who were coming to study in Brazil" (Roquette-Pinto 1953 in Lima 2010). Roquette-Pinto is referring mainly to students of Boas who came to conduct studies in cultural anthropology in Brazil beginning in the 1930s. Souza (2011) provides detailed information on the correspondence exchange between Boas and Roquette-Pinto, which started in the 1910s.

24. See Keuller (2008), Lima (2010) and Souza (2011) on how Boas's ideas circulated among Brazilian intellectuals in the first two decades of the twentieth century.

visited Boas in New York in 1926, he was already completely converted to the idea that the causes of Brazil's backwardness were fundamentally sociopolitical rather than racial. In the preface to the 1919 edition of *Rondonia*, he wrote about his return from his journey to the Brazilian hinterlands in 1912: "[I have returned with my] soul renewed, confident in our people, whom some deem weak and incapable because they are a scrawny and ugly people. . . . It is necessary to go there [to the hinterlands] to strengthen again the trust in the destinies of the race and to return belying the harbingers of their decadence" (Roquette-Pinto 1917:44). In what may be the three main anthropological works by Roquette-Pinto (*Rondonia* [1917], "Nota sobre os typos anthropologicos do Brasil" [1929], and *Ensaios de anthropologia brasiliana* [1933]), Boas is practically not mentioned. Ironically, when he is, it is not in the sense of establishing a connection. In "Nota sobre os typos anthropologicos," the Brazilian anthropologist wrote that among the explanations of eugenicist Charles Davenport on "Mendelian inheritance in the human species" and the challenge by Boas concerning the role of Mendelianism, "my observations lead me to prefer the former" (Roquette-Pinto 1929:139).

Boas is known to have had a strong influence on the development of anthropology in some Latin American countries, particularly Mexico (see Godoy 1977; Rutsch 1996, 2007). He also influenced Brazilian sociocultural anthropology beginning in the 1930s, as in Gilberto Freyre, whose *The Masters and the Slaves* is a by-product of his master's thesis at Columbia University in the late 1920s. In a recent and well-based analysis, the sociologist and historian of science Nísia Trindade Lima also concluded that there was a weak link between Boas and Roquette-Pinto and even called attention to the dissimilarities between the two anthropologists' reflections.

> Franz Boas and Roquette-Pinto display marked differences in their way of conceiving anthropology and the anthropologist's craft. The former was a critique of evolutionism when applied to the notion of the development of human societies, in addition to being considered a pioneer in cultural anthropology. Meanwhile, Roquette-Pinto, under the influence of positivism and the nationalist currents in vogue after World War I, was a follower of the evolutionist perspective, drawing on it to oppose the biologically based fatalism and pessimism and to affirm the possibility of spontaneous development of different human societies. (Lima 2010:268)

Based on the evidence presented in this study, I suggest that it is not necessary to propose a diffusionist argument to explain the development of a certain antiracist current in physical anthropology, favorable to miscegenation, in early twentieth-century Brazil. Physical anthropology at the National Museum, in its self-ascribed role as "guardian angel of the Brazilian people," was not an isolated discourse in the country. It was a key component in the networks and scientific narratives related to the Brazilian nation-building process, closely associated with public policy making, in which the issue of racial mixing, so condemned by many and varied scientific currents of the time, played a central role. Anthropologists at the National Museum were not passive recipients of the racial theories in vogue at the time. Ideas were explored or emphasized as a function of a complex network of links that extended beyond the museum's boundaries as an institution and those of anthropology as a discipline. As stated by Cunha (2002), physical anthropology was part of a "nationalist project in which such themes as 'race,' 'people,' and 'nation' were supposed to be reappropriated by official discourse through a 'scientific' language" (275). Thus, under the "objectivity" that the craniometric and somatometric techniques appeared to confer on the research, there was a dynamic in which science and the political and social debates fed back into each other, or to use terms more in keeping with the anthropological discourse of the time, they "hybridized," "mixed," and "miscegenated."

Acknowledgments

Thanks to Carlos E. A. Coimbra Jr., Claudia Rodrigues-Carvalho, Dominique Sá, Gilberto Hochman, Marcos Chor Maio, Nisia Trindade Lima, Vanderlei Sebastião de Souza, and Verlan Valle Gaspar Neto for the conversations and suggestions on the themes discussed in this study. Susan Lindee, Joanna Radin, and Gonçalo Duro Santos provided insightful comments following the Wenner-Gren Foundation–sponsored symposium, which greatly helped me to revise this paper. Assis Gonçalves helped locate the map of South America (fig. 6) in the Historical Archive Collection of the Museum.

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The Birth of Physical Anthropology in Late Imperial Portugal

by Gonçalo Santos

In this article I analyze the emergence of the field of physical anthropology in the metropolitan academic sphere of the Portuguese Empire during the late nineteenth century. I suggest that Portugal's relatively peripheral position combined with a complex internal conjuncture of political instability and economic impotence gave early Portuguese physical anthropology a less explicitly "colonial" orientation than in other, more central Western European imperial powers. I describe the various national and international exchanges leading to the birth of this naturalist anthropological tradition at the University of Coimbra, drawing particular attention to the foundational role played by the technological assemblage of large osteological collections aimed at the study of the somatic characteristics of the metropolitan "white" population. I situate these technical developments in the context of wider sociocultural and politico-economic processes of both "nation building" and "empire building." These processes had a strong effect on the kinds of questions asked and the kinds of answers that seemed compelling and acceptable to early physical anthropologists.

This article is about a long-standing tradition of scientific imagination concerned with "the systematic study of human unity-in-diversity" (Stocking 1983:5): the anthropological tradition. I focus on the emergence of a particular field of inquiry within this very broad scholarly tradition, but I analyze this process from the perspective of a peripheral arena of scientific production within the Western European core: the metropolitan academic sphere of the Portuguese Empire during the late nineteenth century. I suggest that this relatively peripheral condition combined with a complex historical conjuncture of internal political and economic crises gave early Portuguese physical anthropology a less explicitly "colonial" orientation than in other, more central Western European imperial powers. This started to change in the 1930s with the rise of a powerful dictatorial regime—Salazar's Estado Novo—that supported the emergence of a "colonial anthropology" strongly oriented, at least until the 1950s, toward the field of physical anthropology.

The development of the discipline of physical anthropology started in Western Europe at the end of the eighteenth century and spread to other parts of the world during the second half of the nineteenth century. This process of discipline building produced a remarkable degree of international consistency, but it also engendered considerable variations, especially before the second half of the twentieth century (Blanckaert 2009; Dias 2005; Stocking 1988; Zimmerman 2001). As the editors of this supplemental issue of *Current Anthropology* note, these disciplinary variations remain poorly studied outside core Western European and North American areas, and this article joins recent calls to rethink the history of anthropology more inclusively (Handler 2000; Kuklick 2008) and to focus on diversity in world anthropological production (Cardoso de Oliveira 2000; Krotz 1997; L'Estoile, Neiburg, and Sigaud 2005; Ribeiro and Escobar 2006).

My contribution to this "world anthropologies" agenda is to bring to the surface a little-known Western European perspective on the origins of modern anthropology and the discipline of physical anthropology. In clear contrast to the American anthropological tradition and its four-field approach, the Portuguese anthropological tradition—as I show elsewhere (Santos 2005)—was built on two different but closely intertwined variants of anthropological research. One was more culturalist—focusing on "people," "language," and "customs"—and the other was more naturalist—focusing on "race," "body," and "fossils." It was from within this naturalist camp that emerged in the late nineteenth century the first studies of "physical anthropology." As in the French context (Jamin 1991; see also Blanckaert 1988, 1995, 2009), this early tradition of physical anthropology was so prominent that it was often labeled with the unmodified term "anthropology" (*antropologia*) and contrasted to its other half, "ethnology" (*etnologia*)—the ancestor of modern social-cultural anthropology and modern archaeology.

My account of this early tradition of physical anthropology focuses mostly on the second half of the nineteenth century, but the events narrated below refer more generally to the

Gonçalo Santos is Senior Research Fellow, Max-Planck-Institut für Ethnologische Forschung (Advokatenweg 36, 06114 Halle/Saale, Germany [santos@eth.mpg.de]). This paper was submitted 27 X 10, accepted 12 VIII 11, and electronically published 21 XII 11.

 0011-3204/2012/53S5-0004$10.00. DOI: 10.1086/662329

period of the "Third Portuguese Empire" (Clarence-Smith 1985), from the first half of the nineteenth century until the third quarter of the twentieth century. This is the period immediately after the French invasions (1808–1814), the liberal revolution (1820–1826), and the independence of Brazil (1822), during which the elites embraced modern nationalist ideologies and started dreaming about consolidating a Third Empire in Africa to counter the country's growing political and economic decline.[1] Failure to implement a successful program of modernization together with the political crisis generated by significant imperial losses in the 1880s and 1890s led to the fall of the bourgeois monarchy in 1910 and two decades later to the rise of a powerful dictatorial regime that eventually realized the earlier ambition to consolidate a Third Empire in Africa.

Portuguese modern anthropology crystallized in the period leading to these two radical political upheavals. The field was initially defined in holistic terms as the study of "the antiquity of man," but there quickly emerged a tendency to give analytical primacy to the naturalist concept of "race." This tendency was reinforced in the 1880s with the emergence of a powerful French-inspired tradition of "physical anthropology" that remained institutionally dominant within the wider field of anthropological production at least until the 1950s. Despite the country's vast imperial possessions, this early tradition of physical anthropology focused mostly on the study of the metropolitan population and only started to engage more effectively with colonial terrains from the 1930s onward.

The approach adopted in this article combines a focus on scientific and institutional developments with a focus on wider politico-economic and sociocultural processes—both domestic and international (cf. L'Estoile, Neiburg, and Sigaud 2005). My main goal is to capture the "style" of Portuguese physical anthropology in the late nineteenth century, and in this article I argue that the key to this issue lies in the technological assemblage of large osteological collections aimed at the study of the somatic characteristics of the metropolitan "white" population.

My usage of the term "style" is similar to that of scholars such as Ian Hacking (2002) and G. E. R. Lloyd (2007, 2009). These scholars maintain that what one usually calls "scientific reason"—that is, a mode of deliberating and bringing in the possibility of truth or falsehood beyond the here and now—is bound to vary depending not just on the scientific discipline being considered but also on the specific historical and geopolitical context being analyzed. Hacking (2002) and Lloyd (2007, 2009) use the term "style" to capture these variations in "scientific reasoning," but the term can also be used—as in this article—to capture transformations within one specific disciplinary field.

Before plunging into an analysis of such disciplinary transformations in late nineteenth-century Portugal, I would like to give a brief account of what happened to the entire field of anthropological production from the early twentieth century onward so as to make more explicit the linkages between my "archaeological exploration" and the contemporary anthropological scene.

After a very short-lived First Republic (1910–1926), the dictatorial regime established in 1933 proved very stable and long-lasting but had a very negative effect in the academic sphere. This authoritarian regime repressed freedom of speech, rejected liberal economic reforms, and set out to build a Third Empire in Africa. Anthropologists did not oppose this enterprise and were called on to produce useful "colonial knowledge." Physical anthropologists—most of whom still espoused a holistic conception of the discipline—played a salient role in this process. By and large, their work offered "scientific" support to the regime's colonial rhetoric, which emphasized the civilizing mission of the Portuguese imperial expansion and opposed racial miscegenation (Pereira 2005; Santos 2005; Thomaz 2005).

This rhetoric started to change in the post–World War II period, and the major intellectual figure behind the new official ideology was the great Brazilian anthropologist Gilberto Freyre, whose work on the formation of Brazilian society praised the allegedly humanistic nature of the Portuguese colonial endeavor and civilizing engagement with miscegenation (Castelo 1999; Vale de Almeida 2002). This new official rhetoric again constrained the work of anthropologists, but it was more in tune with the liberal antiracialist and cultural relativist anthropology that became internationally dominant in the post–World War II period (Vale de Almeida 2002, 2008). Starting in the 1960s, there emerged increasing epistemological and institutional divides between physical-biological and social-cultural anthropologists, and the latter gained the upper hand in colonial affairs (Pereira 2005).

It was, however, only after the democratic revolution of 1974 and the beginning of decolonization that anthropology as a whole started to regain its freedom and to catch up with international trends (Pina-Cabral 1991*a*; Vale de Almeida 2008). The field also started to expand its institutional position in Portuguese academia. A new all-inclusive Portuguese Association of Anthropology was founded in 1989, and several university departments were established in the 1980s and 1990s, mainly under the direction of a new generation of scholars trained abroad. This post-1974 expansion built on earlier divisions and redefined the field in terms of two autonomous disciplines—few departments still have expertise in both social-cultural anthropology and physical-biological anthropology.[2] By and large, the tendency has been toward specialization, and most new departments are departments of

1. The "First Empire" was based in India in the sixteenth century, and the "Second Empire" was based in Brazil in the seventeenth and eighteenth centuries. The "Third Empire" focused on the country's vast African possessions (especially the former colonies of Angola and Mozambique). At the time, there were also a few remaining imperial possessions in Asia (Goa, Macau, East Timor), but these were too small.

2. Archaeology started to evolve as a separate discipline.

social-cultural anthropology, which now occupies a position of institutional dominance within the wider field of anthropological production.

When placed in the context of this new historical conjuncture, my "archaeological excursion" into the origins of physical anthropology seeks to counter a tendency among contemporary historians of Portuguese anthropology to neglect the study of the naturalist tradition and overlook the broader epistemological and institutional foundations of the field of anthropology (see, e.g., Leal 2000, 2006; Pina-Cabral 1991*a*, 1991*b*). This neglect is no doubt linked to the strong historical association of the naturalist tradition with the period of the dictatorship and its most extreme nationalist and racist ideologies, but there is also another, less explicit, reason for this exclusion. Most historians of Portuguese anthropology are sociocultural anthropologists approaching the history of the discipline in light of current disciplinary divisions and from the perspective of their own field of expertise.

In this article I do not adopt a similarly exclusionary approach. Rather, I argue that any grounded account of the origins of physical anthropology should pay attention to the position of this subfield of inquiry in the wider anthropological enterprise that created it. The main advantage of this approach is that it recovers something that remains very much in tune with contemporary concerns despite more than a century of an ever-increasing tendency toward specialization: the holistic nature of the modern anthropological enterprise.

"Antiquity of Man in Portugal": Two Variants of Anthropological Research

There is a very rich premodern tradition of anthropological thought in Portugal whose development goes back to the period of the great maritime explorations of the fifteenth and sixteenth centuries (Pina-Cabral 1991*a*, 1991*b*). This old tradition of chronicles, reports, and memoirs includes an important body of work written by Jesuit missionaries that was disrupted in the second half of the eighteenth century by a series of Enlightenment-inspired reforms that included stimulating the growth of science and spreading anticlerical ideas. These reforms were an important precursor to the great liberal reforms of the 1820s and 1830s, which consolidated the state's control over science and education while promoting the expansion of the public education sector, especially the higher-education sector (then largely dominated by the University of Coimbra, one of Europe's oldest universities), with the creation of polytechnics in Lisbon and Porto (Santos 2005:69–74; see also Mattoso 1993).

It was during this period of increasing engagement with modern liberal and nationalist ideologies—especially from the 1840s onward—that the first major efforts to move beyond the biblical and classical anthropological traditions emerged (Correia 1929; Leal 2000; Pina-Cabral 1991*a*, 1991*b*). This development was influenced by the work of romantic historians, by ideas derived from French positivism, and by the influence of the evolutionist revolution. At a most general level, these early writings were concerned with the evolution of humankind and its various "races" and "peoples," but their main subject of empirical research was the "antiquity of man in Portugal" (*antiguidade do homem em Portugal*). Some studies focused directly on the past (including the prehistoric period), others on archaic survivals in the present. Both focused on the origins and specificities of the Portuguese metropolitan population as a "natural race" and as a "historical nation."

One needs to be careful not to overstate this concern with "things Portuguese" given the country's vast overseas possessions and imperial ambitions (especially in Africa). I fully agree with Omar Ribeiro Thomaz (2005:60) that a rigid classification of early Portuguese anthropology as a "nation-building anthropology" rather than as an "empire-building anthropology"—as proposed by João Leal (2000) in line with George Stocking's original terminology (Stocking 1982*a*)—fails to capture the complex historical linkages between nation and empire (see also Vale de Almeida 2008). It must be noted, however, that despite significant political and intellectual interest in the colonies throughout much of the nineteenth century (Alexandre 2000), the development of a sustained tradition of "colonial anthropology" (*antropologia colonial*) only occurred from the 1930s onward under Salazar's dictatorial regime (Pereira 2005).

In a previous book (Santos 2005), I reflect on this late development, showing how the nineteenth-century consolidation of a significant body of research focusing on the Portuguese metropolitan population was the prerequisite for the emergence of a modern "colonial anthropology" built on clear-cut dichotomies and hierarchies between "colonizer" and "colonized," "civilized" and "primitive," "European" and "non-European," "white" and "nonwhite." I argue that the nineteenth-century privileging of a focus on metropolitan Portugal was linked to the influence of romantic nationalism and above all to the effect of a wider intellectual and political rhetoric of "national regeneration" that flourished in a context of growing political and economic decline—both at home and in the imperial frontier (see also Leal 2000).

Briefly, after the French invasions and the loss of Brazil, the elites were quick to endorse modern nationalist ideologies and call for urgent social and economic modernization (Birmingham 2003:99–130; Mattoso 1993). Interest in anthropology was initially linked to this wider nationalist rhetoric of self-strengthening that was particularly concerned with demonstrating the "unity" and the "antiquity" of the Portuguese "nation"/"race."[3] From very early on, however, many looked at the idea of consolidating a Third Empire in Africa as the only way to restore the country's fortunes, but this colonial project was plagued by ongoing political instability

3. These two terms were often used interchangeably in the nineteenth century.

and structural underdevelopment. This project was further undermined in the 1880s and 1890s by a series of forced imperial losses in Africa that exposed the country's imperial fragilities. These losses triggered a climate of political turmoil and social unrest that assured that the dominant axis of anthropological reflection remained metropolitan Portugal, its regeneration, and the rhetorical defense of its place within European "white" hegemony (Birmingham 2003:131–160; Mattoso 1994).

An important point to make is that most nineteenth-century anthropologists were not trained professionals. They were elite intellectuals with diverse educational and professional backgrounds. The writings of these autodidacts were predominantly eclectic in style, and their vision of anthropology was holistic in scope even though there were a few trends toward disciplinary specialization. We have already mentioned the contrast between those who dealt with the past (including the prehistoric period) and those who studied archaic survivals in the present, but there was still another—perhaps more fundamental—contrast in terms of focus.

On the one hand, there emerged a significant corpus of works in the domains of ethnography and archaeology, among others, that focused on national customs, language, and cultural artifacts. This culturalist tradition has been the object of several studies (Branco 1986; Leal 2000, 2006; Pina-Cabral 1991*a*, 1991*b*). It goes back to the work of romantic writers and historians such as Almeida Garrett (1799–1854) and Alexandre Herculano (1810–1877) in the 1840s and 1850s. This tradition was systematized and consolidated in the second half of the nineteenth century by scholars such as Teófilo Braga (1843–1924), Adolfo Coelho (1847–1919), Martins Sarmento (1833–1899), and Leite de Vasconcelos (1858–1951)—mostly historians and lawyers. Its most famous exponent during the period of Salazar's dictatorship was the social-cultural anthropologist Jorge Dias (1907–1973), but one should bear in mind that even then there was a considerable diversity of approaches.

In parallel to this line of research, there was a significant corpus of works in the domains of paleontology, comparative anatomy, and anthropometry, among others, that focused on fossils and human bones. This naturalist tradition emerged somewhat later—in the 1860s and 1870s—with the work of Carlos Ribeiro (1813–1882) and his Comissão Geológica (Geological Commission). However, it was responsible for the creation of the first chair of anthropology at the University of Coimbra in 1885, and it occupied a position of institutional dominance within the wider field of anthropological production at least until the 1950s (Pereira 2005; Santos 2005; Xavier da Cunha 1982). This naturalist tradition was systematized and consolidated in the last quarter of the nineteenth century by scholars such as Ferraz de Macedo (1845–1907), Bernardino Machado (1851–1944), and Fonseca Cardoso (1865–1912)—mostly physicians and engineers. Its major exponents during Salazar's dictatorship were Eusébio Tamagnini (1880–1972) at the University of Coimbra and Mendes Correia (1888–1960) at the University of Porto.

Despite being very different from each other—the naturalists (or the *antropólogos*) drawing mostly on the concept of race, and the culturalists (or the *etnólogos*) drawing on the concepts of language, culture, and society—these two lines of anthropological inquiry were initially integrated in a broader holistic domain of research that gave primacy to the concept of race. In this early period, the two shared a common theoretical paradigm—that is, evolutionism—and with it, a shared belief in the hereditary or cumulative environmental, physical, and cultural inferiority of non-European populations. They also shared a common emphasis on the important role of museums in preserving and exhibiting the material remains (e.g., artifacts, bones, fossils) of historical and evolutionary processes. The coming into being of these two lines of inquiry during the second half of the nineteenth century lies at the heart of the process of autonomization of the modern field of anthropology in Portugal.

The Comissão Geológica and the Big Bang of 1880

The research activities of the Comissão Geológica played an important role in the genesis of the naturalist anthropological tradition, the ancestor of modern physical anthropology. The Comissão Geológica was created in 1848 by the Royal Academy of Sciences of Lisbon, and in 1857 it was placed under the direction of Carlos Ribeiro (1813–1882), a charismatic army officer and civil engineer whose ultimate scientific ambition was to develop a "natural history of the Portuguese" capable of going beyond the efforts of early romantic historians such as Alexandre Herculano. This ambitious project aimed at bringing together the domains of geology, paleontology, mineralogy, and zoology with what was then called "prehistoric anthropology and archaeology" (Correia 1929; Xavier da Cunha 1982).

Starting from the 1860s, Ribeiro and his collaborators undertook a series of field-research activities leading to remarkable discoveries of prehistoric human activity. In the early 1860s, excavations at Cabeço da Arruda, a small elevation in the valley of the Muge rivulet, a tributary of the lower Tagus River, northeast of Lisbon, lead to the discovery of various shell midden sites dating from the Mesolithic (10,000–12,000 years ago) that contained a number of human remains. These sites inspired the publication in 1865 of a groundbreaking study by F. A. Pereira da Costa (1809–1889) that—according to Xavier da Cunha (1982:8)—constitutes the first Portuguese monograph of "prehistoric anthropology and archaeology" (see Costa 1865).

More or less at the same time, Ribeiro coordinated a series of excavations in the lower basin of the Tagus River, northeast of Lisbon, only to discover flakes of flint and quartzite showing signs of human work. The geology of the region indicated that the limestone beds excavated were of Tertiary age, but the prevalent idea at the time was that humans were not older than the Quaternary (the most recent geological age, from about 2 million years ago to the present). For this reason, Ribeiro (1866, 1867) initially assigned Quaternary ages to the implement-bearing strata, but he eventually revised his position, claiming that there were already humans living in Portugal during the Tertiary (Ribeiro 1871).

In 1872, Ribeiro (1873*a*, 1873*b*, see also 1884) presented his impressive collection of Miocene flint and quartzite implements at the International Congress of Prehistoric Anthropology and Archaeology in Brussels, but his claims remained controversial, even after being endorsed in 1878 by the influential French anthropologist Gabriel de Mortillet (1831–1898). In *Le pré-historique* (Mortillet 1883:97–106), Mortillet refers to Ribeiro's "Tertiary humanoids" as a subspecies called *Anthropopithecus Ribeiroii*.

Ribeiro's findings did not gather consensus—and they still do not—but they attracted the attention of the international community. In 1880, Ribeiro and his collaborators hosted the Ninth International Congress of Prehistoric Anthropology and Archaeology in Lisbon—a decisive event that confirmed the popularity of this field of research in Portugal. The congress also had a strong influence in the political sphere, prompting government officials to circulate administrative directives (also in the overseas colonies) that highlighted the relevance of "scientific" endeavors such as collecting human remains and artifacts. The wider intellectual sphere was also charmed by this new scientific enterprise. For example, Oliveira Martins (1845–1894), a well-known member of a leading group of avant-garde intellectuals known as the "70s Generation" group,[4] published in the very year of the congress an important treatise of evolutionist anthropology and an influential monograph on the "colonial question" (Martins 1880*a*, 1880*b*).

Martins's writings, together with the acts of the congress published a few years later (CIAAP 1884), attest the vitality of the field of "prehistoric anthropology and archaeology" (see also Santos 2007). Although the field displayed a significant diversity of themes and approaches, most empirical research activities focused on metropolitan Portugal. In addition to the geological investigations mentioned above, multiple archaeological explorations were undertaken during this period (both before and after the congress), some of them leading to important discoveries. For example, starting in 1873, research teams led and financed by the lawyer Martins Sarmento (1833–1899)—the founder of Portuguese modern archaeology—unveiled the famous Castro city of Briteiros as well as other prehistoric and protohistoric stations in the Minho region and in other provinces of the country (Leal 2000, chap. 2; Martins 2008).[5] This booming scientific activity was part of a broader phenomenon of multiplication of research formations and museological sections—a kind of intellectual "big bang"—that consolidated the field of anthropological production, extending it well beyond the realm of the "prehistoric."

In Lisbon, the Sociedade de Geografia (Geographic Society) was founded in 1875, and its unit of "anthropology and archaeology" was created a few years later. In 1879, a similar unit was created at the influential Sociedade de Ciências Médicas (Society of Medical Sciences); and in 1893–1894, the new Museu Etnológico (Ethnological Museum)—today's National Museum of Archaeology—started to host the large collection of artifacts, fossils, and human remains assembled by the museum of anthropology and archaeology of Ribeiro's Comissão Geológica.

In Porto, 6 years after the death of Ribeiro in 1882, a group of young intellectuals—including Ricardo Severo (1869–1940), Fonseca Cardoso (1865–1912), and Rocha Peixoto (1868–1909)—created the Sociedade Carlos Ribeiro (Carlos Ribeiro Society). This society sought to promote the growth of the natural and social sciences, and its official publications included some of the most prestigious scientific journals of the time.[6] Following the spirit of Ribeiro's Comissão Geológica, the society was divided into four main sections: (1) Geology and Paleontology, (2) Zoology and Botany, (3) Anthropology, and (4) Ethnology. This arrangement reflected Ribeiro's own categorization of the anthropological enterprise as a holistic field of research within the realm of "natural history," a holistic field of research that included, on the naturalist side, a very broad domain of *antropologia*, or "physical anthropology," and on the culturalist side, a very broad domain of *etnologia*, or "social-cultural anthropology" (Correia 1940:623–627).

It was, however, in Coimbra, not in Porto, that this holistic field of research was first institutionalized in the higher-education sector, and here, too, Ribeiro's naturalist orientation prevailed. There was since 1772 a small unit of "prehistoric anthropology and archaeology" at the University of Coimbra's Museum of Natural History, but its activities were limited. Wider interest in this field only started to flourish in the 1850s and 1860s with the growing popularity of evolutionist thought within the intellectual circles of the Faculty of Natural Philosophy (today's Faculty of Sciences) and the Institute of Coimbra (a highly influential cultural society founded in 1852). But it was only in 1885 that a chair of

4. The "70s Generation" group included the likes of Antero de Quental (1842–1891), Teófilo Braga (1843–1921), and Adolfo Coelho (1847–1919). This group was a late nineteenth-century representative of a broader intellectual movement of "national regeneration" that emerged after the liberal wars of the 1820s and that called for the replacement of the "old" clerical and aristocratic order with a more progressive bourgeois culture capable of embracing nationalist ideas and supporting the implementation of a national program of modernization and industrialization.

5. These discoveries played an important role in major debates going on in the public sphere over the origins of the alleged Indo-European ancestors of the Portuguese—the Lusitanians.

6. For example, *Revista de Ciências Naturais e Sociais* (1890–1898) and *Portugália* (1899–1908).

anthropology was officially created at the Faculty of Natural Philosophy. The leading force behind this process of institutionalization was a new field of study largely consolidated in France during the 1860s and 1870s. Its first representatives in Portugal emerged in the early 1880s.

Macedo's *Tábuas Osteométricas* and the Birth of Physical Anthropology

The coming into being of this new field of study—today's "old" physical anthropology—was linked to a series of transformations in the system of raciological representation that followed from the introduction of new statistical notations and the use of anthropometric and craniological measurements under the influence of American anthropologist Samuel Morton (1799–1851) and the Swedish anthropologist Anders Retzius (1796–1860), the inventor of the cephalic index and of the concepts of dolicocephaly and brachycephaly. The French anthropologist Paul Broca (1824–1880) played a major role in bringing together these various developments.

In Portugal, the first major figure behind this disciplinary transformation was a physician, Ferraz de Macedo (1845–1907), whose work was profoundly influenced by Broca's "anthropological revolution" (Correia 1929; Tamagnini and Serra 1942; Xavier da Cunha 1982). Macedo was an innovator. Together with scholars such as Arruda Furtado (1854–1887) and Eduardo Burnay (1853–1924), he initiated a domestic process of "scientific renovation" that entailed (1) the dissemination of new theories of evolution, (2) the introduction of new techniques of observation and measurement of both skeletal specimens and living subjects and of new methods of statistical analysis, and (3) the organization of large osteological collections. I draw particular attention to Macedo's efforts in assembling large osteological collections focusing on the metropolitan population because these efforts proved crucial for later developments.

Born in Águeda (northern Portugal) in 1845, Macedo was one of many early anthropologists with strong Brazilian connections. Macedo moved with his parents to Rio de Janeiro at a young age (7–8 years old), graduated in pharmacy in 1867 at the University of Rio de Janeiro (today's Federal University of Rio de Janeiro), and was awarded a PhD degree by its Faculty of Medicine a few years later, soon after the premature death of his parents.[7] In 1874, he embarked on a 3-year trip to Europe (Macedo 1877) and attended Broca's School of Anthropology in Paris with the likes of Paul Topinard (1830–1911), Armand de Quatrefages (1810–1892), and Léonce Manouvrier (1850–1927), among others.

These were exciting times for physical anthropologists—the heyday of evolutionist thinking—and Portugal was not isolated from these international developments. In 1880, the year of Ribeiro's international congress, Macedo had already moved to Lisbon to devote himself to the practice of physical anthropology as expounded by his Parisian mentors Broca and Topinard—that is, the comparative study of the somatic characters of human populations. At the time, as shown in this supplement issue of *Current Anthropology*, anthropologists in different parts of the world were investing heavily in the organization of osteological collections, and Macedo was no exception.

Macedo was already in possession of a collection of about 100 incomplete skeletons belonging to members of various indigenous populations in Brazil, but he was unsatisfied with the "quality" and "size" of this material. Between 1878 and 1882, Macedo managed to convince the Lisbon city hall to allow him to collect a large number of identified human skeletons from two local cemeteries (Cardoso 2006*a*, 2006*b*; Rocha 1995). At the time, most urban cemeteries in Portugal were already administrated by city halls (not by the church), and the common practice was to bury individuals in temporary graves that had to be exhumed a few years later, once complete skeletonization occurred, so that the grave could be reused. The remains of the deceased were then placed in small urns inside block compartments called *ossários* for a periodic fee. However, if no one claimed the remains—or else did not pay the fee for the *ossários*—the cemeteries issued an order of removal, and after a few years, the bones were simply reburied in a communal grave (Cardoso 2006*a*). Alternatively, so Macedo argued, the bones could be given to anthropologists for the purpose of scientific research.

Macedo's efforts to assemble, document, and curate these skeletal materials resulted in the first identified osteological collection in Portugal—a large collection of 1,200 identified crania and 200 documented skeletons of mostly "Caucasian" individuals born in continental Portugal. This undertaking was very polemical and involved a significant material investment on Macedo's part (Ferreira 1908). Shortly before his death in 1907, Macedo donated his collection to the Bocage Museum (today's Natural History Museum of the University of Lisbon). The collection was destroyed in a fire that damaged the Museum in 1978, but it is still remembered among present-day physical anthropologists for having established a tradition of large osteological collections focusing on the metropolitan population (Cardoso 2006*a*, 2006*b*; Xavier da Cunha 1982).

Macedo's innovations were not just linked to this osteological collection and what it allowed in terms of developing new interpretative skills. His innovations were also linked to the introduction of new techniques of observation, measurement, and statistical analysis in line with the new French tradition of physical anthropology. As early as 1882, Macedo started producing a series of *tábuas osteométricas* (osteometric tables) based on his Lisbon collection that challenged the

7. His PhD thesis focused on prostitution as a social problem, announcing his future interest in the fields of criminal anthropology and pathological anthropology. I believe that this thesis was the first thesis of the Faculty of Medicine of the University of Rio de Janeiro to refer openly to the phenomenon of male homosexuality, linking it to the wider phenomenon of prostitution and corruption of minors in colleges and boarding schools.

tendency to use small samples. These tables contained important data on various linear measurements and indexes drawn from skeletal material that became the first authoritative figures concerning the metropolitan "white" population. Many of these tables were never published; others were included in later works (Macedo 1889, 1892). One of these tables—measuring the degree of complication of the sagittal sutures of 1,000 crania of so-called Caucasian individuals—was published in Bertillon's famous *Dictionary of Anthropological Sciences* (Bertillon et al. 1883; Macedo 1883).

Macedo was favorable to some form of polygenism—quite popular at the time—that incorporated both Darwinian and Lamarckian understandings of human evolution (see Pereira 2001 on the influence of Darwin in Portugal). Writing in 1886, he argued that humans appeared in different parts of the world in the Tertiary or else the Quaternary and evolved in isolated groups or "autochthonous types" with significant morphological variations, some caused by heredity, others by the cumulative effect of the environment (Macedo 1886:65–67). Following Broca, he believed that the comparative study of human crania was an important methodology to capture these morphological variations, but he was also aware of some of the limitations of this comparative enterprise (Macedo 1886:68–69).

Toward the late 1880s, he started to look for a deeper link between behavior, identity, and body morphology, and he became more and more interested in the growing field of criminal anthropology.[8] At the time, he frequently attended the amphitheater of the Medical School of Lisbon (today's Faculty of Medicine of the University of Lisbon) to learn more about brain morphology, and he often visited police headquarters and prisons to collect psychological and anthropometric data on criminals. From very early on, he became convinced that crime had an innate character that resided not in the morphology of the cranial bones but in the morphology of the encephalon (Macedo 1886:92; see also Macedo 1889, 1892). He spent much of his later career—from the late 1890s onward—popularizing the study of criminal anthropology and working for the State Bureau of Criminal Investigation.[9]

Stephen Jay Gould (1996 [1981]) famously criticized these early efforts in physical anthropology and criminal anthropology (see also Kevles 1995 [1985]). It is outside the scope of this article to repeat Gould's or my own arguments against the work of early anthropologists such as Macedo (Santos 2005:91–97). Instead, I would like to use Macedo's work to draw attention to the growing popularity in late nineteenth-century Portugal of a theory of human evolution largely derived from French positivist philosophy and from various non-Darwinian ideas on racial degeneration and the inheritance of acquired characteristics. According to this theory, the progress and longevity of any human "race"/"nation" cannot be ensured if the conditions that contribute to its degeneration are not eliminated. In contrast to scholars such as Teófilo Braga, who preferred to celebrate the "greatness" of the Portuguese "race"/"nation," Macedo acknowledged the possibility of degeneration and tried to identify its natural and social causes; hence, his interest in criminal anthropology.

Macedo never worked on "miscegenation" as a potential source of degeneration, but his work on the somatic characteristics of the Portuguese metropolitan population provided the next generation of physical anthropologists with the necessary tools to challenge racial theories based on the idea of a master Nordic race that ascribed the Portuguese "race"/"nation" an inferior status within the "Caucasian" races and that looked at the Portuguese engagement with miscegenation in the imperial frontier as a source of "racial degeneration."

Macedo was not very successful in the work of institutionalization of his vision of anthropology in the higher-education sector. However, his new style of research inspired the foundation of the first chair of anthropology in Portugal.

The School of Coimbra and the Rise of *Antropologia Portuguesa*

Bernardino Machado (1851–1944)—a wealthy member of Portuguese freemasonry and a highly influential figure in the republican movement[10]—was the major force behind the establishment in 1885 of a chair of anthropology at the University of Coimbra. Like Macedo, Machado had strong Brazilian links: he was born in Rio de Janeiro (the son of a rich Portuguese merchant raised to the nobility) and only moved to Portugal with his parents in 1860 at the age of eight or nine.

Machado obtained a doctorate in natural philosophy at the University of Coimbra in 1876 (4 years after opting for Portuguese nationality) and started teaching at the Faculty of Natural Philosophy in the following year, becoming a permanent faculty member in 1879. However, a mere 3 years after this appointment, Machado joined the Regeneration Party (the major modernizing force of the constitutional monarchy) and started becoming actively engaged in national politics without fully withdrawing from his academic activities.[11]

8. The growing popularity of the field of criminal anthropology in Europe—especially among French and Italian anthropologists—can be illustrated by the fact that seven international conferences were held under that designation in the period between 1885 and 1911.

9. For example, he was one of the main scientific consultants for a famous seven-volume publication titled *Gallery of Famous Criminals in Portugal*, published in Lisbon between 1896 and 1908.

10. Bernardino Machado was the third and the eighth president of Portugal (1915–1917, 1925–1926) during the First Republic. He also played a very active role in Portuguese freemasonry—within the Grande Oriente Lusitano Unido—reaching important positions such as Presidente do Conselho da Ordem (1892–1895), Grão-Mestre (1895–1899), and Soberano Grande Comendador do Supremo Conselho do Grau 33 (1895–1899 and 1929–1944; see GEPB 1960 for more details).

11. At the time, the Regeneration Party was the main political driving force of modernization alternating in power with the Historical Party, which adopted a similar modernizing vision but aspired to slightly less technocratic and more democratic ideals.

In 1883, Machado was appointed director of the section of "prehistoric anthropology and archaeology" of the Museum of Natural History, and his proposal to create a chair of "Anthropology, Human Paleontology and Prehistoric Archaeology" at the Faculty of Natural Philosophy was officially approved in 1885,[12] granting him the power to organize a new museum/laboratory of anthropology (Areia, Rocha, and Miranda 1991:89). Because of his strong political commitments, Machado often relied on substitute teachers, at least until 1894, when he returned more fully to his academic duties (Areia, Rocha, and Miranda 1991:89). That same year, he was elected president of the influential Institute of Coimbra, and in 1896, he founded the first society of anthropology in Portugal—37 years after its French counterpart, 33 after the English, and 27 after the Germans (Areia and Rocha 1985:33).[13] This society was very short-lived because of Machado's growing political engagements, which eventually led him to resign from his university post in 1907 soon after joining the newly founded Republican Party, a key player in the declaration of the Portuguese Republic 3 years later.

Despite having privileged the political over the academic, Machado strongly shaped the institutional and epistemological foundations of anthropology in Coimbra. Most crucially, he followed the likes of Ribeiro and Macedo in placing the chair of anthropology in the realm of natural history. This link to natural history was first established with the creation of the Museum of Natural History in 1772, but Machado's chair of anthropology was much more than a simple replication of an already-existing arrangement. It was the culmination of a longer process of institutional/epistemological differentiation of the realm of natural history in several separate laboratories/fields of knowledge, starting with botany in the late eighteenth century, zoology and mineralogy/geology in 1836, and finally anthropology in 1885. This link to natural history was reinforced by the university reforms of 1901 that placed anthropology (together with botany, zoology, and geology) in the natural history sciences section of the Faculty of Natural Philosophy (Areia and Rocha 1985:16).

The course syllabi taught at Machado's new museum/laboratory confirm this definition of anthropology as "the study of the natural history of man." For example, the syllabus of the academic year 1889–1890 consisted of two main domains of study: "anthropology" and "prehistoric archaeology" (Areia and Rocha 1985:28–32). The domain of "anthropology" was vast, including "zoological anthropology" (mostly paleontology and comparative anatomy) and "general anthropology" (the study of human races and their anatomic, anthropometric, physiological/psychological traits). "Prehistoric archaeology," by contrast, focused on the technological progress and evolution of human societies since the Stone Age.

In the mid-1890s, Machado's course syllabi started to refer to a "general anthropology" subdivided in two major domains: "physical anthropology" or "anthropology," and "social-cultural anthropology" or "ethnology" (Tamagnini and Serra 1942:6–7). "Anthropology" included "primatology," "human paleontology," and "comparative raciology," among others; and the field of "ethnology" included "prehistoric archaeology" (or the study of human artifacts and industries in prehistoric times), "comparative linguistics," and "ethnography," among others. In short, this was a holistic program of research that included, on the naturalist side (the side of the *antropólogos*), a very broad field of "physical anthropology," and on the culturalist side (the side of the *etnólogos*), a very broad field of "social-cultural anthropology."

It is quite tempting to make parallels here with the "general anthropology" of Paul Broca and his School of Anthropology. Like Broca (Jamin 1991:291; see also Blanckaert 2009), Machado maintained that the study of the cultural and social aspects of human diversity should complement the study of its physical and natural aspects. He also believed, like Broca or Macedo, that social and cultural diversity (including the various sociological and technological stages of human evolution) were ultimately the product of physical and natural differences in racial structure.

This link to Broca's anthropological tradition and its privileging of the "physical" is confirmed by the handbooks used by Machado and his assistants during the late 1880s and the 1890s, mostly books written by prominent French physical anthropologists (Areia and Rocha 1985:15).[14] This orientation toward French physical anthropology is again confirmed by the popularity of themes such as osteometry and craniometry in the students' final dissertation projects (Areia, Rocha, and Miranda 1991:91–92).[15] This predominant focus on the "physical" meant that the domains of the "social" and the "cultural" were significantly neglected by both students and researchers.

This naturalist conception of anthropology shaped Machado's practical efforts to modernize and expand the technical resources of his new laboratory/museum. From very early on, he invested significant energy in the acquisition of instruments of observation and measurement associated with the "new" physical anthropology pioneered by Macedo in Lisbon. As early as 1889, he acquired directly from Raoul Mathieu—a famous Parisian instrument maker who worked closely with Broca and Topinard—a large set of anthropo-

12. Machado was also responsible for the 1893 decree promulgating the creation of the Museu Etnológico de Lisboa (Ethnological Museum of Lisbon), directed by Leite de Vasconcelos.

13. The main goal of this Sociedade de Anthropologia, according to its statutes, was "to develop the study of anthropology in Portugal" (Areia and Rocha 1985:33 [my translation]).

14. For example, the school year of 1887/1888 included Broca's *Instructions cranéologiques et craneométriques* (1875), Topinard's *Manuel d'anthropologie* (1876), and Mortillet's *Le pré-historique* (1883; AUC 1888: 173).

15. Of course, there were also dissertation projects focusing on topics well outside the concerns of "classic" French physical anthropology—including human ecology, sociology, and ethnography—but these remained less salient.

logical instruments (many of which were designed by Broca) that included craniophores, craniostats, stereographs, Antelme's cephalometers, occipital goniometers, median facial goniometers, dynamometers, anthropometers, and others (Rocha 1995:34). In that same year, a collection of anatomical models of human skulls and various nonhuman primates was also acquired (Areia, Rocha, and Miranda 1991:91).

Machado also invested significant energy in the conservation and development of his laboratory's museum collections. His efforts to enlarge the centuries-old ethnographic collection of his laboratory were very impressive and should not be underestimated (Gouveia 1978; Martins 1985), but I focus only on his endeavors to assemble human skeletal material and to constitute large osteological collections relating to the metropolitan population because this effort proved crucial for later research developments and because Machado himself considered it a priority.

As early as 1883, soon after being appointed director of the prehistoric anthropology and archaeology section of the Museum of Natural History, Machado took interest in a collection of human skeletal material (35 crania without mandibles) recently arrived from Macau but allegedly from indigenous populations of the former Portuguese colony of East Timor (Areia, Rocha, and Miranda 1991:91; Fernandes 1985:80; Rocha 1995:9–10).[16] Some of his students, including Barros e Cunha (1894), studied this collection, but its "authenticity" remains an open question at best (Roque 2007).

A few years later, already director of the new laboratory of anthropology, Machado acquired ancient skeletal material found near the baroque church of São João de Almedina in Coimbra (built in the late seventeenth and early eighteenth centuries) and at old Roman cemeteries near Cascais. These two sets of unidentified skeletal material are today known as the Collection of São João de Almedina and the Collection of the Old Cemeteries of Cascais (Fernandes 1985:80–81).

In addition to assembling these two historical osteological collections, Machado organized a large fully documented osteological collection relating to the contemporary period (Fernandes 1985:78–79; Rocha 1995:12–14). Following the footsteps of Macedo in Lisbon, Machado believed that modern osteometric research could not be based on small poorly identified samples, and between 1896 and 1903, he acquired a vast amount of identified skeletal material from the medical schools of Lisbon and Porto and from the anatomic museum of the University of Coimbra. This osteological collection—today known as the Medical School Collection (or Collection 02)—includes skeletal material (crania and mandibles) from "Caucasian" individuals of both sexes (366 male, 219 female) born in all major municipalities of continental Portugal. This effort to organize large documented osteological collections reflecting the metropolitan "white" population was continued by later generations of physical anthropologists.

A major case in point is Machado's own successor in Coimbra, the biological anthropologist Eusébio Tamagnini (1880–1972),[17] a former student of Machado who assembled two large documented osteological collections between 1907 and 1950 (Fernandes 1985:78–79; Rocha 1995:15–20). One of these collections—the Identified Skeleton Collection—includes 425 complete skeletons of individuals born in continental Portugal; the other—the International Exchange Collection—comprises skeletal material (mostly crania and mandibles) from 1,075 individuals born in continental Portugal.

These osteological collections were the key technological platform for the institutionalized development—both in terms of research and teaching—of a naturalist anthropological tradition focusing on the metropolitan "white" population.[18] This so-called *antropologia portuguesa* (Portuguese anthropology) was consolidated during the first three decades of the twentieth century, and Machado's School of Coimbra—under the leadership of Tamagnini after 1907—played a major role in this process, publishing from 1914 onward the influential journal *Contribuições para o Estudo da Antropologia Portuguesa* (Contributions for the Study of Portuguese Anthropology), renamed *Antropologia Portuguesa* in 1983. By then, the term *antropologia portuguesa* simply meant "anthropology in Portugal," but originally, the term referred to research focusing on the Portuguese "race"/"nation," especially research in physical anthropology (Santos 1996, 2005).

This focus on the metropolitan "white" population—which included a strong concern with criminal anthropology (Correia 1940; Rocha 1985)—remained institutionally dominant

16. Of the 35 crania received, only 29 are still kept by the present-day anthropology department of the University of Coimbra. Bernardino Machado loaned six skulls in 1902 to the physical anthropologist Rudolf Martin in Berlin. In 1913, Machado's successor, Eusébio Tamagnini, asked for the devolution of this skeletal material but had little success (Rocha 1995:33).

17. The biologist Eusébio Tamagnini was the leading figure of the Anthropological School of Coimbra between 1907 and 1950. Tamagnini studied anthropology under Machado and became his successor at the Museum and Laboratory of Anthropology of the University of Coimbra in 1907. He was the director of this institution—renamed the Institute of Anthropology in 1915—until his retirement in 1950. Tamagnini was one of the leading anthropologists of his generation. He played an important role in the organization of several major congresses of anthropology, and he was one of the founders—and the first president—of the Portuguese Society for Eugenic Studies established in 1937. His work focused mostly on questions of physical anthropology applied to the study of the Portuguese metropolitan population (including topics such as cephalic index, nasal index, femur size, eye color, hair color, or blood groups). From the 1930s onward, he became increasingly interested in the relations between anthropology and biology, and on the political front, he developed close ties with the dictatorial regime of Salazar. Between 1934 and 1936, he was Salazar's Minister of Education, and his ideas on racial hygiene remained influential at least until the 1940s (see GEPB 1960 and Santos 1996 for more details).

18. This was the naturalist version of the anthropological tradition—described by João Leal (2000)—that focused on the customs, beliefs, and traditions of the Portuguese people. This so-called *etnologia portuguesa* (Portuguese ethnology), or *etnografia portuguesa* (Portuguese ethnography), is usually associated with the work of scholars such as Teófilo Braga or Leite de Vasconcelos.

among physical anthropologists at least until the 1920s and 1930s both in Coimbra and in the new museum and laboratory of anthropology founded in 1911–1912 at the Faculty of Natural Sciences of the University of Porto under the leadership of the biological anthropologist Mendes Correia (1888–1960).[19] This *antropologia portuguesa* was initially based on the study of skeletal material (both historical and contemporary), but the standardization of more rigorous methods of measuring bodily characteristics (skin color, eye color, hair color) during the first two decades of the twentieth century allowed it to rely increasingly on the study of living subjects. It was in opposition to this early *antropologia portuguesa* that a naturalist anthropological tradition started to emerge in the 1930s—already under Salazar—focusing on the colonies and their "indigenous" populations (Correia 1944; Pereira 2005). Mendes Correia and the Porto School played a particularly important role in this development, but the history of this *antropologia colonial* (colonial anthropology) falls outside the scope of this article.[20]

One of the major motivations behind the *antropologia portuguesa* of the late nineteenth century and early twentieth century was the desire to demonstrate the "racial unity" of the Portuguese people as well as its "antiquity." But this anthropological work of nation building also defined the Portuguese "race"/"nation" in opposition to colonial subalterns contributing to the reproduction of older imperial racial hierarchies. This point emerges quite clearly in discussions around the topic of *mestiçagem* (miscegenation), increasingly popular in the first two decades of the twentieth century. At the time, the idea that the Portuguese engagement with *mestiçagem* in the colonies resulted in "racial degeneration" was gaining currency in the international circuit, and Portuguese anthropologists felt the need to dismiss this idea. What is interesting, however, is that this dismissal did not entail a rejection of the degenerative effects of *mestiçagem*, as in the Brazilian case with the work of Roquette Pinto and others (Santos 2012), but focused instead on showing that the practice of *mestiçagem* did not affect Portuguese racial quality. This is—in short—what the early *antropologia portuguesa* was also all about: an effort to deny "racial degeneration" and to reaffirm the "natural" place of Portugal within European "white" hegemony.

A late quote from Machado's successor in Coimbra, Eusébio Tamagnini, one of the founders of the Portuguese Society of Eugenic Studies established in 1937, illustrates this last point in a particularly clear manner. The quote is taken from the opening pages of a research article (published as late as 1944) whose main goal was to show that the average nasal index of the Portuguese metropolitan population fitted the standards of variation of other European "white" races.

> It would be foolish to pretend denying the existence of *mestiçagem* between the Portuguese and the elements of the so-called colored races. The fact that they are a colonizing people makes it impossible to avoid ethnic contamination. What one can not accept is the raising of such *mestiçagem* to the category of a sufficient factor of ethnic degeneration to such a point that anthropologists would have to place the Portuguese outside the white races or classify them as negroid *mestiços*. (Tamagnini 1944:7 [my translation])

A Subaltern Naturalist Empire of Knowledge in the Making

What we see emerging institutionally in Coimbra in the late nineteenth century is a Western European peripheral derivation of a much larger transnational naturalist empire of knowledge built around the work of Paul Broca and the French anthropological tradition (see Blanckaert 1988, 1995, 2009 for an overview). There is, however, a major difference between early Portuguese and French "anthropologies": the scale of comparison. If the physical anthropology of Broca was an effort of classification and comparison of human populations aimed at the theoretical synthesis of a "natural history of man" at a global scale, the line of research that starts to be institutionalized in Coimbra in the late nineteenth century—and then also in Porto from 1911 to 1912 onward—is an effort of classification and comparison of human populations aimed primarily at the theoretical synthesis of a "natural history of the Portuguese." Both "anthropologies" required the technological support of osteological collections, but they had very different comparative ambitions.

Of course, the research activities of late nineteenth-century and early twentieth-century Portuguese physical anthropologists did not just focus on the metropolitan "white" population. As early as the 1880s and the 1890s, the practice of

19. The physician-biologist Mendes Correia was the leading figure of the Anthropological School of Porto during the first half of the twentieth century. The School of Porto, like the School of Coimbra, favored research in *antropologia* (anthropology) over research in *etnologia* (ethnology). Its development revolved around two key institutions: the Museum and Laboratory of the University of Porto (founded in 1912 and renamed the Institute of Anthropology in 1923) and the Portuguese Society of Anthropology and Ethnology (founded in 1918). Correia was the leading figure of both institutions until his retirement in the 1950s, and he was one of the leading anthropologists of his generation. His early work focused on questions of criminal anthropology and physical anthropology applied to the study of the Portuguese metropolitan population. From the 1930s onward, he became increasingly interested in "colonial anthropology" and played a key role in the development of this field, both institutionally and scientifically. He was also a successful politician. He was mayor of the city of Porto between 1936 and 1942 and was a member of parliament between 1945 and 1956 (see GEPB 1960 for more details).

20. An important turning point in this direction was the First National Congress of Colonial Anthropology organized in Porto in 1934 (1 year after the rise of Salazar's Estado Novo, which sponsored the event). This congress was organized by Mendes Correia, the leading figure of anthropology at the University of Porto, with the explicit purpose of countering the lack of scientific interest in the colonies. The congress effectively resulted in the launching of a nationwide program of anthropological research focusing on the colonies and their "indigenous" populations, but this program was quickly placed under the control of Salazar's regime (Pereira 2005:213).

collecting human skulls in the overseas colonies for museums back in the metropolis was not uncommon, and there were a few efforts to analyze osteological materials from "indigenous" populations in the colonies (Roque 2001, 2009; Santos 2005; Schouten 2001). These efforts reflected a much wider intellectual interest in the imperial frontier. From the second quarter of the nineteenth century, many looked at the idea of consolidating a Third Empire in Africa as the only viable alternative to being politically swamped again by Spain or to being economically dominated by Britain, but two structural factors—political instability and economic underdevelopment—delayed the realization of this project. The world economic recession of 1870 convinced the upper classes to revive the idea of creating a Third Empire in Africa, but again, this project failed to take off.

Even after the spread of the use of quinine as a prophylaxis in the 1860s and 1870s (that made living in Africa less hazardous), the lower and middle classes were not very excited about the idea of migrating to Africa. Moreover, the dream of a transcontinental empire in Africa was considerably damaged in the 1880s after a series of imperial losses to Belgium and Britain that exposed the country's increasingly peripheral position in the international world order. These losses were depicted as a "national humiliation" and triggered a climate of political turmoil and social unrest that characterized the last two decades of the bourgeois monarchy (1890–1910) and the very short-lived First Republic (1910–1926). Hence, just as the dream of a Third Empire in Africa had to be postponed, so the "colonial terrain" of physical anthropologists had to be postponed, and their dominant concern remained the metropolitan population and its place within European "white" supremacy. Despite initial efforts by scholars such as Barros e Cunha (1894)—whose study of Machado's collection of East Timorese human skeletal material I already mentioned—and Fonseca Cardoso (1897)—whose study of the Ranes of Satari (northeastern Goa) is often considered a landmark in the field (Roque 2001)—the anthropological study of the colonies and their "indigenous" populations was only consolidated in the 1930s with the rise of Salazar's dictatorial regime and the effective implementation of colonialism as a project of modernization (Correia 1940; Tamagnini and Serra 1942; see also Pereira 2005; Santos 2005).

There is still another major difference between early Portuguese and French "anthropologies": the degree of heterodoxy. Both styles of physical anthropology compared measurements of the physical aspects of different human populations, and these comparisons usually resulted in highly determinist conclusions that were quite often informed by racist evolutionary assumptions (for a critique of these assumptions in the French context, see Gould 1996 [1981]; Harrington 1989; Young 1990; for a critique of these assumptions in the Portuguese context, see Santos 1996, 2005).

In France, these racist evolutionary assumptions started to be openly contested in the late nineteenth century by physical anthropologists themselves. Léonce Manouvrier, for example, influenced by Franz Boas, argued that both the environment and nutrition had a strong influence on the physical characteristics of individuals (Hecht 2003). Similar challenges were also developed in the same period by a new generation of scholars—gathered around the figure of Emile Durkheim—who wanted to develop a new social-cultural anthropology that was no longer a servant of physical anthropology but was instead based on a new discipline—sociology—defined as "a natural science of the social" (Blanckaert 1995; Bocquet-Appel 1989; see also Stocking 1982*b* for the Anglo-Saxon context).

In Portugal, by contrast, the critique of the racist evolutionary assumptions of physical anthropologists did not take place in any systematic manner before the second half of the twentieth century. In all probability, the field of anthropology as a whole proved too small, too homogeneous, and too conservative to accommodate diversity and generate innovation. The field also did not benefit from the climate of profound political instability and economic decline that marked much of the nineteenth century as well as the first decades of the twentieth century. Starting from the 1930s, the rise of Salazar's regime restricted the freedom of speech of anthropologists. The regime's colonial rhetoric emphasized the civilizing mission of the Portuguese and opposed racial miscegenation, and this rhetoric only started to change in the post–World War II period under the influence of Gilberto Freyre, a shift that eventually encouraged the development of a critique of racist evolutionary assumptions.

Acknowledgments

I thank the symposium organizers as well as all symposium participants for their critical comments on earlier versions of this text.

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Norwegian Physical Anthropology and the Idea of a Nordic Master Race

by Jon Røyne Kyllingstad

Anthropologists used to consider Norway a homeland for the so-called Nordic—or Germanic—race, which many Europeans and Americans held to be a superior race. This paper deals with the rise and decline of the idea of a Nordic master race in Norwegian physical anthropology. In the 1890s this idea held a key position in anthropological research on the racial identity and origin of the Norwegian population. In the early 1930s, however, leading Norwegian anthropological authorities condemned it as pseudoscientific ideology. I show how Norwegian discussions over this issue were related to greater conflicts within the international eugenics movement, to changing relations between German and Norwegian racial anthropologists before and after the Nazi takeover in Germany, and to conflicting and changing ideas of Norwegian nationhood among Norwegian scholars.

Introduction

The concept of a "Germanic" or "Nordic" race was a leitmotif in the racist ideology of the Nazi movement. The Nazis believed that the Nordic race was superior to all other races, and they were willing to make use of any means to fight for the expansion of this race at the expense of what they perceived to be inferior races. But the idea of a Nordic master race was not a Nazi invention. It was not even a German invention. At the beginning of the twentieth century, it was common among people in Europe and North America to believe that the white-skinned European population could be scientifically divided into a number of "races" and that one of these races was the blond, tall, long-skulled Nordic race. Many also assumed that the Nordic race—from which the north European nations were assumed to have their origin—was a superior race. Some north Europeans and North Americans even wanted to reorganize society in ways that would strengthen the reproduction and expansion of this blond "race" to the detriment of other "races."

Racist ideologists, in Germany and elsewhere, claimed that their theories were based on science. These claims on scientific legitimacy were supported by some influential members of the international scientific community, but they were rejected by others, and the political struggles over racist ideologies were interwoven with a struggle over scientific evidence, legitimacy, and disciplinary boundaries. Who had the right to speak with scientific authority about racial questions, and on which theoretical, methodological, and institutional foundations should this authority be based? These debates strongly influenced physical anthropology, which was dealing with the classification of so-called human races and the study of the origins, evolution, and migrations of these races.

In this paper I deal with the history of physical anthropology in Norway from its beginnings in the nineteenth century into the interwar years. It focuses on the origin, impact, and decline of the concept of a Nordic race within the Norwegian scientific community. Scandinavia held a key position in the worldview of the advocates of the supremacy of the Nordic race. Scandinavia was the core area of the race, and in Norway—many claimed—it was particularly pure and untouched. However, while holding a central position in the worldview of racial thinkers, Norway was a geographic, political, and scientific periphery. This situation preconditioned the development of Norwegian physical anthropology and put its stamp on the Norwegian scholarly debates on the concept of a Nordic master race.

The smallness of the Norwegian academic community made it very responsive to influences from the outside world and at the same time stimulated a large degree of interdisciplinary cooperation within the national scientific community. There were never more than three truly professional physical anthropologists in Norway. This could hardly constitute a national disciplinary community. Instead, the three Norwegian anthropologists should be seen partly as participants in transnational disciplinary networks and partly as members of a wider Norwegian scholarly community comprising a spectrum of disciplines. By studying the racial identity of past and present populations in Norway, anthropology was strongly connected to archaeological, linguistic, and historical studies of Norwegian prehistory and history. It was

Jon Røyne Kyllingstad is a postdoc at the Norwegian Museum of Science, Technology and Medicine (Kjelsåsveien 143, 0491 Oslo, Norway [jon.kyllingstad@tekniskmuseum.no]). This paper was submitted 27 X 10, accepted 15 VIII 11, and electronically published 7 II 12.

 0011-3204/2012/53S5-0005$10.00. DOI: 10.1086/662332

thus part of a research field of great importance to Norwegian national identity.

In order to understand the rise and fall of the concept of a Nordic master race within Norwegian physical anthropology, we need to study the actions of a very small number of individual researchers who participated in, responded to, and were constrained by mainly two spheres of scientific discourse; the national transdisciplinary academic community of Norway and the transnational disciplinary network of physical anthropologists. The Norwegian anthropologists had, however, particularly strong ties to German anthropology, and they can also to some extent be seen as a periphery in a German-speaking and Germany-centered disciplinary community.

In this paper I discusses how Norwegian anthropologists responded to and engaged in both national debates on the origin and identity of the Norwegian nation and international scientific debates on racial questions, and I elucidate how these debates were interconnected. I conclude with a close analysis of the relation between physical anthropology in Norway and Germany in the years leading up to the Nazi takeover in Germany.

Norwegian History and the Idea of a Germanic Race

Norway is a small country at the northern margins of Europe, with under 2 million inhabitants until 1890. Most people lived in rural settlements scattered along a narrow territory stretching far into the north. Historically, Norway had mainly two ethnic groups: the Norwegian majority and the Sami, an indigenous people living in the north. Until modern times, most Sami subsisted as fishermen, hunters, and farmers along the coast and as nomadic reindeer herders inland.

Norway was a part of the Danish kingdom for four centuries until 1814, when a Norwegian constitution and parliament were established. Almost immediately, however, the country was forced into a union with Sweden. This union was peacefully dissolved in 1905, when Norway became fully independent.

Norway was a small country with no continuous history as an independent political entity, and this shaped Norwegian nation building in the nineteenth and early twentieth centuries. An unbroken national history was (re)constructed by historians, folklorists, and philologists who claimed the existence of strong cultural continuity in rural society. The focus on popular culture and rural roots attempted to bridge the gap between the modern nation-state and the independent and powerful Norwegian Kingdom that had existed before Danish rule (Lunden 1995:27–45).

Norwegian scholars started to adopt some kind of biologically defined concept of a Germanic race in the 1830s and 1840s, when the impulses of Romantic nationalism were strong in Europe. As scholars and scientists searched for the roots of nations, ideas of nationhood were often conflated with biological ideas of "races" and "types." Linguists explained the birth of European nations as the result of waves of Indo-European tribes who migrated from Asia into Europe in prehistory. These theories were further elaborated by archaeologists studying cultural remains and by anatomists who investigated the skeletons of prehistoric populations. Danish and Swedish scholars had a leading role in establishing this field of research and in the construction of a biologically defined Germanic race, which became an important building block in the national mythologies of north European countries with a presumed Germanic heritage (Horsman 1976, 1981; Mosse 1997:35–50).

It was Danish archaeologists who first put forward the system of the three prehistoric ages—Stone Age, Bronze Age, and Iron Age—and created a comparative method to classify archaeological findings according to this typology (Trigger 1989:73ff.). This important breakthrough in archaeology was strongly intertwined with a methodological innovation by the Swedish anatomist Anders Retzius, namely, the cephalic index, which had important implications for physical anthropology. The cephalic index measures the relative difference between the length and the width of the skull.

Based on contemporary theories of the anatomy, physiology, and natural history of the central nervous system, Retzius argued that the width-length ratio of the skull—and thus the brain—could be used to classify human mental development. Elaborating previous theories by the zoologist, archaeologist, and ethnographer Sven Nilsson, Retzius claimed that people of different nations had different skull shapes. Thus, the cephalic index could be used to measure the mental ability of ethnic groups and to identify the ethnicity of human archaeological remains (Gustafsson 1996:62–82; Retzius 1843, 1848; Rowley-Conwy 2007:60–65). Retzius and Nilsson suggested that a short-skulled ("brachycephalic") Finnish-Sami people had been the sole inhabitants of northern Europe in the Stone Age and that the Bronze Age was introduced by short-skulled Celts, later supplanted by long-skulled ("dolichocephalic") Germanic tribes bearing superior Iron Age technology (Nilsson 1838; Retzius 1847). In the mid-nineteenth century, politically charged debates on the origin of nations stimulated archaeological research in Europe and the use of craniology to determine the ethnic identity of archaeological remains. Retzius's cephalic index gained general recognition as a key criterion for racial classification.

While no Norwegian natural historians, anatomists, or archaeologists participated in the development of the three-age chronology or the cephalic index, Sven Nilsson exchanged ideas on his migration theory with the Norwegian historian and linguist Rudolf Keyser, who was working on a similar theory on the settlement of Scandinavia (Andersen 1961:232–246). Keyser's theory differed in one important aspect, however. He argued that Scandinavia had been invaded by two Germanic peoples: the south Germanics in Denmark and southern Sweden, and the north Germanics in Norway and northern Sweden. This implied that the Norwegians had a stronger claim on the cultural inheritance of the ancient Norsemen than the Swedes and the Danes (Munch 1849).

Keyser based most of his arguments on comparative historical linguistics and historical sources, but he also made use of the results of Nilsson's craniological studies. He described the ancestors of the Norwegians as biologically superior to those of the Sami. The Germanics were taller, stronger, and better mentally equipped. Norwegian nationhood was thus construed as both a biological and a linguistic and cultural entity, blurring the distinction between biological and cultural traits (Keyser 1868:232–246).

Keyser was professor of history at the only Norwegian university, the University of Christiania (Oslo), and he had a key role in establishing historical scholarship in Norway. He believed it was his task to show that Norway had an ancient history. Together with his younger colleague Peter Andreas Munch, he created a national narrative in which the idea of the Germanic invasion giving birth to the nation was of central importance. This had a great influence on the writing and teaching of national history in Norway (Dahl 1990:80–81, 86–112; Lunden 1995:34–35).

Keyser's migration theory remained more or less unchallenged for the two decades leading up to the late 1860s, when it was undermined by geographical, historical, cultural, and linguistic arguments and by the classical theories of unilineal cultural evolution that emerged in international academic debates in the 1860s and 1870s (Dahl 1990:80, 86–112). The most important criticism was from the historian and ethnographer Ludvig K. Daae, who claimed that migration theories were outdated in light of modern cultural evolutionism. He argued that the physical and cultural traits of the ancient Norsemen were not the result of an invasion of a biologically distinctive tribe but a gradual adaptation to the natural environment in Norway and could be understood in the context of a universal scale of cultural development (Dahl 1957–1958).

Daae embraced "Scandinavism," a cultural and political movement that promoted solidarity between Scandinavian countries. He was therefore inclined to reject Keyser's theory on political grounds. But Keyser's theory was also rejected by the national-liberal historian Ernst Sars, who opposed Scandinavism and spent much of his professional life developing a major national-historical synthesis (Dahl 1990:86–112).

Sars not only dismissed the immigration theory, he even criticized the theoretical foundation on which it was built. He claimed that Keyser had conflated the concept of "nation" with the concept of a "race" with immutable traits and had construed the history of the nation as an unfolding of these fixed traits. Instead, Sars postulated that the nation was the product of its history and that the history of the nation could be explained as the gradual evolution of a social organism adapting to a natural and cultural environment while progressing along a universal path of cultural development (Fulsås 1999:138).

Sars was one of the most influential ideologues of the Venstre movement, the liberal party that dominated Norwegian political life from the 1880s to the interwar years. The Venstre movement embraced a blend of democratic nationalism and social liberalism and was supported by a coalition of farmers and liberal elements of the urban bourgeoisie. According to Sars's national-evolutionary synthesis, the Venstre coalition was the logical outcome of the country's historical development. In this view, a rural society of free farmers had ensured national continuity through the years of Danish rule. After 1814, a national and democratic rural culture had overcome the cultural and political hegemony of the Danish-Norwegian elite, and both urban and rural elements were on a path toward integration into a larger national community (Fulsås 1999).

The Beginning of Physical Anthropology in Norway

In the first part of the nineteenth century, discussions on the origin and identity of the Norwegian nation were dominated by historians, linguists, and folklorists. Archaeology was poorly established before the 1870s, while physical anthropology did not exist as a field of inquiry until the late 1880s. In the 1890s, Norway was swept by a wave of national feeling, leading to the dissolution of the union with Sweden in 1905. It was at this time that a Norwegian tradition of physical anthropological research was established, and prehistoric migration theories returned as an important theme in Norwegian academic debates.

Norwegian anthropology arose in two different institutional settings. In the army, doctors began conducting anthropometric surveys of military recruits, and at the Anatomical Institute at the medical faculty of the University of Oslo, an anthropological collection of human bones was established. Some of the bones were from Sami burials, but most originated from archaeological excavations in southern Norway (Holck 1990:39–49).

In a 1896 study titled *Norrønaskaller* (Ancient Norse Skulls), the prosector of the institute, Justus Barth (1896), suggested that the Norse Germanics were characterized by a certain dolichocephalic skull shape, which he termed the "Viking type" (*vikingtypen*). Five years later, the enlarged collection was reexamined by the army physician Carl F. Larsen (1901), who argued for the existence of no less than five Norwegian racial types.

The true founder of physical anthropology in Norway, however, was not a university anatomist but the army physician Carl Oscar Eugen Arbo. Financed by the Norwegian government, he conducted extensive physical measurements of army recruits. The results were published by the leading scholarly institution, the Norwegian Society of Science and Letters, and were seen as an attempt at a systematic mapping of the racial characteristics of the contemporary population in southern Norway (Arbo 1885–1904).

Arbo measured traits such as the length of the face, width of the cheek, angle of the jaw, body height, eye and hair color, and, most importantly, the cephalic index. Arbo (1897) described a pattern of geographical distribution of skull shapes in Norway with a relatively high frequency of dark-haired and brown-eyed individuals with short skulls ("brachycephalics") along the west and south coast while eastern Norway, especially the inland valleys, was inhabited mostly by people with blond hair, blue eyes, and long skulls ("dolichocephalics"). Arbo suggested that the brown-eyed, dark-haired people with short skulls were the descendents of Stone Age and Bronze Age people, while the blue-eyed and blond inhabitants of eastern Norway descended from Iron Age Germanic invaders. Because both groups were ethnically Norwegians, this meant the Norwegian population was racially divided, and only the blond long-skulled eastern Norwegians could claim genuine biological ties to ancient Norsemen. Arbo (1897) believed these racial differences explained geographical differences in mentality, behavior, temperament, and health, describing the short skulls of the west as weak, shy, nervous, petty, and narrow minded in contrast with the bolder, braver, and stronger long skulls of the inland valleys.

By the turn of the twentieth century, Arbo was the leading Norwegian physical anthropologist. However, the most prominent popularizer of anthropological racial theories was not Arbo but the amateur scientist and writer Andreas M. Hansen, who gathered knowledge from various disciplines, including geology, archaeology, linguistics, and geography, and constructed a historical synthesis based on the anthropometric findings of Arbo and others.

According to Hansen, an inferior indigenous population subsisting on hunting and fishing had lived along the Norwegian coastline during the Ice Age. After the withdrawal of the ice, a superior long-skulled race had migrated overland from the east, settled inland, and established themselves as rulers over the inferior short-skulled coastal dwellers (Hansen 1894–1898:46, 69–75). This theory became well known and hotly debated in Norway, and Hansen claimed that it could explain contemporary regional differences in dialects, mentality, and political attitudes. He showed that the geographical distribution of votes in parliamentary elections coincided with variations in the average cephalic index and argued that short-skulls were conservative, distrustful, and backward looking and therefore inclined to vote for Høyre (the conservative party), while the open-minded and intelligent long skulls supported enlightenment and progress and leaned toward Venstre (the liberal party; Hansen 1809:50).

Even though Norwegian physical anthropology at the turn of the century was mainly concerned with the origin and racial identity of the Norwegians, this issue was intertwined with the question of the prehistory of the Sami. Both Hansen and Arbo rejected Nilsson, Retzius, and Keyser's view of the Sami as remnants of the original inhabitants of northern Europe. This theory had been opposed by scholars from the 1860s, and before the end of the century it was replaced by a theory of two distinct Scandinavian Stone Age cultures. Southern Scandinavia was now seen as part of a European cultural region populated by the Stone Age ancestors of the present-day Scandinavians. The Sami were now considered descendants of a distinct northern Scandinavian Stone Age people strongly connected to similar Stone Age cultures farther east (Furset 1994:19–60; Storli 1993). This meant that the local prehistory of the Norwegians was traced back to the Stone Age, while the ancestors of the Sami were no longer regarded as Europeans but as part of a northern Asiatic region of "Arctic cultures." Moreover, while the southern Scandinavian Stone Age people had evolved culturally with the rest of Europe, the Arctic peoples had until very recently remained at the cultural level of the Stone Age.

Hansen went a step further, claiming that the primitive short-skulled race along the coast were the earliest inhabitants of all Scandinavia, even the Sami regions in the north. The particularly "primitive," "weak," and "dwarflike" Sami, according to Hansen, were not indigenous inhabitants of north Scandinavia but had migrated into Scandinavia from Asia in the Middle Ages (Schanche 1997:40). This theory had political implications for an ongoing debate on the territorial and cultural rights of the Sami. From the end of the nineteenth century, pressure on Sami culture increased, with a strong policy of cultural assimilation of minorities and pressure from farming settlements on the territories of nomadic reindeer herders. After 1905, the tensions assumed international dimensions because of conflict over the grazing rights on Norwegian territory of reindeers owned by Swedish citizens. The cultural and territorial rights of the Sami could be defended on the assumption that they were the indigenous inhabitants of northern Scandinavia. By denying the indigenousness of the Sami population, Hansen's theory supported an aggressively ethnonationalistic stand in the political debate (Kyllingstad 2008:304–318, 330–333). Hansen's theory was strongly opposed by linguists, folklorists, and ethnographers expert on Sami culture, but it influenced public debate and academic research on the ethnicity of the prehistoric settlements in northern Norway (Kyllingstad 2008).

Norwegian Physical Anthropology and Anthroposociology

Back in the 1840s, when Retzius and Nilsson launched their migration theories, anthropology scarcely existed as a discipline. The situation had changed radically by the 1890s, when physical anthropological research was taken up in Norway. In the last three or four decades of the nineteenth century, museums and universities all over Europe established skeletal collections, and racial surveys were undertaken in many countries. Physical anthropology emerged as an institutionalized science with its own journals, conferences, and research activities.

Scientific research and discussions within the new discipline of physical anthropology were infused with ideological implications. Physical anthropology can be seen as a scene where different ideologically charged ideas about evolution and race were discussed, developed, criticized, and tentatively tested against biological theories and empirical findings. Knowledge from anthropological research could be recruited to legitimize racialist, imperialist, and nationalist ideologies, but it could also be mobilized as a rhetorical weapon against racial ideologies. These debates were strongly stimulated in the 1890s, when racialist nationalism attracted growing public attention in northern Europe and a new discipline, anthroposociology, emerged at the cross-section between social sciences and physical anthropology. Anthroposociology was an attempt to create a social science based on the idea of competition between races (Hecht 1999; Massin 1996:106–114).

The anthroposociologists held that the blond, long-skulled northern Europeans were descendants of a warrior race who had conquered Europe in the Iron Age, had established themselves as aristocratic rulers of the native short-skulled races, and had founded European civilization. In line with this theory, they studied the geographical and social distribution of bodily traits such as skull shape and hair color and claimed they could prove that the stratification of European society mirrored the racial quality of the strata. They also claimed that industrialization and urbanization stimulated geographical and social mobility, erased the natural distinction between classes, and led to racial mixing and biological degeneration (Hecht 1999; Massin 1996:106–114).

The main proponents of anthroposociology were the Frenchman Vacher de Lapouge and the German Otto Ammon. In the 1890s, Lapouge led a comprehensive research program and managed to gain some deal of acceptance from the French academic establishment. But he was also opposed by leading anthropologists, and from the turn of the century, he was increasingly marginalized by the French scientific community even though he continued for many years to be an internationally influential propagandist of the blond race (Hecht 1999).

Otto Ammon's career followed a strikingly different path. According to Benoit Massin, German physical anthropology in the nineteenth century was dominated by relatively egalitarian attitudes to race, which led to the exclusion of Ammon from leading anthropological journals and organizations. After the turn of the century, however, his academic credibility increased sharply in line with a broader change of attitudes among German anthropologists. Racial deterministic attitudes gained a growing influence on the discipline (Massin 1996:106–114).

Conflicts on anthroposociology were part of broader scientific debates in German and French anthropology. The theoretical foundations of the entire physical anthropological enterprise and the concept of race were questioned. Leading anthropologists argued that categories such as short and long skulls were arbitrary constructs, pointing out that an infinite number of measurable anatomical traits could be used as criteria for the classification of humans and that none of them, whatsoever, was relevant for understanding human psychology (Hecht 1997; Massin 1996:106–114; Staum 2004). These discussions did not appear to impress those who strove to establish anthropology in Norway, however. Instead, Norwegians turned to anthroposociology for theoretical and methodological guidance. The relatively controversial racial theories of Andreas M. Hansen were strongly influenced by Ammon and Lapouge, but also the respected scientist Arbo supported anthroposociology. Otto Ammon's book *Der natürliche Auslese beim Menschen* (Natural Selection among Human Beings) was the most cited text by Arbo (1885–1904). Even the professor of anatomy Gustav Adolf Guldberg, head of the Anatomical Institute at the University, publicly embraced Ammon's and Lapouge's views.

At the urging of his German colleague Gustav Schwalbe, Guldberg (1997) proposed an extensive physical anthropological survey of Norway. Guldberg referred to Lapouge and Ammon and claimed that a mapping of regional distribution of physical traits would shed light on social, political, and historical questions because of the distinct psychological character of the different races. The suggestion was welcomed by the Norwegian Society of Science and Letters, and a committee to implement the plan was established.

Norwegian Physical Anthropology and the Eugenics Movement

Three of the four committee members died within a short space of time, and the plans were never implemented. After World War I, however, the idea was taken up again by three individuals: the army physician Halfdan Bryn (fig. 1); Kristian

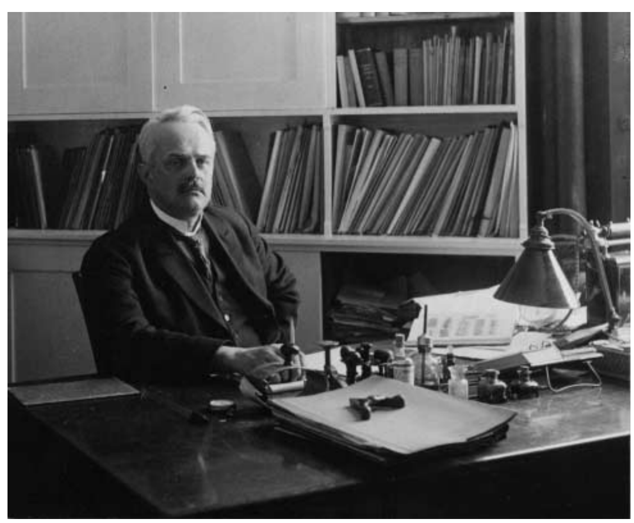

Figure 1. Halfdan Bryn at his desk (unknown photographer; photo from Halfdan Bryn's private archive at Norges Teknisk-Naturvitenskapelige Universitet library, Trondheim, Norway). (A color version of this figure is available in the online edition.)

Emil Schreiner, Guldberg's successor as professor of anatomy (fig. 2); and Schreiner's wife, the medical researcher Alette Schreiner (fig. 3). Under the leadership of Kristian Schreiner, physical anthropology became an important field of research at the University of Oslo's Anatomical Institute.

Initially the growth of anthropology at the institute was related to an intensification of archaeological activity that expanded the collection of ancient skulls. The archaeologists were, however, almost exclusively interested in the Norwegian past, and presumed Sami settlements were seldom excavated. Therefore, to shed light on the prehistoric Sami settlement of northern Scandinavia, the institute began to conduct its own excavations. During the interwar years, many Sami burials were excavated, and more than 500 skulls were taken to the institute (Schreiner 1931–1935:1). Alette and Kristian Schreiner believed that the remains of past populations had to be studied in tandem with the living population. They therefore entered into collaboration with the army doctor Halfdan Bryn and initiated a massive publicly funded anthropometric survey of military recruits of both Norwegian and Sami districts (Kyllingstad 2004:130–133).

This ambitious research project began in the early 1920s under the joint leadership of Bryn and Kristian Schreiner but was to end in professional and personal enmity and a loss of scientific credibility for the former. Bryn had been inspired by Andreas M. Hansen and was a proponent of the concept of a Nordic master race. His professional fate was tied to the diminishing support for this idea by the Norwegian academic community. To understand the context and causes of these events, we must once again examine the world outside Norway and the rise of the eugenics movement and its role in physical anthropology.

The idea of eugenics arose in the late nineteenth century and lead to an organized international eugenics movement in the early twentieth century. The International Federation of Eugenics Organizations (IFEO) was established in 1912 to coordinate cooperation between national groups. Eugenicists were united by the belief that natural selection in humans was decreasing as a result of modern life. The spread of less worthy elements at the expense of valuable human material had to be counteracted by public intervention in the biological reproduction of individual members of society (Kühl 1997). Apart from this common goal, eugenics was a very heterogeneous movement. The idea of improving the human race was not necessarily linked to the concept of "races" in the physical anthropological meaning of the word. Much eugenic thinking was concerned with the frequency of inherited traits for health and disease without much attention to racial classification. However, sections of the eugenics movement adopted racial ideas similar to anthroposociology. Some eugenicists—particularly in Germany, Scandinavia, and the United States—saw the protection of the Nordic race as their primary goal (Kühl 1997). This created a new market for racial anthropology. Eugenics was subsequently considered an important justification for research into the racial differences of humankind.

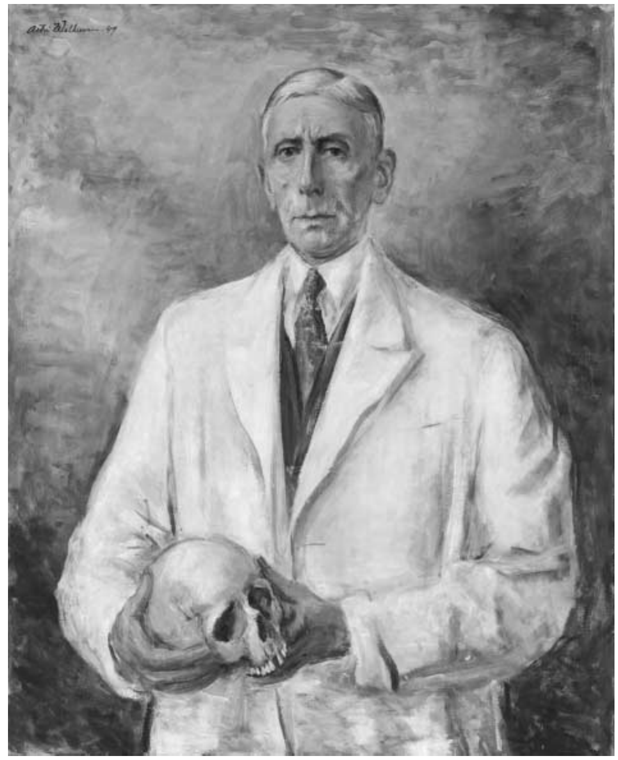

Figure 2. Professor of Anatomy Kristian Emil Schreiner, painted by Astri Welhaven Heiberg, 1949 (photo: Arthur Sand; the portrait is reproduced with the permission of the University of Oslo). (A color version of this figure is available online.)

The growth of eugenics peaked in the wake of World War I, both worldwide and in Norway. In 1914, Jon Alfred Mjøen published *Rasehygiene* (Race Hygiene), a book that provoked considerable debate in Norway. Mjøen was concerned about the issue of the biological quality of the Norwegians, which he strongly—thought not exclusively—related to the purity of the Nordic race (Roll-Hansen 1996). His views influenced public debates in Norway but were almost unanimously rejected by leading Norwegian biologists. When the Norsk Forening for Arvelighetsforskning (Norwegian Association of Genetic Research) was established in 1919 by University of Oslo geneticists, Mjøen did not become a member. Mjøen, already a prominent member of the international eugenics movement, became the Norwegian representative of the IFEO and created his own Norwegian eugenics committee, a private laboratory, and the journal *Den nordiske rase* (The Nordic Race). Members of the Norwegian Association of Genetic Research declined offers to join the IFEO, and the controversy over Mjøen prevented the international eugenics movement from getting a real foothold in the Norwegian scientific community (Monsen 1997:59–74).

It is, however, unlikely that Mjøen's Nordic supremacy views were the decisive reason why he was dismissed by Nor-

Figure 3. Alette Schreiner showing some children an animal skull, ca. 1910 (undated; unknown photographer; University of Oslo; University History Photobase).

wegian biologists. Halfdan Bryn also expounded these ideas but was not similarly rejected by Mjøen's opponents (Kyllingstad 2004:93, 170). In 1915, Professor Schreiner made a heated polemic attack on Mjøen's credibility (Roll-Hansen 1996:156), but 4 years later, Alette and Kristian Schreiner were collaborating closely with Bryn. Kristian Schreiner held a powerful academic position and could facilitate funding and publication opportunities for Bryn, the army doctor who had just taken up a career as a physical anthropologist. By stimulating his academic career, Kristian Schreiner helped Bryn to become acknowledged as a scientific expert on racial questions, and Bryn used this academic legitimacy to promote eugenic measures aimed at the purification and expansion of the Nordic race (Kyllingstad 2001:104–117, 156). Ten years later, however, a schism had occurred between Bryn and the Schreiners. This had devastating consequences for Bryn's standing in the Norwegian scientific community and marked the beginning of a trend where Bryn and the ideas he represented where redefined from "scientific" to "quasi-scientific" (Kyllingstad 2004: 140–150, 162–165).

The antagonism between Bryn and the Schreiners was intertwined with a general polarization of debates on racial questions in the international scientific community. From the second half of the 1920s, racial theories such as Bryn's were increasingly linked together as an interconnected system of dogmas that had an international network of supporters—mainly in the United States, Germany, and Scandinavia—and that was met with increasingly vigorous and coordinated academic opposition (Broberg 1995:60–87; Kevles 1986:123–127; Kühl 1997:71–94).

Opposition to orthodox racist dogmas did not necessarily lead the critics to dismiss views of a hierarchical racial order. Many scientists accepted the basic idea of superior and inferior races while rejecting dogmatic racism and the idea of a Nordic master race (Kyllingstad 2004:153–156). The polarized debates did, however, draw attention to the crucial foundations of the racist strain of eugenics: the notion of racial differences in intelligence, the supposed degenerating effects of racial admixture, and the concept of ancient pure races. Arguments from genetics, physical anthropology, psychology, cultural anthropology, and social science were incorporated into the debate (Barkan 1992).

Differences in opinion strongly affected international cooperation among eugenicists. The IFEO was initially an umbrella organization for the leading institutions in relevant scientific fields that aimed at representing the whole heterogeneous spectrum of ideological and scientific attitudes within the eugenics movement. From the late 1920s, the organization became increasingly affected by polarization and was, according to Stefan Kühl (1997:71–97), more and more taken over by an international network of orthodox racists.

This network was dominated by Americans, Germans, and Scandinavians such as Charles B. Davenport, Fritz Lenz, Eugen Fischer, Ernst Rüdin, Herman Lundborg, and Jon Alfred Mjøen. Except for Mjøen, these men were regarded as prominent scientists in their home countries. From about 1930, however, the North American and Scandinavian members of this clique experienced sinking prestige while their German counterparts gained increased prestige in their home country.

This trend accelerated after the Nazi takeover in 1933 (Barkan 1992:165–168; Broberg 1995:60–87; Proctor 1988).

Bryn and German Physical Anthropology

The conflict between Bryn and Schreiner can be understood in this international context. Let us first consider Bryn's relation to Mjøen and the IFEO.

In the early 1920s, Bryn had considered collaborating with Mjøen. This failed because Bryn was not willing to choose Mjøen's side in the conflict with Schreiner. When Schreiner and Bryn later fell out, this was partly triggered by a disagreement over Mjøen's research on the ills of racial mixing in 1926–1927. Mjøen's research was sharply criticized by U.S. geneticist William Castle, and this sparked a heated discussion at a 1927 international IFEO conference. While Bryn supported Mjøen's theories, Schreiner condemned them as fantasy. When the break between Schreiner and Bryn shortly afterward became inevitable, Bryn resumed contact with Mjøen, who was quick to ensure that Bryn became a member of the IFEO. The IFEO had for many years tried, without success, to gain a foothold in the Norwegian scientific community, but getting Bryn as a member must have been a poor reward to these efforts, because Bryn was about to be marginalized by this community. And, ironically, his new alliance with Mjøen and the IFEO accelerated this process (Kyllingstad 2004:163ff.). Moreover, Bryn's entrance into the IFEO was halfhearted. Even if he had now sided with Mjøen in the conflict with Schreiner, he did not adopt Mjøen's network and the IFEO as his new arena of scientific endeavour (Kyllingstad 2004:164). So where did he turn his attention as his career possibilities in Norway were waning?

When Bryn first went public with his racist social philosophy in the early 1920s, he was clearly inspired by American racist thinkers such as Madison Grant (Kyllingstad 2004:208ff.). He was also in close contact with Swedish peers, most notably Herman Lundborg, whose ideas where similar to Mjøen and Bryn's and who led the internationally acknowledged State Institute for Racial Biology in Uppsala. Lundborg had a much stronger institutional foothold in Sweden than Mjøen and Bryn had in Norway, but he too was met by growing scientific and political opposition in the 1930s. When he retired in 1935, he was succeeded against his will by Gunnar Dahlberg, a proponent of nonracist eugenics who launched a research program in human genetics devoid of anthropological "races" (Broberg 1995:60–87).

It was in Germany that the kind of science Bryn represented seemed to have the brightest future, and from the end of the 1920s, his outlook and professional network became mainly directed toward the German scientific community. After the national humiliation of 1918, an aggressively racist nationalism had gained increasing support in Germany. A growing number of so-called *Völkisch* groups and organizations cultivated Romantic ideas about national origins and racial purity and pride. The *Völkisch* movement was strongly influenced by paganism and occultism, but some *Völkisch* nationalists also wanted to build their ideology on science and turned their attention to physical anthropology and racial hygiene. In the 1920s, German Nordic racial ideology became known by the name *nordische Gedanke* and gained attention and popularity.

This also influenced German physical anthropology. From the mid-1920s, German anthropology became increasingly dominated by the *nordische Gedanke*, anti-Semitism, and eugenic thinking, and this was reinforced in the Nazi period. Anthropology was renamed *Rassenkunde* and became a major scientific tool to legitimize, design, and implement the racial policies of the Nazi regime (Barkan 1992:165–168; Proctor 1988:165–168).

At the center of Bryn's German network was Hans Günther. When Bryn first met him in 1923–1924, Günther was a little-known *Völkisch* writer. He had no scientific background but had been assigned by the anti-Semitic and ultranationalistic publisher J. F. Lehman to write popular books about the races of Europe. In the 1920s, Günther therefore moved around Europe to visit the institutions and scientists involved in racial anthropology. Günther, who was married to a Norwegian woman, lived for a while in Norway and became a personal friend of Bryn (Kyllingstad 2004:167ff.).

Günther's first book on race, *Rassenkunde des deutschen Volkes* (The Racial Ethnology of the German People; 1922), was widely read, and it turned him into a leading proponent of the *nordische Gedanke.* In line with the rise of racist thinking in German physical anthropology, Günther's views had an increasing influence on the scientific debates. From the mid-1920s, a younger generation of men with new ideas joined the field (Mosse 1964:224; Proctor 1988:149), and Bryn established his German network mainly among these young scientists. After 1933, many of them gained key positions in the new Nazi regime. Günther himself obtained a professorship at the University of Jena in 1931 with the help of local Nazi politicians, and he became a leading racial ideologist in the Nazi period (Kyllingstad 2004:166–174; Mosse 1964:224; Proctor 1988:149).

In the early 1920s, Günther was an outsider who needed support from established scientists such as Bryn to build his career. By the end of the decade, the roles were reversed, and Günther could support his friend Bryn's flagging career with the help of his network of German anthropologists sympathetic to the *nordische Gedanke.* Through Günther, Bryn joined the Nordische ring, an organization of supporters of the *nordische Gedanke.* He began publishing in German scientific and ideological journals and was commissioned by Günther's publisher, J. F. Lehman, to write a popular account of *Rassenkunde* in Norway.

When the book *Der nordische Mensch* (The Nordic Man) was published in 1929, Bryn gained great prestige among the adherents of the *nordische Gedanke* within German physical anthropology. The book was not the popularized description of the racial composition of Norway expected by Günther and Lehman but rather a new theory of the evolution and racial characteristics of the Nordic race. Bryn's new theory came to serve as an argument in a highly politicized German debate on the Nordic race. Some leading German anthropologists claimed that the "Nordic race" was a purely theoretical construction, with no empirical foundation. The adherents of the *nordische Gedanke* welcomed Bryn's analysis of his vast data set on Norway as an important counterargument to this claim (Kyllingstad 2004:171–174).

The Schreiners and the Idea of the Nordic Master Race

In the early 1920s, Alette and Kristian Schreiner were somewhat indifferent to the Nordic supremacist idea. In general they seem to have considered it as a set of yet unproven but adequate scientific hypotheses. But in 1929, the year Bryn published *Der nordische Mensch*, Alette Schreiner (1929:460–461) openly denounced as pseudoscience the idea of a Nordic master race. In a radio broadcast 3 years later, her husband stated that racial hygienic measures favoring the Nordic race lacked scientific support (K. Schreiner 1932). These views also seem to have influenced his analysis of the "Norwegian" skulls in the anthropological collection of the Anatomical Institute (Schreiner 1939–1946). One of his main conclusions was that Norway had been dominated in the Middle Ages by a "Nordic" cranial type. He did not, however, see this cranial type as a proof for the existence of an ancient and stabile Nordic race. Instead, he claimed that "Nordic race" was a descriptive term designating a "phenotype" that had occurred in northern Europe as a product of racial mixing during and after the Neolithic (Schreiner 1939–1946).

This conclusion was put forward in a two-volume monograph, published just before and right after World War II, and there are strong reasons to believe that the war strengthened Schreiner's critical attitude to the concept of the Nordic race. Five years of German occupation (1940–1945) opened the way to power for Vidkun Quisling and the Norwegian national socialist party Nasjonal Samling and for attempts at Nazification of the Norwegian society. Nazification pressure against the university provoked resistance from a majority of the students and professors, leading to the deportation of students and the imprisonment of many professors. Schreiner was among those who openly opposed the new regime and its ideology, and he was imprisoned for a period (Fure 2007:138–139, 152). Right after the war, Schreiner gave a lecture titled "Hva er nordisk rase" (What is Nordic race?; Schreiner 1946). Here he attacked an idea that had gained great popularity among "certain people" during "recent years"; namely, the idea of an ancient and originally pure Nordic race threatened by contamination through racial mixing with "inferior" races. Schreiner claimed that his own studies of ancient Norwegian skulls contradicted this theory, and he suggested that the "Nordic race" was a purely descriptive term for a skull shape produced by racial admixture within a prehistoric bastard population (*en bastardbefolkning*; Schreiner 1946).

Kristian Schreiner, the Lappic Race, and the Notion of Superior and Inferior Races

The fact that Alette and Kristian Schreiner already from the late 1920s opposed the notion of a Nordic master race did not imply that they dismissed the basic concept of superior and inferior races. Even after their break with Bryn, they continued to support the general idea of a hierarchy of races that could be ranked according to differences in mental abilities. And even in a study published after World War II, Kristian Schreiner suggested the existence of differences in mental ability between racial groups (Schreiner 1951). This attitude is, however, most clearly expressed in their studies of the anthropology of the Sami, which were published in the mid-1930s.

Kristian Schreiner's study of the Sami skulls collected by the Anatomical Institute in the interwar years was designed to test Andreas M. Hansen's view of the Sami as historical latecomers in Scandinavia. Schreiner rejected this theory (Schreiner 1931–1935:273), claiming instead that the Sami settlements in the north of Norway stretched back to at least before the Viking age. Crucial evidence for Schreiner's argument was provided by several skulls excavated some years earlier by Isak Saba, a Sami teacher, journalist, politician, amateur researcher, and ethnopolitical activist. Saba had been eager to demonstrate the antiquity of Sami history in northern Scandinavia, and in public debates Schreiner's analysis of the Sami skulls was used as support for Sami claims on indigenousness.

It is clear that Schreiner's research was not intended as an instrument for racism or ethnonationalistic chauvinism, but it was still influenced by racial prejudices. Unlike his conclusions about the racial heterogeneity of the Nordic "race," he claimed the existence of a pure ancient Lappic race. According to Schreiner, the Sami were a heterogeneous population heavily admixed with their Scandinavian and Finnish neighbors. But within this blend, he identified a pure and ancient "Lappic" racial element characterized by a certain set of traits, including short stature and legs, long arms, spaced eyes, and protruding jaw, which, according to Schreiner, indicated that the Sami were an ancient, "unspecialized," and "infantile" race. He also claimed that these "primitive" physical traits correlated well with the mental characters of a typical Sami individual, which was described as carefree, joyful, shy, and simpleminded (Schreiner 1931–1935:276–288). The same descriptions of an "infantile" and "primitive" Sami psychology can be found in Alette Schreiner's works on the living Sami populations (A. Schreiner 1932:13).

Notions of Sami primitiveness also colored the conduct of their research. Kristian Schreiner demanded excavations of Sami graves despite local protests (Schanche 1997:50), and Alette Schreiner, in her studies of contemporary populations, sometimes used "mild violence" and "small gifts" to persuade Sami individuals to undress. She explained their shyness and their lacking recognition of the importance of her research as a sign of their racial primitiveness (A. Schreiner 1932:13).

The Norwegian Academic Community and the Nordic Race

The idea of a Nordic master race was part of Norwegian physical anthropology from its establishment as a discipline in the 1890s until the 1920s. This changed from about 1930, when Alette and Kristian Schreiner started arguing that this idea belonged on the scrap heap of outdated scientific concepts. Their change of attitude from indifference to condemnation seems to have been part of a trend among Norwegian scholars and scientists. Halfdan Bryn died the year Hitler seized power in Germany. During the last years of his life, he enjoyed increasing success as a scientist in Germany, but he felt isolated and misunderstood by the Norwegian scientific community, and his enhanced reputation in Germany and declining reputation in Norway was largely due to the same factors; namely, that his research served to justify the idea of a Nordic master race (Kyllingstad 2004:170).

In 1941 the Norwegian Nazi writer Sigurd Saxlund examined the development of racial consciousness in Norway in the years preceding the German invasion. He complained that Norway had been slow to face the racial problem and pointed to Bryn as a lonely and misunderstood prophet in the desert (Saxlund 1941). German researchers of the Nazi organization Ahnenerbe made similar comments when they traveled to Norway in the 1930s in search of sources for the study of Germanic prehistory. They reported to Germany on a regrettable lack of racial consciousness among Norwegian scientists and scholars (Fure 2007:42).

There are several possible explanations for this shift, and I will conclude by discussing some of them by placing physical anthropology in the broader context of Norwegian science and scholarship. It can hardly be claimed that three individuals, Bryn and the Schreiners, constituted a national disciplinary community. Instead, Bryn and the two Schreiners should be seen both as participants in transnational disciplinary networks and partly as members of a wider Norwegian scholarly community made up of many disciplines. Thus, the anthropological study of the racial identity of the Norwegians and Sami was strongly connected to Norwegian archaeological, linguistic, and historical studies of national prehistory and history.

All these disciplines were influenced by ideas on race. Archaeologists, historians, and philologists discussed the racial theories of Arbo, Schreiner, and Bryn. Even Hansen was to some degree taken seriously in the period from about 1890 into the interwar years. However, a view of society and nationhood based on the idea of races with unchanging psychological characters never made a hegemonic breakthrough in any of these disciplines.

Instead, it can be argued that this field of inquiry was far more influenced by an unilineal cultural evolutionism of a type that the aforementioned historian Ernst Sars advocated in the late nineteenth century. In 1900, Sars published an article titled "Norske folketyper" (Ethnological types of Norway) jointly with the professor of folklore Moltke Moe. They stated as an uncontroversial scientific fact that the Norwegian nation was composed of two or more craniologically distinct races. But according to Moe and Sars, this had no bearing on the question of the psychological properties of the nation. Instead, the nation was rather seen as the result of gradual cultural evolution and of the adaptation to a certain natural and cultural environment (Sars 1900:431). Both Moe and Sars were important participants in the debates on the history and identity of the nation, and the type of ideas that they propagated had greater influence on the Norwegian academic community than racial thinking in the style of Arbo, Hansen, and Bryn.

After World War I, the Institute for Comparative Cultural Research (Instituttet for Sammenlignende Kulturforskning) achieved a leading role in Norwegian archaeology, history, linguistics, folklore, and ethnography, focusing mainly on the study of the history and culture of traditional Norwegian rural society and the Sami people. The institute did not have a permanent staff and was a hybrid of a research institute, a scientific publisher, a research council, and a research foundation. It was this peculiar institution who published and partly financed much of the physical anthropological research by Alette and Kristian Schreiner (Kyllingstad 2008).

The institute had a program of comparative cultural research based on the idea of unilineal cultural evolution. The basic idea was that every single "culture" or "society" was the product of adaptation to a certain environment. And by comparing the development of different cultures adapting to different environments, one could reach an understanding of the universal patterns—the laws—behind the development of apparently heterogeneous cultures and nations. This research strategy was based on a basic idea that all human beings are equipped with equal psychological abilities and therefore respond equally to the challenges from the natural environment (Kyllingstad 2008:97–102; Stang 1925).

The idea of a psychic unity of humankind was also a central tenet in the ideology of the Institute for Comparative Cultural Research, which, besides the research agenda, also had a political agenda. The institute was established after World War I as an instrument for peace. It contributed to the restoration of the organized scientific internationalism that had been destroyed by war by bringing together leading scholars from former enemy nations at lengthy conferences in the neutral city of Oslo. (Kyllingstad 2008:11–81, 123–165, 255; Stang 1925). The institute was also intended to stimulate Norwegian consciousness and prestige through the study of national cultural heritage and to contribute to the "branding" of Norway as a peace-loving and peacemaking nation (Kyllingstad 2008: 60–80). So the institute was both shaped by international academic debates on cultural research and scientific internationalism and by the international situation facing newly independent Norway as a small, vulnerable, and neutral nation during and after World War I. There is reason to believe that this situation, along with the activities of the institute, helped foment ideas of humanity, culture, and nationhood rather different from the *Völkisch* ideas that gained increasing support among German academics in the same period. This may to some extent explain the differences between the fate of the scientific concept of the Nordic race in Norway and Germany.

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Physical Anthropology in Japan

The Ainu and the Search for the Origins of the Japanese

by Morris Low

In this paper I examine the quest by physical anthropologists in Japan for the origins of the Japanese. A major focus of this research has been the Ainu people of the northern island of Hokkaidō, who have recently been declared an indigenous people of Japan. The relationship between mainstream Japanese and the very much living community of the Ainu has been the subject of over 100 years of research. Integral to research has been the collection of Ainu skulls, skeletons, and artefacts that have provided a critical if controversial resource for physical anthropologists. This has all been against the backdrop of changing political ideologies about the so-called purity of the Japanese. In the post–World War II period, with the loss of empire, the idea of Japan as a homogeneous nation took hold, and it was only in the last two decades that this notion has been discredited.

There has been a long-standing myth of a monolithic Japan bound together by a "unique" identity, culture, and language (Denoon et al. 1996). Other peoples residing in Japan have been forced to assimilate into this dominant culture or risk not being considered "Japanese." For over 100 years, physical anthropologists have been at the forefront of the quest to find the origins of the Japanese. A key argument of this paper is that physical anthropology in Japan has been part of a nationalist project that has sought to understand the nature of the Japanese people (Yamashita 2006*b*). But as we shall see, there have been multiple strands of nationalism within Japan. There were tensions between the dominant majority of the population known as "the Japanese" and the Ainu, one of the indigenous peoples of Japan. The struggle of the Ainu can be considered a type of indigenous nationalism (Siddle 2006). Other examples of nationalism can be found within Japan, most notably that of the people of Okinawa as well as *Zainichi* (Koreans in Japan) nationalism (Lie 2008). These ethnic minorities have increasingly sought to assert their rights, and their voices have provided a counternarrative to the notion of a mono-ethnic state.

In Japan, two terms in particular have been used to refer to biologically integrated groups of people, and they have become linked to nationalism. The term *jinshu* connotes "human breed" or "human race," and *minzoku* refers to "lineage of people," "nationality," and "race." *Jinshu* tends to refer more to people with shared physical attributes, whereas *minzoku* is closer to "ethnicity" or "ethnos" (Dikötter 1997:3; Weiner 1997:98). In the late nineteenth century, Japanese anthropologists were interested in the *jinshuteki* (racial) origins of the Japanese and relationships to the Ainu *jinshu* (race).

The establishment of Japan as a modern nation-state in the late nineteenth century saw the colonization of the northern island where the Ainu people largely resided and the renaming of it as Hokkaidō (fig. 1) in 1869. The Ainu also lived in adjacent territories known as North Honshū, South Sakhalin, and the Kurile Islands. In 1875, the Treaty of St. Petersburg resulted in the drawing of borders between Russia and Japan right through Ainu territory (Hasegawa 2010:209). Kuril Ainu and Sakhalin Ainu were forced to relocate to Hokkaidō, give up hunting, and become farmers. They were required to adopt Japanese names and be recorded in the national registry. From 1899 to 1997, the Ainu were dealt with by the Hokkaidō Kyūdojin Hogohō, sometimes translated as the Hokkaidō Aborigine Protection Act but more accurately written as the Hokkaidō Former Aborigines Protection Act. Under this law, the Ainu were no longer aborigines once they were assimilated. The law reinforced the notion of a mono-ethnic Japanese people (Creighton 2003:126).

Anatomists and archaeologists, who have been key figures in the development of physical anthropology in Japan, studied the Ainu, and this work constituted a major focus of their research. The historical connection between the Ainu and the modern populations living in Japan has and continues to be a much-discussed problem. Because of differences in physical appearance, mainstream Japanese have considered the Ainu to be racially distinct. Some scholars even considered them Caucasian (Low 1999).

The debate about the origins of the Japanese has thrown light on the fact that Japan is a multiethnic society. In the aftermath of World War II and the loss of empire, Japanese

Morris Low is Associate Professor of Japanese History, School of History, Philosophy, Religion and Classics, University of Queensland (c/LCCS, Level 3, Gordon Greenwood Building, University of Queensland, Brisbane, Queensland 4072, Australia [m.low@uq.edu.au]). This paper was submitted 27 X 10, accepted 16 VIII 11, and electronically published 7 II 12.

 0011-3204/2012/53S5-0006$10.00. DOI: 10.1086/662334

Figure 1. Map of Japan. Adapted from d-maps.com (http://d-maps.com/carte.php?lib = japan_with_ryukyu_islands_map&num_car = 4473&lang = en).

physical anthropologists looked to ethnic minorities within Japan, such as the Ainu and Koreans, as the objects of research. These groups, along with Okinawans, Burakumin (descendants of a former outcaste group), and the Chinese were estimated in the 1990s to make up 4–6 million of a total population in Japan of 125 million (Lie 2001:3–4). Although only 23,782 people identified themselves as Ainu in a survey conducted in 2006, it is estimated that the total population of Ainu consists of some 200,000 people (McGrogan 2010: 358). Many of these live in the cities, do not live "authentic" lifestyles, are of mixed descent, and have suffered considerable discrimination. Many now live outside of the island of Hokkaidō in and around cities such as Tokyo (Watson 2010).

Origins of Anthropology in Japan

Peter Bleed (1986) and others (Sakano 1999) have suggested that the interest in archaeology (and indeed physical anthropology) has its roots in the tradition of collecting and antiquarianism that can be traced back to the Tokugawa period (ca. 1603–1868), a time when the Japanese showed a regular interest in prehistoric remains. Michael Hoffman (1974) has drawn parallels between this interest in archaeology and antiquarianism in Japan and Western Europe during this time. In Japan, key figures were government official and historian Arai Hakuseki (1656–1725) and the scholar Tō Teikan (1731–1798).[1]

Tō examined how ancient Japanese *haniwa* figurines from burial mounds were dressed and argued that similarities with Korean clothing suggested that the founders of the Japanese imperial line were Korean. This controversial claim was an early attempt to use archaeological data to help solve historical problems, something that we see in the fascination of Japanese physical anthropologists with the Ainu and the search for the origins of the Japanese.

On December 9, 1872, it was reported that Japan would shift from a lunar calendar to a solar (Gregorian) calendar as of January 1, 1873. The solar calendar, it was argued, lent itself to greater accuracy. The new Japanese government also saw this as a civilizing measure that would facilitate exchange with the West (Tanaka 2004:5). Despite this belated "synchronization" with the West and the introduction of Western-style archaeology only in the Meiji period, the Japanese were not slow in appreciating the importance of the archaeological record in understanding the "past." As early as May 1872, the Ministry of Education sent out a team led by Machida Hisanari (1839–1897) to the Kansai region of Japan, where they visited Kyoto, Osaka, Nara, and elsewhere and conducted a 4-month search and survey of valuable old artefacts. The team of five included Uchida Masao (1842–1876; Ministry of Education) and Ninagawa Noritane (1835–1882), who worked for the Museum Bureau (Hakubutsukyoku) of the ministry (McDermott 2006:348–349). Ninagawa's diary and notes (Ninagawa 2005 [1872]) have recently been published. These documents are evidence of an early recognition by the government of the need to retain the past in order to secure Japan's future. As Stefan Tanaka (2004) eloquently puts it, "old things became a symbol of stability that grounds a changing society" (36).

It was not open-ended or curiosity-driven research but rather a quest for old things that could be used as evidence of the ancient origins of the imperial line (Tanaka 2004:33). After the Meiji restoration of 1868, the nation had come together with the Meiji emperor as symbolic head, so support for the idea of an unbroken imperial line only served to strengthen support for the new government and stabilize the country in response to the threat posed by Western powers (Edwards 2003:12). Also, there was a need to locate historical items that could be displayed at the 1873 Vienna International Exposition and that might also serve as models to inspire craftsmen wanting to create products for export (Guth 1996). In this way, tradition helped shape Japan's path to modernity.

In the late 1870s, Ninagawa befriended and taught a number of foreigners, including the American zoologist Edward S. Morse (1838–1923; Coolidge Rousmaniere 2002; Imai

1. Throughout the body of this paper, Japanese names are given in the normal Japanese order of family name followed by given name. All names in the list of references follow the normal order of family name first.

2004). Their friendship and diverse interests reflect how the divisions between antiquarianism, art, archaeology, and anthropology were at this time yet to be clearly defined. As Fumiko Ikawa-Smith (1982) has noted, those working in prehistoric archaeology in the late nineteenth and early twentieth centuries often had trained in zoology, geology, and medicine. What is more, they also had an interest in the biological identity of the people whose remains they excavated. It is clear that there was a tradition of constructing the past through antiquities and artefacts.

Morse was one of the key figures in the introduction of anthropology. He had studied under Louis R. Agassiz (1807–1873) at Harvard and was important in the development of not only zoology but also archaeology, physical anthropology, and ethnology through his teaching at Tokyo Imperial University in the years 1877–1879. Despite Morse's significant contribution, Tsuboi Shōgorō (1863–1913) is often credited with establishing anthropology in Japan.

Tsuboi and other Japanese had been affronted by the suggestion by Morse (1879) that the early Japanese had practiced cannibalism during the Jōmon period (ca. 11,000–ca. 300 BCE). Tsuboi felt that the origins of the Japanese should be studied by the Japanese themselves (Yamashita 2006*b*), and the overemphasis of Morse's contribution may have been felt keenly given the realization that there had been Japanese scholars doing relevant work even before Morse had arrived. In 1884, while only a graduate student in the Faculty of Science at Tokyo Imperial University, Tsuboi founded the Anthropological Society of Tokyo (renamed the Anthropological Society of Nippon in 1941). The society's activities have been described as being similar in "spirit of camaraderie and scientific curiosity to earlier European gentry antiquarian societies" (Pai 2009:268). Such was the interest that within 2 years, membership grew to over 200 people. It drew on preexisting intellectual networks of people with interests in antiquarianism and archaeology that previously had no specific disciplinary focus (Sakano 1999). The rapid growth in membership supported the publication of a journal that first appeared in 1886 as the *Tōkyō Jinrui Gakkai Hōkoku* (Report of the Tokyo Anthropological Society) and later as the *Tōkyō Jinrui Gakkai Zasshi* (Bulletin of the Tokyo Anthropological Society). In 1893, Tsuboi established the Institute of Anthropology at Tokyo. The work of the institute included physical anthropology, archaeology, and other areas. We can thus view Tsuboi as representing a transition from antiquarian curiosity to a type of anthropological archaeology (Kaner 2009:83).

Tsuboi argued that the origins of Japanese culture should be explored by the Japanese themselves rather than foreigners such as Morse. The 1880s were a time when the Japanese increasingly asserted their Japaneseness in what is considered the first expression of modern nationalism in Japan (Lie 2001: 39). Tsuboi helped set the nationalistic agenda of anthropology to one focussing on the origins of the Japanese rather than the entirety of humankind (Yamashita 2006*b*:177).

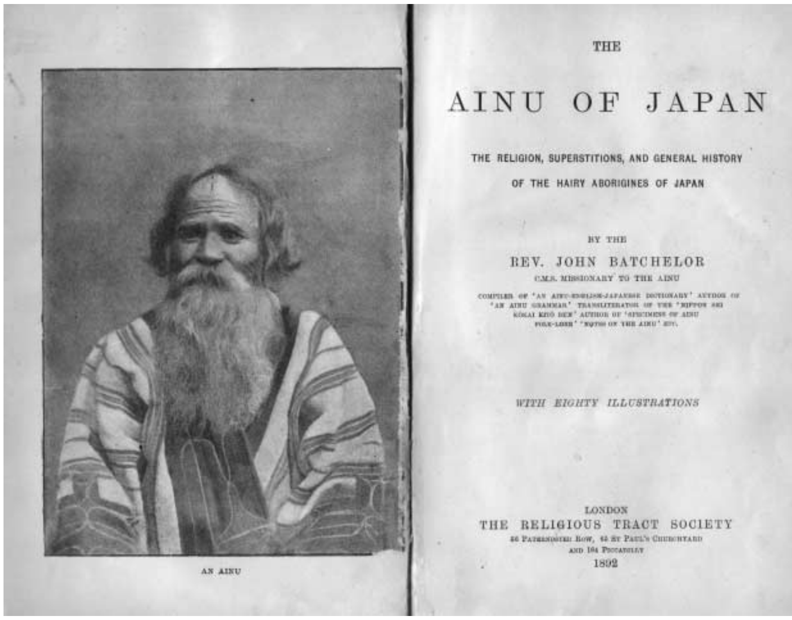
Despite attempts by conservative thinkers to promote a myth of Japanese homogeneity centering on the role of the emperor, there was a diversity of views among scholars regarding the origins of the Japanese. A central debate has been whether or not the Ainu are living vestiges of the Neolithic Jōmon people, the earliest Japanese. In the early nineteenth century, the German physician Philipp Franz von Siebold (1796–1866) wrote about the Ainu while working at the Dutch settlement at Dejima, in Nagasaki (Siebold 1828). He argued that the Ainu could be traced back to Japan's Neolithic people. His work and that of his son Heinrich von Siebold (1852–1908), who lived in Japan after his father's death and served as an Austrian diplomat there, helped to spread knowledge of the Ainu in the West (Askew 2004:71; Refsing 2000:16–18, 44–47; Siebold 1881).

Ainu Studies

Tsuboi saw the Japanese as a mixture of races. In the *Report of the Tokyo Anthropological Society*, he suggested that the Koropokguru (also known as the Korobokkuru or Koropokgru) people were the Jōmon people and were unrelated to the Ainu (Tsuboi 1887). "Koropokguru" was a term used by the Ainu to refer to an earlier people of short stature who had lived in Hokkaidō. Over the next 25 years, over 200 articles on Ainu-related topics would appear in the Tokyo Anthropological Society's journal (Siddle 1997).

Tsuboi and his colleague Koganei Yoshikiyo (1859–1944), who had previously studied anatomy in Germany between 1880 and 1885 and had subsequently been appointed professor of anatomy at the Tokyo Imperial University Medical School, went to Hokkaidō for two months in 1888. Tsuboi found further evidence to support his Koropokguru theory, and Koganei found evidence to support his Ainu theory (Hamada 2006:54). In 1889, Koganei returned to spend a further 3 months conducting research. After excavating and collecting Ainu skeletal remains (166 skulls and 92 skeletons) and carefully examining them, Koganei (1894) published one of the most comprehensive studies of the Ainu. He went on to compare Jōmon skeletal remains with that of the Ainu and challenged Tsuboi's hypothesis, pointing out similarities between the Ainu and Jōmon people. He argued that the Ainu had previously lived throughout Japan but had been forced on to the northern island of Hokkaidō by the Japanese (Hanihara 1991; Yamaguchi 1997).

Around this time, the Reverend John Batchelor (1854–1944), a missionary who had lived in Hokkaidō for 64 years and befriended many Ainu, was publishing his work on the Ainu. As the title of his book *The Ainu of Japan: The Religion, Superstitions, and General History of the Hairy Aborigines of Japan* (1892) suggests, a feature of their physical appearance is considered to be the relative abundance of hair compared with the culturally recognized category of "the Japanese" who have lived on the main island of Honshū (see figs. 2, 3 for


AN AINU

THE

AINU OF JAPAN

THE RELIGION, SUPERSTITIONS, AND GENERAL HISTORY

OF THE HAIRY ABORIGINES OF JAPAN

BY THE

REV. JOHN BATCHELOR

C.M.S. MISSIONARY TO THE AINU

[illegible]

WITH EIGHTY ILLUSTRATIONS

LONDON

THE RELIGIOUS TRACT SOCIETY

56 PATERNOSTER ROW, 65 ST PAUL'S CHURCHYARD

AND 164 PICCADILLY

1892

Figure 2. Frontispiece and title page from Rev. John Batchelor's *Ainu of Japan* (1892).

images of Ainu from Batchelor 1892). While sympathetic to the plight of the Ainu, he nevertheless wrote:

> The chief thing that strikes one on meeting an Ainu for the first time is his fine beard, moppy hair, and sparkling eyes; next, his dirty appearance, poor clothing, and, should he be near at hand, his odour, The Ainu certainly do not, upon first acquaintance, produce a very favourable impression; in fact, to many people, they quickly become repulsive, especially on account of their filth. (Batchelor 1892:18)

Attesting to the belief in their possible Caucasian origins, he noted that "the skin is whiter than that of the Japanese, for they do not possess the bilious-looking complexion so prevalent in the latter race" (Batchelor 1892:20).

Anthropology and Empire

The work of Tsuboi, Koganei, and Batchelor helped construct an image of the Ainu as a primitive race that was considered racially immature. Their studies of the Ainu helped to define the mainstream Japanese as a modern and civilized race that was entitled not only to colonize Hokkaidō but also to expand its empire to Korea and Taiwan. Tsuboi designed a Hall of Mankind (Jinruikan) for the Fifth National Industrial Exposition held in Osaka in 1903 that aimed to show the races of the world in their "natural" settings, but not surprisingly, Chinese, Koreans, and Ryūkyūans (Okinawans) objected to the representation of their cultures as "primitive" (Weiner 1997:112–114). Their opposition to Tsuboi's proposal was partially successful in that only five Ainu, four Taiwanese aboriginals, and two Ryūkyūans were put on display. Visitors contrasted the lives of these indigenous people with those of modern "civilized" Japanese (Yamaji 2008:49).

"Native villages" were also a feature of the Louisiana Purchase Exposition. In August 1903, W. J. McGee, chief of the Department of Anthropology at the exposition, contacted Frederick Starr (1858–1933), professor of anthropology at the University of Chicago. Starr was requested to go to Japan and secure the voluntary participation of eight to 10 Ainu in native villages at the exposition to be held at St. Louis in 1904. Starr duly agreed and arrived in Japan in February 1904. While in Tokyo, Tsuboi showed Starr the large collection of Ainu skeletons that Koganei had amassed. With Batchelor's help, Starr was able to recruit nine Ainu for what became a popular feature of the exposition (Medak-Saltzman 2008; Oppenheim 2005:681; Starr 1904:4; Vanstone 1993). There were also Ainu on display at the Anglo-Japanese Exhibition held in London in 1910, but not all Japanese were comfortable with such representations.

A member of the Tokyo municipal council complained in the *Japan Chronicle* on July 24, 1910, that the exhibition of Ainu and Taiwanese natives in their humble hut dwellings could be regarded as infringing on their personal rights. The historian Ayako Hotta-Lister (1999:133–134) suggests that rather than being troubled by concerns regarding their welfare, the official and other members of the Japanese elite feared that they would be accused of ill treatment of the indigenous people and that such representations might backfire on the Japanese.

Populations seen as prehistoric or living history—suitable for display—were a resource for an imperial nation and for anthropological scientists. In an essay written in November

Figure 3. "Our Ainu Servants," from Rev. John Batchelor's *Ainu of Japan* (1892).

1906 entitled "Primitive Life and Presiding Death in Korea," the Japanese intellectual Nitobe Inazō (1862–1933) lamented the racial decline of the Koreans in the following way: "The very physiognomy and living of this people are so bland, unsophisticated and primitive that they belong not to the twentieth or the tenth—nor indeed to the first century. They belong to a prehistoric age" (Nitobe 1909:214).

The idea of the Japanese as being of mixed racial origins was used to help justify the annexation of Korea in 1910. Common ancestral origins were used as a pretext for colonial expansion. The rise of the Japanese empire, in turn, provided fieldwork opportunities for Japanese physical anthropologists and enabled them to further explore the origins of the Japanese through comparative body measurements of different ethnic groups. In 1912, a Colonial Exposition (Takushoku Hakurankai) was held in Ueno Park, Tokyo, to show how far Japan and its colonial subjects had come. Included in the displays were a traditional Ainu home complete with Ainu family (Yamaji 2008:51–52).

Torii Ryuzō (1870–1953), who had studied under Tsuboi, went to Manchuria and the Liatong Peninsula in 1895 after the Sino-Japanese War, to Taiwan in 1897 after annexation by Japan, to Manchuria in 1905 during the Russo-Japanese War, and to Korea in 1910 and 1912 after it, too, was annexed by Japan and made a formal colony (Askew 2004:60). Torii claimed that both the Ainu and the Japanese had coexisted in the Neolithic period and that the Ainu were racially inferior. This was used to bolster the idea of Japanese racial superiority and to justify colonial expansion into East Asia.

In the two decades that followed, there was a diversity of opinions. In the 1910s, Koganei came to support the idea of hybridization between the Japanese and other ethnic groups. In the late 1920s, Kiyono Kenji (1885–1955) of Kyoto Imperial University excavated Ainu remains in Sakhalin (fig. 1) and surmised that the Neolithic people were ancestors of both the Ainu and the Japanese (Hamada 2006:66). Kiyono was important for applying statistics to biometric data, and his collection of more than 1,000 skeletal remains was an important resource (Hanihara 1992:135).

Japanese physical anthropologists sometimes engaged in public debates about Japanese imperialism. Although Kiyono's writing was nationalistic and supported it, Torii opposed it, and Koganei was wary. This did not, however, prevent their work from being appropriated by others. There were Japanese who were working elsewhere in the Japanese empire who found that their research supported colonial policy. Ueda Tsunekichi was a professor in the Department of Anatomy at Keijō Imperial University in Seoul, Korea. The department was founded in 1924 and became a center for physical anthropology in Korea and part of a Japanese physical anthropology network. Ueda helped to introduce statistical techniques. He and his colleagues conducted a major survey of living Koreans from 1930 to the end of World War II. They argued that there were fewer physical differences between Koreans from middle Korea and Japanese in the Kinki area than there were among Japanese found in different areas in Japan. Such findings helped bolster Japan's assimilation policy that was promoted in Korea (Nobayashi 2003:145–146).

Japan as a Homogeneous Nation

In 1936, Hasebe Kotondo (1882–1969) was appointed to the chair of anthropology at Tokyo Imperial University. As we have seen, the university had been the leading center for anthropology in Japan. Hasebe established the Department of Anthropology there in 1939. The department was important in training physical anthropologists in Japan, many of whom would go on to publish in what became known as the *Jinruigaku Zasshi* (Journal of the Anthropological Society of Nippon), or *Anthropological Science* in English. The journal became an outlet for many papers on Korean physical anthropology written by Japanese anthropologists who taught at Keijō Imperial University, which had been established in Seoul in 1927 (Nakao 2008 [2005]:22).

Hasebe's appointment marked a significant shift to more of a biometric approach that included not only minorities such as the Ainu and colonized people but also the majority Japanese people themselves (Morris-Suzuki 1999:362–363). Kiyono and Hasebe used physical anthropology to argue that the Japanese had evolved from the Jōmon people with very little mixing from outside. In this way, they challenged the idea of the mixed-nation view of the Japanese that had been dominant (Oguma 1995, 2002).

Postwar Studies

The origins of the Japanese continued to be an important topic in Japanese physical anthropology even after World War II. The emperor's renunciation of previous claims to divinity was followed by revision of school history textbooks, where the mythic origins of Japan were replaced with material on archaeological findings about the Jōmon period (Edwards 2008 [2005]). But the loss of empire and restrictions on overseas travel by Japanese meant that Japanese anthropologists were largely limited to studies of the living Japanese population, conducting fieldwork on the Ainu in Hokkaidō and on the Okinawans in the Ryūkyū Islands (Yamashita 2006*a*).

Oguma (1995, 2002) argues that the idea of a homogeneous nation was emphasized in the early postwar period to promote the idea of the Japanese as a peace-loving homogeneous people who had been largely dependent on agriculture. Such arguments were used to help justify the repatriation of "non-Japanese" after the war. Building on Oguma's work, Arnaud Nanta (2008) has recently shown how after the war, physical anthropology was used to promote the idea of the homogeneity of the Japanese and to deny that the Ainu were the indigenous people of Japan. From the late 1940s, Hasebe developed an argument that the Japanese had directly descended from the Jōmon people without any intermixing in the Yayoi period (ca. 300 BCE–ca. 300 CE).

In the 1950s and 1960s, at the University of Tokyo, Suzuki Hisashi (1912–2004), who had studied with Koganei and Hasebe, sought to follow Hasebe's general scheme and promoted the notion of the Japanese as a homogeneous (and unique) people using craniometrical analysis of thousands of skeletons that had been discovered in the ruins of a war-devastated Japan. The idea that there had been uninterrupted genetic continuity of the Japanese since the Jōmon period remained the official position of the Anthropological Society of Nippon until the early 1980s, reflecting Hasebe's long-term authority as president from 1951 to 1968 until just before his death and Suzuki's influence as president from 1970 to 1976 (Nanta 2008).

This tendency to promote the idea of a homogenous nation and unique culture, especially from the 1960s, came amid renewed confidence in the Japanese against the background of rapid economic growth. The removal of overseas travel restrictions in 1964 saw Japanese anthropologists conducting fieldwork in more distant lands in parallel with Japan's economic expansion (Yamashita 2006*a*). The 1960s also saw the introduction of computers, which assisted in statistical analysis. The development of molecular biology gave rise to genetic studies of Ainu and other populations. The combination of genetic and morphological studies suggested that the ancestors of the Ainu were likely to have been the Jōmon people (Hanihara 1992).

What other research did physical anthropologists conduct in postwar Japan? If we examine volume 67 of the *Journal of the Anthropological Society of Nippon*, published in 1959, we can gain a sense of their priorities and the major issues at that time. In the first issue of volume 67, Shima Gorō (Shima 1959) from the Osaka City University Medical School wrote on the toe and finger prints of living Sakhalin Ainu and mixed Ainu who were evacuated from Sakhalin to Hokkaidō after the war. Karafuto (Southern Sakhalin) had been a Japanese colony, but with Japan's defeat, the whole of Sakhalin came under Soviet control.

Shima examined the frequency of certain configurations on the toes and fingers of Ainu and the majority Japanese. The study found that Sakhalin Ainu showed no difference when compared with Sakhalin Ainu-Japanese "hybrids." He concluded that even the data "belonging to the so called Ainu may be constitutionally classified as those of hybrids" because "they are not so different from those of Ainu hybrids and they are far short of being Ainu-like" (Shima 1959:10). In other words, Shima found that the Sakhalin Ainu in his study were far from being the "pure" native Sakhalin that he had hoped for. Shima's realization of the racial mixing of the people of the Japanese islands contrasts nevertheless with attempts by other scholars to portray the Japanese as homogenous.

In a later issue of the *Journal of the Anthropological Society of Nippon* that year, Watanabe Saburō and Yamazaki Fusao (1959) from the Department of Anatomy at Sapporo Medical College, Hokkaidō, managed to gain access to Ainu corpses, although how they did so remains unstated in their paper. They examined tattooed skin taken from the upper lip of an elderly Ainu woman and compared it with the tattooed skin taken from the upper arm of a middle-aged Japanese man.

They attributed histological differences to the differences in tattooing method. The Ainu used incision whereas the Japanese used puncturing.

In the April 1959 issue of the journal, Kōhara Yukinari of the Anatomical Institute, School of Medicine, Shinshū University, reported on research that had been funded by a Ministry of Education Grant in Aid for Scientific Research. He examined the motor performance of 480 males and females in the two mountain villages of Kawakami and Kawashima in Nagano prefecture. All were aged 20 years or over. The inhabitants of both villages performed alike apart from males aged 20–34 years in Kawakami who were particularly agile. Kōhara (1959:72) suggested that the villagers of Kawakami were "more adaptive to [the] natural environment" and were not only smarter and more intellectual but were quicker to adopt new modes of life and industry, especially in agriculture and forestry. In this way, Kōhara drew a link between overall motor skills and responsiveness to modernization. The tendency for Kawashima villagers to be "passive and conventional" had resulted in the younger generation leaving the village for life and employment elsewhere. For a Japan that was rebuilding after the war and intent on pursuing rapid economic growth, this was a reminder not to be complacent.

Many of the two million Korean colonial subjects in Japan returned home at the end of the Pacific war. In the years from 1959 to 1961, some 70,000 Koreans left. Despite this, some 600,000 chose to remain behind. What is more, it appears that tens of thousands of illegal Korean migrants made their way to Japan in the years between 1946 and the 1970s (Morris-Suzuki 2006*a*, 2006*b*). In the April 1959 issue of the journal, a paper on Korean migrants by Kohama Mototsugu, Kamada Naoshige, Furuya Takashi, and Tsubaki Ikuto of the Department of Anatomy, School of Medicine, Osaka University, was the lead article. Kohama and his colleagues reported how they had conducted a study at Shimonoseki, on the tip of the main island of Honshū, back in 1955. They compared Koreans who had emigrated to Japan in the last 10–40 years (during the colonial period) and compared them with Koreans that Kohama had measured in South Korea and Japanese that Kohama had measured in Yamaguchi prefecture. Interestingly, the height of the Korean emigrants was the highest of all three groups. The two groups of Koreans had shorter head length and wider head breadth when compared with the Japanese from Yamaguchi. No discernible differences between the Korean groups with respect to head measurements were observable. The paper concluded that immigration to Japan had affected the other bodily characteristics of Korean emigrants that resembled the Japanese. The paper does not attempt to account for the differences between the two groups of Koreans, but one can only surmise that better nutrition in Japan may have been a factor. During the colonial period and in wartime, much agricultural produce from both Korea and Taiwan was sent to Japan. Both countries accounted for the bulk of the rice that was imported into Japan. In a way, the paper by Kohama et al. (1959) measured the cost of empire on colonial subjects who were deprived of adequate nutrition in order to feed Japan. In this way, the Japanese sought to examine social problems through physical anthropology and biometrics.

It is not surprising that anatomists based at medical schools dominate the pages of the journal. This was the case for authors of the third paper in the April 1959 issue on "Japanese-American hybrids" written by Hoshi Hiroshi from the Department of Anatomy, Faculty of Medicine, University of Tokyo. In the final months of the Allied occupation of Japan in 1951, a longitudinal growth study of Japanese-American mixed-race children was begun by a research team led by Suda Akiyoshi, who was based in the Department of Anthropology, Faculty of Science, at the same university. Hoshi was one of the experts who participated in the project, which examined more than 130 boys and girls from the Elizabeth Sanders Home in Kanagawa prefecture and an additional 40 boys from Boys Town, also in Kanagawa. The children had been born in the wake of Japan's defeat and because of fraternization between American servicemen and Japanese women during the occupation (Burkhardt 1983).

Hoshi's study involved 14 girls who had been measured twice a year for 5 or 6 years and were at the time aged between 10 and 11-and-a-half years of age. Half were of Japanese and white American parentage, and the other half were of Japanese and African-American parentage. It was noticeable that the growth rate of the mixed-race children exceeded that of the Japanese. The two groups of mixed-race children were compared, and differences in face and head measurements were observed with the conclusion that "racial characteristics appear first of all in the nose and mouth region in the infantile period" (Hoshi 1959:30).

The final scientific paper was by Kimura Kunihiko, Hagiya Shukuko, and Kitano Shinsei of the Department of Anatomy, School of Medicine, Tōhō University. In their paper they examined the physique and growth of Japanese children during and after World War II. A major concern had been the incidence of malnutrition among children and youths. The study focussed on height. They found that wartime deprivation, especially in infancy and childhood, had the effect of slowing down growth before adolescence and retarding adolescence. The effect was more pronounced in males than females. Females, they concluded, seemed to have stronger "resisting power to the war privation and bad living conditions" (Kimura, Hagiya, and Kitano 1959:39).

In I961, Sofue Takao speculated as to the reasons for why the Anthropological Society of Nippon and the Department of Anthropology at the University of Tokyo, two key institutions, focussed on physical anthropology and prehistory for so long. He attributed this partly to how the term "anthropology" came to be understood in the German sense of the word, that is, more along the lines of physical anthropology in the United States. Also, what is striking is that many of the graduates of the Department of Anthropology at Tokyo, the authors of the articles published on physical anthropology, and members of the society were anatomists at medical schools. The authorship of the papers referred to above reflect this tendency. As Sofue (1961) explained, the Department of Anthropology was well into the twentieth century "essentially an institute for physical anthropology" (174).

Collection and Return of Ainu Skeletons

The remainder of the rather remarkable April 1959 issue of the *Journal of the Anthropological Society of Nippon* was devoted to the proceedings of a meeting to celebrate the centenary of the birth of Koganei Yoshikiyo. Even for anthropologists wishing to study living Ainu communities, the collection of Ainu remains and artefacts in the past has been a source of controversy because it encouraged Ainu to refuse to cooperate. It is not surprising, given the actions of researchers such as Koganei.

In the decades following Japan's colonization of the northern island of Hokkaidō in 1869, a considerable traffic emerged in skulls as commodities, and local networks emerged to support this activity. Related artefacts, oral histories, and language data were also gathered, sometimes surreptitiously. The anatomist Koganei offered medical treatment to the Ainu despite having no clinical experience. Researchers masqueraded as medical men to obtain blood samples to "solve" smallpox epidemics. Koganei (1935) reminisced how he had secretly collected Ainu skulls by excavating graves at night in Hokkaidō and washing away flesh and skin in nearby streams (Bogdanowicz 2002). Kiyono, too, became a little too zealous in his collection of artifacts and was arrested in 1937 for stealing items from temples and shrines, after which he left Kyoto Imperial University.

In the 1930s, a major project on ethnobiology was conducted under the auspices of the newly established eugenic lobby group known as the Ethnic Hygiene Association of Japan (Nihon Minzoku Eisei Gakkai, later known as Nihon Minzoku Eisei Kyōkai). The association sought to improve the quality of the Japanese race amid concerns regarding the intermarriage of Japanese with other races that were deemed to be less advanced (Morris-Suzuki 1998:96; Otsubo and Bartholomew 1998:557). Kodama Sakuzaemon (1895–1970), a physical anthropologist and professor of anatomy at Hokkaidō University, was a key member of the association (Sakano 2005:204).

Kodama's excavation of Ainu graves and large-scale collection of skeletal remains was particularly problematic. When he joined Hokkaidō University as a professor in the School of Medicine in 1929, he was dismayed to find a small collection of only 15 skulls. He set about conducting excavations at "abandoned" graves and cemeteries in Hokkaidō, South Sakhalin, and the Kurile Islands. These mass excavations resulted in Kodama collecting over 1,000 Ainu skeletons. He made careful measurements of the skeletons, with special attention to the skulls and any possible regional differences. He lamented that "it is very hard for us to obtain the bones of the Ainu race, but it is much harder for us to procure their corpses" (Kodama 1970:184).

Local medical clinics and hospital elites provided access to Ainu patients for anthropometric measurements, and even the village police cooperated by blocking Ainu protestors who attempted to interfere with Kodama's grave digging. What emerged were collection networks in which even stationmasters alerted Hokkaidō University whenever there was a funeral. Bodies were decapitated and graves left in disarray, robbing the dead of any dignity. Such activities had the backing of the Japanese state. Kodama received financial support from the Japan Society for the Promotion of Science soon after its establishment in 1932 (Howell 2009; Lewallen 2007; Siddle 1993; Ueki 2008).

References to the Ainu as a "dying race," even by their friend Rev. John Batchelor (1927) in his book *Ainu Life and Lore: Echoes of a Departing Race*, suggest passivity. This was, however, not the case. There have been demonstrations of Ainu agency and resistance from the beginning. Even in Edward S. Morse's published journal, written in the late 1870s and early 1880s (Morse 1917), he wrote

> Looking up I saw, at a distance of fifty or seventy-five yards, a number of hairy Ainus, in a row, shouting at me and gesticulating. . . . Then it suddenly occurred to me that they thought I was hunting for their graves, which they defend even to the extent of murder, and recalling the deadly poison of the arrow tips I reluctantly got up and walked away. (12)

He later discovered that they were actually warning him of a bear trap that they had set up and were concerned that he might get caught up in. Nevertheless, Morse clearly had knowledge of the prevalence of grave robbing and how the Ainu people strongly objected to it.

What is more, the recent controversy surrounding the discovery of the Kennewick Man (Fiedel 2004; Weiss 2001) and the possibility of an Ainu connection reveal that the Ainu have been a vibrant people who were, in the past, important actors in North Pacific trade. It has also, however, given new credence to the discredited idea of the Ainu as descendants of proto-Europeans that somehow got lost and made their way to Asia. Such ideas were common in the late nineteenth century and were used to argue that the Japanese were in fact white. Kodama categorized himself as a proponent of the Caucasoid theory, arguing that the full-blooded Ainu "are closer to Europeoids than to Mongoloids," citing wavy and abundant hair, skin color, deep-set eyes, and other features, but "much more primitive than the modern Europeoids" (Kodama 1970:265).

However, not only were some of Kodama's collecting methods problematic but also the authenticity of some items can be questioned. For example, in 1865, British consul captain Vyse excavated skeletons at Mori, Hakodate. Ainu villagers demanded their return and reburied 17 skulls that were eventually repatriated from London. But apparently not all were returned, because three skulls were found in the London Museum of Natural History in 1993. The Ainu had been sent some imposter bones. The reburied bones (including the imposter bones) were reexcavated by Kodama Sakuzaemon, so the joke was ultimately on him in what was an act of comedic skulduggery that showed little respect and sensitivity for Ainu culture (Howell 2009; Lewallen 2007; Ueki 2008).

The last few decades have seen a rise in indigenous nationalism against the backdrop of increased activism by minority groups in Japan. There have been Ainu liberation campaigns against Ainu scholars as a form of retribution for having extracted Ainu blood and excavated skeletons (Siddle 1999). Ultimately, the skeletons have been used by scholars for what can be regarded as nationalist purposes, and the Ainu have increasingly refused to cooperate. Today, the skeletal remains and artefacts discussed in this paper are housed at universities and museums throughout Japan along with collections of skeletons assembled from cadavers used to teach medical students. The Ainu skeletons that Koganei collected continue to be a useful resource at the University Museum, University of Tokyo (Ossenberg et al. 2006), but such collections continue to court controversy. In 1987, the fate of remains collected by Kodama was the subject of negotiation between Hokkaidō University and the Ainu Association of Hokkaidō. To apologize for past wrongs, the university built a charnel house on university grounds to store the remains that are now technically owned by the association. A small number of remains have been returned (Bogdanowicz 2002). Japan's colonial expansion into Asia also facilitated the collection of specimens and artefacts from other cultures, but their repatriation is a problem that Japan has yet to adequately deal with.

Physical anthropology was a tool that conveniently provided metaphors of mixed origin at the time of the growth of the Japanese empire. Ironically, continuing debate regarding the origins of the Japanese has contributed to a neglect of the significance of the Ainu and any claims that they might have to land rights (Stevens 2001). Recent evidence suggests that the Ainu are closely related to the Neolithic Jōmon people (Tajima et al. 2004). This corresponds to some of the early Ainu theories proposed by pioneers of Japanese anthropology such as Philipp Franz von Siebold and Koganei Yoshikiyo (Imamura 1996:160).

Japan and Southeast Asia

The major question driving research has been whether most Japanese today have descended from the Jōmon people or from later immigrants around the time of the Yayoi period (Kumar 2009:74–75). Modern Japanese appear to be descendants of both the Jōmon and Yayoi populations, but the relative genetic contribution of each and their geographic origins are still hotly debated (Hammer et al. 2006; Hanihara and Ishida 2009). Recent research points in the direction of origins in Southeast Asia. Hanihara Kazurō's paper on his "Dual Structure Model for the Population History of the Japanese" (1991) argued that the ancestors of the Jōmon people came from Southeast Asia and that a second wave of immigrants came from Northeast Asia beginning in the Yayoi period.

In contrast, a paper by Yongyi Li et al. (1991) published that same year in the *American Journal of Physical Anthropology* was surprised to find evidence that the Yayoi people originated in Southeast Asia. This was backed up by Hammer et al. (2006), whose research has found that the ancestors of the Yayoi people are of Southeast Asian origin. Some physical anthropologists have claimed that more than three quarters of the Japanese gene pool can be traced to the Korean peninsula (Edwards 2000:380).

How do we resolve this? A recent study published in *Science* on December 11, 2009, by members of the Human Genome Organization's Pan-Asian SNP Consortium suggests that Asians probably originated from Southeast Asia and then migrated northward, with the Japanese arriving via the Korean peninsula (An 2009; Normile 2009). Furthermore, the study shows that genetic ancestry is strongly correlated with linguistic affiliations and geography (HUGO Pan-Asian SNP Consortium 2009). Ann Kumar (2009:2) has also recently argued that scientific evidence in the form of DNA, rice genetics, and historical linguistics point to immigrants from Indonesia rather than Korea or China as having been responsible for the transformation of Japan from a hunter-gatherer society in the Jōmon period to one more dependent on agriculture in the Yayoi period. Research continues, but it is clear that through the combined study of DNA and older methods of physical anthropology such as craniofacial metrics, we will come closer to understanding the origins of the Japanese.

The nationalistic Japanese preoccupation with the Ainu and the detailed description of the physical characteristics of the population according to region or district has been seen as contributing to a general lack of international recognition for Japanese anthropology in the past (Frisch 1963:223; Yamashita 2006*a*:29). This has served to reinforce the semiperipheral position of the field vis-à-vis the center.

The Ainu as an Indigenous People

The idea that the roots of Japanese people can be found in the Jōmon period and in Ainu culture has been promoted by the nationalistic intellectuals Umehara Takeshi and Umesao Tadao (Habu and Fawcett 1999:591) as well as the physical anthropologist Hanihara. The Ainu are seen as remnants of a proto-Japanese culture that failed to evolve over the last 1,500 years (Howell 1996:175; Siddle 2003:461; Sleeboom 2004:55–56; Umehara and Hanihara 1982) and are thus subsumed under the broad banner of "the Japanese." Ainu culture thus becomes part of Japanese culture. This has worked against the efforts of Ainu activists to have the Ainu recognized as an indigenous people with their own history. In 1986, prime minister Nakasone Yasuhiro claimed that Japan had no racial minorities in the sense that Ainu had assimilated into the Japanese population. His own bushy eyebrows were cited as evidence of this, along with the work of Umehara Takeshi (Siddle 2003:449).

On March 27, 1997, some progress was made. The Sapporo District Court recognized that the Ainu fit the legal category of indigenous people. This landmark decision was the result of action taken by Ainu activists against the Hokkaidō Development Agency's attempts to forcibly resume their land, including burial sites and other sacred sites, in order to build the Nibutani Dam. Nibutani was one of the few villages in Hokkaidō that was predominately Ainu. Meanwhile, the government enacted the Ainu Cultural Promotion Act. While initially welcomed, it has since been criticized for emphasizing a traditional version of Ainu culture as being authentic (Siddle 2002, 2003:455–459, 2009:33–34).

Epilogue

On Friday, June 6, 2008, the Japanese parliament belatedly passed a resolution that recognized the Ainu as one of the indigenous peoples of Japan (Ito 2008; Onishi 2008). Given the long-standing belief that the Japanese were an ethnically homogeneous nation, this was a historic moment in the history of the search for the origins of the Japanese. The Japanese do not necessarily acknowledge that they are descended from the Ainu, but they do now see the Ainu as somehow being part of the racial mix even if scholars are not sure of the details. Reflecting this uncertainty, the Japanese parliament remains reluctant to support the rights of the Ainu as an indigenous people.

In contrast, the long-standing interest in Japanese physical anthropology in Japan can be seen in popular science publications (Newton Graphic Science Magazine, Shinoda, and Ōtsuka 2009) and among the Japanese public who have avidly taken to the idea that Japanese can be divided into two physical types. In several museums in Japan, visitors can use a computer game to determine whether they are more a Jōmon type of Japanese or a Yayoi, depending on answers to questions such as the size of one's nose and the like (Hudson 2006:421). While such games reinforce simplistic ideas about the origins of the Japanese, they do indicate a shift away from notions of the Japanese as a unique homogeneous people—a welcome development that reflects over 100 years of debate and controversy regarding where the Japanese came from. It is an acknowledgment that the origins of at least some of the Japanese people may be traced back to the Jōmon period and that there is, indeed, cultural and physical diversity in Japan (Habu and Fawcett 1999:588).

The history of physical anthropology in Japan shows us that Japan is not a homogeneous nation-state. Rather, there are multiple nationalisms and a multiethnic state that includes the Ainu people. The Ainu's resistance provides us with a counternarrative to that of Japanese nation building and claims of a unique "Japanese" culture. Much work remains to be done on elucidating the relationship between the Ainu and the modern populations living in Japan. It is important for anthropologists to continue to work with Ainu communities but in a way that involves consultation, cooperation, and collaboration and in a manner that strives to achieve positive benefits for a people who have been wronged in the past and deserve much better.

Acknowledgments

I am grateful for the constructive comments provided by two anonymous readers and the helpful advice of the guest editors of this supplemental issue of *Current Anthropology*.

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Isolates and Crosses in Human Population Genetics; or, A Contextualization of German Race Science

by Veronika Lipphardt

Historians have drawn a line between scientific racism, exemplified in the typological approach of German race scientists, and population-based approaches toward races or human genetic diversity. The postwar time is often understood as a watershed in this respect. My argument is that typological and population-based race concepts cannot be so easily segregated either before or after World War II. In spite of noteworthy differences between the two, on closer inspection, one finds population-based concepts in German race science before World War II as well as typologies and typological aspects in human population genetics after World War II, and continuities between them. In this paper I aim at viewing German race science in its contemporary international context up to the 1960s. With regard to its theoretical groundings, research problems, research designs, methods, practices, results, and interpretations, German race science was far more embedded in contemporary research on human diversity around the world than is generally assumed. Most notably, researchers in the field have been preoccupied with identifying and examining "isolated" and "mixed" populations from the mid-nineteenth century until the present. Yet instead of rendering German race science harmless, this contextualization aims at drawing attention to the generally precarious aspects of research into "human variation" or "human diversity."

Much has been written and said about German racial science. For good reasons, most commentators agree that it simply cannot be viewed as proper science. Recently, this understanding has been elaborated on and differentiated, with historians now contending that although some German race scientists were using methods that were internationally considered state-of-the-art, their political agenda disqualified their scientific work (Schmuhl 2008; Weiss 2006).

However, the underlying understanding of science still seems problematic to me. It suggests that one could potentially approach human biological diversity in an "objective" way. Nancy Stepan rightly claims that any attempt to produce scientific facts about "race"—and, we might extend her view, about human biological diversity—is already part of a political argument (Stepan 2003:334) or grounded in social tensions. And yet, the quest for objectivity is as old as race science itself. Some German scientists—as scientists in other countries—invested much energy into research designs and representations that would count as "objective" and "purely scientific."

Veronika Lipphardt is Research Group Director at the Max Planck Institute for the History of Science (Boltzmannstraße 22, 14195 Berlin, Germany [vlipphardt@mpiwg-berlin.mpg.de]). This paper was submitted 27 X 10, accepted 30 VIII 11, and electronically published 16 II 12.

Hence, if we view early twentieth-century German race science in its contemporary international context up to the 1960s, an assessment proves to be complicated. Physical anthropologists and human geneticists worldwide considered human evolution and human diversity core issues of their fields. Human heredity of normal as well as pathological traits moved center stage in this respect. As I want to demonstrate, German race science—with regard to its theoretical groundings, research problems, research designs, methods, practices, results, and interpretations—was far more embedded in contemporary research on human biological diversity around the world than is generally assumed. With this approach, I do not mean to render German race science harmless—quite the opposite. Rather, I aim to draw attention to the generally precarious aspects of research into "human variation" or "human diversity," as life scientists have come to label it.

Historians (and famous biologists, most notably Ernst Mayr) have drawn a line between scientific racism, exemplified in "typological race science"—German race science is generally seen as the most notorious example—and population-based scientific approaches toward races or human genetic diversity. The political and epistemological rejection of race science after 1945 went hand in hand; however, as Stepan (2003:239) points out, it was a "slow, hesitant, piecemeal and very incomplete" process. Although 1945 is no longer seen as a clear break between the two, the postwar time is still

 0011-3204/2012/53S5-0007$10.00. DOI: 10.1086/662574

understood as a watershed in this respect. My argument is that typological and population-based race concepts cannot be so easily segregated either before or after the Second World War. This is not to say that there were no noteworthy differences between the two. But on closer inspection, one finds population-based concepts even in German race science before World War II, typologies and typological aspects in human population genetics after World War II, and some unexpected continuities.

Postwar Critiques of German Race Science

Historians have concentrated on two aspects of German race science that were doubtless hazardous and dangerous: eugenics and scientific racism. This approach might be sufficient for historical actors who were, more than anything else, popularizers of race science. But with regard to German scientists who were internationally acknowledged before 1933, it seems problematic to concentrate solely on those two aspects and thereby neglect the scientific dimensions of their work. At least their scientific work should be considered with the same attentiveness as that of scientists from other countries.[1]

Interestingly, early contemporary critiques of German race science did not aim at race science in general and not even at German race science as such. Rather, they attacked the notion of "Nordic supremacy."[2] At the center of their critique were race theorists;[3] rarely did the critiques mention those German human geneticists and physical anthropologists who had some sort of an international record before 1933. Yet there is more to say about German race scientists than simply stating how preoccupied many of them were with Nordic supremacy. Those scientists constituted a scientific community with certain peculiarities and differentiations, some of which will be illuminated below.

Two U.S. geneticists were highly influential in the postwar critique of race science and in the UNESCO initiatives:[4] Leslie Clarence Dunn and Theodosius Dobzhansky, founding directors of the Institute for the Study for Human Variation at Columbia University (Gormley 2007, 2009*b*). They proposed that population genetics would be the proper way of studying human variation and of overcoming racial ideologies. In their view, the typological race science of the early twentieth century had been "antievolutionist," and it had devolved all the way down to those notorious misunderstandings, misinterpretations, and abuses of the race concept that were put to use in racist policies, discrimination, and genocide (Dobzhansky 1951:264–266; Dunn 1951; L. C. Dunn 1959; Dunn and Dobzhansky 1946; Gannett 2001; Marks 1995).[5]

This general depiction—that "race" was abandoned and replaced by "population"—resonates in historical studies (Barkan 1992; Stepan 1982, 2003). Yet recent studies stress that in the postwar decades, leading geneticists such as Dobzhansky and Dunn did not abandon race but believed that races existed and could be studied scientifically (Gannett 2001; Marks 2008; Reardon 2004, 2005:10ff.). They just drew a line: the old typological approach toward "race" was bad science, and the new population-based approach was good science. What, in their view, was wrong with the old race science?

First, the typological approach, which had employed abstract types to characterize races, seemed to have failed to account for the statistical distribution of genes within populations. Second, according to Dunn and Dobzhansky, it had assumed the existence of "stable" unchanging races, a view that they considered to be "antievolutionist" (Dobzhansky 1951:264) and as such anti-Darwinian. A third mistake of the typological approach was its obsession with taxonomy and the classification of humankind instead of viewing populations as biological, dynamic, evolving entities. Finally, one of the most critical features of the old race science was the distinction it drew between superior and inferior races.

By contrast, Dunn and Dobzhansky held that studies of human variation needed to be based on the theory of evolutionary synthesis. They promoted a "genetic race concept" (Dobzhansky 1951:265) that combined neo-Darwinian theory with Mendelian and population genetics.[6] In commenting on Boyd's book, Dobzhansky held that mankind should be viewed as a "Mendelian population" defined as "a population sharing a common gene pool" comprised of a "complex system of isolates" (Dobzhansky 1951:264) that were reproductively kept apart by geographic or social factors.[7] The "subordinate populations" of humankind "differ in relative frequencies of genes for various traits in their gene pools. Such different populations are races" (Dobzhansky 1951:264). The conclusion sounded perfectly simple and elegant: "The

1. A recent study that attempts such an approach is Schmuhl (2008). Still focused on eugenics but also oriented toward human genetic knowledge production is Weiss (2004, 2005). To be sure, Stefan Kühl's (2002) study about international cooperation among eugenicists is an important contribution, but it does not fill this gap in the history of science.

2. See, e.g., the book *We Europeans*, which aimed at combating national socialist racism (Huxley, Haddon, and Carr-Saunders 1939).

3. Particularly Hans F. K. Günther and race theorists that had strongly influenced German cultural discourse, namely Arthur Gobineau and Houston Stewart Chamberlain.

4. The UNESCO statements on race of the early 1950s are commonly seen as the turning point in the international history of race science signifying a rejection of race concepts on the part of academics as well as scientists. As recent studies have shown, however, the background story of the UNESCO proclamations was far more complicated and controversial (Barkan 1996; Müller-Wille 2003; Reardon 2005; Stoczkowski 2009).

5. One could read their comments as an attack on Nazi ideologists and German race scientists but also on their Anglo-American colleagues who believed in Nordic supremacy.

6. "Selection," according to Dobzhansky, was "in all probability the key to understanding of human evolution" (Dobzhansky 1951:265).

7. "Human populations exist by virtue of the reproductive bonds that give them biological reality. Genetic differences between such populations exist regardless of whether anthropologists choose to attach racial labels to them. What is not arbitrary is the recognition that a certain group of individuals constitutes a Mendelian population" (Dobzhansky 1951:265).

way to describe races is, then, to study the frequencies in human populations of variable genes" (Dobzhansky 1951: 265).

Recent powerful critiques directed at the boundary work done to distinguish old from new race science concentrated mainly on the theoretical and political statements of leading geneticists (Gannett and Griesemer 2004; Pogliano 2005; Reardon 2005; Sommer 2008). But how did the new approach play out in empirical designs of genetic studies? Gannett's and Reardon's critiques of population genetics in the 1950s are extremely important and fruitful, yet they provided no detailed account of the so-called typological race science before World War II as well as of the very practices employed to investigate empirically human variation after World War II.[8] My aim here is to take their critical approach farther back into the history of race science in order to complicate the pre–World War II part of the story and then reconsider post-1945 research into human population genetics from that perspective.

German Human Genetics and Physical Anthropology before World War II

While a synthesizing history of styles of thought (and practice) in German human genetics and physical anthropology still needs to be written, in the following I want briefly to summarize the situation in Germany around 1930.[9] Although recent studies have dealt with specific aspects, we lack an overview of the variety of scientific endeavors undertaken in the study of human biological diversity, all of which need to be viewed as more or less entangled with racial theory and eugenics.[10]

Before going into details, I would like to highlight two peculiarities of the German situation. It needs to be recognized that German anthropology was exclusively, by definition, physical (i.e., biological) anthropology. The fields of ethnology and sociology were—albeit comparatively weak—substitutes for the social and cultural anthropology that emerged in the United States and Great Britain around 1900. Furthermore, German physical anthropology and human genetics were neither totally separated nor homogenous communities. After 1900, a single scientist could be both a physical anthropologist and a human geneticist. Furthermore, both fields not only overlapped but were also comprised of various traditions and several "schools," most of which promoted some sort of eugenic agenda.[11] Some of these schools remained influential even after 1945 (Cottebrune 2005, 2008; Hoßfeld 2005; Lipphardt 2009*b*; Massin 1999; Preuß 2006*b*).

In Berlin, Professor Felix von Luschan, both ethnologist and physical anthropologist, and his student Egon von Eickstedt, professor in Breslau, claimed that physical anthropology and ethnology should go hand in hand. Understanding themselves as *Geisteswissenschaftler* (i.e., academics and not scientists; the German "Wissenschaftler" comprised both) but relying on extensive anthropometric measurements, they resisted Mendelian genetics methods but addressed genetics and evolution in theoretical essays (Laukötter 2007; Preuß 2006*a*). Cultural, mental, and biological traits were barely distinguishable in this vein of research. Whereas Luschan had been the most influential physical anthropologist around 1900, his influence decreased when physical anthropologists turned to Mendelian genetics in the following two decades.

Human genetics was institutionalized rather late, but there were influential institutions outside universities where human genetics gained a remarkable foothold. Eugen Fischer, director of the Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre und Eugenik (since 1927) and professor of physical anthropology at Berlin University, had introduced genetics into physical anthropology beginning with his book *The Rehobother Bastards* (Fischer 1913). The institute's members employed an impressive variety of approaches and methods, most notably anthropometry, twin research, serology, and genealogy (Schmuhl 2008). With Fritz Lenz and Otmar von Verschuer,[12] Fischer established the most influential physical anthropology and human genetics school in the Weimar Republic, and after 1933 they cooperated willingly, if not enthusiastically, with the Nazi regime (Lösch 1996; Schmuhl 2008). Their credo was very much informed by eugenics and "the Nordic ideal," yet their scientific work was internationally acknowledged as important and state-of-the-art at least up until 1933 (Fangerau 2001).

Ernst Rüdin pursued genetic psychiatry and biological genealogy studies at the German Research Institute in Munich. Pathological-trait pedigrees and extensive mass data collection characterized his approach. Rüdin, who after 1933 also collaborated with the national socialist regime, can be considered a crucial node in the network of human geneticists and eugenicists in the early twentieth century (Mazumdar 1996; Weber 1993).

Scientific interest in genealogical and medical genetics grew rapidly at the beginning of the twentieth century. The biomedical—and predominantly eugenic—milieus of cities such

8. M'Charek (2005) focuses on the practices of human population genetics in the Human Genome Diversity Project but neglects historical dimensions.

9. On the basis of secondary literature, the basic work includes Rheinberger and Müller-Wille (2009), Weindling (1993), and Weingart, Kroll, and Bayertz (2006). For German physical anthropology and human genetics, see Hoßfeld (2005), Mai (1997), Massin (1996), Mazumdar (1996), Schmuhl (2008), and Schwerin (2004). On German serology, see Spörri (2009). On thought styles in German genetics, see Harwood (1993).

10. Following mainly a discursive approach and focusing on basic concepts, I am aware that changes and continuities in research practices call for more attention. See De Chadarevian (1998), Marks (1996), and Mazumdar (1995).

11. Those scientists who held a chair or teaching position in "physical anthropology" or *Rassenkunde* were usually trained either in medicine, a subdiscipline of biology, or the humanities, followed by intensive postdoc training in the methodology of physical anthropology.

12. Weiss (2006); Sheila Faith Weiss is currently working on a biography of Verschuer (Weiss, forthcoming).

as Munich and Leipzig became centers of this movement. Many doctors outside universities started to systematically assess pathological traits in the pedigrees of their patients. Furthermore, mathematicians—most notably Wilhelm Weinberg and Felix Bernstein—developed statistical approaches toward human genetics (Mazumdar 1996).

German serologists were leading experts in the innovative branch of seroanthropology. Scientists with Jewish background influenced the field substantially, most of all Fritz Schiff, the Austrian Karl Landsteiner, Felix Bernstein, and the Hirszfelds. At the same time, nationalist non-Jewish colleagues—most notably Otto Reche, professor of ethnology in Vienna and Leipzig and a former student of Luschan—successfully excluded Jews from the ranks of their scientific institutions (Geisenhainer 2002; Okroi 2004; Spörri 2009).[13]

Based at Jena, Hans F. K. Günther was among the most influential German writers on race from the 1920s up to the national socialist period. However, because he lacked scientific training, he was not fully acknowledged by German scientists (geneticists and anthropologists), although many of them, especially after 1933, appreciated his work as "inspiring" and were willing to cooperate with him (Lipphardt 2009*c*). More than anyone else, Günther embodied the "typological" field of race science. But even before Günther was appointed in 1930, Jena biologists supported a brand of anthropology as race science informed by genetics with a strong interest in neo-Darwinian evolutionary theory, maintaining a staunch eugenic and anti-Semitic atmosphere (Hoßfeld 2003; Lipphardt 2008*a*:97–102).

Whereas all these physical anthropologists believed strongly that intellectual, mental, psychological, or cultural traits could be inherited alongside biological ones, the school of Rudolf Martin (1864–1925), professor at Zurich and Munich, rejected this genetic connection and concentrated instead only on the inheritance and anthropometry of somatic features. Two students of Martin, Walter Scheidt and Karl Saller, both practitioners of anthropometrics (but not as distanced from eugenics and mental-trait inheritance as Martin), found it a bit more difficult than Fischer's group to align themselves with national socialist racial thought (Preuß 2006*b*; Schmuhl 2008; Vetsch 2003).

As heterogeneous as this community was, they were all concerned with questions of human diversity and human evolution. Because anthropometric measurements did not suffice to answer these large questions, other methods had to complement the toolbox of physical anthropology, most notably methods of investigating the inheritance of normal and pathological traits.

A hitherto neglected aspect of this heterogeneity is that in all of those traditions and schools, researchers with Jewish backgrounds pursued academic careers, but with little success, however, even before 1933. Both their understandings of human biological diversity and their Jewish background set them apart in the eyes of the mainstream, and they were kept out of influential positions (Lipphardt 2008*a*). Many of them advocated a relatively moderate form of eugenics, and scientists with Jewish backgrounds were strongly represented in the small camp of antieugenicists (Lipphardt 2008*b*).

In many ways, scientists with Jewish backgrounds were remarkable and unique in this context. Franz Weidenreich, professor for physical anthropology at Frankfurt until 1935, was unique among German anthropologists for his decidedly neo-Lamarckian convictions. Already around 1930 he challenged mainstream physical anthropology on the basis of his considerations of biological variation, following the approach of "anthropometry without psychological inclinations" (Hertler 2002; Lipphardt 2008*a*:266, 259–277). Much opposed to Fischer but befriended by Boas, he emigrated and worked as a paleoanthropologist at Columbia University and in Beijing, China, through the war years (Hertler 2002; Lipphardt 2008*a*).

Wilhelm Nussbaum, one of the few PhD students of Fischer and Verschuer with a Jewish background, tried to establish a carrier within Fischer's institution. In 1933 he had to give up these aspirations, and he founded his own institution to do genetic and anthropometric research on German Jews. In 1935, he emigrated to the United States, abandoned his eugenic beliefs, and continued his anthropometric work with Boas.[14]

In Rüdin's institute, Franz Kallmann was one of the few researchers who came from Jewish families. With Boas's help, he emigrated to the United States, where he founded genetic psychiatry (Cottebrune 2009). Jewish doctors were innovative in research on the genetics of pathological traits, as, for example, eye specialist Arthur Czellitzer, who created innovative methods and tools for genetic pedigrees and founded the society for Jewish family research in 1924, or Samuel Weissenberg, who published many pathological-trait pedigrees. Population genetics greatly profited from the work of Wilhelm Weinberg. Felix Bernstein, professor for mathematics at Göttingen, was the first to statistically assess the distribution of blood group alleles. Both Weinberg and Bernstein hailed from Jewish families, which contributed to their being marginalized in the late 1920s (Mazumdar 1996).[15]

Many researchers with Jewish backgrounds contributed actively to the scientific debate about the so-called "Jewish race," a debate that also functioned as a central forum for discussions of "race," "genetics," and "evolution" in German-speaking countries. Here, scientists of Jewish and non-Jewish backgrounds not only engaged in fervent debate but also considered each other's "racial" identities in terms of racial biology (Lipphardt 2008*a*). However, some geneticists from

13. Theodor Mollison, professor in Munich, also employed a combination of serology and physical anthropology with nationalism and eugenics.

14. On this and other examples for Boas's networking activities with German Jewish physical anthropologists and geneticists, see Lipphardt (2008*a*, 2009*a*, forthcoming).

15. Bernstein emigrated with Boas's help.

Jewish families did not participate in this debate, Weinberg being only one example.[16]

As the debate became more heated after World War I, the predominance of non-Jews, most prominently Fritz Lenz and Ernst Rüdin, grew. Non-Jewish colleagues even considered certain scientific stances to be (genetically) linked to the Jewish descent of their opponents. Particularly, Lenz picked on his Jewish colleagues for their—as he put it—notorious neo-Lamarckism: they were unable to accept, said Lenz, that they could not change their "racial fate" (Lenz 1914:247). Some but by no means all Jewish colleagues retained neo-Lamarckian ideas up to the Kammerer scandal in 1926. Non-Jewish colleagues claimed the divide between neo-Lamarckians and neo-Darwinians mirrored the divide between Jewish and non-Jewish scientists, thus defaming their colleagues and accusing them of being incapable of keeping abreast of current scientific standards (Lipphardt 2008*a*:96–102, 137–143, 163–177).

It is a bitter irony that to this day the most eloquent and outspoken opponents of German race science and eugenics have been totally neglected by historians: Samuel Weissenberg, Hans Friedenthal, Franz Weidenreich, Hugo Iltis, Peter Schneider, Fritz Merkenschlager, and also Paul Kammerer published stunningly radical critiques of physical anthropology and human genetics, particularly Fischer's and Rüdin's work, before 1933. Max Marcuse, a German Jewish physician and scientist, provoked a fervent discussion on *Rasse* in the German medical journal *Die medizinische Welt* in 1927. He invited Franz Boas, Walther Scheidt, and Otto Reche, in a very open and fair manner, to respond. While Boas and Marcuse took a critical stance on race science but did also engage in self-critique, Scheidt and particularly Reche rejected any criticism and refused to engage in a constructive dialogue (Lipphardt 2008*a*:180–186).

Scientists such as Marcuse constantly challenged the mainstream, but they also tried to build bridges. Yet their critiques fell on deaf ears. Those who had held neo-Lamarckian views up until 1926 were successfully discredited and marginalized as scientifically outdated, even though most of them gave up neo-Lamarckism after 1926. Even worse, although not all considered themselves to be Jewish—and even Lenz found no definitive proof of his assumption that Iltis and Kammerer had Jewish ancestors—their neo-Lamarckian beliefs sufficed to mark them as Jewish in the eyes of Lenz. Their attempts to find allies in the United States and in Great Britain before and after 1933 all failed, in some cases ending in tragic stories, such as the suicide of Hans Friedenthal. In 1945, Weidenreich set up a memorandum in which he accused Fischer and other colleagues of being "war criminals," thus going much further than his colleagues from the United States and the United Kingdom (Lipphardt 2008*a*:277–278). No one took note of it.

On the one hand, this reluctance of scientists in the United States and the United Kingdom might have had to do with their anticipation of—or even direct inclination toward—racist tendencies in their own societies. Even for those who sympathized with a strong critique of German race science, to question race concepts fundamentally seemed too bold a claim to make.[17] Given the racist tendencies in academic milieus, it was bold enough to question Nordic supremacy. On the other hand, influential scientists such as Dunn and Dobzhansky were much concerned with establishing a new neo-Darwinian evolutionary theory aligned with population genetics and antiracist declarations. They wished to discredit a "antievolutionist" sort of theorizing about human biological diversity, and German race science was no easy enemy in this respect. And finally, it was generally believed that Jewish scientists' positions would be biased with regard to questions of race.[18] As early as 1927, Marcuse had noted that any position on the problem of race would be biased, but this seemed too radical a critique of science at the time.

Antievolutionist? German Scientists and Biological Change

Did German physical anthropologists and human geneticists indeed adhere to "antievolutionist" typological race concepts in contrast to geneticists elsewhere? German genetics certainly differed from genetics in the United States and Great Britain, but there was also much communication and some substantial agreement with regard to methods and theoretical approaches between the communities (Harwood 1993). Most German human geneticists and physical anthropologists—for example Fischer, Lenz, and Verschuer—were not very interested in population genetics and the evolutionary syntheses.[19] However, the same held true for scientists in other countries in the 1930s: although population genetics and the synthesis were the dernier cri in genetics and in spite of the fact that a few population-genetic studies on humans had already been published, most students of human biological diversity did not employ population genetics in their work (Provine 1992; see also Lasker 1954).

But apart from this delay, most German scientists did not simply believe that human races were pure and stable types, nor were they obsessed with classification (although Günther,

16. The same can be said of Heinrich Poll, professor at Hamburg, and Richard Goldschmidt, director at the Kaiser Wilhelm Institute for Biology, who neither employed anthropometry nor participated in debates on human races. Poll, a moderate eugenicist, investigated the inheritance of "normal" (nonpathological) traits in animals and particularly in humans using methods such as twin studies (Braund and Sutton 2008). Goldschmidt worked on biological variation and heredity in animals (Satzinger 2009). In Jena, the anti-Semitic atmosphere may have added to the local hostility toward one of the greatest neo-Lamarckians, Richard Semon, who was reported to have had a Jewish background.

17. See, e.g., a letter by Charles Singer to Ignaz Zollschan, September 18, 1936, A 122/11/4, Central Zionist Archive, Jerusalem.

18. With special reference to Boas, see Barkan (1988) and Lipphardt (2009*a*).

19. See Schmuhl (2008) and Schwerin (2004); for a differing account, see, e.g., Hoßfeld (2003).

who was not a scientist, certainly was). Even notorious racist and eugenicist Otto Ammon stated as early as 1900 that, by principle, it was incorrect to assume pure unmixed races (Ammon 1900). The notion of races as dynamic evolutionary groups with different but overlapping gene frequencies was more widespread in German science than historians believe today, and the great majority of German race scientists did not pursue "antievolutionist" approaches (Massin 1996).

Between 1880 and 1930, German scientists took into account various possibilities of evolutionary change: neo-Lamarckian mechanisms, environmental plasticity, mutation, selection, and "racial mixing." Except for neo-Lamarckian mechanisms,[20] these were all well-recognized Darwinian factors of evolutionary change in humans already in the 1920s. Not all scientists attributed the same relevance to these various evolutionary forces, and they also differed in their views on how fast these forces—particularly selection and mutation—would yield a significant change in racial makeup.

The most prominent German race scientist, Eugen Fischer, ridiculed statistical approaches: human genetics would get no further with statistics.[21] On the other hand, his notion of human biological diversity was not far removed from population genetics:

> Consider for a moment the many factors contributing to normal morphological developments and physiological events. As far as we know, of all these factors, many occur in all human populations. They can appear at about the same frequency everywhere; or they can be almost regular in some places, and remarkable and rare exceptions in others, so much so that superficial description would attribute their effect to individual variation. We see a great number of such individual traits and characteristics in all human populations and we attribute many of them to specific genes. At the same time, however, it appears that we also have morphological and physiological phenomena that derive from genes—phenomena that exist only in unique geographic or socially isolated populations, while being absent in other populations. Each such population has a number of its own unique genes. It also always has numerous genes that it shares with any other population. We consciously and purposely ignore all of these common genes whenever we wish to distinguish between populations and to consider and label them as being separate. (Fischer 1930:135. V. Lipphardt, trans.)

Fischer agreed that when speaking about human biological diversity, it would be better to avoid the term "race" completely and instead find new terms for the systematic ordering of populations of different sizes, which is very similar to Dobzhansky's "complex system of . . . subordinated populations" (Fischer 1930:136). In spite of the outdated vocabulary and although Fischer did not mention the evolutionary changes occurring in such populations, this sounds quite similar to the population concept provided by population geneticists much later. Even the assumption of "unique" genes in isolated populations still echoed in publications of U.S. scientists in the 1950s (Lipphardt 2010*a*).

It seems that for human geneticists and physical anthropologists in the 1920s and 1930s in Germany as elsewhere, the question of biological change in humans was closely aligned with questions of how fast it appeared, how it was brought about (e.g., by selection, drift, or environment), and whether change occurred only within assumed evolutionary units—for example, in populations or in species—or only by reproductive contacts between groups. Many attributed high relevance to "racial mixture," a "factor" believed to be easily observable in everyday life. This, however, was based on the assumption that "pure races" or "unmixed populations" had mixed in the course of human history. This in turn depended on the concepts of "reproductive isolation" and "reproductive barriers"—for example geographical constraints—as a prerequisite of Darwinian evolution. Thus, questions of purity and mixing were not simply a matter of apartheid, racism, eugenics, or other political ideologies but epistemic challenges to researchers of human biological diversity.

"Human Mating Is Uncontrolled": Isolation, Reproductive Barriers, Pure Lines, Mixing

Much has been said and written about the eugenic obsession with "purity" being closely aligned with notions of "selection" and "survival of the fittest." I claim that in studies of human variation throughout the late nineteenth and twentieth centuries, notions of "isolation" and "reproductive barriers," equally derived from Darwinian theory, and their projection onto humankind had serious consequences, too.

The scientific talk of "isolation" and of the "reproductive barrier" has undergone considerable change since the late nineteenth century; however, it has always comprised one crucial aspect of human genetics and physical anthropology. It is important to note the difference between "purity" and "isolation."[22] "Purity" was conceived of as a permanent status of a race or a population with regard to a certain type or trait or gene. "Isolation" was considered the process by which purity might be obtained and is a concept that draws on Darwinian as well as Mendelian ideas. Whereas the expression "pure race" was used predominantly in the late nineteenth century and mostly denoted a stable condition since prehistoric times, after 1900 one finds terms that rather evoke notions of processuality, temporality, evolution: "inbreeding community," "isolate," "isolated populations." Geographic isolation was central to Darwinism, as well as other nongeo-

20. On the neo-Darwinism/neo-Lamarckism controversy in Germany and its culmination in 1926, after the Kammerer scandal, see Lipphardt (2008*a*:96–102, 137–143, 163–177).

21. Mazumdar (1996:634–635). Richard Goldschmidt had agreed on Fischer's view.

22. For different understandings of "pure populations" in the context of the UNESCO statements, see Reardon (2004:60, n. 12).

graphic factors that might separate individuals and keep them from interbreeding. This might then lead to the emergence of varieties that would subsequently cease to interbreed with others and thereby become new species.

The Mendelian term "pure lines" also evoked the notion of a process because it denoted a temporary result of consequent inbreeding. It seemed to suggest an analogy between plant and animal genetics and human genetics. Stable "pure lines," obtained by "reproductive isolation"—that is, consequent inbreeding through several generations—were a crucial first step in genetic hybridization experiments of plant and animal geneticists. However, researchers constantly complained about an inherent problem of human genetics: no experiments were allowed, and no pure-line inbreeding technology could be used. Thus, researchers watched out for human groups that, supposedly, had historically been isolated for a long time and then "mixed" with other "pure lines" of humans. Accordingly, Mendelian genetics offered new terms, metaphors, and a theoretical framework to approach human groups as "isolates." Not only was the "pure-line technology" projected onto human history but also the whole operational sequence of Mendelian hybridization experiments became a kind of evolutionary thought experiment. Hence, human geneticists thought that in well-defined societal situations, they could observe the same kind of phenomena that other geneticists—working with plants, animals, or fungi—observed in their labs (Lipphardt, forthcoming). The children of, for example, Africans and Europeans were then termed "F1 generation." The present state was read as the outcome of a breeding experiment that had taken place on an evolutionary scale (McLaughlin and Rheinberger 1982).

In this thought experiment, the "isolated human population," presumably isolated by history in analogy to the isolation of breeding lines in the laboratory, was an indispensable practical device. "Isolates" that had presumably remained "unmixed" were considered the most promising starting point for studies of human heredity and diversity. Scientists took extensive notes on families, pedigrees, inherited characteristics, and striking features of "isolates" they had identified. Although neither Europeans nor Africans, for example, were seen as "inbred" populations in a narrow sense, they seemed sufficiently distanced by reproductive barriers. Variation within each isolated group was admitted but considered to be insignificant compared with the enormous differences between the isolate and the larger surrounding population or between two very distant populations, respectively.

Physical anthropologists and human geneticists paid much attention to Mendelian hybridization studies not only because "racial mixture" was a hotly debated political issue but also for epistemic reasons. In the 1920s, this was international state-of-the-art research, and many scientists pursued work along these lines. It was the inheritance of "normal genetic characters" that scientists hoped to understand better by this kind of investigation.

Again, it was Fischer who expressed this hope in 1930 in accordance with his colleagues in other countries.[23] He complained that pathological human genetics—the specialty of his rival Rüdin—was far ahead of the genetics of normal biological traits (*erbnormalbiologischen Genetik*). The only way to proceed, according to Fischer, was to study "racially mixed populations" as the result of a Mendelian experiment:

> From the perspective of scientific inquiry, human mating is uncontrolled so that lineages are, in a manner of speaking, hereditarily impure. As a general consequence—and especially in European populations—there exists such an enormous diversity of gene combinations that, in practice, it becomes extraordinarily difficult to trace the effects of individual genes. It is impossible for us to isolate and breed a single interesting trait using systematic crossbreeding. That is the reason why the preferred method must be the study of "bastards," whose parents are assumed or have been shown to have very different sets of genes, i.e. stand at considerable distance from one another in the anthropological system. (Fischer 1930:130–131. V. Lipphardt, trans.)[24]

Fischer did not speak of inbreeding isolates but rather of two large groups that had evolved along separate paths for quite a long time. Nevertheless, reproductive barriers—and thus the isolation of the two groups from one another—were supposed to be at work here. Each of the groups could display high variability within themselves; but the variations between the groups were rendered more significant. However, it was difficult to find a mixed population that would not display uncontrollable mixture.

> Many of these bastard populations are not always perfectly suited to answer some questions because we are either unaware or insufficiently aware of prior crossings. . . . And some genetic studies promise to reveal new and interesting insights even though the ascent of the crossing lineages is incomplete or questionable. . . . More urgent still are studies of new crossings between relatively pure breeds that are occurring under our very noses. In many places in the South Seas there are still intact indigenous populations that are being crossed by individual Europeans, Chinese, or others. . . . Today it's still possible to observe large numbers of initial crossings, i.e. F1 bastards, and equally certain recrossing of F1 [bastards] with one or both of the parents' races. (Fischer 1930:223)

What Fischer pointed to here is the importance of historical knowledge, of biohistorical narratives: to establish a popu-

23. See Fischer (1930:128ff.). Fischer gives an extensive international bibliography in the annex. However, he complained that although many single studies on detailed aspects appeared, only a few monographs, such as his book on the "Rehobother Bastards," were published.

24. Fischer used the term "bastard," probably referring to Hugo de Vries's use of the term (Vries 1903). The use of *Bastardisierung* in German and "hybridization" in English points out how important it is to recognize that many scientific terms in the context of human biological diversity and genetics differed significantly in German and Anglo-American scientific communities.

lation's status as isolated or mixed, one had to find out about its history using all available means.

Practical Problems: How to Isolate Isolates and How to Control Mixing

For the empirical study of human variation, heredity, or hybridization, locating isolates was crucial, but it proved to be a challenging task. As convincing as the theoretical tool of reproductive isolation might have been, scientists who tried to empirically nail down an isolate—or two isolates that had mixed under epistemically controllable conditions—faced serious practical problems. While it may have proven easy to breed and isolate pure lines of drosophila or peas in the lab, it was much more difficult to identify wild animal and plant populations in which the genetic approach would yield valid results. And it was still more difficult to isolate a human group that was supposedly held together by reproductive bonds. Empirical studies of human variation had to define a population under study, a reproducing unit that had stayed away from others for long enough to allow for evolutionary changes, and so it was all about finding, delimiting, or even isolating the isolate.

Scientists turned to supposedly isolated populations in a very Darwinian fashion, namely, to island inhabitants, to people separated by geographic barriers such as mountains or rivers, to people that could be studied abroad, in remote places, in colonial contexts.[25] Obviously, colonial administrations provided ample networks and infrastructure for such scientific projects. Minorities, outsider communities that historically were notorious for having been socially separate, seemed to offer themselves as prominent examples in certain regional contexts. These were often groups that were blamed for strong social tensions within national or imperial contexts. For Swiss anthropologists, the most obvious isolated group to study were the inhabitants of the Swiss Alps. For Americans, Native American populations were among the most promising isolates. For Indian and British scientists, isolation was to be studied in the caste system.[26]

However, at the same time, researchers were confronted with the fact that because of colonization, even very remote small populations had at one point or another uncontrollably mingled with European colonizers. Hence, "racial mixing" or "hybridization" was initially considered a problem for those looking for isolated groups. But on the other hand, mixing seemed a most promising phenomenon for human geneticists to study if only the conditions of mixing could be controlled. After a few pioneer studies, the empirical interest shifted toward racial mixture as an epistemic tool to investigate human genetics and racial biology in the 1920s.

As with locally well-known "isolates," certain "mixed populations" seemed more obvious to study than others depending on the social and political situation at hand. Each national discourse had its own prominent example of interethnic contacts and tensions. Herskovits's work in the United States was dedicated to the "pure" and "mixed" "Negro": "We have in the American Negro Population almost laboratory conditions for the study of the effects of racial crossing" (Herskovits 1927*a*:299). Geneticist Leslie Clarence Dunn, in the early phase of his career, studied "race crossings" in Hawaii (Dunn 1928; MacCaughey 1919). One cannot infer a racist motivation: rather, they attempted to approach "race problems" "objectively"; their attempt to stabilize "isolates" and "crossings" reveals what they deemed objective at the time.

For German scientists, the most obvious "isolate" to study were the Jews.[27] Jews were generally seen as a "pure" and "foreign race," not only by anti-Semites. As with other minorities in other countries, in light of societal and political discourse it seemed only natural to study them as an isolate. The Jews constituted the largest minority, and they were the main target of discriminatory behavior. Scientists claimed to approach them "objectively," and thus scientific debates developed somewhat differently from general public discourse (Lipphardt 2008*a*). Around 1880, still in accordance with general views, German scientists had believed that the Jews were notoriously incorrigible, nonmalleable, and separate by choice.[28] As evolutionary thinking gained a foothold before 1900, it was first assumed that in contrast to larger populations, an isolate such as the Jews did not evolve at all, giving an example for the tenacity of heredity and racial homogeneity. Then, Jews were seen as an inbreeding isolate, with all of the inherently bad or good consequences it presumably entailed (Lipphardt 2007, 2008*a*:102–113). Finally, it became a standard assumption that precisely because the Jews constituted an inbreeding isolate, dispersed throughout Europe in differing environments, they were ideally suited for the study of evolution and heredity. Jews were seen as an interesting example of evolution just like Darwin finches and their geographic dispersal. All these assumptions were laden with more or less subtle sociocultural valuations, most with pejorative undertones (Lipphardt 2008*a*). One can consider this an example of the scientification of a myth.

In this vein, the biohistorical narrative of everlasting Jewish isolation was gradually substituted by a new one around 1900. Allegedly, the Jewish race had resulted from an ancient cross between three oriental racial types (Amorites, Semites, and Hethites) and could thus not be considered a "pure race" but

25. For examples, including the research endeavors of Anglo-American scientists, see Fischer's bibliography (Fischer 1930:224–234). Fischer's students pursued research in the Pacific and elsewhere (Schmuhl 2008:74–75, 82–91).

26. For a bibliography with these and other examples, see Fischer (1930:224–234).

27. There were only a few minorities in Germany, e.g., the Sorbs, but most of them constituted very small communities and were difficult to access.

28. See, e.g., Anonymous (1880); see also the positions of the anti-Semitism controversy in 1879 and 1880 and racial theorists' writings in the second half of the nineteenth century.

rather a "race mixture."[29] After the temple destruction, so the narrative went, Jews were dispersed throughout Europe, a story of migration into various geographic environments. Because it was assumed that they did not intermarry with non-Jewish societies, the ancient "race mixture" became a "pure stock" that reproduced only within its own community: an "inbred" or even "incestuous" race. Variations between the dispersed Jewish groups were explained as local adaptations induced either by environmental factors or selection. Medieval ghetto life had alledgedly had tremendous selective effects on the biological makeup of the race, making them even more distinct, and emancipation in modern times was supposed to lead to race mixture (one out of many examples of this typical account: Gutmann 1920).

Jewish history was told in biological terms—a second wave of a myth's scientification (Lipphardt, forthcoming). This biohistorical narration also circulated in scientific communities in other countries,[30] but it appears to have been most elaborated in Germany. The common notion of the Jews as a pure and inalterable race turned into a presupposition for empirical research, and the history of the Jews oscillated between its being an epistemic object and its serving as an epistemic tool.[31]

Transferred to the field of Mendelian genetics, the supposed reproductive isolation of the Jews led researchers to distinguish the "Jews" from "non-Jews," or from "European" populations, and to render the children of intermarriages as F1 generations. Although both lines showed considerable variation, the distinction seemed much clearer than any distinction between two so-called European groups. Europeans were, much to the regret of some (e.g., Hans F. K. Günther), perceived to be thoroughly mixed by migration; hence, anthropologists admitted that it was impossible to delimit national races. But when compared with (supposedly) much more distant groups, as Africans or Jews, Europeans were considered to be quite homogenous and clearly distinguishable (Lipphardt 2009*b*). However, there was no substantial African or Asian community in Germany; other minorities from neighboring European regions were not "distant" enough for that kind of contrast game. Hence, Jews also seemed to provide the most obvious and locally available mixing example to study. Fischer, for example, strongly recommended investigating Jews and Jewish-Christian mixed marriages and ignored the German Jewish scientist Max Marcuse who argued that nothing of biological interest could be discovered in those mixed marriages (Fischer 1930:223; Lipphardt 2008*a*:152–160).

Scientists with Jewish background challenged the isolation narrative—or at least aspects of it—by drawing on historical knowledge and new empirical data (Lipphardt 2008*a*). They particularly attacked pejorative notions of "inbreeding" or "mixing" periods. But even those critiques rested within the realm of biology, not surprisingly. Accepting that humans had evolved and that this caused biological variations in humankind—as in animals and plants—the commonly accepted biohistorical narrative could be modified but not abandoned. Some questioned the historical sequence of isolation and mixing, insisting that throughout 2000 years there had been much more interbreeding with Christians than historiography accounted for (Fishberg 1911). And put into practice, the idea of studying Jews as a biohistorical isolate had yielded no significant empirical results: Investigations had revealed heterogeneity and variability within the Jewish isolate as well as in other supposed isolates (Fishberg 1911; Luschan 1892; Weissenberg 1927*a*, 1927*b*), but the German non-Jewish scientists would simply ignore this substantial critique.

Endogamy: Isolates in the 1950s

As mentioned above, population genetics was seen to provide a clear break with racial science and to bring the new evolutionary synthesis to the study of human variation after World War II. In what follows, I aim to demonstrate how durable the narratives of isolation and mixing nonetheless were. The new approach required that isolated populations and their allele frequencies be studied in comparison with the frequencies of other isolated populations, and hence "racial crossing" remained a problem.[32] Investigations of "endogamous communities" thus experienced a boom in the 1950s. For example, in 1956 a study investigated social endogamy and blood type distribution in Western Apaches.[33] A series of studies was conducted on endogamous groups—castes—in India under the supervision of L. D. Sanghvi, one of the fellows at the Institute for the Study of Human Variation, who employed this conceptual framework of reproductive isolatedness in 1949.[34] Sanghvi claimed that "for any investigation on the distribution of genetic characters of the people of India, the ultimate racial units of importance are the endogamous groups" (Sanghvi and Khanolkar 1949:62). Notably, population geneticists did not require that these units be isolated since prehistoric times, as proponents of race concepts in the early twentieth century would have. A time span of a few thousand, even of some hundred years of isolation was enough to bring about the desired experimental results: differing allele frequencies. Thus, the timescale on which ge-

29. See Luschan (1892:95). The *Rassengemisch* hypothesis came to be the standard doctrine in the "biology of the Jews" in the beginning of the twentieth century (Lipphardt 2008*a*:72–85).

30. E.g., in Salaman (1911).

31. For "epistemic objects" and "epistemic tools," see Rheinberger (2001).

32. The problem was tackled as a quantitative one by Lasker (1954: 433–436).

33. To give but one example, see Kraus and White (1956).

34. Typically, only well-established genetic traits were tested, such as blood group alleles, taste reaction to phenylthiocarbamide, and green-red color blindness. In his study of six endogamous groups in India, Sanghvi, together with Khanolkar, explicitly challenged older racial classifications of the people of India (Sanghvi and Khanolkar 1949).

netic diversity was imagined to emerge changed considerably by the mid-twentieth century.

As demonstrated above, the rhetoric of "isolation" and "crosses" or "mixing" had held sway since the mid-nineteenth century. But thus far there had been no reason to provide empirical proof of reproductive isolation. Even Sanghvi in 1949 noted only briefly that his test populations were endogamous, pointing to their marriage laws. By the 1950s, this changed. Now in their empirical work geneticists would not only have to account for genetic characteristics but they also had to empirically identify isolates, endogamous groups, and their reproductive behavior. "Crossbreeding" with other populations would obviously alter the distribution of alleles in the population under study. Hence, much work was invested to stabilize the group under study as an evolutionarily coherent isolate.

To meet this challenge, geneticists had to rely on experts from the humanities and even on "nonscientific" knowledge. They drew on old myths, on family histories,[35] or on scholarly accounts by ethnologists, historians, or linguists. Hence, just as in race science, nonbiological knowledge entered the research design, mostly by a priori classifications and biohistorical narratives of the respective isolation history. Geneticists began to ask their research subjects to fill out questionnaires; they evaluated marriage registers and community records and conducted interviews with community officials.

Among the endogamous groups studied in the 1950s were the Jews of Rome, and here the biohistorical narrative of Jewish history described above—used worldwide but most elaborated in Germany—appears again (Lipphardt 2010*a*). In 1954, geneticist Leslie Clarence Dunn aimed at studying the genetic effects of social endogamy in small human groups or, as he called them, "biological communities," "isolates," "mating groups," "marriage circles," or "Mendelian populations." For him, they provided the perfect material for "testing some of the elementary factors which appear in theoretical models of evolution: changes in gene frequency due to mutation pressure, selection, random genetic drift, and gene-flow (migration)" (Dunn, n.d.:1). Dunn was by no means motivated by racism (Gormley 2009*a*; Lipphardt 2010*a*). What he wanted to prove by investigating endogamous groups was that they were biologically distinct not because of some inexorable racial fate; instead, they had become biologically distinct in a relatively short time because they had been separated from other humans by social and cultural means. "Historically, human populations probably passed through stages in which the size and extent of the interbreeding groups were affected by isolating factors of various kinds, both geographic and cultural or social" (Dunn, n.d.:1).

Out of many endogamous groups around the globe, Dunn chose the Jews in Rome (Gormley2009*a*, 2009*b*). One reason for his choice was their well-documented history, for Dunn a prerequisite for biological investigation: "Many such groups are available for study today, both amongst aboriginal peoples as in Australia and parts of Africa, but also as mating groups, identifiable by written records, existing as enclaves within more advanced societies. The latter offer favorable opportunities for study by the methods of gene-frequency analysis now available" (Dunn, n.d.:1).

From the beginning, Dunn and his research team, including his son and wife, were convinced that the "inbreeding," "biological," "ghetto," or "nuclear" Jewish Community of Rome had lived in social and reproductive isolation from the larger Christian society for many hundreds of years. To establish this assumption, Dunn's son, a cultural anthropologist, together with Dunn's wife analyzed community marital records, patient records and statistics, interviewed community members, and studied historical accounts. They concluded that Jewish families living in the area of Rome's former ghetto had not (or not significantly) intermarried with Christians. Jews who had married Christians had moved to other parts of town and thus were no longer regarded as part of the "nuclear community." They concluded that there was no influx into the gene pool, only an efflux out of it.

After they saw their assumption confirmed, the next step was to convince Jewish families to have their blood groups tested. This could only be achieved with incentives and the help of medical institutions that allowed access to patient files. As expected, test subjects reported that their families had always lived in the area of the former ghetto and had only married other Roman Jews. According to population genetics, such an isolation would yield a difference in allele frequency, perhaps even a new mutation that had potentially spread in the isolated gene pool by genetic drift or selection. Blood samples were taken and analyzed.

Although the blood group tests did not yield a new unique mutation, they did show a significantly higher percentage of blood type B among members of the "nuclear community" vis-à-vis the majority Christian population. It was out of the question that selection accounted for this, because respective selective advantages had not been discovered. Dunn considered genetic drift, but his main explanation followed a different line of argument: after comparing his evidence with historical information from other European countries and more recent data from Israel, Dunn concluded that Jews emigrating from Palestine had brought this genetic trait with them to Europe and preserved it through reproductive isolation. In other words, he bought into the same old narration that had informed German race science before 1933—that is, the Jews had been a biologically foreign group in Christian Europe on their arrival in ancient times. This was far from his initial claim that social isolation could bring about biological distinctness. Dunn summarized: "The ghetto population of Rome, then, does show definite biological differences from the rest of the Roman population. The blood tests confirm that this group has been genetically isolated in the midst

35. On the importance of family histories for the study of genetic diseases in such an isolated group, see Lindee (2003).

of the metropolis in which it has been living for centuries" (Dunn and Dunn 1957:127–128).[36]

However, social and biological realities are much more complex than the biological concept of the isolated population allows for and the research design of the population geneticists can account for. Social separation cannot simply be equated with biological isolation. Whether European Jewry has been socially separate has been a topic of heated debate among historians; today historians believe that there were many social contacts with Christian majority populations. Whereas historians argue on the basis of historical sources, geneticists can neither infer biological isolation from historians' findings nor answer the question of social isolation with biological methods.

Dunn presumed homogeneity within his isolated community and neglected all possible further differentiations of that community. He also did not pursue comparative studies of neighboring Roman Christians but compared his results with an average Italian population, ignoring possible local differentiations. Also, Dunn rendered genetic influx from the outside population insignificant even though there had been verifiable mixing. Within a small isolate, or in a bottleneck situation, genetic drift could bring about a dramatic change of gene frequencies in quite a short time. It depended on how fast genetic drift worked.

As it happens, Dunn even alluded to the potential of such a bottleneck event in the history of Rome's Jewish Community. As historical accounts reported, in the late fifteenth century, the community's size had decreased to some thousand inhabitants when a wave of immigrants arrived after their forceful expulsion from Spain in 1492. The French anthropologist Kossovitch had reported that Spanish Jews had displayed an unusually high percentage of blood group B (Kossovitch and Benoit 1932). Dunn referred to Kossovitch's analysis in his narration of the ancient Jewish community, but he ignored Kossovitch's agreement with Fishberg (1911), who had claimed that the European Jews had considerably mixed with Christians.

Dunn compared his data with those of an Israeli human geneticist who had found a comparably high percentage of blood group B in some Jewish groups from the Mediterranean while other groups displayed a significantly lower percentage. Dunn suggested that those Jewish groups with a high percentage of blood group B might stem from the expelled Spanish Jews and represented a common ancestor from ancient times.

However, Kossovitch had also reported that Spanish Christians and Muslims also revealed a high percentage of blood group B. Thus, Dunn could also have told a story of religious integration and tolerance resulting in mixture, but he did not. The notion of the isolate, backed by historical and sociological accounts, seemed just too plausible to be questioned.

Most notably, Dunn neglected the historical and social contingencies that would never be reported in interviews or patient and community records—stories that usually do not make their way into the orderly world of official registration, doctor-patient conversation, and social scientist's interviews; stories about patchwork families and complicated family structures of unmarried mothers or adulterous spouses. Or on another scale, there are stories of political suppression of minorities and of the vagaries of war, all events that had no place in the animal world; stories of mass rapes, forced prostitution, and other "reproductively relevant" events. The population-genetics approach fails to account for the fact that marriages in human groups are not contracted according to biological rules. Rather, they are dependent on symbolic conditions and ascriptions. And although Dunn was very much distanced from racist superiority claims, in describing the Jewish community using his "only efflux–no influx" rhetoric, he hardly made it appear to be an attractive place.

Dunn did realize that there might be problems with his approach.

> The other difficulties inherent in the genetical study of human populations do not call for special comment here. Identification of family members, of the families composing a community, determination of the mating pattern and of the continuity of endogamy are problems which each investigator must face and overcome as best he may. They are, so to speak, the price he pays for dealing with animals over which he has no control. (Dunn, n.d.:6–7)[37]

Thus, the problem of this scientific field has to do not only with the potentially political agenda of the researcher but also with the fact that anthropologists and geneticists, buttressed by their own objectivist ethos, choose to study society as though it were amenable to "laboratory conditions." Analogous to lab studies in animal and plant genetics, the assumption of historical isolation of populations was a basic precondition for working with both the well-defined uniqueness of some populations as well as with the supposedly "mixed" status of others.

Yet biohistorical narratives from all kinds of sources enter a priori into the classification, and necessarily so. To investigate human biological diversity means to engage in dialogue with humans in order to elicit information from individuals

36. Dunn gave a paper about his findings at the annual congress of the American Society of Physical Anthropology in 1955 (Dunn, n.d.). The scientific data were never published. The Dunns published a report in *Scientific American* in 1957 (Dunn and Dunn 1957), and Stephen Porter Dunn included his ethnographic material in his dissertation manuscript "The Influence of Ideology upon Culture Change: Two Text Cases" (1959). In Dunn senior's book *Heredity and Evolution in Human Populations* (1959), the Jewish Community of Rome stands as a valid example for a genetically distinct group isolated by religion. For a more complete account of Dunn's publication, see (Gormley 2009*a*, 2009*b*).

37. Herskovits noted that it might be "hazardous" and "jeopardizing any results" to base genealogies on the accounts of the test persons but regarded the problem as irrelevant in light of his research design (Herskovits 1927*b*:76).

about their origins and the communities they belong to.[38] It also depends on interdisciplinary dialogue:[39] other disciplines might also perpetuate myths of origin, migration, isolation, or miscegenation that have undergone scientification decades earlier. Who will keep the population geneticist updated in the fields of history, linguistics, and ethnology to prevent him from locking in and essentializing nongenetic knowledge?

As the example of Jewish anthropologists in pre-1933 Germany shows, accepting human evolution as a fact makes the acceptance of biohistorical narratives unavoidable. Accounting for alternative evolutionary stories—as in the case of Dunn and the community of Christians, Jews, and Muslims in Spain—would certainly make the task of human population genetics a more complicated one, but it would also help to avoid the trap of reification.

Acknowledgments

I thank the symposium organizers, the symposium participants, and Alexandra Widmer and Eric Engstrom for helpful comments on earlier versions of the paper.

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38. A "WHO Scientific Group on Research in Population Genetics of Primitive Groups" would note in 1970 that "All groups have learned individuals, e.g., experts on oral traditions and those with systematized knowledge and interpretations of natural phenomena. . . . Such information is pertinent to their cultural and therefore biological history" (WHO 1970:233).

39. "As is always the case," the French anthropologist Jean Hiernaux wrote in 1966, "[the definition of the units of study, the breeding population] required the co-operation of an ethno-sociologist who already gained acquaintance with the people" (Hiernaux 1966:288). And Hiernaux alludes to his benefiting from a linguist, a demographer, and an economist for his fieldwork in central Africa.

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A Useless Colonial Science?

Practicing Anthropology in the French Colonial Empire, circa 1880–1960

by Emmanuelle Sibeud

Physical anthropology is quite often presented as one of the favorite and most nefarious tools of colonial rule. Approved by science, racial categories shaped colonial segregationist practices, and reciprocally, colonial empires offered anthropologists new opportunities to survey differences among various people. Practices and discourses of physical anthropology thus directly and indirectly spread and deepened modern racism. There are obviously many links between colonial experiences and racism that need to be thoroughly explored. Building on the history of anthropology as well as on the impressive renewal of interest in colonial history for the last two or three decades, I focus on the cumulative anthropological situations created by investigations in physical anthropology in the French Empire from the 1880s till the 1950s to analyze their circumstances, the agendas in which they made sense, and their scientific and political results. Such investigations were indeed scarce, and beyond exceptional displays, such as universal and colonial fairs, anthropologists were at a loss to offer convincing support to colonial and metropolitan authorities. What, then, were the rationalities behind these quite demanding investigations? French physical anthropology tried to qualify as a colonial science in the 1910s and then in the 1940s, but with what results?

> *I recently experienced a great, if brief, joy. I had just ended a war between two Baule tribes, the Faafoué and the Saafoué, and I had forced the former to give back to the latter the heads they had cut off. I was brought a load of skulls. By night—so as not to be accused of witchcraft—I took out my callipers and the skulls hoping to make a few useful measurements. I was much disillusioned. First, none of them had either upper or lower jaws; the Faafoué had kept those as trophies. Then, all but one had been sawed off at the beginning of the forehead so as to make drinking vessels from the skull cap; both parts, hacked apart with a knife did not match up, I did not even know which cap to place with which skull. In sum, I could not use any of them. Moreover, the Saafoué were not happy, stating that these were only half skulls; in my opinion, they are right.* (Delafosse 1896)

In 1896, Maurice Delafosse, a junior colonial agent, wrote to Ernest-Theodore Hamy, professor of anthropology in Paris Natural History Museum and his scientific patron, on his anthropological woes in Ivory Coast. Though Delafosse favored ethnography, he was equipped for collecting bones and taking measures on living subjects. Yet he could not always use the tools he brought to the field effectively, and instead of measurements, he sent this sarcastic colonial tale to Hamy, who nonetheless published it. A decade later, in 1911, the ironies of practicing anthropology in a colonial situation were more crudely exposed by folklorist Arnold van Gennep when he drew the comical portrait of an imaginary Parisian anthropologist, gone to Africa, confronted there with Natives fiercely refusing any measurement, and thus restricted to ethnographical interviews during which he alternatively applied Frazer's "Notes and Queries" and thrashing (van Gennep 1911). By the 1910s, van Gennep and Delafosse were closely associated in France and thoroughly engaged in an effort to promote fieldwork and thus ethnography as the true basis for anthropology. Delafosse explicitly doubted the scientific relevance of racial categorization; still, he did not share the utter dismissal of physical anthropology of van Gennep,[1] and he supported instead the long-standing idea that anthropology should somehow embrace physical anthropology, cultural ethnology, and ethnography.[2] Delafosse's skepticism as well as

Emmanuelle Sibeud is Associate Professor in the Department of History, Sciences de l'Homme et des Humanités, Institut Universitaire de France (Paris 8-IDHE [UMR 8533], 2 rue de la Liberté, 93526 Saint Denis Cedex, France [esibeud@gmail.com]). This paper was submitted 27 X 10, accepted 13 IX 11, and electronically published 21 II 12.

1. In June 1914, van Gennep organized a conference devoted to ethnology and ethnography in Neuchâtel, where he had been appointed to teach ethnology in 1912. He wanted to cut cultural ethnology and ethnography from physical anthropology, which he presented as an outdated science nurturing racism. As most anthropologists were then engaged in reviving the more consensual international anthropological and archeological conferences, his project was coldly received and soon to be forgotten after the war broke out (Sibeud 2002).

2. The French use of the terms "anthropology" and "ethnology" differed from the English one. "Ethnography" and "ethnology" referred to cultural and social anthropology, whereas "anthropology" was at the same time the generic term for the whole field and synonymous with "physical

 0011-3204/2012/53S5-0008$10.00. DOI: 10.1086/662682

van Gennep's controversial antagonism suggest that implementing physical anthropology practices in the French Empire was indeed an uneven and intricate process, arousing now and then debates contributing to as well as challenging the evolutions of anthropology's conflicting subspecialities. Racism was a by-product of this process. Anthropologists were requested to provide a scientific frame legitimizing everyday and symbolic colonial segregation. Studying how they did actually fit in colonial projects and practices thus points to spaces and logics where colonial racism was, at least partly, crafted, and it also enables us to take a closer look at the many actors involved and the various ways they dealt with one another.

Physical anthropology practices in the French Empire have been extensively studied mostly in Algeria (Boëtsch and Ferrié 1993; Fogarty and Osborne 2003; Lorcin 1999), and this research brilliantly demonstrate what can be gained by minute case studies. Many more studies should certainly be provided on various colonial settings. Yet such cases should also be situated in a wider background. Physical anthropology investigations in the colonies were often isolated ventures, and even more often they were complacently presented as eccentricities highlighting the difference of Natives and the dedication to science of their colonial administrator and scientific surveyor. Still, they piled up in a few scientific journals, and their results were claimed by specific networks. Thus, a very different story can easily be drawn from the accounts provided by scientific journals of physical anthropology as a mere tool of colonial rule (Reynaud-Paligot 2006). To bridge this gap between deeply localized investigations in physical anthropology and global political claims on their uses, I propose to look for imperial rationalities and agendas connecting them. In other words, I build on a couple of awkward questions: did the French Empire make sense for anthropologists, and how did anthropologists deal with it? And quite unsurprisingly, there is a contrasted answer: physical anthropologists in France were hardly in a position to engage with the French Empire and to provide scientific surveys of it; still, their imperial callings, experiences, and failings offer interesting insights on an imperial formation process allowing for messianic pledges as well as for many silent drawbacks. Besides, they were not the sole actors involved, and it is certainly as important to analyze why colonial agents engaged in the tedious, seemingly endless, and hardly rewarding task of collecting bones or measuring reluctant Natives. As racial hierarchies were already firmly set before the Republican colonial expansion in the 1880s and the laboratory was supposedly the true space for physical anthropology, what was to be gained on colonial fields? And what was going on there? Focusing on the practicalities of physical anthropology in a colonial setting urges us to study heterogeneous encounters between would-be measuring agents and willing or unwilling measured subjects. And they might be the most interesting result of many poorly fitted enquiries.

There is another question at stake in the broad chronicle I try to reconstruct. Alice Conklin lately stressed the persistent presence of "skulls in display" in the Musée de l'homme, reorganized in 1937 and mostly devoted to cultural ethnology relying on fieldwork (Conklin 2007), as if physical anthropology were an inescapable legacy and all the more so when it was claimed by new and threatening forms of state racism. Still, 2 years later, in 1939, Henri Vallois, newly appointed secretary of the Paris Anthropological Society, called for a "systematic study of races in the French Empire" (Vallois 1939: 5), conspicuously reviving the project of surveying living human races proposed by Broca in 1879 and pretending it had actually never been implemented (Vallois 1939). By the end of World War II, in 1946, an "Anthropological Mission" was created in Dakar, measuring Natives by the thousands. Was then physical anthropology a belated colonial science? Yet why did it fit in the French Empire after World War II when racism was being internationally banned? Why did it not before, at the time of colonial expansion, or in the 1910s, when new research institutes were funded by colonial authorities and by colonial agents looking for scientific prestige? How should we account for this paradoxical and broken evolution? How far did it shape the evolution of racism in France?

I raise these questions rather than thoroughly address them, which is obviously beyond the scope of this paper. I rely on the assumption that studying physical anthropology investigations offers a far-too-neglected path to analyze why, how, and to whom the scientific framing of race and the invention of "colonial races" mattered in the French Empire from the 1880s till the 1950s. To contribute to a more systematic exploration of this path, I first provide a brief sketching of French anthropological networks and of colonial collecting, then turn to two different moments of tensions when French physical anthropology aimed, in the 1910s and in the 1940s, at becoming a colonial resource and at qualifying as a colonial science with various results.

The Incomplete Institutionalization of Anthropology "in the Broadest Sense"

Maurice Delafosse's mentor, Ernest-Theodore Hamy, was one of the most prominent French anthropologists. He qualified by publishing a thorough study of existing skull collections,

anthropology," underlining the prominent part played by this subspecialty in nineteenth-century anthropological practices and theories. Semantic changes occurred in the 1910s and in the interwar period: colonial fieldworkers and academics, most notably Marcel Mauss, vindicated ethnography and ethnology, which clearly won the day (Sibeud 2004). Thus, the content of anthropology was rather muddled until 1945, when cultural and social anthropologists lead by Claude Levi-Strauss claimed again the term "anthropology," modeling on British and American social anthropology. In this paper, "ethnology" and "ethnography" are used in their French sense and refer to social and cultural anthropology. When no other detail is provided, "anthropology" is meant as the generic term it was to contemporaries, and it is systematically distinguished from "physical anthropology."

Crania ethnica, with Armand de Bréau de Quatrefages in 1883. From 1892, when he succeeded Quatrefages, till his death in 1908, he taught anthropology at the Natural History Museum. He was also the founder and the curator of the new ethnographical Trocadero museum (Dias 1991). He belonged to the Paris Anthropological Society and to the Paris Geographical Society, eventually presiding over both. He also created in 1895 a Society for American Studies over which he also presided. He developed important ethnographical and archaeological research and was rewarded by his election to the archaeological section of the academy in 1890. Still, he carried on with physical anthropology in the dedicated laboratory at the Natural History Museum, welcoming travelers or colonial employees looking for training and advice and publishing any results they submitted in one of the numerous scientific journals he ran. Hamy thus embodied "anthropology in its broadest sense," as Broca phrased it in the 1860s, implying that all sciences studying the human species should unite under the umbrella of anthropology (Blanckaert 1989). However, notwithstanding his central position and his influence, Hamy conspicuously resigned from the Trocadero in 1906 in protest of the poverty of his museum, which had been granted the same state allowance since 1882.

The situation of "anthropology in its broadest sense," as Hamy's funding troubles suggest, remained indeed rather muddled. Important institutional steps were made in the 1860s when the Paris Anthropological Society was recognized as a nonprofit organization and Broca's laboratory was placed under the aegis of the Ecole pratique des hautes études (Williams 1985). Still, by 1900, the new organization and the rise of Republican universities shifted anthropology again to the academic fringes. Some training courses were haphazardly offered by universities, but they did not fit in any degree program before 1925. The most important chair in anthropology belonged to an institution, the Natural History Museum, also marginalized by the ascendancy of universities (Bonneuil 1998). The Paris Anthropological School was even more peripheral because it had been created and was funded by private donors and by the Paris town council. The creation of the Institut d'ethnologie de la Sorbonne was thus a decisive change in 1925. But physical anthropology was only granted a minor place at the institute. In other words, till the 1920s, "anthropology in its broadest sense" garnered no significant institutional gain from colonial expansion even though it was regularly called on for universal expositions or imperial exhibitions. Thus, to answer its colonial calling, it mostly relied on its learned society, the Paris Anthropological Society.[3]

At its height in 1885, the society boasted some 700 members, but this rapidly receded to 400 by 1914 (Wartelle 2004). The decrease sped up after World War I, first to 200 members in the 1920s, then to 50 at its lowest in 1938. The size of the journal of the society, the *Bulletins et mémoires de la Société d'anthropologie de Paris*, varied accordingly from 800 pages each year in the 1880s to 500 until 1914 to under 100 in the 1930s. On the verge of falling apart by the end of the 1930s, the society nevertheless managed a rebirth at the end of the following decade and by 1955 could again publish a thick annual volume of 500 pages and convince 200 members to pay their fees. Even in the 1880s, the total number of Parisian anthropologists could not compare with the 10,000 geographers who were members of French geographic societies in 1881 or even with the 2,000 geographers in the Paris branch of the geographical society (Lejeune 1993). The Paris Anthropological Society was no match for prominent national learned societies. Still, it was the natural representative of French anthropology in its broadest sense, as provincial anthropological societies (in Lyon, Bordeaux, or Grenoble) were mostly devoted to local ancient history.

The Paris Anthropological Society is usually credited with much political influence among Republican networks. The 1880s, however, registered bitter dissensions among its members. Broca's sudden death in 1880 opened a decade of sharp dispute for his inheritance. Paul Topinard, who chaired the society from 1880 to 1886, openly quarreled with the group of Materialist thinkers led by Charles Letourneau.[4] The Materialists won. In 1886, Letourneau took over the management of the society for the next 16 years, and in 1889 Topinard was dismissed from his chair at the Paris Anthropological School after he had petitioned the Department of Education against the Materialists (Wartelle 2004). This public feud somehow justified Republican double-bind discourse toward anthropology as a useful but unsupported science. Furthermore, the Paris Anthropological Society never officially abandoned its promotion of "the broadest sense of anthropology," but it was de facto confronted with the narrowing of its allotted scope as more specialized societies and new research institutes claimed their own separate realms (Sibeud 2002). Thus, the image of a triumphant science and professional society gradually expanding its power with the expansion of colonial rule should obviously be revised. As important recent works show, the politics of anthropology went two ways (Blanckaert 2001), and the Paris Anthropological Society was subject to colonial forces even as it tried to control and benefit from them.

The Era of Colonial Gleaners

In 1879, Paul Broca published his general directions for research on living subjects, urging anthropologists to turn toward the study of contemporary races. He firmly believed that such a survey would build a definitive hierarchy of human

3. The rather spotty papers of the society were recently classified to be removed from the former Musée de l'homme. They still need to be thoroughly studied.

4. Built around scientific and political figures such as Gabriel de Mortillet, Abel Hovelacque, Yves Guyot, or Charles Letourneau, Materialists were highly Republican freethinking scientists promoting a Lamarckian understanding of biological and social evolutions (Harvey 1984; Wartelle 2004).

races (Blanckaert 1996). The debates on the unique or diverse origin of human races and on their essential equality or inequality still divided the Paris Anthropological Society. Such prominent members as Hamy and Quatrefages maintained that human races descended from one single origin and defended the essential equality of all human beings. Broca's project was, however, an exciting new agenda echoing Republican colonial expansion. And after his death in 1880, his heirs were left with the many challenges of developing this agenda notwithstanding their theoretical and political disagreements.

Numerous explorers were sent to Asia and Africa until the 1890s, and till World War I, colonial agents and officers delineated colonial frontiers. Hamy sat on most of the boards commissioning scientific missions at the Natural History Museum, in the Department of Education, or at the Paris Geographical Society. He added anthropological investigations to many research agendas, training willing travelers and colonial employees. However, the returns from these missions for anthropology were meager. In 1883, Pierre Savorgnan de Brazza was granted the most well-founded mission for West Africa: one million francs and 40 collaborators. None was trained in anthropology; the physician of the mission was in charge of anthropology study by default, as was usually the case in large missions. The mission brought back hundreds of specimens for zoologists and botanists and many sets of ethnographical objects for the Trocadero, but only two skulls and no measurements (Rivière 1886). The habit of relying on barely trained physicians proved an enduring one, as shown by the anthropological beginnings of Paul Rivet in 1901. When he applied in 1901 for the Geodesic Mission in Ecuador, Rivet was a young military physician with a keen desire to travel but no former interest in anthropology. Once he got the position, he rushed to the Natural History Museum to acquire some rudimentary training. This proved rather effective, because he brought back impressive collections and was able, on his return in 1906, to complete his theoretical training to be recruited in 1909 as assistant to Hamy's successor at the Natural History Museum, René Verneau (Laurière 2008). If Rivet's quick career success revealed the importance of fieldwork for anthropology, his participation as *the* anthropologist in a high-profile mission, which should have provided outstanding opportunities for anthropological investigations, suggested that French anthropological networks were not doing a particularly good job of exploiting opportunities and organizing fieldwork.

This failure was a legacy of Broca. He distrusted observers and demanded highly technical measurements forbidding them any comment (Blanckaert 2009). Laboratory research, in his view, was the true realm of anthropologists. In the lab they could order series of bones and cross-check observations to produce reliable anthropological data. After Broca's death, Topinard criticized this attitude toward observers and observation in the field and pleaded for simplified measuring methods and systems. Hamy joined with him, designing a handy anthropological kit for travelers and keeping half a dozen of these kits to be lent from his laboratory (Hamy 1883). Facilitating the collection of data and materials was, however, the least controversial aspect of the problem. Topinard also challenged Broca's idea that human races were persistent, asserting instead that they had mixed up for centuries. Looking for pure races was thus an illusion, and observers were making this clear as they probed the gap between races registered in the laboratories and living races. Yet by 1889, Topinard was not any more in a position to continue this debate, and Hamy was no polemicist. Materialists could then impose their rather simplistic position: as intellectual and cultural competencies derived from physical differences, surveying living races was the primary target of anthropology. Races were thus confused with ethnic groups, as in the ethnographical questionnaire published by Letourneau in 1883 (Blanckaert 1995). However feeble, this shortcut was congruent with the ready-made idea conveyed by colonial discourse that colonies and colonized peoples should be surveyed to establish colonial rule.

Still, who should engage in such surveys? Parisian anthropologists could not afford to visit the colonies.[5] By 1880, they harshly criticized the exhibitions of so-called savages for making money out of vulnerable people. By the end of the decade, they had surrendered to the lure of the colonial sections in universal exhibitions to measure and study colonized subjects. The lack of a proper debate allowed such subrogate practice of fieldwork. Yet it did not prevent Parisian anthropologists from warmly welcoming colonial visitors. Few colonial agents entered the society:[6] fees were high, and subscribing to the more influential Paris Geographical Society seemed a sounder bet. And those who did were looking for something more than anthropological guidance. In 1891, Paul Godel, a young trade employee departing for Guinea, asked the society for instructions. In 1892, he brought back a collection of objects and answered Letourneau's questionnaire, stating that Soussou people did not wear socks (Godel 1892). He was much applauded, entered the society, and was granted the title of national correspondent for Gabon, his next assignment. In 1894, he sent a Bateke skeleton collected in Congo by a colonial colleague he recommended to the society. Still, his involvement waned by the end of the decade, maybe because it had fulfilled its aim: getting some Parisian patronage for a modest colonial career. Such nonscientific expectations were

5. In 1895, they entertained the dream of answering the invitation to visit West Africa proffered by entrepreneurs of ethnographic shows. They would travel on the ship bringing back home the Sudanese displayed on the Champ de Mars and stop in Spain, Morocco, the Canary Islands, Senegal, and Guinea (Verneau 1896). Though he was assistant to Hamy at the Natural History Museum, René Verneau was working as a physician to make a living.

6. In 1885, 16 of the 505 (3%) permanent members lived in the colonies or were engaged in colonial careers. In 1905, this held true for 33 of the 494 permanent members (6%), and in 1914 there were 11 out of 193 permanent members (6%).

easily met, and colonial amateurs, such as Godel, were happy to remain mere gleaners.

They could rely anyway on the popular understanding that reduced anthropology to the odd gathering of skulls, obscure measurements, and other curios. Colonized groups were also well aware that skulls and bones, particularly human ones, were valued objects for physical anthropology. In 1880, Gabonese King Felix-Denis sent a gorilla skeleton to the society. In 1883, a freed slave from Mozambique supposedly sold his own skull to a member of the society.[7] Lists of such heterogeneous gifts were a regular feature in the journal of the society. They suggest that the society was the passive recipient rather than the promoter of colonial anthropological investigations. As the tradition of writing special instructions for travelers bound to some exotic destination wore out in the 1880s, and as the society was too poor to lend anthropological tools or even to give observation sheets,[8] Parisian anthropologists had indeed not much to offer. And colonial investigators obliged them with their chance discoveries without engaging in more demanding research. Anthropology in a colonial situation was thus marked by two unbecoming characteristics. First, it was a secondary leisure for most colonial investigators. In 1894, for instance, the judge Paul d'Enjoy, a member of the society whose prime interest went to ethnographical studies, sent from Hanoi a skull accidentally exhumed before his eyes during construction work along with an unusually long fingernail and a clump of hair from a prisoner guilty of an outrage in which d'Enjoy was wounded and a Cochin China state prosecutor murdered (SAP 1894). Materials collected were random, unsystematic, and unplanned by those with no commitments to anthropology. Second, anthropological investigations were most often restricted to places (prisons) and situations (punitive raids) in which the colonial rule was most bluntly exerted.[9] Colonial gleaners knew and claimed that colonies were not open fields for excavation and measurement. Hamy regularly received letters making this plain. In 1882, the naval physician Savatier explained from Senegal that instead of unattainable anthropological specimens, he would send photos.[10] In 1896, Delafosse underlined that collecting skulls, even war trophies, endangered colonial authority. The handling of bodies in a colonial situation seemed far too controversial to allow systematic collection of bones or of measures on living subjects, and anthropologists had to be content with odd collecting.

A much-praised book published in 1900 by Joseph Deniker, a librarian at the Natural History Museum, offered a rare opportunity of balancing the achievements of colonial physical anthropology. In *Les races et les peuples de la terre: Eléments d'anthropologie et d'ethnographie* (Deniker 1900), Deniker provided a global overview of some measures, such as the median heights of men. Among the 288 groups measured by 1900, only a tenth were made up of colonial subjects. Furthermore, the colonial series were closer to the critical size to obtain results (25 individuals) than to the minimal amount guaranteeing trustworthy information (50 people). Colonial subjects were thus a weak point of comparison to 400,000 measured French draftees, and one could wonder whether physical anthropology was indeed a product for colonial export.[11] A survey of the 1909 anthropological acquisitions at the Natural History Museum pointed to a similar thinness of colonial gatherings compared with metropolitan and European ones. Two hundred and seventy-five skulls and skeletons entered the Natural History Museum in 1909; still only one fourth (70) came from the French colonies, whereas over half came from Europe.[12]

By the 1910s, French colonial anthropology was still in limbo. Specialists had to be trained in colonial ranks and in metropolitan ones. Theoretical and political questions on the meaning and possible use of anthropologically surveying colonized peoples had to be addressed too. And they turned out to be far more cumbersome as they intersected with the bitter

7. Paul de Jouvencel presented the society with "the skull of a *Mozambique*, a former slave from inland Africa; on his death bed, this individual sold his head for half an English pound (12.50 francs) to an inhabitant of the colony of the Cape of Good Hope who gave it to us to be sent along" (SAP 1883).

8. In 1885, when a catholic missionary requested instruments, observation sheets, and instructions for the Guinea coast, the society established a commission to write up the instructions, without any result (SAP 1885).

9. Paul Neis, naval doctor stationed in the penal colony of Poulo Condor, then in Saigon, sent to the society first a series of 120 measures taken from living subjects in 1880; then photos in 1881; and finally 33 skulls, 2 skeletons, 16 brains, and 300 hair samples in 1882. A dozen years later, in 1894, Dr. Calmette sent the skulls of 65 convicts from New Caledonia. In 1900, a naval physician sent from Madagascar a skull with "identity papers," i.e., a racial certificate written by the officer who had deadly wounded its owner. Letter from doctor Danjou, Diego-Suarez, September 4, 1900, Papers of the Paris Anthropological Society.

10. "He [another doctor] must have told you as well that it is not always easy to acquire anthropological specimens. The Blacks want to keep their heads and those of their ancestors. I have nevertheless given instructions to my younger colleagues up and down the river and the coast to take advantage of any opportunity to send me anything they can find of interest so that I can send it along to the Natural History Museum. One of our young members is energetically studying photography in order to acquire the most types possible. We will start a collection as soon as he has achieved a good number. I have asked quite a bit from my thirty or so doctors." Lettre du 22 juin 1882, Ms. 2255, 1878–1883, Papiers Hamy, Bibliothèque du Muséum.

11. Deniker's book was reissued in 1926 with updated data. The number of groups measured in the French Empire grew (from 288 to 403 groups); still, the comparison remained unbalanced with less than 5,000 measured subjects (3,500 by 1900). In 1936, Jacques Millot and Paul Lester published a new synthesis to replace Deniker's, but they did not include statistical data (Millot and Lester 1936).

12. There were 145 European skulls; 69 skulls and 5 skeletons from Africa; 41 skulls and 3 skeletons from Asia (of which only one came from Indochina); 11 skulls and 1 skeleton from the Americas; 21 plaster molds sent to the Anthropological Museum of Florence; and finally 133 photos from Madagascar and Senegal (Verneau 1910). In 1938, the survey of Broca's museum by Vallois showed the same trend: two thirds of the skulls came from Europe; over half from France; and barely a fifth from Africa, Asia, or the South Pacific.

dissensions aroused by the Dreyfus case among French anthropologists.

The False Start of Colonial Anthropology in the 1910s

In February 1908, Robert Hottot, a well-off traveler who had been granted a mission to Congo and Chad, landed in Boma. He traveled with a young military physician, Léon Poutrin; they went up from the Atlantic coast to Lake Chad and back, measuring 600 subjects and bringing back more than 30 skulls. Hottot was more interested in zoology, but he was looking for an outstanding scientific achievement. Anthropological measurements seemed a good choice, though the measurements were mostly performed by Poutrin. A military physician, Poutrin had measured some draftees, but as soon as he was recruited for the mission, he rushed to the Natural History Museum and to Rivet, who requested a week to teach him his trade.[13] The mission could rely on much help from colonial authorities—in Belgian then in French Congo—from Catholic missionaries, and even from indigenous chiefs duly requested to assist them. Moored ships on the Congo River and barracks were privileged locations to measure willing or unwilling colonial subjects. The mission excavated any tomb of interest, and its members joined with the punitive raid led in the Lobaye area (French Congo) by a Captain Prokosh against villagers refusing to pay taxes.[14] In a letter published by the Paris Geographical Society, Hottot explained it was a good opportunity for ethnographical and anthropological investigations because these untamed people (the ones being attacked) were little known.[15] He collected 150 objects and 16 skulls in the assailed villages.

The mission was a success. Poutrin was assigned to the Natural History Museum from 1909 on to deal with the results, and he published several papers in *Bulletins et mémoires de la Société d'anthropologie de Paris* and in *L'anthropologie.* He became the referee for colonial missions, presenting the measures they eventually brought back. In 1912, he was commissioned to write an ethnological synthesis on French Equatorial Africa (Poutrin 1914). Furthermore, he was Rivet's right hand,[16] joining in the battle against the methodological weaknesses of French anthropology Rivet had already begun (Zerilli 1998). Poutrin's promising beginnings revealed a change in the official interest in scientific surveys, including anthropological ones. Some colonies were more supportive. For instance, the military administration in Chad was eager to expand its territory with the help of scientific information and to boast that it was carrying on a traditional military regard for learned activity. In other colonies, less aggressive administrative priorities opened new prospects for anthropological investigations. In 1913, a Captain Robert, stationed in Upper Tonkin, obtained permission from his superiors to conduct a census on all men from 18 to 60 years old whom he undertook to measure. This was accepted once the men to be measured realized there was no punishment or accompanying tax involved, and this made Robert—in his own words, the "gentle loon . . . known for his obsession of measuring people" (Robert 1918:34)—happy. Such testimony was certainly univocal; still, it underlined the resources and constraints of colonial sojourns that lasted several years and the constant play between coercion and conciliation.[17] It also stressed the rising relevance of anthropological investigations in a colonial situation, though they were obviously entrusted with many differing meanings, even when the measurer and measured subjects could reach some agreement.

Missions insufflated a new dynamic to colonial anthropology, but their mere object, "colonial races," was still confused. Hottot and Poutrin boasted they had built a thorough survey of races inhabiting the areas they had crossed, and Poutrin proved able to extend it to a racial survey of French Equatorial Africa. Still, they could not explain what could be achieved through such documents and measurements. Significantly, Poutrin focused his later works on the much-studied Babinga Pygmies and immersed himself in the minute, if classic, study of their skulls. He thus got caught between two possible meanings of race: the loose political one that permeated colonial discourse and the scientific one that was already deeply undermined by the dissensions subsequent to the Dreyfus case in French anthropological networks (Hecht 1997; Mucchielli 1997). Like many of his fellow anthropologists in Paris and in the colonies, he did not comment on this gap and could therefore not cross over it to propose a new rationality for colonial physical anthropology.

13. Letter from Poutrin to Hottot, December 1908, Oxford, Pitt-Rivers Museum, Hottot Papers, 2/138.

14. Meanwhile, administrative reports for this area grew more and more critical of Captain Prokosh, who tried to terrorize people and only increased the disorder as some people flew away and others took up arms to fight back. Aix-en-Provence, French Colonial Archives, Gouvernement général-Afrique équatoriale française, 5 D 10, Moyen Congo, pacification, 1908–1914.

15. "During several weeks, I traveled through the insurgent country, attending the military action and thus collecting many ethnographical documents, anthropological specimens and observations, which were all the more interesting as the M'Bagha race, utterly cannibal, savage and cruel, had been repellant to any attempt to civilization." Letter from Hottot to Paris Geographical Society, August 10, 1908, Oxford, Pitt-Rivers Museum, Hottot Papers, 1/1.

16. For instance, he was the first to review Franz Boas's major work on the changing forms of immigrants' skulls.

17. Measures collected in 1909 by Eugène Brussaux, a colonial administrator in French Congo, provide another interesting case. Brussaux was sent on mission in 1909, but instead of measuring people he met—rarely, indeed, as most of them hid away—he measured the Moundan carriers he had enrolled to accompany him, knowing them from his preceding colonial assignment, and he waited for a long stop in Léré to impose measurement on them. Aix-en-Provence, French Colonial Archives, Fonds ministériels, Missions, 106. Brussaux was a member of the Paris Anthropological Society. He died on his return journey to France, and his results were published in 1911 by Georges Papillault, who had trained him in Broca's laboratory (Papillault 1911).

"Colonial races" was a fabricated category amalgamating scientific and political logics. Colonial investigators, such as Delafosse, and young Parisian anthropologists, such as Rivet, did criticize simplistic uses of racial categories;[18] still, they were at a loss to untie the confusions about "colonial races." The much-praised paper on the "geographical distribution of races" in French Guinea published in 1906 by Charles Macland, a naval physician and a colonial administrator, exposed the intricate mixture of politics and science underlying the best-researched works on "colonial races."[19] Maclaud proposed a disparaging description of a warlike situation among the Guinean races, characterizing it as a "pandemonium" of races. He concluded that only colonization would stop the war by removing individuals from their racial sphere and melting them down in a "new race." Still, he denounced the "degeneration" of Guinean races, explaining that the Fula skulls he brought back in 1898 could not coincide with the referenced Fula skull in the collections of the Natural History Museum because of the reprehensible polygamous proclivities of the Fula, which thus exposed "their" race to miscegenation (Maclaud 1906). This was not just another piece of colonial prejudice. Maclaud had spent years in Guinea among the Fula, who successfully provided him with their prejudices against "savage" and non-Muslim aborigine populations and told him their own account of the war between two races that he then translated in his colonialist idiom. In other words, such studies were permeated with intertwined layers of racial cross-identification, and as these transactions implicitly supported colonial rule, their opacity baffled anthropological analysis.[20]

Meanwhile, some anthropologists revived the debate on the notion of race. In 1906, Louis Lapicque—associate professor at Paris Faculty of Sciences, a Dreyfusard, and a member of the Paris Anthropological Society—gave the prestigious annual Broca lecture. He demonstrated the unity of the black race in Africa, Asia, and the South Pacific, relying on anthropological investigations he had carried on between 1892 and 1903 in Asia and Africa. He methodically deconstructed the idea that the Negritos from Malaysia and the Andaman Isles formed a "pure race" retraining its original traits because of isolation. And he concluded with the unity of human species, remarking that races were "transitory groupings that call themselves peoples and believe themselves to be races" (Lapicque 1906). Lapicque did not question colonial use of anthropology. Upon his return from India, he praised well-trained English colonies, where a tight and eventually violent control of Natives made investigations easier (Lapicque 1904). But he was discontented with the confusion of races with ethnic groups. He thus questioned the fragile consensus, built around 1900 notably by Deniker, that there were myriads of differing but nonetheless equal races. Crossing over imperial boundaries, his work on Negritos urged for wider frames and suggested that colonial categories should be at least discussed. Yet there was a long way to go to disentangle colonial physical anthropology from colonial discourse, and the road was soon cluttered up by the clumsy attempts of the Paris Anthropological Society to back up colonial racial policies.

Lapicque led the way in May 1910 when most young critical anthropologists withdrew their membership from the society after tensions had flared with the inquiry on miscegenation initiated by a relevant committee in October 1908 (Saada 2002; Zerilli 1995). Intent to build new links with colonial authorities, teachers from the Paris Anthropological School chose to address the controversial issue of mixed-blood children. As they stuck to Broca's opinion that crossbreeding was lessening fertility, any debate with Lapicque or Rivet, who sat at first on the committee, soon died away. Nonetheless, a questionnaire was produced and dispatched in French West Africa in 1910 with the support of the general governor William Ponty. Thirty answers (for more than 100 administrative districts) were sent back. They provided unconvincing observations, as most mixed-blood children were still infants, and many observers lacked tools to measure them. Besides, the questionnaire was harshly criticized: a Catholic missionary deplored the questionnaire's racist bias; the governor of Dahomey, a colony where people of mixed ascent were numerous and influential, regarded the whole inquiry as a violation of socially sensitive issues bordering on political misconduct.[21] And the society proved powerless when Colonel Bonifacy, one of its most prominent members in Indochina and the father of a mixed-blood girl, asked its support to recommend the schooling of mixed-blood children so that they should be

18. In his major work, on *Haut-Sénégal-Niger* (present Mali and Burkina Faso) and its inhabitants, Delafosse explicitly thrust aside any racial classification, remarking that appreciation of skin color was too cultural to pass for a scientific unbiased settlement and claiming that linguistics and history were better resources (Delafosse 1912).

19. This essay was published in the prestigious geographical journal edited by Hamy for the Historical Sciences Committee. It was most favorably reviewed by Rivet in *L'anthropologie* (Rivet 1907). Rivet and Maclaud shared the same interest in anthropology in the broadest sense and belonged to the same scientific networks, as shown in their correspondence. Rivet Papers, Library of the Quai Branly Museum.

20. Parisian anthropologists, too, were aware that racial identification was a social fact. In 1906, Hamy wrote a long note on his former model at the laboratory of the Natural History Museum, Toukou le Haoussa. He paid homage to the "black prince" he identified as a Fula from Sokoto but who used to claim he was "Saïd of Tombouctou, first choice model for Algerian and oriental genres." He also told his life story: Toukou-Saïd had been captured in Sokoto, sold, and sent to Oran, where he grew up. He then fought in the Crimean War and settled in Paris, where he married a French woman and served as a model for artists and for Hamy's anthropological lessons (Hamy 1906).

21. "In Lower-Dahomey, we ran up against a serious difficulty: the self-esteem of the mixed-bloods who do not wish to account for any other than their white ancestors. This state of mind as been far too useful for far too long for us to wish to fight it now. It has been in any case the reasons for many vehement protests from them, who perceived the questions asked in the name of the Anthropological Society to be an intolerable attack of their personal dignity. No mixed-blood individual would want to take part in the survey under the prescribed conditions unless he were violently forced to do so" (*Revue d'anthropologie*, 1912, 366).

raised as French citizens. The society duly wrote to the department of colonies and was granted a delaying answer. But it could not help Bonifacy when his paper in the *Bulletins et mémoires de la Société d'anthropologie de Paris* was viciously used by a colonial journalist to prove that mixed children were doomed to turn Natives.[22] The survey on mixed-blood children thus illustrated the misunderstandings between the most conservative Parisian anthropologists and their putative colonial partners. It also exposed the limits of authoritarian management of scientific investigations in a colonial situation, and it worsened the divide among anthropologists because antiracist anthropologists condemned a failure they had foreseen.

Furthermore, this failure legitimated the cynical instrumentation of anthropology already promoted in 1900 by General Gallieni to impose his so-called "races policy" in Madagascar (Boëtsch and Savarèse 2000) and transferred to French West Africa by Ponty in the 1910s (Conklin 1997). In a very similar way, Colonel Charles Mangin used the Paris Anthropological Society to present his project of a French black army in 1911. The survey mission to prepare for the project entailed no anthropological investigations, and his discourse plainly contrasted the decline of the "French race" and the supposedly innate military gifts of some "black races" (Mangin 1911). The Paris Anthropological Society was but a tribune for his conference. Such shallow alliances deepened the confusion on "colonial races" and weakened the claims of physical anthropology to qualify as a colonial science, as ethnology successfully did in the 1920s.

The "Mission anthropologique de l'Afrique occidentale française": a Colonial Reinvention of French Anthropology?

If controversial, the 1910s were full of promises. Led by Delafosse, colonial investigators claimed ethnography as their choice science and propelled debate on fieldwork (Sibeud 2004). This dynamic also benefited anthropology. Delafosse was one of 50 members of the French Anthropological Institute created in 1911 to reorganize anthropology in its broadest sense. Before 1914, 71 papers were presented at the institute—very evenly distributed between anthropology, archaeology, and ethnography—and anthropologists engaged in debates the Paris Anthropological Society would not host. This dynamic was ruined by the war. When the institute resumed its activity in November 1919, the war had taken a heavy toll among young anthropologists. The tensions inherited from the 1910s became an impassable gap when anthropologists from the Paris Anthropological School launched a rival International Anthropological Institute in 1919. They explicitly banned German scientists and claimed positions indistinguishable from those of the racist "anthropo-sociology." The biased internationalism of the new institute was soon denounced, particularly by Franz Boas, who began a lasting correspondence with Rivet after both turned down the invitation to join the first congress of the new institute (Zerilli 1998). Thus, the gap dramatically deepened between anthropologists criticizing the notion of race and working along with social scientists and those obstinately locked in the past supremacy of their speciality grounded in race.

The price paid for this was the decline of the Paris Anthropological Society, its isolation from current international debates, and its image as representative of a tedious old science that one ought to study but that did not offer any professional prospects. Though training continued at the Sorbonne's new Ethnological Institute,[23] research and debates in physical anthropology were deserted. Rivet devoted his attention to cultural ethnology and woke up quite late indeed, in the middle of the 1930s, to realize that physical anthropology had been seized on by racist theories. As Alice Conklin demonstrated, Rivet responded by displaying skulls in the new Musée de l'homme to build on this lasting popularity of anthropology. He also presented physical anthropology as a useful auxiliary speciality in the volume *Human Species* he directed for the French encyclopedia edited by Lucien Febvre. He used prosaic images: as a handrail, physical anthropology could help one to climb, but it was hardly the end goal. He also compared physical anthropology with meteorology: both accumulating data with little intrinsic meaning and still useful when built on by other sciences (Rivet 1937). This pedagogical ecumenism could, however, hardly compensate for debates that had been skipped, and Rivet engaged in the battle against racism at the Vigilance Committee of Antifascist Intellectuals (Conklin 2002).

The evolution of physical anthropology practices was just as puzzling. Research was still carried on, notably in the colonies. But World War I provided ambivalent opportunities, as many colonial soldiers were stationed in France. Colonial observers built on the examination of draftees to present "colonial races," as did Gaston Joseph, a colonial administrator in Ivory Coast and a member of the society from 1910 on.[24] Metropolitan anthropologists, such as old Ernest Chantre from the Lyon Anthropological Society, benefited from the proximity of a hospital for colonial soldiers to measure some of them (Chantre 1927). Such observations erased the fragile progress made in the 1910s toward a critical debate on fieldwork, as they allowed new subrogate practice. The prejudice in favor of laboratory examination of a few anthropological

22. Letter from Bonifacy to the society, July 9, 1913, Paris, Papers of the Paris Anthropological Society. Auguste Bonifacy, "Les métis franco-tonkinois," *Bulletins et mémoires de la Société d'anthropologie de Paris*, December 1, 1910, 607–642.

23. Anthropology was taught by Rivet and then by Paul Lester.

24. He published ethnographical works in the *Bulletins et mémoires de la Société d'anthropologie de Paris* before 1914. He served as secretary to the governor of Ivory Coast from 1914 to 1917 and used data on draftees to draw a rather biased portrait of Ivory Coast "races," contrasting the beauty of Malinke, lasting allies of French colonization, with the ugliness of Baule, "true niggers" who fiercely opposed French conquest until the 1910s (Joseph 1917).

pieces was reinforced, too, and such studies still outnumbered fieldwork in the *Bulletins et mémoires de la Société d'anthropologie de Paris* in the interwar years. Besides, as ethnology scarcely included physical anthropology investigations,[25] colonial investigators were once more left to their own devices.

Meanwhile, circumstances of colonial investigations underwent deep changes. The Sorbonne Ethnological Institute was mostly funded by colonial subsidies, and it trained colonial administrators who then joined colonial research institutes that were steadily developing. Training at the Sorbonne Institute was highly recommended but not compulsory. Ethnology benefited all the more from this ambivalent status. Colonial professions were still seeking social prestige and valued ethnology as their special erudite form of educated leisure. Conversely, physical anthropology was not funded by colonial authorities and seemed less congruent with the rather literary formation of their agents. Still, it appeared more and more as the preserve of colonial physicians. The war escalated the draft, which had been imposed on colonial subjects in 1912, and in the 1920s, a civilian contingent was added for forced labor (existing in the French Empire until 1946). Colonial physicians were decisive to these extensive recruitments: soldiers and laborers had to be fit. Significantly, one achievement of anthropological measurements accompanying such examination consisted in adjusting current robustness ratings scales to African morphology (Lefrou 1931). Moreover, new hygienist policies invested colonial physicians with a more prominent part in colonization and enlarged their access to colonized populations as services of medical assistance for Natives were more or less efficiently organized (Conklin 1997). The recruitment and qualification of colonial physicians significantly improved in the 1930s: they were more numerous, and specific academic examinations (*agrégations*) were created for the most brilliant so that they could teach in colonial medical schools (Michel 1985). The first colonial *agrégés* clustered around the Tropical Medical Institute of the Military Health Service in Marseille and soon elected physical anthropology as their choice leisure practice to add scientific gloss to their new body.

They practiced fieldwork and laboratory research, and instead of relying on the resources of the Paris Anthropological Society, they offered a possible association with the new discourse on colonial development (Cooper 1996). Furthermore, as some of them advanced from colonial to metropolitan positions, they delineated a socially satisfying way of filling the gap between fieldwork and metropolitan investigations. They were not merely seeking the vanishing prestige of the Paris Anthropological Society. Racial categorization served as a framework for spotting robust soldiers or workers and for writing medical reports based on touring to assess public health and medical needs in a particular colonial district. Colonial physicians thus turned to anthropology to thoroughly study "colonial races." They approached the notion as a heritage from medical geography and as an essential tool for their professional practice. Some of them obviously agreed with racist theories, but their interests in "colonial races" also revealed how the new development policy could reinforce racial categorization regardless of scientific belief (Lachenal 2009).

Modernizing the study of races to reclaim supremacy for anthropology against cultural ethnology was also the project of Henri Vallois, professor of anatomy at the Toulouse Medical School since 1922. Vallois was in 1928 the unsuccessful rival of Rivet as the chair of anthropology in the Natural History Museum. After 1938, he taught at the Institute of Paleontology, edited *L'anthropologie*, and ran the Paris Anthropological Society. Vallois took advantage of the controversies flaring between racist and antiracist scientists, choosing an ambiguous medium position (Bocquet-Appel 1989). He maintained that racial classification was possible, though it should not be confused with a hierarchy allowing for unequal rights and unequal treatment. Measurements should be simplified and standardized to produce statistical data. Besides, physical anthropology should extend to comparative physiology. This was an appealing project for colonial physicians who could easily enter new prospects conveniently relying on extensive collecting. Asking in 1939 for a "systematic study of races in the French Empire," Vallois build on their partnership to delineate an ambitious, if new, agenda for colonial fieldwork in physical anthropology (Vallois 1939). If he deplored French delays in the field, he was eager to praise any anthropological "tradition" in the colonies. Still, his presentation was rather biased. He loudly applauded the creation of the Anatomic Institute of the School of Medicine in Hanoi but neglected to mention the Indochinese Institute for the Study of Man, where anthropologists and ethnologists (graduated from the Sorbonne Institute) had been working together for a couple of years. This omission was significant of the gap existing between commitments to anthropological research in some colonies and his rearguard fight against cultural ethnology and very conservative understanding of race. Investigations in physical anthropology in Hanoi were indeed a belated addition to the thorough archaeological and ethnological study of earlier and contemporary Vietnamese races in which colonial and Vietnamese physicians worked side by side in both institutes. However, on the eve of World War II, Vallois and colonial physicians concluded a fruitful alliance: he backed his racial studies with colonial relevance and salvaged the Paris Anthropological Society,[26] and with their sup-

25. Anthropology almost disappeared even from the French Anthropological Institute: only 15 of the 244 papers presented from 1920 to 1930. And a sole colonial graduate from the Ethnological Institute, Jean Leca, undertook anthropological investigations in 1927 in the French Sudan.

26. Vallois visited Syria in 1935–1936 to study ancient human remains recently excavated. He was also interested in the quite classical Babinga Pygmies from Cameroon, where he would go in 1946–1947 (Vallois 1950).

port he brokered a deal in which they qualified after 1945 to carry out a grand "Anthropological Mission" in French West Africa.

The short story of the Anthropological Mission was the most ironic moment in the history of practicing anthropology in the French Empire. By the end of the war, recovering the empire implied demonstrating that French colonial policy met the basic needs of colonized populations and would eventually insure their welfare. A thorough study of African nutrition claimed precedence in 1945 when Léon Palès, a colonial *agrégé* and an anthropologist, presented the colonial ministry with his project: a survey of West African races. By amalgamation, this project became the working basis for the needed survey on African nutrition, and the misunderstandings produced lasted until 1951, when Palès and his anthropological studies were dismissed (Bonnecase 2009). Palès had been trained at first by Vallois in Toulouse, then in Marseille.[27] After Rivet had to flee to South America in 1941, his chair was given to Vallois, who claimed Palès as his assistant in the Musée de l'homme. Both were removed in 1945; still, Palès benefited from the efficient solidarity of colonial *agrégés*, some of them being war heroes. Moreover, as they worried that new research institutions, such as the Office of Colonial Scientific Research, would encroach on their own research institutes, they supported Palès's project. He thus rallied metropolitan and colonial funding, though his project was confined to accumulating measurements to produce indisputable data, to add physiologic studies, and to build a supposedly definitive map of "colonial races" in French West Africa. Attention to the nutrition part of the survey was minimal, too: Palès plainly argued that knowing races would help meet their basic needs. The grand Anthropological Mission in the French Empire thus relied on a politically questionable anthropologist and on a poorly planned project.

By 1951, the mission was obviously a failure. Palès collected enough measurements to boast statistical relevance (700,000 measures from some 10,000 subjects), but his basic frame, "colonial races," proved scientifically and politically wrongheaded. At first, he measured soldiers in barracks around Dakar and was thus trapped in an already lost battle: defending the virtues of military anthropology (Palès 1949). He then toured colonial territories, giving a detailed account of the kilometers traveled in his yearly reports. He proudly signaled that his statistical conclusions usually confirmed racial identifications proffered by the trusted chauffeur of the mission, and his reports made it plain that he heavily relied on such transactions because racial identification was both the premise and the conclusion of his inquiries. Conversely, his complaint that Natives were prone to offer assumed racial identities revealed that he was actually besieged with social dynamics he discerned but could not make out.[28] Palès presented the Paris Anthropological Society with several papers on his mission, thus contributing to the rebirth of its journal. Still, after 1951, when he was elected again as deputy curator for the Musée de l'homme, on a less-controversial basis this time, he rapidly deserted the data he had accumulated to return to his primary research on French prehistory. Apart from a huge and incomplete map of races in French West Africa, uselessly detailing and fixing varying racial identifications, he could never present any practical result from his mission. And colonial authorities simply got rid of the anthropological mission in 1951, as they were criticized by international bodies for the irrelevant data they sent regarding nutrition.

Nevertheless, anthropological investigations did not disappear from French colonies. In Dakar, Palès contributed to medical research organized by the Pasteur Institute, and he initiated a survey on the incidence of goiter. Analyzing the blood samples extensively collected by his mission, he presented them as another proof of the unity of human physiology. Like Palès, most colonial physicians combined anthropological investigations with medical research. Though they still favored monographic studies of "colonial races," they also engaged in comparative anatomy or blood formula surveys that highlighted the unity of human species and incidentally used racial categorization as medical guides. In their own pragmatic way, they upheld the broad definition of anthropology by combining all these investigations, thus contributing to the long-awaited modernization of French physical anthropology. They obviously shared the conservative vision of races defended by Vallois,[29] and most of them approved colonial hierarchies relying on racist prejudice; still, they cannot be pictured as efficient promoters of scientific racism. They rather evaded the debate on race, submitting to the new international agreement that races were essentially equal and going on with their descriptive use of the old confused notion of "colonial races," eventually embracing the less-controversial notion of ethnic group. Besides, the failure of Palès's anthropological mission convinced them to revel in their amateur status—the more "professional" effort had failed—and they actively supported, with the papers they presented and with their fees, the rebirth of the Paris Anthropological Society.

27. He was due to embark as colonial physician with the Dakar-Djibouti Expedition in 1931 but was left behind by lack of funding. Marseille, Service de Santé des armées, Archives de l'Institut de Médecine Tropicale, file 417.

28. In 1949, he measured 50 Moors, thanks to the governor general's helpful interpreter who presented himself as an authentic Trarza Moor and summoned merchants and jewellers on business to Dakar, guiding them to the anthropological laboratory in the *Institut français d'Afrique noire* and overcoming their reluctance with a speech of his own on the beauties of scientific research (Palès 1949). Palès noted the scene in his study on Moors but did not quote the discourse. His deafness to African answers to his research is an obvious shortcoming of the thick reports he wrote.

29. After Rivet retired in 1949, Vallois was elected to the chair of the Natural History Museum, and he served in many international committees, notably at UNESCO as chair for the International Union of Anthropologists from 1951 till 1962.

The part they played in decolonization is little known and should be thoroughly explored, as they were obviously quite ambivalent agents, training Native physicians and meanwhile studying, eventually with them, their "races." After decolonization, as the society became one of their few metropolitan bastions, the shadow of empire, distorted and certainly enlarged, quite heavily weighted on French anthropology till the 1980s, when historians began systematically probing its past (Blanckaert, Ducros, and Hublin 1989).

As a colonial practice, physical anthropology proved rather delusive: investigations required a command on bodies and territories colonization unevenly attained, and findings obsessively pointed toward the need for a debate on the notion of race in which neither colonial authorities nor most anthropologists cared to engage. As metropolitan anthropologists and colonial agents tried in the 1910s to answer the call to create new colonial knowledge by setting out on more systematic investigations, they proved unable to overcome their dissensions to engage any further in the debate on the notion of race. Colonial races remained blurred categories, and linguistics or cultural ethnology provided a sounder basis to classify colonized people. Physical anthropology thus appeared as a peripheral and eventually troublesome form of leisure, implicitly reserved to colonial physicians after World War I.

By the 1940s, changes in the formation and positioning of colonial medical staff and the development of medical care for Natives offered new grounds and new bodies for investigations in physical anthropology. Still, if combining medical and anthropological research partly cleared anthropology from its lasting association with the most brutal colonial constraints, most investigations simply draw on the inherited descriptive frame of "colonial races," thus giving it a renewed legitimacy and hindering once again any thorough debate on the notion. Yet reckoning that physical anthropology was neither an efficient colonial tool nor a racist plot projected from the metropolis on the colonies could be the most efficient plea to further the study of physical anthropology investigations in the French Empire and more generally in any colonial setting. Their obvious frailty and the uncertainties regarding their political uses invite historians to develop a better understanding of these investigations. They were indeed fascinating and highly ironic colonial encounters: bodies had to be handled, and constraint was seldom the sole way to get access to them. However deaf colonial investigators were to local discourses on race, they could not prevent any interference with them whether in their own understanding or, less openly, in what was presented or not to their investigation.

There is still much work to be done to understand how the participants in such anthropological situations accommodated one another and came to terms with their diverging discourses, practices, and projects. Such studies shall enlarge the social history of colonization; they shall also contribute to a history of racism fully acknowledging the diffracted reverberations of colonial experience.

Acknowledgments

I wish to thank Susan Lindee for her careful editing of this paper and the referees for their stimulating comments.

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Racial Hybridity, Physical Anthropology, and Human Biology in the Colonial Laboratories of the United States

by Warwick Anderson

In the 1920s and 1930s, U.S. physical anthropologists imagined Hawai'i as a racial laboratory, a controllable site for the study of race mixing and the effects of migration on bodily form. Gradually a more dynamic and historical understanding of human populations came to substitute for older classificatory and typological approaches in the colonial laboratory, leading to the creation of the field of human biology and challenges to scientific racism. Elite U.S. institutions and philanthropic foundations competed for the authority to define Pacific bodies and mentalities during this period. The emergent scientific validation of liberal Hawaiian attitudes toward human difference and race amalgamation or formation exerted considerable influence on biological anthropology after World War II, but ultimately it would fail in Hawai'i to resist the incoming tide of continental U.S. racial thought and practice.

In 1920, Henry Fairfield Osborn, the forceful president of the American Museum of Natural History, wrote to a young physical anthropologist on his staff telling him how to conduct research into pure Polynesians and mixed-race people in Hawai'i. Osborn had recently returned to New York from the islands—the territory of the United States—having found their exotic beauty enthralling and their inhabitants amenable to racial study. Like many other visitors, Osborn took surfing lessons on Waikiki with Duke Kahanamoku, the Olympic swimmer, whom he regarded as a "model chieftain type." "Do not fail to make the acquaintance of Duke," the keen eugenicist Osborn urged Louis R. Sullivan, "and secure his measurements, ascertaining if you can, without giving offence, whether he is full blooded."[1] In particular, Osborn wanted the diffident, frail anthropologist, a student of Franz Boas at Columbia University, to "obtain any data regarding swimming adaptations in the limbs and feet." He hoped, too, that bathing and surfing in the refreshing climate would improve Sullivan's consumptive tendencies. Additionally, Osborn demanded measurements of other types, including "fishermen," "poi makers," "tapa makers," and "hula dancers." He heard that the "Hawaiian and Chinese blend is an excellent one; in the schools, intelligent, upright, persistent." Collecting "primitive" types was compelling because Osborn planned a Polynesian hall at the American Museum; the United States boasted a "historic connection" with Hawai'i, and the evaluation of racially mixed peoples might offer insight into contemporary social problems on the mainland, including New York.[2]

During the 1920s, physical anthropologists from the American Museum of Natural History and Harvard University treated Hawai'i as a racial "laboratory," a controlled site where they might assess an experiment in human biology (MacLeod and Rehbock 1994). They came to the Bernice P. Bishop Museum in Honolulu to study the origins of Polynesians and the process of contemporary race formation in the islands, presumably the result of environmental adaptation of newcomers and hybridization between different groups.[3] In this sense, anthropologists such as Sullivan and his successor Harry L. Shapiro pursued a Boasian program in physical anthropology, elaborating on their mentor's earlier work on race mixing and the modification of the bodies of immigrants, and producing dynamic and historical accounts of human difference (Boas 1910; Herskovits 1953; Kroeber 1942). Even though conservative eugenicists such as Osborn and his friend Charles B. Davenport initially had promoted research in the islands, the Pacific soon became a Boasian laboratory—to their consternation—a workshop for investigators skeptical of racial typologies and fixities. Most of these rising anthropologists arrived in Hawai'i already discontented with the

Warwick Anderson is Research Professor in the Department of History, University of Sydney (School of Philosophical and Historical Inquiry, Quadrangle A14, Sydney, New South Wales 2006, Australia [wanderson@usyd.edu.au]). This paper was submitted 27 X 10, accepted 12 VIII 11, and electronically published 7 II 12.

1. Osborn to Sullivan, 7 July 1920, folder April–June 1920, box 10, Central Archives, American Museum of Natural History, New York.
2. Osborn to Sullivan, 17 April 1920, folder April–June 1920, box 10, Central Archives, American Museum of Natural History, New York.
3. The Bishop Museum was established in 1889 on the grounds of the Kamehameha School, both endowments of Bernice Pauahi Bishop, the last descendant of King Kamehameha.

 0011-3204/2012/53S5-0009$10.00. DOI: 10.1086/662330

complicated and contradictory typological enterprise, and experiences there propelled their drift toward racial recusancy. The vast sea of islands, with Hawai'i in the middle, proved an exemplary site where physical anthropology could be refashioned and a new human biology might emerge.

Research into the human biology of Hawai'i therefore reflected the crisis in physical anthropology between the wars. It took place as older certainties about racial boundaries and the cultural capacity of human types were coming under challenge or succumbing to the burden of accumulated contradiction, inconsistency, and confusion. Many of the anthropologists working in Hawai'i thus represent transitional figures between the older classificatory physical anthropology and the emerging more-dynamic biological anthropology—moving intellectually from anatomical essentialism to increasingly complex biological or ecological conceptions of human diversity. Experiences in Hawai'i influenced their trajectory. Ironically, U.S. physical anthropology would become more worldly and cosmopolitan—perhaps even postcolonial—through the exigencies of conducting research in this special colonial setting, a site where traditional relationships and identities at the same time were undergoing transformation and coming to assume more conventional "national" forms. Even as a segment of U.S. physical anthropology was, in a somewhat romantic sense, being "Hawaiianized," the social reality of Hawai'i increasingly came to resemble continental U.S. structures and patterns.

A Limitless American Lake

Facing west from California's shores in the 1920s, the Pacific looked like a limitless American lake. Since 1898, the United States had acquired the Philippines, Guam, and numerous other colonial possessions, annexing the Hawaiian Islands and incorporating them as an insular territory. From the late nineteenth century, white Americans, locally called *haoles*, settled in Hawai'i, dominating commerce and government in the islands. In search of cheap labor, planters imported Japanese and Chinese workers and their families; later, Filipinos, Portuguese, and Puerto Ricans joined them. As the number of "pure" or "Native" Hawaiians dwindled, the ranks of immigrants and mixed-race peoples swelled (Lind 1955; Merry 2000). Despite recurrent communal tensions and the deprecation of supposed clannishness among Japanese, the islands developed a reputation for free mingling and tolerance among the races. Visiting Hawai'i in 1925, the young Australian historian Stephen Roberts regarded it as "the best example of racial experimentation in the Pacific and, in many respects, the whole world"; in his magisterial survey of population problems in Oceania, Roberts described the islands as "an admirable observation post for those interested in interracial relations," the "melting pot" of the Pacific, a "veritable ethnographic museum," a "racial laboratory," the "statistician's playground," and the "happy hunting ground" of the "racial enthusiast" (Roberts 1927:315–316). Like so many other liberal scholars, Roberts became obsessed with the polymorphous attractions of Oceania. In particular, Hawai'i was for him a "testing ground in the amalgamation of Occident and Orient [where] the future lies not with the obliteration of everything pertaining to any of the component cultures, but with harmonious cooperation, a blending" (see Anderson 2009:149).

After World War I, the American cultural and scientific discovery of Hawai'i—and Polynesia more generally—was predicated on a sense of ownership, or at least the need to assert possession before the Japanese did so. Writers and filmmakers especially found the islands alluring, staging seductively multiracial encounters, sometimes coupling dusky maidens to white beachcombers or brave mutineers. Although the South Seas would receive more lurid treatment, Hawai'i also began to be represented as a special place of carnality and pleasure available for white American rest and recreation. Waikiki, adjacent to Honolulu, was growing into a tourist destination, allowing the old Moana Hotel and Halekulani to expand and the new Royal Hawaiian Hotel—the pink palace—to squeeze in between them. During the 1920s, the U.S. administration drained the Waikiki wetlands, built a local natatorium and zoo, and completed the Ala Wai canal, opening the area for real estate development. Hollywood added to the islands' appeal with the release of *Hula* (1927), in which the new *It* girl, Clara Bow, played the sexy half-Hawaiian daughter of a pineapple king. From the late twenties, Johnny Noble led the band at the Moana, inventing *hapa-haole* (or mixed-race) music and producing hits such as the "Hawaiian War Chant" and "My Little Grass Shack in Kealakekua." Hawaiian music rapidly gained popularity in the 1930s, shaped largely by Harry Owens and his orchestra at the Royal Hawaiian, which featured the hula dancer and comedian Hilo Hattie. From 1935, Webley Evans hosted the radio show "Hawaii Calls" under the banyan tree at the Moana, bringing the sound of the waves on the beach at Waikiki to millions and offering them brief respite from continuing economic depression and mainland social conflict. Soon Bing Crosby was crooning "Blue Hawaii" and "Sweet Leilani"—Owens's Oscar-winning song—in the film *Waikiki Wedding* (1937), in which he played a smooth public relations man extolling Hawai'i as he tried to suppress Shirley Ross, the caustic Miss Pineapple Princess. The next year, Fred MacMurray joined Owens and his Royal Hawaiians in *Cocoanut Grove* (1938), another immensely successful musical. No wonder, then, so many white American scientists and scholars were flocking each winter to their very own land of aloha.

Race Crossing in America

Louis Sullivan, Osborn's young emissary, was not the first mainland expert to evaluate racial diversity and mixture in Hawai'i. After studying the decline of the northern "Negro," the punctilious statistician Frederick L. Hoffman traveled to the islands to investigate the effects of Pacific "miscegenation." Not surprisingly, his analysis of vital statistics revealed the supposedly baleful results of "Hawaiian mongrelization," thereby confirming his prejudices (Hoffman 1916, 1917, 1923). Alfred M. Tozzer, the Harvard anthropologist, was rather more sympathetic. From 1916, he visited his wife's (*haole*) family on Oahu each summer and measured the bodies of Chinese-Hawaiian and white-Hawaiian neighbors. After struggling with the statistics of race crossing, Tozzer, a close friend of Boas, handed over his data on 508 subjects to Leslie C. Dunn, a progressive young geneticist. While lamenting the unreliable "pedigrees," Dunn could find no signs of "degeneracy" among the mixed offspring—by which he meant no obvious physical disharmony or mental deficiency.[4] He noted that the first generation of European-Polynesian crosses showed native pigmentation and lacked hybrid vigor, but supposedly Hawaiian corpulence disappeared and finer European features emerged. Dunn complained of the difficulties calculating white hybrids: whites seemed too heterogeneous to fit one type or even to sort neatly into conventional Nordic, Alpine, and Mediterranean divisions (Dunn 1923). After further analysis, Dunn (1928:2) decided that Hawaiian-Chinese crosses generally reverted toward their Asian ancestry in what he called "this great experiment in race mixture."

Race mixture or miscegenation excited considerable scholarly interest and public indignation in the continental United States during the early twentieth century. According to the 1910 census, the number of self-identifying "mulattoes" in the U.S. population had risen to two million, more than 20% of African Americans. This development prompted concern among some white social theorists. In 1918, Madison Grant (1918) predicted the passing of the great white race: "mongrelization" across the globe was leading to dilution and degeneration. A few years later, Lothrop Stoddard (1921) echoed Grant's predictions. Through the 1920s and 1930s, marriage between African Americans and European Americans remained illegal in more than 40 states but not in the insular territories (Hollinger 2003; Kennedy 2003; Moran 2001; Pascoe 1996; Sollors 2000; Spickard 1989; Williamson 1980). In 1924, Virginia promulgated the "one-drop" rule to define more rigidly the boundaries of white identity. The following year, Leonard "Kip" Rhinelander scandalized New York when he sued Alice Jones for passing as white and deceptively luring him into marriage. Black men accused of lustful behavior toward white women were still being lynched in the South. In 1935, the African American intellectual W. E. B. DuBois observed that fear of race mixing was "the crux of the so-called Negro problem in the United States" (DuBois 1980 [1935]:99). Nonetheless, in places such as Harlem, New York, a self-conscious and assertive "mulatto" culture emerged during this period (Huggins 1973; Watson 1995).

American physical anthropologists and scientists tried to elucidate the biological principles of this controversial social issue. Even in the 1890s, Franz Boas, a liberal Jewish-German émigré inspired by the environmentalism of his mentor Rudolf Virchow, was scouring American Indian reservations and boarding schools looking for "half bloods" to measure. He noticed that rather than blending their ancestry, mixed children manifested features favoring one or the other parent, but he thought this segregation of heredity scarcely constituted "degeneration," however defined. Indeed, mixing seemed to have a "favorable effect upon the race" (Boas 1902, 1940 [1894]; Stocking 1982). Miscegenation also intrigued less sympathetic physical anthropologists. "I am seeking information concerning the offspring of mulattoes," Charles B. Davenport wrote in 1906 to Aleš Hrdlička at the Smithsonian Institution. "That is, I wish to learn if white skin color and black are produced as well as mulattoes. Are such pairs of mulattoes perfectly fertile and are their children vigorous?"[5] The anatomist Hrdlička was stumped. He suspected three-quarters of the people of color in Washington, DC, were part white, but the "question of the mixed bloods of white and Negroes and of their progeny still awaits scientific investigation."[6] Over the following years, Hrdlička frequently urged the aging eugenicist to use the resources of the research station at Cold Spring Harbor, New York, to look into this question.[7] But not until the late 1920s did Davenport enlist Morris Steggerda to measure and assess sociologically mixed-race people—and then in Jamaica. By this time their condemnation of disharmonious race crossing would appear exceptionally vehement and absurd. The scientists worried that Jamaican "hybrids," inheriting the short arms of whites and the long legs of blacks, had trouble stooping and picking things off the ground; browns became "muddled and wuzzle-headed" (Davenport and Steggerda 1929:469).[8]

Boas and other physical anthropologists sought to counter facile Mendelian defamations from Davenport and his kind.[9]

4. Dunn to Davenport, 20 August 1921, series I, Charles B. Davenport papers, B D27, American Philosophical Society, Philadelphia (hereafter cited as Davenport papers).

5. Davenport to Hrdlička, 27 November 1906, Davenport papers; see Davenport (1913, 1914, 1917). On Davenport and his Eugenics Record Office at Cold Spring Harbor, see Allen (1986), Kevles (1995), and Rosenberg (1983).

6. Hrdlička to Davenport, 28 November 1906, Davenport papers.

7. Hrdlička to Davenport, 19 May 1915, Davenport papers.

8. For criticisms, see Castle (1930) and Pearson (1930). In a strongly worded letter to Davenport, Boas wrote, "The point which I rather stress is that the distribution of organic differences in each race is so wide that racial differences do not amount to much" (3 April 1929, Davenport papers).

9. Tozzer reported to Boas that Hrdlička "told Hooton he did not like Davenport (but who does?), but he added he must keep in with him for fear he (Davenport) might black-ball him as he was coming up for election to the National Academy. Isn't that crude?" (9 September 1919, series I, Franz Boas papers, B B61, American Philosophical Society, Philadelphia [hereafter cited as Boas papers]). Later, Tozzer wrote complaining that there was "too much abroad of the Madison Grant and Lothrop Stoddard variety of anthropology" (21 May 1923, Boas papers). After reading Grant's work, Hrdlička let Boas know "I do not remember having ever seen a book either more pretentious or more biased" (6 May 1918, Boas papers).

In 1911, Boas had written, “Notwithstanding the oft-repeated assertions regarding the hereditary inferiority of the mulatto, we know hardly anything on the subject. . . . Owing to the very large numbers of mulattoes in our country, it would not be a difficult matter to investigate the biological consequences of this question thoroughly (Boas 1911:274).” “The importance of research on this subject,” he continued, “cannot be too strongly urged since the desirability or undesirability of race mixture should be known” (Boas 1911:275). In the 1920s, Boas encouraged his Columbia graduate student Melville J. Herskovits to study the physical attributes and cultural accomplishments of the mixed-race black Americans creating the Harlem Renaissance. But efforts to snare research subjects proved futile until Herskovits enlisted the apparently insouciant but secretly disgruntled Zora Neale Hurston to stand on a street corner holding a pair of calipers and enticing anyone eligible to be measured. Herskovits discovered these “New World Negroes” were strangely homogeneous physically despite mixed ancestry, and he praised their efforts to develop a distinctive civilization (Herskovits 1927, 1929, 1930; see also Gershenhorn 2004; Hemenway 1977). Meanwhile, out on the prairie, the Minnesota anthropologist Albert E. Jenks (1916, 1917) followed Boas in measuring and evaluating children of mixed European and Native American parentage, extolling their potential for assimilation into modern society. Earnest A. Hooton, the leading anthropology professor at Harvard—the major center for training physical anthropologists (Spencer 1981, 1982)—declared that the study of miscegenation was “perhaps the most important field of research in anthropology today” (Hooton 1926:312; see also Hooton 1921). He taught a graduate course on the biology of race mixing, and from the early 1920s he sent out most of his advanced students—the Peabody boys—to the Pacific and elsewhere in order to investigate the results of recent racial amalgamation (Anderson 2006).

Several biologists—most of them located at elite institutions on the eastern seaboard of the United States—shared this interest in the contemporary consequences of interracial sex. Generally, their speculations on the quality of mixed offspring were ambivalent though occasionally surprisingly favorable. In 1926, William E. Castle, a professor of zoology at Harvard, suggested that race crossing produced a population “more adaptable to a new or changing environment either physical or social” (Castle 1926:151). Opposition to miscegenation was, he claimed, based on “assumption backed up by loud-voiced assertion” (Castle 1926:147). There was no obvious biological impediment or risk. Herbert Spencer Jennings, a Johns Hopkins zoologist, declared that “the mating of two slightly diverse races often gives offspring that are superior to either [parental] race”; yet he remained concerned about the mating of two very different types or of low-grade individuals of any type (Jennings 1930:276). Most of these biological estimates of race mixing took place within the context of plant and nonhuman animal breeding. Thus Castle discussed the issue in his genetics textbook, subtitled “a reference book for plant and animal breeders,” and E. M. East and D. M. Jones’s 1919 botany text *Inbreeding and Outbreeding* (East and Jones 1919) concluded with a chapter on human reproduction. But field studies of human populations remained rare.[10]

The Human Biology Laboratory

Leaders of U.S. philanthropic foundations observed the anthropological fascination with human hybridity and race formation and considered putting the issues to the test in the American Pacific. Davenport was among the first to suggest this exotic research site. In 1914, he had visited Melbourne, Australia, to attend the annual meeting of the British Association for the Advancement of Science, and on his return journey he stopped at the Brewarrina Aboriginal mission in outback New South Wales, where he conducted a perfunctory and disparaging examination of the local “half castes” (Davenport 1925). Steaming back to California, he was startled by Oceanic miscegenation. He wrote of New Zealand to Robert S. Woodward, the president of the Carnegie Institution of Washington, his loyal supporter: “There is much in the sheep breeding methods and in the hybridization of human races there that might add to the value of my scientific work.”[11] Tahitians also impressed Davenport, because they were evidently Aryan with hardly a trace of “Negroid blood.”[12] Adding his endorsement, Aleš Hrdlička wrote to Woodward telling him “it is most desirable to solve, if possible, the problem as to exactly who the Polynesians are.” For years the Smithsonian anatomist had been studying skeletons plundered from Hawaiian cliff burial sites, but he remained puzzled. “I know that you are deeply interested in the Pacific problem,” Hrdlička slyly noted.[13] “I beg to assure you that I have a lively interest in this matter,” responded Woodward.[14] “Without the help of physical anthropology,” Hrdlička persisted, “the problems of the Pacific cannot possibly reach their solution.”[15] Yet the Carnegie Institution firmly would resist any major Pacific investment until the appointment of president John C. Merriam, a paleontologist from the University of California. In 1922, Merriam assured the president of the Bishop Museum in Honolulu that he was “in more or less continuous contact with the work of organizations concerned with Pacific prob-

10. For partial overviews of the biology of race crossing, see Provine (1973) and Farber (2003, 2009).

11. Davenport to Woodward, 4 April 1914, folder 3, box 3, subseries 3 (Department of Genetics), series 2, Administration Archives, Carnegie Institution of Washington, DC.

12. Davenport to Woodward, 10 August 1914, folder 3, box 3, subseries 3 (Department of Genetics), series 2, Administration Archives, Carnegie Institution of Washington, DC.

13. Hrdlička to Woodward, 31 July 1919, Hrdlička folder, box 18, Correspondence Files, Carnegie Institution of Washington, DC.

14. Woodward to Hrdlička, 1 August 1919, Hrdlička folder, box 18, Correspondence Files, Carnegie Institution of Washington, DC.

15. Hrdlička to Woodward, 2 August 1919, Hrdlička folder, box 18, Correspondence Files, Carnegie Institution of Washington, DC.

lems."[16] Working through the Bishop Museum, the Carnegie Institution intended to provide funding for anthropological research in the region, including if possible Sullivan's studies of Hawaiian miscegenation. Yet older patterns of patronage soon were reasserted. Thus, on this occasion, Carnegie chose instead to sponsor Davenport's Jamaican studies, although Merriam soon regretted the decision and resolved to direct funds elsewhere in the future.

Attending the first Pan-Pacific Science Congress in Honolulu in 1920, Clark Wissler, the curator of anthropology at the American Museum of Natural History, wrote to his superior Osborn reporting that Sullivan's research would "put us in the lead in the racial problems of the Pacific. In two years Sullivan will be the authority on the subject."[17] A notorious schemer, Wissler believed the investigation would give them "the entire Polynesian field so that in the future this Museum must be looked to as a center for this subject."[18] Sullivan, a "quiet fellow, sane, clear-eyed, persistent, not afraid of work," was completing his PhD at Columbia studying the morphology of Sioux Indians (Sullivan 1920) with Boas as his advisor.[19] Appointed to the American Museum in 1916 to develop an exhibition on human classification, the young physical anthropologist had complained he could find few pertinent materials in the institution's vast collection. He bonded closely with Osborn, who shared his concerns, having little patience for Wissler's concentration on social anthropology. When Grant and Davenport commended the plan for a Hawaiian survey, Osborn sought immediately to assign Sullivan to the task. The American Museum would continue to pay the anthropologist's salary while the Bishop Museum covered his expenses.[20]

Sullivan dedicated himself to determining the racial origin of Polynesians, though he soon realized the population comprised a complex and frustrating assortment of characters. "I'm trying to work out a method for isolating race types in a badly mixed population" he wrote to Herbert Gregory, the Yale geologist who directed the Bishop Museum.[21] "The Polynesian physical problem is much more complex than had previously been thought," Sullivan reported. "At present there is undoubted evidence for the existence of four different types in Polynesia."[22] He cataloged 240 islander crania in the Bishop Museum and exhumed skulls from old burial grounds. In the Kamehameha School, another Bishop endowment, he measured "pure" and part-Hawaiian children and arranged for a nurse to take some blood for grouping. At Kona, on the big island, the anthropologist received assistance from an agent of the Bishop estate, an expert at "handling the natives." He measured the plantation workers and took hair clippings. "The people were very good about it, and the leaders gave us their most hearty cooperation." At first he took photographs of clothed research subjects, but Osborn demanded they pose naked for purposes of exhibition in his future Polynesian Hall.[23] "I'm head over heels in the Polynesian problem" Sullivan wrote to a superior at the American Museum. "The ultimate solution of race relationships must depend on the results of skeletal studies." Yet there he faced stiff competition. "Hrdlička gets everything that isn't nailed down. He surely is a hog of collectors."[24] Even so, within a few years Sullivan measured almost 11,000 Hawaiians and made records of more than 300 skulls.[25]

The museum required Sullivan to define and represent "pure" Polynesians, but he could not resist becoming engrossed in identifying mixed-race subjects, thereby returning unavoidably, if somewhat captiously, to the Boasian program. Wissler stolidly accepted the hybrid truth of Oceania. "The mixed data," he assured Osborn, "we can use as a separate exhibit to form the nucleus of an exhibit for race mixture, to be given a place in the general racial exhibit."[26] For the Second International Congress of Eugenics in New York in 1921, the American Museum, its host, specially "prepared a rather elaborate exhibit of racial types in Hawaii," including *haole* children from Punahou School, a full-figure statue of Duke Kahanamoku, and more than fifty busts and hundreds

16. Merriam to A. P. Judd, 17 May 1922, Bishop Museum folder, box 3, Correspondence Files, Carnegie Institution of Washington, DC.

17. Wissler to Osborn, 25 July 1920, folder July–September 1910, box 10, Central Archives, American Museum of Natural History, New York. Wissler met Duke Kahanamoku, who taught him to surf at Waikiki—he claimed as a pupil to be superior to Osborn. Robert E. Park, Alfred Kroeber, and Tozzer also attended the congress, held at Punahou School, but sadly there is no record of them surfing.

18. Wissler to Osborn, 21 September 1920, folder July–September 1920, box 10, Central Archives, American Museum of Natural History, New York. A midwesterner, Wissler graduated from Columbia with a PhD in psychology and became Boas's assistant at the American Museum of Natural History before succeeding him in 1905. On Wissler, see Freed and Freed (1983). Tozzer told Boas, "I have never had any time for Wissler. . . . I have nicknamed him Contact Wissler" (5 December 1925, Boas papers). Tozzer thought Wissler an intriguer and found his work "slovenly and ineffective" (13 April 1926, Boas papers).

19. H. E. Walter to Wissler, 5 April 1916, folder March–June 1916, box 9, Central Archives, American Museum of Natural History, New York.

20. Confirmed in the Memorandum of Conference on Polynesian Research in Cooperation with the Bishop Museum, 6 October 1920, folder July–September 1920, box 10, Central Archives, American Museum of Natural History, New York.

21. Sullivan to Gregory, 7 December 1924, Louis R. Sullivan papers, MS SC, Bishop Museum Archives, Honolulu (hereafter cited as Sullivan papers).

22. Report of Louis R. Sullivan for 1923, Sullivan papers. See Sullivan (1923, 1924).

23. Sullivan to Osborn, 14 June 1920, folder April–June 1920, box 10, Central Archives, American Museum of Natural History, New York.

24. Sullivan to F. A. Lucas, 8 July 1922, folder July–December 1922, box 11, Central Archives, American Museum of Natural History, New York.

25. Tozzer told Boas, "I have measured almost 500 natives but Sullivan's work is so much more impressive that I hesitate to publish. I like Sullivan very much and we get on well together" (4 October 1920, Boas papers).

26. Wissler to Osborn, 25 July 1920, folder July–September 1920, box 10, Central Archives, American Museum of Natural History, New York.

of photographs, mostly of mixed types.[27] The racial diversity of the islands and its implications for race formation excited much discussion at the congress.

Sullivan's estimate of those with mixed parentage was ambivalent, especially when corresponding with Davenport. "I have been forced to conclude that from the standpoint of the Polynesian race mixture has been successful, particularly when the other element is Chinese," Sullivan wrote; "From the standpoint of the Whites or Chinese it is a failure of course. The resulting offspring stands out half way between the parental stocks on average."[28] To his advisor Boas, Sullivan wrote explaining that although "this is a wonderful experimental laboratory, the data is of much less value on account of the very poor pedigrees." European standards of morality were observed only superficially, so the investigator often had to guess ancestry. Thus the result of the great racial experiment remained elusive.

Pulled between Davenport and Boas, Sullivan's health began to suffer. Osborn insisted the young anthropologist recuperate from his severe chest infections in the dry climate of Arizona. "It is a real calamity to anthropology that he has again broken down in health" Osborn wrote to his counterpart at the Bishop Museum. "He has a pleuritic infection which we hoped his Hawaiian residence would benefit. He is a most brilliant and lovable man."[29] In the desert, Sullivan soon reported a gain in weight and confidence.[30] "I believe I have learned how to take care of myself," he wrote.[31] Yet tuberculosis continued to stalk him. In 1925, he died, aged 32, taking his last breath of the dry air of Tucson.

Sullivan's illness looked as though it might thwart Wissler's plans for Oceanic hegemony. In 1922, Wissler reported to Osborn that Merriam had decided to direct some Carnegie resources to a "group of researches on the innate equipment of man and the biological basis for his social behavior"; therefore, the American Museum "should emphasize all aspects of the race mixture problem, instead of those few which lend themselves to . . . exhibition."[32] Otherwise, Davenport would as usual snare all the money. But with Sullivan increasingly debilitated and his studies of Pacific miscegenation stalled, the museum lacked the necessary research capacity. Indeed, Wissler, normally so equable and unflustered, took to wondering "whether we will proceed on the old plan or leave the Pacific entirely to some other institution."[33] Meanwhile, wily Davenport proposed an institute of "human biology," which would focus on the anatomical and physiological investigation of Aboriginal Australians, "the most primitive living race of mankind."[34] But Merriam was unimpressed, especially when he realized the enterprising scientist had also been courting the rival Rockefeller Foundation. "What we need first," Merriam wrote dismissively, "is a man with vision and ability to lead"—not Davenport, who simply wanted support to continue his current activities.[35] Accordingly, Wissler understood that he must quickly find a replacement for Sullivan, someone who could convincingly represent studies of Pacific miscegenation as the epitome of "human biology," the latest fashion.

It soon became clear that Davenport had miscalculated in appealing in a blatantly opportunistic manner to the commitment of Edwin R. Embree, director of the Division of Studies at the Rockefeller Foundation, to "developing a concentrated approach to what may be described as Human Biology."[36] "Contact" Wissler would be more adroit in his dealings with Embree. Witty and urbane, Embree had grown up in Kentucky, proud of his abolitionist and Quaker ancestry, learning to live "with delicate balance on the disharmonic margins of many cultures" (Johnson 1946:334).[37] In the 1920s, he sought to reinvent physical anthropology, to give it broader biological form, emphasizing genetics, studies of growth and sexuality, psychology and mental hygiene, experimental evolution, anthropometry of immigrants, and race mixing in the Pacific (Cowdry 1930; Embree 1930). The boundaries of the new field remained vague and elastic, but they excluded the formal typological study of race and meretricious or obtuse application of Mendelian principles—the sort of work for which Davenport was well known.[38] Interested in the potential of Hawai'i as the laboratory for human biology, Embree attended the 1925 Institute of Pacific Relations conference in Honolulu and later traveled around Oceania with Wissler and Princeton biologist Edwin G. Conklin looking for additional

27. Annual Report of Louis R. Sullivan 1921, Sullivan papers.

28. Sullivan to Davenport, 15 August 1922, Davenport papers.

29. Osborn to A. P. Judd, 16 March 1922, folder 1922–1924, box 16, Central Archives, American Museum of Natural History, New York.

30. Sullivan to Osborn, 21 May 1922, folder May–June 1922, box 11, Central Archives, American Museum of Natural History, New York.

31. Sullivan to F. A. Lucas, 4 July 1923, folder March–December 1923, box 11, Central Archives, American Museum of Natural History, New York.

32. Wissler to Osborn, folder May–June 1922, box 11, Central Archives, American Museum of Natural History, New York.

33. Wissler to George H. Sherwood, 1 February 1927, folder 1927, series 1216, box 606, Central Archives, American Museum of Natural History, New York.

34. Davenport to Edwin R. Embree, 3 March 1924, Davenport papers.

35. Merriam, written on the margins of W. M. Gilbert, Memorandum Concerning Dr. Davenport's Proposal, 15 January 1926, folder 2, box 3, subseries 3, Department of Genetics, series 2, Administration Archives, Carnegie Institution of Washington, DC.

36. Embree to Arthur L. Dean, president of the University of Hawaii, 28 May 1926, folder University of Hawaii, box 1, series 214, record group 1.1, Rockefeller Archives Center, Tarrytown, New York.

37. A Yale graduate, Embree had edited the alumni magazine before becoming secretary at the foundation. From 1924 to 1928 he directed the Division of Studies at the Rockefeller Foundation before moving to Chicago as president of the Julius Rosenwald Foundation, which he used to support interracial educational programs in the South. See Embree (1931).

38. For Embree's polite evasion of Davenport's plans for an institute of human biology at Cold Spring Harbor, see Embree to Davenport, 27 April 1925, Davenport papers.

research opportunities.[39] Embree so liked the islands he based his family on Oahu for a year, sent his children to Punahou School, and even imagined staying. In the report of their Pacific survey, Conklin declared "there is probably no place in the world where problems of human races, hybrids, heredity and evolution could be carried on as successfully as in the Hawaiian Islands. . . . Extensive studies should be undertaken on the physical, intellectual and social traits of the human races and hybrids represented in the islands." To start, Hawai'i needed a man trained in anthropology and genetics to replace Sullivan.[40] Embree wanted an investigator from an ethnic minority—excluding Davenport—because "the best of us tend in any such study to act on a number of preconceptions on the basis of our racial inheritance and our racial education."[41] Wissler knew just the man.

The Boasian Island Platform

At the beginning of 1927, Herbert Gregory wrote from his office at the Bishop Museum to Wissler in New York. Sullivan's death had interrupted the association with the American Museum, but Gregory hoped that relations might soon be restored. "Any change," he wrote, "would bring a dark cloud over the skies of Kalihi Valley. . . . I hope you will continue to assume responsibility or at least oversight of the Pacific work in physical anthropology." Colleagues were praising young Harry Shapiro, Sullivan's successor at the American Museum. Hooton's first PhD student at Harvard and one of the pioneering Jewish students in the graduate school, Shapiro had examined the hybrid descendants of the *Bounty* mutineers on Norfolk Island (Shapiro 1929). In New York, though, he was passing the time since his appointment in 1926 studying old skulls. His longing to return to the South Seas was frequently bruited abroad. "How would it do to turn over to Shapiro all the stuff on hand," Gregory asked disingenuously, "then after he has prepared such papers as the data make advisable, send him to the Pacific, say in 1928."[42] Osborn and others agreed this was an excellent suggestion. Gregory was delighted that the American Museum was again prepared to "assume the strong man's share of the work in Polynesian anthropology."[43]

From 1929 through the early 1930s, Shapiro busied himself at the Bishop Museum, establishing an additional and ultimately more lucrative association with the University of Hawai'i, which organized the local studies with Rockefeller funds.[44] His principal task was supervision of biological research into race mixture in the Hawaiian Islands, but he was often allowed to travel in the South Pacific with fellow anthropologists Kenneth Emory and Peter Buck (Te Rangi Hiroa) and the linguist Frank Stimson. On his South Seas adventures, doing Bishop Museum work, Shapiro happily set about measuring Polynesians, especially those whose ancestry was mixed, whereas in Hawai'i his assistants collected most of the records. During his southern excursions, Shapiro became unusually intimate and sentimentally engaged with the islanders, sensing a close relation to them, while in Hawai'i his disconnection from fieldwork and institutional insulation allowed him more detachment, leading even to alienation. Casting himself as the virile explorer of the South Seas, Shapiro was especially pleased to visit the descendants of the *Bounty* mutineers on Pitcairn Island in 1934–1935 aboard Templeton Crocker's yacht *Zaca*. Later he extolled the hybrid and inbred group in his popular book, *The Heritage of the* Bounty (1936; see also Anderson, forthcoming).

In Hawai'i, Shapiro concentrated initially on documenting Chinese-Hawaiian crosses, a group that seemed sufficiently recent to provide reliable genealogies yet numerous enough to make adequate sampling possible. In 1930, he recruited William Lessa, an eager Italian American undergraduate student from New Jersey studying at Harvard, to do most of the fieldwork. Lessa, according to Hooton, was a "very ardent anthropologist," though not "very brainy."[45] Once in the islands, he measured as many of the inhabitants with mixed Chinese and Hawaiian ancestry as he could find, and within

39. An embryologist, Conklin emphasized the importance of development in achieving hereditary potential and questioned the extreme hereditarianism that Davenport promoted. All the same, he was a eugenicist who advocated segregation and sterilization of the unfit. On Conklin, see Cooke (2002) and Reumann and Fausto-Sterling (2001).

40. Conklin, Report on Possibilities and Needs of Research in Biology in the Pacific (ca. 1926), folder Bishop Museum, box 1, series 214, record group 1.1, Rockefeller Archives Center, Tarrytown, New York. Wissler endorsed the report. Embree had already found a start-up grant for the American Museum, channeled through the National Research Council. Tozzer predictably was scathing: "Wissler is a perfect stinker. He . . . received an appropriation of $7000 for the study of race mixture, had his son appointed a special assistant and left everyone else out in the cold. There is some actually crooked business associated with it" (4 May 1924, Boas papers). Tozzer and Boas were mollified when the money eventually was spent on Harry Shapiro.

41. Embree to A. L. Dean, 6 October 1926, folder University of Hawaii, box 1, series 214, record group 1.1, RF, Rockefeller Archives Center, Tarrytown, New York.

42. Gregory to Wissler, 19 January 1927, folder 1927, series 1216, box 606, Central Archives, American Museum of Natural History, New York.

43. Gregory to Wissler, 20 November 1928, folder 1928, series 1216, box 606, Central Archives, American Museum of Natural History, New York. Wissler regarded this as "a good opportunity to broaden and intensify our work in the Pacific" (Wissler to George H. Sherwood, 24 February 1930, folder 1930, series 1216, box 606, Central Archives, American Museum of Natural History, New York). The idea was that Margaret Mead, another recent appointment at the American Museum of Natural History, would work with Shapiro, but she had different plans.

44. Embree had decided that the University of Hawai'i would focus on Hawai'i, the University of Sydney on Australasia, and the Bishop Museum on the rest of the Pacific—"with a view to developing a concentrated approach to what may be described as Human Biology" (Embree to A. L. Dean, 28 May 1926, folder 2, box 1, series 214S, RF, Rockefeller Archives Center, Tarrytown, New York).

45. Hooton to Shapiro, Thursday [October 1928], folder 1928, box 3, H. L. Shapiro papers, MSS S537, Archives and Special Collections, American Museum of Natural History (hereafter cited as Shapiro papers).

months he boasted of a series of more than 1,000 individuals, the majority children. Yet still he felt the "Chinese-Hawaiian group is in a sad state. These people do not work together as a class, so that their occupations scatter them in various walks of life making it impossible to get them in bunches."[46] Recording each subject had taken hours of meticulous measurement. Lessa also obtained blood when he could and sent it to Karl Landsteiner at the Rockefeller Institute in New York for grouping. "I am disappointed at the scarcity of pure Hawaiians and of Chinese-Hawaiians without Haole blood," he wrote to Shapiro; "Many of those who claim to be pure Hawaiians are quite apparently mixed."[47] Shapiro breezily tried to reassure him: "Don't be discouraged about the Haole fly in the Hawaiian ointment," he wrote from New York.[48] But the following month Lessa was still worrying about pedigrees. "They lie right to your face," he complained, "and it is only by roundabout ways that I do discover other mixtures. The less I say about the Hawaiians the better."[49] But by then the young American, scarcely 22, was sharing a house in Waikiki and signing off with "aloha."

Lessa depended on Margaret Lam, a Chinese graduate student born in Hawai'i, to recruit research subjects and to elicit their sociological profile. Anxious and fastidious, she delicately compiled life stories for each adult Lessa measured. But she found the neophyte physical anthropologist pushy and intrusive. "I am not getting any more people for Mr. Lessa to measure," she told Shapiro. "Lessa and I have been working quite faithfully and *outwardly* he is wonderful."[50] But they functioned independently—"too independently I should say. Lessa has not helped me a bit," Lam complained.[51] She also fretted that sociologists from the University of Hawai'i were duplicating her studies as they expanded their own research into race mixing. "She is a nervous little body and worries a great deal," the president of the University of Hawai'i wrote to Shapiro, "but in spite of it I think is doing some good work."[52]

"In the process of organizing the problem," Shapiro reflected late in 1930, "it became apparent that whereas the Hawaiians were a population long established and adjusted to local environmental conditions, the Chinese on the contrary were newcomers to a milieu differing radically from their former environment." He was thinking along Boasian lines. Since moving to New York and with an introduction from Tozzer, Shapiro had become a close friend and protégé of Boas. Inspired by his new mentor's earlier controversial studies of changes in the head form of immigrants to the United States, Shapiro decided to supplement his race-mixing investigations with examinations of the bodies of Chinese and Japanese residents in Asia and comparing them with those born in Hawai'i. People living in Canton and Hiroshima, the areas from which the immigrants to Hawai'i had sprung, would represent control groups.[53] "The whole subject of the modifiability of the human organism becomes more fundamental the longer I study it," Shapiro told the University of Hawai'i president.[54] He arranged to send Lessa to China and to recruit another Hooton student, Frederick Hulse, to assay the stability of the Japanese type. The shift in the research agenda additionally had the advantage of extricating him from the hopeless confusion of his Hawaiian race-mixing data, the 6,000 records of dubious ancestry. All of these data had been sent to the women in the Peabody Museum calculating office, where they checked indexes, punched cards, and fed the cards into Hollerith machines—a long arduous labor now of questionable value. Shapiro continued to assert that his biological analysis of race mixing in Hawai'i would provide the answer to the social problem of miscegenation in the continental United States, but he began to doubt whether the work was now capable of satisfactory completion.[55]

Lessa showed little enthusiasm for any move to China. "I think it would be advisable for me to make as thorough a cleanup of Hawaii as I can," he suggested to Shapiro.[56] But his supervisor was adamant. From the start, Lessa found China frustrating, as he expected; he struggled with the language and found the inhabitants obstinate and crotchety. Sometimes his requests incurred abuse; on other occasions Cantonese villagers "acted nice, but [followed a] policy of non-cooperation." Their loud talk, constant spitting, and habit of staring irritated him. "The country people are a most unattractive and dull lot," he recorded in his journal. "They have wretched figures and no gentleness at all." Rumors of bandits and revolutionaries abounded.[57] "Getting blood from people isn't such an easy job," Lessa reported. "Some people have a horror of the very word 'blood' and I was lucky to

46. Lessa to Shapiro, 4 August 1931, folder William Lessa, box 2, Shapiro papers.

47. Lessa to Shapiro, 14 October 1930, folder 1930, box 3, Shapiro papers.

48. Shapiro to Lessa, 28 October 1930, folder 1930, box 3, Shapiro papers.

49. Lessa to Shapiro, 11 November 1930, folder 1930, box 3, Shapiro papers.

50. Lam to Shapiro, 4 December 1930, folder 1930, box 3, Shapiro papers; emphasis in original.

51. Lam to Shapiro, 11 March 1931, folder Margaret Lam, box 2, Shapiro papers.

52. D. L. Crawford to Shapiro, 8 January 1932, folder Bishop Museum, box 1, Shapiro papers.

53. Shapiro, Summary of the Anthropological Studies Conducted on Behalf of the University of Hawaii, folder 1933, box 4, Shapiro papers. Boas's study had lacked any control group.

54. Shapiro to D. L. Crawford, 19 September 1933, folder 1933, box 4, Shapiro papers.

55. Shapiro, Summary of Anthropological Studies, folder 1930A, box 3, Shapiro papers.

56. Lessa to Shapiro, 11 January 1932, folder William Lessa, box 2, Shapiro papers.

57. William A. Lessa, Journal, 1 December 1932 and 4 January 1933, folder Hawaiian-Chinese research correspondence, box 3, William A. Lessa papers, National Anthropological Archives, Smithsonian Institution, Suitland, Maryland (hereafter cited as Lessa papers).

get as many samples as I have."[58] The experience put Lessa off physical anthropology, and China, for life. "About the only thing a Chinese respects is a fast-moving bus," he mused. "How much does a good bus cost?"[59] Shapiro regretted he could never find much use for Lessa's data.[60]

Shapiro's other assistant, Fred Hulse, proved a more successful member of the "Peabody club" of Hawaiʻi. The errant son of an Episcopalian missionary, Hulse was writing a dissertation with Hooton that compared the bodily forms of Andalusians and Cubans, emphasizing the selection pressures of migration and environmental change. Hooton observed that his graduate student was "excessively timid and retiring and has found it necessary to compensate by adopting certain leftist points of view in social and political matters and by adjusting his appearance to his ideal of a proletarian"—yet he still recommended him as a "modest and a thoroughly sound and honest young man."[61] Hulse's Hawaiian assignment required him to assess the physiques of Japanese immigrants to Hawaiʻi and compare them with those of their relatives back in Japan. It was an arduous process because each subject needed 43 measurements and 41 observations. Eventually he made records on more than 2,500 individuals, those in Hawaiʻi constituting 2.5% of the Japanese population there. Hulse found most of his "victims" in the public schools and at the University of Hawaiʻi. "In general the fellows here have been pretty good and docile," he assured Shapiro. But many of his subjects refused to remove their shoes, and the "examination of the mouth has at times not been easy."[62] Several resisted his attempts to photograph them, and they "don't like me to measure their noses either."[63] In Hiroshima, however, he found the people far more cooperative, though rather dour. His work there seemed almost mechanical.

Hulse enjoyed life on Oahu during the economic depression. He quickly learned "the art of surf-boarding" and spent most of his spare time catching waves off Waikiki.[64] "Surfing looked too difficult for me but a grand sport," Shapiro wrote enviously from wintry New York. "You must be as brown as a beach boy and getting burly."[65] But not everyone approved of surfing and hanging out at beach bars. Hulse's relations with his secretary, Jitsuichi Masuoka, soon became strained. "Masuoka is, unfortunately, a convert, and neither drinks, smokes nor swears," Hulse told Shapiro. "That doesn't make life too easy for a sinner like myself."[66] But he charmed Margaret Lam. "He's a peach and an ideal person to work with," she wrote to Shapiro.[67] "He's just marvelous—a very pleasing personality and very likable and cooperative." She found Hulse a "marked contrast" to the more saturnine Lessa.[68] "It was awfully hard to see Fred leave as he and I got along so beautifully. . . . I'm about shot to pieces now."[69] But when the Rockefeller funds dried up in 1934, the Peabody boys dispersed. "I intend to dust off the calipers again whenever the opportunity offers, and oil up their joints," Hulse assured Shapiro. "I still think our brand of science is the most fun."[70] But neither Masuoka nor Lam ever found lasting joy in anthropometry. Both of them were drawn into the orbit of the Chicago sociologists and henceforth tried to avoid gung ho *haole* biologists.[71]

As the results came in, Shapiro was elated. "I am terribly excited about the stuff," he told Connie Tyler, the data analyst at Harvard. "It has jelled perfectly. . . . If Hootie has seen the sheets he will understand my frenzy."[72] Three thousand Japanese had "submitted to a long, tedious schedule of measurements" to assess the effects of a change in environment on the physical features of a migrating population (Shapiro 1939:v). Shapiro compared those who remained in Japan with migrants to Hawaiʻi and their Hawaiian-born racially un-

58. Lessa to Shapiro, 30 June 1932, folder William Lessa, box 2, Shapiro papers.

59. Lessa, Journal, 4 December 1932, folder Hawaiian-Chinese research correspondence, box 3, Lessa papers. Lessa started graduate studies in cultural anthropology at the University of Chicago just before World War II and could not complete his dissertation until 1947. He taught cultural anthropology at the University of California, Los Angeles (UCLA), and conducted fieldwork on Ulithi atoll (Yap) in the Pacific, focusing on folklore and religion. At UCLA he discovered a precocious undergraduate, Carlos Castaneda.

60. Shapiro (1931) wrote a prospectus for the Chinese-Hawaiian study but never completed analysis of the results.

61. Hooton, Recommendation of Hulse, 1937, folder Hulse, box 13, E. A. Hooton papers, 995-1, Peabody Museum Archives, Harvard University, Cambridge, Massachusetts (hereafter cited as Hooton papers).

62. Hulse to Shapiro, 29 January 1932, folder Fred Hulse, box 2, Shapiro papers.

63. Hulse to Shapiro, 17 May [1931], folder Fred Hulse, box 2, Shapiro papers.

64. Hulse to Shapiro, 19 February 1931, folder Fred Hulse, box 2, Shapiro papers.

65. Shapiro to Hulse, 10 February 1931, folder Fred Hulse, box 2, Shapiro papers.

66. Hulse to Shapiro, 9 June [1931], folder Fred Hulse, box 2, Shapiro papers.

67. Lam to Shapiro, 11 March 1931, folder Margaret Lam, box 2, Shapiro papers.

68. Lam to Shapiro, 14 January 1931, folder Margaret Lam, box 2, Shapiro papers.

69. Lam to Shapiro, 4 August 1931, folder Margaret Lam, box 2, Shapiro papers. Lam was referring to Hulse's departure for Japan.

70. Hulse to Shapiro, 5 October 1935, folder July–December 1935, box 4, Shapiro papers. Hulse became an archeologist digging in Georgia for the Works Progress Administration and then served during the war in the Office of Strategic Services advising on Japan. He taught physical anthropology at the University of Washington, Seattle, and the University of Arizona, where he studied the genetics of Native American groups, emphasizing cultural and environmental effects. He edited the *American Journal of Physical Anthropology* (1964–1969) and was elected president of the American Association of Physical Anthropologists (1967–1969). See Hulse (1971, 1981).

71. Lam spent a semester at the University of Chicago with Robert Redfield and completed her MA in sociology at the University of Hawaiʻi (Lam 1933, 1935). Masuoka undertook graduate study with Park at the University of Chicago and became professor of sociology at Fisk University (Masuoka and Valien 1961). Henry Yu (2001) mentions both.

72. Shapiro to Constance Tyler, 6 July 1932, folder Hawaiian study, box 1, Shapiro collection, Division of Anthropology Archives, American Museum of Natural History, New York.

mixed descendants. He determined that Japanese migrants represented a physically distinct group within the larger domestic population and that the first generation born in Hawai'i was different again from their parents and those who remained behind. Hawaiian-born Japanese showed changes in 80% of measurements and 90% of indexes even after age and occupation were taken into account. On average, they became taller, and the height and width of their heads increased. "From the evidence of the Japanese in Hawaii," Shapiro wrote, "man emerges as a dynamic organism which under certain circumstances is capable of very substantial changes within a single generation" (Shapiro 1939:198–199). Yet the "vast bulk of comparative racial studies rests on [the] tacit belief in the stability of the statistical balance of the component variables of a population" (Shapiro 1939:184). Of course, Shapiro was not claiming that Japanese in the islands would eventually turn into Native Hawaiians—change took place only "in accordance with the fundamental structure of the organism and only to a limited degree" (Shapiro 1939: 202). Still, his Pacific research demonstrated the "dynamic and plastic character of the human organism" (Shapiro 1939: 199).[73]

Bombing the Sewing Circle?

"In physical anthropology," Shapiro lectured the Anthropology Club of Philadelphia in 1935, "it has become increasingly discernable that new currents of investigation are finding their way into the stagnant pools of inherited beliefs and are washing away the obscuring scum of a century." He condemned the older generation's obsession with racial typologies, lamenting that, "like the heads of the Hydra monster, such classifications appear to have a limitless capacity of regeneration." Rather, he wished to postulate the instability of human form. In so doing, of course, he was elaborating on Boas's earlier description of changes in the physique of immigrants, an argument that was "like a bomb dropped upon a genteel afternoon meeting of a sewing circle."[74] Shapiro's disavowal of the pedantry and muddle of racial classification may have been based on Boas's arguments, but it had gained force and conviction through his own Pacific research on human hybridity and the processes of environmental modification. In the early 1930s, his Hawaiian studies, along with close encounters in the South Seas, drove him to criticize conventional racial typologies and hierarchies and to develop instead a dynamic and historically conditioned understanding of human variation. He retained a nominalist attachment to the idea of race, but for him this represented a flexible zoological category, a biological cluster of convenience lacking sharp objective boundaries and devoid of psychological or moral value. On his return from the Pacific in 1935, he observed with particular disgust the yoking of racial science to German policies of extermination, which incited him to antiracist activism. He and Boas thought about organizing a statement from physical anthropologists condemning Nazi racial excesses, but they realized that as Jewish intellectuals they would be dismissed as interested parties. Shapiro therefore approached Hooton, who, somewhat to his surprise, also was eager to discredit scientific racism. Although Hrdlička and Tozzer supported Hooton's antiracist draft, most American physical anthropologists quibbled or resisted outright. Eventually, Hooton (1936) published his statement alone (see Barkan 1988).

In later years, Shapiro became more publicly active in combating scientific racism, though he never completely discarded the idea of racial differentiation, rebuffing the postwar skepticism of Ashley Montagu and other critics (Barkan 1996). In 1944, Shapiro lamented that race had become unnecessarily "encrusted with psychological attributes and assignments of value." He continued, "We all know how this monstrous doctrine has been elevated into a credo, how it has been used to inflame and manipulate masses of men." Yet as an anthropologist he understood that "it is practically impossible to set arbitrary lines of division between one type and another"; the anthropologist "deliberately undertakes to study man as a biological phenomenon like any other organism" (Shapiro 1944:376). Therefore, he attempts to determine how human differences derive from "environmental adaptations, historical accidents, local developments or simply superficial physical mutations of no intrinsic value" (Shapiro 1944:375). There should be no consort of human biology with common racism. After the war, Shapiro enthusiastically joined efforts to combat scientific racism through UNESCO. His commissioned pamphlet on race mixture argued that miscegenation presented no biological problem—indeed, the production of such racial mosaics might generate the human variation necessary to advance civilization (Shapiro 1954). But in broader discussions at UNESCO of the biological validity of racial classification, Shapiro tried to temper those such as Montagu who sought to nullify altogether the concept of race (Barkan 1992; Brattain 2007; Reardon 2005).[75]

Even in the 1930s, as racial classification seemed ever more meretricious, it was getting hard to imagine race mixing as a compelling biological problem—as a phenomenon that physical anthropologists might sensibly analyze. Sociological investigation came to appear far more pertinent. Lam and Masuoka recognized at an early stage in their careers the limitations of biological studies, choosing to reframe miscegenation as a social problem or cultural issue. They jilted the

73. Hooton disputed this conclusion, arguing that "the sons continued and emphasized the modern evolutionary trends of their parents" and would have shown the same characteristics "whether they were born and bred in Hawaii or anywhere else" (Report on H. L. Shapiro's *Migration and Environment* [1939], folder Shapiro, box 23, Hooton papers).

74. Shapiro, Anthropology Club, Philadelphia, 1935, box 38, Shapiro papers.

75. After the war, Shapiro tended to focus on museum work and public outreach. In 1948, he served a term as president of the American Anthropological Association.

physical anthropologists in favor of sociologists at the universities of Hawai'i and Chicago. At the University of Chicago, sociologist Robert E. Park was arguing for observation on the margins of advancing populations, especially in remarkably heterogeneous sites such as Hawai'i, which he visited repeatedly in the 1920s and 1930s. "Hybrid peoples," Park claimed, demanded social study because they offered "tangible evidence of the extent and character of European cultural contact" and revealed the character of race relations (Park 1934:78). Park and his students now wanted to chart the "miscegenation map of the world," tracing the rise of these "marginal men," who like the Jews before them were creating a "new and cosmopolitan civilization" (Park 1934:84, 1950 [1926]; see also Bulmer 1984; Furedi 2001; Rosa 2001). One of his associates, Edwin B. Reuter, wintered at the University of Hawai'i, advising on sociological studies of race mixing in Hawai'i (Reuter 1918, 1931). The leading social analyst in the islands, Romanzo C. Adams, had studied at the University of Chicago—along with all of his departmental colleagues, making their part of the Manoa Valley an island resort of Chicago's Hyde Park. With Chicago support, Adams treated Hawai'i as a "research laboratory in race relations" and wrote a laudatory account of the social dynamics of marriage between immigrant groups and between outsiders and Native Hawaiians, or Kanaka Maoli (Lind 1935:47; see also Adams 1926, 1934, 1937; Gulick 1937; Smith 1934; Stonequist 1935). Toward the end of his career, even Shapiro (1966:21) conceded that miscegenation was an area that primarily fell "within the scope of cultural anthropology." He urged scholars to investigate "the decisive part that cultural and other conditions play in fostering or inhibiting the process of race mixture" (Shapiro 1966:26). To understand race mixing, one must understand the "milieu." Thus, the study of human hybridity had so disturbed and transformed physical anthropology that it eventually extinguished its conditions of biological credibility—it had became biologically meaningless and indefensible, increasingly displaced into the critical study of social problems and cultural perceptions.

Despite the sense of scientific comity, this valorization of hybrid peoples between the wars in Hawai'i and the continental United States often ran counter to prevailing currents of opinion. Efforts to stabilize and sanction mixed-race identities—perhaps to Hawaiianize racial thought—proved ineffective on the mainland and seemed at best unwanted in Hawai'i. Although hybridity still could be recognized and validated—sometimes even extolled—in Hawai'i, the gradual Americanization of popular racial categories and territorial policy was challenging mixed identifications during this period, re-sorting and simplifying group affiliations. From the early 1920s, the *haole*-dominated territorial government insisted that people needed at least 50% Kanaka Maoli ancestry to be defined as Native Hawaiian, disentitling those with less. Contrary to traditional means of assessing relatedness, the notion of a "blood quantum" constructed "Hawaiian identity as measurable and dilutable," not as complex, partial, mixed, and historical. The "blood quantum" was an exclusory racial technology imported from colonial management practices in American Indian reservations (Halaulani 2002; Jung 2006; Kauanui 2008).[76] At the same time, Japanese and Chinese officially were racialized as separate alien groups and as economic and political threats, just as on the West Coast of the United States. While scientists were praising human hybridity, enjoying their modernist biological moment, mainland typologies and classifications gained a foothold on the islands. By the 1970s, when Barack Obama was growing up in Honolulu, the tension between these contrasting racial evaluations would be keenly felt.[77]

Acknowledgments

I am grateful to Susan Lindee, Ricardo Ventura Santos, and other participants in the Wenner-Gren workshop for their comments and suggestions. Thanks also to Clare Corbould, Fae Dremock, Margaret Jolly, Ross Jones, Emma Kowal, Lisa O'Sullivan, and Charles Rosenberg for helping to frame this essay.

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76. There was a sharpening of divisions between *haole* and Kanaka Maoli after the Massie case in 1932 (Stannard 2005).

77. Obama (1995) rhetorically juxtaposes "the mixed blood, the divided soul, the ghostly image of the tragic mulatto trapped between two worlds" (xv) with the search for "a workable meaning for his life as a Black American" (xvi). Later, he remarks, "the legend was made of Hawaii as the one true melting pot, an experiment in racial harmony"—but he was then "too young to know I needed a race" (Obama 1995:24, 27).

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Humanizing Evolution

Anthropology, the Evolutionary Synthesis, and the Prehistory of Biological Anthropology, 1927–1962

by Vassiliki Betty Smocovitis

In this paper I explore the various attempts to integrate anthropology—and anthropologists—within the wider synthesis of evolution in the interval of time between 1927 and 1962 by tracking intersecting individuals and groupings at critical junctures such as conferences, commemorative events, and collaborative publications. I focus on the discipline as a unit of historical analysis and on a series of rhetorical arguments used to discipline and bound areas of study that grounded the secular philosophy of evolutionary humanism. I trace the beginnings of an originary narrative and offer a kind of prehistory of what was first referred to as "human evolution" and then "biological anthropology," an area of study that brought humans into the discipline of evolutionary biology. I examine the key roles played by "architects" of the evolutionary synthesis—such as Theodosius Dobzhansky, Julian Huxley, G. G. Simpson, and Ernst Mayr—and their relations with the anthropologists Sherwood Washburn, Ashley Montagu, and Sol Tax at pivotal meetings such as the Cold Spring Harbor meeting of 1950, the Darwin centennial at the University of Chicago in 1959, and a number of Wenner-Gren symposia culminating with the Burg Wartenstein symposium (no. 19) that saw the emergence of the new "molecular anthropology."

> *For nearly two centuries anthropology and biology have developed almost independently, although both have been profoundly influenced by such fundamental discoveries as Darwin's theory of evolution and his finding that man is a part of nature. In our century, the development of genetics, which studies the phenomena of heredity and variation, has caused a gradual drawing together of biological and anthropological research.* (Demerec 1950)

> *So we come to a science which proclaims itself the "study of man," yet views culture as though it were not part of man; which studies the evolutionary process and traces the origin of man through the fossil record, yet steadfastly separates man from all other animals; generally denies social and cultural evolution, yet uses the word "primitive"—apologetically—for most of the living peoples and cultures it studies.* (Sol Tax, "The Celebration: A Personal View")

We can understand why Darwin did not say much about human evolution in 1859: he knew little about it, and it was after all a delicate subject to raise in the context of a controversial theory.[1] But what are we to make of the fact that it also did not appear during the synthesis of Darwinian selection theory and the newer science of genetics nearly 100 years later? The "evolutionary synthesis," or the "modern synthesis," or "neo-Darwinism," whatever term we employ, was supposed to account for the origins and maintenance of biological diversity. It was supposed to bring to consensus a range of different and frequently conflicting perspectives, resolve a number of persistent problems in evolutionary theory, and provide a more secure footing for the new discipline of evolutionary biology. It was supposed to integrate a variety of disciplines informing evolution—from the newer genetics to the older systematics and paleontology to the even older and more amorphous discipline of botany—using a wide range of organisms, extant and extinct, from fruit flies to weeds, birds to mammals, and even a genetically engineered *Raphanobrassica* (a new species hybrid resulting from a radish crossed with a cabbage).[2] It was, in short, supposed to offer one coherent universalizing and unifying narrative of life's

Vassiliki Betty Smocovitis is Professor in the Department of Biology and the Department of History, University of Florida (P.O. Box 118525, Bartram Hall, University of Florida, Gainesville, Florida 32611, U.S.A. [bsmocovi@ufl.edu]). This paper was submitted 27 X 10, accepted 31 VIII 11, and electronically published 20 II 12.

1. He famously devoted only one sentence to humans in his *On the Origin of Species*, published in 1859. Darwin later revealed his thoughts on humans in 1871 in his *Descent of Man, and Selection in Relation to Sex*.

2. I am here summarizing a broad range of interpretations of a much contested "historical event." The literature is vast and draws on approaches from history, philosophy, and sociology of science. For a representative sample, see Provine (1971), Mayr and Provine (1980), Smocovitis (1992, 1996), and Gayon (1998). For nationalist histories of the synthesis, especially in Germany, see Junker and Engels (1999).

 0011-3204/2012/53S5-0010$10.00. DOI: 10.1086/662617

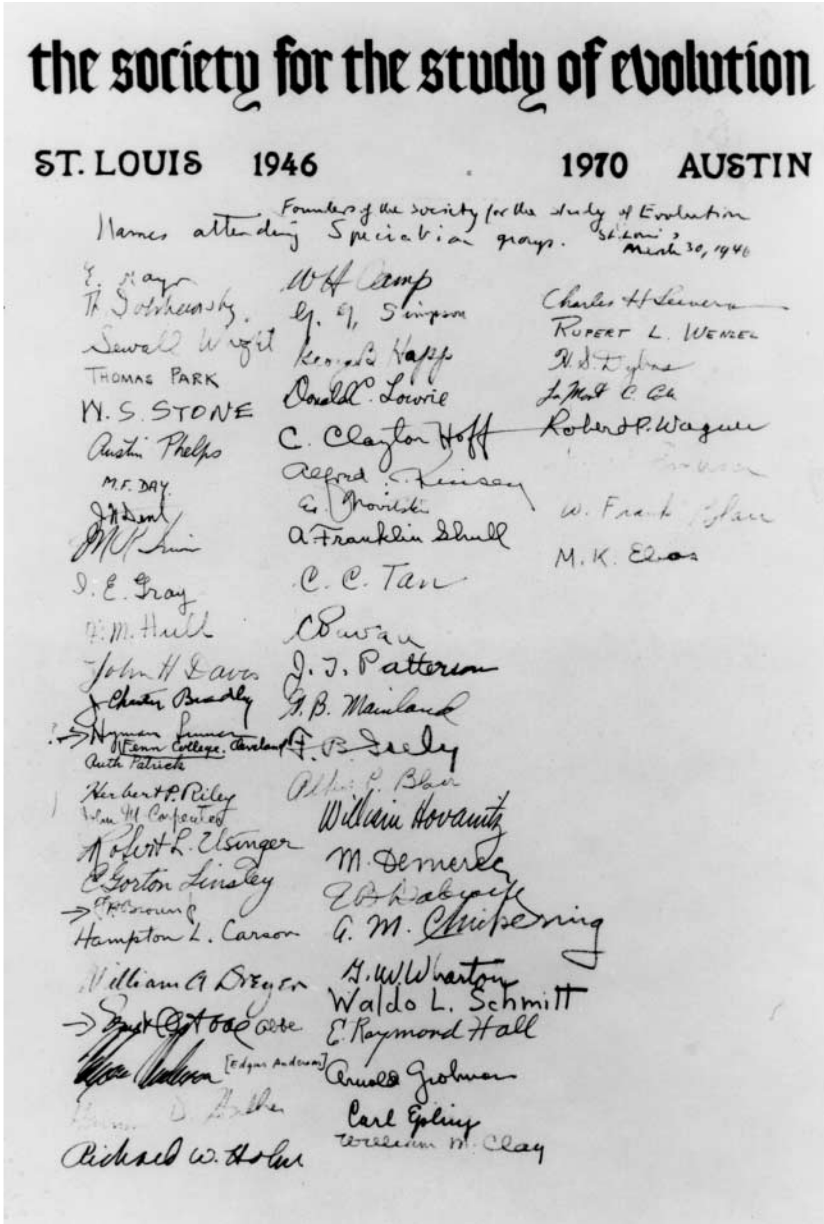

Figure 1. Signatories for the founding of the Society for the Study of Evolution, 1946. This copy (in author's possession) was reproduced for the 1970 meetings in Austin, Texas, and distributed to members. Courtesy of James Crow.

origins encompassing all life on earth and indeed elsewhere in the universe (Smocovitis 1992, 1996).

The fact was that anthropology, the discipline that dealt most immediately with human evolution, had been curiously removed from organizational and intellectual efforts to synthesize evolution in the 1930s and 1940s. No major texts associated with humans were part of the synthesis, no major journal articles, and no significant evolutionary insights associated with humans were part of the emerging consensus. There were no signatories representing human evolution in 1946, when a group of evolutionists came together to found a new international society, the Society for the Study of Evolution (SSE), and only two anthropologists out of 72 participants attended the famous meetings at Princeton University in 1947, at which time evolutionists celebrated the birth of what they termed the "synthetic types" of evolutionists (Muller 1949:421; figs. 1, 2).[3] By the late 1940s, the absence of anthropology—and anthropologists—at the new SSE meetings and the difficulty of securing suitable manuscripts for

3. It fell to J. B. S. Haldane to represent the topic of human evolution, its past and future, at the 1947 meetings (Haldane 1949).

Figure 2. Photograph of participants at the 1947 Society for the Study of Evolution meetings in Princeton. Photograph reproduced from the edited publication of the proceedings (see Jepsen, Simpson, and Mayr 1949). Identification of the all-male participants included therein.

issues of *Evolution* (the new international journal for evolutionary study) had been noted by key figures in organizing *Evolution*, such as Ernst Mayr. Though he tended toward the orthodox in his inclusionary criteria as the first journal editor, Mayr worked energetically at soliciting articles from anthropologists and at recruiting anthropologists to his new journal and society (Smocovitis 1994). Nonetheless, only one out of the first 22 articles was devoted to the subject in the first volume of the journal, and only three out of some 332 articles appeared in the first 10 years of the journal.[4]

The absence was especially noteworthy as anthropology logically had to be brought into agreement with the larger evolutionary synthesis. Humans were, after all, animals whose evolutionary history was encompassed by the new fusion of genetics with selection theory; and the architects of the evolutionary synthesis had already begun to envision it as part of the synthesis as early as the 1940s in their bid to promote the new unified science of evolutionary biology that unified biology and indeed all knowledge. The paleontological record of humans mattered, as did genetics, of course, but so too did the cultural components of human evolution, the understanding of the evolution of something called "mind," and a set of concerns traditionally associated with culture, such as behavior, all falling within the domain associated with the social sciences. All had to be incorporated, integrated, or synthesized with the modern synthesis of evolution as the architects were conceiving it, and indeed promoting it, to increasingly wider audiences in the 1940s. George Gaylord Simpson, Julian Huxley, Ernst Mayr, and especially Theodosius Dobzhansky were actively paving that way and calling for such a synthesis by addressing the evolution of man, mind, and culture in their semipopular and popular works (Smocovitis 1992, 1996).

From the architect's point of view, therefore, the science that encompassed human evolution and human culture, namely anthropology, had to be included in what was emerging as a new evolutionary cosmology. The ultimate goal, not unlike that of their nineteenth-century analogues accommodating Darwinism, was determining "man's place in nature." But the path toward that unified perspective was difficult because anthropology, for a number of complex historical reasons, was programmed to avoid rank reduction to biology (Armelagos 2008, 2011; Barkan 1992; Cravens 1978, 2010; Silverman 2005; Stepan 1982; Stocking 1968; 1988*a*).[5] In this paper, I wish to explore various attempts to

4. The paper in the first volume was by Franz Weidenreich, then at the American Museum of Natural History (Weidenreich 1947). Volume 2 included a paper on blood groups (Lundman 1948), and volume 8 included a paper on prehominid dentition and hominid evolution (Robinson 1954). These papers generally upheld a typological view of humans.

5. Briefly stated, the influence of Franz Boas and his many students and protégés in America worked against the naive and destructive view of late-nineteenth-century evolutionary anthropology that emerged from

integrate anthropology—and anthropologists—within the wider synthesis of evolution in the interval of time between 1927 and 1962 by tracking intersecting individuals and groupings at critical junctures such as conferences and collaborative publications in the history of twentieth-century evolutionary biology and anthropology. I focus primarily on the discipline as my historical unit of analysis and on an unfolding of a series of rhetorical arguments used to discipline and bound areas of study as I begin the process of tracing part of an originary narrative—a kind of prehistory—of what was first referred to as "human evolution" and then "biological anthropology" in the latter half of the twentieth century that brought study of humans into the newer discipline of evolutionary biology (Smocovitis 1996). I begin my historical account with Dobzhansky, the key figure in establishing the science of evolutionary genetics and the architect who did the most to extend this domain to anthropology. I then work my way to the Darwin anniversary year of 1959 and to the very different figure of Sol Tax, a cultural anthropologist who sought to unify anthropology with the new evolutionary biology as part of a grand and ambitious program for modern anthropology. Organizing the single most successful celebration in honor of the 1959 centennial celebration associated with Charles Darwin and the publication of his *On the Origin of Species*, Tax used it as an opportunity to bring anthropologists into the fold of what appeared to be a unified evolutionary cosmology that would once again determine and define "man's place in nature" for an international postwar community of both elite intellectuals and the American public. I close with the Wenner-Gren Foundation symposium 19, held in 1962, that brought some of the same group of evolutionary biologists into an emerging new biological anthropology seen in molecular terms.

The Evolutionary Synthesis and Its Unifying Argument

Darwin of course knew nothing about genes and provided no convincing evidence for natural selection.[6] It took a small army of workers in the early years of the twentieth century to provide a framework for knowledge of the mechanism of heredity and proof of the efficacy of natural selection and then to align them in such a way as to make for a materialistic and mechanistic science that explained the maintenance and origin of biological variation. That was not easy. A number of individuals from varied disciplines, schools, methods, and even epistemic styles laid claim to Darwin's legacy (Bowler 1983; Provine 1971). All offered some means to amend his framework or to come up with the best fit of available data, but it took some time, new methods, new disciplines, and a turnover in personalities to provide a viable general theory of evolution. That theory had to be consistent with claims made in a number of diverse disciplines—from the newer genetics to systematics to paleontology and even to botany—to encompass a wide range of organisms and to employ the rigorous standards demanded of a positivistic science, namely observation and experimentation. It took, in short, the making of a new science of evolution, known as evolutionary biology, grounded in the mechanistic and materialistic principles familiar to the physical sciences.

The travails of evolution after Darwin are familiar lore and need not concern us greatly here.[7] What does matter is that a great deal happened in the interval of time between 1930 and 1950 to establish a "synthetic" science of evolution that set up a range of expectations for the relations between the disciplines of knowledge that demanded the inclusion of anthropology. Central to the relations in the disciplinary order was the belief in the unity of science—that all knowledge could be unified, usually through reduction to one universal language or protocol. Without getting too heavily into philosopher's categories of kinds of unification and whether or not a proper unification happened or even arguing specifics about the "influence" of positivist movements stemming from the Enlightenment and associated with figures such as Auguste Comte, Ernst Mach, or the Vienna Circle's "unity of science movement" in varied intellectual communities, I wish to argue instead that one outcome of the "evolutionary synthesis" was a strong argument for evolution as a unifying science within a unified view of biological science and a unified theory of knowledge (Smocovitis 1992, 1996). That argument was based on the following set of agreements: that the primary mechanism of evolution was natural selection; that it operated gradually and acted on small, individual differences; and that a continuum between microevolution (evolution below the species level) and macroevolution (evolution above the species level) existed. In other words, the mechanisms responsible for evolution were one and the same no matter what the level of selection might be: gene, individual, population, species, or higher-order group, including even opening the discussion to "mind" or "culture."

The continuum is crucial to understanding relations between the biological sciences and the social sciences. What it effectively did was to make possible the reduction of domains

and sustained racist and imperialist doctrines. For these reasons, the discipline of physical anthropology in the middle decades of the twentieth century had to distance itself from its own historical origins in such deterministic and racist doctrines. For the interplay of evolution, eugenics, race, and anthropology, see especially Cravens (1978), Marks (1995), Stepan (1982), Barkan (1992), and Armelagos (2004, 2011). For more on the history of American physical anthropology, see Haraway (1988) and Little and Kennedy (2010). See also the suite of papers devoted to the history of American physical anthropology in Spencer (1982). For an interesting philosophical account of the continued resistance to unification, the bounding between anthropology and biology, and the work of anthropologist Alfred Kroeber, see Kronfeldner (2010). See also Farber and Cravens (2009).

6. His evidence was indirect at best.

7. See the references in footnote 1, and see Bowler (2009) for a recent overview of the history of evolutionary thought.

associated with the social sciences to the biological sciences and ultimately to physics and chemistry. Thus, within that positivist ordering, the social sciences would logically be reducible to the biological sciences or at the very least would or should be in close relation to the science of biology. In other words, logically speaking, anthropology, the science of humans, whatever its manifestations (physical or cultural), had to be integrated or be "brought" to synthesis if in fact that synthesis were to follow its undergirding commitments to positivistic logic.[8]

It took a community—indeed a discipline—to craft this argument, but no one figure better represented it than Theodosius Dobzhansky. More than any other figure, it was Dobzhansky who was associated with the "synthesis" of genetics and selection and who argued explicitly for the continuum between microevolution and macroevolution.[9] It was also Dobzhansky who did the most to reach out to anthropologists and who provided the argument for the problematization of the concept of "race," which was used to bolster arguments for the synthesis between biology and anthropology.

Understanding the Synthesis: Dobzhansky and Evolutionary Genetics, 1927–1947

Dobzhansky's intellectual biography is by now well known.[10] Having trained in Russia, he early on came under the influence of populational approaches to systematics and genetics that placed emphasis on understanding geographic variation of natural populations (Dobzhansky was especially keen to work on the natural history and systematics of insects). Immigrating to the United States in 1927, he then trained with classical geneticist Thomas Hunt Morgan and his "fly" group. He broke with the preferred laboratory-oriented studies in *Drosophila melanogaster* and switched to *Drosophila pseudoobscura*, a species whose salivary chromosomes showed inversion frequencies (and other chromosomal traits) in particular patterns according to geographic locale. Tracking the inversion frequencies in varied populations of *D. pseudoobscura*, Dobzhansky and a cohort of workers inferentially recreated the phylogenetic history of the species and some of its relatives in an influential series of publications known as the "Genetics of Natural Populations," or the "GNP" series (Lewontin et al. 1981).

Dobzhansky was initially aided in his efforts by mathematician Sewall Wright, whose theoretical models postulated that selection would be most efficacious in small subdivided populations (Provine 1986). Wright in turn was enabled in his theoretical constructs by the well-known theorem developed earlier and widely known as the Hardy-Weinberg principle, which described the conditions under which evolutionary equilibrium would be maintained (i.e., conditions under which changes in gene frequencies do *not* take place).[11] By demonstrating how the "variables" of natural selection, genetic drift, and mutation could theoretically interact in natural populations, Wright and other theorists, such as R. A. Fisher and J. B. S. Haldane, worked in tandem with fieldworkers such as Dobzhansky in formulating experimental designs to demonstrate the efficacy of natural selection operating in wild (or nonlaboratory) populations in a way that made it measurable and quantifiable (Provine 1986; Smocovitis 1992, 1996).

The gene had been constructed to particularize and individuate and at the same time to limit the rate of change, while mutations were made to be the determinants of evolutionary change. Dobzhansky's "synthesis" offered an account of evolutionary change that would therefore limit and make deterministic the rate of evolutionary change. From then on, measures would be taken to calculate and determine evolutionary change, while evolution, in turn, would simply be defined as change in gene frequencies. Viewed as a problem in accounting for change, the Hardy-Weinberg principle, which effectively set the conditions under which there would be no evolutionary change, converted the variables of natural selection, mutation and population structure, random genetic drift, migration, and systems of mating into causal explanations for evolutionary change.

Genetics (and the physical world of the gene) thus was used as the grounding for the new "evolutionary genetics" (a new term for the synthesis of evolution and genetics) and formed the basis for Dobzhansky's belief in the continuum between microevolution and macroevolution (which stretched from the gene to the human and to human culture). The title of Dobzhansky's well-known *Genetics and the Origin of Species* (1937) reflects this grounding, which deliberately set itself as the genetical basis for Darwin's "incomplete" the-

8. Elsewhere I explore the history of this undergirding positivist logic and argue that it can be understood as a kind of epistemic package (Smocovitis 1996). The argument for unification should sound familiar; it undergirds the unifying vision of contemporaries such as Edward O. Wilson (1998) of *Consilience* fame and even more benign manifestations by thinkers such as David Sloan Wilson (2010), as in his famous "evolution for everyone" argument. The "domains" of biological knowledge were represented explicitly in diagrammatic form in E. O. Wilson's *Sociobiology: the new synthesis* (1975). Belief in the unity of knowledge undergirds traditional narratives of the history and philosophy of scientific knowledge originating with pre-Socratic philosophers such as Heraclitus. See also Rena Lederman (2005) for another exploration of the intersection of positivism, evolution, and anthropology.

9. In arguing for the continuum between microevolution and macroevolution, Dobzhansky had drawn on the insights of Sergei Chetverikov and the Russian school of population genetics that was later dispersed or destroyed by Lysenkoism. For more on Chetverikov's influence and on the school of population genetics that had influenced Dobzhansky, see Adams (1968, 1980, 1994).

10. For biographical treatments of Dobzhansky, see Provine (1981), Levine (1995), Adams (1994), and Kohler (1994); and see Glass (1980).

11. This is variously known as the Hardy-Weinberg equilibrium or the Hardy-Weinberg equilibrium principle or even the Hardy-Weinberg law. It has also been termed the Castle-Hardy-Weinberg principle, acknowledging the contributions of W. E. Castle. "Gene" frequencies have been replaced by the more precise "allelic" frequencies.

ory (it was incomplete because it did not offer understanding of the mechanisms of evolution in genetical terms). It offered a framework that brought together the material basis for evolution through the work of geneticists such as Dobzhansky with causo-mechanical explanations for evolutionary change made possible by the work of the mathematical modelers. But most importantly for us here, this mechanistic and materialistic framework grounded in genetics, and ultimately in the Hardy-Weinberg principle, could also account for higher-level phenomena—which included the origin of humans, of mind, of behavior and culture—now unifiable and reducible to lower-level phenomena.

Although the levels appeared unifiable and reducible, Dobzhansky and the other architects who followed in his wake also took measures to avoid complete reduction to the physical sciences; a complete reduction to genetics led to genetic determinism and the end of free will as well as the complete elimination of belief in a purposeful, progressive, and meaningful life. It also meant that biology as a discipline itself would literally become subsumed or engulfed or reduced to the physical sciences so that an argument for the autonomous status of the biological sciences could not be supported. As a group, or discipline, more accurately, the architects of this evolutionary synthesis negotiated and struck just the right balance between mechanistic materialism and physicalism and some form of emergentism to avoid complete reduction to the physical sciences. Hence, properties and phenomena deemed "emergent" were often evoked to argue against complete reduction to the physical world. This was the case with Dobzhansky and Ernst Mayr. Mechanistic materialism was upheld by George Gaylord Simpson, who argued that chance events and historical contingency—processes associated with history—introduced a kind of indeterminism into evolution and thence to biology, which made it unlike the physical sciences.[12]

Dobzhansky's argument was additionally convenient because it enabled him to engage the eternal question of "what is man?" and to locate "man's place in nature." Descended from a family of Russian Orthodox priests (hence the name Theodosius) and an observant follower of the Russian Orthodox church, Dobzhansky was obsessed with metaphysics through much of his life. He wrote a number of essays and books after 1940 that explicitly addressed the "phenomenon of man" (to use Teilhard de Chardin's term, which he approved of greatly), tackling the subjects of the meaning of life, death, and questions of meaning and existence (Dobzhansky 1967). He also fought vigorously against genetic determinism, arguing that though an animal, "man" could escape its own brute nature through use of "mind." "Man" as the "Pinnacle for Evolution" was a constant theme that appeared from his textbooks, as in *Evolution, Genetics and Man* (1955:373), to "Man," in his final chapter of the "Center of the Universe," as he noted in *Mankind Evolving* (1962). These were inconsistent if not contradictory assertions, of course, given the fact that selection was a deterministic, materialistic, and mechanistic force, at least as the architects had been construing it. Indeed, mechanistic materialism was supposed to undercut teleology, but they characterized much of Dobzhansky's thinking from the evolutionary synthesis on.

Dobzhansky's intellectual contortionism was not unique; virtually all the major evolutionists in the 1940s and onward frequently engaged such heady topics in their semipopular and popular essays and works, which were read widely in the middle decades of the twentieth century. This was certainly true for Julian Huxley, who famously introduced Teilhard de Chardin's 1950 *Phenomenon of Man*, extolling progress and the advancement of humans and human "improvement" at the same time he promoted the mechanistic materialism of natural selection in the book that heralded the synthesis and gave it its name: *Evolution: The Modern Synthesis* (Huxley 1942). This was also true of G. G. Simpson, who waxed metaphysical about the mechanistic and materialistic science in *The Meaning of Evolution: A Study of the History of Life and of Its Significance for Man* (1949), *This View of Life: The World of an Evolutionist* (1964), as well as in his textbook titled, auspiciously, *Biology and Man* (1969).[13] Progressive evolution—a serious contradiction in terms—grounded much of this worldview.[14] Best expressed in Huxley's *Evolution: The Modern Synthesis* (1942) and echoed by others such as Simpson, H. J. Muller, Dobzhansky, and an entire generation of evolutionists, the argument linking progress with selectionism and metaphysical materialism went something like this: humans were "unique" in their capability to modify their environment; in other words, technology was what made humans unique (small wonder humans became defined as special creatures because of an opposable thumb). Through the same technology, humans were able to control their own development through conscious wilful—and disciplined—use of intelligence. This same intelligence would also be able to generate human values, such as "morality, pure intellect, aesthetics, and creative activity." "Man" was therefore unlike any other animal forms. Evolution was thus as "much a product of blind forces as is the falling of a stone to earth or the ebb and flow of the tides," but purpose itself would only come from human will. Strikingly, Huxley wrote "purposes in life are made, not found" (Dobzhansky 1955; Huxley 1942:576; Muller 1949). A true celebration of humanism, albeit of a

12. Stephen J. Gould has echoed Simpson in his use of historical contingency; see Gould's *Wonderful Life* (1989) for one example of this use of historical contingency in evolutionary philosophy. I am here focusing on the discipline or group rather than exploring individual opinions or tracking their evolution over time. In keeping with the positivist trajectory of the discipline, I am therefore stressing the unity rather than the diversity of opinion. See Smocovitis (1996) for more historiographic discussion on the advantages and disadvantages of the discipline as a historical unit of analysis.

13. Simpson, it should be noted, inspired a great deal of the lyricism seen in his intellectual heirs, such as Stephen J. Gould.

14. See Nitecki (1988), especially the paper by Provine in the collection; see also Ruse (1996) and Haraway (1988, 1989).

secular kind, such a progressive worldview enabled the human to be a creature subject to selection but also able to override its evolutionary destiny through conscious wilful use of its mind. "Man," according to Huxley, could therefore "stand alone."[15] So influential were works of Huxley in tandem with the works of Dobzhansky, Simpson, and others—all of which were associated with establishing a new scientific, evolutionary, or secular humanism—that intellectual historian and critic John Greene deemed these "sacred" texts and referred to them as the "Bridgewater treatises of the twentieth century" (Greene 1981:163).[16]

This evolutionary worldview would do one more thing: it would help stabilize an ideology. Most clearly expressed at the end Huxley's 1942 book, Huxley revealed his fears of the politics of the 1930s and of the great collectives that threatened to lead to the "subordination of the individual" or of leading a life whose purpose would be fulfilled in "a supernatural world" (Huxley 1942:578). In his view, the struggle between these two opposing extremes was the challenge facing the modern world. His vision of progressive evolution would help provide solutions to a number of growing global problems of concern to the growing community of international intellectuals. With selection acting on the individual, the individual could be unique at the same time that it existed in a social group or collective. Neither totally mechanistic or materialistic (thus avoiding left-wing extremism in communism and atheism) nor too vitalistic/mystical/spiritual (thus avoiding right-wing fascism, Nazism, and religious fundamentalism), this evolutionary framework balanced mechanistic materialism with purpose and progress to sustain and justify a moderate liberal ideology. As the war ended and as the horrors of the Holocaust, the cold war, and the nuclear nightmare took center stage, belief in selection and the adaptability of life as it was being articulated by the architects that offered a sense of progress, a liberal ideology, and an optimistic and coherent worldview with humans as the agents of their own evolution intensified. Such a view would also help account for, justify, and enable the inexorable progress of the atomic age and then of space-age technology. Human "control" of evolution, in a non-"Hitlerian" manner avoiding "negative eugenics," would again elevate humanity from its dark past as it moved into the future, assuming the creature did not destroy itself through the development of the new atomic weapons (Haldane 1949; Muller 1949).

With the end of the war, Julian Huxley formally took his interest in solving global problems into the political realm by serving as the first secretary general to UNESCO in 1946 (Waters and Van Helden 1992). At his and Joseph Needham's insistence, the *S* for science was formally included in the name of the new organization. His new cosmology, "scientific humanism" (later termed "evolutionary humanism"), which substituted evolution for conventional religion as a source of knowledge or ethics, grounded his new political philosophy (Blue 2001; Smocovitis 2009). Drafting a 60-page report, Huxley wrote what amounted to a document that was part political philosophy, part evolutionary manifesto, setting forth the policies of the organization. It drew more than a bit of criticism for its rank atheism, requiring a small slip to be included stating that the contents were those of the author alone and not of the organization. In 1950, just after stepping down as the first director general, he played an active role in helping to draft the UNESCO statement on race challenging the existence of the innate biological differences between different human races. That statement on race, which included a number of other contributors such as anthropologist Ashley Montagu, was inspired by a view of race fundamentally shaped by the new evolutionary genetics, which grounded its claims on Dobzhansky's views on speciation (Barkan 1992, 1996; Gayon 2003; Haraway 1988, 1989; Stepan 1982).[17]

Bring on the Anthropology

Like the other architects, Dobzhansky shared Julian Huxley's liberalism and a similar kind of ethical system (an "ethos") grounded in evolution. Crucial to our purposes, his evolutionary genetics did one more thing to enable the integration of anthropology and biology: it drew attention to mechanisms of speciation, to geographic variation, and to isolation that "moved" or rendered dynamic the process of species formation, ultimately giving shape to the modern definition of the biological species concept as a population of actually or potentially breeding individuals. Dobzhansky's views were based on his experiences with geographic races in the natural populations of *Drosophila pseudoobscura*. As is now well known, by redefining race in terms of populations, Dobzhansky "loosened" the boundaries of the term to locate it in time, place, and space; in other words, speciation and race formation were rendered processual. As he himself defined it, race was a "tool for description, not of individuals, but of subdivisions of species," and the system for race was "open" and therefore fluid, unlike the species system, which he viewed as "closed" (Dobzhansky 1941, 1944:138).[18] Dobzhansky's

15. Huxley wrote an entire book arguing for the uniqueness of humans titled *The Uniqueness of Man* in 1940. The American edition was titled *Man Stands Alone.*

16. For an especially insightful discussion that explores the redefinition of humanity in light of technology and its evolutionary past as "man the hunter" in the wake of the evolutionary synthesis and the work of Sherwood Washburn, see Donna J. Haraway's essay "Remodelling the Human Way of Life" (1988) and see Haraway (1989). For more on Dobzhansky's international influence, especially in Brazil, see Araújo (1998, 2004).

17. A great deal has been written on the history of the UNESCO statement on race. In addition to Haraway (1988), Barkan (1992, 1996), and Gayon (2003), see also Marks (2008, 2010) and Michelle Brattain (2007). See also Montagu's own exegesis in Montagu (1972).

18. See Farber (2009) for a precise explanation of Dobzhansky's view of "race." For a philosophical perspective, see Gannet (2000). In 1947 Dobzhansky collaborated with his Columbia colleague L. C. Dunn on a widely read book exploring heredity and the newer meaning of race (Dunn and Dobzhansky 1947). See also Melinda Gormley (2009) for a discussion of race in Dunn and Dobzhansky (1947) that focuses on Dunn.

view of evolution as a whole placed emphasis on populational rather than typological approaches to the natural world, to use Ernst Mayr's celebrated distinction (Mayr 1980). The populational recrafting enabled the de-essentializing of the term that had been most responsible for enabling the bounding—and separation—of anthropology and biology.

Dobzhansky's redefinition had emerged from his work in insects (Dobzhansky 1937; Dobzhansky and Epling 1944), but he did not hesitate to recognize its significance for humans; he made the deliberate effort to promote the new biological meaning of the term (Dobzhansky 1941) and took it formally to physical anthropologists in 1944 with an article on human evolution that appeared in the *American Journal of Physical Anthropology*, which had just undergone an editorial change. The argument set forth in 1944 later reappeared in the 1950 UNESCO first statement on race. But even before 1944, Dobzhansky's redefinition was seized on by a younger generation of physical anthropologists, such as Ashley Montagu and Sherwood Washburn, keen to integrate it with biology.

In 1940 Montagu presented a paper at the American Association of Physical Anthropologists laying out the new biological, populational, and de-essentialized meaning of the term based on Dobzhansky's *Genetics and the Origin of Species* (1937). Like the dynamic evolutionary understanding gained in *Drosophila*, so too would a similar approach be used to understand evolution in humans: human species could be seen in terms of populations subject to the same evolutionary parameters determined for any species—they were subject to mutation, selection, migration, mating, and random genetic drift. Race was just a population of individuals subject to the same kinds of forces. Going beyond Dobzhansky and building on the work of others—such as Julian Huxley (Huxley and Haddon 1936), who had argued that the concept of race was a mythic construct—Montagu proposed the substitution of the term with "ethnic group" (Montagu 1942*a*). In 1942, he rolled much of this growing concern with race and biology together into a monograph that he subsequently published as his enormously influential *Man's Most Dangerous Myth: The Fallacy of Race* (1942*b*). He also explicitly collaborated with Dobzhansky to reach an audience of scientists in 1947 (Dobzhansky and Montagu 1947). Though the two shared fundamental points of agreement, they increasingly disagreed on the use—and reality—of the term because Montagu increasingly argued that it was a social construct that emerged from an attempt to subordinate historically significant groups. For Dobzhansky, the term "race" continued to preserve a biological reality and taxonomic utility; he refused to abandon its use and sparred with Montagu all through the 1940s about the topic (Farber 2009).

Despite such differences, the two came together on numerous occasions, and in 1950 they were part of one of the first attempts to bring together formally geneticists with anthropologists at a large Cold Spring Harbor symposium of 1950. That meeting was orchestrated by the geneticist Milislav Demerec, but the program for the symposium had been organized by Dobzhansky and his anthropological collaborator Washburn. Like Montagu, Washburn had instantly appreciated the emphasis on populational thinking that delegitimated typological thinking about race. This enabled him and like-minded physical anthropologists to ground physical anthropology in the new discipline of evolutionary biology without fear of evoking essentialistic views of race at the same time it allowed him to extend his own work in functional comparative anatomy and behavior (Haraway 1988, 1989). Washburn had assimilated the tenets of the evolutionary synthesis as they were emerging from Dobzhansky but especially from the work of mammalogist and paleontologist G. G. Simpson (1944), and he eagerly sought to bring anthropologists into the fold. Indeed, citations of the "new systematics" and of the zoological literature were generous in Washburn's written work. He wrote, "the meeting of genetics, paleontology, and evolutionary zoology created a new systematics (neozoology), just as the impact of the new evolutionary theory is creating a new physical anthropology" (Washburn 1952:715). To that end, Washburn orchestrated a number of meetings and publications, of which the 1950 Cold Spring Harbor symposium was the largest and most visible.

The edited volume of the symposium, titled ambitiously as *Origin and Evolution of Man*, listed no less than 129 paid attendants, including nearly all of the major anthropologists, geneticists, and evolutionists active at the time. The most important aspect of the symposium for our purposes here is what the organization of the program reveals, namely, the fundamental argument for the unification of anthropology with biology grounded in a view of evolutionary genetics as applied to solving long-standing problems of human evolution. Beginning with population genetics or "population as a unit of study," the topics moved to the "origin of the human stock" to the "classification of fossil men" (a rather transparent elimination of "fossil women") to no less than three entire sessions dedicated to the "genetic analysis of racial traits" known to that time. Then, working from the topic of "race concept and the human race" to the final topic called "constitution," the program reflected the narrative of synthesis and unification between two disciplines that had been kept apart (as Milislav Demerec's foreword to the edited volume reveals in the opening epigraph here). Whatever the differences in participant's individual views, which the papers reveal were abundant, and no matter how heated the exchanges, the meeting reflected the undergirding logic of unification within a unified theory of knowledge. Most importantly, it offered prescriptives for future research.

Reflecting on the meeting over 30 years later, Ernst Mayr recalled that the most memorable session was the one titled "origin of the human stock," which included the paper given by Sherwood Washburn titled "The analysis of primate evolution with particular reference to the origin of man." According to Mayr, Washburn "discussed certain evolutionary processes using the word *population* in just about every second or third sentence. After Washburn had finished, Hooton

[Ernest A., the influential Harvard anthropologist with whom Washburn had trained] got up and said 'I hate the word population.'" Mayr added "I am afraid he [Hooton] fought a losing battle. By 1950, population thinking had established a well-entrenched beachhead in anthropological thought."[19] Mayr's own sense of the importance of the meeting was summarized as "the occasion that the study of fossil man was integrated into the evolutionary snythesis [sic]" (Mayr 1982: 231).

In this, Mayr was likely right; no less than one year later, Sherwood Washburn proclaimed the birth of a "new" physical anthropology in a famous 1951 paper that "codified" what historian Donna Haraway described as "the polemic and research program joining physical anthropology to the evolutionary synthesis" (Haraway 1988:224; Marks 2008, 2010; Stepan 1982; Washburn 1951). Washburn himself continued to promote the "new" physical anthropology and especially the emerging area of primatology through not only his own publications but also through his numerous students and protégés as well as by organizing conferences and symposia, especially through the New York–based Wenner-Gren Foundation, which changed its name from the Viking Fund in 1951. Its first director, Paul Fejos, had become a professional friend after Washburn had moved to Columbia. Between 1945 and 1952, while first secretary of the American Association of Physical Anthropologists and then president of the group, Washburn organized a number of summer seminars and ensuing publications that were funded by the Wenner-Gren Foundation. In 1951, just over 10 years after the founding of the organization, Washburn was part of the planning team "to assess the accomplishments of anthropological science to date and to solicit answers on what direction future research would be likely to take" (Fejos 1953:v). In his own contribution to the Wenner-Gren symposium of 1952, the published proceedings of which became *Anthropology Today*, Washburn further extended the argument developed in 1951 in a paper titled "The Strategy of Physical Anthropology" (Washburn 1952). Along with the 1951 paper, it heralded the "new" physical anthropology that not only looked to the achievements of the evolutionary synthesis but that also announced a revolutionary break with the older version popular in the late nineteenth century that had been plagued by essentialism, racism, and biological determinism. Both papers were reprinted and cited heavily by the new generation of anthropologists, who entertained a revivified science of human evolution as seen in the abundant publications, conferences, and projects in the area (Haraway 1988).

The 1953 volume itself—a behemoth at just under 930 pages of written text—was edited by Alfred L. Kroeber and subtitled *An Encyclopedic Inventory*. It was the most ambitious attempt to bring together diverse representatives and perspectives that had both grown but that had also diversified enormously in the postwar period and to "hammer" what Kroeber described as "a vast array of knowledge" into "a set of coherent interpretations" (Kroeber 1953:xiv). For Kroeber, that unifying principle was the concept of "culture," but the collection as a whole additionally signalled the transformation of the "new" physical anthropology and included not only perspectives from Washburn but also William C. Boyd, whose "The Contributions of Genetics to Anthropology" drew on the view of races articulated in *Drosophila* spp. by Theodosius Dobzhansky and Carl Epling (1944) applied to human blood groups (Boyd 1953). Such edited collections—based on workshops or research projects that encouraged interdisciplinarity and that enabled dialogue between the increasing numbers of anthropologists all over the world who shared commitments to the problematization if not the complete elimination of something called "race"—thus proved crucial to the emergence of what increasingly came to be known as "biological anthropology" or later termed the "biocultural perspective" (Armelagos 2008). Between 1951 and 1961, for example, the Wenner-Gren Foundation alone sponsored some 47 research projects (or some 61% of its research budget) on paleoanthropology or on additional support for conferences on "early man" or the publication of results (Baker and Eveleth 1982).

Celebrating Darwin in 1959: The Unifying Vision of Sol Tax and Anthropology at the University of Chicago

Clearly, by the middle decades of the twentieth century, a growing number of anthropologists were entertaining evolutionary approaches to their field that combined knowledge of genetics, paleontology, and systematics as it had emerged from the evolutionary synthesis the previous decade. The same extension of the evolutionary synthesis to the social sciences was also seen in areas such as psychology, which began to integrate mind, culture, and behavior with newer postsynthesis areas such as ethology (Burkhardt 2005). Formal conferences and meetings to bring psychology into the fold took place in 1955 and 1956 and were sponsored by the American Psychological Association and the SSE. George Gaylord Simpson and his spouse, psychologist Anne Roe, were prominent players in these meetings, but so too were the other architects of the synthesis such as Ernst Mayr and Julian Huxley along with anthropologists such as Sherwood Washburn (Roe and Simpson 1958).

Such grand and ambitiously interdisciplinary and often international meetings that attempted to unify biology with social sciences such as psychology and anthropology were part of a growing trend to unify all knowledge. By the middle decades of the twentieth century, the drive to unify all knowledge was made apparent by the frequent appearance of terms such as "culture," "discipline," "unity," and "diversity" in both elite and popular discourse (see Tagliacozzo 1962). In 1954, for example, an enormous meeting took place that brought an astonishing number of social scientists, biologists, and physical scientists together with humanists in a conference

19. A variant of this story is retold by Marks (2010).

Figure 3. Sol Tax at the final evolution session at the Darwin centennial at the University of Chicago, 1959. Archival photographic files (apf3-00592), Special Collections Research Center, University of Chicago Library. Reproduced with permission.

titled "The Unity of Knowledge" at Columbia University (Leary 1955). It included a number of the same architects of the evolutionary synthesis along with a number of prominent anthropologists and psychologists.[20] But that drive for unification between the sciences of evolution, biology, and anthropology was nowhere more apparent than in making the preparations for the anniversary year of 1959, which celebrated the twin events of the 150th anniversary of Charles Darwin's birth, and the 100th anniversary of the publication of his magnum opus, *On the Origin of Species.*

Bringing biology and anthropology together through evolution and the occasion of the great anniversary became the brainchild of Sol Tax, a social and cultural anthropologist at the University of Chicago best known for his studies of some North American and Latin American indigenous cultures (fig. 3). It was in fact the perfect opportunity to draw attention to anthropology and to the University of Chicago, which Tax thought was especially well positioned for the celebration; when it came to anthropology and the social sciences, Chicago had always seemed a special "world unto itself" (Bulmer 1984; Silverman 2005:272; Stocking 1979). At the height of his career, Tax was a talented organizer, an editor, an effective networker, and best of all, a visionary keen to bring it all off (Rubinstein 1991; Stanley 1996; Stocking 2000; Wax 2008). He was already predisposed to the subject of "bioanthropology," having written an honors paper while still an undergraduate at the University of Wisconsin on animal behavior and culture (Haraway 1988), and he was directly involved in coediting the "encyclopedic" *Anthropology Today* with Alfred L. Kroeber, Loren Eiseley, Irving Rouse, and Carl F. Voegelin.

Recounting the origin of his idea in his memoir of the celebration, Tax admitted that it came to him in 1955 while current editor of *American Anthropologist* and while in the library of the Wenner-Gren Foundation in New York attending a "supper conference" with William W. Howells as the featured speaker on physical anthropology. Looking up the anniversary date of the publication of Darwin's *Origin*, November 24, he noted it was a "good season" for academics and that it would be grand to celebrate at his home institution, which was founded 10 years after Darwin's death. Stressing the "purely intellectual and scientific" interest in the occasion,

20. See especially part three of the edited collection titled "The Knowledge of Man" that included contributions by Julian Huxley, Gardner Murphy, and Alfred L. Kroeber. The conference included participants such as Teilhard de Chardin, Theodosius Dobzhansky, Margaret Mead, B. F. Skinner, Willard Van Orman Quine, Ernest Nagel, and Phillip Frank.

Figure 4. Darwin centennial committee; standing, from left to right: H. Burr Steinbach, Everett C. Olson, Ilza Veith, Sol Tax, Alfred E. Emerson, and Chauncy Harris. Archival photographic files (apf3-00598), Special Collections Research Center, University of Chicago Library. Reproduced with permission.

Tax also stated clearly that it would be a perfect way to unite the fields of biology and anthropology. As he pointed out in his recollections, for historical, sociopolitical, and intellectual reasons, twentieth-century anthropology had seen the "complete separation" of "man as an organism from man as a member of society and bearer of culture." "Culture" and "evolution," which had been united in the thoughts of Darwin and his "bulldog" Thomas Henry Huxley (Julian's grandfather), had gone their separate ways in the twentieth century as cultural anthropologists railed against overly rigid, deterministic, "evolutionary" explanations for culture. As a result, anthropology had become a science with a "split personality" (Tax 1960:271–272, 1988).

Tax realized that the University of Chicago had already begun to heal that rift. Within the anthropology department, a graduate-level course called Human Origins (the first segment of a three-part course, numbered Anthropology 220) taught by Robert Braidwood, Wilton M. Krogman, Robert Redfield, and Sol Tax had attempted to integrate approaches from biology and anthropology beginning in 1945. With the addition of Sherwood Washburn to the department in 1947, an integrative perspective on evolution and culture was already in place. Washburn had instantly begun to inject his own understanding of genetic mechanisms and paleontological insights into that class with reading lists that included the works of Dobzhansky and Simpson, and because Tax had worked with Washburn in that course, both had developed a shared vision of the discipline's newer directions. The time was clearly ripe for such a celebratory event that would recognize the new integrative directions in anthropology, and Chicago was the perfect place to showcase the new union of biology and anthropology. With a small interdisciplinary group of colleagues including Everett Olson (a paleontologist) and Alfred E. Emerson (an insect systematist), Tax began the planning in earnest shortly thereafter. Two of the committee members, Karl P. Schmidt and Robert Redfield, died in the interim, so the final committee was comprised of Everett Olson, Chauncy Harris (a geographer), Alfred E. Emerson, Ilza Veith (a historian of medicine), and H. Burr Steinbach (a zoologist; fig. 4).

With the ambitious goal of opening discussion into the varied meanings of the term "culture," Tax set out to target distinguished anthropologists for his meeting. He invited Clyde Kluckhohn from Harvard University, A. Irving Hal-

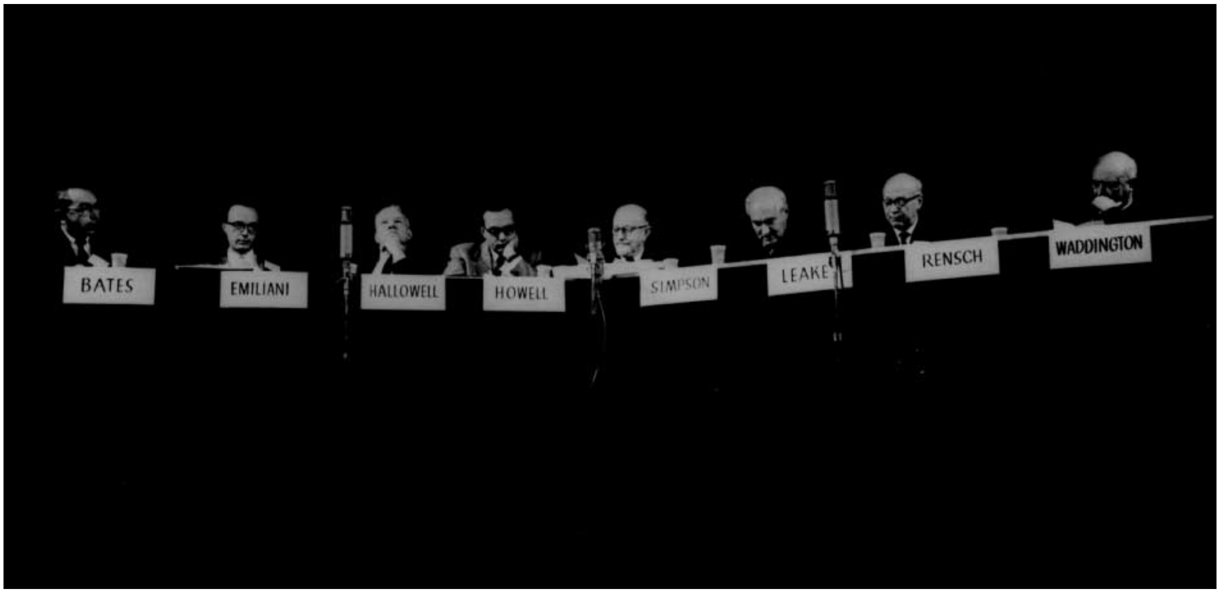

Figure 5. Darwin centennial, panel 3, "Man as an Organism." A transitional panel, bringing anthropological concerns to evolutionary biology, it included evolutionary biologists with an interest in human evolution and biographically trained anthropologists and paleontologists. Marston Bates, Cesare Emiliani, A. Irving Hallowell, Louis B. Leakey, Bernhard Rensch, and C. H. Waddington. The chairs were George Gaylord Simpson and F. Clark Howell. Darwin Centennial Papers (negative number T 0861226), Special Collections Research Center, University of Chicago Library. Reproduced with permission.

lowell from the University of Pennsylvania, and Alfred Kroeber from the University of California, Berkeley, all of whom joined the impressive list already assembled at the University of Chicago, which included F. Clark Howell. Tax studiously worked to provide funds for each of them, working with the Wenner-Gren Foundation for Anthropological Research. He did not confine his anthropological invitation list to the narrower domain of academic anthropology. In a brilliant strategic move, he extended the anthropological sphere of influence and drew the attention of the wider public by obtaining special funds from Wenner-Gren to invite Louis B. Leakey, an anthropologist with increasingly broad popular appeal. Taking the opportunity to visit the United States for the first time and further promote his research, Leakey brought the latest of his sensational fossil hominids, *Zinjanthropus boisei* ("Zinj"), with him to the celebration (2009 marked the fiftieth anniversary year of "Zinj" it will be recalled). Both Leakey, his wife Mary, and "Zinj" served as major highlights of the celebration conference, drawing attention to anthropology as a central study in evolution. Indeed, some of the most well-known iconic images of the celebration featured Louis and Mary Leakey and the fossil find.[21]

Elsewhere I have described in detail the happenings in Chicago orchestrated by Sol Tax in honor of the Darwin centennial of 1959 (Smocovitis 1999). For our purposes here, I draw attention to the centerpiece of the celebration festivities, a series of five panels with a number of participants formally arranged.

Panel 1. "The Origin of Life." Biochemists and astronomers discussed cosmic evolutionary processes on Earth and other suitable planets. The participants were Sir Charles Galton Darwin, T. Dobzhansky, Earl A. Evans Jr., G. F. Gause, Ralph W. Gerard, H. J. Muller, and C. Ladd Prosser. The chairs were Harlow Shapley and Hans Gaffron.

Panel 2. "The Evolution of Life." Evolutionary biologists discussed current understanding of evolutionary processes with natural selection as the dominant process. This panel included many of the architects of the evolutionary synthesis: Daniel I. Axelrod, T. Dobzhansky, E. B. Ford, Ernst Mayr, A. J. Nicholson, Everett C. Olson, C. Ladd Prosser, G. Ledyard Stebbins, and Sewall Wright. The chairs were Julian Huxley and Alfred E. Emerson.

Panel 3. "Man as an Organism." This was a transitional panel, bringing anthropological concerns to evolutionary biology. It included evolutionary biologists with an interest in human evolution and biologically trained anthropologists and paleontologists. The participants were Marston Bates, Cesare Emiliani, A. Irving Hallowell, Louis B. Leakey, Bernhard Rensch, and C. H. Waddington. The chairs were George Gaylord Simpson and F. Clark Howell (fig. 5).

Panel 4. "The Evolution of Mind." This panel brought together psychologists and physiologists to discuss currents of

21. See, e.g., the photograph of Louis and Mary Leakey and "Zinj" reproduced for the Web site of the University of Chicago's 2009 celebration: http://darwin-chicago.uchicago.edu/50th-anniversary.html (accessed May 20, 2011).

Figure 6. Darwin centennial, panel 5, "Social and Cultural Evolution." This panel represented the bridge between biological and cultural evolution. Robert M. Adams, Edgar A. Anderson, Sir Julian Huxley, H. J. Muller, Fred Polak, Julian H. Steward, Leslie A. White, and Gordon R. Willey. The chairs were Clyde Kluckholn and Alfred L. Kroeber. Archival photographic files (apf3-00595[2]), Special Collections Research Center, University of Chicago Library. Reproduced with permission.

thought on the evolution of mind. The participants were Henry W. Brosin, Macdonald Critchley, W. Horsley Gantt, A. Irving Hallowell, Ernest Hilgard, Sir Julian Huxley, H. W. Magoun, Alexander von Muralt, and N. Tinbergen. The chairs were Ralph W. Gerard and Ilza Veith.

Panel 5. "Social and Cultural Evolution." This panel represented Tax's bridge between biological and cultural evolution and brought together anthropologists and behavioral ecologists. It included Robert M. Adams, Edgar A. Anderson, Sir Julian Huxley, H. J. Muller, Fred Polak, Julian H. Steward, Leslie A. White, and Gordon R. Willey. The chairs were Clyde Kluckholn and Alfred L. Kroeber (fig. 6).

Like the Cold Spring Harbor symposium of 1950, the ordering of the panels is telling. It reveals to us again Tax's vision of the unification of the disciplines and the location of each discipline within the positivist ordering to knowledge. From physics to chemistry and cosmology to evolution and biology and to the social sciences (including here psychology and anthropology), the panels offered a narrative of unification locating "man's place in nature." Unlike the meeting at Cold Spring Harbor, however, the unification was much broader in nature, extending the reach of evolution not only to the reductionistic, mechanistic, and materialistic physical sciences but also directly to the social sciences, opening discussion into the evolution of not only human origins and culture but also behavior and even "mind." Examined broadly, the panels represented the big picture of the unification of scientific knowledge so as to answer once again the question of "what is man" and locating "man's place in nature." More immediately for Sol Tax, the panels—and the celebration—offered a visual and very public demonstration of the union of biology and anthropology that had been torn apart by bankrupt anthropological and evolutionary theories that made up an older "evolutionary anthropology." As he wrote later in his reflections, "So, for me, the Centennial brought Darwin and evolution back into anthropology, not by resurrecting analogies, but by distinguishing man as a still-evolving species, characterized by the possession of cultures which change and grow non-genetically." He further added an escape clause: "Human evolution includes the addition of culture to man's biology; 'cultural evolution' at the human level is quite a different matter. Anthropologists accept the first without question; they are divided about the second" (Tax 1960:282).

Tax's celebration was not the only such forum bringing anthropologists together with evolutionary biologists. The 1959 Cold Spring Harbor symposium in honor of the centenary once again brought students of evolution from the fields of "genetics, anthropology, and paleontology" to "join forces" for the second time in a decade at the same place (Demerec 1960). The program of the symposium was planned by Dobzhansky with the assistance of anthropologist Carleton S. Coon, plant evolutionary biologist G. Ledyard Stebbins, and geneticist Bruce Wallace. In contrast to the meetings of evolutionists in the 1930s and 1940s, therefore, anthropologists and the subject of human evolution had become fairly standard fare at meetings devoted to general views of evolution.

Nonetheless, divisions existed and indeed in some instances were magnified in the next decade of research. The number of self-identified biological and evolutionary anthropologists, however, continued to increase as human evolution proved an increasingly productive and very popular area of inquiry.[22] In 1962, for example, an entire Wenner-Gren meeting was devoted to the subject of human evolution, bringing together some of the same evolutionary biologists with a background in animal systematics with some of the new physical anthropologists. Organized by Sherwood Washburn and titled "Classification and Human Evolution," the meeting was the first such Wenner-Gren symposium devoted expressly to "human evolution" and to discussion of human phylogenetic history, or human systematics. It was designated as the Burg Wartenstein symposium (after the Viennese-castle-turned-conference-center for the Wenner-Gren Foundation). The symposium brought Washburn's "new physical anthropology" to an even newer direction as it sought to integrate the newer techniques coming from molecular biology to human systematics by bringing together evolutionists such as Ernst Mayr, Theodosius Dobzhansky, and George Gaylord Simpson together with psychologist Ann Roe; anthropologists Sherwood Washburn, Louis Leakey, and Irven de Vore; and Emil Zuckerkandl and Morris Goodman, who had backgrounds in biochemistry and serology relying on molecular techniques.[23] Taking place just at the peak of what E. O. Wilson termed the "molecular wars" of the early 1960s that followed the emergence of molecular biology (Wilson 1994), it brought to light the tension between the newer reductionist molecular approaches and older antireductionist approaches that had also grounded evolutionary humanism.[24] The new placement of humans in a phylogenetic category alongside chimpanzees and gorillas based on these molecular techniques and the declaration that a new discipline called "molecular anthropology" had emerged (Goodman 1962; Zuckerkandl 1962) did not sit well with the same architects of the evolutionary synthesis who had done so much to argue for the special or unique status given to humans and who had worked hard to preserve the delicate balance between the unity of science and the autonomy of biology with a view of a central unifying science of evolutionary biology. No strangers to controversy or vitriolic attacks, the architects fired back as fireworks ensued and continued throughout the decade of the 1960s as molecular data on primate classification increasingly pointed to the relatedness of humans, gorillas, and chimpanzees (Goodman 1996).[25] That explosive mix of integrative argument—and reductive methodology—continued well into the 1990s and culminated with the Human Genome Project, which launched a full-scale human biodiversity project. By the end of the 1990s, as "biology" and "anthropology" were overlapping to an unprecedented extent, arguments began to emerge for the division of anthropology, with independent and indeed departmental status given to "biological anthropology" and even something called "human evolutionary biology" (Leslie 2000; O'Toole 1998; Shea 1998; Troianovski 2005). By then, however, and just as the voices of the architects had begun to wane, a number of anthropologists, more precisely, human evolutionists, had begun to adopt the evolutionary synthesis as part of their own historical narrative of origins (Bowler 1986; Cela Conde and Ayala 2007; Delisle 2007; Goodrum 2009; Tattersall 2000).

Analytical Perspective and Closing Thoughts

In this brief paper I have tried to follow efforts to unify anthropology with evolutionary biology following the "modern synthesis" of evolution that took place between 1930 and 1950 and the establishment of evolutionary genetics as it

22. See, e.g., the increasing emphasis given to topics such as primatology and paleoanthropology in the pages of *National Geographic* in the 1960s and in other popular venues such as television specials.

23. Participants included Josef Biegert, Bernard G. Campbell, Irven DeVore, Theodosius Dobzhansky, Irenaus Eibl-Eibesfeldt, Morris Goodman, K. R. L. Hall, G. Ainsworth Harrison, Harold P. Klinger, L. S. B. Leakey, Ernst Mayr, John Napier, Jean J. Petter, Adolph Schultz, George Gaylord Simpson, William L. Straus Jr., Sherwood Washburn (who served as organizer), and Emile Zuckerkandl. The signature book of the Wenner-Gren Foundation located at the foundation office also includes Ann Roe as one of the signatories, accompanying her spouse George Gaylord Simpson. The meeting was held between July 8 and 21, 1962. See also the collection of papers to come out of the symposium (Washburn 1963).

24. For a historical account of the complex disciplinary negotiations that ensued from the origins of molecular biology as a scientific discipline whose methods pushed the biological sciences further into reductionism and determinism, see Smocovitis (1992, 1996), and see Beatty (1990) for a brief historical and philosophical treatment of the "DNA bandwagon effect." For more on the conflicts surrounding the introduction of molecular techniques in evolution, anthropology, and in the origins of molecular anthropology, see Dietrich (1998), Morgan (1998), Hagen (1999, 2009), Aronson (2002), and Sommer (2008).

25. See, e.g., a similar series of vitriolic exchanges with biochemists and astronomers over "exobiology" and the argument for the nonprevalence of "humanoids," first by G. G. Simpson and then Ernst Mayr. These exchanges, I have argued, were also part of the complex negotiations between related disciplines of knowledge that preserved the boundaries, giving enough unity between related disciplines that also preserved autonomous status (Smocovitis 1996).

emerged through the work of Theodosius Dobzhansky. I have also explored his fellow architects, who grounded their claims in evolutionary genetics and then attempted to create or build an evolutionary cosmology within a unified theory of knowledge, and how the architects functioned as "the unifiers" of knowledge, balancing and preserving enough of the autonomy of the sciences but also making possible reduction to the physical sciences. Humans in this worldview, denoted by the phrase "evolutionary humanism," made "man" a biological creature, one that set "him" apart from all others. That continued well into the second half of the twentieth century and expanded in scope. By the early 1960s, even George Ledyard Stebbins, a plant evolutionary biologist and architect of the synthesis but far removed from human evolution, began to chant the theme of "man" in a humanized evolution as he extended this unifying vision of the life sciences to improve the condition of "man" in his leadership of the International Biological Programme and the International Union of Biological Sciences (Stebbins 1962). It continued in courses of biology instruction all over the world, but it was best seen in the biological sciences and curriculum study textbook series "blue" version titled *Molecules to Man*. And it was used to ward off antievolutionist attacks that were galvanized by the very public success of evolutionary biology in 1959 (Smocovitis 1992, 1996, 1999). Evolution increasingly was thought to be the "central" science that unified biology and that then extended itself to the social sciences—as an entire generation of biologists followed Dobzhansky's famous dictum, "nothing in biology makes sense except in the light of evolution" (Dobzhansky 1964:449, 1973)—and into the social sciences and indeed into the humanities (D. S. Wilson 2010; E. O. Wilson 1975, 1998). It continued through the decades of the 1980s and 1990s even as challenges to the synthesis and the synthetic theory were mounted from within as the next generation of evolutionary biologists amended that framework to accommodate molecular evolution, paleobiology, and what resulted from the fusion of evolution, genetics, and development, the "new science of evo-devo." For evolutionary biologists, the synthesis continues to serve to varying extents as a narrative of origins, though that also begins with the figure of Charles Darwin, who should more appropriately be considered a naturalist (the word "evolution" does not appear in his *Origin*, and the term "evolve" appears only as the final word in the text).

From another direction, they were joined by a number of evolutionary anthropologists, paleoanthropologists, human geneticists, and newer biological anthropologists who grounded their claims in the synthesis and who adopted a similar narrative of origins for their own discipline variously termed the "new physical anthropology," "biological anthropology," "paleoanthropology," "evolutionary anthropology," and even "human biology" or "human evolutionary biology"; such was the "biocultural synthesis" that took place by the late 1950s grounded in "the modern synthesis" of the preceding two decades. The extent to which cultural anthropologists uphold, resist, or subvert that narrative remains a lively and at times heated topic for discussion as well as the extent to which biological anthropologists properly "unite" with evolutionary biologists today.[26] Though there are deep points of resonance and shared commitments to something termed "biology," biological anthropologists preserve some measure of autonomy through their own societies, publication venues, and appointments usually located in departments of anthropology or in medical schools and not in departments of biology. Journals such as *Evolution*, the primary publication venue for evolutionary biologists, still do not include significant or broad coverage of human evolution (including here human genetics or paleoanthropology), though these are of course within the intellectual purview of the journal.

As a category of scientific knowledge, moreover, biological anthropology (indeed "anthropology" itself) continues to make an intriguing distinction: it is the only such disciplinary category allying itself as part of the life sciences devoted to the study of one species. To drive home my point, let me ask, what would an equivalent biological scientific category devoted to fruit flies look like? (Would that be "drosophilology," or more specifically "*Drosophila melanogasterology*"?) And what are we to make of the fact that primatology, which boomed in the wake of Washburn's influence in the 1950s, is a category of scientific study devoted to primates but which not only excludes humans but is also a subset of the larger anthropology instead of the reverse, as biological logic would dictate? Clearly, the existence of anthropology in general and biological anthropology in particular in some measure still preserves the special or "unique" status given to humans despite the fact that humans are animals. From a proper biological perspective, this is of course profoundly anthropocentric, as we might expect of an area devoted exclusively to humans. Given this etymological confusion, should we perhaps rethink the meaning of the category of "anthropology," so heterogeneous that it has become the locus for interdisciplinarity, as something even more inclusive but amorphous, such as "human studies"? And what would then be the relationship to the age-old category of "the humanities"?

Finally, let me close this paper by stating that what it has offered is a kind of prehistoric originary narrative of the attempt to "humanize" evolution that was part of a growing area of study that came to be known as "biological anthropology." It remains unclear to me precisely when that term gained widespread currency or when precisely it began to

26. See, e.g., Lederman (2005), and see the collection edited by Segal and Yanagisako (2005) that "unwraps the sacred bundle," a phrase alluding to a paper by George Stocking titled "Guardians of the Sacred Bundle: The American Anthropological Association and the Representation of Holistic Anthropology" (Stocking 1988*b*). The "sacred bundle" refers to the traditional "four fields" or quadrants comprising American anthropology: archaeology, biological anthropology, sociocultural anthropology, and linguistic anthropology. See Silverman (2005) for explication on the history and organization of anthropology in the United States. For a critical backlash, see D'Andrade (2000).

serve as a disciplinary substitute for physical anthropology and for whom. It is for this reason that I deem my narrative prehistoric; it is that part of the story that remains unwritten, or uninscribed, as of yet. My sense is the introduction of something called "biological" anthropology followed shortly after or alongside the phrase "human biology" sometime in the late 1960s or 1970s and was adopted by those whose original training and whose primary allegiance remained with anthropology rather than biology, especially those working within departments of anthropology in America, at least. The narrative I offer here has also followed a postpositivist historiography that sees science as discourse and culture. In this view, the drive for unification and the need to create a mechanistic and materialistic narrative for human origins that accounted for the unity of life and the diversity of life was scripted by historians and philosophers of science upholding the Enlightenment project of the unity of knowledge with a positivistic worldview. The extent to which such historical scripts are written and rewritten and the extent to which they drive us to performance and tell us who we are remains a lively discussion topic for historians and philosophers as well as, of course, anthropologists.

Acknowledgments

I wish to thank Susan Gillespie, John Krigbaum, Mike Miyamoto, Connie Mulligan, Steve Phelps, and Marta Wayne, all at the University of Florida, as well as Matthew Goodrum, Maria Kronfeldner, and Tim White for useful discussions on the relationship between anthropology and biology, and I thank the participants at the "Ruster-fest" at Florida State University who heard an early draft of this paper. George Armelagos also read a draft of this paper and shared his abundant insights on biological anthropology and made suggestions for readings. Special thanks to Leslie Aiello, Susan Lindee, Laurie Obbink, Ricardo Santos, and all the participants of the 142nd Wenner-Gren symposium on "The Biological Anthropology of Modern Human Populations: World Histories, National Styles, and International Networks" for all their insights and their helpful comments during the workshop in Teresópolis, Brazil, in 2010.

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Human Population Biology in the Second Half of the Twentieth Century

by Michael A. Little

Human population biology can be identified as the biocultural study of living humans from evolutionary, historical, populational, developmental, biomedical, and anthropological perspectives. Biological anthropology really "came of age" during the second half of the twentieth century, after the end of World War II. Human population biology, as a subfield of biological anthropology, was a part of this "scientific maturation" of the discipline. Contributions to the postwar transformation of living population studies were (1) wartime studies of military personnel exposed to novel environments, (2) an increase in young academic professionals with new ideas, (3) a decrease in both racist (racialist) attitudes and interest in race typology, and (4) the explosion of research and literature on human biology and behavior.

Introduction

An early foundation of human population biology dates back to the first half of the twentieth century, with the work of Franz Boas (1858–1942) and Raymond Pearl (1879–1940). Boas, an anthropologist, conducted extensive and pioneering research on the body size of Native Americans, child growth, developmental plasticity, and population statistics (Little 2010). Pearl, a biologist, founded the journal *Human Biology* in 1929 and published widely in the areas of evolution, genetics, demography, biometry, nutrition, disease, growth, and senescence (Little and Garruto 2010). In 1940, Marcus Goldstein conducted a comparative survey of articles published in the journals *Human Biology* and the *American Journal of Physical Anthropology*. Both journals published papers by anthropologists and had anthropologists as members of their editorial boards. The principal difference that Goldstein (1940) observed was that *Human Biology* published papers that were more likely to deal with what he called "group" (population) studies, in contrast to papers in the *American Journal of Physical Anthropology*, which were more "anatomically" (and individually) oriented. This "population" orientation has distinguished the field of human biology up to the present.

The second half of the twentieth century saw the development of human population biology, within the field of physical anthropology, from a largely descriptive science with interests in typological treatment and classification of race to a hypothesis-driven science with a theoretical grounding in human evolution. These changes in direction took place after the end of World War II, but modern human biology and its counterpart developed during the war. The period immediately after the war in the late 1940s and early 1950s was marked by a revitalization of the profession of physical anthropology, including expanded studies of living populations incorporating body composition, child growth, nutrition, environmental physiology, epidemiology, and demography. The 1960s and early 1970s saw the maturation of human population biology and an increase in worldwide scientific exchange arising from the International Biological Programme (IBP) and its Human Adaptability (HA) component. Integrated multidisciplinary studies that were promoted during the IBP were continued marginally into the 1980s with the development of UNESCO's Man and the Biosphere (MAB) program. At the same time, genetics was being transformed from an analytical mode of "phenotypic inference" to a more sophisticated mode of "direct DNA" or "molecular genetics" analysis. New fields of investigation from the 1980s through the end of the century included reproductive ecology, behavioral evolution, Darwinian medicine, psycho-neuro-physiological stress, and biomedical and health research. The historical period to be emphasized in this paper is the formative and maturational years of human population biology from the 1950s through the early 1980s, although some background leading up to and including World War II is necessary for context.

Human Biology during World War II

It is difficult to exaggerate the magnitude of World War II and its devastating effects on people around the globe. There are estimates that as many as 50 million individuals died as a result of the conflict, and the stress experienced in war zones

Michael A. Little is Distinguished Professor of Anthropology at the State University of New York, Binghamton (P.O. Box 6000, Binghamton, New York 13902-6000, U.S.A. [mlittle@binghamton.edu]). This paper was submitted 27 X 10, accepted 23 VIII 11, and electronically published 15 II 12.

 0011-3204/2012/53S5-0011$10.00. DOI: 10.1086/662440

by military and civilian survivors alike must have been overwhelming. In the United States, more than 16 million military personnel participated in the war between December 1941 and December 1946. Of these, nearly 12 million men and women were deployed overseas in East Asia, Southeast Asia, the Mediterranean, North Africa, Europe, the North American Arctic, and the Pacific (U.S. Census Bureau 2003). The war effort stimulated substantial research and development—some basic, most applied—to deal with health, nutrition, climate, disease, work requirements, and other modes of stress experienced by GIs living under the often alien, unsanitary, dehumanizing, and life-threatening conditions of active warfare. Much of the research was highly practical, as in the development of the antimalarial drug chloroquine, during the war. This research was stimulated by the fear that supplies of quinine might be cut off from Java (Slater 2009:109). Not only were diseases suffered by GIs serious threats to health, but by debilitating military personnel, they also jeopardized the success of military campaigns. Malaria, as one of the most serious diseases of the tropics, was particularly problematic in the Pacific theater, where it contributed to about 178,000 hospital admissions between 1942 and 1945 (Smallman-Raynor and Cliff 2004:596).

Research on disease and illness was largely conducted by medical personnel. Other research was conducted in the United States to address specific problems faced by troops living and fighting in diverse environments. In the developing field of environmental physiology, tests were designed in the laboratory, and then practical field experiments were conducted under naturalistic conditions. This coordinated application of laboratory and field science became a common practice in postwar studies of human biology. Anthropological data were drawn on to explore what were called "folk adaptations to climate" in peoples native to areas of climatic extremes (Wulsin 1949). Two areas of applied research were related to the climatic extremes to which military personnel were exposed: high environmental temperatures (both hot-dry and hot-wet conditions) and low environmental temperatures (principally arctic and alpine conditions).

United States forces began fighting in the North African military theater in May 1942, although British and British Commonwealth (Australian, New Zealand, and Indian) soldiers had begun to fight German and Italian forces a year and a half earlier. Fighting in the Libyan and western Egyptian desert areas stimulated American studies that were initiated shortly after the Pearl Harbor attack. One major study of hot-dry desert conditions was conducted during the war by a group of physiologists from the University of Rochester (Rochester Desert Unit; Adolph 1947). The basic study was centered at various sites, while the desert studies were carried out on military personnel on maneuvers in Southern California between 1942 and 1945. The research team was charged with determining water and food requirements, sweat rates, energy balance, heat tolerance, work capacity, and survival rates while soldiers were living and working under desert conditions. Other studies considered how long men could survive without water under a variety of conditions, including men on life rafts at sea. A sobering result of some of these studies was a series of maps of the world's oceans identifying estimated survival time for men on life rafts without water by season and location (Adolph 1947:311–314). A part of this research also was done under the hot-wet conditions of southern Florida to simulate troops working in tropical rain forests. This wartime research would later be identified as the foremost U.S. work in climatic-stress physiology and one that would serve as the basis for later work by anthropologists and human biologists on native populations.

Cold stress was also studied extensively in the United States and the United Kingdom during the war (Burton and Edholm 1955; Carlson 1954), again principally by environmental physiologists. There were, or course, comparable studies of cold tolerance conducted by Germany and Japan, some of which were legitimate while other research violated human ethical principles. Much of this considerable research by the United States and the United Kingdom was buried in government reports, but a great deal was documented in the open journal literature. Interests focused on conditions among ground troops exposed to cold temperatures during winter, at high latitudes, and at high altitudes. The air force was interested in airmen who had to bail out at extreme altitudes with high wind chill or those who were exposed to cold seawater. There were several practical health concerns associated with cold exposure: first, acute exposure and frostbite leading to severe tissue damage; second, chronic tissue damage produced by prolonged exposure to temperatures slightly above freezing (e.g., trench foot); and third, hypothermia, or the drop in body temperature to dangerous levels from loss of body heat content. Cold tests on unclothed men and on men when they were wearing different kinds of protective clothing were conducted at the U.S. Army Climatic Research Laboratory in Lawrence, Massachusetts, and other government research laboratories (Belding et al. 1947; Blaisdell 1951; Siple 1949). The postwar interest among anthropologists in cold stress and adaptation was built on these early World War II and postwar studies.

Diet and nutrition were of direct interest to the U.S. Army Quartermaster Corps because of its responsibility for feeding the troops. Energy and nutrient requirements were established under a variety of stressful conditions, including intense work activity, high-altitude residence, and extreme thermal environments (hot-wet and hot-dry tropics and cold; Mitchell and Edman 1951). King and Salthe (1946) surveyed the contributions to nutritional sciences that were made during World War II that included (1) recognition of the need for nutritional training among physicians, (2) improved methods of appraising nutritional status, and (3) an increased understanding of the relationships between good nutrition and resistance to infection, enhanced tolerance to cold and altitude, and improved night vision. "The attention given to nutrition in World War II by the Office of the [U.S.] Quartermaster General and the Office of the Surgeon General stands in sharp contrast to experiences during World War I" (King and Salthe 1946:882).

Hunger and starvation were common among civilians in war zones and military personnel who were cut off from supply lines. With this need for greater understanding, an experimental study on nutrition and starvation was initiated at the School of Public Health at the University of Minnesota during the last year of the war. The Minnesota semistarvation study was headed by Ancel Keys (1904–2004), a physiologist with interests in metabolism and nutrition and who was best known for having developed K rations during the early part of the war. Other principal scientists on the project were Josef Brožek (1913–2004), a psychologist with interests in body composition, Austin Henschel (1907–1991), a physiologist interested in temperature regulation, Olaf Mickelsen (1912–1999), a chemist and nutritional scientist, and Henry Longstreet Taylor (1912–1983), an exercise physiologist. The study began in November 1944 with 36 conscientious objectors who volunteered for the 11-month investigation in which the subjects were on a normal control diet for 12 weeks, a 24-week semistarvation diet (1,500 kcal/day), and then a restricted rehabilitation diet for 12 weeks. As an additional means of energy expenditure, the volunteers were expected to maintain moderate activity by walking 22 miles per week. The study was intensive, with almost daily biochemical, physiological, psychological, and body-composition measurements taken. Average body weight in the men dropped from 69 to 53 kg during the 24-month period, a loss of close to 25%. The results of the study were published in two massive volumes that stand as state-of-the-art research even today, particularly because such a high-risk treatment of human subjects would probably not be permitted during present times (Keys et al. 1950).

Body composition (adipose- and lean-tissue composition) changed in the subjects during the 24-week semistarvation diet of the Minnesota study as a result of the negative balance between food energy intake and energy expenditure (principally basal metabolic rate + activity). Before this time, interest in body composition among physical anthropologists had been largely confined to traditional skeletal analyses of hard tissue, directly through postmortem osteometrics or indirectly by use of x-ray images or anthropometric measurements of the living. These new interests in soft-tissue body-composition variation and associations with physical activity, nutrition, climatic stress, and disease and epidemiology began to be pursued at the basic research level after the end of the war. At the same time, some anthropologists participated in ongoing applied and basic military research that continued into the 1950s during the Cold War.

International Scientific Exchange and the Cold War

The war had virtually halted international exchange in scientific research other than applied wartime efforts by each of the opposing sides of the conflict. During the 1920s and 1930s, internationalism in science was on the rise, particularly among elite members of national academies. Contacts were common between American scientists and those abroad, notably from the United Kingdom, Germany, and France. Many scientists read journals in languages other than their own native tongues, international congresses brought scientists together from time to time, and correspondence by mail was extensive. For example, the International Council of Scientific Unions (ICSU; now the International Council for Science), now the largest world scientific body, was founded in 1931, and the International Congress of Anthropological and Ethnological Sciences had its first meeting in 1934. Raymond Pearl's editorial board for the journal *Human Biology*, founded in 1929, had 16 members: seven from the United States and nine from England, France, Germany, Italy, Sweden, and Japan. Seven members of the board were anthropologists (Little and Garruto 2010).

Following World War II, limits to international scientific exchange took a new tack in the context of the East-West conflict, or Cold War. Science became integrated with national foreign policies, and there were restrictions on international travel and exchange. Many of the restrictions were centered on the physical, nuclear, and space sciences as well as on mathematics and technology, but the social and biological sciences were affected as well. On the other hand, in the United States, competition with the Soviet Union led to strong governmental support through funding for science in general, including the social and biological sciences (Shindell 2010). As described below, internationalism in human population biology was revived and stimulated with ICSU's IBP, which was launched in the 1960s.

The Period Immediately after World War II

Few students completed advanced degrees during World War II, and most of the prewar PhDs were either in the military service or teaching during the war. Immediately after the war, Sherwood L. Washburn (1911–2000), who had been teaching anatomy at Columbia University, along with his junior colleague Gabriel W. Lasker (1912–2002), began organizing summer seminars in physical anthropology. Lasker, who had been a conscientious objector during the war, had just begun teaching anatomy at Wayne State University in 1946. These summer seminar meetings were held in New York City annually throughout the late 1940s and were sponsored by the Viking Fund (later to become the Wenner-Gren Foundation for Anthropological Research). They were intellectually exciting gatherings of young and older professional anthropologists who were being exposed to the newer ideas of physical anthropology. These newer ideas were based on evolutionary theory and scientific hypothesis testing rather than the racial typologizing and preoccupation with descriptive anthropometric measurements that had been in vogue before the war (Little and Kaplan 2010).

Although Washburn had limited interest in human biology, he nevertheless contributed substantially to the transformation of this subfield of physical anthropology by the force of his personality and the appeal of his ideas. In "The New Physical Anthropology" (1951), he drew on the innovative exchanges that took place during the summer seminars, exchanges that were largely derived from his own ideas and those of younger members of the profession. Washburn's interests in evolutionary theory and adaptation to the environment were becoming part of an important framework in physical anthropology in the United States (Birdsell 1953; Coon, Garn, and Birdsell 1950; Newman 1953).

Physical anthropology and human biology research were promoted directly after the war by the Viking Fund (founded in 1941; Dodds 1973). During the years after the war, most anthropological research was sponsored by private foundations (Baker and Eveleth 1982). The Viking Fund, under the direction of Paul Fejos (1897–1963; fig. 1), was one of the most important of these private foundations and the only one devoted entirely to anthropology. Through the Viking Fund, Fejos promoted intellectual communication and contributed to the growth of anthropology and its biological component in a variety of essential ways after the war: biweekly supper conferences were held at its New York City headquarters, several of which were devoted to biological anthropology (Szathmáry 1991); the summer seminars were held annually from 1946 to 1955; the *Yearbook of Physical Anthropology* was established in 1946 and supported for many years; the watershed Cold Spring Harbor Symposium XV in 1950 was jointly sponsored by the Viking Fund and the Carnegie Corporation.

Figure 1. Paul Fejos, director of the Viking Fund, later named the Wenner-Gren Foundation for Anthropological Research (photo with the permission of the Wenner-Gren Foundation for Anthropological Research).

Figure 2. Joseph S. Weiner, International Convener, Human Adaptability Section of the International Biological Programme (photo with the permission of Edmund Weiner).

At the same time, in the United Kingdom a dominant influence was provided by Joseph S. Weiner (1915–1982; fig. 2). Weiner was a South African trained in physiology, anatomy, and anthropology who came to the United Kingdom in 1937. During the war he worked with the Medical Research Council, and in 1945 he was hired at Oxford University by Wilfrid E. Le Gros Clark (1895–1971) to fill a readership in physical anthropology. In 1963, he moved to the London School of Hygiene and Tropical Medicine to direct the Medical Research Council Environmental Physiology Unit, and two years later he was appointed Professor of Environmental Physiology at the University of London. According to Derek Roberts (1997:1108), "He saw human communities as dynamic functional entities displaying adaptive responses to the demands and stresses of the environment, as well as of day-to-day events. . . . As such, Weiner is considered to have played a major role in establishing the intellectual contours of the discipline during the second half of the twentieth century." Another important and influential anthropologist/human biologist from this postwar era was Nigel Barnicot (1914–1975) of University College London. Although he was trained in physiology and zoology, he was housed in the anthropology department. His research was broadly based, with studies of nonhuman primates, blood-group genetics, hair and skin color in West African populations, and the human biology of Hadza hunter-gatherers in East Africa (Sunderland 1975). Harrison (1982) has identified Weiner and Barnicot as the two most influential physical anthropologists from Britain conducting research and training students in human population biology during the postwar era.

The 1950s as a Transformational Era

By the early to mid-1950s, there was an active group of young and midcareer scientists who were interested in human population biology and were trained other areas as well as in anthropology. Although not described as such, there was a "new human biology" developing in part out of physical anthropology but also drawing on other sciences. Of the three giants in physical anthropology—Franz Boas, Aleš Hrdlička (1869–1943), and Earnest A. Hooton (1887–1954)—only Hooton had lived into the early 1950s. Among Hooton's last students from the 1940s and 1950s were a number of individuals who contributed to the scientific modernization of human population biology. Alice Brues (1913–2007) and James Spuhler (1912–1992) worked in genetics; Marshall T. Newman (1911–1994) and Joseph B. Birdsell (1908–1994) conducted studies in population, biogeography, and climate in North America and Australia, respectively; Gabriel W. Lasker used Boas's model to explore plasticity with Chinese migrants; William S. Laughlin (1919–2001) studied Aleut populations; Stanley M. Garn (1922–2007) worked in areas of growth and body composition; Edward E. Hunt Jr. (1922–1991) worked with Micronesian populations; and Paul T. Baker (1927–2007) conducted research on desert heat stress.

Although the vast majority of new students were being trained at Harvard by Hooton and then, after Hooton's death in 1954, by William W. Howells, some biologically oriented anthropologists were beginning to be trained elsewhere. Building on Anthony Allison's (1954) discovery of the evolutionary association between the sickle-cell polymorphism and malaria, Frank B. Livingstone (1958) demonstrated relationships among patterns of subsistence, mosquito populations, malaria, and changing frequencies of the sickle-cell gene in Liberia, West Africa. Livingstone was trained at the University of Michigan, which had built one of the strongest human genetics programs in the United States.

Russell W. Newman was a Berkeley-trained anthropologist. After his doctorate in 1949 and a year teaching at the University of Oklahoma, Newman spent his entire career as a civilian conducting research for the U.S. Army Quartermaster Corps (Baker 1981). He worked as an anthropometrist for clothing and equipment design and the identification of Korean War dead. However, his major interests were in environmental physiology and medicine, on which he conducted research at the Climatic Research Laboratories in Natick, Massachusetts (Newman 1970; Newman and Munro 1955).

Interest in human adaptation to climatic extremes was widely pursued in both the United States and the United Kingdom during the 1950s and 1960s, partly as a carryover from military research during World War II. A military center that served as a training ground for both military and civilian physiologists and anthropologists who were interested in human adaptation to the environment was the U.S. Army Soldier Systems Center at Natick (commonly known as the Natick Labs; fig. 3). The Natick Labs were constructed throughout the early to mid-1950s and eventually constituted a research complex of more than 100 buildings (Earls 2005). The physical anthropology branch of the Quartermaster Research and Development Command conducted research on clothing and equipment design (Baker 1957; fig. 4), skeletal analysis for the identification of war dead (Baker and Newman 1957), and cold- and heat-stress studies of physiology and body morphology (Baker 1958; Daniels and Baker 1961). This was a period in which the traditional skills of the physical anthropologist were being transformed from skeletal description and analysis to morphology, body composition, physiology, and the relationships of these variables to environmental adaptation.

Studies of body composition were stimulated by a conference that was organized at the Natick Labs in early 1959. The conference was organized by Josef Brožek and Austin Henschel (1961), two of the coinvestigators in the World War II Minnesota semistarvation study. Several anthropologists, all of whom had been Hooton's students, participated in the conference (Baker 1961; Garn 1961; Hunt 1961), as did other scientists whose research contributed to anthropological interests. Some of the research interests presented at the conference were (1) prediction of weight from height and several bone diameters (useful for forensic analysis and archaeology), (2) correlations of body composition with physique or somatotype, (3) quantitative appraisal of body photographs (photogrammetric evaluation), (4) techniques for estimating percent body fat and lean body mass (skinfolds, anthropometry, underwater weighing [specific gravity determination], radiographic measures), (5) the relationship between bone density/mineral content and muscle size, (6) the relationship between body composition and basal metabolic rate, and (7) how changes in nutritional status influence body composition (fat, water, muscle loss).

At the same time that body-composition research was expanding, so was the extension of anthropometry into new realms of body measurement. In 1955, Earl W. Count (1899–1996) organized a series of contributions for a New York Academy of Sciences conference on dynamic anthropometry (Miner 1955). Anthropometry (measurement of the living) had been the "time-honored" method of physical anthropology embraced by Hrdlička before the war and then later rejected by Sherwood Washburn in his 1951 postwar treatise

Figure 3. Entrance to the Natick, Massachusetts, Laboratories in 1956 (U.S. government document).

on the "new physical anthropology." These new applications of anthropometric methods incorporated hypothesis-driven studies of child growth (in the tradition of Boas), body composition, allometry, biomechanics, and functional morphology. Anthropometry had come full circle and returned to studies of human biological variation as a legitimate method of data collection.

Human genetics and evolution studies were on the rise, thanks in part to collaborative conferences that included anthropologists and geneticists as coparticipants. The decade began with Washburn's coorganization of the 1950 joint anthropology-genetics symposium with the distinguished geneticist Theodosius Dobzhansky (1900–1975). This Cold Spring Harbor Symposium on Quantitative Biology was a watershed conference that previewed some of the newer interests in human population biology (Warren 1951). More than 25 anthropologists and an equal number of geneticists and evolutionary biologists participated in this conference. The end of the decade was marked by the centennial of the publication of Charles Darwin's *Origin of Species* (1859). In celebration of this event, the twenty-fourth Cold Spring Harbor Symposium was organized by Dobzhansky, along with several other evolutionary geneticists and the anthropologist Carleton S. Coon (1904–1981; Wooldridge 1960). Only about a half-dozen anthropologists participated in this conference, but the array of distinguished evolutionists who attended was truly remarkable. In the fall of that same year, Sol Tax (1907–1995), a cultural anthropologist from the University of Chicago, organized a major conference for the Darwin centennial (Smocovitis 1999). The conference included notables from all major sciences allied with evolution, including animal behavior, anthropology, astronomy, biology, chemistry, genetics, paleontology, physiology, and psychology (Tax 1960). Anthropologists from each of the subfields were major participants in the conference, which was a tribute to both Tax's planning and a reflection of the positive status of evolution within the science of anthropology at that time.

The 1960s: Ecology, Adaptability, and the IBP

The decade of the 1960s was marked by an event unusual in the history of anthropology: three of its subfields—cultural anthropology, archaeology, and biological anthropology—were unified under a similar theoretical paradigm. Or at least there was unification among some practitioners of the three subfields. The paradigm included the application of the scientific method, materialism, adaptation, and ecological approaches to inquiry. Julian Steward moved cultural ecology a step forward by rejecting the "fruitless assumption that culture comes from culture" (Steward 1955:36). He also developed the concept of "culture core" as the behavior patterns most closely linked to the environment (e.g., subsistence and food acquisition). Archaeologists were also influenced by Steward's ideas on multilinear evolution. At the same time, ecological theory in biological anthropology became a fusion of evolutionary and ecological theory (Bates 1953, 1960), along with ideas from environmental physiology (Dill, Adolph, and Wilber 1964), human biogeography (Coon, Garn, and Birdsell 1950), demography (Spuhler 1959), and human biology (Baker 1962). Building on Steward's ideas, Vayda and Rappaport (1968) called for a unified science of human ecology to be studied with the population rather than culture as the

Figure 4. Paul T. Baker in experimental white polar clothing as part of U.S. Quartermaster Corps arctic testing in Fort Churchill, Canada, in the 1950s (photo with the permission of Thelma S. Baker).

unit of study. Both adaptational and demographic perspectives were encouraged in this model. The authors concluded with the statement that for a unified science of ecology it might be required to sacrifice the autonomy of a science of culture but that the broader scope of applications could be worth the sacrifice: "A unified science of ecology has definite contributions to make towards the realization of anthropological goals and does not entail any appreciable sacrifice of traditional anthropological interests" (Vayda and Rappaport 1968:497).

Many of the ideas employed to develop an "ecological anthropology" were derived from the biological sciences, including ideas from ecology and evolution that were incorporated by Steward (1955) and Leslie White (1949). During the late 1950s and early 1960s, there was a heightened concern with anthropogenic effects on the environment that coincided with the postwar rise in science and technology. Scientists were becoming aware of the destructive influence of humans on the biosphere, and there was a rising interest in the ecology of the planet.

It was in this setting that the IBP was established. The IBP was launched by the major world scientific body the ICSU and based on the model of the International Geophysical Year (IGY) that had begun in 1957 (Worthington 1975). The IGY had dealt with the geosphere, whereas the IBP was to deal with the biosphere. Discussion of the IBP began in 1959, but objectives were undecided until the May 1962 meeting of the IBP planning committee at Morges, Switzerland. The basic objective of the IBP was identified as understanding "the biological basis of productivity and human welfare in the context of global ecological problems" (Weiner 1977:1). The term "problem" was viewed in the scientific sense of "a problem to be explored" and also in the practical sense of "a problem that would cause difficulty for humans." Hence, basic research on ecological problems could be conducted to solve applied problems affecting human welfare. This ambiguity helped to justify the expenditure of substantial research funds, which were much more than had been applied to biological research in the past.

Joseph S. Weiner, who was later to be appointed as international convener (director) of the HA section of the IBP, proposed a program of research for human studies, which suggested

> that a world-wide ecological programme concerned with human physiological, developmental, morphological and genetic adaptability was feasible and timely and would form a fitting counterpart to the remainder of the IBP [HA]. . . . The IBP required that living human populations be investigated as functioning entities interacting with a large variety of habitats and therefore be understood in adaptive and selective [evolutionary] terms. (Weiner 1977:2)

Emphasis was placed on integrated approaches, methods drawn from a variety of disciplines, and application of the comparative study of human populations drawn from a variety of ecological settings. Aside from its fundamental research value, comparative analyses also allowed Third World nations with limited resources to participate in the international program by contributing basic data that could be used for combined analyses.

The Paris IBP assembly, on July 23–25, 1964, established the three phases of the IBP decade (1964–1974) and the tasks to be completed during the "preoperational phase" (1964–1967). These were (1) establishing national HA committees, (2) full international discussion of HA proposals, (3) agreement on methods and procedures, (4) encouraging pilot and feasibility studies, (5) developing training programs, (6) establishing data and coordinating centers, and (7) securing more precise proposals for research by nations or groups of nations. At the end of this preoperational phase in 1967, there were 45 nations participating in the HA section of the IBP. The second and third phases included a 5-year "operational" or "research phase" from 1967 to 1972 and a "synthesis phase" from 1972 to 1974. Four categories of proposed HA research were agreed on at the 1964 Paris assembly (Weiner 1965, 1966):

1. Survey of sample populations in conformity with a world scheme. This category included rapid surveys of populations over a wide geographical range to identify gaps in our knowledge, including surveys of gene frequencies of polymorphic systems and surveys of growth and physique. One of the most successful comparative projects was of child growth.

2. Intensive multidisciplinary regional studies based on habitat contrasts. This category included single-population studies with an array of biological, demographic, and social data to be collected. Some of the most successful projects were conducted under this category.

3. Special investigations on selected populations. This category included studies of specific populations based on interesting problems, including physiological fitness, disease and selection, sociodemographic factors, population dynamics and reproduction, and special nutritional problems. Some of these projects included opportunistic or natural experimental studies of populations with special or unique properties and conditions (see Garruto et al. 1999).

4. Investigations related to current WHO (World Health Organization) activities. Suggested topics included blood-pressure surveys, hematological data, antibody levels, blood constituents, and congenital defects. The WHO was interested in child development, occupational health, cardiovascular fitness, and other topics, and it sponsored collaborative IBP research and organized and supported scientific meetings to promote its health interests.

With these HA objectives, the human component fit squarely into IBP objectives.

A Wenner-Gren Foundation conference was held at Burg Wartenstein in Austria from June 29 to July 12, 1964, immediately before the Paris IBP assembly. Organized by Joseph S. Weiner with the assistance of Wenner-Gren director of research Lita Fejos Osmundsen, the purpose of the conference was to bring a group of about 20 human biologists from all over the world to participate in the global planning for the HA research. One of the products of the meeting served as a framework for HA research that Weiner presented at the Paris assembly later that month. Another product was an important state-of-knowledge volume titled *The Biology of Human Adaptability* (Baker and Weiner 1966). In the first chapter of this volume, Weiner (1966) discussed and justified the four categories of HA research in considerable detail, while other chapters dealt with methods, research design, child growth, and state-of-knowledge research reviews on Africa, South America, Asia, Australia, the Pacific, and both circumpolar and high-altitude zones. Attendance at this planning conference was international, with an assemblage of truly distinguished leaders in human biology.

The United Kingdom was represented by Weiner, Geoffrey A. Harrison, and L. G. C. Pugh, who discussed high-altitude studies; James M. Tanner, who proposed the HA growth studies; and O. G. Edholm, who designed an in-migration project for Israel. Jean Hiernaux, from Belgium, and Phillip V. Tobias and Cyril Wyndham, from South Africa, focused on African populations, including diverse environments, genetics, and adaptation to exercise and temperature. Francisco M. Salzano, from Brazil, and James V. Neel and Paul T. Baker, from the United States, dealt with Native Americans from South America from both genetic and physiological perspectives. Arctic populations were represented for both genetic and anthropological characteristics by William S. Laughlin, from the United States, and for health by J. A. Hildes, from Canada. M. S. Malhotra and L. D. Sanghvi surveyed the genetics, physical fitness, nutrition, and health of Indian populations. Finally, R. L. Kirk and R. K. Macpherson, both from Australia, focused on the human population biology of Australian and New Guinea peoples.

Other important HA documents that were developed during this period included a field methods manual by Weiner and Lourie (1969), a Japanese symposium proceedings on methodology (Yoshimura and Weiner 1966), an Indian symposium proceedings on physical fitness (Malhotra 1965), and proceedings of a Warsaw conference on problems in human adaptability held in 1965 (Dzierżykray-Rogalski 1968). Conferences were held in many parts of the world to define regional population research problems and to assist national committees in identifying the scope of their HA programs.

During the course of his travels around the world to promote HA research, Joseph Weiner would often encourage members of national academies to participate in the IBP because of its importance in providing a comparative base of data and the need for international collaboration. Moreover, he would often use the concept of East-West competition to persuade U.S. policy makers to fund HA research and then make the same arguments with Soviet policy makers.

The scientific output from this worldwide research effort from the HA section was several thousand published works, works that were based on more than 230 research projects (many of them multiyear projects) and included data from human communities in 90 nations (Collins and Weiner 1977). In addition to national compendiums and syntheses, several synthesis volumes were published to represent the comparative international research. They included volumes on circumpolar populations (Milan 1980), high-altitude residents (Baker 1978), human growth variation (Eveleth and Tanner 1976, 1990), human physiological work capacity (Shephard 1978), and population structure (Harrison 1977).

IBP Research in the United States

At the international level, there was a concern during the early planning that the program would be a success only if the United States participated fully in the IBP. Active U.S. involvement was strongly encouraged and supported by one of the key figures in early planning, G. Ledyard Stebbins, the distinguished American evolutionary biologist (Weiner 1977). Joseph Weiner was also instrumental in encouraging U.S. participation in the HA component of the IBP and encouraging financial support by exploiting the scientific competition between the Soviet Union and the United States during the Cold War. Weiner, as HA convener, spent considerable effort traveling around the world to lobby and advise the scientific community about the advantages and benefits of participation in the HA effort.

By 1967, the beginning of the "operational" or "research phase" of the IBP, the United States had a well-organized structure of committees and subcommittees under the National Academy of Sciences and the National Research Council (U.S. National Committee 1967). Anthropologists, although not heavily represented, were nevertheless players in the early IBP planning. William S. Laughlin, an anthropologist who had worked with Aleut populations, and Dimitri B. Shimkin, an anthropologist who did some work in public health, were members of the U.S. National Committee for the IBP, while three anthropologists—Laughlin, Shimkin, and Paul T. Baker—were members of the 12-person HA subcommittee. Although a number of research projects were ongoing at that time, considerable effort and expenditures were devoted to planning and information-exchange conferences, both national and international. The 10 other subcommittees or panels included a variety of ecological research categories (e.g., productivity of marine, freshwater, and terrestrial communities; systematics and biogeography; and biometeorology). The United States modified its organizational structure from the international guidelines and identified two principal research components: an Environmental Management (EM) component that included the largely ecological projects, and a much smaller HA component that focused on human population projects.

Between 1967 and 1970 (U.S. National Committee 1971, 1974), the United States fully modified its program to include two main categories of research within each of the main components (EM and HA): "integrated research programs" (tightly and centrally organized research projects with integrated goals) and "coordinated research programs" (associations of related research projects with a principal objective of rapid sharing of information). At this time, the integrated research program that was centered on the analysis of ecosystems had become the dominant aspect of the U.S. IBP effort. Within this broad category, there were several "biome" or "ecosystem" programs, including the Grasslands, Deciduous Forest, Coniferous Forest, Desert, Tundra, and Tropical Forest biomes. These projects were multidisciplinary and multi-institutional and applied systems analysis and mathematical modeling to the analyses of these complex biomes. The biome projects also drew the greater part of the roughly $50 million expended on the U.S. IBP research during the period from 1967 to 1974 (in 1970, the U.S. Congress established a line-item [earmarked] fund for IBP research).

As noted, HA projects also fell under the integrated and coordinated research programs. There were three HA integrated research projects: (1) International Studies of Circumpolar Peoples, jointly led by William S. Laughlin (Aleuts) and Frederick A. Milan (Alaskan Eskimos); (2) Population Genetics of the American Indian, led by James V. Neel; and (3) Biology of Human Populations at High Altitudes, led by Paul T. Baker (Hanna, Friedman, and Baker 1972; U.S. National Committee 1974). The U.S. circumpolar Alaskan research was part of an international four-nation effort that included Canadian research at Igloolik and Danish and French research at Upernavik, Greenland. The American Indian research focused on South American tropical lowland populations who lived along the tributaries of the Amazon and Orinoco rivers in Brazil and Venezuela. Scientists from both these South American countries were collaborators on the research. The high-altitude research was carried out on the Peruvian altiplano at elevations between 4,000 and 5,000 m above sea level, in this case with Peruvian physiologists and anthropologists as collaborators. Although the three projects were U.S. based and both multidisciplinary and multinational, the fundamental approaches were of human population biology—the circumpolar project was concerned with Eskimo health and the effects of modernization; the South American tropical-native project focused on population genetics, demography, and evolution; and the high-altitude study concentrated on patterns of adaptation to hypoxia and cold. Two of the three projects were reported on in synthesis volumes (Baker and Little 1976; Jamison, Zegura, and Milan 1978), whereas all three of the projects contributed extensively to the scientific literature.

There were two HA-coordinated research projects: (1) Nutritional Adaptation to the Environment, headed by C. Glen King; and (2) Biosocial Adaptations of Migrant and Urban Populations, directed by Everett S. Lee. Both projects were loosely affiliated collections of research projects. Other HA-affiliated projects included research in New Guinea, the eastern Mediterranean, Hawaii, Southeast Asia, the Solomon Islands, and the Middle East.

An HA coordinating office for U.S. IBP/HA research was established at Pennsylvania State University from 1971 to 1975. During this period, three scientific coordinators were appointed, each for a year, with responsibilities to organize conferences, communicate research results, and assist research programs throughout the United States in organization and implementation. A biannual *Human Adaptability Newsletter*—written and edited by Sharon M. Friedman, a science writer and journalist—was also published and distributed widely during this 4-year period.

Strengths and Shortcomings of the IBP and HA Research

A positive outcome of the IBP ecosystem studies was the success of large-scale multidisciplinary research and the development of mathematical models of complex systems that could be used to characterize and simulate change in ecosystems. Attempts to use systems analysis in HA studies (e.g., carbon flow in the Aleutian Islands and energy flow in the Andes) were less well developed but still useful in characterizing the environments of the populations under study (Little et al. 1990). The multidisciplinary approach taken by the HA integrated research projects proved to be successful and established a trend that continued for several years (Little, Leslie, and Baker 1997). Another important contribution of the HA research was in the establishment of baseline information on human variation for vast numbers of populations around the world. With this information, it was possible to measure change and to chart adaptive processes in these populations as they encountered changing technology, new disease conditions, and acculturation. Perhaps the most lasting contribution to science in the United States was the training of hundreds of students through PhDs as the result of the funding of IBP programs. Just as a rough estimate, there were probably 50 PhDs trained in human biology and anthropology through HA projects between 1967 and 1975. Geoffrey Harrison (1997:19) noted that "during the course of the IBP it became increasingly recognized that more fundamental [than simple population comparisons] was the study of variability within populations. It is the operation of processes within populations that ultimately determines what the differences between populations will be."

A principal omission in the HA studies was the limited involvement of social scientists interested in social and cultural processes of adaptability (Ulijaszek 1997). Because most of the research was organized and implemented by human biologists, there was a tendency to exclude cultural anthropologists, who were less receptive to materialist ideas that included adaptive and evolutionary processes. But also, because most of the HA research was geared toward team efforts and multidisciplinary approaches, the anthropologists with a tradition of working alone were less easily incorporated into this kind of integrated research. Attempts to bring systems ecologists, social scientists, and human biologists together to plan collaborative studies were dismal failures, partly because of differences in interests that related to scale, boundaries, and conceptual frameworks but also because not enough time was spent together, so scientists from different fields could not begin in-depth communication and begin to understand their colleagues' differing perspectives. Sociocultural anthropologists believed that ecologists were simplifying human behavior, and ecologists believed that incorporating humans into their mathematical models of "natural" systems would overly complicate them. Both of these opinions were correct, but neither group was willing to put in the effort to manage and resolve these differences. In many cases, granting agencies are willing to support multidisciplinary research but are unwilling to provide the funds necessary to support ongoing meetings and conferences to enrich the collaborative learning process.

On balance, the IBP and its HA component contributed substantially to the growth and maturation of the field of human population biology. To quote Harrison (1997:25), "Notwithstanding its limitations it [IPB and HA research] played a major part in converting the old defunct physical anthropology into the vibrant and exciting component of biological anthropology as it is today."

Multidisciplinary Studies in the 1970s and 1980s

Attempts were made to maintain the momentum of the HA research and its international flavor of collaboration and exchange beyond the years of the IBP, but without much success. UNESCO's MAB program, which was first conceived in the late 1960s, was viewed hopefully by human biologists as a program to which the IBP HA projects might be transferred and continued (Weiner 1977). Its primary objective was established at its first Paris meeting "to identify and assess the changes in the biosphere resulting from man's activities and the effects of these changes on man" (UNESCO 1972). This broad mandate had the potential to incorporate some of the ecologically oriented multidisciplinary studies that characterized HA. However, aside from some early planning (Baker 1977), the program never incorporated human biologists into much of its research. The MAB program was not fully funded in the United States, and the withdrawal of the United States from UNESCO in 1984 limited U.S. participation. Later, the U.S. National Committee for MAB was placed under the U.S. State Department, and the program was redirected toward conservation, the development of biosphere reserves, and the loss of biodiversity on a global scale.

There were a number of integrated and multidisciplinary projects organized in the late 1970s and 1980s that maintained some of the perspectives and goals of the IBP and addressed some of the HA shortcomings (Little, Leslie, and Baker 1997). Steegmann (1983) organized a study of Canadian Algonkian peoples to explore their arctic adaptations to the boreal forest environment. A further investigation of the genetics and environment of high-altitude Andeans along an altitudinal gradient in Chile was initiated in the 1970s (Schull and Rothhammer 1990). Baker, Hanna, and Baker (1986) launched the Samoan Migrant Project to study the effects of modernization on health in a number of Samoan populations (Samoa, American Samoa, Hawaii, and California). Genetics and modernization were explored in Black Carib or Garifuna peoples of Belize, Guatemala, and Honduras, where there was a successful collaboration between human biologists and social scientists (Crawford 1984).

The South Turkana Ecosystem Project, which dealt with the adaptation of northwest Kenya pastoral nomads to the arid savanna, was initiated in the late 1970s and continued through the late 1990s (Coughenour et al. 1985; Dyson-Hudson and McCabe 1985). The research attempted to address the criticisms leveled against the IBP/HA projects by establishing fully collaborative partnerships among rangeland ecologists, human biologists, and sociocultural anthropologists. The project continued for nearly two decades, providing longitudinal data on climate, seasonal variability, ecosystem changes, livestock productivity, human health, human and livestock fertility, diet and nutrition, child growth, culture change, intertribal conflict, and social organization and processes (Little and Leslie 1999).

Human biologists, who have extensive training in anthropology, are particularly receptive to multidisciplinary/collaborative research that draws on the skills of both social and natural scientists outside of anthropology. At the same time, because of their breadth of scientific interests, human population biologists also constitute what Paul Baker (1982) identified as a "transdisciplinary science," that is, a science that transcends physical anthropology by incorporating ideas from the anthropological, biological, demographic, environmental, and health sciences to define its interests, objectives, and theory. Its theory is largely drawn from evolution, adaptation, and an understanding of the importance of human variation, development, culture, and genetics. Methods incorporate anthropological concepts of the "natural laboratory," comparative approaches, and "natural experiments," including populations that migrate to different environments and different populations that converge to live in the same environment (Baker 1976, 1977).

Discussion

What has been discussed in this paper is certainly not a comprehensive review of the research that constitutes work in human population biology over half of the twentieth century. The scope of the task is vast. Rather, specific key events and processes are highlighted to illustrate the development of a science that has made important contributions to our understanding of our own species. Important points that have been made are (1) research during World War II contributed to our understanding of human responses to environmental stress and limits to human adaptability under these stresses; (2) international collaboration and exchanges have enriched our understanding of human variability, even among scientists from nations with highly advanced research capabilities; (3) multidisciplinary studies are extraordinarily expensive but highly productive in dealing with complex systems such as ecosystems and their human residents; (4) genetic and evolutionary approaches are unproductive unless there is equal attention to cultural and biological plasticity in the context of lifetime experiences.

The science of human population biology has a history that spans the twentieth century. The science can be identified by its incorporation of living populations as units of study, adaptation and evolution as its theoretical framework, measurement of variation as its fundamental methodology, integrated understanding of the interaction of behavior (culture) and biology as a basic practice, and scientific problem solving as its ultimate objective. Anthropological perspectives play a crucial role in the pursuit of research in human population biology, and it is inconceivable how studies in this discipline could be carried out without these perspectives.

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Internationalizing Physical Anthropology

A View of the Study of Living Human Variation from the Pages of the *American Journal of Physical Anthropology*

by Clark Spencer Larsen and Leslie Lea Williams

CA+ Online-Only Material: Supplement A

In this paper we present an overview of an increasingly global community of physical (biological) anthropologists as it pertains to the study of living human variation (human biology) and as it is represented in the *American Journal of Physical Anthropology* (*AJPA*), focusing especially on the period of 2001–2007, when Clark Spencer Larsen served as editor in chief. The journal was founded by Aleš Hrdlička in order to provide professional identity of physical (biological) anthropology as practiced in the United States. By the mid-twentieth century, the journal editorship under T. Dale Stewart called for greater presence of international research collaboration and publication in *AJPA*. By 1960, international collaboration and non-U.S. authorship began to have significant presence in the journal, a pattern that has continued to the present. As in the pre-2000 period, although non-U.S. contributions cover all major topics in human biology, they tend to focus on population genetics and population history. For the period of 2001–2007, there is an increased presence of multinational collaborative research and non-U.S. authorship, a trend that will likely increase. The recent rise in non-U.S. submissions and authorship is due in large part to increased international collaboration and electronic access to the submission process.

The year 1918 was an important one for American physical (biological) anthropology. It was in 1918 that Aleš Hrdlička—first curator of the Smithsonian Institution's Division of Physical Anthropology and a powerful force in the development of the discipline's identity and early growth—engineered the founding of a new journal, the *American Journal of Physical Anthropology* (*AJPA*). Like many scientific periodicals in the United States, the idea for such a journal developed from models established decades earlier in western and central Europe, especially in France and Germany (Little and Sussman 2010; Ortner 2010; Spencer 1979). This is similar to many other scientific disciplines that were beginning professional journals during the general period of World War I. Hrdlička had high hopes of starting a major scientific periodical where all aspects of human variation in both the living and the dead could be published. By this point in his career, he was a leading figure in American anthropology, and he had the kind of reputation required for launching such an ambitious enterprise. For many anthropologists in the early twentieth century, the term "physical anthropology" was nearly synonymous with Aleš Hrdlička.

In his opening editorial to volume 1, number 1, Hrdlička argued for this new journal to (1) fill a void in the United States at a time when other countries had such a journal; (2) act as a voice for the Committee on Anthropology of the National Research Council (the predecessor of the National Science Foundation); (3) advertise to other disciplines the growing science of physical anthropology; and (4) "assist in such important prospective national movements as the universal training, the anthropological survey of the United States populations, the development of the census, the regulation of immigration, eugenic progress, and all other endeavors tending to knowing, safeguarding, and advancing the physical status of man in this country" (1918*a*:2). This statement shows that one of the founding missions of *AJPA* was to promote the policies and scientific programs of researchers in the United States. Indeed, this was to be truly the American journal of physical anthropology.

The journal had an immediate and profound effect on providing an identity for the discipline as represented in the United States. (The secondary role of applying its research to national issues of the day, such as eugenics and immigration policies, fortunately had limited long-lasting influence.) It was

Clark Spencer Larsen is Distinguished Professor of Social and Behavioral Sciences and Chair of the Department of Anthropology, Ohio State University (4034 Smith Laboratory, 174 West 18th Avenue, Columbus, Ohio 43210-1160, U.S.A. [larsen.53@osu.edu]). **Leslie Lea Williams** is a PhD candidate in the Department of Anthropology, Ohio State University (4034 Smith Laboratory, 174 West 18th Avenue, Columbus, Ohio 43210-1160, U.S.A.). This paper was submitted 27 X 10, accepted 27 VI 11, and electronically published 31 I 12.

 0011-3204/2012/53S5-0012$10.00. DOI: 10.1086/662292

important that the establishment of the journal gave Hrdlička the opportunity to simultaneously define the field and establish a network of like-minded scientists (Little and Sussman 2010; Spencer 1979, 1997; Stewart 1981). Indeed, in a series of three essays in the inaugural issue, Hrdlička defined the discipline as he saw it at the time, emphasizing the comparative approach to human biology and variation in living and past humans in all places and all times around the globe, an approach different from the other biological or social sciences (Caspari 2003, 2009; Hrdlička 1918*a*, 1918*b*, 1918*c*, 1918*d*; Stewart 1940).

In contrast to other contemporary leaders in the discipline and hugely influential on the development of biological anthropology, Franz Boas, the founder of the American brand of anthropology, emphasized the plasticity of the biology of humans (Caspari 2009, 2010). Consistent with much of anthropology in the early twentieth century, Hrdlička's interests were heavily dominated by defining race types and categories from a decidedly nonevolutionary perspective. Nonetheless, from the beginning of *AJPA*, variation in living humans was a central element of his vision for the field along with the study of ancient human remains, anatomical variation, pathology, paleoanthropology, and nonhuman primates. He advocated interdisciplinary research, mentioning in his 1918 editorial the role of chemistry, physiology, and anatomy in the science of physical anthropology (Hrdlička 1918*b*:18). While his vision was typological and nonevolutionary, the scope of areas covered is broadly similar to what we have today, except, of course, genetics and genomic science.

Hrdlička was the leading force in developing physical anthropology and promoting the essential context for the growth of a community of like-minded scientists with overlapping research interests. Had he had students of his own during his career, especially during the period of his 25-year editorship of *AJPA*, we suspect that his influence would have been even greater. Key, though, was his inclusion of living human variation in the journal in particular and the discipline in general. Missing at this point was the promotion of international authorships or a global community of scientists.

The next phase of the journal commenced with the first issue of 1943 under the new editorship of T. Dale Stewart, Hrdlička's protégé and successor as curator of physical anthropology at the Smithsonian Institution. In Stewart's (1943) opening editorial, while praising Hrdlička for his leadership in the founding and development of the journal, he pointed to fundamental changes that he envisioned for the journal, so much so that with his editorship the journal began a new series. The following changes were explicitly identified by him: (1) editorial supervision by the American Association of Physical Anthropologists (AAPA; i.e., selection of an editor and approval of the editorial board membership to ensure wide representation of different areas and interests in professional physical anthropology); (2) greatly expanded involvement of the editorial board in advising the editor, reflecting a shift from a bunch of names on the masthead with limited input to use of the board members in peer review; (3) diversification of content to include "all phases of physical anthropology," essentially moving away from articles "heavily [based on] . . . craniometry" to a wider range of topics; and (4) use of peer reviewers in the review process, because as Stewart offered, "such expressions of (peer) opinion, provided they are honest and presented in a dignified manner, give spice to a publication" (1943:3). With Stewart's editorship began the tradition of fixing an editor's tenure to a term of more or less 6 years, which in later years included an extra half year for making editorial decisions on manuscripts submitted during the latter part of the editor's term (table 1). The only exception in the post-1942 period was the half term served by Sherwood Washburn in the mid-1950s.

Finally, Stewart (1943) made a crucial point that we believe is fundamental to the future international development of the journal. He actively sought "greater interest in our colleagues to the South" (3), especially calling for involvement from physical anthropologists in Latin America. Or, in Stewart's words, "it will be to our mutual benefit . . . to become better acquainted" (Stewart 1943:4).

Today, about 30% of the contents of *AJPA* relate to all manner of living human variation (human biology). The other 70% covers bioarchaeology, dental anthropology, living primates, osteology/paleopathology, paleoanthropology, primate evolution, and other areas pertaining to the field. In this paper we explore the rise of the global community of anthropologists that identify themselves as physical or biological anthropologists and their research pertaining to living human variation. So as to provide consistency with other contributions to this supplemental issue of *Current Anthropology*, we refer to the field as "biological anthropology" and its practitioners as "biological anthropologists," terms that are synonymous with physical anthropology and physical anthropologists, respectively. In this paper, the trends and patterns of globalization of the discipline are documented from the pages of *AJPA* for the period of time when one of us (Clark Spencer Larsen) served as the journal's editor from 2001 to

Table 1. Editorship history of the *American Journal of Physical Anthropology*

Years (n)	Volumes	Editor
1918–1942 (25)	1–29 (old series)	Aleš Hrdlička
1943–1948 (7)	1–6 (new series)	T. Dale Stewart
1949–1954 (6)	7–12	William W. Howells
1955–1957 (3)	13–15	Sherwood L. Washburn
1958–1963 (6)	16–21	William S. Laughlin
1964–1969 (6)	21–31	Frederick S. Hulse
1970–1977 (7)	32–46	William S. Pollitzer
1977–1983 (6)	46–60	Francis E. Johnston
1983–1989 (6)	61–78	William A. Stini
1989–1995 (6)	79–97	Matt Cartmill
1995–2001 (6)	98–115	Emőke Szathmáry
2001–2007 (6)	115–133	Clark Spencer Larsen
2007–	133–	Christopher B. Ruff

2007. Via a meta-analysis of the journal contents, we characterize the participation of the international community of scholars in *AJPA* in terms of research topics, geographical origins, international collaborations, and other evidence of the growth and development of this community, especially from the perspective of study of living populations.

Based on our analysis of this record, we suggest that the rise in non-U.S. ("international") contributions to the journal have served to increase the connectivity between U.S. and non-U.S. practitioners, especially in a number of areas, such as human genetics. We provide reasons for greater growth in some areas and not in others. *AJPA* has from its beginning to the present stood as the "only journal with international reputation that publishes in all areas of physical anthropology" (Larsen 2007:i). Given this special status along with the growth in biological anthropology, it comes as little surprise that the publication of articles in the journal since 1918 has increased so substantially (fig. 1).

Material and Methods

We developed a database made up of manuscripts submitted and articles presenting research results published in *AJPA* for the years 2001–2007. This database was limited to those contributions formally identified by the journal as "research articles," "brief communications," "technical notes," and "perspectives." The database does not include contributions identified as "replies," "notes and comments," "news and views," or articles published in the annual *Yearbook of Physical Anthropology* or other supplements. Those articles with topics relating only to human biology (the subjects had to be alive in the study) were identified and were grouped in one of six categories: (1) human variation; (2) functional anatomy; (3) human adaptability; (4) growth and development; (5) life history, demography, and health; and (6) anthropological genetics (table 2). In order to be included in the analysis, the research subjects had to have been alive at the time of the study, or the living person represented by impressions or images, such as dental casts, radiographs, CT scans, census registries, or telephone directories. Additional data for each article were collected relating to the institutional and national affiliations of the authors, the primary country where the research took place, and the cultural/ethnic groups being investigated.

Analysis of countries examined relate to them as political entities, not cultural domains. For example, Tibet was listed as being part of China, Sicily as part of Italy, and Siberia as part of Russia. The single exception to this classification was Easter Island, which was kept separate from Chile owing to its geographical affiliation with Pacific Island populations rather than South America.

A major focus of this project is the influence of the international community on research in *AJPA*. We assessed this international contribution by examining two elements, namely, (1) the nationality of the first author and (2) collaborations between authors from different countries. Author nationality was determined from institutional affiliation listed in the article's byline. While we recognize that this may not always represent the formal nationality of individual authors, we considered this method both expedient and reasonable given the likelihood of individuals to affiliate with institutions within their own countries. Some authors had multiple institutional affiliations. In these cases, the first affiliation was

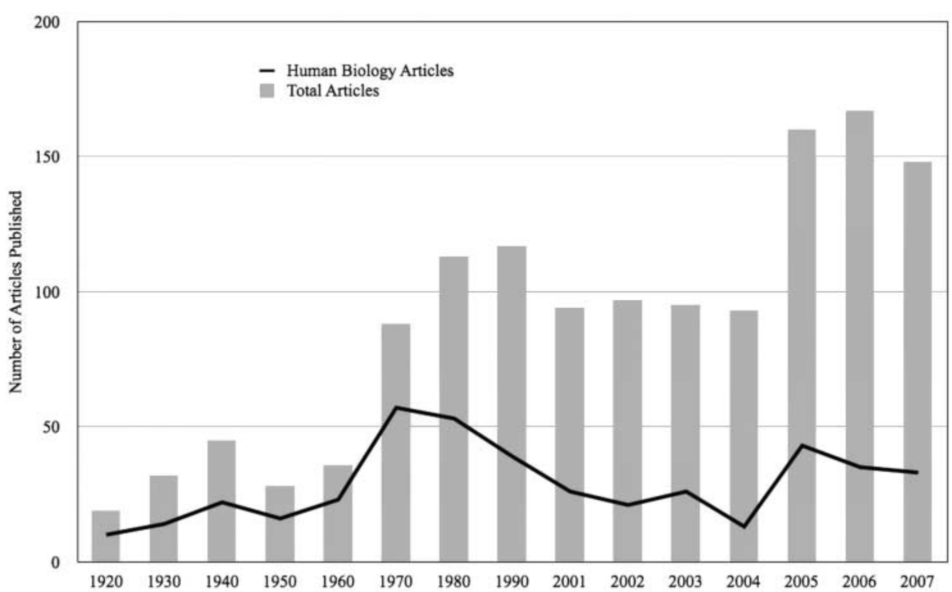

Figure 1. Research articles published in *American Journal of Physical Anthropology*, 1920–1990, 2001–2007.

Table 2. Areas of human biology in the *American Journal of Physical Anthropology*

Topic	Description
Human variation	Race, age, sex, anatomy, forensic, etc.; mostly descriptive
Functional anatomy	Muscle, bone, brain; behavior implicated
Human adaptability	Heat, cold, solar, altitude
Growth and development	Body mass, adiposity, height, growth, teeth, bone, secular trend, age and sex variation in growth processes
Life history, demography, health	Diet, nutrition, mental, work/energetics, epidemiology
Anthropological genetics	Molecular, population, population structure and history, migration, genotype, phenotype, polymorphisms, evolutionary forces (admixture, gene flow, drift, selection, mutation)

used. This was only applicable for three first authors in the data set, whose multiple affiliations were China and the United States, Germany and Japan, and the United States and Brazil.

Fundamental to this discussion is international collaboration. This was assessed by examining the author institutional affiliation of each article. The articles were classified as having an international collaboration if more than one country was represented among the affiliations of the authors. If there was only one author, this was counted with the noninternational collaborations because by definition the research was not collaborative, at least with respect to authorship. Likewise, studies that included local authors from the country where the research was taking place were recorded.

The AAPA, the organizational affiliate for *AJPA*, encourages the participation of researchers from nonindustrialized and developing countries by charging a reduced registration fee rate at annual meetings and for association membership (table A1 in CA+ online supplement A). To assess the representation of individuals from these standard fee-exempt (called fee-exempt) countries as first authors of human biology articles in *AJPA*, the status of the authors as either exempt or nonexempt was recorded.

The primary research location was the country or countries in which the research subjects lived. Both this and the primary continental (and by inference, diaspora) ethnicity of the research subjects were usually identifiable in the article abstract (e.g., Nigerian, African American, European, Euroamerican, Native American). Ethnicity was confirmed via descriptions of the population in the materials section of the article. In some cases, multiple ethnicities were the focus of the study, and these were also recorded in the category "multiple ethnicities" and were then subdivided into their constituent ethnicities based on details presented in the materials descriptions. This category proved the most problematic to assess, especially because many investigations examined the results for admixed populations or included individuals of mixed ethnicity. The "mixed ethnicity" category therefore included those studies in which the research subjects were from admixed populations. Likewise, owing to identification of research subjects from countries with high levels of admixture of diasporic populations, especially during the colonial and postcolonial eras (e.g., Canada, United States, United Kingdom), ethnicity was not clear. Therefore, if these studies did not specify the ethnic/diasporic identity of the subjects, then the articles were excluded from the analysis. In total, there are 183 articles in the data set employed in this analysis of the ethnicity of study subjects.

The raw frequency of articles published in a scientific periodical does not illustrate the relative research influence on the field (see Stojanowski and Buikstra 2005). Thus, for assessing the effect of an article, we employed a method developed by Stojanowski and Buikstra (2005) in their meta-analysis of skeletal biology articles published in *AJPA*. This was accomplished by recording the total number of citations for each article from the ISI Web of Knowledge citation database for science and social science on October 15, 2009. Of course, the longer an article has been in print, the greater the number of citations associated with it. In order to control for the number of years in print, the number of citations was divided by the number of years in print for each article (and see Stojanowski and Buikstra 2005). This was recorded as the "average impact" or "power index" for each article. Likewise, the visibility of the articles was assessed in a manner similar to that calculated by Stojanowski and Buikstra (2005), namely, by comparing the number of human biology articles with all research articles (listed as articles, research articles, perspectives, technical notes, brief communications, and original articles) published in *AJPA* for the years 2001 through 2007.

To provide a comparative framework for the results obtained in this analysis, pre-2000 *AJPA* articles were examined and the international nature of their research recorded in terms of author nationality and international collaboration. In particular, *AJPA* articles from eight specific years—1920, 1930, 1940, 1950, 1960, 1970, 1980, and 1990—were examined. From these years, human biology articles were identified and author affiliations recorded based on the same criteria as in the analysis of articles from 2001 to 2007.

Similarly, to provide a basis for understanding the population from which articles are accepted for publication, we have included a summary of submission data. Included in the submission data set are those articles that were submitted between July 1, 2001, and June 30, 2007, meaning the information for years 2001 and 2007 represent the submissions for half a year only. This distinction is important, because the published human biology data set includes all articles published between 2001 and 2007, some of which were accepted for publication before 2001. The submissions data therefore reflect the research that is being done at that moment, while the publications data reflect the research that became public well after the submission and acceptance process. It is important to point out that the submissions data included under the category of human biology do not correspond exactly to the data presented for published articles. This is because the topical categories for these data are based on those assigned by the editor at submission, not those created by us for evaluating the published data. For the most part, articles with the topics of anatomy, demography, epidemiology, dermatoglyphics, dentition, growth and development, metrics, and population genetics/history were classified as human biology submissions, whereas articles with the topics of living primates, osteology, paleoanthropology, paleopathology, and primate fossils/evolution were classified as non–human biology. In some cases, more specific information was available in the submission database to allow further differentiation of categories, such as metrics, which might be ambiguous.

The submissions data need some additional definitions. There is a varying delay between the date of acceptance and the eventual date of publication. Therefore, the category "accepted" includes nine articles accepted between 2001 and 2007 but that were published after Clark Spencer Larsen's tenure as editor of *AJPA*. There were 11 manuscripts whose status had not been determined by the end of Larsen's editorship but that did not subsequently appear in *AJPA*. These 11 articles were classified as "rejected." Additionally, these totals do not include 31 manuscripts that were withdrawn before publication/review. One manuscript did not have country data and was not included in the international submission assessments.

Results

Article Submissions: Submission and Acceptance in All Areas

Submissions to *AJPA* steadily increased between 2001 and 2007 (table 3), while the percentage of articles accepted during this period declined (fig. A1 in CA+ online supplement A). Manuscripts submitted for review tend to be from non-U.S. countries, with international submissions accounting for 1,000 articles from 2001 to 2007, or 61.3% of all submissions during this period. Likewise, the international level of submissions increased throughout the study period to over 60% from 2004 onward (table A2 in CA+ online supplement A; fig. 2). There is a disparity, however, in the acceptance status between international and U.S. articles. Of the articles accepted for publication between July 2001 and June 2007, non-U.S. articles (51.8%) slightly outnumbered U.S. articles

Table 3. *American Journal of Physical Anthropology* submissions by year (2001–2007)

Year	Accepted	Rejected	Total
2001	52	40	92
2002	138	107	245
2003	132	140	272
2004	116	155	271
2005	109	166	275
2006	138	158	296
2007	57	93	150

(48.2%). However, of rejected articles, 69.9% are non-U.S. articles and only 30.1% are from the United States (fig. A2 in CA+ online supplement A). Despite the international status of article submissions, the vast majority of submissions are from nonexempt countries. Almost three times as many articles from exempt countries are rejected ($n = 228$) as accepted ($n = 78$), while acceptances ($n = 664$) outnumber rejections ($n = 630$) for nonexempt countries (table A3 in CA+ online supplement A).

Human biology submissions make up over 40% of the submissions between 2001 and 2007. There were 629 submissions with a human biology focus, while 836 submissions were not related to non–human biology. Manuscripts without submission topic data are excluded from the following results as were those with miscellaneous topics. Human biology submissions make up between 33.7% and 47.5% of the manuscripts submitted in any given year between 2001 and 2007, reaching their highest percentage in 2003 and then leveling out afterward (fig. A3 in CA+ online supplement A). Data on exemption status in human biology submissions indicate a similar pattern to the overall submission data. More manuscripts were submitted from nonexempt countries ($n = 459$) compared with exempt countries ($n = 170$). Therefore, exempt countries make up 27% of the human biology submissions while they only make up 13.8% of the submissions for non–human biology articles. This pattern also holds for international submissions to the journal, in which both categories have over 50% of their submissions from non-U.S. countries, with 67.1% of human biology articles and 57.5% of non–human biology articles coming from non-U.S. countries.

Number of Articles: Human Biology Publications

The number of human biology publications per year increased dramatically, especially in a comparison of the study period with previous decades and in the later part of the study period itself (fig. 1). Year 2004 represents a decline in human biology publications, with only 13 articles. This pattern must be examined in terms of the total number of articles published in *AJPA* during this period, because the journal increased by seven to eight articles per issue after 2005. Human biology articles represent between 20% and 30% of the articles pub-

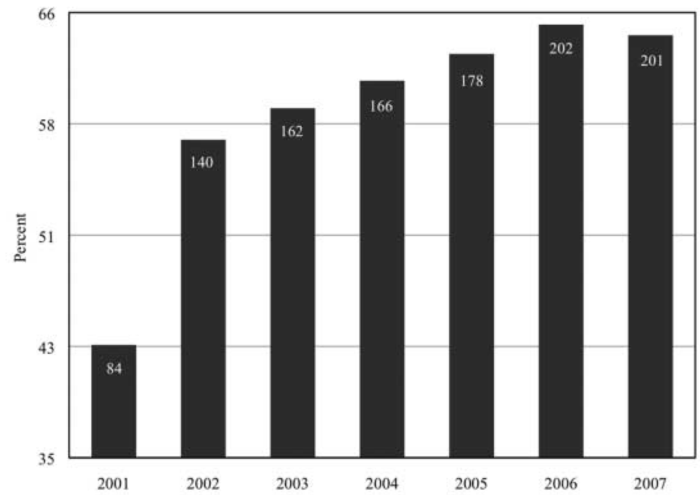

Figure 2. Percentage of articles submitted to *American Journal of Physical Anthropology* from international (non-U.S.) countries. While the analysis pertains just to Clark Spencer Larsen's editorship from July 2001 to June 2007, the data for 2001 and 2007 are reconstructed from the proceedings of the seventy-first American Association of Physical Anthropologists meeting (AAPA 2002) and the proceedings of the seventy-seventh AAPA meeting (AAPA 2008), respectively. The numbers on the bars represent the total number of articles submitted each year from international countries.

lished during the study period 2001–2007 (table 4; fig. A4 in CA+ supplement A). The year 2004 is the lone exception, with human biology articles representing only 14% of the research articles for the year.

Publication Topics

Over 11% of all research articles—that is, all topics including human biology—published between 2001 and 2007 were anthropological genetics human biology articles (table A4 in CA+ supplement A). These 95 genetics-based articles represent 48.2% of the human biology publications during this period (fig. A5 in CA+ supplement A). When the publications are examined by topic in relation to all research articles in the study period, articles on anthropological genetics represent between 4% and 14% of all articles in a given year (table A5 and fig. A6 in CA+ supplement A). If 2004 is excluded from the comparison, then these articles represent between 10% and 14%.

Anthropological genetics also represents a large proportion of the human biology articles published in any given year, ranging from 31% to 62% of all human biology publications (table A6 in CA+ supplement A; fig. 3). Compared with other years in the study period, 2004 is clearly abnormal, in which growth and development publications outnumbered anthropological genetics publications by 7%.

Research Locality: Geopolitical Location of Study and International Authorship

During the period 2001–2007, human biology research was published pertaining to 70 different countries from all major regions of the world (table A7 in CA+ supplement A; fig. 4). These numbers total 234 (more than the 197 articles in the data set) because some articles included research subjects living in more than one country. Studies focusing on populations residing in the United States are represented by the highest number of articles ($n = 31$) followed by studies with a global population focus ($n = 16$). Australia ($n = 12$) and Brazil ($n = 10$) are the only other countries with 10 or more publications.

When countries are subsumed into regions, they cover every continent except Antarctica, with research also conducted in the Mediterranean and on a global scale. Forty-seven articles focus on research in South America, 44 articles focus on research in North America, and 38 article focus on research in Europe (table A8 in CA+ supplement A). When the global and Mediterranean categories are excluded, publications focusing on the Americas make up some 45% of the article research locations for the study period (fig. 4, inset). The first authors of human biology publications are from 31 different countries, with the United States the most represented with 80 first authors; the United Kingdom ($n = 13$), Italy ($n = 11$), Brazil ($n = 10$), Spain ($n = 9$), Australia ($n = 9$), and France ($n = 8$) are the next most represented countries (table A9 and fig. A7 in CA+ supplement A).

An examination of the continent of ethnic origin for the research subjects represented in the studies reveals a large focus on global studies or on studies in which ethnic groups from more than one continent were included (table A10 in CA+ supplement A). These categories include studies in which research subjects were studied in their country of origin as well as diasporas of ethnic groups from particular conti-

Table 4. Human biology in the *American Journal of Physical Anthropology*, 2001–2007

Year	Articles published	Human biology articles published	%
2001	94	26	27.7
2002	97	21	21.6
2003	95	26	27.4
2004	93	13	14.0
2005	160	43	26.9
2006	167	35	21.0
2007	148	33	22.3
Total	854	197	23.1

nents to other regions. Over 30% of the studies include multiple continents of ethnic origin (fig. A8 in CA+ supplement A) including those studies that examined research questions on a global scale. Studies focusing on ethnic groups from Europe (18.6%) and Asia (13%) were also large proportions of the sample, while Australia (2%), Central America (2%), and North America (2%) were the least represented.

When partitioned by year and grouped by U.S. and non-U.S. first-author affiliations, the increasingly international composition of authors contributing to the journal becomes especially clear (table 5; fig. A9 in CA+ supplement A). Except for the years 2004 and 2005, there are more international first authors than U.S. authors. In 2006 and 2007, the peak years for number of international first authors, there are nearly twice as many international authors as U.S. authors.

To put the international contribution (i.e., non-U.S. first authorship of *AJPA* articles) into perspective for research articles published since 1920, it is far more common for *AJPA* before the 2001–2007 study period to have (considerably) more U.S. first authors than international first authors in all research areas (table A11 in CA+ supplement A). Figure 5 includes the affiliation of first authors of research articles in *AJPA* recorded every 10 years from 1920. In these years, U.S.-affiliated first authors outnumbered international first authors in every year but 1960, when both categories—U.S. and non-U.S. authorship—had 18 articles each. This trend also holds true for human biology articles in *AJPA*, which show an increasing trend toward international first-author affiliation (table A12 in CA+ supplement A; fig. 6).

The average power-index value for each country that has more than one first author is shown in table A13 and figure A10 in CA+ supplement A. First authors affiliated with China ($n = 3$) and Germany ($n = 6$) had the highest power indexes at 4.07 and 3.90, respectively.

Author Country Affiliation (*Exempt vs. Nonexempt*)

Approximately 22% ($n = 44$) of the articles are from countries that are "exempt," or where first authors pay reduced rates for AAPA membership. The average power index of the exempt first authors was 1.879, while the power index of the nonexempt first authors was higher, at 2.191.

When examined year-by-year for the 2001–2007 study period, the year with the lowest number of exempt first authors is 2004, with only two authors affiliated with exempt countries (table A14 in CA+ supplement A; fig. 7). When the number of articles per issue increased in 2005, the number of first authors from exempt countries did not rise accordingly but

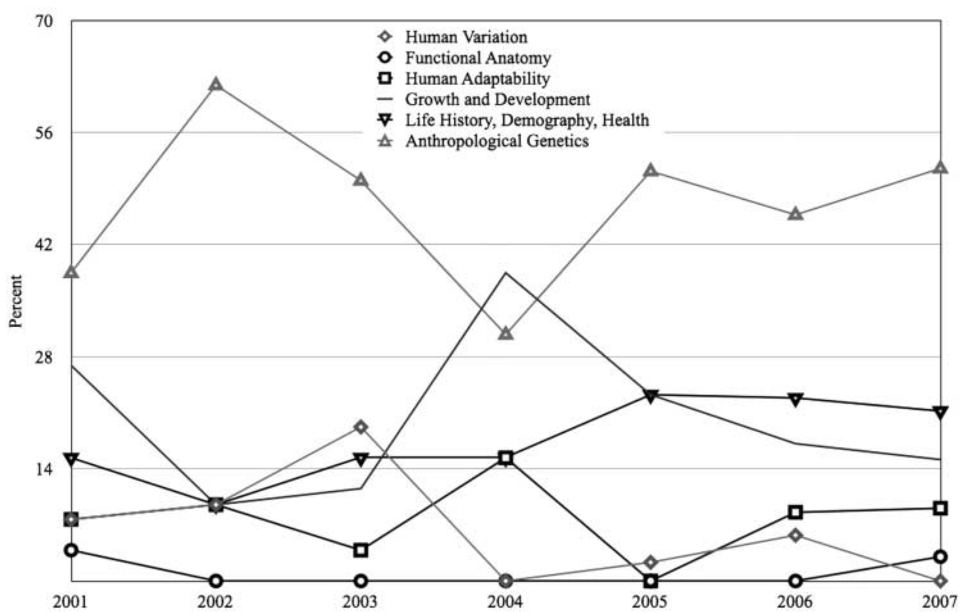

Figure 3. Human biology publications by topic, *American Journal of Physical Anthropology*, as a percentage of human biology articles published each year, 2001–2007.

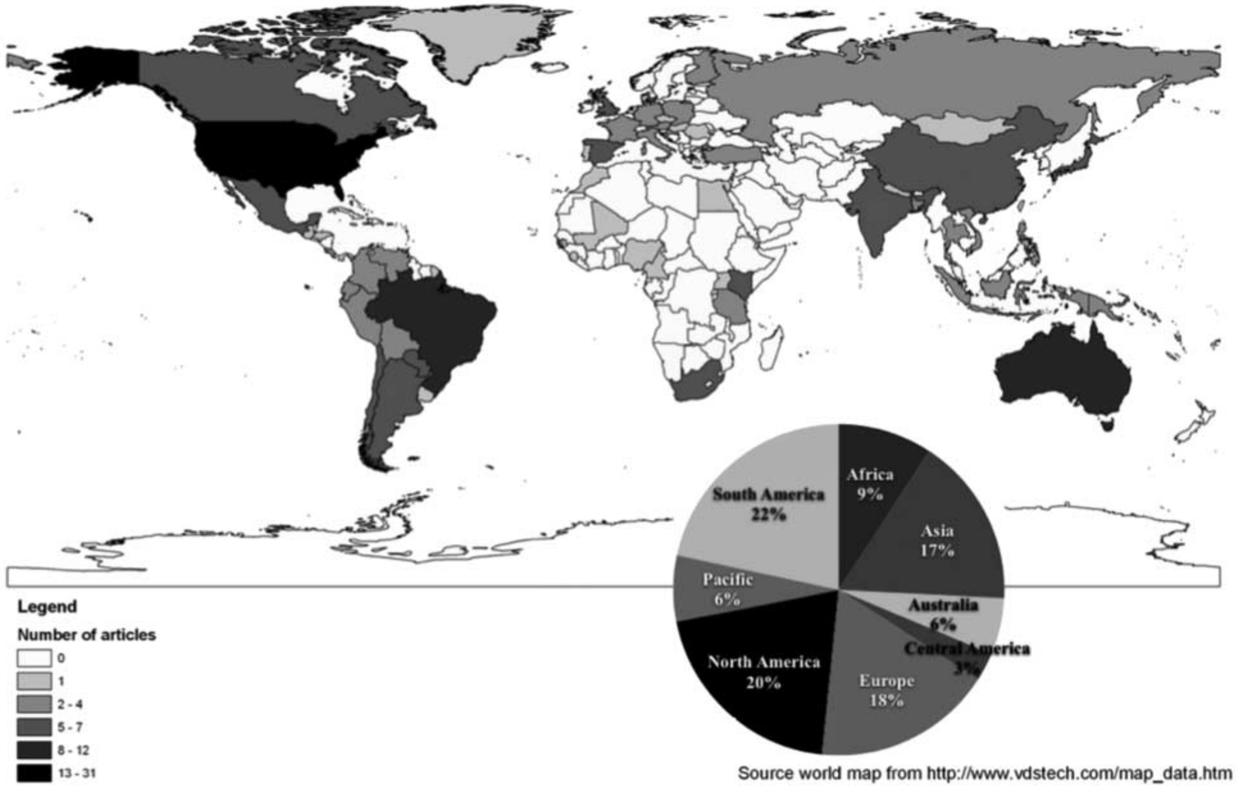

Figure 4. Research location of published human biology articles by country, *American Journal of Physical Anthropology*, 2001–2007.

instead returned to the number seen in 2001 and 2003 ($n = 6$). Thus, the bump in the number of articles was largely from countries that are in the nonexempt category of authorship.

The contribution to the journal by authors from exempt and nonexempt countries is more clearly illustrated by examining the percentage of human biology first authors that are affiliated with exempt countries (fig. 7). This peaks at 38% in 2002 and drops to approximately 14% in 2004 and 2005. However, in the other years, the number of first authors affiliated with exempt countries remains relatively constant at between 23% and 24%, with a rise from 2005 to 2007.

When first-author exemption status is examined by topic, by far the dominant research area represented in *AJPA* is anthropological genetics (table A15 in CA+ supplement A; fig. 8). Indeed, 61% of authors affiliated with exempt countries published articles on anthropological genetics compared with 44% of authors from nonexempt countries.

Local Authorship

The participation of local scholars in human biology research was assessed through the examination of both the country affiliation of all study authors and the primary research location of the study itself. Local authors (authors with affiliations at institutions in the country of the primary research location, including authors of global studies) were included in 75% of the human biology articles published between 2001 and 2007. Of those articles without a local author, 84% of them were from countries defined as "exempt" by the AAPA (table A16 in CA+ supplement A).

International Collaborations

Collaborations involving coauthorship by authors representing two or more countries account for 45% (88 of 197) of human biology articles in the study period (table A17 in CA+ supplement A). When examined on a year-by-year basis, international collaborations outnumber noninternational collaborations in 2001, drop dramatically from 2002 to 2004, and then increase between 2005 and 2007 (fig. A11 in CA+ supplement A). In 2007, international collaborations again outnumbered noninternational collaborations.

Power Index

The impact, or power index, for human biology articles published in *AJPA* is highest in 2002 and lowest in 2007 and is generally higher in the beginning of the study period (table A18 and fig. A12 in CA+ supplement A). Human variation articles had the highest power index (2.634) followed closely by anthropological genetics (2.340; table A19 and fig. A13 in

Table 5. Human biology author affiliation by year, United States versus non-United States

	2001	2002	2003	2004	2005	2006	2007	Total
United States	11	7	7	8	23	12	12	80
Non-United States	15	14	19	5	20	23	21	117
Total	26	21	26	13	43	35	33	197

CA+ supplement A). Functional anatomy, human adaptability, growth and development, and life history, demography, and health all ranged between 1.625 and 1.852. These patterns were also consistent when the mean number of citations per topic was examined (table A20 and fig. A14 in CA+ supplement A).

Discussion and Conclusions

A number of findings emerge from this analysis of *AJPA* content. They are as follows.

Submissions

1. There has been remarkable growth in the journal, including in human biology, especially after 1960 and then dramatically after 2004. This growth reflects the unique contribution of biological anthropology in regard to coverage of all places and all times and with a special emphasis on variation.

2. From 2001 to 2007, the acceptance level for articles submitted to *AJPA* decreased overall. However, the number of submissions from non-U.S. countries increased.

3. Submission data reveal a disparity in acceptance rates between non-U.S. and U.S. submissions in which U.S. articles have a greater chance of being accepted than non-U.S. articles. This pattern, true for submissions from exempt countries compared with nonexempt countries, is mirrored in the human biology submission data.

Publications

4. The contribution of human biology to the journal as a percentage of human biology articles to total research articles rises until 1970. It then declines until 2005.

5. For the study period 2001–2007 in particular, growth is reflected in all areas. These include human biology and especially anthropological genetics.

6. Over the history of the journal, authorship has become increasingly international. This is especially evident given the minimal presence of international authors in the early years of the journal.

7. For the study period, human biology research is represented in the study of human populations in 70 countries, including virtually every inhabitable landscape globally. There is a dominance of research focusing on populations residing in the United States. On the continental scale, the number of articles is equally represented from South America, North America, and Europe.

8. First authorship of human biology articles is from 31 countries, with the United States dominating. The other leaders are Australia, Brazil, Italy, Spain, and the United Kingdom. The topics of these articles are dominated by anthropological

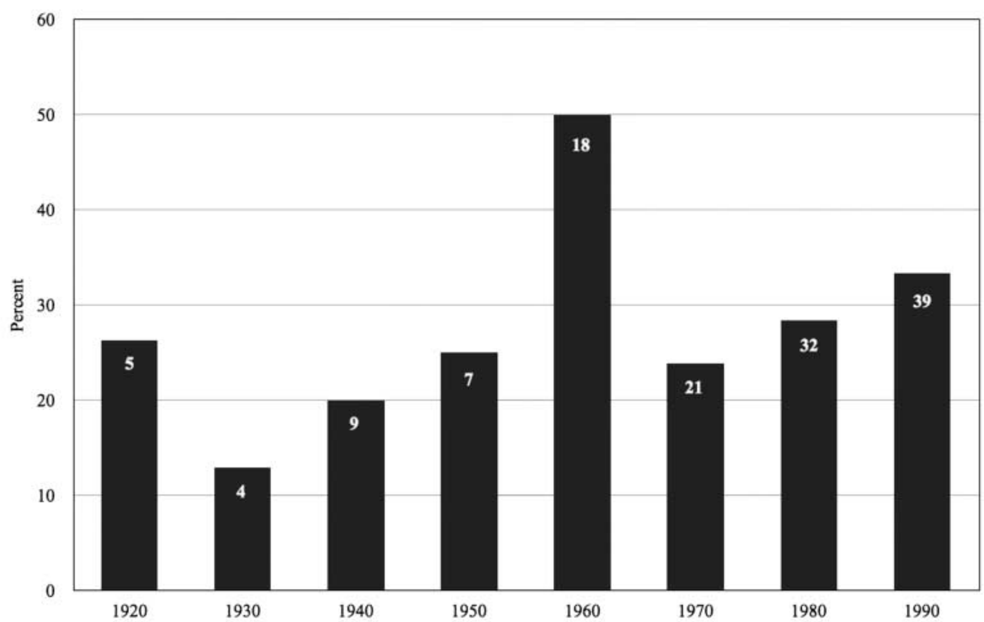

Figure 5. Percentage of all published research articles by international (non-U.S.) authors, 1920–1990. The numbers on the bars represent the total number of published articles each year from international first authors.

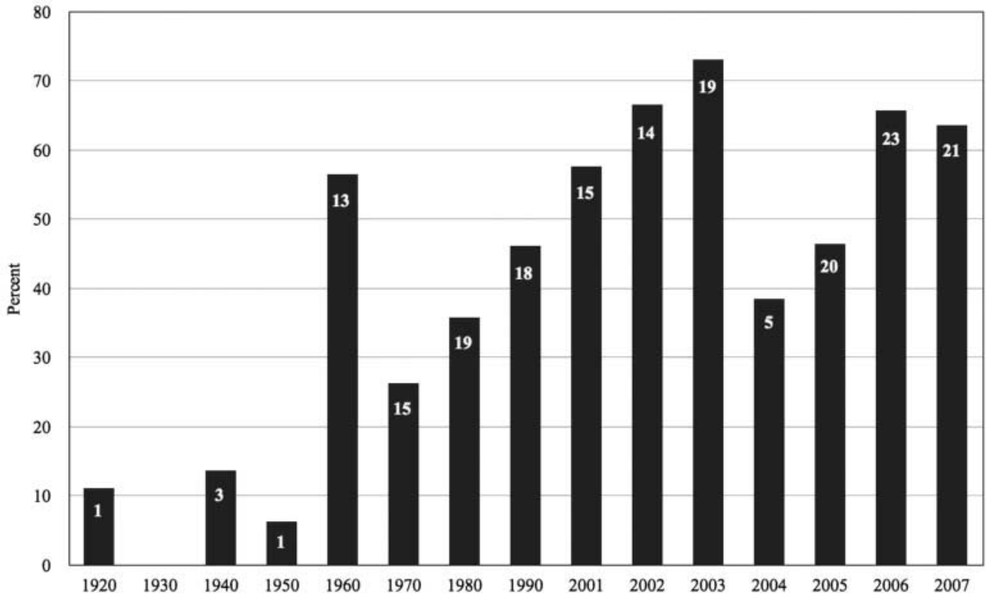

Figure 6. Percentage of published human biology articles with non-U.S. first-author affiliations. The numbers on the bars represent the total number of published human biology articles each year from international first authors.

genetics. There is a clear trend toward increased internationalization. Except for 2 years of the study period (2003–2004), the number of international authors exceeds the number of U.S. authors.

9. For the study period, over 20% of the first authors are from "exempt" countries. Their articles had a lower average impact as measured by the power index than articles written by nonexempt authors.

10. The greatest number of articles in the exempt category is in anthropological genetics. This category represents nearly 20% more than anthropological genetics articles in the nonexempt group.

11. Local authors—authors with affiliations to institutions in the primary research country—were included in three-quarters of the human biology studies, representing a trend in which local human biologists/biological anthropologists contribute to research within their country.

12. For the study period 2001–2007, nearly half of the articles in human biology are international collaborations where at least one author is from a non-U.S. country.

What does this analysis tell us about the international community of biological anthropologists who study living human variation? First and foremost, *AJPA* has become more international, there is a significant presence of human biology, and the trends over the long term suggest continued growth. This international aspect of the journal emerged, judging from the 1920–1990 data, after World War II. Indeed, non-U.S. researchers were virtually absent from the journal before 1960 despite Stewart's urging in 1943 to grow the boundaries of the journal. This increasingly international montage beginning ca. 1960 may reflect the postwar technological boom, which would make communications between authors in different countries more convenient. Eventually, the international collaborations that are dominant within the pages of *AJPA* during the beginning of the twenty-first century also include a substantial number of local (non-U.S.) collaborations in which local non-U.S. researchers participate in human biology research within their own country. However, most of the studies that did not include local researchers were from AAPA "exempt" countries, perhaps indicating that the tendency for Western anthropologists to enter the field and research the "other" is still alive and well within biological anthropological research in general and human biology in particular.

We believe that in the large picture, the community of scientists that make up biological anthropology is international, involves a large number of international collaborations, and speaks to a pattern that will only continue to grow. A couple of questions emerge from our survey that require additional exploration. First, there is a suggestion of a reduction in the number of human biology contributions during the study period. Why? We suggest that the reduction, albeit slight, may be influenced by the appearance and growth of alternative publication outlets. In particular, the *American Journal of Human Biology* (*AJHB*; founded in 1988) was added to the ISI Web of Knowledge citation list for anthropology in 2003. We suspect that the increased growth and visibility of this journal may have led some authors to submit their work to *AJHB* rather than to *AJPA*. It is also likely that those working in the area of human biology may be increasingly

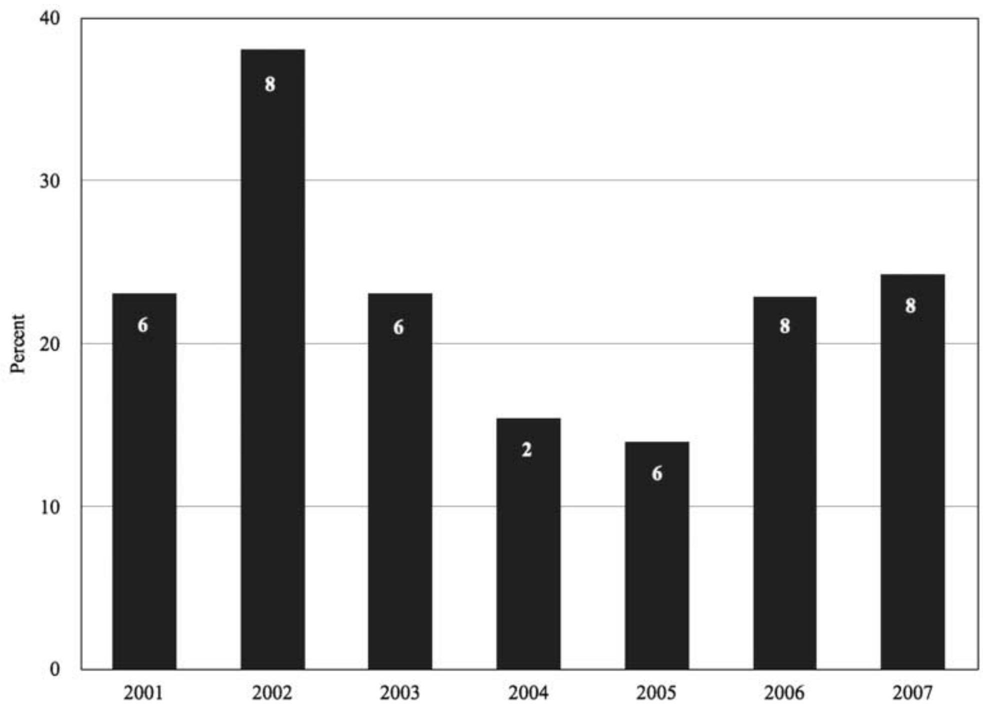

Figure 7. Percentage of published human biology articles with a first author from an exempt country, 2001–2007. The numbers on the bars represent the number of published human biology articles each year with first authors from exempt countries.

submitting their work to other prominent journals with a focus in this area, such as *Annals of Human Biology*, *Human Biology*, and more recently *Economics and Human Biology*. Similarly, anthropological geneticists are submitting their work to genetics journals. The pattern as it is expressed in *AJPA* for the period of 1920–1990 and 2001–2007 indicates that human biology in general has become a less-dominant part of published research in *AJPA*. It is also possible that the resurgence of skeletal biology and bioarchaeology after the 1970s (Buikstra and Beck 2006; Larsen 1997) in part explains the relative decline in human biology within the journal.

Second, there is a strong interest in publication in the area of population genetics, both in the composite of the 2001–2007 study period and especially in exempt-status countries. Why? This could be related to variation in traditions of biological anthropology in different countries. In the United States, genetics is a strong area of inquiry, but biological anthropology is probably more diverse than in any other country. That, of course, is a testable hypothesis, and it is beyond the scope of this paper. In non-U.S. countries, genetics may be a more focal point of study and research. For example, in Brazil, population genetics has been a long-standing strength in biological anthropology. We further suggest that the roots of this interest in genetics may lie in an old and strong interest in questions of population origins, addressing questions of where a population is from and how it differs from other populations, including those nearby. Was there admixture when two (or more populations) came into contact? In other words, a traditional area of biological anthropology is population history, and a common tool for answering questions about population history is derived from genetics. Furthermore, the increase in genetics may be due to advances in genomics, including those associated with the Human Genome Project. In addition to research undertaken by individual scientists, there are projects that have developed out of national interests (e.g., Iceland).

Our preliminary review of journal contents in the area of population genetics is that it tends to be descriptive and not generated by an interest in testing specific hypotheses. Rather, the research provides results with ad hoc conclusions of what these results mean about the history of a particular population or group of populations. Our discussion below is not meant to place value on specific studies. That is, descriptive population history is not more or less valuable than some other area of the study of human biology. Rather, these differences and tendencies illustrate the traditions of biological anthropology represented in every country around the world. Our review of the literature suggests that there are broad traditions that characterize the discipline. There is a clear and strong interest in characterizing population history, which we believe is a holdover from the long-standing traditional interest in race and typology. Of course, this is not as prevalent today, but we believe that it is still of strong interest on the global scale. We speculate that while population genetics is not ty-

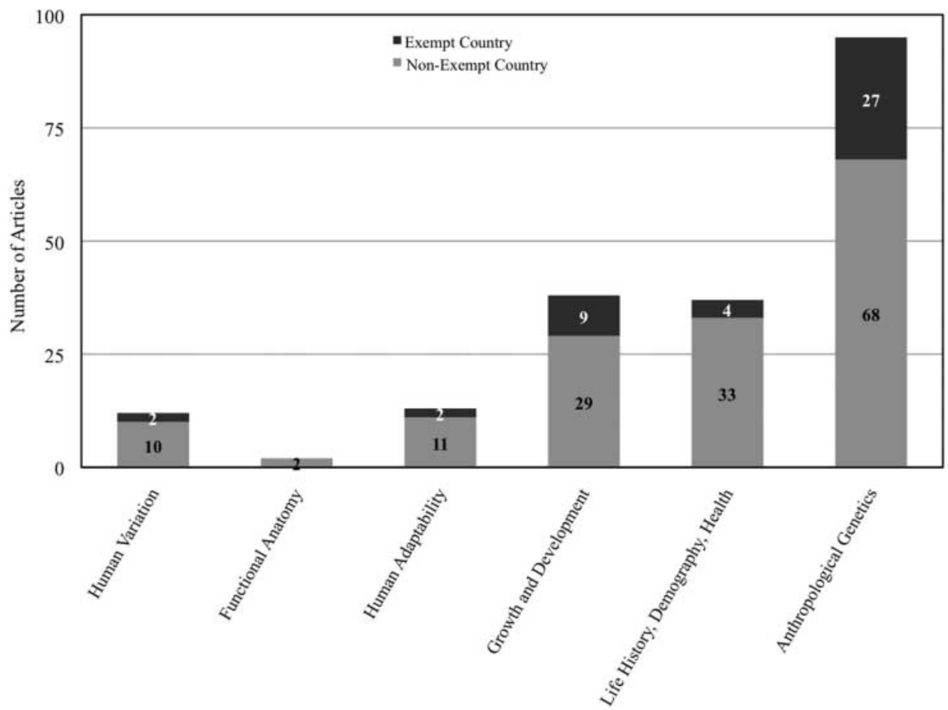

Figure 8. Human biology first-author exemption status, 2001–2007. The numbers on the bars give the number of published articles in each topic category whose first authors are from either exempt or nonexempt countries.

pological—although admittedly characterizations such as haplogroup A, B, C, and D have a strong ring of typology—it is clearly linked to traditional focus on population history.

That the focus on anthropological genetics and the questions of population history that so many of these studies attempt to investigate is so strong in the international community, especially with exempt authors, may be related to this history of biological anthropology in the United States. United States biological anthropology grew from the theoretical backgrounds of its three most prominent patriarchs of the discipline: Franz Boas, Aleš Hrdlička, and Earnest Albert Hooton. Hrdlička's research, so influential in the early decades of the history of *AJPA*, focused on human variation and typology. However, unlike most other areas of the globe, most U.S. biological anthropologists at the time would have been learning under the tutelage of Boas and his students, an approach rooted in four-field anthropology. The contributions of international and U.S. researchers to *AJPA* may, therefore, represent a difference in the theoretical background of anthropologists from these regions. Biological anthropology in the United States is predominantly associated with the four-field Boasian approach in which biological anthropologists, sociocultural anthropologists, archaeologists, and linguists are grouped together within the same department. Biological anthropology in most other countries, however, is primarily associated with the medical or biological fields. Rarely is it associated with archaeology, sociocultural anthropology, or linguistics. It is, therefore, not too surprising that the research being done on living humans in the United States might be topically skewed differently than research in other countries given this theoretical background.

What does the future hold for the growing global community of collaborating biological anthropologists? Will the trends that we have identified in this paper continue? We believe that the research collaborations will be increasingly collaborative and global. Within Clark Spencer Larsen's editorship of *AJPA* (2001–2007), profound changes occurred in journal access, both with regard to readership and research-manuscript submission; that is, it is during this time that the journal became available electronically. For the first time, access to reading and submission of research manuscripts could be gained by having access to a computer and the World Wide Web. Access to readership by institutional subscription is costly and virtually unattainable for many of the countries we discuss in this paper. However, the individual cost is also relatively low for exempt countries, and anyone, regardless of whether they are a subscriber or not, can submit a manuscript for review simply by accessing the Wiley manuscript submission Web site (http://mc.manuscriptcentral.com/ajpa). The high cost of subscription will continue to discourage individuals and institutions with fewer resources from reading the journal. On the other hand, access for submission is free, and anyone with access to the Internet can submit an article.

During Larsen's tenure as editor, the submission of research manuscripts from non-U.S. countries dramatically increased (from 42.8% in 2001 to 64.4% in 2007). In the long term, this trend will promote the continued growth of the international community of biological anthropologists (see Larsen 2007). This will result in a shift in traditions of biological anthropology to include more research outside the realm of population history. As long as race continues to remain a part of science and society overall, the interest in population history as it pertains to classification in particular will continue to be with us, no matter where the topic is studied.

Acknowledgments

We thank the organizers of the conference "The Biological Anthropology of Living Human Populations," Ricardo Ventura Santos and Susan Lindee, for their invitation to participate and to prepare this manuscript for publication and for their helpful input on earlier drafts. We also thank all of the other conference participants for their helpful comments during the week in Teresópolis, Brazil. We especially thank Mike Little and Noël Cameron for their comments on the symposium draft paper and other insights into the history of the discipline. Special thanks go to two anonymous reviewers and to Christopher Ruff and Joshua Sadvari for their helpful suggestions for revision and clarification of text. Last, we thank the Wenner-Gren Foundation for Anthropological Research and especially Leslie Aiello and Laurie Obbink for their support.

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Biological Anthropology at the Southern Tip of Africa

Carrying European Baggage in an African Context

by Alan G. Morris

One of the biggest surprises in the rise of apartheid in South Africa in the 1940s was that, unlike in prewar Germany, it was not rooted in the physical anthropology of the previous decades. The engineers of apartheid were, for the most part, Afrikaans-speaking ethnologists operating out of the Afrikaans-medium universities, where little or no physical anthropology was taught. The University of the Witwatersrand and the University of Cape Town, both English-medium schools based on the traditions of British academia, were the centers of biological anthropology. Although none of the early practitioners from these schools were directly involved in the implementation of the apartheid policy, their strict typological approach to human variation provided a solid growth medium in which the government policies could develop without credible scientific opposition.

Inherent in the structure of the apartheid ideal that held sway in South Africa from 1948 to 1991 was the idea that people's behaviors were underlain by an unchanging culture that was in its own turn underlain by a rigid biological identity. With biology and culture neatly overlapped, it was easy to compartmentalize each "race" and then apply the laws promulgated by the apartheid ideologues as they locked the system in place during the early 1950s. The apartheid legislation had four cornerstones: (1) the Population Registration Act, which created a register on which every citizen was classified by race; (2) the Group Areas Act, which dictated where people would live according to their identity on the register; (3) the Separate Amenities Act, which determined which schools, leisure facilities, and jobs were accessible to each racial group; and (4) the Immorality Act, which determined not only whom one could marry but also made sexual activity between races illegal. All of this could not function without clear and overlapped definitions of "race" and "culture."

South African society was already highly racialized by the end of the nineteenth century, but there was no overarching system of racial segregation. Elitist schools in the Cape Colony were for white students only (Bickford-Smith 1995), and there was strong religious and social pressure against intermarriage between blacks and whites in the Boer Republics, but there also was a partial franchise allowing "educated" blacks to vote in the Cape Colony. The real pressure for a legal system of segregation only came after the end of the Second Anglo-Boer War in 1902. Although before the war the British government had used the mistreatment and exclusion of Africans from political life as one of the excuses for going to war with the Boer Republics, its representatives after the war agreed to continue the exclusion of people of color from political power in the Boer provinces of the new Union of South Africa. The Union Parliament passed the Land Act in 1913 that restricted African ownership of land, and they also passed an early version of the Immorality Act in 1927, but the real stimulus for the implementation of legislated segregation came with the rise of Afrikaner nationalism in the 1930s. Dubow (1995) suggests that the immediate cause of apartheid policy was the recognition of the large degree of poverty among white Afrikaners and how they could be uplifted and protected from competition with blacks by using Afrikaner nationalism to create a "Volks Republic" where whites (specifically Afrikaans-speaking whites) were dominant. The philosophical base for the new idea of "apartheid" (coined in 1935 and meaning "separateness") was not primarily in the biological sciences. Its strongest proponents were social scientists.

Physical anthropology was separate from social anthropology in the South African academic model, and it became heavily focused on paleoanthropology after the first discoveries of early humans in South Africa in the 1930s. Even though it did continue to examine aspects of racial variation, the separation from social anthropology and its focus on paleoanthropology caused physical anthropology to be bypassed and eclipsed by social anthropology in the sociopolitical development of the country. By the 1940s, social anthropology had divided into two distinct camps: the liberal tradition of British social anthropology at the English-speaking univer-

Alan G. Morris is Professor in the Department of Human Biology, Faculty of Health Sciences, University of Cape Town (Observatory 7925, South Africa [alan.morris@uct.ac.za]). This paper was submitted 27 X 10, accepted 8 VI 11, and electronically published 8 XII 11.

 0011-3204/2012/53S5-0013$10.00. DOI: 10.1086/662289

sities, and "volkekunde" at the Afrikaans-speaking universities. Volkekunde was descriptive anthropology based on the German "ethnos" theory, the cultural analogue of biological typology. Each "volk" (people) was said to have its own ethnos or culture linked to inherited physical and mental characteristics (Sharp 1981). Students of volkekunde concentrated on the description and geographic distribution of culture traits in order to differentiate between groups, and interpretations were strongly social Darwinist because of this historical and evolutionary approach. Just like biological organisms, societies were believed to evolve from simple to complex and finally go through cultural deterioration until extinction was reached. Material culture was an important calling card that marked the ranking of societies on a developmental scale, and cultures were examined for distinct features that could be used to identify their evolutionary state. Under this system of teaching, it was easy to overlay group psychology, level of technological advancement, and political structure onto a typological definition of race.

Social anthropology in the English-medium institutions was distinctly British in its comparative examination of social phenomena such as organization, moral codes, religions, and modes of communication (Kuper 1983). The focus was on the commonality of human behavior and how all humans have the ability to adapt to local conditions. The approach was ahistoric and distinctly non-Darwinian. South African social anthropologists were strongly influenced in this by Bronisław Malinowski and A. R. Radcliffe-Brown. Malinowski taught a seminar at the London School of Economics between 1920 and 1938, and nearly every English-speaking South African social anthropologist attended this at one point in their training. Malinowski took special time to criticize older anthropological ideas, especially the model of "diffusionism" that rejected independent invention of important cultural traits. Although he was an evolutionist himself, he did not believe that living societies and their cultures were at given stages of evolution (Kuper 1983). These approaches were emphasized by Radcliffe-Brown during his South African tenure at Cape Town between 1920 and 1925 and again at Grahamstown in the late 1930s. If anything, Radcliffe-Brown's antidiffusionist opinions were even stronger than those of Malinowski, and the functionalist approach was entrenched in South African social anthropology. For the English-medium students in South Africa, the sociological aspects of anthropology were divorced from historical issues, and the common processes by which societies adapt became the central element of study.

These social anthropological debates had particular importance as the idea of apartheid arose in broader South African society. Although the development of apartheid was strongly influenced by theologians and politicians (Dubow 1995), its intellectual heart was provided by the volkekundists at the University of Stellenbosch and later the University of Pretoria (Sharp 1981). These Afrikaans ethnologists were deeply suspicious of the "liberal" British anthropology, and they drew instead on German romantic ideas to provide the intellectual underpinnings of apartheid (Kuper 1983).

In the midst of the cauldron of apartheid construction, there were South African physical anthropologists, scientists whose express interest was the examination of racial variation and racial origins. Yet, in the words of Phillip Tobias, "no South African physical anthropologist was involved in providing scientific underpinning for the government's race classification processes" (Tobias 1985:32). Tobias is correct in the strict sense that no physical anthropologist submitted proposals to the government, nor did they join in the legislative or administrative process, but some were at least passively involved because their personal views of race were not very different from those being promulgated by the apartheid government. None took the explicit path of engaging in the design of the new government's racialist programs, but I would argue that much of their writings before World War II provided a fertile growth medium in which the apartheid ideology could flourish. Some of these physical anthropologists were South African born, but others came from Europe, and all of them were strongly influenced by the ideas of science prevalent at the time.

The Rise of Physical Anthropology as a Scientific Discipline in South Africa

Higher education was not available in South Africa until the turn of the twentieth century. There was a University of the Cape of Good Hope, but this was a regulating body that controlled the quality of high school education at the "Colleges" around the country, and it did not teach as a separate institution. Scientific research was primarily an activity of the museums. Bloemfontein, Kimberley, Pretoria, Grahamstown, Port Elizabeth, and Cape Town had museums housing human skeletal remains and an interest in anthropology by the first decade of the twentieth century. It was at this time that the British Association for the Advancement of Science held a joint meeting with the newly formed South African Association for the Advancement of Science in 1905. The meeting's impact on South African anthropological and scientific progress was not immediately apparent, but the meeting allowed an important cross-fertilization of ideas that was to direct the future of anthropological research and education in the union (Morris 2002).

Dubow (1995) has emphasized that physical anthropology in particular took the lead in the study of South African prehistory after the 1905 meetings. With typological approaches to studying humans emphasized by the European visitors, physical anthropology entered a new and directed phase of research with a focus on racial studies, but it did so in isolation of the growing disciplines of social anthropology and volkekunde. Physical anthropology research had begun first in the museums, but when it reached the new universities, it fell clearly within the realm of the medical faculties, and it was divorced from its social and archaeological sisters.

The first two South African educational institutions to attain university status were the South African College in Cape Town and the Victoria College in Stellenbosch, both in 1918. Each college had been providing university-level courses for about a decade under the University of the Cape of Good Hope (Boucher 1973). Cape Town College was to host the first medical school, and in anticipation, preclinical courses in anatomy and physiology were taught beginning in 1911. The majority of Cape Town's medical practitioners in the late nineteenth century had been trained at the University of Edinburgh, and it was this model that was chosen for the new medical school. Edinburgh was an elitist school in the sense that it saw itself at the peak of medical science research. The first two anatomists, R. B. Thomson (from 1911 as professor) and M. A. Drennan (from 1913 as lecturer), were drawn from that illustrious tradition. Their training in Edinburgh was in detailed comparative anatomy, human osteology, and embryology, and these subjects were to become the research focus of the new department (Tobias 1990). The first five human skeletons were not from dissected cadavers but instead from the excavation of old graves in the Richtersveld of the Northern Cape, and they became the subject of the first scientific publication from this institute (Thomson 1913). When Thomson resigned because of ill health in 1919, Drennan began his long tenure as head of department that was to last until his own retirement in 1955.

With no medical school planned for the University of Stellenbosch, human anatomy and human variation were not subjects offered, but the university did develop a strong comparative anatomy program in the Department of Zoology under C. G. S. (Con) de Villiers and his colleague C. S. Grobbelaar. De Villiers, a South African who had obtained a PhD in zoology from the University of Zurich in 1922, had studied anthropology briefly under Rudolf Martin, the author of *Lehrbuch der Anthropologie*, before Martin left for Munich in 1918. Grobbelaar began teaching in zoology at Stellenbosch at roughly the same time as de Villiers after receiving a doctorate on the anatomy of the frog *Xenopus* from Friedrich Wilhelm University in Berlin. His interest in physical anthropology was sparked initially by Robert Broom, who taught him as an undergraduate at Victoria College (Grobbelaar 1955). Grobbelaar was so interested in physical anthropology that he had actually planned to go to Munich to study anthropology in 1939. Had he done so, he would have been trained in one of the most extreme of the German schools of racial science (Spencer 1997). He did in fact spend one year at the University of Cape Town studying physical anthropology under Matthew Drennan, but he did not complete a degree. Although neither de Villiers nor Grobbelaar formally trained postgraduate physical anthropologists at Stellenbosch, Grobbelaar did offer undergraduate lectures in the subject.

The choice of Cape Town and Stellenbosch for the new universities was not without some acrimony. Pressure to start a university in the growing mining city of Johannesburg was strong from the turn of the twentieth century. When official permission was granted to begin the planning of the new University of Johannesburg (eventually called the University of the Witwatersrand), the medical faculty was the first faculty to be inaugurated. E. B. Stibbe was hired as professor of anatomy in 1919, three years before the university gained its charter. The character of the new medical school was quite different from that of its elder sister in Cape Town. The pattern in Johannesburg was along the lines of the London teaching hospitals, with the medical school quite separate from the university. The university itself was filled with the ethos of the "working class" town, and it was far less conservative than the University of Cape Town (Murray 1982). In the 1920s and 1930s, the University of the Witwatersrand was to become the center of Johannesburg economic liberalism embracing many socialist developments.

When in 1922 Stibbe was forced to resign because of an extramarital relationship that became public knowledge (Johannesburg liberalism was restricted to economic and not sexual matters), his replacement was the dynamic Raymond A. Dart. The circumstances of Dart's appointment to the chair of the Department of Anatomy in Johannesburg are well known (Murray 1982; Tobias 1984; Wheelhouse and Smithford 2001), but of great importance was that Dart's personality and energy were to make his department the most influential school of physical anthropology in the country.

Officially, the new universities of Cape Town and Stellenbosch were bilingual, but in very short order they each developed a strong linguistic bias, with Cape Town operating in English and Stellenbosch teaching in Afrikaans. The college system under the University of the Cape of Good Hope placed English as the predominant language in higher education, but after 1918, the University of the Cape of Good Hope was restructured as the University of South Africa, and each of the regional colleges that had not yet been registered as a university began to take on specific characters. The Afrikaans-language group developing at the University of Stellenbosch under the influence of the comparative anatomist de Villiers was quite different from the other Afrikaans-medium group developing in Bloemfontein under T. F. Dreyer. Dreyer was very much an independent, with a background in insect ecology and an interest in anthropology that developed as he traveled around the southern Cape and Free State in the teens and early 1920s. Dreyer had received a PhD in entomology from the University of Halle in Germany in 1911 and had returned to South Africa as an agricultural biologist. He joined Grey University College in Bloemfontein as a professor of zoology and geology in 1912 and stayed there as the college was transformed into the University of the Orange Free State in the mid-1940s. He finally retired in 1950. Dreyer began developing an interest in physical anthropology shortly after his return to South Africa, and this intensified in the 1920s with his excavation of Matjes River Cave and even more so after his discovery of the pre-Holocene human skull from

Florisbad in the early 1930s. Dreyer maintained a strong link with the National Museum in Bloemfontein, which curated his discoveries, and he worked closely with A. J. D. Meiring and A. C. Hoffman, both research staff at the museum. Although he did not train research students (with the exception of his mentoring of Meiring), Dreyer's group at the college and in the museum paralleled the physical anthropology research work being done by Drennan in Cape Town and Dart in Johannesburg.

Three distinct "schools" at Cape Town, Bloemfontein, and Johannesburg had developed by the 1930s, but there was one other physical anthropologist who worked alone and was never aligned to any particular school. That was Robert Broom.

Broom was born in 1866 in Paisley, Scotland, and attended the University of Glasgow as a student of science and medicine, graduating in 1889. As far as Broom was concerned, medicine was a vehicle that would give him an assured living and a chance to explore his real interests in zoology and geology. Broom was never a great success as a medical practitioner (Findlay 1972). He held several "locums" in South Africa from his arrival in 1897 before joining Victoria College at Stellenbosch as professor of zoology and geology from 1903 to 1910. Again returning to medicine, he took appointments in several small towns, the longest period being in Douglas in the Northern Cape. He finally joined the Transvaal Museum in Pretoria from 1933 as a researcher and enjoyed a very productive "retirement" at the Museum until his death in 1951.

Schools, Personalities, and Philosophies

The three schools of physical anthropology took on their own characters as they developed in the 1920s and 1930s. Matthew Drennan's anthropological research interests were on the origin of southern African peoples. He published widely on the subject and also produced the first South African textbook on physical anthropology (Drennan 1930). Despite his extensive publications and activities in South African scientific organizations, Drennan left few students, and much of his scientific efforts were overshadowed by the nonanthropological discoveries of his colleagues in the departments of physiology and zoology. Drennan was a teacher of detailed surgical anatomy, and his lectures only included embryology or anthropology if they could be directly applied to practical medicine (Phillips 1993). He taught no research methods to his medical students, and he produced few research students. The one student who had the potential to carry on Drennan's anthropological interests, Solly Zuckerman, did indeed develop into one of the most important anthropologists of the twentieth century, but he was disparaging about his South African roots and felt that he had learned very little under Drennan's tutelage (Morris 2009). Drennan's emphasis on practical medicine meant that his department was unattractive to research academics, and he was able to recruit no active researchers until J. A. Keen joined him in 1940 and Ronald Singer was appointed as lecturer in 1949. Drennan's research interests did bring him into contact with the developing School of African Studies at the University of Cape Town, and he collaborated with the archaeologist John Goodwin on several projects, most notably the description of the mid-Holocene remains from the Oakhurst Cave on the Cape south coast. But Drennan's interests did not extend to social anthropology, and he had little academic contact with Isaac Schapera when he took over the school in the 1930s. It was Raymond Dart who would write the "Biological Origins" chapter in Schapera's important book *The Bantu-Speaking Tribes of South Africa* in 1937 (Schapera 1937).

Unlike Drennan, in Johannesburg Dart tried to be more than just a teacher of anatomy. He initiated a separate science program in the Department of Anatomy, including the collection of specimens of anthropological interest, and as a result trained generations of research-oriented students. He infected his students with enthusiasm for the subject even though many of his ideas were outspoken and provocative (Tobias 1984; Wheelhouse and Smithford 2001). His efforts attracted skilled anthropologically oriented staff, and he was joined in 1927 by Alexander Galloway from Aberdeen and later by his own students Laurie Wells and Phillip Tobias. When a dental school was added as a university faculty, Dart's influence ensured that it, too, would be research oriented. Dart was also a key figure in the rise of the Department of Bantu Studies (the precursor of the Department of Social Anthropology) at the University of the Witwatersrand. The department was launched in 1923 with Winifred Hoernlé as the lecturer in ethnology. Hoernlé and her students struggled to deal with South Africa's growing political racialization in the 1930s. The department had been started to teach ethnography in the same manner as the volkekundist department at Stellenbosch, but it was strongly influenced by Malinowski's seminar and the radical historians and social scientists at the University of the Witwatersrand. Hoernlé's students saw their commitment to anthropology partly in political terms. They could not "avert their eyes from the realities of power and deprivation in the colonial societies, [and] they found it difficult to ignore the context of the systems which they investigated" (Kuper 1983:144). Although Dart was a contributor to the University of the Witwatersrand journal *Bantu Studies*, his writings were always strongly ethnological, and he completely avoided any political interpretations or coauthorship with those who wrote in that vein.

The school of physical anthropology initiated by Dart in Johannesburg inherited the particular biases that Dart had obtained in his own training. Dart had, by his own admission, fallen under the spell of Grafton Elliot Smith when he worked in his lab between 1919 and 1922 (Dart 1974). Smith was an extreme diffusionist who supported the concept that the essence of civilization had only occurred once, in ancient Egypt, and that all complex societies had borrowed their cultural complexities from that source. Upon his arrival in South Africa, Dart took up the diffusionist cudgel and published over 40 papers between 1924 and 1970 showing prehistoric connections between South Africa, Egypt, Europe, and Asia. Dart was adamant that the marvelous ruins of Zimbabwe could not have been built by native Africans and engaged in vigorous public debate with those who championed an indigenous origin for the archaeological complex (Hall 1990). Along with this diffusionist view, Dart also believed that KhoeSan peoples were both physically and culturally primitive, and he was responsible for popularizing the idea that the San were "living fossils" (Dubow 1995, 1996).

Both Dart and Drennan studiously avoided any political views in their writings. Neither were members of any political party. It was not as if they were "apolitical," it was just that in their view science did not sully itself with political issues. Dubow (1996) noted that he could find no real sign of any political conviction either in Dart's writings or in his actions as dean of the University of the Witwatersrand Medical School, and this is absolutely consistent with Dart's view of politics. Neither Dart nor Drennan took any part, either in support or in rejection, of the South African eugenics movement.

The eugenics movement appeared early in the century. Many of its supporters belonged to the South African Society for the Advancement of Science (Dubow 1995). A eugenics committee was set up in 1920 under H. B. Fantam, professor of zoology at the new University of the Witwatersrand. The committee strived, among other policies, to argue for the separation of races (Jenkins 1990). The movement lost support in the 1930s after the emigration of Fantam to Canada, but even if its social policies had not been popular, much of its racial policies were revived in the political platform of the National Party in the 1940s. Ultimately, the international eugenics movements were to provide the scientific basis in biology for the aggressive racial programs in the pre–World War II United States and parts of Europe, but the influence of eugenics was less in South Africa because the apartheid ideals were driven by the volkekundist concept of culture rather than the biologists' ideas of race.

T. F. Dreyer was based in Bloemfontein at the heart of one of the Afrikaner cultural centers of South Africa, but in a pattern similar to Dart and Drennan, he tended to express no political opinion in his writings. Not so his colleagues at the National Museum. E. C. N. van Hoepen, the director of the museum in the 1920s, published only in Afrikaans and encouraged his junior associates to publish in the National Museum journal *Navorsing van die Nasionale Museum*. Van Hoepen was Dutch born, but he immigrated with his parents to Pretoria in 1889 and volunteered for the Boer forces during the Second Anglo-Boer War, when he was captured and deported by the British. He obtained a PhD in geology from the University of Delft in 1908 and returned shortly after to South Africa. Van Hoepen was a difficult man who ultimately was removed from the directorship of the museum because of his constant state of loggerheads with the museum council and staff (Badenhorst 1968). He saw archaeology as a South African subject that should only be done by South Africans (presumably Afrikaans-speaking South Africans) and demanded that the Museums Association authorize the transfer of all archaeological collections in Cape Town to his curation in Bloemfontein (Mason 1989). The Museums Association ignored his request. Among van Hoepen's junior colleagues at the museum were A. C. Hoffman and A. J. D. Meiring. Hoffman had trained in zoology under Con de Villiers at Stellenbosch, but Meiring was a homegrown product of the Grey University College in Bloemfontein who was taught and mentored by Dreyer.

De Villiers's teaching in Stellenbosch was beginning to influence a generation of Afrikaans-speaking comparative anatomists who were interested in human as well as nonhuman variation. One feature of a German influence in de Villiers's work was the use of Mendelian genetics in the study of race. The approach fused Mendelian genetics and anthropometry and was based on a theory of particulate inheritance where racial "essences" could be passed on. Physical or mental traits, no matter how complex, were interpreted as unified entities whose inheritance was unaffected by environmental conditions and determined by single genes. The chief proponent of the "new" approach to racial studies was Eugen Fischer, who did an exhaustive study on the Rehoboth Basters (Fischer 1913) as an example of a Mendelian first-generation cross between two races.

V. H. Brink was a Stellenbosch undergraduate who went on to study medicine at Oxford. He had excavated a series of skeletons from old Griqua graves before his departure. While at Oxford, he did a preliminary study of these skeletons using the genetic principles of Fischer (Brink 1924), and on his return to South Africa, he completed a DSc at Stellenbosch on the vertebral columns of the skeletons with de Villiers as his supervisor. Brink's published work (Brink 1924, 1933) and that of Dreyer, Meiring, and Hoffman (1938) followed nearly identical analyses of racial traits according to the German approach. This application of genetic theory was not present in either of the Johannesburg or Cape Town schools and seems to have been an influence from Germany via Brink and de Villiers.

Dreyer was the preeminent physical anthropologist within this school, yet he seemed to be less concerned with some of the more extreme views of van Hoepen and Hoffman. In the same manner as Dart and Drennan, he tried to separate the politics from the science. To Dreyer, the issue was the advancement of South African science, not politics. His daughter, Marie Barry, remarks that her father was a supporter of both the National Party and the South African Party in the 1930s and saw himself as a South African patriot, not a political patriot.[1] He privately thought that the implementation

1. Interview with Marie Barry (née Dreyer), Oudtshoorn, May 29, 1993.

of apartheid was unnecessary because it already effectively existed. He was proud of his Afrikaner roots, yet he was happiest writing in English and always spoke of his pre–World War I experience in Germany as the most exciting time of his life.[2] Whatever his precise views on the politics of South Africa, when the Grey University College was transformed into the University of the Orange Free State in the late 1940s, Dreyer was a vocal supporter of the group who unsuccessfully petitioned the government to make it a home for both English and Afrikaans speakers. Perhaps the best way to describe Dreyer would be to include him as part of the movement toward the "South Africanisation of Science" (Dubow 2006) that would also have encompassed the approach of both Drennan and Dart.[3] Although all of these researchers perceived science as within the domain of "white" South Africans, their main desire was to put South African science and scientists on the world stage as equal to the science and scientists of their European mentors, and it had not yet been captured by the post-1948 ethnic nationalism (Dubow 2006).

On the margins of the three schools of physical anthropology sat Robert Broom. For him, the issue of modern human variation was one of secondary importance in comparison with the broad theater of evolution that he saw in the mammal-like reptile fossils. Broom was a devotee of Sir Richard Owen, and although he never met him (he did briefly correspond with him in 1890, two years before Owen's death), as a student, Broom had read all of Owen's papers on zoological subjects (Findlay 1972). Owen was an "idealist" remembered in particular for his opposition to Darwinian evolutionary theory. Idealism was a philosophical concept that searched for the general pattern, or "archetype," in a lifeform. The archetype was the unchanging and ideal pattern of the original form, and the idealist was by philosophy a "splitter" constantly engaged in the examination of detail in order to locate the anatomy of each new fossil or species in the lattice of archetypical forms. Idealists searched for the "eternal and immutable," not the "temporary and variable" (Hull 1988:45).

Broom did not follow Owen's strongly anti-Darwinian stand, but his ideas on evolution were distinctive. He believed that evolution followed a grand plan controlled by an intelligent power that continued in a predetermined evolutionary direction ending in human beings (Štrkalj 2003). The variation seen in modern humans was to be expected as part of the process of evolution, but each human sample needed to be carefully studied to identify the ideal form from which it had evolved. It is therefore no surprise to see that Broom's approach was hypertypological and that each individual could be quantified as a mixture of very precise archetypes representing the original patterns now confused by intermixture (Štrkalj 2000).

South African Physical Anthropologists and the Age of Typology: 1920–1955

A characteristic of nearly all of the work of Dart, Broom, Dreyer, Drennan, and their associates and students was that very few individuals were described in the definition of these types, and statistics were almost never used in the description of new specimens. The reason is that the research in the museums and the medical schools was solidly in the typological mold, and large samples were not needed to make firm conclusions.

The Linnaean vision of type descriptions had been firmly rooted in the field of anthropology by the mid-nineteenth century. The "type" was defined as an ideal individual who possessed all of the important characteristics of the race. Hence, the focus was only on those features that could differentiate between races. The place of the individual was well defined in typology. Because the type was an ideal standard, the individual could be compared with the type and his or her purity assessed. Variations were impurities, but the characteristics of the type could still be dissected out through careful observation by the skilled observer (Stepan 1982).

The advent of typology on the South African scene was most evident after the discovery of the Boskop skull in 1913. This mineralized partial cranium was assumed to be of great antiquity. Its large size was associated with what were seen as San features, and this suggested that it was different from previously described human crania and living southern African peoples. When F. W. FitzSimons of the Port Elizabeth Museum sent the skeletons from the deepest layers of his coastal caves to Dart, the Boskop-like features of these few individuals suggested to Dart that a whole population of large-headed San must have existed in prehistoric times. Dart created the "Boskop type" in order to represent this population (Dart 1923).

To Dart and his students, the Boskop type was a new race comparable to other living races of South Africa. Alexander Galloway became a particular convert, as did Laurie Wells. Broom even took an opportunity to describe the importance of the specimen in 1918. Drennan in Cape Town identified a modern cadaver from his dissection room as a Boskop man (Galloway 1937). This extension of an extinct race into the present was completed when Dart identified the presence of Boskop ancestry in a group of San from the northern borders of the Cape Province in the Kalahari Gemsbok Park (Dart 1937). Dart assessed each individual according to his or her percentage "Bushman" or "Boskop" ancestry. Features that fitted neither of these preconceptions were said to be signs of intermixture from Bantu-speaking, Mediterranean (Hamitic), Armenoid, or Mongoloid peoples. Dart's racial ex-

2. Interview with Baz Edmeades (Dreyer's grandson), Cape Town, December 1992.

3. Dreyer was also a nationalist in the ecological sense in that he wanted to use science to improve the lives of South Africans. He was the treasurer of the Kalahari Thirstland Redemption Society of E. H. L. Schwarz, which had a grand plan to redirect the waters of South African rivers to flood the Kalahari so that rainfall would be increased and agriculture throughout the subcontinent improved (Eales 2007).

planation was a direct reflection of his cultural diffusionist beliefs. He interpreted each physical feature as if it were a cultural artefact passed genetically from "hand to hand" by a flow of visitors.

The stage was now set for the flowering of typology in South African physical anthropology. Robert Broom applied the same typological methods he used in his studies of mammal-like reptiles to his studies of humans. He searched for racial "essences" on the crania of his archaeological skeletons and tried to reconstruct past racial history from these features. He added a new and fundamentally more ancient type than "Bush" or "Boskop" in his Australoid Koranas (Broom 1929, 1941). Broom hypothesized that the robust features he saw in the living Korana were the last remnants of a very ancient genetic strain akin to the Australian Aboriginals who had lived in Africa as part of a worldwide primitive race in ancient times. Broom could therefore characterize any individual as possessing percentage ancestry according to genetic lines of "Australoid," "Negroid," "Bush," or "Boskop," giving him a typological model almost as complex as that of Dart.

This passion for types continued during the 1940s and early 1950s with a particular focus on the origin of KhoeSan peoples. T. F. Dreyer and his junior colleague A. J. D. Meiring from the National Museum in Bloemfontein proposed that the living Khoekhoe were descendants of Hamitic migrants from the north, and they set about proving this by excavating a series of graves from along the banks of the Orange River near the town of Kakamas (Dreyer and Meiring 1937). A typologically selected set of five crania became the "Kakamas type," and this formed the basis for their expectation of what the North African Hamitic ancestor should look like. When some of their ideas were debated by Broom and Wells, they accused these foreign-born researchers of not understanding the term "Hottentot" in the same manner as "used by South Africans born in the country" (Dreyer and Meiring 1952:19). This was a sign of the Afrikaner nationalism that was becoming a factor in Bloemfontein.[4]

In a summary paper on the origin of the Khoekhoe written in 1955, Tobias (1955) tried to create some order out of the profusion of physical types. He identified no less than eight genetic lines crossing and combining to produce the living KhoeSan and the Bantu-speaking peoples of southern Africa. Although Tobias accepted the various types listed by previous workers, it was clear that he struggled to make sense of the confusion, and this was nearly the last of his published papers that accepted the typological model without criticism.

The Crumbling of the Typological Edifice

Typological assessments of race origins for living South Africans were still at their height when the National Party won the 1948 elections and began to implement its racial policy of apartheid. Racial discrimination was not a new thing in South Africa, and rural land tenure and voting rights were already racially segregated by 1948, but what had changed was the attempt to implement all governmental and social policies on a racial basis.

Dart had had a nervous breakdown in Johannesburg in 1943, and when he returned to the department after a long leave, he started to take less of an active role in teaching and student supervision (Wheelhouse and Smithford 2001). Laurie Wells took over much of his anatomical teaching duties, and the young, newly graduated Phillip Tobias took up the task when Wells left for a readership in physical anthropology at Edinburgh. Dart's research interests were also moving toward the new australopithecine site of Makapansgat, and he published relatively few typological or diffusionist papers after 1950. Dreyer was nearing retirement age, and neither he nor Broom, who died in 1951, had produced students to take up the typological message on their behalf. Dreyer's colleague Hoffman had taken over the directorship of the museum, but he was not of the same academic standard as Dreyer and produced little research of note as director. In Cape Town, Drennan was also reaching retirement age, but a combination of having new research-oriented staff and the discovery of the new exciting fossil specimen at Elandsfontein gave anthropology in the department a new impetus, but not in a specifically typological mode. The new generation of physical anthropologists taking over from old guard was represented by Ron Singer in Cape Town and Phillip Tobias in Johannesburg.

Ronald Singer had joined the Department of Anatomy in Cape Town in 1949 as a lecturer. He was interested in embryology, but when Drennan refused to support his application for the expensive equipment needed for embryological research, Singer focused instead on human evolution and variation (Morris and Tobias 1997). With an approach based on genetics, Singer soon realized that typology was a false trail. He presented a paper on the "Boskop 'Race' Problem" in Philadelphia in 1956 (Singer 1958) in which he rejected this mythical race and suggested instead that the various types created by Dart, Dreyer, Broom, and others were just part of the range of normal variation seen in KhoeSan and Negro populations. Although Singer was not overtly political, he was very disturbed by the refusal of the universities to take more than a symbolic stand against the imposition of racial segregation in higher education in 1959. This, along with better career prospects in the United States, led him to leave Cape Town for Chicago in the early 1960s, and his influence on future directions in South African physical anthropology was therefore limited (Morris 2005).[5]

4. Dreyer's daughter, Marie Barry, says that toward the end of his life, Dreyer was offered an honorary degree from the University of the Witwatersrand but refused the opportunity because he did not want to be linked to Dart's school in Johannesburg.

5. Singer had applied for the chair of anatomy in Cape Town in 1955, but the position had been given to Laurie Wells on his return from the United Kingdom. He also applied for the chair of anatomy at the new University of Stellenbosch Medical School in 1956. The selection com-

In Johannesburg, the young Phillip Tobias was far more explicit in his opposition to the racial discrimination of the government. As a student in the late 1940s, Tobias, a member of the National Union of South African Students, was a vocal critic of the government's race policies. The teachings of Dart on typology were very influential in Johannesburg, and there appears to have been a lag between the onset of Tobias's political awareness and his scientific rejection of typology (Morris 2005). He noted his disquiet about race determination in a paper in 1953, but his writings were still very typological until 1959. His real break with typology came in 1961 with the publication of *The Meaning of Race* (Tobias 1961) by the antisegregationist South African Institute of Race Relations. This small pamphlet, revised and enlarged in 1972, articulated a genetical model of population variation and directly argued against the policy of classification as performed by the South African government.

The common factor shared by Tobias and Singer was a background in genetics, a subject not well understood by the physical anthropologists of the 1930s and 1940s. Both workers also had opportunities to gather data on large populations in the field. For Tobias, this had occurred in 1952 when he joined the Rene Panhard expedition to the Kalahari. For Singer, it was when he helped to gather serological data on the Malagasy in Madagascar in 1954. In addition, both Singer and Tobias benefited from the post-typological development of the "New Physical Anthropology" (Washburn 1951) when they traveled to conferences and on fellowships to the United States and Europe. This new approach, which was launched in the early 1950s, looked at variation from a statistical view and concentrated on genetics and population dynamics rather than static types.

Despite the progressive ideals propagated especially by Tobias and his students since the 1960s, the new physical anthropology had virtually no impact on the policies of the apartheid government. Among the reasons for this were that it came too late to affect the formulation of nationalist government policy, but even if it had, the government would have ignored it because the Afrikaans-speaking volkekundists held sway. The theoretical base of apartheid did not require biological data because the practitioners of apartheid formed their arguments purely in terms of a need for "cultural separation." Because they firmly believed that culture was an outcome of biological heritage, cultural attributes reflected the biological underpinnings. When they needed biological information, the typological data collected in the works of the physical anthropologists was readily available. This was in essence the legacy left by the typologists.

Typology and diffusionism as practiced by the South African physical anthropologists in the first half of the twentieth century provided support for apartheid in three distinct ways. (1) Diffusionism ensured that living African peoples were seen as existing in a kind of backwater. Negroid peoples were dismissed from the mainstream of history and denied a level of civilization, and KhoeSan people were portrayed as primitives. (2) Typology provided an unambiguous mechanism for identifying and separating groups on morphological grounds and ensured that the average South African did not easily perceive any scientific argument against the policy. The typologists still saw "racial history" as the main focus of anthropological research and chose to avoid statistical analyses precisely because the complex mathematical techniques dealt with variation more than with racial identity (Stepan 1982). (3) The separation of social anthropology from physical anthropology allowed the physical anthropologists to ignore the social issues as a point of debate. It was the sociocultural specialists who became involved in both the development and criticism of apartheid. For Dart, Drennan, and most of the other physical anthropologists, science was somehow separated from politics. Social anthropologists were there to deal with the social issues, while scientists dealt only with the facts of biology.

With the exception of Tobias and to a much lesser extent Singer, scientists active at the start of National Party rule did not make the jump from scientific data to scientific activism. Perhaps some of them privately did not support the new policy, but none of them took any practical action. There were absolutely no editorial other published comments about the new policy on race in the pages of the *South African Journal of Science* between the years 1948 and 1955, and both it and the *South African Medical Journal* continued to be uncritical of the nature of categories in their medico-anthropological articles right up to the late 1980s (Ellison and de Wet 1997). The near total separation of the schools of social anthropology from those of physical anthropology prevented the necessary cross-fertilization that would have perhaps sparked this critical debate.

The implementation of apartheid after 1948 was a political process that was out of step with most of the post–World War II world. The advent of the "New Physical Anthropology" in the 1950s did indeed influence the ideas of Tobias and others as the decade progressed, but it had no influence on the structure of the apartheid legislation as it was applied by the government. The ideology that supported apartheid came out of the central European theory of "ethnos" as imported to South Africa by the volkekundists, and it was supported by the parallel imports of typology and diffusionism. Although the local physical anthropologists did create novel ideas of their own and formed a kind of South African scientific identity (especially for the Afrikaans-speaking scientists),

mittee chose him as the leading candidate, but the senate refused to confirm the appointment because they did not like the idea of a first-language English speaker of Jewish origin in this premier Afrikaner establishment. They overrode the selection committee and appointed J. F. Kirsten, a zoologist with no human anatomy experience but who was a member of the Broederbond (Serfontein 1979). Singer also applied for the chair at the University of the Witwatersrand on the retirement of Dart in 1959 and was short-listed along with Tobias and Joe Weiner from Oxford. The committee ultimately chose Tobias, and as a result, Singer's prospects for a chair in South Africa looked bleak.

it was the European baggage of ethnos, typology, and diffusionism that colored much of the South African research.

Apartheid as a form of government died when race was no longer recorded on the population register after 1991, but physical anthropologists steeped in the "New Anthropology" models of clinal variation and indistinct racial boundaries are finding that the public conception of race still remains firmly in a typological mold. The post-1994 South African government calls itself a nonracial democracy, yet it faces a conundrum in trying to correct the inequalities of the past if it does not still consider the historic apartheid racial categories. How is it possible to apply redress and redistribution if racial categories are no longer considered? Ethnic identities are resurfacing as communities contest access to a "fair share" of the economic pie. In some circles, Afrocentric nationalism is replacing Afrikaner nationalism in a form just as aggressive and inflexible as the earlier form. The lesson from the past is that physical anthropology is not strictly biological and that we can never be disconnected from the effects of the social dynamics of the times we live in.

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The Origins of Anthropological Genetics

by Jonathan Marks

Although we often date the conflict of "molecules and morphology" in biological anthropology to the 1962 Wenner-Gren conference "Classification and Human Evolution," the roots of the conflict extend considerably deeper. In the first half of the twentieth century, two established research traditions applied genetic data to problems in physical anthropology: racial serology and systematic serology. These had a tense relationship with the more mainstream areas of racial anthropology and primate taxonomy. Both produced conclusions that were often difficult to reconcile with traditional physical anthropology but that laid claim to the authoritative voices of genetics and evolution. They were also less relevant and less threatening to general anthropology than the other movement for the application of genetics to anthropological problems—eugenics—had been. I discuss the relations of genetics to anthropology as manifested in the areas of eugenics, race, and primate taxonomy in the early twentieth century and the field's transformation into anthropological genetics in the 1960s.

Introduction

There is a mythic history of the intersection of genetics and anthropology. One half concerns the zoological relationships of humans as a species to other species. In this story the crude similarity of human blood (and presumably therefore genes) to ape blood was noted at the turn of the twentieth century but largely ignored until the 1960s, when Morris Goodman finally correctly apprehended the phylogenetic intimacy of humans and African apes. Thus, after Nuttall's work in the early 1900s, "Nothing much happened for the next sixty years, except perhaps that people tended to forget the genetic intimacy between humans and the African apes" (Lewin 1987: 106; see also Goodman and Cronin 1982).

The other half of the mythic history concerns the use of genetic data to study the products of human microevolution. In this story, cultural anthropologists were naturally averse to genetics (which is, after all, science), and aside from work during World War I, anthropology generally ignored genetics, again until the early 1960s, in this case led by heroic figures such as James Neel and Luca Cavalli-Sforza (e.g., Pollitzer 1981). Thus, in the introductory chapter of a recent textbook (Crawford 2007) of anthropological genetics, we read that

> Ludwik Hirschfeld and Hanka Hirschfeld (1919), during World War I, demonstrated that military personnel of various so-called "racial groups" or ethnicities differed in the frequencies of the ABO blood groups. In the few decades that followed, additional blood group systems . . . were shown to vary in human populations. (4)
>
> Unfortunately, until the 1950s, there were few anthropologists with adequate training in human genetics. The reason behind this paucity was that most physical anthropologists were traditionally trained in morphology and racial classification based on typology. (7)

Both of these mythic histories are notable for what they omit. In particular they omit the active research programs of systematic serology and racial serology, the vexed conclusions often drawn by practitioners, the troubled state of human genetics before World War II, and the intellectual shifts in the 1960s and 1980s that resulted in the Human Genome Project and the unprecedented privileging of genetic data. These are the issues I will address in this paper.

Early Human Genetics as the Unanthropology

In the early twentieth century, Franz Boas transformed American anthropology in large part by infusing it with the German liberal humanism of Rudolf Virchow. Virchow was distrustful of naturalistic theories of human history, in particular those of his former assistant Ernst Haeckel (1876 [1868]). Haeckel's evolutionary theory held the "Indo-Germanic" branch of the Mediterranean (i.e., Caucasian) race to be the highest form of life and aggressively dehumanized the rest of the human species in Darwin's name:

> Of course the relative number of the twelve species [of people] fluctuates every year, and that too according to the law developed by Darwin, that in the struggle for life the more highly developed, the more favoured and larger group of forms, possess the positive inclination and the certain ten-

Jonathan Marks is Professor of Anthropology, Department of Anthropology, University of North Carolina at Charlotte (Charlotte, North Carolina 28223-0001, U.S.A. [jmarks@uncc.edu]). This paper was submitted 27 X 10, accepted 16 VIII 11, and electronically published 7 II 12.

 0011-3204/2012/53S5-0014$10.00. DOI: 10.1086/662333

> dency to spread more and more at the expense of the lower, more backward, and smaller groups. Thus the Mediterranean species, and within it the Indo-Germanic, have by means of the higher development of their brain surpassed all the other races and species in the struggle for life, and have already spread the net of their dominion over the whole globe. (324)

> The various branches of the Indo-Germanic race have deviated furthest from the common primary form of ape-like men. . . . [It is the Germans and the English] who are in the present age laying the foundation for a new period of higher mental development, in the recognition and completion of the theory of descent. (332)

Virchow, on the other hand, demonstrated empirically in the 1870s that various European peoples could not reliably be categorized by a single skull type and that the Aryan or Teutonic appearance was present in only a small minority of Germans. Nor did he embrace the thesis of Darwinian racial superiority with the vigor of Haeckel (the subtitle of *The Origin of Species* was *The Preservation of Favoured Races in the Struggle for Life*, after all). Indeed, the knowledge of "how to differentiate between exclusively national politics and universal human science" was specifically what Virchow (1872) had mocked French anthropology for lacking after the Franco-Prussian War (Manias 2009). Virchow then notoriously called for evolution, or at least whatever Haeckel was speaking on behalf of, not to be taught in schools. In this he found an unlikely ally in Thomas Huxley (1879), who harbored his own ambivalences about the need to teach evolution:

> It is not that I think the evidence of that doctrine insufficient, but that I doubt whether it is the business of a teacher to plunge the young mind into difficult problems concerning the origin of the existing condition of things. I am disposed to think that the brief period of school-life would be better spent in obtaining an acquaintance with nature, as it is; in fact, in laying a firm foundation for the further knowledge which is needed for the critical examination of the dogmas, whether scientific or anti-scientific, which are presented to the adult mind. (xvii)

Thus Virchow's protégé Franz Boas emigrated to the United States with an intellectual inheritance of (1) an appreciation for the racial type as an empirical fallacy, (2) a distrust of biologistic explanations of human social difference, and especially (3) a distrust of the invocation of Darwin in support of the doctrine of racial inequality. In New York a generation later, Boas would play Virchow to the geneticist Charles Davenport's Haeckel. Davenport, the leading human geneticist in America, published his major work (*Heredity in Relation to Eugenics*) the same year as Boas published *The Mind of Primitive Man* (1911). Where Boas made it clear that history is not driven by the gene pool and is not explained by racial endowments, Davenport made quite the opposite claim, that human groups and social classes differed in their fundamental genetic worth, which in turn explained their political and economic status.

Five years later, Davenport's friend, the naturalist Madison Grant, published a best seller, *The Passing of the Great Race*, which explained human history in terms of the racial superiority of the Nordics and called for the immediate sterilization of the American unfit, "extending gradually . . . and perhaps ultimately to worthless race types" (Grant 1916:47). In 1916, Boas not only published a comprehensive critique of Davenport's cherished eugenics program but also a devastating review of Madison Grant's book in the *New Republic*. Nevertheless they all served together on the National Research Council, vying to control the intellectual direction anthropology would take. And even as British eugenicists vilified Davenport's work and ideas in scholarly and public forums as early as 1912, Davenport retained the power and authority as the leading researcher in American human genetics (Spencer and Paul 1998).

The point I am trying to make is that in the early decades of the twentieth century, the antiracist anthropology that Boas was attempting to establish was being aggressively counterbalanced by a racist anthropology predicated on a fanciful view of heredity and nevertheless promoted by the leading authorities and spokespeople for human genetics. With the principal exception of Columbia's fruit-fly geneticist Thomas Hunt Morgan, most geneticists followed the lead of Davenport and Grant, serving under them on the American Eugenics Society and reviewing their work favorably in scientific forums.

It was not at all clear that human genetics was relevant to or even compatible with the scholarly study of the human species. It was little more than a scientific instrument to oppress the poor and marginalized, as Clarence Darrow came to realize during the Scopes Trial (Darrow 1925, 1926). Lawyers and anthropologists were able to see the poverty of reasoning that suffused the field of human genetics far more clearly than the geneticists could.

Physical Anthropology and Racial Serology

Physical anthropology was only slightly more welcoming to human genetics than was cultural anthropology. Aleš Hrdlička and Earnest Hooton were among those who served below Madison Grant on the American Eugenics Society, which successfully represented itself as having a scientific biological solution to America's social problems. While Hrdlička complained privately about Grant, he was willing to accommodate himself to Grant so long as Grant financially supported his interests in professionalizing physical anthropology. The vagaries of history are such that when it became clear that Grant would not underwrite the *American Journal of Physical Anthropology*, Hrdlička booted him off the founding editorial board and replaced him with Boas (Spiro 2009). Hooton, for his part, remained an avid eugenicist long after it fell out of fashion. He wrote Grant in 1933 to thank him for a copy of his latest book and added "I don't expect that I shall agree with you at every point, but you are probably aware that I have a basic sympathy for you in your opposition to the flooding of this country with alien scum."[1] But after Grant's death, Hooton (1940) mocked him:

> Madison Grant had a vivid personality and a long head, but, as I remember him, rather a swarthy complexion. I was curious about his conception of Nordicism; so I tackled him on the subject of my own racial type. I said, "Mr. Grant, I have a round head with a cephalic index of 85, brown hair, mixed eyes, a moon face and a blobby nose—all these attractive features going with a muddy complexion. How would you classify me as to race? I should call myself a mixed Alpine." He asked, "Are you not of purely British ancestry?" I replied, "Yes, my father is an Englishman and my mother is a Scotch Canadian." He said, "Then, damn it, you're a Nordic." That is the only occasion when I have been so classified. (184)

Charles Davenport, on the other hand, remained in high repute within physical anthropology. Although his work had been publicly ridiculed by British eugenicists in 1912 and his eugenical ideas had precipitously fallen out of favor within the American genetics community by the mid-1930s, Davenport could still be elected president of the American Association of Physical Anthropologists—a position he held at the time of his death in 1944, on Hooton's nomination.

The research in human genetics that most interested the leaders of physical anthropology was, however, of a different sort. Physical anthropology's primary research question was race, but the most pressing methodological question was the choice of inherited features by which to identify it. The ABO blood group, discovered at the turn of the century, afforded such a stably inherited Mendelian feature. The variation across human populations was studied during World War I by Ludwik and Hanka Hirszfeld (Hirschfeld), initiating the study of racial serology (Schneider 1995).

The problem faced by the field of racial serology as it gained momentum in the 1920s was that the entities it identified were not recognizably racial. Using the frequencies of the blood-group phenotypes, the Hirszfelds managed to divide the human species—based on its principal combatants—crudely into European, Intermediate, and Asio-African. A few years later, using allele frequencies, Laurence Snyder (1926) partitioned the human species into European, Intermediate, Hunan, Indo-Manchurian, Africo-Malaysian, Pacific-American, and Australian. These genetic divisions, however, were exceedingly arbitrary, sometimes self-contradictory, and difficult to relate to the general racial groups with which physical anthropologists were familiar.

By the late 1920s, physical anthropologists were beginning to throw up their hands in despair at the data of racial serology (Mendes-Corrêa 1926; Young 1928). Hooton's 1931 textbook (Hooton 1931) reviewed the area and concluded that "the fact that some of the most physically diverse types of mankind are well nigh indistinguishable from one another [serologically] is very discouraging. At present it seems that blood groupings are inherited quite independently of any of the physical features whereby we determine race" (490). Likewise, Alfred Kroeber's (1933) general textbook: "It is clear that we have in these blood-group occurrences an astonishing set of data which may yet profoundly modify the current ideas of race relationships, but which for the present are more provocatively puzzling than illuminating" (12).

Worse yet, in addition to blood-group data—whose data were real, even if cryptic—there were other sorts of blood studies whose data were equally cryptic and less real. Thus, one of the biological rages of the late 1920s involved a Russian hematologist who claimed to be able to tell male blood from female blood by adding chemicals, shaking it up, and observing what color it turned. The Manoilov Blood Test was discussed in major scientific forums, worked just as well on plants (in spite of their lack of blood), and could also be adapted for the determination of race and sexual preference in humans (Marks 1995; Naidoo, Štrkalj, and Daly 2007). Through the intervention of Charles Davenport's assistant, the geneticist Harry Laughlin, Hrdlička published the work in the *American Journal of Physical Anthropology*,[2] explaining the procedure by which Manoilov's laboratory in Leningrad could distinguish the blood of races such as Russians, Jews, Poles, and Latvians from one another. Here, the results were not so much uninterpretable as impossible: to Hooton, these blood tests "do not inspire confidence. . . . The test seems to prove too much. It is inconceivable that all nationalities, which are principally linguistic and political groups, should be racially and physiologically distinct. Jews, for example, are not racially pure, but extremely heterogeneous; so are Russians, Poles, and Letts" (Hooton 1931:491).

Hooton's skepticism in his 1931 physical anthropology textbook can be profitably contrasted with the naïveté expressed in a textbook of genetics published the same year: "According to Manoiloff, the oxidizing process in a certain blood reaction occurs more quickly in Jewish blood than in Russian blood; tests of race based on this difference proved correct in 91.7 per cent of cases" (Shull 1931:299). Knowing a bit about the nature and composition of human groups turned out to be useful for gauging the reliability of the Manoilov Blood Test, decimal point or no.

Blood, a metaphor for heredity itself, was indeed a very

1. Hooton to M. Grant, November 3, 1933, E. A. Hooton Papers, Peabody Museum, Harvard University.

2. Harry H. Laughlin to Hrdlička, June 15, 1926; October 5, 1926; October 19, 1929, Aleš Hrdlička Papers, National Anthropological Archives, Smithsonian Institution.

special juice.[3] Physical anthropology was on the horns of a dilemma—it wanted stable hereditary markers for the discrimination of race, but the most stable hereditary markers available produced racial nonsense. Moreover, the sense and nonsense produced by genetic analyses were often inseparable from one another, and the geneticists themselves seemed either unwilling or incapable of making that distinction. It would not be for several decades until the constructedness of race itself would be appreciated and would explain the lack of fit between genetic patterns and racial patterns (see below). That was not, however, how that lack of fit would be understood within racial serology, that is to say, by the first generation of human population genetics.

They reasoned instead that their own data superseded all others. Of course, it was rarely if ever articulated so baldly, but the message came through. J. B. S. Haldane and Grafton Elliot Smith debated the hegemony of genetic data following Haldane's presentation at the Royal Anthropological Society in 1932. It arose again in the pages of *Science* in 1946 on the placement of Oceanic peoples among the Mongoloids (Montagu 1946; Wiener 1946). And a few years later, it resurfaced in the *American Journal of Physical Anthropology* (Birdsell 1952; Stewart 1951; Strandskov and Washburn 1951) in response to a proclamation by the serological geneticist William C. Boyd (1947) that "it would seem hardly too much to say that serology (or rather, genetics), is destined to oust craniometry and anthropometry as the main tool of racial anthropology" (46).

This was, to some extent, turf patrolling, but the turf was the epistemic ground separating narrow and reductive, if trendy, research from scholarly and comprehensive, if hoary, knowledge. Moreover, not only was there something vulgar in the geneticists' uncritical self-promotion, but also they were not even actually extracting race from their data—they were superimposing race on their data and merely describing the results (Rowe 1950).

The field of racial serology effectively died off with Boyd's (1963) review in *Science*, which identified thirteen serological races—one African, two Asian, five European, one American, and four Oceanic. The cultural edifice that underlay seeing one kind of African but five kinds of European became a bit more obvious during the era of the civil rights movement.

In sum, the use of genetic data to address questions in physical anthropology had a long if not particularly distinguished history spanning half a century before being reinvented as human population genetics. The problem was that aside from self-interested rhetoric, genetic research did not seem to have anything to add to the corpus of physical anthropology that was either not obviously false or manifestly useless aside from documenting additional differences among human populations. If there was a lack of enthusiasm within physical anthropology for genetics, it was not for lack of interest; the meaning of the work for understanding what physical anthropologists were primarily interested in was simply very unclear.

Physical Anthropology and Systematic Serology

Cultural anthropologists were put off genetics for its social program and unhistorical interpretations of history; physical anthropologists were put off genetics for its simultaneous meaninglessness and claims to transcendence. There was another anthropological question where genetic data might prove valuable: that of what Huxley called "man's place in nature," or more generally, of the relationships of primate species to one another.

As noted earlier, anthropologists were familiar with George H. F. Nuttall's work on the blood reactions of different species, including humans. (His sister Zelia was a respected archaeologist of Mexico.) The fact that human and chimpanzee bloods appeared to be more similar to one another than the horse and donkey bloods was brandished at the time of the Scopes trial as evidence of our kinship to the apes (Hussey 1926).

What is sometimes lost in the mythic history of molecular anthropology is the fact that the close relationship of human and ape was long known and was consequently not particularly threatening. A Roman poet named Quintus Ennius had commented around 200 BC on "how similar we are to the monkey, the most horrid beast" (*simia quam similis turpissima bestia nobis*); the remark was preserved by Cicero (*On the Nature of the Gods*, 1.35) and quoted in Francis Bacon's *New Instrument* (*Novum Organum*, 1620) and Carl Linnaeus's *System of Nature* (*Systema Naturae*, 10th edition, 1758), both widely read and highly influential works. The similarity of human and ape is no surprise; what is surprising is that anyone could deny their differences. That denial would have to wait for the emergence of molecular anthropology in the 1960s and from the same kinds of data as the systematic serologists used in the previous decades.

Most of the time, the blood data showed relationships among species that paralleled those derived from classical anatomy. Thus, Nuttall (1902) confirmed the evolutionary distance of the Platyrrhini but was stumped by how distant the lemurs appeared. He coyly suggested that perhaps the lemurs ought to be removed from the order Primates, but of course this simply recapitulates the practice of the racial serologists, assuming that their data transcend all others. The problem, however, is a significant epistemological one: when the blood/genetic data are harmonious with the traditional/anatomical data, we accept them both; but when they are discordant, how do we know which to believe? There have to be checks and balances for the genetic data (Gregory 1917). In fact the blood tests were not at all simple to execute or to interpret and often required extensive hermeneutics. Apparently tarsier blood also failed the test (Hartman 1939); should

3. Goethe, *Faust*, line 1740. This line, spoken by Mephistopheles, was used as an epigram by Nuttall (1904) and Boyden (1951).

tarsiers also be removed from the Primates, the testimonies of their bodies notwithstanding?

The most respected practitioner of systematic serology from about 1930 to 1960 was Alan Boyden. Boyden maintained cordial relationships with morphological systematists and was frank about the limitations of serology, being "no simple guide to animal relationship. The very complexity of the problem demands the use of all possible pertinent data. The data of systematic serology, where comparable methods are used, are as valid as those of systematic morphology, and the two methods of analysis should be considered complementary to each other" (Boyden 1942:141–142).

Hooton's revised edition of *Up from the Ape* (Hooton 1946) invoked the serological data to help position the human species in the natural order. In particular, Hooton presented the work of Christian von Krogh of Munich, who had pursued the study of the serological intimacy of human and ape. Hooton went on: "The weak similarity of the orang to other species suggests a lengthy process of separate development for this animal and its early branching off from the stock of chimp and man" (45). This is noteworthy in two ways. First, this specific inference would be highlighted as a radical and unanticipated discovery of molecular anthropology a generation later. And second, it implied nothing to Hooton about the classification of the primates: "The differences between man and the great apes are enough to justify us in recognizing a separate family for man, the Hominidae" (47).

Disciplinary Transformations

After World War II, the fields of human genetics and physical anthropology were in disrepute and needed to be reinvented. Hooton (1936) had struggled in vain to differentiate good American physical anthropology from bad German physical anthropology; his student Sherwood Washburn (1951) would proclaim a "new physical anthropology" focused on evolutionary process, human adaptability, and nonhuman primates. In parallel, James Neel would help construct a human genetics that focused on medical rather than social pathologies; that was oriented toward helping the family, not the race; and that exposed patients to optional services, not coercive surgery.

"Molecular anthropology" was coined at a Wenner-Gren conference organized by Washburn in 1962, "Classification and Human Evolution." Two significant claims were raised at this conference held in Burg Wartenstein, Austria. First, Emile Zuckerkandl (1963) argued that because the protein sequences of human and gorilla hemoglobin were so similar, we ought to privilege "the point of view of hemoglobin" and regard humans and gorillas themselves as slight variants of one another. Second, Morris Goodman (1963) serologically rediscovered the genetic intimacy of human and chimpanzee and the greater evolutionary distance to the orangutan, and he argued to reclassify them on that basis.

Neither claim was particularly well received. Paleontologist Louis Leakey, for example, "at times found it hard to be patient with the views of some of my colleagues." Primate anatomist Adolph Schultz acknowledged that "some of my comments on tentative conclusions may have sounded rather critical."[4] G. G. Simpson (1963, 1964) could not imagine privileging the point of view of hemoglobin over that of the hallux, ilium, or gall bladder, nor privileging the genetic similarity of human and chimp over the ecological difference. Under the existing principles of animal taxonomy, which he had recently summarized in a book called *Principles of Animal Taxonomy* (Simpson 1961), the optimal scientific product was one that best encapsulated the diverse glimpses afforded by different approaches and data sets. Thus genetics, and more generally phylogeny, was simply a piece of a puzzle, the puzzle of representing evolutionary relationships, which subsume both descent and divergence, and encoding them in a simple linguistic framework. This was a tenet of what Julian Huxley had called "the new systematics," for which Simpson had emerged as the principal spokesman. The new systematists had recently repulsed a challenge from the numerical taxonomists (Hull 1988), who were at least biologically competent, if philosophically at odds with contemporary practice. But in privileging genetic data over all others (notwithstanding the crass self-interest in doing so), particularly data in which the differences between human and ape are not readily visible, and arbitrarily privileging phylogenetic relationships over all other kinds of relationships, Zuckerkandl and Goodman were seen by the synthetic theorists as simply biologically incompetent and best left to their biochemistry.[5]

Goodman later accused Simpson of rejecting his classification for reasons of anthropocentric and antievolutionary prejudice (Goodman 1996; Hagen 2009). Zuckerkandl wrote it off to a prejudice against genetics (Aronson 2002; Dietrich 1998; Sommer 2008; Suárez-Díaz and Anaya-Muñoz 2008). In fact, it was the arrogance and ignorance behind the claims themselves that put the systematists off molecular anthropology. Alan Boyden was no less dismissive of Goodman's interpretations than Simpson was (Boyden 1973; Hagen 2009).

The late 1960s brought the great triumph of molecular anthropology, Sarich and Wilson's demonstration that leading paleontologists had grossly misrepresented the significance of the fossil *Ramapithecus* to human evolution, for *Ramapithecus* was about three times as ancient as the human lineage was. This discovery did not necessarily have any bearing on either the value of the viewpoint of hemoglobin (the similarity of the blood and the intimacy of the biological history implied by that similarity were familiar but newly quantified; and Goodman himself rejected the molecular clock) or the ne-

4. L. S. B. Leakey to Lita Osmundsen, July 24, 1962; Adolph Schultz to Lita Osmundsen, July 23, 1962, Wenner-Gren Foundation for Anthropological Research.

5. Ironically, the field of numerical taxonomy had little interest in the molecular anthropological work. It was predicated on the analysis of bodies, not biomolecules, and undervalued phylogeny.

cessity of reclassifying the apes (aside from *Ramapithecus*) on that basis. The viewpoint of hemoglobin, however, would come to be increasingly privileged in the 1980s, in lockstep with the rise of the Human Genome Project and its attendant public relations campaign, which came to be known as "genohype" (García-Sancho 2007; Holtzman 1999). By 1992, Jared Diamond could parlay Zuckerkandl's inability to tell an ape from a human genetically into the central argument of his best seller, *The Third Chimpanzee.* And the rise of phylogenetic systematics (Eldredge and Cracraft 1980; Hennig 1965), radically revising the premises of classificatory practice in biology, would make Morris Goodman into a prophet, as opposed to having simply misunderstood the principles of contemporary systematics as they existed in the 1960s.

The point is that the rise of molecular anthropology in the 1980s had less to do with discoveries and data and far more to do with changing epistemic assumptions within evolutionary biology (Marks 2009). In particular, the decade of the Human Genome Project came with a higher privilege accorded to genetic data and relations (not to mention a higher privilege accorded to genetic explanations for human behavior; see below). The simultaneous privilege accorded to cladistic classification—that is to say classifying only by descent with no attention given to divergence—also placed a premium on genetic data, which tend to preserve a retrievable record of descent moreso than of adaptive divergence.

The 1960s saw the decline of racial serology in parallel with the ascendance of Washburn's "new physical anthropology"—refocusing human biology on the common themes of being human at the expense of the old pseudotaxonomic divisions (Haraway 1988; Marks 2000). Patterns of human variation had come to be seen differently, with the human species "constituting a widespread network of more-or-less interrelated, ecologically adapted and functional entities" (Weiner 1957), which began to call into question the very ontology of race. Adaptation was biocultural and local, and higher-order clusters of people were ephemeral and united as much by economic and political histories as by gene flow (Hulse 1962). Moreover, geographical variation in the human species was seen to be patterned principally clinally (Livingstone 1962). This tended to make the entire racial enterprise, centered on the pseudotaxonomic question of how many basically different kinds of people there are, seem nonsensical. Thus, the major reviews by Campbell (1962) and Boyd (1963) passed largely unnoticed, marking an intellectual dead end. Indeed, the study of race itself began to acquire a distinctly unappealing flavor in the 1960s; human differences were not nearly as important as equality and fairness, which were issues of social justice, not biology. Moreover, those with the most intense scientific interest in race sometimes seemed unsettlingly the most committed to its use as a social weapon, as Carleton Coon's *The Origin of Races* (purporting to show that blacks had become *Homo sapiens* 200,000 years after whites) was brandished by the segregationists, with the author's private blessing (Jackson 2001). By the 1970s, major texts on human variation could casually get by without the word "race" even appearing in the index (Johnston 1973; Underwood 1979). And further, the geneticists were reinventing the problem.

Richard Lewontin's 1972 study "The Apportionment of Human Diversity" (Lewontin 1972) is generally taken as a landmark, showing that race "is a myth" or "doesn't exist." But the race concept had been under criticism as a natural structure of the human species for decades and had undergone a significant transformation. Into the 1920s, race was considered to be an essential property of the body transmitted genetically (although according to cultural rules apparently quite distinct from those that geneticists had been formalizing), and where ambiguous, it was to be diagnosed as a physician diagnoses a disease (Hooton 1926). In other words, it was a part of you. A convergence of population genetics and the rise of the racialized Nazi state stimulated a series of publications that reconceptualized race not as something that was a part of you, but as something that you were a part of—that is to say, as a population (Boyd 1950; Dobzhansky 1937; Huxley and Haddon 1935; Montagu 1942). This transformation was effectively completed at the 1950 Cold Spring Harbor Symposium on genetics and physical anthropology, organized by Dobzhansky and Washburn, during which the elderly Hooton told his former student, "Sherry, I hope I never hear the word 'population' again!" (S. L. Washburn, personal communication).

If people were now considered to be parts of gene pools rather than embodiments of distinct types, the question remained, just how discrete were these gene pools? Certainly a dedicated racial theorist, such as Carleton Coon, could navigate readily between the two concepts—race as embodied type and race as gene pool—assuming that the gene pools in question were considerably different from one another. Lewontin showed that human gene pools were not very different at all; thus, even if one conceptualized race as a gene pool, and even if one compared the most divergent populations, there was still considerably more overlap than difference.[6]

A similar finding had been made by Luca Cavalli-Sforza, who adopted the new statistical and computational methods of numerical taxonomy to construct trees of human populations from their minor genetic differences. The relationship between these trees and human history was never particularly clear, however. Hooton (1946) knew that the "racial" history of the human species involved so much admixture that he drew it literally as a capillary system, with reticulating networks of diverging and converging "blood streams." Twenty years later, the population geneticists could produce bifur-

6. The observation that the ranges are far broader than the mean differences among human racial groups was a familiar one and is explicit in the second (1951) UNESCO Statement on Race. Genetics now could quantify that observation, and indeed it has held up with many kinds of genetic markers. It ignores the possibility of focusing specifically on the differences between the most divergent populations, characterizing them and redefining that as race, however.

cating trees, but appreciated that human history was not in fact a series of cladistic events. The trees represented similarity, reduced from multiple dimensions to two dimensions, but could not legitimately purport to represent the history that produced the pattern of similarity (Marks 2002).

Nevertheless, historical inferences were precisely what the human population geneticists began to derive, and like the early racial serologists, they saw their results as confuting the anthropologists. In particular, the issue Cavalli-Sforza chose was, which two of the three major races are most closely related? The esoteric statistical analysis of serological data suggested Europeans and Africans; a similar analysis of anthropometric data suggested Europeans and Asians. As the serologists had done decades earlier, Cavalli-Sforza simply concluded that the genetic inference was correct and the anthropometric data were somehow misleading (Cavalli-Sforza 1974; Cavalli-Sforza and Edwards 1964). Other population geneticists with other statistics managed to retrieve the ostensibly "anthropometric" tree (Nei and Roychoudhury 1972, 1974), and it would not be until the wake of "mitochondrial Eve" that Cavalli-Sforza would acknowledge how dodgy these conclusions actually were:

> Blood groups and enzyme polymorphisms gave different results with respect to the location of the root, with blood groups still showing greater similarity between Africans and Europeans than between Europeans and East Asians. . . . With enzymes and proteins, however, Europeans were closer to East Asians than to Africans. . . . With some contradiction [new DNA data] tend to confirm the African-non-African split, but they are affected by biological or statistical weaknesses. (Cavalli-Sforza et al. 1988:6002)

Actually, the emerging consensus following the mitochondrial Eve work was "none of the above." If the African gene pool is ancestral to the European and Asian gene pools and subsumes them, then it cannot be used as a contrast to them. It is rather like asking whether Rodentia are more closely related to Primates or to Mammalia; Mammalia subsumes the other two categories, thus rendering the answer produced by the computer largely meaningless, because it depends entirely on which particular specimens of Mammalia are chosen to represent that group. While the structure of the tree itself is sensitive to demographic histories such as migration, amalgamation, and population expansion, and to the assumptions built into the clustering algorithm, it is also sensitive to the choice of samples and what they are intended to represent. Population "splits" as cultural-historical events might indeed be there, but it is unclear just how to identify them from a tree of genetic similarity.

It is worth noting that race never left the forefront of this research in human population genetics (Reardon 2004). The geneticists, however, utilized it in diverse ways. In some hands race was negated (Cann, Stoneking, and Wilson 1987; Lewontin 1972, 1974); in others it was adopted as an unproblematic analytical category (Nei and Roychoudhury 1974); and in still others it was—somewhat paradoxically—simultaneously both mythologized and reified (Bowcock et al. 1991; Cavalli-Sforza, Menozzi, and Piazza 1995).

Big Science and Corporate Science

In the 1980s, human genetics came to recrystallize around the goal of sequencing a human genome at the cost of several billion taxpayer dollars. Bolstered by the geno-hype (Holtzman 1999) mobilized to secure popular interest and federal funding, the purple prose and hyperbolic inanities ("We used to think our fate was in the stars. Now we know, in large measure, our fate is in our genes," crowed James Watson epigrammatically [Jaroff 1989:67]) of the Human Genome Project fertilized other nearby fields as well. Hereditarian political philosophy took old concepts and repackaged them pseudogenomically with considerable public fanfare (Herrnstein and Murray 1994). Another beneficiary was the reborn field of human behavioral genetics, regularly finding (and subsequently losing) genes for homosexuality, alcoholism, aggression, depression, and other nonnormative behaviors, or brandishing curious anecdotes of identical twins separated at birth (Holden 2009) as if they represented unproblematic scientific data.

Once it was observed that the Human Genome Project seemed to be rooted in a naively Platonic view of the genome (Walsh and Marks 1986), human population geneticists created an opportunity for themselves. A Human Genome Diversity Project (HGDP; Cavalli-Sforza et al. 1991; Roberts 1991) could augment the Human Genome Project and be a boon to human population genetics, but it would require a rhetorical justification for the public expenditure. Tellingly, that justification would be drawn from antiquated views of anthropology, which left anthropologists ambivalent about the project in spite of its own population-level geno-hype (Diamond 1991; Kidd, Kidd, and Weiss 1993; Roberts 1992; Weiss, Kidd, and Kidd 1992).

Blood collection and analysis had become an anthropological staple since it was first carried out by Carleton Coon in 1922 in the wake of the Hirszfelds' work to see whether the Rif in Morocco possessed racial blood traits that matched their racial physical traits. By the 1970s, following the work of James Neel in Amazonia and Cavalli-Sforza in central Africa, collecting blood samples had become commonplace in anthropology, although it was carried out on a small scale and a largely ad hoc basis. That, however, permitted it to fly under the bioethical radar, so to speak. By shining a bright light on the field, the diversity project inadvertently began to call into question the crucial data-collection practices of human population genetics in an era of heightened sensibilities about the property rights of indigenous peoples. Why did they need to make a collection of the DNA of the world's human populations?

First, they invoked the tropes of "salvage anthropology," namely, the imminent extinction of indigenous peoples, which they complemented with discourses of isolation and purity (Barker 2004). It is worth noting in this context that half a century earlier the serologist William C. Boyd was challenged for genetically reifying his Navajo samples: he said they were "pure," but the anthropologist Clyde Kluckhohn knew the ethnohistory of the community and knew that they were not at all "pure" (Kluckhohn and Griffith 1950). The Hopi geneticist Frank Dukepoo (1998) made the same point to the HGDP: "My father (a "Hopi") is a mixture of Hopi, Ute, Paiute, Tewa and Navajo; my mother, on the other hand, (a "Laguna") is a mixture of Laguna, Acoma, Isleta, Zuni and Spanish. Members of other tribes share similar admixture histories as our ancestors raided, traded or kidnapped to ensure survival of their numbers. . . . [I]t is reasonably safe to surmise the same situation for members of other ethnic groups" (242).

Second, in a post-NAGPRA era, one could hardly fail to take note of the complexities associated with making collections of blood as museums were being obliged to return their collections of bones. Issues of informed consent, financial interests, and the responsibilities of the researchers were raised reactively, if at all. Worse still, the organizational meetings pointedly spoke about indigenous peoples but not to them. The HGDP seemed to be recapitulating the colonial science of an earlier era (Cunningham 1997).

Third, the issue of consent itself in a cross-cultural context was complicated by the possible use of the samples against the wishes or interests of the subjects. In a civil case filed in 2004, the Havasupai sued researchers from Arizona State University in part on the grounds that, had they known that their DNA samples were going to be used to build scientific narratives and undermine their own narratives of autochthonous origins, they would not have given the samples.[7] But even more problematic is the use of the DNA samples for work that is manifestly racist. In 2005, geneticist Bruce Lahn purported to find a genetic deficit in two brain genes of the peoples of Africa (Evans et al. 2005; Mekel-Bobrov et al. 2005; Regalado 2006) using the HGDP (now the HGDP/CEPH) DNA collection.[8] One suspects that if the people were made aware of the use to which their blood samples were being put, they might be inclined to reconsider consenting.

And fourth, the HGDP appropriated to itself the cultural authority of science in matters of ancestry and very casually delegitimized any other ideas about kinship and descent (Atkinson, Bharadwaj, and Featherstone 2006; Egorova 2007; Tutton 2004). With an uneven track record, it was never clear that the HGDP could deliver on this promise, and it is not clear just how reliable the claims to historical accuracy are. In many cases, the genetic patterns are exceedingly subtle or may even be statistical reifications (Moore 1994; Novembre and Stephens 2008; Templeton 1998). Nevertheless, the scientific authenticity of their narratives of ancestry would be the principal product marketed by its successor, the Genographic Project. But when the Genographic Project acknowledges that they only study "a small fraction of the genome—less than 2%" (Wells and Schurr 2009:184), it is hard to know how they could produce a picture of an individual's ancestry that is either comprehensive or accurate.[9]

The Genographic Project was initiated in 2005 as a privately funded venture in human population genetics supported principally by National Geographic and IBM and hoping to transcend the issues that had undermined the HGDP. Once again, they were quickly burdened by ethical questions (Harmon 2006; Nicholas and Hollowell 2009) centered on consent and exploitation. A 2007 solicitation from the Genographic Project invited wealthy patrons to participate in a $50,000 "Journey of Man" tour in a "VIP-outfitted Boeing 757" to visit exotic subaltern people and have the head of the Genographic Project personally analyze their DNA and establish fictive kin relations for them (Marks 2007).

The innovation of the Genographic Project was to identify a product to market, namely, ancestry (Wald 2006). The HGDP had been criticized for its interest in indigenous people and its lack of interest in populations it considered to be admixed, notably the urban and acculturated peoples of the world, which is most of the world. The Genographic Project would use those peoples to subsidize the study of the indigenous peoples. For $99.95, I (the least indigenous person I know) can purchase either a mitochondrial DNA test or a Y-chromosome test and have my own haplotype matched to those of "global populations." Their Web site explains:

> To be clear—these tests are not conventional genealogy. Your results will not provide names for your personal family tree or tell you where your great grandparents lived. Rather, they will indicate the maternal or paternal genetic markers your deep ancestors passed on to you and the story that goes with those markers.
>
> Once your results are posted, you will be able to learn something about that story and the journey of your ancestors.[10]

But because their mtDNA test would only be examining one of my eight great grandparents, it is therefore not analyzing ancestry in the familiar sense of the term; nor do they discuss the complexity of what ancestry actually means in reference to lives lived and journeys made hundreds of generations ago, when the number of my genetic ancestors was astronomical.[11]

7. The case was settled out of court in April 2010 (Marks 2010*a*).

8. Of course, the claim has not stood up.

9. The 2% value given includes analyzing the Y chromosome along with mtDNA. For women, only mtDNA is studied, which reduces the value by several orders of magnitude.

10. https://genographic.nationalgeographic.com/genographic/participate.html (accessed September 12, 2009).

11. Conservatively assuming four generations/century, my ancestors 20,000 years ago are two to the eight hundredth power, or a number with 240 zeroes after it. That would be 233 orders of magnitude greater than the number of people alive at the time if there were 10,000,000 people alive at the time. Many of those are common ancestors (i.e., I

The novelty here is the commodification of DNA information—the identification of a market, the construction of a demand for the information—and the continuity is provided by the acquisition of the comparative database from the bodies of indigenous people, guided by ethical considerations (or the lack thereof) of several generations past, but now unconstrained by the need for government approval or oversight to be funded. The funding is already in place.

The allure of the market and the creation of wealth through the production of genomic information has stimulated the development of corporate human genetics internationally, most notably in Iceland (Pálsson 2007). The most significant innovation of deCODE in Iceland was to dampen the criticism that the construction of a comparative database replicated colonial relations; Icelanders would be studying their own gene pool for the advancement of knowledge and, it is hoped, profits. Indeed, the growth of corporate science has stimulated historian Steven Shapin (2008) to argue that the corporate model is an alternate normative model of scientific knowledge production rather than simply an aberration of an idealized pure form of academic science. Nevertheless, even millennia ago, it was widely appreciated that when truth and wealth are concurrent goals, truth invariably suffers as a result (Matthew 6:24).

The "big science" triumph of molecular anthropology has been the chimpanzee genome, released with great fanfare in 2005. The most interesting claims involve identifying a baseline average level of difference between the DNA sequence of human and chimp and then identifying regions that appear to be "too similar" and presumably vital for survival, and regions that appear to be "too different" and presumably at the root of our adaptive differences from chimpanzees. While possibly valid in some cases, these assumptions have proven epistemologically difficult to sustain at face value (Prabhakar et al. 2006; Shi, Bakewell, and Zhang 2006).

Certainly the oddest results come from combining studies. The peopling of the New World, for example, has been argued on genetic grounds to have occurred in one wave, two waves, three waves, and more than three waves. The root of the genetic tree of human populations is generally taken to lie within African populations (Campbell and Tishkoff 2008), but it has also proven surprisingly difficult to exclude non-African input into the gene pools of the rest of the world (Reich et al. 2010; Templeton 1993).

Most paradoxically of all, the DNA from Neanderthals has been recently interpreted as indicating their sufficient *difference* from modern humans as to be separated from us at the species level, as *Homo neanderthalensis* (Lalueza-Fox et al. 2005). Concurrently, the DNA from chimpanzees has been recently interpreted as indicating their sufficient *similarity* to modern humans as to be separated from us at the species level, as *Homo troglodytes* (Wildman et al. 2003). Yet because the difference between humans and Neanderthals is considerably smaller than that between humans and chimps, it follows that both of these inferences cannot simultaneously be true. One or the other or both must be wrong. Unfortunately, the molecular geneticists do not seem anxious or willing to explain to the rest of the scholarly community which of them it is. Quite possibly they cannot tell.

am somewhat inbred), and many of them overlap with other people's ancestors (i.e., we are all related).

Conclusions

The most basic conclusion from observing the crossroads of genetics and anthropology over the last century is that superficially you see very different patterns when you examine genetic data than when you examine more traditional kinds of data. This is as true when the gaze of hemoglobin is applied to human ancestry as when it is applied to human diversity. In both cases, however, the significance of the genetical viewpoint is strongly inflected culturally. The intimacy of human and chimpanzee bloods was long familiar to students of human evolution without the concomitant inference that that particular bit of knowledge necessitated a different representation of our place in the natural order, that is to say, without the belief that the apparent genetic relations were more "real" than all others. Moreover, within the human species, the genetic data revealed races when they were expected to, negated races when they were expected to, and consequently leave geneticists in disagreement on the subject at present (Koenig, Lee, and Richardson 2008).

This leads to the second conclusion, that human genetics gives out mixed messages about race because it only has access to one component of it (studying difference); anthropology provides the other (studying meaning). Race is not so much difference (because all populations and all individuals are biologically/genetically different); rather, it is meaningful difference (a subjective judgment that certain differences or patterns of difference are more important for classificatory purposes than other kinds and patterns of difference). Consequently, geneticists do not have privileged access to race and never have, because they study only difference. But reducing race to simply measurable difference leads to confusion. Indeed, the ambiguities expressed in the genetic work have led one philosopher of biology to try to resuscitate race as a set of formal naturalistic categories on the basis of a thoroughgoing confusion of genetically produced dendrograms with cladistic events in the prehistory of human populations (Andreasen 2004; Gannett 2004; Marks 2010*b*).

Finally, molecular anthropology reinforces the conclusions that contemporary historians are drawing about the highly mythologized scientific history of the nineteenth century. Most significantly, the central importance of human diversity in the origins of evolutionary biology has been considerably undervalued. The scientific positions of monogenism (one origin of *Homo sapiens*, most compatible with biblical literalism) and polygenism (different origins of the races, with the biblical story relating merely the most recent creation, most compatible with geology) correlated with the political poles of abolitionism and slavery, respectively. Monogenism, being more morally defensible, necessitated an evolutionary view—one that saw Adam as the sole progenitor of all the diverse races—but was scientifically problematic, given the increasingly apparent antiquity of the earth and the succession of life. Darwin, coming from abolitionist families, made the monogenist position scientifically respectable by implying a single ancient origin of the human species, with that origin not being Adam, but rather a sort of ape (Desmond and Moore 2009; Livingstone 2008). Being already evolutionary, monogenism could readily adapt the Darwinian position to its ends.

And likewise today, human evolution and human politics are connected. Various political movements attempt to draw legitimacy from molecular anthropology, such as the push for ape rights (Cavalieri and Singer 1993) and more importantly ape conservation; and the molecular similarity of humans and chimpanzees is regularly recruited to help exaggerate their similarities to establish claims on behalf of ape "language" and "culture" (Fouts and Mills 1997; Savage-Rumbaugh and Lewin 1994; Wrangham et al. 1994).[12]

Thus, while the relationship between genetics and anthropology has evolved over the last century, through the emergence of race studies, American eugenics, Nazi race hygiene, the civil rights movement, molecular genetics, postcolonialism, and corporate genomics, we can see some general trends. Normative human genetics is not value neutral and is not disconnected from contemporary social and cultural politics. Indeed, it has commonly been more of an applied science than an abstract theoretical one, while nevertheless rarely if ever confronting its track record as applied science. Consequently, the value of anthropology for contemporary genetics probably resides strongly in helping to explore the cultural assumptions that inhabit the production and interpretation of its data, and that have for over a century.

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12. These usually involve confusing particular properties of language and culture for definitions. In humans, language and culture are seen as intimately connected, as related expressions of human social cognition; but in chimpanzees, the claims for language generally come from captive studies of carefully reared animals, while the claims for culture come from field studies of wild animals, and are largely epistemologically disconnected from one another.

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Beyond the Cephalic Index

Negotiating Politics to Produce UNESCO's Scientific Statements on Race

by Perrin Selcer

This paper analyzes the production and reception of UNESCO's Statements on Race from 1950, 1951, and 1964 to track the consolidation of a scientific consensus on the biological significance of race. The race statements played a key role in the establishment of the postwar liberal antiracist orthodoxy, and their history illuminates broader dynamics in the production of scientific scripts intended to influence political debates. The consensus was rooted in the synthesis of physical anthropology and population biology but depended on a parallel strengthening of antiracist social norms in the international community. As much as race, conflicts over the race statements were disputes over scientific authority—over who, if anyone, should be authorized to speak for science. Because international civil servants and activist scientists had to negotiate disciplinary, national, and international politics to achieve an acceptable consensus, a close reading of this history reveals the importance of shifting political and social meanings of equality on scientific statements of biological facts. Two central ironies that emerge are the shifting association of bell curves from representations of liberal racial tolerance to icons of enduring racism and the importance of increasing racial and national diversity in the international scientific community to discrediting biological determinism.

Historians of race routinely cite the United Nations Educational, Scientific, and Cultural Organization's (UNESCO's) 1950 and 1951 Statements on Race as the end of an era of scientific racism. During the nineteenth century, the story goes, scientists had performed increasingly elaborate measurements to show that white economic, social, and political supremacy reflected a natural racial hierarchy. After reaching its apex of technical sophistication and political influence with the eugenics movement in the years following the First World War, scientific racism lost ground during the 1930s. In part, it was a victim of its own success; the Nazi's doctrine of Aryan superiority discredited race science and provided leverage for liberal scientists' antiracist campaigns. By the start of the Second World War, the consensus among psychologists (although not yet among biologists, geneticists, and physical anthropologists) held that there was no discernible innate difference in intelligence between races—at least it did in the United States, the world's largest multiracial nation and the most enthusiastic adopter of IQ tests. The horrors of the Holocaust dealt a final blow to the respectability of racism, and the equality of all races was explicitly proclaimed in the founding documents of the United Nations (UN). Racism—and racist scientists—certainly did not disappear. But UNESCO's Statements on Race mark a decisive transition in the scientific community from a presumption of racial inequality to a presumption of racial equality. Inequality, therefore, was determined not by nature but rather by culture (Banton 1996; Barkan 1992; Brattain 2007; Carson 2007; Jackson 1990; Kevles 1986; Marks 2008; Muller-Wille 2007; Provine 1986; Stepan 1982; Stocking 1968).

As scientific documents designed to make "race prejudice . . . a shameful sentiment that men will hesitate to avow," UNESCO's Statements on Race are indeed excellent markers of the postwar culmination of this transition (Métraux 1950*b*: 390). The statements responded to UNESCO's constitutional mandate to fight "the doctrine of the inequality of men and races" and a call from the UN Economic and Social Council to disseminate scientific facts disproving racial prejudice. They also represented the belated victory of antiracist intellectuals on both sides of the Atlantic who had been attempting to organize declarations condemning racism since the rise of the Nazis in the 1930s. In 1950, the UN Social Sciences Department released the consensus statement of an international meeting of social scientists. The "Statement by Experts on Race Problems" rather lyrically expressed the by-then standard antiracist liberal position on the biological significance of race. It received substantial and overwhelmingly positive coverage in the international press and became a key resource for educators throughout the world. Within the scientific

Perrin Selcer is Lecturer in the History Department at the University of Texas (327 West Lullwood Avenue, San Antonio, Texas 78212, U.S.A. (pselcer@mail.utexas.edu). This paper was submitted 27 X 10, accepted 9 VI 11, and electronically published 30 XI 11.

 0011-3204/2012/53S5-0015$10.00. DOI: 10.1086/662290

community, however, the statement provoked considerable controversy. Physical anthropologists, especially in England, resented that they had not been adequately represented on the committee of experts and questioned the scientific soundness of its conclusions. To appease this small but vocal group, UNESCO organized a second meeting of experts consisting entirely of geneticists and physical anthropologists, which produced its own "Statement on the Nature of Race and Race Differences." The second statement adopted a more matter-of-fact tone but was based on the rather esoteric argument that biological diversity must be understood through a population rather than a typological approach and more clearly hedged on the actual equality of races. Nevertheless, the second statement surprised even many of its own signatories with the strength of its antiracism, and UNESCO successfully presented it as another weapon in the fight against racial prejudice.

The Statements on Race signified a real and important change in the social and political significance of race science. The well-noted irony is that the process of drafting the statements left a rich historical record demonstrating the persistence of the supposedly defeated racist science. As Jenny Reardon has shown, the statements did not mark and were not even intended to mark an end to race as a legitimate biological category; instead, race remained a problematic and contested scientific concept (Reardon 2005).[1] Indeed, as subjects for historical investigation—as opposed to historical markers—the great value of UNESCO's Statements on Race is the complexity of the intersecting story lines and the variety of possible themes, not the clarity of the antiracist triumph.

The danger of the Statements on Race as subjects of historical inquiry is that the hapless historian will get sucked into yet another round of the tedious and distracting argument over the biological significance of race: the difficulty of distinguishing between environment and heredity in the case of complex traits and the importance of culture in determining personality; the biases of mental tests and the invalidity of the concept of G (i.e., general intelligence); the importance of diversity within populations compared with differences between populations; the history of human mobility that undermines any claim of racial purity and the very "reality" of the race concept;[2] and so on. These claims and the predictable counterclaims were all made during the debates over the race statements; in fact, despite the rise of molecular biology, it probably would be possible to reconstruct the basic arguments of any extant position on the scientific meaning of race just from the various texts—correspondence, pamphlets, editorials, reviews, articles—in UNESCO's archival files on the race statements. In the act of debunking scientific racism, the antiracist intellectual inadvertently keeps the focus on the very biological facts he insists are insignificant. It is still a necessary performance, but it often feels like merely participating in the debate keeps the show going past its rightful closing date.

The debate is sterile because it follows a well-known script (Tilly 1998). By script I mean a political debate that has crystallized into a familiar performance in which two opposing camps repeat predictable lines. The line one advocate speaks largely determines the line of his opponent, and combatants choose a part based on convictions instead of forming convictions based on the persuasiveness of the argument. This does not mean that scientific statements on race were merely incidental or unscientific. The continual repetition of argument and counterargument assured that only the most compelling claims survived; indeed, the script on the biological significance of race is so highly refined that it is extremely difficult to write new lines.

The Statements on Race are more interesting for what they can reveal about the production, reception, and evolution of scientific scripts than for insight into the fertile but well-tilled intellectual history of race science. The production of the statements was a key event in the consolidation of the postwar liberal racial orthodoxy. Because race structured so much of twentieth-century society, from international politics to playground etiquette, the script had to play well in a demanding variety of venues: the popular media and scientific journals; UN general conferences and high school classrooms; the United States, its imperial allies, and newly independent nations. The script, therefore, had to be polysynonymous—that is, carry multiple meanings so that different audiences could discover congenial interpretations. Because the script had to work for each of these audiences, each partially determined its content. Changes in the content or interpretation of the script, therefore, imply corresponding changes in the social and political significance of race.

In this paper, I am more interested in analyzing how scientists negotiated social and political pressures than in speculating about the social and political effects of the statements. Because of space considerations, I focus on debates in the British and American scientific communities. In order to capture how changes in the social and political meaning of equality affected the scientific script on race, I analyze the production of the 1950 and 1951 statements in the first section of this essay and then investigate a third statement, published in 1964, in the second section. Of the many possible themes, I pay close attention to debates over scientific authority—over who, if anyone, should be authorized to speak for science—and to the reversal of the political significance of overlapping bell curves of human population traits from a key

1. Races—often synonymous with subspecies and understood as incipient species—were a core concept of the population genetics that galvanized the midcentury evolutionary synthesis (cf. Dobzhansky 1951). For the diversity of international scientific opinion on the race question, see *The Race Concept: Results of an Inquiry*, a compilation of scientists' letters commenting on the race statement (UNESCO 1952).

2. As John P. Jackson has noted, scholars outside the history of science generally assume that because race is socially constructed, it is not a "real" or natural biological thing. But "historians of science have excelled in showing how biological categories are just such constructions" (Jackson 2002:2). DNA, the global climate, and race are all social constructions, and reality is not at issue.

component of the antiracist script to a stock argument supporting the biological inequality of races.

Biological Individuality and Equality of Opportunity

Nearly all historical references to UNESCO's Statements on Race begin, like most contemporary accounts did, with some version of, "In the wake of the horrific mass murder of six million Jews." It would seem hard to exaggerate the transformative power of the Holocaust on the intellectual and political history of race. And yet too great an emphasis on the Holocaust can obscure other contexts that were as important in motivating the statements and more important in shaping their meaning. In metropolitan France, antiracism was predominately framed as a fight against anti-Semitism. But for UNESCO's other two most powerful member states, the race question was a problem of skin color. The Second World War transformed race relations in the United States and in the colonial world. The U.S. and British governments appealed to African American and colonial soldiers on patriotic grounds, framed the war as a struggle for freedom, and promised that victory would mean the right of all peoples to self-determination. Whatever the hypocrisy epitomized by a segregated U.S. army and Churchill's reneging on the scope of the Atlantic Charter, both African Americans and colonial subjects were determined that the bargain—service in defense of the nation or empire for the full rights of citizenship—would be upheld (Anderson 2003; Bleich 2003; Rich 1990). The Holocaust certainly provided critical moral leverage for antiracists, but in the UN, the race question was foremost about how to reconcile the ideals of a new liberal world order with the realities of European colonialism and American Jim Crow.

The Holocaust can also be a subtly misleading context in which to interpret the behavior of antiracist scientists. It can too easily imply that scientists acted out of a sense of collective responsibility for the barbarous excesses of Nazi race science. Although they loudly decried the perversion of science to destructive ends in war, the mood in the postwar scientific community was hardly one of guilt or humility. Instead, scientists tended to portray the Second World War as a warning against the dangers of irrational power politics and emotional primordial loyalties in a technologically advanced and interdependent global society. Internationalists called for the adoption of scientific rationality throughout society, from international to interpersonal relations. In an internal memo suggesting ways UNESCO might implement a 1947 General Conference resolution calling for the organization of popular discussion groups on the "social implications of science," a program officer in the Natural Sciences Department suggested the topic of human genetics. "Knowledge about genetics and human heredity," the thought piece noted, "is already sufficiently advanced to be of direct practical value . . . in the broad field of eugenics. Human genetics has placed the equality of man on a solid scientific foundation. It has proved a powerful factor in fighting and liquidating racial antagonism on a scientific front."[3] Far from a renunciation of interwar eugenics, this early proposal for a scientific educational campaign against racism was framed as a logical extension of the progressive eugenics movement.

Within UNESCO's sprawling program, however, the race problem did not fall under the Natural Sciences Department's jurisdiction. Following the American organization of knowledge production, the sciences at UNESCO were divided between departments of natural and social sciences, and the Social Sciences Department (SSD) took the initiative in developing "the social implications of science" as they related to race. In January 1949, the SSD proposed an "Educational Offensive" to popularize "the results of recent developments in the social and natural sciences, indicating a) the *unimportance* of racial or biological factors in determining the behaviour of nations and b) the importance of social and historical factors in explaining such behaviour." Ironically, given the subsequent history of the race statements, the SSD noted that this "task would unite the interests and knowledge of both the natural and the social sciences." More importantly, for an organization and a department routinely accused of impractical idealism, such a campaign would "furnish [a] concrete indication that UNESCO is dealing boldly and concretely with the problem of eliminating from the minds of men those ideas which lead to misunderstanding and conflict." Yet antiracism also offered "a point of agreement among nations otherwise separated by differences in political ideologies."[4] Race, in other words, was a subject that could enhance intellectual cooperation between the natural and social sciences, bureaucratic cooperation between UNESCO's sciences departments, and political cooperation between the East and West, North and South.

In this sense, contemporary appeals to the Holocaust in support of antiracist activism reveal a key aspect of the dynamics of racial discourse in the international community. The great advantage of the Holocaust as a foil for antiracists was that denouncing the Nazis alienated no member states. Criticism of member states, especially the United States and Britain (the two most prestigious members and largest donors—the United States alone contributed 40% of UNESCO's budget), was another matter. While member states were happy to go on record denouncing racism, few would tolerate criticism from an international organization whose budget they provided and whose program they approved. Addressing the race question was thus a perilous prospect for any UN agency.

Yet precisely because race was such a problematic issue in

3. "Memorandum on Group Discussions on the Social and International Implications of Science," undated, 5:304, Social Implications of Science, Correspondence Files, UNESCO Archives, Paris.

4. "Social Implications of Science," January 21, 1949, 5:304, Social Implications of Science, UNESCO Archives, Paris.

a liberal international world order, promoting racial liberalism was a principal function of the UN. The United States' legitimacy as the leader of the multiracial Free World was undermined by domestic racial strife. The United States responded to Soviet propaganda, which exploited the country's shameful record on race, with a propaganda campaign of its own that emphasized the democratic superpower's peaceful progress toward racial equality. In essence, this meant embracing the midcentury liberal orthodoxy most famously articulated in Gunnar Myrdal's *American Dilemma* (1944). This argument dismissed the biology of race as trivial and acknowledged the country's racist past and problematic present but emphasized the complex of positive values—the American creed—that contradicted these historical realities. The American creed promised a future in which all citizens would enjoy equality of opportunity (Dudziak 2000; Jackson 1990; Layton 2000). The cautious optimism of the American creed complemented the colonial powers' civilizing mission. The current contradictions of a liberal empire were justified by the promise of future equality; colonies would be granted independence once their people had reached the necessary standard of civilization to form modern nations (Mehta 1997). Political realities prevented UN agencies from exposing concrete manifestations of racial oppression but created the opportunity to promote the legal equality of nations and individuals.

The postwar antiracist script had to reconcile the biology of race with the ideal of a meritocratic democracy. UNESCO's second director-general, Jaime Torres Bodet, told the group of physical anthropologists and geneticists convened to draft the second statement that the "'dogma' of racial inequality" they were gathered to combat was "not factual inequality—like the inequalities to which history bears witness, of power, ability, or merit—but legal inequality, that is to say, inequality of worth."[5] Scientific truth was not intended to reveal the actual equality of the races but the potential equality—the legal equality—of individuals and nations. The equality at stake in the first two race statements was equality of opportunity.

Equality of opportunity meant that individuals should be judged on their own merit rather than the reputation of their race. As the head of the SSD's race program, the anthropologist Alfred Métraux, wrote in an article that accompanied the publication of the first statement in UNESCO's popular magazine the *Courier*, "Ironically, the worst sufferers of racial dogma are usually the people whose intellect most forcibly demonstrates its falseness" (Métraux 1950*a*:8). This was essentially a middle-class sentiment that focused attention on the unequal treatment of worthy (i.e., hardworking, smart) individuals rather than the debilitating poverty of the masses and the structural determinants of racial inequality.

This emphasis on individuals was why the distinction between typological classifications that reified the average racial characteristics of "pure races" and descriptions of dynamic groups that stressed a population's full range of variation was such an important component of the antiracist script. Thus, the second statement conceded that "it is possible, though not proved, that some types of innate capacity for intellectual and emotional responses are commoner in one human group than in another." But the depressing implications of this possibility were effectively countered by the proved fact that "within a single group, innate capacities vary as much as, if not more than, they do between different groups."[6] Furthermore, the range of variation between groups overlapped so that "some members of the group of inferior performance surpass not merely the lowest ranking member of the superior group but also the average of its members" (Montagu 1972: 144). Differences between races, in other words, could be visualized as a set of overlapping bell curves. Although there might be differences in the curves' peaks and extremes, the substantial overlap provided a biological foundation for a nonracial liberal meritocracy. Individuals of any race could possess the talent to rise up the social hierarchy.

The cautious optimism of the race statements was intended to encourage reconciliation and reform, not to incite conflict. The first Statement on Race (which began "Scientists have reached general agreement") was supposed to represent the scientific consensus to a popular audience, not change the consensus. By correcting the public's misguided biological determinism, it would clear the ground for the race program's series of educational pamphlets, The Race Question in Modern Science, which presented a more complex guide to the social implications of science.[7] Yet it was scientists who sparked the controversy over the statement. "If it were not for the wave of hostile criticism our Statement met with in England," wrote Métraux, "we should never become involved in [the second Statement on the biological aspects of race]."[8] Like the fuzzy divisions between races that population biologists described, however, the boundaries of the British physical anthropology community were porous. Biologists and

5. "Address Delivered by the Director-General to the Meeting of Physical Anthropologists and Geneticists for a Definition of the Concept of Race," 323.12 A 102/064(44) "51," Statement on Race: Expert Meeting of Physical Anthropologists and Genetists [sic]: Paris 1951, UNESCO Archives, Paris.

6. Although attention to variation within groups was often presented as a cutting-edge innovation of the new population biology, it was actually a long-standing component of scientific antiracism. Most importantly, at the beginning of the century, Boas had stressed the greater range of variation within compared with between races in his deconstruction of racial typology (Stocking 1968, 161–194).

7. For a concise review of the work of the race program, see Gastaut (2007).

8. Métraux to Dover, February 12, 1951, 323.12 A 102/064(44) "51," Statement on Race: Expert Meeting of Physical Anthropologists and Genetists [sic]: Paris 1951, UNESCO Archives, Paris.

American, British, and European physical anthropologists participated on both sides of the conflict.[9]

Even before the first statement was released, its advocates recognized its weaknesses. The problem was less the facts it presented than the style in which they were presented, which violated scientific propaganda's norm of caution. As the social psychologist and leading antiracist expert on intelligence Otto Klineberg advised the SSD, the statement would be more effective (and less open to attack) if "the tone of the Statement [was] less dogmatic than it is at present." Most importantly, instead of suggesting that there were no hereditarily determined racial differences in psychology, the statement should report that there was no proof of such inborn differences.[10] Despite months of arm twisting, cajoling, and pleading, the rapporteur, Rutgers University anthropologist Ashley Montagu, refused to budge, most importantly insisting on keeping the conclusion that "biological studies lend support to the ethic of universal brotherhood."[11]

Fears of a scientific backlash quickly proved well founded. The certainty with which the statement dismissed the importance of racial biology but affirmed "biological drives towards universal brotherhood" was exactly what William Fagg, the secretary of the Royal Anthropological Institute, claimed most anthropologists regarded "as distinctly controversial in the present state of our knowledge" in a letter to the London *Times* that initiated the controversy. Although Fagg captured UNESCO's attention by challenging the scientific authority of the statement in a popular forum, the controversy would be largely contained within the scientific community. The main battlefield was the correspondence pages of *Man*, the monthly bulletin of the Royal Anthropological Society, which Fagg edited.

Read out of context, it is easy to miss the nonracial components of the debate. *Man* provided a sort of clubby forum in which anthropologists matched wits and tested each other's pedantic mettle. The January 1951 issue celebrated *Man*'s 50 years of service "as a vehicle for scientific controversy." By "a series of happy coincidences," the jubilee issue included five letters feeding the fire of the UNESCO conflict and three salvos in an ongoing battle sparked off by E. E. Evans-Pritchard's attack on the scientism of social anthropology's functionalism. "May controversy long flourish freely in *Man*" proclaimed the editor in praising the founders' refusal to provide a rigid framework or coordinated planning for the bulletin (*Man* 1951:5).

Fagg's pairing of controversy with freedom signaled that the conflict over the Statement on Race was part of a long-running debate within the British scientific community over whether science should be planned. Joseph Needham, the Natural Sciences Department's first director, and Julian Huxley, UNESCO's first director-general, were key figures in the Social Relations of Science movement, which pushed for the rational planning of science and the scientific planning of society. The Society for Freedom in Science was a right-leaning reaction to this broadly left-wing movement (Kenny 2004; McGucken 1984). There was actually significant overlap between the two camps, however, and no necessary correlation with racial positions. And although UNESCO was supposed to organize international science, it also was supposed to facilitate the "free flow of information." The question of whether UNESCO ought to be in the business of issuing official scientific statements, therefore, touched on the very purpose of the organization. It was not just the specific content of the statement scientists found objectionable but UNESCO's presumption to declare correct scientific opinion, which smacked of totalitarianism.

This reasoning opened the door to the absurd claim that UNESCO's antiracist campaign was equivalent to Nazism. Métraux was understandably shocked that German scientists—whose careers, incidentally, had not suffered under the Third Reich (Proctor 1988)—had the gall to defend nastily racist positions with this argument.[12] Of more long-term significance, however, the defense of intellectual freedom became a reliable theme in the reactionary script that labeled advocacy of racial equality politically correct wishful thinking. In fact, righteous antitotalitarianism provided a sort of respectable cover for racist scientists' attack on postwar antiracism.

And cover was necessary. In his letter to the *Times* undermining the original statement, Fagg declared that the Royal Anthropological Institute supported the statement's contribution "to the abatement of racial prejudice" and agreed with its "essential thesis—that racial persecution and discrimination (in the bad sense) cannot be justified on the basis of facts established by anthropology."[13] For Fagg, discriminating

9. In fact, prominent members of the American physical anthropology community felt that they had not fulfilled their scientific responsibility by leaving it to their British colleagues to challenge the validity of the original statement (Stewart 1951).

10. Klineberg to Angell, January 25, 1950, 323.12 A 102, Statement on Race, Part I up to December 31, 1950, UNESCO Archives, Paris.

11. Surely the UNESCO official who chided Montagu that her "only wish now is . . . that anthropologists were as co-operative among themselves as other human beings [according to your thesis]" was not alone in noting the irony. Although Métraux later attributed all the problems with the statement's tone to Montagu, it was actually Métraux who supplied the following clause: "for man is born with drives toward co-operation, and unless these drives are satisfied, men and nations alike fall ill." Angell to Montagu, April 26, 1950, 323.12 A 102, Statement on Race, Part I up to December 31, 1950, UNESCO Archives, Paris. Tead to Montagu, March 5, 1952, 323.12 A 102, Statement on Race, Part IV from January 1, 1952 to March 31, 1953, UNESCO Archives, Paris.

12. Robert Proctor (2003) has similarly argued that liberal antiracist science—labeled "the UNESCO response to Auschwitz"—has retarded scientific progress on human evolution even as he concludes that stories of human origins are moral tales. In fact, the standard strategy of antiracists (and UNESCO's race program) was to make public, often by publishing in their own journals, the writings of racist scientists. Furthermore, the best known advocate of the position Proctor favors—a bushy family tree—was also the best known antiracist (i.e., Stephen Jay Gould).

13. Fagg to the *Times*, July 24, 1950, 323.12 A 102, Statement on Race, Part I up to December 31, 1950, UNESCO Archives, Paris.

between the races was not prejudice if the distinctions were real. But this nuance was less important than the fact that in the postwar world, even scientists who believed in the biological inequality of races felt compelled to publicly support antiracism. Their arguments had to be framed within the liberal orthodoxy.

The authority and political usefulness of scientific statements, however, depended on maintaining a boundary between politics and science (Gieryn 1999). In his welcoming address to the committee gathered to author the second statement, Director-General Bodet declared that UNESCO's "weapon" against the lies and irrationality of racism must be "the truth, and nothing but the truth." "The prime merit of science," he flattered his audience, "is the example it gives of objectivity in its purest and most stable form."[14] Accordingly, it should not have mattered which scientists were assembled at UNESCO House in June 1951; the objectivity of science would assure that only the truth emerged. Of course, as none knew better than the scientists themselves, who was in the room mattered a great deal. The chairman of the American Museum of Natural History's anthropology department, Harry Shapiro, put it bluntly to Métraux: "The choice of delegates . . . is a matter of tactics rather than [scientific] suitability or unsuitability."[15]

One of the key tactical considerations was the disciplinary representation of the expert committee. The mid-twentieth century was an especially vulnerable moment for physical anthropologists. With the triumph of the evolutionary synthesis and the accompanying ascendance of population genetics, biologists who counted the bristles on flies or measured the wingspans of birds were regarded as authorities on evolutionary processes and classifications of all kinds, including the history of human racial differentiation (Mayr and Provine 1980). Cultural anthropologists and sociologists had succeeded in claiming the social implications of race as their turf. Understandably, physical anthropologists were a defensive group.

The 1950 statement exemplified the diminished authority of physical anthropologists. After social scientists drafted the statement, Montagu and the SSD sent it for peer review to leading biologists and geneticists.[16] The only anthropologist who participated in revisions of the first statement was Don Hager of the Department of Economics and Social Institutions at Princeton. Hager attributed the vitriol of British and French physical anthropologists' attacks to "a reaction against the fact that the scientific frontier has pushed beyond the cephalic index, the bigonial diameter, the bizygomatic diameter, and all the rest." The ascendance of human genetics, he explained, had led to a shift from "taxonomic-descriptive studies to studies of function, process and diversity" (Hager 1951:54). Population biology and social science provided the tools to understand human difference, and physical anthropologists could either adapt or perish.

Many British physical anthropologists had not pushed beyond the frontier of precise measurements of trivial significance, however. In an editor's note, Fagg defended "classical" or "old-world" physical anthropologists from Hager's condescension and wondered "whether American physical anthropologists generally regard 'intergroup,' for example, as synonymous with 'racial,' and if so whether they do not feel that they are defining themselves out of existence" (Hager 1951:54). For the proud bearers of the classical tradition, physical anthropology was a science of race, and this was an essentially descriptive practice. For instance, in 1950 *Man* published the results of metastudies of the relative usefulness of various characteristics of living subjects (e.g., nose breadth) and skulls (e.g., orbital height) for helping "the student of race discriminate between different populations." The studies did not attempt to define race but simply compared the averages of popular measurements in the published literature; each sample series essentially represented a "race," and the greater the difference between interracial averages, the greater the significance of the characteristic (Tildesley 1950; van Bork-Feltkamp 1950). Such studies seem designed to demonstrate the vapid circularity of the race concept, but they also reveal physical anthropologists' vested professional interest in protecting the disciplinary turf of race from social scientists and population biologists. Their expertise was discriminating between races; if racial differences were of trivial social significance, so were they.

Despite Fagg's disavowal of discrimination "in the bad sense," the social value of this form of physical anthropology depended on a slippage between good scientific and bad social discrimination. In *Man*, Miriam Tildesley, the chairman of the International Committee for the Standardization of Anthropometric Technique and author of one of the studies described above, provided a hagiographic review of Sir Arthur Keith's autobiography, declaring that the aged don of British physical anthropology possessed "a hallmark of culture—tolerance" (Tidesley 1952).[17] Whatever his virtue as a colleague and friend, Keith is best remembered today for his theory that racial prejudice was a natural and necessary feature of evolution (and for being taken in by the Piltdown hoax, which

14. "Address Delivered by the Director-General to the Meeting of Physical Anthropologists and Geneticists for a Definition of the Concept of Race," 323.12 A 102/064(44) "51," Statement on Race: Expert Meeting of Physical Anthropologists and Genetists [sic]: Paris 1951, UNESCO Archives, Paris.

15. Shapiro to Métraux, February 20, 1951, 323.12 A 102, Statement on Race, Part II from January 1, 1951 to August 31, 1951.

16. They were almost all American or British: biologists Julian Huxley (UNESCO's first director-general) and Edwin Conklin; biochemist (and former director of the natural sciences department) Joseph Needham; and geneticists L. C. Dunn, Theodosius Dobzhansky, Gunnar Dahlberg, Curt Stern, and H. J. Muller. American social psychologists and sociologists and Gunnar Myrdal also reviewed the statement, but these were mostly experts who happened to be hanging around UNESCO House.

17. Ironically, the review appeared just above generally positive reviews of four pamphlets in UNESCO's The Race Question and Modern Science series.

would so embarrass the "classical anthropology" Tidesley practiced; Stocking 1988). Desirable evolutionary progress, according to Keith, was a product of often violent racial discrimination—an idea that hardly epitomized tolerance in the twentieth century. Fagg had solicited both Tildesley's and Keith's opinions on the original race statement before initiating the controversy with his letter to the *Times*. Professional interests and political convictions were entangled.

Indeed, the disciplinary conflict over the race statement obscured a more basic battle over who had the authority to represent science. The content of the fifteen paragraphs of the 1950 statement is less surprising than the list of eight authors. Before the war, when Shapiro and Franz Boas had attempted to organize an antiracist statement from physical anthropologists in response to Nazism, Boas recruited Earnest Hooten as a front man. During the 1930s, it was conventional wisdom that a Jew's authority on the race question would be undermined by a perception of bias (Barkan 1992:316). With farcical assuredness, WASPs generally ignored the corollary that the discovery of their own natural superiority might itself be suspect. (This is why sarcastic humor is generally a more effective weapon against scientific racism than earnest pleading.) WASPs, however, were noticeable by their absence in the expert panel convened to author the 1950 statement. Not only was the rapporteur Jewish, so were the representatives of France (Lévi-Strauss) and the United Kingdom (Morris Ginsberg). Even more dramatically, the committee elected the black American sociologist E. Franklin Frazier its chairman. The panel also included Ernest Beaglehole from New Zealand, Juan Comas from Mexico, L. A. Costa Pinto from Brazil, and Humayun Kabir from India. The emphasis on national/racial diversity reversed the virtually unchallenged prewar assumption that only whites really had the objectivity to answer the race question. The credibility of the 1950 statement was supposed to derive from the fact that it expressed the consensus of leading experts who represented the true diversity of the UN.

In a sense, this epistemological strategy accorded with Fagg's celebration of freely flourishing controversy in science; truth depended on the clash of different ideas within the scientific community. But Fagg appealed to a very different notion of authority when he objected that the leading British and French physical anthropologists had not participated in drafting the statement and branded it "the Ashley Montagu Statement." More significant than the disciplinary identity of the geneticists and physical anthropologists who authored the second statement was their nationality and race. A few Jews still made the cut, but no experts of color or scientists from outside Western Europe, Britain, and the United States did. Nevertheless, the principle of geographic and national diversity was institutionalized in the bureaucratic structure of the UN. Despite the challenge to the authority of the first statement, it signaled that the national and racial representativeness of the international scientific community was now an issue—inclusivity, as well as exclusivity, would increasingly become a mark of credibility (Selcer 2009).

"Not Just Equality as a Right and a Theory but Equality as a Fact"

When the General Conference resolved that UNESCO should produce a new Statement on Race in 1962, the politics of race in the international community had changed dramatically. This decade experienced a shift away from the human rights of individuals to the collective rights of "minorities"—from a focus on weakening primordial affiliations to the empowerment of oppressed groups (Burke 2008). This change was both a cause and an effect of decolonization. In order to speed the process of decolonization and leverage their position in the Cold War, colonial peoples joined together in the nonaligned movement. The Third World was defined in relation to the poles of the Cold War, but the solidarity of new nations was based on shared colonial histories and often expressed in racial terms. 1960 was a watershed; that year 17 newly independent African nations joined the UN system, accounting for nearly 20% of the votes in UNESCO's General Conference. A corresponding shift reverberated in the United States. By the mid-1960s, the civil rights movement transformed into what has been called a "minority rights revolution" (Skrentny 2002). In 1965, President Johnson issued Executive Order 11246 mandating affirmative action "to correct the effects of past and present discrimination" and announced at Howard University that Americans "seek not just legal equity but human ability, not just equality as a right and a theory but equality as a fact and equality as a result."[18] Equality of outcomes, not just opportunities, was now at stake.

Sol Tax, the inimitable editor of *Current Anthropology*, made the change in stakes clear in a letter copied to the SSD in preparation for the third Statement on Race. After rehearsing a favorite argument of liberal social scientists—that proving innate mental inequalities would require establishing true equality of opportunity—Tax added a new addendum. In the highly unlikely circumstances that substantial differences still persisted after multiple generations, "Negroes would have to be given greater educational opportunities to make up for whatever weaknesses have been demonstrated."[19] A simpler solution would have been to better norm tests of talent to the value of racial equality, but the point was much the same.

UNESCO's race program struggled to adapt to the evolving politics of race, but the executive board and NGOs continued to push for action in the fight against racial prejudice. The impetus for continued action received a strong push by per-

18. Lyndon B. Johnson, "To Fulfill These Rights," commencement address at Howard University, June 4, 1965. http://www.lbjlib.utexas.edu/johnson/archives.hom/speeches.hom/650604.asp.

19. Tax to Booker, February 12, 1962, 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, Part I up to December 31, 1963, UNESCO Archives, Paris.

ceived recrudesces of racism in Europe and the United States as well as the antiracist convictions of the new member states. Race riots in Notting Dale and Nottingham in 1958, which targeted West Indian immigrants, showed that colonial racial tensions were now a domestic British problem. In 1959 the desecration of a synagogue in Cologne inspired a wave of anti-Semitic acts in West Germany that spread into France (Bleich 2003; Rich 1990; UNESCO 1960).[20] In the United States, the success of the civil rights movement sparked a campaign of massive resistance throughout the South (Klarman 1994). One component of the push back against desegregation was a new wave of race science, exemplified by the founding of the journal *Mankind Quarterly* in 1960, which sought to demonstrate the biological basis of racial inequality. Most disturbingly for liberal anthropologists, the new generation of racist "pseudoscience" threatened to return to mainstream respectability in 1962 with the publication of Carleton Coon's *The Origin of Races* (Coon 1962). In this context, shoring up the scientific foundations of the antiracist script took on renewed urgency (Jackson 2005). And updating the race statement was also a mission with which Western governments—and crucially the United States—could find common ground with Third World nations.

Métraux's replacement at the race program, Francisco Benet, a young Spanish anthropologist with a degree from Columbia University, planned the drafting of the third statement. Benet essentially took two documents as the basis for discussion: UNESCO's 1951 statement and Coon's *The Origin of Races* (Coon 1962). He initially envisioned a single comprehensive statement addressing both the biological (which he termed anthropological) and sociological dimensions of race. He proposed that a sociological commission debate issues such as "the misuse of the term 'race'" and racial relations in "a context of cultural diversity" while an anthropological commission surveyed "the present state of our knowledge regarding the social life of early man" and tackled "one of the most important problems of the conference": the polytypic origins of *Homo sapiens* from different racial stocks of *Homo erectus*, which "in the hands of C.S. Coon, [had become again] a plausible scientific hypothesis."[21] With the notable exception of Julian Huxley, however, the experts Benet consulted (mostly anthropologists from the United States and Great Britain) warned against lending further credence to Coon's profoundly flawed work. In the end, the SSD chose to produce two documents, the first focusing solely on "the biological aspects of race" and clearing the ground for a second statement taking on the social determinants of racism. Yet again, physical anthropologists were called on to declare their own irrelevance.

SSD director T. H. Marshall pointed out another fundamental problem with Benet's initial agenda: was the new statement's aim "to make a scientific review of current knowledge, or to prepare a statement that will influence the attitude of the public at large?"[22] In fact, the proposed agenda's focus on recent and controversial findings implied that it was intended to influence the judgment of scientists. Although this distinction was never clearly articulated, the 1964 statement ended by asserting that "anthropologists should endeavour to prevent the results of their researches from being used in such a biased way that they would serve non-scientific ends." It is hard not to read this conclusion as a reprimand of Coon, who was a member of the expert committee that negotiated the statement, because the standard complaint against *The Origin of Races* was that it was easy fodder for racist propaganda (Dobzhansky 1963). Indeed, although the first statements had been intended to demonstrate the social implications of science to the public, they turned out to be key instruments for establishing and policing the norm against racism within the scientific community.[23] One of the ironies of propaganda is that often those who produce it are the ones who find themselves obliged to behave as if it were true.

Some of the participants in the first round of race statements welcomed the opportunity to correct what they perceived as flaws caused by political exigencies and the process of negotiation. Huxley, for example, wrote that he had "only signed the earlier [statement] with reluctance" and recommended Coon's book. He urged that UNESCO "must hold the fort between the two extreme positions, both untenable—that biological races are clear cut genetic entities . . . and the political extension of this view that some races are superior and some inferior; and the view that there is no scientific or genetic basis at all for the idea of genetically distinguishable ethnic groups, with mean differences in various genetic properties."[24] Huxley was not alone in hoping that the new statement would reassert the importance of biology. But the 1964 statement, titled "Proposals on the Biological Aspects of Race," turned out to be an even stronger declaration of biological equality and cultural determinacy. It stressed, for example, that racial classifications were never made on the basis of traits having "a universal biological value for the survival

20. Mara to Métraux, July 19, 1960; Métraux to Mara, August 10, 1960, 323.12:342.7, Race Discrimination and Human Rights: General, Part II from January 1, 1959 up to April 30, 1961, UNESCO Archives, Paris; Métraux to ODG, "14th session of Sub-Commission on Prevention of Discrimination and Protection of Minorities," December 21, 1961, 323.12:342.7, Race Discrimination and Human Rights: General, Part III from May 1, 1961 up to August 31, 1962, UNESCO Archives, Paris.

21. "International Meeting on Race, 1964," 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, Part I up to December 31, 1963, UNESCO Archives, Paris.

22. T. H. Marshall, "Some Comments on the Plan," 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, Part I up to December 31, 1963, UNESCO Archives, Paris.

23. For example, Juan Comas called out the racists behind the *Mankind Quarterly* by referring to the scientific consensus approved by the 1951 statement, and *Current Anthropology* reprinted the entire statement along with a new collection of letters on the subject (Comas 1961).

24. Huxley to Benet, June 20, 1963, 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, Part I up to December 31, 1963, UNESCO Archives, Paris.

of the human species," and therefore it was impossible "to speak in any way whatsoever of a general inferiority or superiority of this or that race," and it asserted that "differences in the achievements of different peoples must be attributed solely to their cultural history" (Montagu 1972:150–151).

More than a liberal drift in elite scientific opinion, the institutionalization of the norm of diversity assured this strong antiracist position. Although the SSD again consulted mostly American and British experts in planning the meeting, the logic of the UN required that the expert committee represent the diversity of the community of nations. The UN's increased membership meant that an even more diverse group of experts participated in drafting the third statement than had the first. Among the 22 signatories were representatives of Nigeria, Senegal, Japan, India, Mexico, Venezuela, Brazil, France, Belgium, Canada, Germany, Norway, the United States, and the United Kingdom. Most significantly, the Soviet Union had joined UNESCO in 1954, and three Russians, a Pole, and a Czech represented the Eastern Bloc. By coincidence, the seventh congress of the International Union of Anthropological and Ethnographic Sciences met in Moscow in 1964, and the SSD decided to save money and assure prestigious participants by linking its meeting of experts with the congress. This decision had the effect of focusing Soviet attention on the statement.[25] The third statement, therefore, had to make sense from a truly global diversity of perspectives.

That the meeting actually succeeded in producing a consensus demonstrates the hegemony of the antiracist script. Even scientists who did not necessarily believe the script lent their signatures to the third statement. After the meeting, the great German zoologist Bernard Rensch reported to the SSD that he did not actually agree with the statement's strong skepticism regarding genetically determined psychological differences between ethnic groups and thought "that in this case political convictions were stronger than scientific considerations"—the Russians, in particular, had been obstinate. But Rensch signed on to the statement because he believed the "practical target" of a unanimous antiracist declaration was more important than "little differences in formulation."[26]

Within the scientific community, the 1964 statement was relatively uncontroversial. The changed context was illuminated by the University of Chicago physiologist Dwight J. Ingle's attempt to provoke a sort of revival of the controversy over the 1950 statement in *Perspectives in Biology and Medicine*, which he edited. *Perspectives* is a free-ranging journal dedicated to the interdisciplinary exploration of philosophical and theoretical questions at the frontier of the life sciences. Like *Man*, *Perspectives* celebrated its role as "a forum for free and honest criticism, not carping but searching, and . . . not beholden to the renowned figure or figurehead or fearful of uncovering error or flabby thinking." In this early essay, a member of the editorial board invoked Thomas Jefferson: "For here we are not afraid to follow truth, wherever it may lead" (Bean 1958:225).

On the race question, Ingle believed scientific truth had been distorted by political pressure, and he made *Perspectives* a forum for interrogating the errors of "equalitarianism." In 1963, he "opened the pages of *Perspectives* to debate on the issue of race and intelligence," noting that he had "personally opposed the dogma of both equalitarians and racists because both groups rationalize value judgments behind a façade of flimsy evidence" (Ingle 1963:539). To provoke debate, he published UNESCO's 1964 statement, "Proposals on the Biological Aspects of Race," inserting his own point-by-point critique in an effort to reveal it as the epitome of equalitarian dissimulation (Ingle 1965).

By 1967, however, Ingle (1967) complained that "no one [had] accepted the invitation" to debate the race question in *Perspectives* (499). For many scientists, the visionary sociobiological race science of *Perspectives* appeared to look backward. Ingle not only published but promoted articles by the geneticist H. J. Muller and Julian Huxley (who served on the journal's advisory board) promoting eugenics, including elaborate artificial insemination schemes so that the genetically disadvantaged could have children (Huxley 1963; Muller 1959). Huxley's paper, originally delivered as the Galton Lecture to the Eugenics Society, explained that such a eugenic program would not be racist because of the greater intra- rather than interracial variation (although he was also busy advocating the notion that exceptionally high intelligence was more frequent in some races). Ingle used this same logic to describe himself as a champion of civil liberties and a righteous opponent of racial prejudice: he did not adhere to the typological concept of pure races; ipso facto, he was not racist. He could have signed UNESCO's second race statement in good faith.

But Ingle emphasized a crucial point that proponents of the postwar racial orthodoxy had elided: "the question of average genetic differences among the 'races' is important in the struggle for social and biological values" (1964:1528).[27] In 1967, after several summers of race riots in U.S. cities, Ingle explained the danger of ignoring innate average differences in aptitudes. Not only did equalitarianism mandate mediocrity, but also it would raise expectations: "When all Negroes are told that their problems are caused solely by racial discrimination and that none are inherent within themselves,

25. Hochfeld to Elmendjra, July 27, 1964, "Participation soviétique à la reunion d'experts sur les aspects biologiques de la question raciale," 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, "Part III from July 1, 1964 up to," UNESCO Archives, Paris.

26. Rensch to Bertrand, September 12, 1964, 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, "Part III from July 1, 1964 up to," UNESCO Archives, Paris.

27. This quotation is from Ingle's response to a flurry of outraged letters attacking his article in *Science* calling for further investigation into the determinants of differential racial intelligence and behavior and for eugenic interventions—importantly, he proposed subsidized sterilization for carriers of "substandard culture" as well as genes.

the ensuing hatred, frustration behavior—largely negative and destructive—and reverse racism become forms of social malignancy" (Ingle 1967:498). Equality was a legitimate social value, but it had pathological effects on a liberal society when wishful thinkers denied the fact of biological inequality.

A key biological argument of the antiracist script at the beginning of the 1950s—that innate capacities varied more within populations than between them—was now central to the script justifying racial inequality. Indeed, Ingle deployed the history of the argument as a shield against attacks that he was racist. And he allied with key actors in the production of the first Statements on Race. In addition to Huxley, Ingle published the population geneticist Theodosius Dobzhansky's "On Genetics, Sociology, and Politics" (Dobzhansky 1968). Perhaps the most respected postwar authority on the question of biological diversity, Dobzhansky was a renowned antiracist who had served as a sort of final judge of the biological correctness of the first UNESCO statements. "On Genetics, Sociology, and Politics," originally prepared for a 1964 Wenner-Gren symposium, argued for equality of opportunity but warned against expecting it to result in equality of "types" (Dobzhansky 1968:554). Indeed, in a series of popular lectures published in 1964, he appeared more worried about cultural than biological determinists and assumed a similar position on the race question as Ingle: "The sentimentalism of the equalitarians and the selfishness of the racists are equally purblind in light of biology" (111). In a section titled "In Praise of Diversity," Dobzhansky asserted that with equality of opportunity, members of all races would fall short of their ambitions.

> This involves pain and disappointment. It is debatable whether the pain and disappointment are easier to accept if they are felt to be owing to one's own shortcomings than to an injustice inflicted by others. The former is preferable to the society. It is surely intolerable to be told that one is not entitled even to try to climb a height because of the color of one's skin or a lack of social status in one's ancestors. (1964:111–112)

In a just meritocracy, everyone would have the chance to achieve excellence. But the biology of overlapping bell curves might, indeed probably would, still determine that each race was equal in its own way—some races, on average, might be better analytical thinkers, others more musically gifted.

Despite the name, population biology focused attention on minute differences between individuals. This focus was congenial to the movement for legal equality. But when social equality—equality of outcome—was at stake, it could slip into a rationalization of inequality. Tellingly, the bell curve became the de facto title of the new generation's racist script (Hernstein and Murray 1994; Jacoby and Glauberman 1995). The point is not exactly that the postwar liberal orthodoxy had always possessed the potential to rationalize racial inequality. Rather, it is that it had succeeded in winning the imprimatur of the scientific community and the blessings of UNESCO's member states precisely for this reason. Scripts are constructed and interpreted to match the political opportunities of particular historical moments. Even when they aspire to timeless truth, they must be assessed as conjunctural achievements.

Conclusion

UNESCO's Statements on Race were a product of both international and disciplinary politics. Although the statements were originally intended to demonstrate the social implications of race science to a popular audience, it is easier to read in their history the scientific implications of racial politics. Race had provided the organizing principle of physical anthropology; sorting their collections into racial categories was what physical anthropologists did. This scientific practice was meaningful because society was organized according to a racial hierarchy. Race was both a scientific and a commonsense classification system. The delegitimation of race as a social category, therefore, plunged physical anthropology into an existential crisis. The identity of physical anthropology was at stake in the production of scientific Statements on Race for a popular audience.

Like the lay citizens they attempted to educate, scientists' judgment of the social significance of race was determined by their life experiences. When Métraux was disturbed by the geneticist Alfred Sturtevant's racism, Dobzhansky wrote him that, considering Sturtevant had grown up in Alabama, "his views on the race problem must be considered remarkably liberal and reasonable."[28] *Drosophila* genetics were irrelevant. In accepting an invitation to participate in drafting the 1964 statement, Rensch (an ornithologist by trade) wrote, "Based on my personal experience with educated people and more primitively living tribes in India and Indonesia, I am convinced that the mental background of all races is rather similar. . . . But we cannot pretend that there are no important inherited differences of psychical capabilities."[29] When the taxonomist, leading architect of the evolutionary synthesis, and historian of biology Ernst Mayr explained the relationship between "the biology of race and the concept of equality" in an article written in 2002 (but which could as easily have been published in 1952), he asserted, "The rule that no individuals are the same was as true for the Stone Age natives of New Guinea as it is for a group of my Harvard colleagues"; he knew because his ornithological research had led him to be "one of the first outsiders to visit a native village in the interior of New Guinea" (Mayr 2002:93). The ultimate appeal to personal experience was not confined to scientists who studied flies or birds. In reply to Montagu's damning review of *The Origin of Races*, Coon dismissed Montagu's claim that

28. Dobzhansky to Métraux, April 29, 1952, 323.12 A 102, Statement on Race, Part IV from January 1, 1952 to March 31, 1953, UNESCO Archives, Paris.

29. Rensch to Benet, August 19, 1963, 323.1:574 A 064 (470) "64," Expert Meeting on the Biological Aspects of Race: 1964: Moscou, Part I up to December 31, 1963, UNESCO Archives, Paris.

"the more one gets to know people of different races the more fundamentally alike they appear to be" (Montagu 1963:362). "His impression is of no validity and less interest," Coon scoffed, "because he has done no field work. Having worked with people of all races but the Capoid on every inhabited continent, I know whereof I speak" (Coon 1963:363). Coon's appeal to the experience of fieldwork—not the standardized data of mental tests, for example—was a claim to privileged personal experience. The more he got to know people of different races, the more fundamentally different he found them. Examples could be multiplied, but I do not think the point is controversial. On the race question, scientists interpreted data in light of common sense. Perhaps because history was on their side, antiracist scientists were more honest about acknowledging that the answer to the race question depended on who possessed the power to measure merit.

The controversies over the Statements on Race were fundamentally about who was authorized to speak in the name of science—about whose common sense the scientific community would legitimate. As the anthropologist Alan Beals noted in a satirical response to Ingle's eugenic proposals, "Bigots" performed well "only on intelligence tests of their own devising" (Beals 1964). This common suspicion was why the political imperatives that assured the postwar institutionalization of the norm of diversity in the international scientific community were critical to the production of credible Statements on Race.

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Decode Me!

Anthropology and Personal Genomics

by Gísli Pálsson

With the advancement of genomic research, the issue of human variation has been redefined. Genomic anthropology has played an important role, drawing on and expanding anthropological understanding of human genomes and their differences. Focusing on the deCODEme and 23andMe projects, which offer personal services to people who wish to assess genetic risks for common diseases and to explore geographies of ancestry, in this article I discuss some of the larger implications of these developments. I shall argue that in the process, the boundary between experts and lay persons has been blurred and refashioned. I suggest, however, that it is also essential to attend to the potential hierarchies in the making in the assembly of personal genomic material and information through which consumers become active collaborators.

In November 2008, *Time* magazine announced its list of the best innovations of the year (Hamilton 2008). This time the retail DNA test 23andMe, a project drawing on genomic anthropology in one form or another, ranked number one.[1] Based in Mountain View, California, 23andMe offers to estimate a person's predisposition for a number of traits and diseases on the basis of a saliva test in return for $399.[2] The journal *Nature* also listed "Personal Genomics Goes Mainstream" as a top news story for the year. On November 16, 2007, a few days before the launching of 23andMe, another company, deCODE genetics, based in Reykjavík, Iceland, announced a similar service—deCODEme.[3] Several other companies, including Navigenics and Pathway Genomics, have either started or scheduled one form or another of retail genomics.[4] In 2008, 23andMe held a "Spit Party" during New York fashion week (Salkin 2008); volunteers would spit into a test tube. This is, indeed, consuming genomics, a rapidly growing business receiving both substantial financial support and intense public attention. Focusing on the deCODEme project, in this article I discuss some of the spin-offs of these developments; their larger implications for the understanding of self, personhood, and ancestry; and the ways in which they implicate both the public and genomic anthropology. What is anthropology to make of personal genomics and its own predicament?

With the advancement of genomic research, the issue of human variation has been redefined. Genomic anthropology has played an important role in both personal genomics and studies of human variation, drawing on and expanding anthropological understanding of human genomes and their differences. I shall argue that in the process of its development, genomic anthropology has coproduced new biosocial networks of associations along with a whole series of technologies and agencies engaged with biomedical research (Lock and Nguyen 2010). At the same time, the boundary between experts and lay persons has been blurred and refashioned. Projects such as 23andMe, deCODEme, Pathway Genomics, and Navigenics "democratize" genomics both in the sense that they offer test kits for a low price (ranging from $250 to $2,500)—within the reach of the public, at least not just the research elite and the wealthy—and in the sense that analyses and interpretations of genome scans are now a matter of intense public discussion through all kinds of media, including Web browsers and blog sites.

I would argue that up to a point, personal genomics has democratized genomic discourse. However, I also suggest that it is essential to attend to the biosocial relations of production

Gísli Pálsson is Professor of Anthropology in the Department of Anthropology at the University of Iceland (101 Reykjavík, Iceland [gpals@hi.is]). This paper was submitted 27 X 10, accepted 23 VI 11, and electronically published 14 XII 11.

1. Among the remaining 49 best innovations on *Time*'s list were the bionic hand (14), the synthetic organism (21), bionic contacts (24), the biomechanical energy harvester (33), and enhanced fingerprints (39). The year before, Apple's iPhone was the winner.
2. https://www.23andme.com/ (accessed December 8, 2008).
3. http://www.decodeme.com/ (accessed March 10, 2009).
4. A somewhat similar and yet radically different project is that of the Personal Genome Project, a study at Harvard University Medical School. Its goal is to challenge conventions on privacy by assembling genetic samples from 100,000 volunteers who agree "to share their genome sequence and other personal information . . . with the scientific community and the general public" (http://www.personalgenomes.org/participate.html [accessed December 8, 2008]), thereby advancing medical research. This is not a business enterprise and no payments are involved. The prime benefactors are research units that freely access people's genomes out in the open, a strange fusion of genetic voyeurism and genetic exhibitionism. It should be noted that 23andMe is also moving in the direction of health activism.

 0011-3204/2012/53S5-0016$10.00. DOI: 10.1086/662291

involved (Pálsson 2009*a*), the potential hierarchies in the making in the assembly of personal genomic material and information through which consumers become active collaborators. The new genetics has not only called for the notion of the biosocial (Rabinow 1996); to address the radical conflation of the natural and the cultural with the refashioning of life itself, associated advances in biomedicine and bioscience also make it pertinent to address the broad context of biosocial relations and labor processes (Dickenson 2007). While, as I shall suggest, in a late-Foucauldian fashion, the users of personal genomics can be said to work on themselves, seeking to know themselves and plan their future, their labor also needs to be situated in the biomedical mode of production that has generated personal genomics, the hybrid complex of living material and social relations evident in the ongoing production of biovalue. How, we must ask, do people become implicated in new labor processes as they subscribe to personal genomics services?

The outline of the discussion is as follows. The first two sections focus on shifting perspectives on human differences in the wake of the new genetics during the latter half of the twentieth century, as genomic anthropology began to compete with physical anthropology as the prime forum for analyses of human variation and history. This shift is illustrated partly with reference to the Icelandic context and the arrival of the biomedical company deCODE genetics. The next two sections deal with the deCODEme and 23andMe projects and the emerging biosocial communities associated with the development of personal genomics and the changing demarcations of expertise it represents. This is followed by a discussion of the implications of genetic ancestry and relatedness for understandings of self and personhood, the labor carried out by the users of personal genomics and the biosocial relations involved, and, finally, a concluding commentary on the main theoretical and empirical grounds covered. The empirical part of the discussion is primarily based on exchanges on the Web among consumers and genomic experts and my own experience of requesting a genome scan and exploring the results.

From Physical Anthropology to Molecular Anthropology

One useful avenue into early twentieth-century physical anthropology is the work of Ernest Albert Hooton at Harvard University. While his views were somewhat difficult to specify—he criticized Nazi physical anthropology, but at the same time he sponsored racist projects (see Marks 2008*a*: 244)—he was one of the most prominent physical anthropologists in America. His work *Up from the Ape*, originally published in 1931, emphasized the continuity of human reasoning on anatomical difference.

> Actually, science is forced to recognize the differences in physical characteristics between the great divisions of mankind. . . . For, the ordinary layman—the plain, untutored *Homo sapiens*—today and for the past thousands of years has observed these gross anatomical differences between the principal groups of this kind, has drawn the generally correct inference that they are transmitted from parents to children, and has attributed to them enormous political, sociological, psychological, and biological significance, rightly or wrongly. (Hooton 1946 [1931]:440–441)

While Hooton emphasized the antiquity of the theme of variability, he was eager to establish the autonomy of his scientific discipline with respect to the ignorant public. Commenting on race, he suggested that "confusions of usage are usually confined to the non-anthropological writing public. All anthropologists agree that the criteria of race are physical characteristics" (Hooton 1946 [1931]:447). At the same time, Hooton was keen to set his kind of science apart, irritated by challenges from social scientists.

> Man is an inveterate amateur of the taxonomy of his own kind. He cannot be argued out of the habit of connecting the physical differences he sees in individuals or groups with their equally obvious variations in behavior by any set of "social scientists," however loudly and persistently they tell him that there is no difference between black and white skins apart from exposure to the sun, and no difference between the psychology of a Mongolian and a White, apart from their having grown up in the Rice Bowl and the Dust Bowl, respectively. (Hooton 1946 [1931]:447)

For Hooton, skeletal material was the main source of information on human variability. The measurement and classification of bones became an obsession. This is underlined by the extensive cross-cultural bone collections of the Peabody Museum at Harvard University, a kind of ethnographic atlas engraved in human bones. One of the students at Harvard, the Canadian-Icelandic anthropologist-explorer Vilhjálmur Stefansson, who contributed to the Peabody collection with a sample of medieval Icelanders, later did extensive ethnographic fieldwork among Canadian Inuit (between 1907 and 1918). A frequent phrase in his notebooks is "measured a few heads today" (Pálsson 2005). Stefansson would move from one Inuit camp to another, lining up his noble savages to have them photographed and measured, setting up what he might have called measurement parties, precursors to the spit parties of 23andMe.

While the measurements of the bone people were exceedingly detailed, the classificatory scheme within which they were placed was highly simplistic and archaic, often with an implicit racial tone. Inevitably, the arrival of human genetics and biological anthropology after World War II provided a new avenue into the understanding of human variability, moving the kinds of measurements and classification of skeletal material typically practiced by physical anthropologists during the first half of the twentieth century to the sidelines. Did they represent new paradigmatic "thought styles," in

Fleck's sense of gestalt shifts in intellectual interests, in "the readiness for directed perception and appropriate assimilation of what has been perceived" (Fleck 1979 [1935]:142)? How radical were the breaks represented by the discovery of the double helix, the mapping of the human genome, the IT revolution and the Internet, the new economy, and related events and developments? Are we able to meaningfully situate ourselves and to domesticate our biases?[5] Interestingly, given the context of skeletal analyses, one of the key illustrations in Fleck's book, originally published in 1935, demonstrates the usefulness of his thought-style perspective through discussion of changing understandings of human anatomy. "To obtain an even clearer picture of how scientific observation differs when two different thought styles are involved," he suggests, "it is perhaps appropriate to compare anatomical descriptions and illustrations in early and recent text books" (Fleck 1979 [1935]:133).

As Glick (2008) remarks, the "great debate over race passed the Darwinian divide with scarcely an acknowledgement that anything had changed. Race was an issue marked by lack of ontological control, and that control would be gained slowly and with continuing conceptual difficulty until 'population thinking' introduced some clarity after World War II" (240). The anthropological terrain of human variability in the wake of the new genetics, however, proved to be highly differentiated and rapidly changing, with both radical innovations in methods and perspectives and surprising continuities. Sommer (2008) emphasizes successive contests, following the birth of what Zuckerkandl identified as "molecular anthropology" (Zuckerkandl 1963), over what counts as a legitimate epistemic object and authoritative information in the reconstruction of hominid evolution and human variation. The early molecular anthropologists became convinced of the intrinsic superiority and mathematical precision of direct molecular data in comparison with the subjectivity of readings of anatomic data and the fossil record. For instance, at Berkeley, Washburn "heralded the new technologies as finally providing some scientific base to claims about human evolution," ridiculing "comparative anatomy as a kind of pseudo-science that had given rise to just-so stories" (Sommer 2008:502). Over time, it was assumed, molecular studies would reduce if not eliminate the endemic bias involved when a species was studying itself informed by the belief in human exception. Ironically, however, in due course the analysts became overwhelmed by their faith in their methods. Vincent Sarich, for instance, one of those Washburn recruited at Berkeley, eventually argued against the mantra of "direct evidence" on the basis of molecular data, suggesting that "one no longer has the option of considering a fossil specimen older than about eight million years a hominid *no matter what it looks like*" (Sarich, cited in Sommer 2008:504 [emphasis Sommer's]). The thought style of molecular studies had become hegemonic, avoiding contradictory evidence.

Personal Genomics via Oxford and Reykjavík

The archaeology of the kind of personal genomics we now have on the horizon has several layers, among them maps of human genome diversity, population biobanks, digital genealogies, and the Internet. One of the pioneers of the genetics of ancestry and its commercialization through the Internet is Bryan Sykes of Oxford University, a human geneticist who published the first report on retrieving DNA from ancient bone (Hagelberg, Sykes, and Hedges 1989) and founded the genetics testing firm Oxford Ancestors, probably the first service of its kind. His company offers people an opportunity to see which "clan" they belong to, tracing their ancestry to one of the seven daughters of Eve (Sykes 2001). To attract customers, Sykes (2001) dramatizes the saga of the seven daughters: "What were they like, these women to whom almost everyone in Europe is connected by an unbroken, almost umbilical thread reaching back into the deep past?" (197). For him, the power of DNA consists in the "token or a symbol of the shared ancestry it reveals rather than the body chemistry it directly controls" (290). Common membership in a clan establishes a profound connection: "We look at each other and sense our deep umbilical connection. . . . I feel we have *something very deep* in common" (289 [emphasis added]).

One of the interesting sites of biomedical experimenting and personal genomics over the last decade is Iceland. Here as elsewhere, the introduction of human genetics represented a change in thought style, with new players and perspectives. Soon after World War II, physical anthropology developed at the University of Iceland through the work of Jón Steffensen, professor of medicine (see, e.g., Steffensen 1953), and Jens Pálsson, founder and former director of the Institute of Anthropology (see, e.g., Pálsson 1976). Both of them worked on skeletal material.[6] Their interests, however, differed significantly (Pálsson and Guðbjörnsson, forthcoming); Steffensen emphasized medical and cultural issues without any hint at racial issues, whereas Pálsson tended to draw on public discourses on racial differences and "Nordic" people (for discussions of similar themes in Denmark, Norway, and Sweden,

5. Fleck (1979 [1935]) warns that the taken-for-granted status of the thought style of the observer poses a serious problem of bias: "To the unsophisticated research worker limited by his own thought style, any alien thought style appears like a free flight of fancy, because he can see only that which is active and almost arbitrary about it. His own thought style, in contrast, appears imperative to him . . . as a result of education and training as well as through his participation in the communication of thoughts within his collective" (141).

6. The earliest skeletal study of Icelanders involved the Stefansson collection at the Peabody Museum. The most detailed discussion the collection received was in a doctoral dissertation by Carl C. Seltzer, submitted to Harvard University (Seltzer 1933), which dealt with the physical characteristics of Icelanders and their racial origins. Before Seltzer wrote his thesis, the collection gave rise to some speculation by Hooton concerning comparison of the physical characteristics of Icelanders and other peoples.

see, e.g., Koch 1996; Kyllingstad 2012; and Pred 2000, respectively).

Jens Pálsson (no relation to me) partly studied in Germany, where he became affiliated with the Mainz Institute of Anthropology. In Mainz he not only found a place for his fascination with racial types but also received financial support. In particular, he was supported by Ilse Schwidetzky, head of the institute, who was keen to get access to Icelandic data. Despite skillful lobbying in Iceland, decades of data collection, teams of collaborators, and considerable national and international funding, J. Pálsson's legacy remains small. Thus, he never taught or mentored anybody to speak of. Also, his publication record was meager. Moreover, his text was devoid of theory, focusing on classification and catalogs in the fashion of German physical anthropology, particularly the Breslauer or Breslauer/Mainzer Schule (Preuß 2009:129). Perhaps he felt at the end of his career, at a time of rapid advances in human genetics, that his work was rather suddenly out of touch, a thought style that failed to sustain attention. Perhaps, too, he had also come to realize that the close connections to Schwidetzky and her entourage at Mainz, a branch of anthropology that remained publicly implicated with the Nazi past, isolated him both internationally and at home. Schwidetzky's anthropology was firmly grounded in the measurements of the Breslauer/Mainzer Schule and the racial typology of Egon Freiherr von Eickstedts, the leading racial theorist of Nazi Germany (see Preuß 2009:132–134).

The establishment in 1966 of the Genetics Committee at the University of Iceland marked the local development of human genetics. The committee focused on "the recording in one place on punch cards various genetic information on Icelanders" (Genetics Committee of the University of Iceland 1974:2). The database of the genetic committee was an indirect precursor to the key projects of deCODE genetics, its patient group studies, and its doomed plan for the Icelandic Health Sector Database proposed in 1998 for the purpose of advancing personal medicine (Pálsson 2007). The deCODE projects draw on the development of flexible interdisciplinary research teams, powerful genomic laboratories, bioinformatic frameworks, and digital genealogies, all of which have been involved in the making of deCODEme.

Biological anthropology arrived on the Icelandic scene with the doctoral work of Agnar Helgason at Oxford University. Since completing his doctorate—which explored the history of Icelanders from the time of settlement in the ninth century through mitochondrial DNA (mtDNA) sequences, Y-chromosome haplotypes, and genealogy (Helgason 2001)—Helgason has been employed by deCODE genetics, teaching at the same time in the Department of Anthropology at the University of Iceland. His work on a range of genomic projects, including deCODEme (Helgason and Stefánsson 2010), illustrates the new networks and associations within which anthropology is currently embedded (Pálsson 2008), freely straddling between disciplines, fusing at the same time the practical, the theoretical, the local, and the global.

The deCODEme project is able to draw on several kinds of assets, in particular large-scale efforts over several years to discover the genetic factors involved in common diseases and extensive genomic anthropological work on human populations and their migrations, ancestry, and mixing. All of this has been important for developing the analyses and interactive frameworks offered by deCODEme. The project now offers both a "complete" scan ($2,000) and two more narrow scans focusing on specific conditions: cancer ($500) and cardiovascular problems ($500).

deCODEme: A Somewhat Personal Guided Tour

I signed up for the complete scan, curious to find out how anthropological expertise was implicated in the project, to explore the analyses it offers, and to see what the scan might tell me about myself and my roots. Two weeks after I sent my cheek swabs and the relevant forms, I received an e-mail from the company. The results were now available, and I would be able to access them through the password provided. Since then, I have regularly received messages from the company alerting me to both updated and new conditions and to further analyses of traits and health risks. Once I logged on to see the results, I was urged to "have fun browsing [my] . . . genome," "dig into [my] . . . DNA," explore my ancestry and my "genetic risks," play with maps and other visuals, search for specific genetic variants (SNPs or "snips"), and download my genotypes for 1.2 million SNPs (a 33-Mb data file).

The search for ancestry has six key features. The first, the "atlas," provides a comparison of one's genetic code with that of people from all over the world based on several hundred thousand genetic variants and more than 1,000 reference individuals from 50 different populations worldwide (fig. 1). The atlas compares my genome with reference populations throughout the world, ranking regional clusters (1 to 6) in terms of their relevance for me, in the order of genetic similarity: Europe (1), Southwest Asia (2), East Asia (3), America (4), Oceania (5), and Africa (6). In each case, I can zoom in on the population involved.

My genome, not surprisingly, turned out to have most in common with European reference groups (a genetic similarity of 83.99%), in particular those of Iceland, the Orkney Islands, France, and Russia. More astonishingly, the second feature, "ancestral origins," indicates that judging from chromosomes 1–22, my ancestry is no less than 7% East Asian, 16% according to the X chromosome, considerably higher than for most Icelanders. I found this an interesting and puzzling revelation. To speak of "genealogical dis-ease" (Rapp, Heath, and Taussig 2001)—to use a term developed by anthropologists studying what people make of genetic information about their roots and ancestry—would, however, be an overstatement.

The analysis of mtDNA establishes one's place in a matri-

Figure 1. My genetic atlas (according to deCODEme 2009; http://www.decodeme.com/). A color version of this figure is available in the online edition of *Current Anthropology*.

lineal family tree spanning 170,000 years. It turns out I belong to "mitogroup R*," a category shared by 4.8% of deCODEme users, all of whom can trace their mtDNA to a woman thought to have lived about 60,000 years ago, probably somewhere in the Near East. The analysis of my paternal DNA, on the other hand, shows that I belong to "Y-group R1a," a category shared with 10.3% of deCODEme users tracing their Y chromosome back to one man who is thought to have lived about 10,000 to 15,000 years ago, probably in Western Asia.

A further feature allows users to explore their "map of kinship," a visual representation of genetic space on the basis of principal component analysis. Given this evidence, I occupy a somewhat marginal position, neither firmly within the European reference group nor any of the others, probably reflecting the puzzling observation mentioned above about my East Asian ancestry. The final feature allows the user to compare his or her genome with that of a reference individual from any of the populations included in the data set.

The other main service offered by deCODEme is that of analyzing the genome with respect to specific traits and health risks. For some weeks I resisted the lure of the health results. Both of my parents had struggled with cancer, and I was not that interested in the kind of fortune telling offered by personal genomics. I guess news of the New York spit party helped to change my mind. Somehow, collective spitting appealed to the anthropologist curious about the social life of DNA and the implications of the new genetics in the modern age. My results for the 47 diseases and traits currently covered are based on calculations comparing my genome to sequences of participants in studies published in the scholarly literature. To access results for some diseases, I was invited to read about the genetic and medical details and to sign a statement about informed consent by clicking on "Accept." I need not bore the reader with the personal details. Suffice it to say that some of the information provided sounds trivial (no alcohol flush reaction), some of it resonates with what I thought I already knew (I am less likely than the general population "to become nicotine dependent [15% or less]"), some results are encouraging (I have low lifetime risk for some diseases, much less than for males of European ancestry in general), and some details may promote the hypochondriac in me to request further medical information (my risks for some diseases are slightly higher than those of my genetically significant others). When presented with these results, I was offered details on the mathematics of risk analysis. Also, I was invited to zoom in on my genomic landscape, focusing on a part of a chromosome and the location of specific mutations reportedly responsible for potential traits or diseases. Again, there are some surprises and some food for thought.

The Genome Browser of deCODEme allows users to compare their complete data with friends and family. While my reference group of friends and family includes both hypochondriacs and anthropologists, so far they have seen few good reasons to participate and, as a result, there is not much to compare. The Web site, however, allowed me to examine my genetic sharing with "famous" people. Here, sharing is indicated visually by the coloring of the relevant bits of the chromosomes. No doubt personal genomics is becoming both a family affair and a global concern. At any rate, a thriving imagined community of the users of personal genomics projects has been developing on the Internet.

Cyberspace: The Experts and the Rest

A number of Web sites testify to a lively discourse on the issues involved, including thinkgene.com, dna-forums.org, Eye on DNA,[7] Urban Semiotics, and Dienekes' Anthropology Blog. The last one is "dedicated to human population genetics, physical anthropology, archaeology, and history." Judging from these Web sites, there is more interest in exploring ancestry than health risks. Perhaps users are reluctant to reveal their health risks in public, although they may be keen to download the relevant information for their own purposes. Some of the Web sites referred to are focused on specific personal genomics projects while others are more general. Users engage with the goals of personal genomics, analyses of their own genome, and comments expressed through the expanding virtual community of the Internet.

Often, users comment on each others' roots and genetic identification, usually identifying themselves by first names or nicknames. In World Families Forum,[8] one user offers the following statement, referring to one of Sykes's seven daughters of Eve, Ursula: "Both my wife and I are U5a1a (Clan Ursula) members and on my Y side I am an R1b1c. We believe we are part of the Vandals, Visigoths, and the Normans groups of people who settled in Sicily—who were ancient Vikings/Scandinavians." Another user responds: "Some information about your R1b1c. . . . It is unlikely that it is of Viking descent. More likely it originated in western Russia." In some cases, users deliberately request advice or interpretation: "This is a nice website and I have enjoyed it. I have recently had my mtDNA tested and have been identified as a U5 (weak match). Can anyone please tell me what the 'weak match' means? I thought a match was a match. Thanks bunches!!"

Clearly, this is a biosocial community in the making, social networks based on identification with genomic characteristics. A certain "countess" comments (New Unofficial Oxford Ancestors), "I just found out that I am a daughter of Ulrike. Wow, how amazing to feel this connection to the past. . . . My ancestry is almost all British Isles, but I didn't know we were descended from Vikings!"[9] Another user, "PDHOTLEN," reported after "psyching" himself or herself to have the results of a mtDNA test: "My results (U5) say that my maternal ancestors were responsible for the demise of the Neanderthals in Europe. . . . Does anyone out there have a similar mtDNA?" Soon there was a response: "I also have U5, but my ancestors were probably not among the cave decorating Cro-Magnons. They are the Saami people up in the northwestern corner of Europe. . . . Getting the results from the genetic tests has made me a hobby genealogist and with good helpers I have been able to trace some Saami relatives back to the beginning of 1600."[10]

While many users become knowledgeable and skillful readers of genomic texts in the process of blogging, sometimes the technical jargon becomes overpowering, reaffirming the dividing line between experts and lay persons. Consider the following exchange.

> Didier: The problem with the shorthand naming is that you lose contact with the subgrouping. Only those familiar with R1b haplogroups would know that R-U106 is distinct of R-U152, R-SRY2627, R-L21 because those last 3 are P312+ while U106 is P312−. I would favor a simplified nomenclature. . . . The others are cryptic, very interesting for the specialist (those of us discussing these issues are specialists) but very confusing for non-specialists.
>
> Banks: It appears to me that you specialists will be preaching to the choir because the congregation (non-specialists) will be long lost in a state of utter confusion. . . . I have no knowledge of the "longhand" let alone the shorthand of DNA classifications. I only recently learned to spell haplotype.[11]

Occasionally, renowned experts drop in to offer a point of view or to clarify some issue. At one point it seems that James D. Watson, who shared a Nobel Prize for unraveling the structure of DNA, entered the scene, to underline the reliability of the testing provided by personal genomics: "It is important to note that no one is questioning the accuracy of the actual sequencing behind these services"; immediately, there was a respectful response from "docduke": "If that last post was by James D. Watson, I would like to take this opportunity (if you check back), to thank you for your candid, and honest, statements here as elsewhere. Intellectual honesty is becoming altogether too rare in public scientific discussions these days!"[12]

Quite possibly, the name "docduke" suggests that the blogger's point in referring to "candid, and honest, statements" is primarily to show support implicitly of Watson's statement in the *Times* of London about race, genetics, and the intelligence of many Africans: "All our social policies are based on the fact that their intelligence is the same as ours—whereas all the testing says not really" (Watson 2007). Because Watson's own genome is online, his ancestry is open for scrutiny. Exploring the data, Stefánsson at deCODE genetics and his team concluded, with a hint of irony, that whatever Watson

7. http://www.eyeondna.com (accessed December 10, 2008).
8. http://www.worldfamilies.net/forum/index.php (accessed December 10, 2008).
9. http://www.familydna.co.uk (accessed December 10, 2008).
10. http://www.familytreedna.com (accessed December 18, 2008).
11. http://www.worldfamilies.net/forum/index.php (accessed December 10, 2008).
12. http://blogs.chron.com (accessed December 19, 2008).

may think of black Africans, 16% of his own genes probably come from black ancestors.[13]

Some bloggers take a playful attitude to genome testing. One woman who had her husband "tested" for fun was questioned about her ethics.

> Megan: I admit it. I have no self-discipline when it comes to genetic genealogy. When deCODEme was launched, I had to be one of the first in line to get tested. So I ordered . . . and received results . . . my husband's results, that is. I thought this might be a little more interesting since he sports a Y chromosome.
>
> Marie: Did you get your husband's permission before using his DNA? . . . How does he feel about you sharing his results with the world?
>
> Megan: Yes, rest assured, I checked with him several times just to be sure.[14]

Sometimes people check whether they are being cheated by genomic services. One blogger claimed to know of "at least one case . . . where a customer deliberately submitted a dog's DNA just to 'test' the company. He was willing to pay for his little experiment, and yes, the company figured out exactly what had happened!" Sometimes users subscribe to two or more services to compare their usefulness and explore their methods and reliability.

> I tested for both 23andMe and deCODEme. I just received my 23andMe results and I am quite surprised by the admixture test named Ancestry Painting. Indeed, I am half Berber, half French (E3b1b-M81, MtDna = I) but my 23andMe results showed 100% European (African and Asian = 0%) whereas my deCODEme results showed 81% European, 13% African ancestry and 6% Asian. . . . Do you know if these very different results can be because of different method used between 23andMe and deCODEme?[15]

Concerned more with estimates of health risks than ancestry, Francis Collins, director of the U.S. National Institutes of Health, caused some stir in 2009 by announcing that he had signed up for several personal genomics services under a false name, comparing and testing their prognoses. While sequence-wise, he concluded, the tests appeared to be "highly accurate," the final risk score varied from case to case: "one company used 5 single nucleotide polymorphisms, or SNPs, to calculate risk for a particular disease, pronouncing Collins at low risk. Another used 10 SNPs, placing him at high risk, and the third used 15, concluding that he is at average risk."[16]

It seems that the virtual community of genetic citizens is actively debating and negotiating roots, identities, and health risks by fusing the expertise of geneticists and nonprofessionals for the purpose of scrutinizing SNPs and comparing haplotypes (McGuire et al. 2009). To some extent, however, the experts and the public engage in separate conversations. Nevertheless, representing just a tip of a rapidly expanding iceberg, the preceding excerpts from the Internet give some idea of the discursive community involved, the relations established, and the concerns people have.

Technologies of the Self: From Genome to Identity

In some of his last writings, Foucault (1988) shifted his attention from systems of domination to the agency and experience of the individual, drawing attention to the particular kind of subjectivity characteristic for the modern age and what he called "technologies of the self." It seems reasonable to argue that personal genomics represent one example of technologies of the self (Hacking 2006). Indeed, the genomics of ancestry is often assumed to provide an important avenue into identity and personhood. As Pinker (2009) observes, "Affordable genotyping may offer new kinds of answers to the question 'Who am I?'—our ruminations about our ancestry, our vulnerabilities, our character and our choices in life." Significantly, Sykes's book on ancestry (2001), which opens with the question "Where do I come from?" closes with a chapter titled "A Sense of Self." This point is also underlined by Anne Wojcicki, the cofounder of 23andMe; the 600,000 genetic markers interpreted by 23andMe, she argues, are "the digital manifestation of you" (Hamilton 2008). Knowing where we come from, we apparently also know who we are.

Not only is there a growing popular literature that equates human beings with their genomes (Angrist 2010), this is also a theme explored in endless blogs on the Web sites mentioned above. One of the relevant statements suggests, in a somewhat humorous tone, that DNA "speaks" to its host.

> I received my DNA results earlier this year and was surprised to find myself in clan Ulrike. I have traced six generations of maternal ancestors in the Beds/Northants borders region. The Viking invaders did travel into this area. . . . I have always been attracted to northern wilderness and have visited Alaska, Greenland/Iceland and Siberia. *Is this my DNA speaking*?![17] (Emphasis added)

DNA, then, from the past tends to be seen as an avenue into the future, our essence and fate.

Battaglia (1995) suggests an "approach to selfhood as embodied and historically situated practical knowledge," which in her view "prompts a larger question of rhetoric, namely, what *use* a particular notion of self has for someone or for some collectivity" (3). Given such an approach, an important theme on the anthropological agenda is to explore the rhetoric of personal genomics and the pragmatic uses of genetic notions of self for the actors involved (see, e.g., Gibbon and

13. http://abcnews.go.com/blogs/technology/2007/12/the-genome-of-j/ (accessed December 10, 2007).

14. http://rootstelevision.com/blogs (accessed December 19, 2008).

15. http://dienekes.blogspot.com/ (accessed December 8, 2008).

16. http://scienceblogs.com/geneticfuture/, June 2009 (accessed May 5, 2011).

17. http://www.familydna.co.uk (accessed December 10, 2008).

Novas 2008). Nowadays, with personal genomics, the collection and analysis of DNA is closely linked to commercial marketing of identity, including "race." Anxious to explore the unique signatures of their genomes and to care for their selves and their bodies, people are buying in rather than being solicited or tracked down. In the process, they facilitate the construction of gigantic DNA assemblies, coproducing knowledge of genomic differences.

The companies involved in personal genomics suggest that their services facilitate the "democratization" of genomic knowledge, emphasizing consumers' relative independence of the medical establishment. Thus, the claim by deCODEme: "We wanted not only to empower the public, but also to give students, academics, physicians and other professions with an interest in genetics a chance to get a more in-depth view of their code and genome."[18] Indeed, as we have seen, users draw their own conclusions and sometimes engage in dialogues with genomic experts, becoming experts themselves in the process (see, e.g., Heath, Rapp, and Taussig 2004; Lee and Crawley 2009). In a sense, then, this is science from below (Harding 2008). One example is SNPedia. Drawing on summaries of peer-reviewed papers presumed to be relevant for given genomic data, it allows users of different testing services to pool personal data, to learn more about their own genotypes, and to explore the effects of variations in DNA. A growing literature empirically explores the extent to which personal genomics facilitates changes in lifestyle in terms of health, diet, and exercise (see, e.g., Bloss, Schork, and Topol 2011); so far, the results are not that convincing.

Spitting and Snipping: Biosocial Relations of Production

No doubt, personal genomics of the kind discussed here involve an element of empowerment. Some qualifications, however, are needed. Prainsack et al. (Prainsack et al. 2008) argue that while relaxing the genetic protectionism rampant in recent decades may be a good thing, giving people an opportunity to become active governors of their genomes, the arguments about individual freedom, informed choices, and the unregulated genomic marketplace should be taken with a grain of salt. For one thing, they disguise the fact "that personal genomics is pushing the individualization of responsibilities one step further" (Prainsack et al. 2008:34). Anthropology can play an important role on this front by exploring what such individualization means and what people expect from genomics, providing "thick" descriptions (see, e.g., Browner and Preloran 2010; Nelson 2008; Santos et al. 2009).

Another qualification relating to the agency of the users of genomics services is also essential; this concerns the labor they perform for personal genomics services rather than their opportunity to comment, interpret, and engage in a dialogue on methods and results. It is pertinent to broaden the notion of the biosocial in order to highlight biosocial relations of production, the labor processes and hierarchies associated with emergent biocapital (Pálsson 2009*a*). To what extent do genomic services engage the bodies and labor power of their "consumers" in biosocial relations and hierarchies? The spokespersons for 23andMe, unlike most of the other projects, have been quite open about the issue of alternative uses of their data. Wojcicki suggests signing up for 23andMe is "a great way for individuals to be involved in the research world. . . . You will have a profile, and something almost like a ribbon marking participation in these different research papers. It will be like, 'How many *Nature* articles have you been part of?'"[19] This is highlighted in a comment on one of the Web sites: "23andMe will be sitting in one of the largest genetic databases on Earth. And there's no opting out."[20] Drawing on her work on 23andMe, Levina (2010) argues that "life in the network society requires of its denizens a constant contribution to the growth of the network" (2).

Arguably, then, the people contributing cheek swabs to personal genomics services are engaging in a labor process that ultimately results in large-scale biobanking. deCODEme is part and parcel of its mother company deCODE genetics, whose purpose is to advance biomedical research and pharmaceutical development. Although the company seems to have no plans to directly draw on its personal genomics data in its biomedical research, a closer integration might take place later on. The kind of personal medicine pioneered by deCODE genetics and similar companies has suffered serious setbacks as the promised medical results have largely failed to materialize. While knowledge of the human genome and human variation has been significantly advanced, so far there is not much to sell.

Whatever their current ambitions, personal genomics projects are likely to connect with larger biomedical projects in the future. Given the possibility of hacking genomic data (Aldhouse and Reilly 2009), the clients of personal genomics companies may eventually be contributing to projects beyond the awareness and control of the services they have contracted. Leaving aside somewhat marginal cases of legal violation, the financial and technical links between 23andMe and Google may be indicative of new hybrid forms of biobanking and bioinformatics that fundamentally change the rules of the game. These developments necessarily invite pressing ethical questions. While the original phase of sampling may have respected community concerns and standard procedures of informed consent, such concerns and procedures may later on, during the phase of large-scale banking, become mean-

18. http://www.decodeme.com/ (accessed March 10, 2009).

19. "23andMe's mission: connecting all people on the DNA level or social networking XY.0." http://venturebeat.com/2007/11/19/23andme-will-the-personal-genomics-company-need-big-pharma-to-make-money/. November 19, 2007.

20. http://venturebeat.com (accessed December 10, 2008).

ingless and ineffective. Much depends on governance structures and the designs of the assemblies involved. Personal genomics needs not be the patient-centered medicine consumers are often promised (Brody 2009, chap. 3).

The possibility of linking a variety of scattered biomedical databases is not that remote. Thanks to the development of bioinformatics and the Internet, it is no longer necessary, or even feasible, to assume a central "hub" with monopoly of access. Already, there is much talk of "federated" databases; such databases "are a more complicated solution in terms of the required technologies, but they bring certain advantages that cannot be endowed by a centralized database" (Thorisson, Muilu, and Brookes 2009:13). Record details from remote sources may now be directly searchable by other computers taking part in federation. Spitting and snipping, after all, is biosocial work, potentially contributing to the global networks and hierarchies involved in the manufacture of biovalue.

Concluding Remarks

The image and the report in the *New York Times* regarding the launching of 23andMe draws attention to the role of metaphors. Metaphors, just like DNA, frequently undergo mutations, recombining available material from everyday language and experience. Like life itself, social discourse generates new frameworks in the process of its unfolding. The notion of the "spitting image" is a case in point (Pálsson 2009*b*). While 23andMe is probably the only personal genomics project that uses "spittoon" samples and the others seem generally to draw on buccal swabs, the "Spit Party" nicely captures various aspects of personal genomics and the gene talk on which it is based, the mechanisms of inheritance, the image of the double helix, the matching or mismatching disclosed through the sequencing of DNA material, and the establishing of distance and ancestry, both genetic and social. When spitting out one's saliva, one is presumed to provide a spitting image of oneself encoded in DNA. It is important to keep in mind, however, that the genome is not just a personal issue but also a matter of cultural identity and "ethnic" belonging. Genome researchers have repeatedly been painfully reminded that for some "native" groups the human genome is primarily a sacred phenomenon. To the extent that these groups describe the genome as a "resource," it is cultural capital, intangible property that is inseparable from the cultural meanings that it represents, that needs to be collectively guarded.

As we have seen, there is a rapidly growing interest in personal genomics for the purpose of reconstructing our past and celebrating our emerging biosociality and for managing our lives and our future. Day by day, the companies involved offer additional services on their Web sites, further details on diseases and traits, higher resolutions of data, and more powerful machines, diagnostic chips, visual presentations, and interactive features. Anthropology is implicated in these developments at several levels, contributing to the key data sets employed on human history and variability.

Several personal genomics services provide information on both ancestry and health risks. For consumers, it seems, the former kind of information, the extraction of history from what Zuckerkandl called molecular "semantides" (see Sommer 2008:506), usually involves a fair amount of playfulness, sometimes with undertones on identity, race, and networking, while the latter is inevitably associated with serious issues relating to medicine and lifestyle. Explorations of ancestry and health risks, however, overlap, enmeshing users in new biosocial relations and networks. One may wonder whether the relative importance of the two kinds of services may not be considerably altered in the near future.

There are serious anthropological and philosophical doubts about important issues relating to ancestry, including the shape of the family tree, the validity of the molecular-clock hypothesis, and the sampling of populations (Bolnick et al. 2007; Gannett 2003; Marks 2008*b*). Analyses of ancestry in personal genomics services, however, are likely to remain more or less intact, partly because there is not so much at stake and, in any case, it is largely play. Studies of the genomics of diseases, in contrast, are riddled with contests, doubts, and conflict. Most common diseases, it seems, are only minimally explained by genetic factors, and in each case a great number of genes is likely to be involved. Last but not least, there is growing evidence for the importance of epigenetic factors beyond the simple concept of DNA sequence. Prainsack et al. express some of the doubts: "Personal-genomics customers are already going through a process of disenchantment: it is increasingly clear how little power SNP-based readouts of a person's 'genotype' offer for predicting future ailments in an individual" (Prainsack et al. 2008:35).

Given the evidence and the growing public awareness of it (see, e.g., Hall 2009), why would people bother to consult their health risks with personal genomics services? The hype has, indeed, faded a bit, and some of the key players, in particular deCODE genetics and 23andMe, have experienced financial difficulties. There may be good grounds, however, for arguing that personal genomics will continue to thrive. While deCODE genetics filed for bankruptcy in November 2009, it was quickly refinanced and restructured, and deCODEme is still in operation. It seems unlikely that the narcissistic pleasures involved in the exploration of ancestry and the genetics of health risks are withering away, given the central place of the human body in late modernity. Also, there are immense financial stakes and concerns on the global level for biotechnical and pharmaceutical companies. Moreover, the quality, magnitude, and comprehensiveness of knowledge can only increase with time. The power of computing machinery continues to expand, and cheap complete sequencing is within reach. As a result, one may expect personal genomics projects to expand, realigning experts and consumers, institutions and disciplines, including genomic anthropology.

The narcissistic pleasures of late modernity are reaching new levels that neither Lasch (1979) nor Foucault (1988) could anticipate. Personal genomics is just one example. Another example, perhaps, is the use of humans as model organisms in biological experimenting, as human cellular material replaces that of mice and fruit flies, a point emphasized by Rheinberger (2009): "Many experiments are now being carried out with human cells directly. . . . With man becoming, in a sense, a model organism of his own, 'modeling' inevitably takes on the meaning and the form of *human modification*" (8). One suspects this significant shift will have profound implications for anthropology and its understanding of the species and its variability, although the terrain is just beginning to be explored.

In recent years, the notion of the tree of life—a central notion for Darwin, Linnaeus, and much of biological thought since their time—has been seriously challenged by a strange mix of scholars such as W. Ford Doolittle, Tim Ingold, Marilyn Strathern, Gilles Deleuze, and Félix Guattari. For one thing, it has been reasoned, the tree of life might turn out to be a net or a rhizome, with endless reconnections rather than treelike bifurcation. While it is possible that theoretical challenges to the tree model and the empirical evidence available only minimally blur the main picture, simply shaking the tree a bit, they raise fundamental questions as to what should count as relatedness. Why, indeed, Helmreich (2009) wonders, should we stick with reductionist gene talk? In the current age of biosociality, life has become increasingly disembodied, cultured, informatic, and rewritable.

Just as the history of physical anthropology and the bone collectors of the last century coalesced at a measuring party, the saga of modern biological anthropology and personal genomics coalesces at a spit party. In some respect, the two parties are rather different. Thus, whereas at the former case, research subjects were tracked down for measurements, often in the context of a colonial hierarchy, in the latter case, subjects offer themselves for the project in the hope that they may, in the process, discover and take care of their selves and bodies. This underlines radically different biosocial relations of production. The measuring party and the spit party, however, have one serious flaw in common, namely, the subtext of human variation, "race," and the presumed split between the biological and the social. After all, "biology" and "society" are not separate realities or categories of being. Human variation, including that identified as "race," is a thoroughly relational, biosocial state of affairs, collective history embodied in the habitus. Theodosius Dobzhansky once remarked that "nothing in biology makes sense except in the light of evolution" (Smocovitis 2012). Given the deliberate conflation of the biological and the social in the wake of the new genetics, the human configuring of life itself, and the advancing evidence on epigenetics and developmental systems (Griffiths and Gray 1998), it would be more appropriate, twisting Dobzhansky a bit, to state that nothing in biology makes sense anyway, except in the light of the irreducible unity of the biological and the social.

The big challenge for anthropology now is to realign the biological and the social on new terms in a nonreductionist fashion. We can continue to craft our professional selves on two different tracks and to practice the study of *anthropos* as if it involved the investigation of two radically separated domains, defending the subdisciplinary boundaries as if they were engraved in our subjects, but it would be both ethnocentric and out of time. It is time to rethink the field on the assumption that *Homo sapiens* is an undivided being and that decoding it—to the extent that the language of "decoding" is the appropriate one—requires integrative perspectives that in the absence of a better nondualistic language resonate with our biosocial natureculture. This will not be easy, but it is the only meaningful way to go.

Acknowledgments

The study on which this article is based has been funded by the University of Iceland and the Icelandic Center for Research (Rannís). I thank Carole H. Browner (University of California, Los Angeles), Barbara Prainsack (King's College, London), and Halldór Stefánsson (European Molecular Biology Organization, Heidelberg) for their careful reading of a draft. Also, I thank Sigríður Sunna Ebenersersdóttir for locating some of the sources I use on public discussions and responses to personal genomics. Finally, I thank the editors, an external reviewer, and the workshop discussants and participants for excellent comments.

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Biohistorical Narratives of Racial Difference in the American Negro

Notes toward a Nuanced History of American Physical Anthropology

by Rachel J. Watkins

This paper examines the scientific construction of racial differences through the lens of early twentieth-century bioanthropological studies of American Negro skeletal and living population samples. These studies, as well as the scientists who conducted them, are generally distinguished from one another based on their adherence to quantitative and/or qualitative measures of racial difference. However, these binary distinctions tend to obscure the rather complex processes of racial formation in which scientists and research subjects were engaged. Both racialist and nonracialist scholarship positioned American Negroes as products of white, African, and, sometimes, Indian admixture. As the singular label used in these studies connotes, "the American Negro" was also classified as a distinct racial type based on elements of skeletal and physical morphology. Studies reveal that multiple definitions and meanings of race were operating and being generated in the process of situating American Negroes in these seemingly opposed positions. Finally, I consider the implications of this discussion for developing critical histories of American physical anthropology and engaging contemporary public and academic discourse around race, health, and biological diversity.

Introduction

Historical treatments of early twentieth-century studies of African American living and skeletal populations tend to separate physical anthropologists ideologically into racialist and nonracialist intellectual camps. Scholarship conducted by researchers invested in constructing racial hierarchies is commonly juxtaposed with that produced by scholars with a more "progressive" approach to understanding human variation.[1] In addition to providing historical context, these comparative studies have been useful in demonstrating that science is indeed a social practice (Armelagos and Van Gerven 2003; Baker 2010; Blakey 1996; Gould 1996).

In recognizing the social context of this research, it is important not to lose sight of the fact that at the time these early studies were conducted, they were exemplary of bona fide rigorous science. This has implications for understanding how race, as constructed through scientific practice, was and continues to be presented as an "objective" biological reality separate from a social sphere of influence. In the same way that diseases were historically associated with particular racial groups, health disparities continue to be discussed as truths of biological racial difference that can even be identified on cellular levels (Kittles and Royal 2003; Lindee, Goodman, and Heath 2003; Marks 2010; Satel 2001; Templeton 2003). Therefore, our understanding of race as a social construct must consider the sustained role that biology plays in making race appear to be real.

Similarly, we must consider how simply categorizing early twentieth-century bioanthropologists as racialist or nonracialist might obscure complex processes of racial formation in which both scholars and research subjects were engaged (Crenshaw 2003).[2] With that in mind, this paper suspends

Rachel J. Watkins is Associate Professor in the Department of Anthropology at American University (4400 Massachusetts Avenue NW, Washington, DC 20016, U.S.A. [watkins@american.edu]). This paper was submitted 27 X 10, accepted 18 VIII 11, and electronically published 22 II 12.

1. The terms "racialist" and "nonracialist" are used in this paper to distinguish between scientists typically classified in historical treatments as having or not having a research orientation toward categorizing humans into hierarchical racial groups. Some scholars would strictly identify such an orientation as racist (Appiah 1990; Fluehr-Lobban 2000). However, there is an equally substantial body of literature in which the terms "racialist" and "racist" are used interchangeably (Baker 1998, 2000; Strkalj 2009; Taylor 2000). There are also scholars that question this distinction because it implies that one must have an a priori belief in race to be racist (Balibar 1991). Aleš Hrdlička and T. Wingate Todd are commonly juxtaposed in historical treatments as representative of what I refer to here as opposing racialist and nonracialist orientations (Baker 1998, 2000; Jones-Kern 1997; Rankin-Hill and Blakey 1994; Watkins 2007).

2. Critical race theorist Kimberle Williams-Crenshaw uses the term "single axis" to describe the ideological positioning of stratifying practices as if they are discrete entities. As a result, it is difficult to address the multiple levels on which individuals are being marginalized. I use the term to refer to the way in which racial ideologies are positioned as if they are mutually exclusive rather than mutually constructed. As a result, it is not possible to consider the multiple axes on which research subjects

 0011-3204/2012/53S5-0017$10.00. DOI: 10.1086/662416

analytical adherence to a racialist/nonracialist binary to offer a close reading of American Negro skeletal and living-population studies dating between 1924 and 1950. I do so without disregarding outright the points of departure in the work of scholars traditionally placed in different ideological camps. However, I argue that these differences are not necessarily reflected in the use of discrete or uniform definitions of "race" on the part of either group. Rather, scholarship associated with both camps presents multiple and shifting definitions of race in published research papers. I use the American Negro's designation as a product of admixture—on the part of both racialist and nonracialist scholars—as a point of entry for exploring the nuances of scientific methodology and constructions of race and how they exemplified scientific, biological, and social entanglements.[3]

American Negroes were explicitly defined as hybrids of European, African, and in some cases Native American (then known as "Indian") ancestry. As a result, among other things, skeletal and living Negro populations served as a historical record of social and sexual liaisons between blacks and whites in the United States. This particular biocultural interface was an integral part of framing studies that examined differences in skeletal morphology and phenotype between racial groups. At the same time, Negroes were also considered to be a biologically discrete racial group unto themselves. This "fact" justified the population being situated as an anatomical landmark of sorts for mapping and identifying distinct racial characters. This simultaneous construction of the American Negro as both a hybrid and racially distinct suggests that multiple definitions of race and understandings of racial difference were at work in constructing the American Negro as a research subject. This is not surprising when we consider that scholars involved in this work represented a variety of perspectives on human biological diversity. As such, this research can be considered a matter of "boundary work" in the midst of methodologies and subjects that cannot be easily or distinctly categorized (Lipphardt 2010). This also suggests that these studies must be considered within the larger context of bioanthropological interest in studying mixed-race populations to identify the source of biological change in humans. Scientists inside and outside of the United States engaged in research to determine whether or not this change occurred within populations by way of selection or solely by interbreeding with different groups.[4]

The time period discussed here is associated with a methodological focus on description. Measurements were supplemented with mathematical models, and the comparative integrity of biological and genealogical data was explored. Topically, anthropology was noted as following the field of medicine in turning its attention to the Negro (Tapper 1998), including a substantial amount of work produced by women.[5] Anthropological examinations were definitely informed by medical science's focus on hybridity but were not merely extensions of this work.[6] While anthropologists with stronger ties to medicine focused on linking disease and racial biology, those involved in placing more professional distance between anthropology and medicine did not follow suit. Studies conducted by the latter group examined the comparative skeletal morphology of whites and Negroes. A continued interest in using genealogical and phenotypic data to differentiate racial characters represented another point of departure (Spikard 1989; Tapper 1995).

The initial studies highlighted in this discussion were chosen by way of doing a random search for papers focused on Negro-white skeletal comparisons in volumes of the *American Journal of Physical Anthropology* between 1920 and 1950. The majority of papers were produced by scientists at Washington University in St. Louis, Missouri, or Western Reserve University (now Case Western Reserve) in Cleveland, Ohio.[7] As a result, the skeletal collections housed at these institutions were the primary source of research data. In addition to the Washington University and Hamann (Western Reserve) collections, the Von Luschan (New York) and National Museum (Washington, DC) collections are often used in comparative analyses.[8] A secondary set of studies was added to the discussion by way of a random search of databases for studies of Negro-white differences in statistically oriented and naturalist journals that reflect the change of focus in methods of evaluating and analyzing racial differences. Studies of living populations highlighted in this discussion were conducted by Melville Herskovits (1926*b*, 1928, 1930), Caroline Bond Day (1932), and Morris Steggerda (1928, 1940).

These studies of the American Negro point to the nationally specific character of race and admixture studies in the United

are situated in the process of racial formation, including the process of examining racial differences.

3. The terms "Negro" and "white" are used to refer to African and European Americans as they were labeled in most of the literature discussed in this paper.

4. Veronica Lipphardt (2012) presents another dimension of this interest in studying mixed-race populations.

5. Washington University professor Mildred Trotter is a well-known figure associated with American physical anthropology during this time. Several women are also listed as coauthors or research assistants in publications by T. Wingate Todd (Kitson 1931; Todd and Lindala 1928; Todd and Russell 1923; Todd and Tracy 1930). Caroline Bond Day (1932) also completed her thesis under the direction of Earnest Hooton within the time period discussed in this paper.

6. Early twentieth-century medical research was focused on the negative effects on Negro health resulting from the "bleaching" or "whitening" of the Negro through white admixture. This represented a significant departure from nineteenth-century discourse, which revolved around the inherent lack of fitness in Negroes (Holmes 1937; Tapper 1998).

7. The majority of studies highlighted in this discussion were published between 1926 and 1942. Although much of this discussion focuses on skeletal studies, many of the comparative racial studies published within the time period focused on soft tissue (such as Evans 1925; Levin 1937; Miloslavich 1929; Seib 1938; Terry 1942; Williams et al. 1930).

8. The use of these collections for this purpose is all the more interesting because these same skeletal series are now considered representative of pure racial groups in a forensic context.

States. However, these studies also included discussions of scholarship on mixed-race groups that were the focus of research outside of the United States. Most notably, Eugen Fischer's study of the Rehobother Bastards influenced much of the research on mixed-race groups during the time period discussed.[9] In addition, these studies involved comparisons between Negro and racialized population samples of Native American (WIN tribe), African (Batatela), European (Kisars), Asian (Ainu), and named "Oriental" (Jewish) descent (Estabrook and McDougle 1926; Kitson 1931; Todd and Tracy 1930; Wallis 1938).

Critical race theory and racial formation theory are used to situate this research within a broader scope of bioanthropology and to examine how race is both biologically and socially constructed through scientific practice. As an exploration of "foundational knowledge," this discussion can contribute to more critical analyses of contemporary racialized scientific discourses and practices that continue to disadvantage people of color.

The Fact(s) of Admixture

> *The American Negro carries the implication of admixture, and especially white admixture.* (Trotter 1929:97)

As stated, admixture in Negroes was constructed simultaneously through the mapping and identification of racial characters on hard (skeleton) and soft (skin, hair, nose, lips, eyes) tissue. Moreover, studies of living and skeletal Negro populations served to explore the nature of admixture in the recent and evolutionary past. The work of Washington University scholars Mildred Trotter, Robert Terry, and Raymond Lanier provide representative examples of how scientists associated with more racialist leanings discussed admixture in the American Negro. Echoing Trotter's quote above, Lanier stated that "The term American Negro almost necessarily implies a white-black hybrid" (Lanier 1939:343). The American Negro skeletons in the Washington University collection, often used as primary research material during the time period discussed, were characterized as "descendants of original slaves" showing "characters ranging from Negroid to varying extents of Negro-white, and possible Indian admixture" (Lanier 1939:343). Noted progressive T. Wingate Todd and colleagues from Western Reserve University also characterized the American Negro as "clearly intermediate in character" (Todd and Tracy 1930:76), so much so that "a pure Negro population is not attainable in practice" (Todd and Tracy 1930:71).[10] This notion of long-standing hybridity in the American Negro also extends to discussions about recent Negro-white mixtures, often noted by the extent of white blood measured in fractions of eighths (Bond Day 1932; Hodges 1950). Within this context, researchers note that the admixture they observed was the result of crosses between Negroes with white admixture rather than between "pure crosses."

Specific studies discussed in the next section indicate that both racialist and nonracialist scholars accept that a range of variation is expressed in the skeletal morphology and phenotype of even the most "pure" racial groups. However, populations of mixed ancestry were assumed to present an increased amount of variation related to an increase in the number of possible combinations of characters. Statistical analyses of this variation (variation in means and coefficients of variation) were used to measure differences in the expression of traits between groups of "pure" and mixed background. Groups were also distinguished as pure or mixed via the construction of biohistorical narratives associating them with varying degrees of geographical isolation. The seemingly homogenous traits observed in Jewish, Asian, and Polynesian racial groups were attributed to this process (Herskovits 1934; Lipphardt 2010; Wallis 1938). Negroes were not products of the same cultural or geographical isolation and as such were not expected to present the same low means of variation as more "inbred" groups.

However, surprisingly, researchers discovered lower degrees of variability than expected in both isolated racial hybrids and American Negroes. In some cases, these groups presented lower variation than populations regarded as "pure" (Herskovits 1926*a*, 1927, 1934; Todd and Tracy 1930; Wallis 1938). All of these hybrid groups provided further evidence of the dubious correlation between variation and racial purity. This "fact" of variation necessitated a careful and deliberate negotiation of boundaries to construct racially distinct groups.

Race Comes First: Constructing Boundaries

Earnest Hooton, Melville Herskovits, and M. F. Ashley Montagu are commonly placed in different ideological camps, with Hooton being labeled a racialist and the latter two nonra-

9. Fischer's study (1913) played a central role in how variability was interpreted as a reliable indicator of racial purity or hybridity in mixed and "pure" populations: "Fischer found in his investigation of the anthropometry of the Rehobother Bastards, a group of Dutch-Hottentot crosses, that these people show an exceedingly low comparative variability in numerous traits. Sullivan, working through Boas's Siouan material, showed that half-blood Sioux Indians are less variable Thai full-bloods in certain traits, and, presumably, than the white ancestry party to the cross" (Herskovits 1927:70).

10. Todd was one of many noted progressives who characterized Negroes as hybrids (Cobb 1934, 1943; DuBois 1909; Herskovits 1930). Most bioanthropological studies did not directly reference the social dimensions of concern about admixture related to anxieties over miscegenation and racial degeneration (but see Castle 1926). However, researchers were well aware that Negroes were indeed a biohistorical record of interracial liaisons between black and whites. For instance, Herskovits states that "crossing has been going on for countless ages. Indeed, the matter becomes the more impressive when it is realized that no matter how rigidly restrictions against crossing are set up, they do not seem to be of any avail. . . . Attitudes and beliefs of this kind have not proved strong enough to prevent mating across any line that society may draw. . . . This means, then, that all human groups living to-day are of more or less mixed ancestry, that no 'pure' race may be said to exist" (Herskovits 1934:540).

cialist. All three nonetheless recognized that humans' ability to interbreed made them a part of the same stock. Moreover, there are also similarities in how they employ the concept of race to distinguish, involving the implicit or explicit reference to physical characteristics, between American Negroes, Africans, and Europeans.[11] Hooton's understanding of implicit admixture in Negro populations led him to define contemporary populations as "secondary races" (1926, 1927).[12]

As a firm believer that physical differences are the foundation of racial difference, Hooton is critical of what he sees as the indiscriminate use of the term "race" to categorize groups on the basis of linguistic, religious, and geographical differences:

> The term "race" as applied to man is commonly employed with no accurate and well-defined meaning. . . . All anthropologists agree that the criteria of race are physical characters. The tests of racial distinction are the morphological and metrical variations of such bodily characters as hair, skin, nose, eyes, stature—differences in shape and proportions of the head, the trunk and the limbs. (Hooton 1926: 75)

Specifically, Hooton argues that characters that are not greatly influenced by environment or nutrition are the only reliable measures of racial variation. Traits that "originated in functional modifications, but have become so stabilized as to persist in certain stocks even in contravention of their original function" (Hooton 1926:77) are also cautiously recognized as useful, including eye form and color; hair form, color, and quality; and form of the lips and ear. Skeletal indicators include suture patterns, prognathism, form of incisors, relative head breadth and length, and presence of features such as the postglenoid and pharyngeal tubercles (Hooton 1926:77). As such, "a race is a great division of mankind, the members of which, through individually varying, are characterized as a group by a certain combination of morphological and metrical features, principally nonadaptive, possibly caused by environmental stimuli" (Hooton 1926:78).

Herskovits is known for producing research that belies ideas of Negro racial inferiority and argues that by virtue of long-term admixture, no such thing as a "pure" race exists. Nonetheless, he recognizes the "fact" that "some crosses have occurred between peoples of more dissimilar physical characteristics than has been the case with other mixtures, and that it is in the former instances that the term 'race-crossing' is properly applied" (Herskovits 1934:540). However, the distinctions between these groups are more evident in some characters than others:

> If the cross were between long-headed Britishers and long-headed West Africans, the shape of the head would be of relatively small significance. In this latter case, it would be much more important to study what had happened to the thickness of the lips or to the width of the nostrils or to the different bodily proportions which characterize the two parental types. (Herskovits 1934:541)

In other words, even groups that are extremely different from one another will not present variation reflecting the extent of their difference in all traits. However, only those traits reflecting the racial distance that is understood to exist between groups should be the focus of studies. Justification for giving disproportionate attention to said traits is rooted in a collective a priori understanding of "essential" difference between racial groups. Rather than simply writing this off as "bad science," it is more appropriate to understand this historically as a reflection of the "racial common sense" that makes it possible to uncritically accept this sort of research decision as part of rigorous scientific practice (Omi and Winant 1994).

Ashley Montagu (1942) makes similar points to Hooton and Herskovits in stating that "The common definition of 'race' is based upon an arbitrary and superficial selection of external characters" (374). Nonetheless, Montagu characterized American Negroes as a distinct type unto themselves: "The American Negro must be regarded as one of the newest types of mankind. He represents the effect of a considerable amount of mixture among different African varieties, American Indians, and whites of every kind—principally whites of British origin. Out of this mixture has come the unique type or ethnic group represented by the American Negro" (Montagu 1942:56; also see Montagu 1944).

In sum, researchers determined that the mixed heritage of American Negroes did not produce a racially ambiguous skeleton, but rather the opposite. Admixture presented itself in the skeleton in specific ways that contributed to the "fact" that the American Negro was a distinct racial type (a later discussion will illustrate that this same fact of admixture presents in living populations). The irony of this outcome makes the important point that racial purity and racial specificity are not mutually dependent on one another in the process of racial formation. The fact that American Negroes were not

11. Rest assured, these scholars took different positions on the specific traits that were reliable in identifying racial difference as well as whether or not it was possible to forge connections between physical characteristics and language, material culture, mental capacity, or social organization. Hooton is known for examining the latter issue in his 1939 study of criminals. Herskovits did not associate physical characteristics with culture per se but did argue by way of his research that skin-color distribution in black communities reflected a type of social selection on the part of lighter-skinned women for darker-skinned men. As such, this does reflect an example of cultural/social organization standing in relationship to race as determined by physical characteristics (Herskovits 1926*a*, 1934). Lipphardt (2012) also discusses Jewish people as being characterized as being a biologically isolated group by way of religion.

12. Hooton makes the following distinction between primary and secondary races: "A primary race is one which has been modified only by the operation of evolutionary factors including the selection of its own intrinsic variations and of the modifications, adaptive or nonadaptive, possibly caused by environmental stimuli. A secondary or composite race is one in which a characteristic and stabilized combination of morphological and metrical features has been effected [*sic*] by a long-continued intermixture of two or more primary races within an area of relative isolation" (Hooton 1926:76).

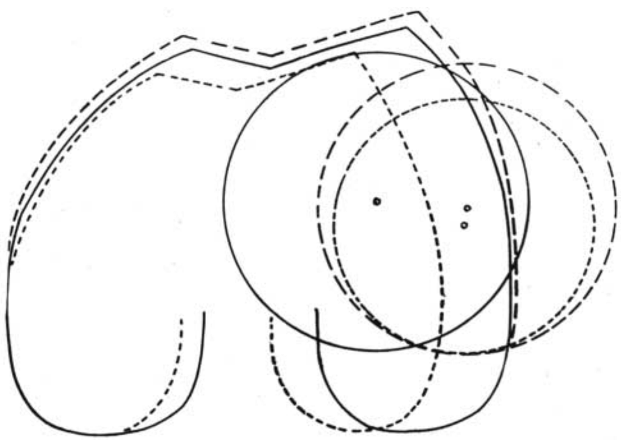

Figure 1. Comparative schematic of relative position of the femoral head and knee joint. The continuous line indicates that white male femoral proportions represent the most "advanced type" of knee (in that the femoral head falls closest to the knee joint; Ingalls 1926:360).

"racially pure" did not remove the possibility of subjecting them to precise classification.

The following section provides further details on strategies used for constructing racial boundaries between Negroes and the individual groups from which they descend. This includes identifying negroid traits in individuals that phenotypically appear to be "white."[13]

The Body in Parts: Racial Differences

Comparative analyses of skeletal and living populations were largely based on examinations of the shape, angle, and contour of crania and other skeletal elements. Differences in stature, weight, and limb length and the presence of phenotypic traits were also key variables in these studies. Both racialist and nonracialist scholars illustrated the "fact" of distinct white and Negro features evident in the hybrid body in their studies, both inside and outside of a hierarchical context. The continued ability to identify "types and tendencies" of Negro and white skeletal morphology was grounded in the scientific "fact" that unlike more isolated populations, the Negro's morphology was both an intermediate between and markedly different from the primarily white and African types that produced it.

Western Reserve scholar N. William Ingalls's published studies of the Negro and white femur provide clear examples of discourse around racial differences in the skeleton. His discussion included careful language that attempted to avoid making qualitative statements in regard to racial differences. Rather, Ingalls made the point that outside of the context of race, his results pointed to an objective difference in the structure of Negro and white femora. Specifically, Ingalls noted greater variation in cartilage thickness, head shape, and the relative position of the femoral head in relation to the knee (see fig. 1).

Ingalls states in the conclusion of the paper that Negro femurs "give one the impression of a joint rather less finished and less stereotyped than in the white. Both of these characters would indicate a joint in which the bony apposition is less accurate, if not also less adequate, or, in addition, a joint in which there are special demands upon cartilage of sufficient degree to call forth distinctive characters" (Ingalls 1926:372). Ingalls takes care to note that he is not making a statement about the comparative value of the femur and knee joint in whites and Negroes. Rather, the data indicate a greater accuracy and precision in the structure of the white knee joint:

> We do not mean to imply that the knee joint, for example, is not as good a joint in the colored as in the white. The evidence from the cartilage as we see it, seems to indicate . . . a greater specialization, if one may be permitted to use the term, for a smaller range of movement on the one hand, but especially for greater security or stability on the other. In this sense the white joint would represent an advance over the colored. (Ingalls 1926:373)

Ingalls makes a similar conclusion in his 1927 study: "The condition found in the white would be more favorable to the general stability and efficacy of the knee" (Ingalls 1927:405). Of note in his racial comparison is the greater variation in Negro femoral and knee joint structure and the implication of its not being as well suited for upright walking.[14] The qualifying ("we do not mean to imply") statement that preceded his conclusions reflects an attempt at what critical race theorists refer to as a formal-race, colored-blind analysis of differences (Gotanda 1991). By assuming this neutral position, Ingalls suggests that his statements are in no way connected to existing discourses around the inferiority of the Negro. However, his references to which knee is better suited for upright walking and which presents a more advanced structure do seem to reflect broader social and scientific attitudes about differences between Negroes and whites. I want to reemphasize here that the focus of my discussion is not on

13. It is important to note that this research is taking place at a time of general social and scientific concern for identifying Negroes among people with a "white" phenotype. For instance, Melbourne Tapper (1998) and Keith Wailoo (2001) write about how diagnostic techniques for identifying people with sickle cell came to be relied on for identifying people with white phenotypes as Negroes.

14. Extensive variation, a lack of uniformity, and greater asymmetry are all commonly attributed to the Negro skeleton and associated joint complexes in many of the studies discussed and/or cited in this paper. In contrast, whites are often presented as a rather uniform racial type as evidenced by similarities in measurements and morphology across populations (such as Trotter 1934*a*, 1934*b*). When extensive variation is not found in Negro samples, it is noted as being "a fact which does not fit in with the current theory of greater variability in the Negro" (Letterman 1941:115; also see Strauss 1927; Todd and Tracy 1930).

confirming or denying whether or not Ingalls's comments were "racist." Rather, I want to draw attention to the rhetorical and ideological strategies employed by scientists to construct racial meanings around their research.

Todd was heavily invested in developing more precise methods of determining cranial capacity (Jones-Kern 1997). In addition to employing mathematical models, his efforts toward greater precision involved adding additional measurements beyond those developed by Morton (see fig. 2).

Todd's papers on the linear dimensions of the cranium (Todd and Russell 1923) and shape of the Negro cranium (Todd and Tracy 1930) reflect his efforts toward greater precision and provide additional examples of formal-race analysis. In "Cranial Capacity and Linear Dimensions in White and Negro" (Todd and Russell 1923), Todd says of the Negro skeletons, "It is apparent that all these groups of crania come essentially from the same people, that our series is fairly representative of the population at large, and that contact with the white man, and even the formation of hybrid material, over three hundred years has not in the slightest obscured the plainly Negro characters" (136). We see a similar implicitly fixed characterization of racial features in the Negro cranium in the paper coauthored with Barbara Tracy (Todd and Tracy 1930). The authors observed the supraorbital ridge, upper-orbital margins, glabella, fronto-nasal junction, and interorbital distance in African, Negro, and white skulls from the Von Luschan and Hammann collections. Following the logic of focusing on the trait presenting the greatest "racial distance" discussed in the previous section, Todd and Tracy base their study on the shape of the supraorbital ridge. The trait appeared to be more undulating (U-type) in Negroes and mesalike (M-type) in whites (fig. 3).

In addition to illustrating differences between the white and Negro skulls, African skulls from the Von Luschan collection were compared and found to be "distinctly more negroid" (U-type) than American Negro skulls (Todd and Tracy 1930:74). That far less M-type skulls were found in the African skeletal collection proved that this population represented a "more pure" strain of Negro type than American blacks.

Todd and Tracy made the explicit point that they were not adhering to a notion of racial fixedness but rather noting the tendencies of a specific skull type to present more readily among Negroes and the other among whites:

> The preponderance of M-skulls in the White series gives point to our contention that the U-type of American Negro skull is the real Negro form and that the M-type is indeterminate in character. There is no intention of insisting that U-type skulls are necessarily Negro or M-type skulls are certainly White. The only justifiable position to take is that . . . a collection of White skulls will be richest in M-type features and a collection of Negro skulls richest in those of U-type. (Todd and Tracy 1930:68)

Although it is the case that one rarely sees Negro forms in pure white stock and white forms in purely Negro stock, these traits are not to be seen as absolutely distinct. Rather, "The situation is better expressed by visualizing so-called typical white and Negro forms at the ends of a long range" (Todd

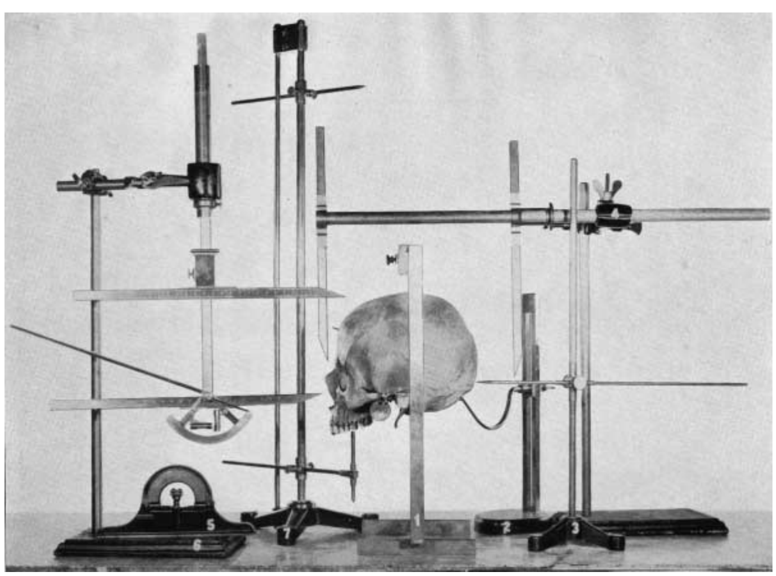

Figure 2. Apparatus for measuring linear dimensions of the cranium (from Todd 1923:145).

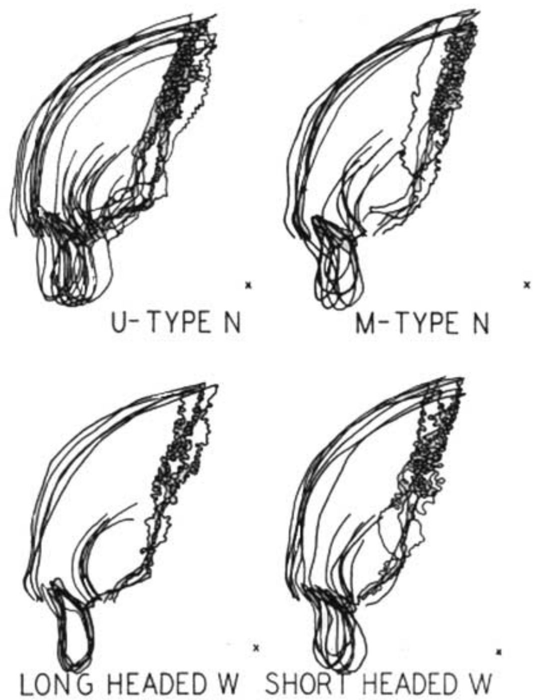

Figure 3. Illustration of M- and U-type crania (frontal bones).

and Tracy 1930:76). With specific regard to the cranial type of the American Negro, the authors concluded that "All evidence appears to bear out the conclusion that in this study, we have found two types of skull, namely, the true Negro or U-type and the white pattern. Between the two extremes, the M-type Negroes and the 'mixed' forms provide a continuity" (Todd and Tracy 1930:108).

Todd and Tracy's approach to formal-race analysis was rooted in their evocation of continuity to frame their discussion. In doing so, they provided themselves with space to distance themselves from the notion of fixed racial types at the same time that they in part relied on this notion to draw their conclusions about true Negro and white cranial patterns. More explicitly than Ingalls, Todd and Tracy placed no comparative value on the differences between the Negro or white skulls. Nonetheless, this research methodology is rooted in the "common sense" that racial differences between whites and blacks will manifest in skeletal morphology (Omi and Winant 1994; Winant 2000). This collective knowledge is what makes it possible for their designation of U- and M-type skulls to be situated as "neutral, apolitical descriptions" merely reflecting ancestral origins (Gotanda 1991:4).

Other comparative studies were developed using a status-race analysis, whereby the inferiority of the Negro was part of the premise for the study (Gotanda 1991). This inferiority was often expressed by way of establishing evolutionary relationships that overtly expressed a more advanced morphology on the part of whites. Being careful not to over-associate a formal-race analysis with scholars historically categorized as nonracialist, it is important to note that the process of status-race formation is evident in Todd's work as well. In his discussion about the most reliable way to accurately test cranial measurement techniques, he states that crania of the Ainu and Negroes should be compared because "the most closely related races" should be used:

> The latter is considered to be a primitive race near the evolutionary starting point of the Europeans. . . . The Congo-Gaboon type with which our Negroes are undoubtedly originally associated is a lower branch of the stem which unites Europeans and Negroes together through some trunk type near to which the Aino [*sic*] probably stands. If then we choose a White type for comparison with our Negroes it is plainly the Aino which we should use. (Todd and Russell 1923:177–178)

Todd's primary goal in this study is to verify the accuracy of his methods for objective racial identification. However, we see that Todd situated Negroes on a "lower branch" of a stem that unites them with Europeans. His categorization of the Ainu reflected their general identification at the time as a proto-Caucasian racial type (Chamberlain 1912; Kotani 1993; Lewallen 2007; see Low 2012 for a more detailed discussion). Therefore, his selection criteria for research subjects was in part based on hierarchically organizing Europeans and non-Europeans according to degree to which they have "evolved." Todd's words illustrate that status- and formal-race perspectives were employed simultaneously in the process of racial formation.

In Washington University professor William Ossenfort's study of the atlas in whites and Negroes, morphological differences suggested that the white atlas deviates more from the mammalian type than the Negro (Ossenfort 1926:442). Trotter's study on the presence of septal apertures in whites and Negroes (Trotter 1934*a*) offers another example of the line of thinking and language used to articulate these status-race differences. Based on results from a previous study conducted by Hrdlička (1932), Trotter worked from the premise that this was a naturally occurring phenomenon unrelated to activity stress. Her data show that the aperture is more common in women and Negroes. Echoing other comparisons of negroid features in Africans and American Negroes, the aperture is found more frequently in the former group: "Topinard (1885) in a larger series found a higher incidence . . . than in other studies or among our Negroes . . . but his material was African as opposed to our American groups which must be recognized as having other racial strains than Negro" (Trotter 1934*a*:222). Regarding the evolutionary significance of the presentation of this trait in whites and Negroes, Trotter notes, "The discussion of racial incidence brings us to a consideration of the question of the variation being an atavism. . . . The aperture occurs in other mammals occasionally and on the whole the tendency seems to be toward obliteration in higher development" (Trotter 1934*a*:222). Trotter concludes that the greater presentation in Negroes and women corresponds with it being hindered from manifesting itself in the stronger bone of the stronger limb of the stronger individual (Trotter 1934*a*: 223). Therefore, her research also provides examples of how the construction of analogous relationships between racial and sexual differences played an important role in racial formation. Although the focus of the septal aperture study is on Negro-white differences, the significant presence of the feature in white women does not affect the established status of American Negroes. At the same time, it does suggest a fundamental difference between male and female skeletal morphology. In quoting Hrdlička, Trotter racially marks women by associating them with Negroes as being among the weaker individuals with weaker limbs and bones. In her study of fusion in the sternum and manubrium (Trotter 1934*b*), she also identifies the higher frequency of this trait in white women as a racial difference (440). Trotter's study of variation in the white and Negro vertebral column (Trotter 1929) offers similar conclusions about male-female and Negro-white differences in skeletal morphology. In making allusions to racial differences between white men and women, she observes that they represent opposite extremes of variation—with males having the longest column lengths and females the shortest. Moreover, she observes that the degree of variation in Negro males and females is situated between the two. These findings reflect the common analogies, constructed through systematic measurement, that allowed scientists to use sexual differences to explain racial differences and vice versa (Leys Stepan 1993). That women were considered to be the "lower race of sex" made the observation that white women—not Negro women—had vertebral-column lengths that represented the extreme opposite of white men seem reasonable (Leys Stepan 1993). This illustrates the intersectional positioning of white and Negro research subjects in the process of racial formation (Crenshaw 1996, 2003). The "common sense" of racial difference is built on scientific constructions of analogous relationships between race and sex. The following discussion illustrates the important role of living-population studies in the work of constructing racial boundaries between American Negroes, whites, and Indians.

Mapping Admixture from the Inside Out

Studies of both skeletal and living-population samples played an equally important role in the construction of racial boundaries and meanings. Together, both data sets were used to establish important correlations between physiognomy and skeletal morphology that helped to affirm the "fact" of racial boundaries. The methodology used in studies of living populations reflected a priori assumptions about the physical appearance of racial purity and racial specificity. In the same way that samples with distinct racial characters were identified for comparative skeletal studies, researchers that studied living populations utilized similar criteria to select research subjects. Skin color and hair texture were often used as primary indicators of racial purity and admixture for this purpose. For instance, Steggerda searched for Negro, white, and Navajo girls of "purer stock" for his 1928 study of comparative stature. With this in mind, Negro women with light skin or "soft" hair were excluded from the study (see also Seib 1938; Steggerda 1940). This suggests that research on living populations was predicated on a more dependent relationship between racial purity and specificity. Unlike the skeleton, the physical appearance of admixture could be racially misleading. As such, mapping physical changes associated with degrees of admixture was a critical part of attributing racial specificity to individuals and families with even the most racially ambiguous characters. Careful documentation of changes in factors such as skin color, hair texture, and nose shape helped to reinscribe racial boundaries onto bodies that appeared to belie them.

Measuring degrees of admixture in some cases involved recording family genealogy. However, because it relied on individual self-reporting, some researchers questioned the scientific validity of this information (Letterman 1941). Medical scientists abandoned the methodology for identifying the complete racial history of individuals for this very reason (Tapper 1995, 1998).[15] These sentiments were largely directed toward the widely known studies on racial crossing in Negroes conducted by Melville Herskovits (1926*a*, 1927, 1930, 1931). In addition to his reliance on genealogy, Herskovits's initial research was critiqued for including anthropometric data on Negroes that arguably did not reflect an adequate degree of racial specificity (and therefore purity). In addition to being lighter-skinned, these individuals were students at Howard University, which also called into question how representative they were of Negro cultural specificity. The cultural and racial specificity of his sample was further questioned on the grounds that most subjects were northerners who were assumed to have less Negro blood and therefore a greater drive for migration (Herskovits 1931). In response to these criticisms, Herskovits engaged in comparative studies including rural, urban, northern, and southern American Negroes. The results of his studies indicated no significant differences in stature or skeletal morphology between Negroes in any of these categories as well as in lighter-skinned, college-educated Negroes. These findings confirmed his assertion that

> The American Negro, as indicated by the genealogies collected in the course of the study, represented much more racial crossing than had been generally recognized, and secondly, that in spite of this crossing, a physical type which combined the characteristics of the African and European ancestral populations and which was relatively homogenous in character had been formed. (Herskovits 1931:193)

15. Genealogies were a key part of constructing rhetorical explanations for the presence of sickling in apparently white individuals—as a result of racial admixture in the remote past (Ogden 1943).

Figure 4. Stewart family genealogy beginning with the "original crosses": four-fourths white H. Stewart and four-fourths Negro Anna Stewart.

Although his methodology was questioned, Herskovits's conclusions were consistent with those of physical anthropologists emphasizing the specificity of the American Negro physical and racial type.

In fact, Todd and Tracy (1930) relied on Herskovits's research to make an explicit correlation between skull morphology and skin color in their study of racial features in the Negro cranium. Noting the near-identical pigmentation Herskovits found in the males and females in his studies, Todd and Tracy noted the striking and "relative darkness" of many of their females. They concluded that the high number of dark-skinned females in their sample was a result of the social selection, as explained by Herskovits (1926*b*), that favored light-skinned females for mates:

> It seems, therefore, quite likely that a laboratory population should include a relatively large number of darkly pigmented females which are balanced in the average figure by a small number of lightly pigmented subjects. . . . *We cannot help but noting in this connection that 70 per cent of our 85 females must be classed as U skulls, which we feel to represent a relatively purer Negro ancestry*. (Todd and Tracy 1930:60; emphasis added)

Therefore, the darker skin of the Negro females in their sample reflected social selection patterns in the living population. Skin color also reflected the lesser amount of white admixture in this group, which was substantiated by the high percentage of corresponding U-type (Negro) skull morphology among them. Therefore, we see that hard- and soft-tissue data were used in tandem to identify distinct racial characters as well as racial admixture. That Todd and Herskovits referenced each other in their studies underscores how studies of living and skeletal populations worked together to situate Negroes as a distinct racial type.

Caroline Bond Day's study on Negro-white families in the United States (Bond Day 1932) is another example of anthropological studies drawing on genealogical and anthropometric data. In spite of difficulties gathering data (such as fear of exposing family members passing for white), Bond Day successfully recorded "indisputable evidence" of blood proportions (Bond Day 1932:5).[16] Bond Day's study concluded that Negro-white crosses produced individuals that primarily fit into three major classifications: dominant, re-

16. Degree of admixture ranged from one-eighth white blood to seven-eighths or more (Bond Day 1932:9). Indian admixture is noted, but not consistently.

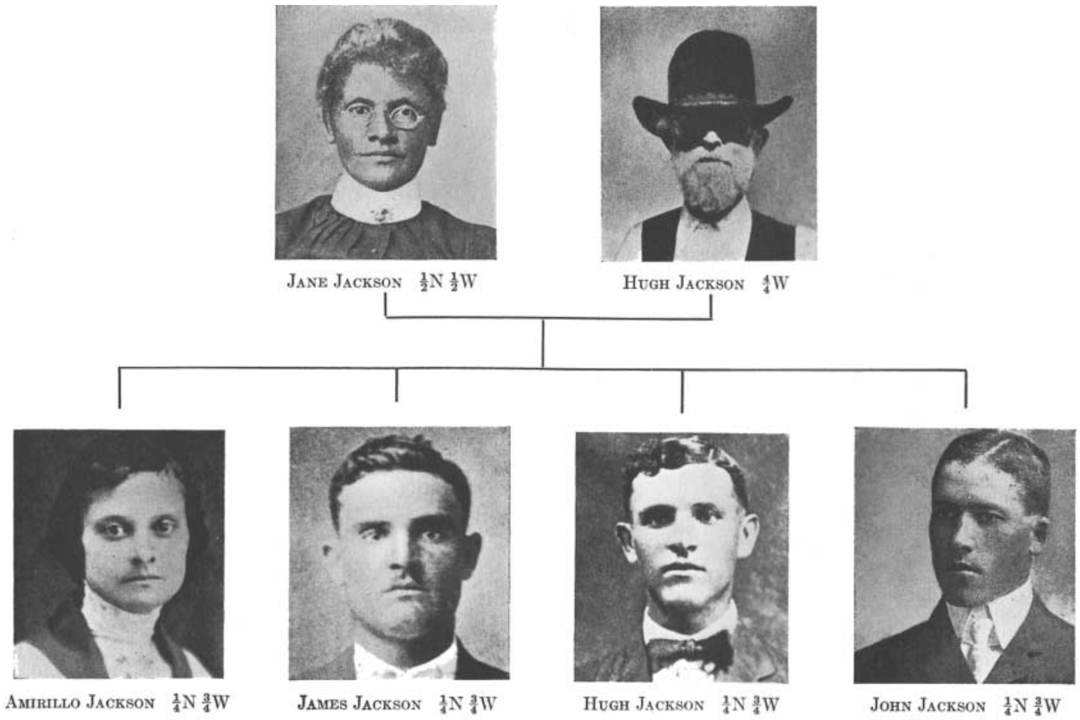

Figure 5. Jackson family genealogy. This genealogy is more characteristic of the genealogies provided in Bond Day's thesis (1932; missing what Bond Day refers to as the "original cross").

cessive, and intermediate.[17] These categories reflected the gradual lightening of skin color, "lightening" of facial features, and softening of hair that resulted from increasing amounts of white admixture. Rather than merely presenting as white or Negro according to degree of admixture, Bond Day observed that individuals with more than four-eighths white blood could present phenotypes identical to whites or people of different nationalities.[18] One of the stated purposes of Bond Day's study was to dispel myths about the achievement of Negroes being directly proportional to the degree of white admixture. She counters this notion of innate biological difference by arguing that Negro individuals and families more closely associated with white relatives were in a better position to benefit from the social and economic privileges they had:

> I should like to state at the outset that however the achievements of this group may seem to argue for the advantages of race crossing, it is my firm belief that Negroes who are of unmixed blood are just as capable of achievement along all lines as those who are mixed. Although it may seem that the bulk of accomplishment lies among the latter group, that fact is, in my opinion, entirely due to some early economic or cultural advantages accruing to the progeny of white fathers or mothers because of this very circumstance. (Bond Day 1932:6)

The "fact" of racial boundaries was also countered by Bond Day's assertion that Negroes with extensive white admixture did not appear to be Negro at all. The narrative portion of Bond Day's thesis, however, is accompanied by detailed visual documentation of families representing varying degrees of white-Negro admixture. In other words, this project reflects how researchers participated in simultaneously dispelling and constructing racial truths.

Bond Day's genealogical charts are accompanied by narrative detailing the physiognomy of parents and offspring. In carefully mapping physical changes associated with varying degrees of admixture, her descriptions clearly attribute certain facial features and hair textures to white or Negro racial groups. Moreover, drawing on the "common sense" of racial boundaries and racial specificity, her descriptions mark all offspring as Negro regardless of their physiognomy. The narrative description of the Stewart genealogy (see fig. 4) reflects a discursive adherence to these boundaries.

> Ada Mills' children, of a marriage with a full Negro husband, Colonel Young, skin color #28, hair grade B3, revert strongly to the Negroid type of features, although neither of them is as light in skin color as the mother or as dark as the father. The girl, who is slightly fairer than the boy, appears

17. Bond Day makes a point of stating that she is not using the terms "dominant" and "recessive" in the Mendelian sense.

18. In addition, her photographs were compared with photographs of mixed-race individuals in other parts of the world to draw conclusions about similarities in "phonotype" (Reading Eagle 1930).

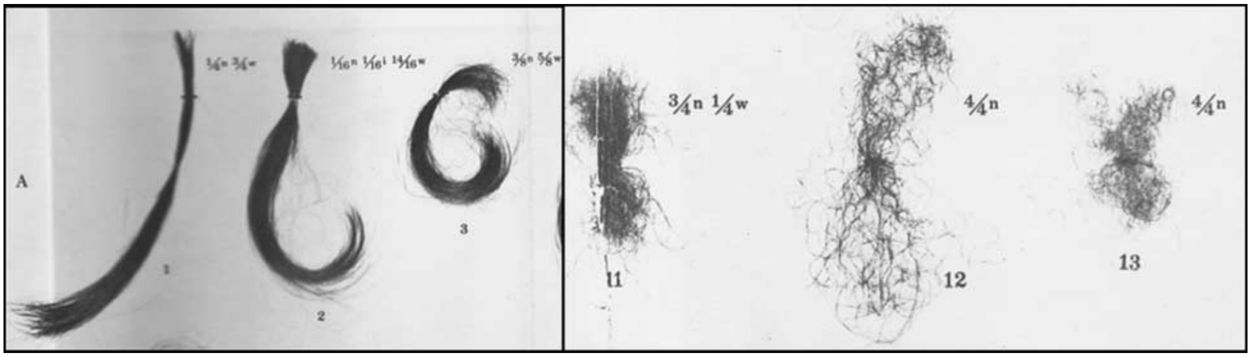

Figure 6. Hair samples Bond Day collected representing the greatest (one-fourth Negro, three-fourths white) and least (four-fourths Negro) amount of white admixture. Notice that the inclusion of Indian descent is arbitrary (based on the self-reporting of participants).

> to have a broader nose. Both have hair which is naturally intermediate in quality between that of the parents. (Bond Day 1932:20)

The description of the Jackson family (fig. 5) provides another example of discursive adherence to the truth of distinct racial characters.

Bond Day describes the family as a good illustration of "the attenuation and practical disappearance of negroid nasal and lip characters in the first back-crossed generation" (Bond Day 1932:21). Bond Day specifically notes the presence of the long and narrow "leptorrhine nose" in the father and the children.

In addition to describing facial features, Bond Day provides a chart illustrating variations in the hair texture of individuals with admixture ranging from four-fourths Negro to one-fourth Negro–three-fourths white (see fig. 6). Again, like the genealogical descriptions, this chart suggests that racial mixture can be carefully mapped such that racial specificity can be identified in the midst of physical ambiguity.

Visuals and narrative are followed by supporting anthropometric data outlined by her advisor, Earnest Hooton, in the last section of the thesis. These data were compared with measurements taken of whites and Negroes from the Harvard Criminal Survey and studies by Herskovits (1930), Hrdlička (1923, 1932), and Davenport and Steggerda (1929). Hooton's analyses of body proportion and size as well as features such as nasal breadth and trunk and arm length indicated how admixture both obscured and retained distinct racial characters. Generally speaking, he found that admixture had little effect on body proportion or size. However, changes in the nose, trunk, and arm length followed the changes in physical form seen in hair with increasing amounts of white admixture. Hooton notes that while this outcome could be the result of small data sets, he goes on to state that "On the other hand, it may be an anthropological fact that, with the exceptions noted, Negroids tend to preserve proportions even when white blood becomes predominant. I am inclined to attribute the results to the latter cause, because of the clear regression exhibited in the nose breadth and nasal index" (Hooton 1932: 81).

Therefore, Bond Day and Hooton's study of admixed living populations produced results similar to those resulting from skeletal studies. As there were skeletal characters that scientists identified as racially indeterminate, Hooton and Bond Day identified bodily proportion and size in the same way. In the same way that the shape of frontal (cranial) bones and vertebral-column length presented racially specific differences, Hooton and Bond Day found the same to be the case in the shape of the nose, in hair texture, and in limb length. Therefore, in the absence of racial purity, researchers developed methods to pinpoint the racial specificity of individuals and families. Interestingly, Bond Day and Hooton concluded that this same segregation of racially specific and nonspecific features is what made it possible that the "occasional mulatto may easily be *mistaken* for a pure white" (Hooton in Bond Day 1932:107; emphasis added). The wording used here makes an important point about the multiple levels on which researchers engaged in racial formation: where racial boundaries appeared not to exist, they were evoked by way of discursive maneuvers implying that Negroes could be identified as white by mistake.[19]

Conclusion

The purpose of this discussion was to draw attention to the complex processes of racial formation that American physical

19. It goes without saying that the different perspectives on racial variation held by Hooton and Bond Day are yet another aspect of the mosaic of racial meanings involved in shaping the study. Regarding what he refers to as miscegenation, Hooton states at the end of Bond Day's thesis that "I see no reason to 'view with alarm' the biological results of such mixtures. On the other hand one is hardly impelled to 'point with pride.' The superiority, inferiority, or mediocrity of the offspring is probably dependent upon the individual contributions of the various parental strains" (Hooton 1932:107).

anthropologists were involved in during a time that is commonly framed by oversimplified divisions between racialist and nonracialist scholarship. In doing so, we can examine more critically the process by which scientific practice as a social, political, and intellectual force determines the importance of racial categories. The studies reviewed in this paper indicate that the dynamics associated with Negroes being a representative product of admixture and racial specificity required the definition and redefinition of racial boundaries. Among other things, this involved establishing morphological and evolutionary distance between whites and Negroes: skeletal analyses included narratives placing Negroes in hierarchical relationships with European and named proto-European groups in distinguishing cranial features. Negroes were further distanced from European and European-descendant groups by research that presented a higher degree of "advancement" in European skeletal morphology. Research on living populations in particular lent to the construction of American Negroes as a group marked by both racial ambiguity and distinction. That Negroes with extensive admixture could be mistaken for white emphasized the unstable nature of racial boundaries that necessitated their definition and redefinition. The identification of characters that remain unchanged with admixture, along with the discursive construction of racial boundaries, assisted in countering this instability. Rather than pointing to fixed notions of race, these constructions point to the diverse ideological content around which different racial meanings were constructed through research practices. The simultaneous consideration of racially indeterminate characters, characters that changed with admixture, and characters identified as racially specific were foundational elements of this content. This process of defining racial boundaries through scientific practice indicates how race operates within and outside of the confines of scientific authority.

This discussion also lends itself to a broader examination of racial formation that highlights the intersections in bioanthropological developments in the United States and abroad. This is most clear in the international influence of certain researchers, living populations, and skeletal collections in the work of understanding racial variation and constructing racial difference. There are equally important implications for contemporary racial discourse and scientific practice, especially as it relates to the delineation of racial groups in medical science. Regarded as foundational knowledge, this discussion can be used to examine critically contemporary manifestations of racial and scientific ideologies. Unfortunately, there are fewer differences between early twentieth-century and twenty-first-century racial constructions than assumed. In directing a critical eye toward the conceptualizations of race and scientific practice in the past, we can do the same for the present. Bioanthropologists are playing a more central role in national conversations on race as it relates to social, economic, and political effects of racism.[20] Scholars are also involved in important critical examinations of current medical research as it relates to pharmaceuticals, genetic testing, informed consent, and health disparities in African American populations (Goodman and Jones 2005; Keita 2006; Outram and Ellison 2006; Yu, Goering, and Fullerton 2009; see also Satel 2001). This discussion suggests that this activism can and should be extended to more critical engagement of repatriation issues for African-descendant populations (Mack 2007). Current discussions raise new questions about how African-descendant populations continue to be constructed as racialized others and their bodies continue to be located outside of the realm of legal protection.

Acknowledgments

A hearty thanks to Susan Lindee and Ricardo Ventura Santos, organizers, for their invitation to participate in the symposium on world histories of physical anthropology. Thanks also to fellow participants for helpful feedback on the initial draft of this paper.

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An Anthropology of Repatriation

Contemporary Physical Anthropological and Native American Ontologies of Practice

by Ann M. Kakaliouras

The policies and politics around the repatriation of ancestral human remains and biological materials to Native North Americans and other indigenous peoples have largely been rooted in attempts to reconcile divergent worldviews about cultural heritage. Even though repatriation has been a legal and practical reality for over 2 decades, controversies between anthropological scientists and repatriation proponents still often dominate professional and scholarly discourses over the fate of Native American human remains and associated artifacts. The epistemological gap between Western scientific and indigenous or Native American perspectives—however crucial to bridge in the process of consultation and achieving mutual agreements—is likely to remain. Moreover, although it is a productive legal, sociopolitical, and cultural strategy for many indigenous groups, repatriation as practiced still struggles to fundamentally transform anthropology's relationship to indigenous peoples, at least in the United States. In this article I will explore new theoretical foundations for repatriation and "repatriatables" that bring Western and physical anthropological conceptions into greater symmetry with indigenous perspectives regarding the active social power and potential subjectivities of skeletal and material cultural remains.

Ownership gathers things momentarily to a point by locating them in the owner, halting endless dissemination, effecting an identity. (Strathern 1999:177)

Repatriation in Bioanthropological Discourse: A Partial History

In 2010, 20 years had gone by since the passage of the Native American Graves Protection and Repatriation Act (NAGPRA, PL 101–601), the instantiation into U.S. federal law of a movement in Native North America with a deep and complicated history (Nash and Colwell-Chanthaphonh 2010). In the 1970s, Native American and Hawaiian people, long and angrily aware of the collecting practices of anthropologists, began to request—and in some cases demand—the return of artifacts and skeletal remains from museums and universities (Fine-Dare 2002). Almost 40 years have passed since members of the American Indian Movement hijacked an archaeological field school in Iowa to protest the treatment of their ancestors (McGuire 1997; Watkins 2000), thereby inaugurating the repatriation movement as one of moral, spiritual, and political action in Native American communities. (For a more nuanced and comprehensive history of the repatriation movement, see Fine-Dare 2008.) Before the passage of NAGPRA in 1990 and as a response to this burgeoning movement, numerous states had already developed repatriation and reburial programs in consultation with tribal governments, museums, and universities (Ubelaker and Grant 1989). In the mid- to late 1980s, individual museums also began to repatriate long-requested items to specific tribes and nations, such as Harvard's Peabody Museum's return of the sacred pole (Umo^{n}'ho^{n}'ti) to the Omaha people (Ridington 1993) and the Smithsonian's repatriation of "war god" figures (Ahayu:da) to the Zuni (Merrill, Ladd, and Ferguson 1993).

Likewise, over the past few decades, laws, agreements, and many hours of both mandated and freely volunteered consultations in Canada and Australia have crafted new relationships between anthropologists, First Nations, and aboriginal peoples, respectively (Smith and Wobst 2005). In many ways, then, the process of repatriation of ancestral remains, sacred objects, and objects of cultural patrimony has become de rigueur for indigenous peoples, physical anthropologists, museum professionals, and archaeologists throughout North America and Australia (Nash and Colwell-Chanthaphonh 2010). Similarly, repatriation is becoming more global, with numerous nations, ethnicities, and cultural groups attempting to resecure their material heritages, whether they are objects considered treasures of Western art—the Parthenon Marbles,

Ann M. Kakaliouras is Associate Professor in the Department of Anthropology, Whittier College (P.O. Box 634, 13406 East Philadelphia Street, Whittier, California 90608, U.S.A. [akakalio@whittier.edu]). This paper was submitted 27 X 10, accepted 16 VIII 11, and electronically published 9 II 12.

 0011-3204/2012/53S5-0018$10.00. DOI: 10.1086/662331

for example—or individual former "scientific curiosities" such as Sara Baartman (e.g., Lobell 2006; Qureshi 2004).[1]

For about 2 decades before and after NAGPRA's passage, many prominent U.S. archaeologists and physical anthropologists voiced in the academic literature their resistance to the idea and increasing reality of repatriation (e.g., Buikstra 1983 [cf. Buikstra 2006]; Meighan 1992; Turner 1986). Usually appealing to Western universalist notions of the ancient past, anthropologists made the following sort of antirepatriation case, though one stated more forcefully here:

> I explicitly assume that no living culture, religion, interest group, or biological population has any moral or legal right to the exclusive use or regulation of ancient human skeletons since all humans are members of the same species, and ancient skeletons are the remnants of unduplicatable evolutionary events which all living and future peoples have the right to know about and understand. In other words, ancient human skeletons belong to everyone. (Turner 1986: 1)

This position suggests that the information of value embedded in human remains and archaeological artifacts is only accessible to academic specialists; therefore, repatriation would represent not only an irreversible loss to "science" but also create insurmountable obstacles to "everyone's" understanding of the past.[2] More recently, archaeologists and physical anthropologists have also attempted to educate in the repatriation literature, more explicitly articulating why the study of human and artifactual remains is important to understandings of the past lifeways of Native Americans in particular (Baker et al. 2001; Landau and Steele 1996; Larsen and Walker 2005; Walker 2000).

For most of the first decade of the twenty-first century, though, disciplinary discourses about repatriation in anthropology have shifted toward intercultural collaboration, dialogue, and reconciliation (see Kakaliouras 2008*b*:46). This shift could be attributed to an acknowledgment that the cultural context for the practice of archaeology and bioarchaeology in a few nations has been transformed because of repatriation. Perhaps also, antirepatriation voices have, in large part, simply left the professional literature or, as Weiss (2008) has asserted, have left their research sites in North America. Either way, the disciplines that have traditionally studied material remains in the absence of their makers (archaeology) or biological remains in the absence of their descendants (osteology or bioarchaeology) now exist alongside repatriation.[3] Furthermore, since the early 1990s vibrant literatures have developed—particularly in archaeology and academic law—examining the effects of repatriation on numerous stakeholders, from museums to specific tribes and nations to the courts (e.g., Colwell-Chanthaphonh 2009; Harding 1997, 2005; Killion 2007). Moreover, an entire discipline of "indigenous archaeologists" of Native and non-Native cultural descent has recently emerged; indigenous archaeologists have, arguably, relatively new opportunities to train at prestigious graduate schools, run field schools, and publish Native-oriented interpretations of material remains (Lippert 2008; Smith and Wobst 2005; Watkins 2005).

The road for repatriation and the attention to indigenous knowledges in archaeology it has helped to foster has of course not occurred without contention. One needs only to think of the Kennewick Man/The Ancient One skeleton to conjure the still wide differences between how anthropological scientists and indigenous people see their worlds. Kennewick Man, a 9,000-year-old individual uncovered from a riverbed in Washington State, was the center of a bitter legal dispute from 1996 to 2004. A confederation of five northwest tribes/nations claimed the remains under NAGPRA as an ancestor, and eight anthropologists sued the Department of the Interior, who had control of the remains, to prevent the skeleton from being repatriated. The plaintiff scientists prevailed in federal district court, and the Kennewick Man/The Ancient One skeleton remains in curation at the Burke Museum in Seattle, Washington.[4] In this case, oral historical information and Native perceptions of an ancestor kin relationship between the disturbed remains and themselves were dismissed as unfounded. The court found in favor of morphological data that led some anthropologists to conclude that Kennewick Man/The Ancient One was not Native American despite his archaeological context (e.g., Owsley and Jantz 2002).

Another example of continuing tensions between the worldviews of archaeologists/physical anthropologists and indigenous people (which are no longer mutually exclusive identity categories) is a recent discussion about the interpretive power, or lack thereof, of indigenous archaeology. Briefly,

1. That repatriation has become more institutionalized in English-speaking settler colonial nations will be addressed in more detail.

2. The idea that scientists or professional archaeologists are the proper stewards of any people's past has a rich history of its own, traced in no small part in the United States to the passage of the 1906 Antiquities Act, which made Native American archaeological sites and their contents the property of the U.S. government.

3. The terms "bioarchaeology" and "osteology" will be used interchangeably in this article to refer to the subdisciplines of biological or physical anthropology that focus on anatomically modern human skeletal remains as evidence for behavior and conditions of life in the past (e.g., Buikstra and Beck 2006; Larsen 1997).

4. Although beyond the scope of this piece, very ancient remains throughout the Americas still represent key flash points for repatriation controversy. Much of the academic discourse around these remains questions the application to them of a Native or indigenous identity because of morphological features that do not appear "indigenous." Kennewick Man was originally classified as "Caucasoid," a move that refueled critique of racialist interpretations in archaeology. Similarly "Lucia," a skeletal individual from Northeastern Brazil, has been referred to in the mainstream press as "negroid" and as more closely related to ancient Australians ("First Americans were Australian" http://news.bbc.co.uk/2/hi/sci/tech/430944.stm [accessed December 15, 2009]).

the practice of indigenous archaeology aims to both open archaeological investigation to indigenous peoples as well as to serve as a critique of and remedy for Western and colonialist bias in mainstream archaeology, such as the use of archaeological classifications that alienate indigenous perspectives (e.g., Atalay 2006; Dongoske et al. 1997). Indigenous archaeology may also include performing and drawing on the spiritual traditions and oral historical sources of descendant communities (e.g., Anyon 1991). Recently, however, McGhee (2008) contended that indigenous archaeology recreates an older anthropological concept, what he calls "Aboriginalism," or the notion "that indigenous people form a class of humans with unique qualities and abilities that are not shared by non-Aboriginals" (594). He further argues that archaeological capitulation to indigenous viewpoints risks turning the discipline into a collection of mythic subjectivities. Zimmerman (2009), among others, has responded that the science of archaeology "can [and has] hurt people" and that scientists also practice recklessness in their assumptions of Western universality.

Lately, too, there has been a brief reprise of framing repatriation as a struggle between science and religion, not unlike the evolution/creation debate in the United States. Skeletal biologist Elizabeth Weiss, in *Reburying the Past: The Effects of Repatriation and Reburial on Scientific Inquiry* (Weiss 2008), depicts repatriation activists as inauthentic religious fundamentalists who are allowed to breach separation of church and state and impinge on scientific freedom: "The government pays for ceremonies and supports the various rituals and methods Native Americans claim for the treatment of these remains even though most Native Americans converted to Christianity and had previously sold 'sacred objects'" (Weiss 2008:61).

At any rate, what this admittedly brief and partial history should index is that while repatriation has changed the practice of archaeology and physical anthropology (i.e., Killion 2008), it has not, counter to the hopes of indigenous archaeologists and their allies, transformed the basic positivistic and universalist premises with which these sciences operate. Similarly, despite the obvious benefits of increased consultation, cooperation, and mutual respect for both anthropologists and indigenous peoples (e.g., Larsen and Walker 2005), repatriation is still seen as a fundamental loss for science; the struggle to retain culturally unaffiliated skeletal collections attests to this continuing concern among physical anthropologists in particular. Furthermore, physical anthropologists and archaeologists have been and are able partners in repatriation efforts, but the end results in these disciplines are often conceived to benefit only the Native people receiving the ancestral remains and artifacts (i.e., the new but "original" indigenous owners; Lippert 2006:431). This is a rather essentialist concept of cultural property and ownership that obscures the processes of appropriation that so successfully recast specific indigenous human remains as keys for the understanding of all people's histories. That is, for the anthropological sciences, education in the history of collection practices—and the power the West had and has to own and deploy the ancestral and cultural heritage of non-Western peoples—usually takes a back seat to technical and skills training (Kakaliouras 2008*a*:121–122). Narratives of the massive colonialist collection of indigenous and ancient remains to serve a Western scientific story of the past have been told many times over (e.g., Bieder 1986; Gould 1981; Mihesuah 2000; Thomas 2000). What has been missing from this literature of objectification and from discourses about repatriation in general, though, is (1) an analysis of the cultural work that "repatriatable" materials do before and after their return, and (2) a consideration of physical anthropological and indigenous subject-making processes through the lenses that the potential for repatriation provides.

In the rest of this article, I hope to sketch a picture detailing how repatriation—both a vibrant indigenous movement and one of the most radical and massive "public anthropology" projects in the last century—may be brought further into the sphere of anthropological theory making and analysis. There are as many distinct microhistories of repatriation as there are indigenous peoples, descendant communities, museums, universities, and scholars, so I do not claim to capture holistically or typify particular experiences or conflicts (e.g., Clouse 2009; Fine-Dare 2002; Kerber 2006; Larsen and Walker 2005; Ousley, Billeck, and Hollinger 2005). Furthermore, I do not intend to provide a comprehensive treatment of NAGPRA, the U.S. federal law that has come to define and control repatriation processes in the United States. I do mean, though, to stretch the anthropological imaginary about repatriation as a phenomenon in the United States (without reappropriating the process from indigenous peoples). I have chosen the United States as a broad cultural and discursive context principally because of the acrimonious character of the legal, political, and scholarly conflicts over repatriation, especially as compared with Australia or Canada (Buikstra 2006:410–412). Further, in the United States, repatriation discourse occurs within a complex and long-standing pan-Indian politic, one that is often de-emphasized in anthropological accounts of relationships between specific Native tribes, nations, and scientists (cf. Biolsi and Zimmerman 1997; Zimmerman 2008). This pan-Indian ethic, though, is frequently employed by Native scholars and activists to make claims for the return of ancestral remains and artifacts (e.g., LaDuke 2005; Peters 2006). Yet rather than describing repatriation as a resolution or instigation of conflicts between Native and scientific worldviews, as it has often been imagined, I would like to explore, in it is hoped a symmetrical way (Latour 1993), the new objects, subjects, and relationships created by repatriation in the last few decades. It may be theoretically and practically fruitful to extend our responsibility as scholars toward considering repatriatable "objects of study" not just when they are in our control but as they move through diverse and often contradictory cultural contexts, effecting different identities, to roughly paraphrase Strathern's epigraph above.

Toward an Anthropology of Repatriation

Recently some sociocultural anthropologists have begun to steer ethnographic research toward the recognition of different ontological worlds and away from the more conventional anthropological practice of using various social theories to describe, translate, and analyze other peoples' systems of knowing, or epistemologies (i.e., Henare, Holbraad, and Wastell 2007; Viveiros de Castro 2003, 2005). Coming variously out of material culture studies (e.g., Miller 2005), science studies (Latour 1993, 2004, 2005), sociocultural anthropology, and archaeology (e.g., Meskell 2004, 2005; Tilley 1999), this "ontological turn" has opened a set of intriguing questions regarding anthropological perspective and interpretation. To oversimplify for a moment, it is traditional in the West to believe that there is one natural and material world made up of arrangements of the same basic stuffs; different peoples then, we assume, think of this one world in different ways. Anthropology as a discipline in this tradition is "the *episteme* of others' *epistemes*, which we call cultures" (Henare, Holbraad, and Wastell 2007:9; emphasis in the original).

In the West, for example, bones are the biological husks of a once living but now dead being. Those who imbue human skeletal remains with other properties, such as being containers of spirits or embodiments of ancestors, apply a distinctly different view to the substance of what bones are. Yet if the material world is one and cultural perspectives are multiple, can radically different perspectives on the same materials actually be given interpretive equivalency?

> For if cultures render different appearances of reality, it follows that one of them is special and better than all the others, namely the one that best *reflects* reality. And since science—the search for representations that reflect reality as transparently and faithfully as possible—happens to be a modern Western project, that special culture is, well, ours. (Henare, Holbraad, and Wastell 2007:11)

This problem becomes practical in knotty cases such as the Kennewick Man/The Ancient One conflict, where Native claims to cultural affiliation based on oral history and antiquity of residence were deemed to lack evidentiary weight, whereas anthropometric dissimilarity to modern Native people was privileged.[5] In a battle of worldviews, where anthropological science has authoritatively told the story of Native North Americans for more than a century, it is not surprising that when put to a legal test, the unfamiliar of the worldviews cannot stand.

A relativist position might give purchase to both perspectives—whether about bones in general or specific skeletal individuals in particular—working to understand each in reference to the other. But as Henare and colleagues further query, "How . . . can relativists assert without contradiction that our representations are *both* partial with respect to others' *and* rich enough to translate them?" (Henare, Holbraad, and Wastell 2007:11). That is, a sympathetic bioarchaeologist can respect that some Native American people believe that their ancestors are present in or express themselves through skeletal remains. Or a Native person can know that osteologists infer past behavior from skeletal morphological evidence and even perhaps agree that such study may be useful. Yet a skeletal pathology indicating vertebral arthritis and a resident ancestor do not easily inform each other's existence or reconcile views about each other in the people who come into contact with the remains (e.g., Blom, Petersen, and Wiseman 2006:83). Simply speaking, bones that are ancestors inhabit different worlds from bones that become informative about past nutritional conditions or population movements or whatever category of information physical anthropologists are interested in.

So, another facet of this "ontological turn" is the notion that "'different worlds' are to be found in 'things,'" (Henare, Holbraad, and Wastell 2007:15), and further, that the Western practice of attaching dynamic meanings to static things already precludes other peoples' understanding of the "things" in question, returning their conceptions, over and over again, to the status of fetishism—which does not exactly give indigenous people the status of "philosophers blessed by a better appreciation of the agency of things" (Miller 2005:30). It is not that one way of seeing a thing is more true than another but that the things themselves are produced, maintained, conceived of, and operate in different worlds. Repatriation as a phenomenon has brought new and intense attention to the question of to which worlds large categories of "things" belong and who should be the stewards of specific cultural and material pasts as enacted by the control over and interpretation of archaeological artifacts and human skeletal remains. It may, then, be useful to draw out some of the ways that repatriation acts to produce novel knowledges and interactions—in perhaps familiar examples and narratives—to push and prod at the kinds of "things" that repatriation has brought into the world.

5. 367 F.3d 864 (9th Cir. 2004). Neither the biological nor the oral historical line of evidence was suggestive of cultural affiliation or lack thereof in this case. The cranium could not be reasonably morphologically affiliated with any population, modern or ancient, and the oral history could not be tied to this skeleton in particular. That the cranial morphology was dissimilar to modern Native people does not foreclose the possibility that Kennewick Man/The Ancient One was an ancestor of the tribal claimants, as morphology as well as oral history changes over time. The relativistic difficulty here emerges in the text of the law pertaining to the evidentiary standard for cultural affiliation: "Such Native American human remains and funerary objects shall be expeditiously returned where the requesting Indian tribe or Native Hawaiian organization can show cultural affiliation by a preponderance of the evidence based upon geographical, kinship, biological, archaeological, anthropological, linguistic, folkloric, oral traditional, historical, or other relevant information or expert opinion" (PL 101–601; 25 USC 3001–30013). Putting "anthropological" or "biological" with "folkloric" and "oral traditional" simply begs for the privileging of the scientific when the various spheres of evidence conflict.

"Repatriatables": Ontological Twists and Turns

I would like, therefore, to consider how repatriation has changed the world of things (or thing-worlds) for both Native American people and physical anthropologists specifically. Although the following discussion could also apply to the cultural remains of interest to archaeologists, human skeletal remains—the stuff of interpretation for osteologists and bioarchaeologists—are particularly charged in repatriation discourses and were the flash points for the repatriation movement's efforts for decades (Fine-Dare 2002; Mihesuah 2000). That is not to say that the many and varied human actors involved in and touched by repatriation subscribe to views that are invalid or that one set of perspectives will emerge victorious, either in this analysis or in real-world struggles over material and spiritual heritage. Yet I do wish to address the following questions. What can we learn from de-emphasizing different views about what is considered for repatriation or repatriated in favor of understanding what those things (or for Native Americans, ancestors or people) do in the cultural worlds they occupy? What distances may be bridged or even widened between indigenous and anthropological conceptions of repatriation when we examine the experiences of "repatriatables"? In short, what I attempt to delineate here is a set of possibilities for conceiving repatriation ontologically rather than epistemologically.

This should not require a massive leap of faith for physical anthropologists, who typically do not consider skeletons to be without a kind of agency—one, though that is produced through methods of "reconstructing life from the skeleton" (Işcan and Kennedy 1989). Admittedly my use of the word "thing" here to describe human skeletal remains has already tipped my hand toward a presupposition that the Western scientific view of bones is the correct one; however, I use "thing" here and "object" infrequently below, clearly recognizing that many Native American people conceive of human skeletal remains as ancestors or simply as people (e.g., Hemenway 2010:173). My characterization of the physical manifestation of Native American ancestors (human skeletal remains) in the next section ("Repatriatables and Physical Anthropological Subjects"), therefore, in no way indexes a preference for seeing skeletal remains as objects rather than subjects. On the contrary, I attempt only to faithfully represent a common physical anthropological perspective about human bones. Likewise, what I hope to accomplish is an analysis of the ways in which physical anthropologists construct and employ a kind of subjectivity for human skeletal remains in their interpretations of the lifeways of past peoples. Though beyond the scope of this particular analysis, there is much work that could be done in engaging or unpacking an object/subject divide in physical anthropological method and practice (see Boutin 2009 for an engagement of this issue). In the next section ("Repatriatables and Physical Anthropological Subjects"), then, I will only explore the varied subjects that physical anthropologists make and gather in their investigations and how in the context of discourses about repatriation they interact with those conceived of by Native Americans.

First, however, if I can be allowed this presumption, before repatriation the bulk of tangible Native American human skeletal remains could be located in two general spaces: museums and academic institutions, and under the surface of the earth (or depending on their ritual treatments, above ground). Repatriation has opened the possibility for Native ancestral remains to occupy a whole different set of spaces and places: to be in transit across large geographic regions, to be in new tribally run curation facilities (Larsen and Walker 2005), or to be simply set apart from other bones, perhaps waiting for a repatriation claim to be made or settled. Further, repatriated remains not only travel in space but also perform a bit of cultural time travel, forming an uneasy bridge between the "prehistoric" and the contemporary. At one time they were buried or otherwise placed in a mortuary context. Then they are preserved, curated, and used for anthropological interpretations regarding their lived past. Perhaps last, they experience a certain "reuse" in the contemporary version of the communities from which they came as they are either reburied or re/stored in this new-to-them cultural context. Archaeologists commonly perform this sort of thought work about material remains:

> If archaeology is concerned with fossilised remains from the past, they are nonetheless preserved in the present, and it is effectively in our present that they are manifest to us. . . . More deeply still, we are ourselves producers of archaeological materials, and when we practise the discipline, we do little more than add a new archaeological episode to the existence of places and things that have often already known a long series of functions and uses. (Olivier 2001: 180)

Following Olivier, repatriation has produced a new category of archaeological and contemporary material culture (Miller 1998), the "repatriatable"—a kind of remain that has the possibility to be returned to a Native American tribe or nation. This category is distinct temporally and affectively from the burial and institutional contexts where skeletal remains have typically resided. Repatriatables as such have significant power in the present and have stirred a whole set of complex and long-standing cultural and historical sentiments toward them from Native people and anthropologists alike.

For many Native American people, for example, repatriatables can embody ancestors, but they also give material evidence to the destruction, dispossession, and scientific objectification of their cultures and heritages (Dumont 2011; Riding In 1992; Thomas 2000). Additionally, the reception and ritual integration of repatriated human remains is often mournful, therapeutic, and empowering (Ayau and Tengan 2002; Hemenway 2009; Hubert and Fforde 2002; Johnson 2007). Some tribes even developed new ceremonies specific to reburials because that category of ritual never existed before the possibility of receiving remains for their care became a reality. Repatriatables, even before any return, also marshal people to act differently around them (Latour 2005); they receive visits and ministering from Native ritual specialists as well as increased sensitivity from others, including museum or institutional staff and anthropological researchers.

At a larger scale of analysis, repatriatables have been reflections of historical and contemporary policies regarding who can be officially Native American in the United States. That is, until recently only tribes and nations that were federally recognized by the U.S. government were able to receive remains under NAGPRA (and only from public or federal lands). Federal recognition grants sovereign status, the special "government-to-government" relationship possible between the United States and tribes and nations, as well as access to grants and other funding for economic development. The process, however, for most tribes and nations has involved meeting a list of criteria related to "Indian" identity; these include historical and ongoing maintenance of a Native identity in a circumscribed community, "identification as an Indian entity by anthropologists, historians, and/or other scholars," and "cultural patterns shared among a significant portion of the group that are different from the non-Indian populations with whom it interacts," to name only a few.[6]

To add another layer of complexity to already complicated legal terrain, new NAGPRA regulations bearing on the disposition of culturally unidentifiable or unaffiliated remains went into effect last year (Department of the Interior 2010). These regulations officially open repatriation to groups not federally recognized who can prove linkages to human remains through ancestral residence on the land from which the remains came. Museums and other agencies holding culturally unaffiliated Native American remains may also now consult with communities not federally recognized about the final fate of these collections.[7] There are over 200 Native groups not federally recognized who have official recognition in their state of residence.[8] There are also a number of Native communities who have neither state nor federal recognition. Although it was considered logical that NAGPRA, as a federal law, would only apply redress to federally recognized groups, state and unrecognized peoples—some of whom can trace their material heritages to museums and academic institutions—had previously been largely cut out of official policy, in part creating the category of "unaffiliated" or "unidentifiable" remains. Some groups not federally recognized mobilized in response to their original exclusion from repatriation legislation. The Muwekma Ohlone of the San Francisco Bay area, for example, continue to lobby for the repatriation of remains and artifacts from the Phoebe Hearst Museum at the University of California, Berkeley; before passage of NAGPRA, Stanford University in Palo Alto, California, voluntarily gave 700 skeletal individuals back to the Muwekma Ohlone, helping to set a precedent that encouraged tribes not federally recognized to advocate for repatriations (Ramirez 2007; Russell 2007).

There are some state laws, such as California NAGPRA (AB 978), that had provided state-recognized tribes access to repatriation processes. Additionally, and before approval of the new regulations, other unrecognized tribes and nations had been able to participate in repatriation via petitioning the NAGPRA review committee (e.g., Goodby 2006:98–99). Now that the federal law is open to communities not federally recognized, thousands of remains that were once considered "unidentifiable" may gain new cultural affiliations and be returned to tribes and nations that hitherto had few rights under law. NAGPRA and state laws that govern repatriation and reburial, then, have literally produced the categories of repatriatables to which Native Americans may have access and that museums and academic institutions may eventually deaccession to them.

From yet another scale of interaction, though, repatriatables also become a fulcrum for communication and cooperation between many different social and political actors, from individual consultations between tribal and museum representatives to large public meetings of the NAGPRA review committee, a body mandated by law, including both academic and indigenous representatives for the purpose of setting policy and mediating disputes, among other things.[9] In these meetings, histories of anthropological and archaeological research of specific Native American peoples are brought into a public sphere larger than perhaps ever anticipated by these disciplines. The minutes of a single Native American Graves Protection and Repatriation Review Committee meeting (2001), which focused on a dispute over an ancient skeleton, are rich with dialogues over archaeological chronologies, Native perspectives, physical anthropological findings, questions

6. 25 C.F.R. Part 83.7.

7. The new regulations, however, do not apply to associated funerary objects (items buried with the skeletons). While the rules are controversial to many anthropologists because of the threat of losing previously unaffiliated skeletal collections, Native and non-Native repatriation activists are mobilizing to encourage the Department of the Interior to include associated funerary objects as well (Amy Lonetree, personal conversation, 2010). Furthermore, the terms "culturally unaffiliated" and "culturally unidentifiable" are not necessarily synonymous. "Culturally unaffiliated" is or was a term that applied to remains, under the law, that are either affiliated with a group not federally recognized or who currently do not have a clear cultural provenance. "Culturally unidentifiable," as Dumont (2011:25) observes, effectively replaced "culturally unaffiliated" in repatriation discourses soon before the law was passed, and it has a rather different valence—that linking those remains to any living Native peoples is, and perhaps will always be, impossible. Finally, the politics around "culturally unidentifiable" remains are contentious indeed, with Native scholars and activists claiming that anthropological scientists have purposefully used only their own scientific criteria to trace remains to Native groups and that they have further not "identified" remains in order to keep them under their control (Dumont 2011; Lalo Franco, personal conversation, May 2010).

8. http://www.ncsl.org/?tabid = 13278#state (accessed December 15, 2009).

9. http://www.nps.gov/nagpra/REVIEW/INDEX.HTM (accessed December 20, 2009).

of the utility of DNA study, and practices of determining cultural affiliation.[10] Thus, repatriatables also gather people and resources to action: they bring people from across the United States to the same meeting place, they marshal funding to mount federal lawsuits, they motivate tribes and nations to build or renovate museums, they bring anthropologists and Native people to the same consulting tables, and they help shape discourses about the past in the present. In the next section, I will focus on how human remains as repatriatables have transformed physical anthropology in the United States and have challenged the very particular subjects that physical anthropologists work with as they bring meaning to skeletal remains in the present.

Repatriatables and Physical Anthropological Subjects

It should be clear from the preceding sections that repatriatables have effectively, if sometimes contentiously, bridged anthropological and Native American object worlds. Even in moments when the gaps between them are potentially rent wider—such as physical anthropological avoidance of North American research sites or calls for the wholesale repatriation of all Native remains (Riding In 1992)—their proliferation results in novel and compelling cultural work between previously isolated communities (McGuire 1997). In this section, I briefly perform one more ontological shift, turning to the process of subject creation in osteology and bioarchaeology. Before I begin, though, let me provide a brief history of bioarchaeological relationships to repatriatables (for a wider discussion of the history of repatriation and bioarchaeology, see Buikstra 2006).

Over the last 20 years, repatriatable Native American human remains have helped shape the arc of the disciplines of osteology and bioarchaeology. As discussed previously, many physical anthropologists, worried that their access to skeletal remains would be curtailed, responded to repatriation by publishing on the question of why human remains are valuable for learning about the past. Ironically, skeletal remains became both more and less accessible to osteologists and bioarchaeologists in the 1990s. Job positions became available to post-baccalaureate students to assist in NAGPRA-mandated inventories of culturally affiliated human remains (Kakaliouras 2008*b*). A volume of data-collection standards was produced to address methodological inconsistencies and anticipate further research (Buikstra and Ubelaker 1994). Arguably, an entire generation of U.S. osteologists and bioarchaeologists was trained through the NAGPRA inventory process. At the same time, however, more stringent permissions became required to access and study Native human remains for the purposes of osteological research. Weiss (2008:69–71) has documented that the number of professional publications and graduate theses in osteology and bioarchaeology using Native American remains as research subjects has dropped precipitously from 1990 to 2005. There is also related evidence, though anecdotal, that U.S. physical anthropologists have increasingly sought out research opportunities outside North America because of the implementation of NAGPRA. If Weiss's publication study reflects such avoidance, perhaps we can see repatriatables as producers of disciplinary desires—to evade perceived constraints on research, interactions with empowered descendant communities, and the implications that scientific research is not the only way to "see" human remains.

The ways in which indigenous peoples make human remains as well as sacred and other cultural objects into active subjects in the present have enjoyed unprecedented attention in the repatriation and indigenous archaeological literature (e.g., Mihesuah 2000; Smith and Wobst 2005; Swidler et al. 1997). Scholarly work in both bodies of literature justifying the need for and justice inherent in the repatriation of Native remains has both stressed these affective relationships between remains and their descendants and critiqued if not excoriated the objectifying nature of nineteenth- and twentieth-century collection and curation practices. Similarly, the history of scientific racism in anthropology and the depiction of "authentic" Native Americans as "vanishing" and dying peoples is inextricably bound to these narratives (Bieder 2000; Hinsley 2000; McGuire 1997). Yet contemporary practice in physical anthropology is rarely engaged in these discourses except in work that has consciously attempted to explain the importance of what osteologists and bioarchaeologists do with human remains (e.g., Baker et al. 2001; Landau and Steele 1996). How osteologists conceive of bones and make them into subjects in the present, though, is key to understanding repatriation anthropologically. If there is one thing that physical anthropologists who work with human skeletal remains and Native American repatriation activists can agree on, it is that human remains are powerful—powerful manifestations of wrongfully disturbed ancestors in the present, powerful tools for interpreting the past, and/or powerful nodes of political struggle in the history of the repatriation movement. The nature of these very different conceptions of power, though, has been and will be a pressure point in repatriation discourses for years to come.

There are two broad experiences involved in osteological subject making: the tactile experience of the remains themselves and the placement of skeletal individuals and populations into larger (pre)historical social and environmental

10. The skeletal individual in question is called "Spirit Cave Man" and is of similar antiquity to Kennewick Man/The Ancient One. The Paiute-Shoshone claimed the remains as an ancestor, while the Nevada State Museum disputed the claim, asserting that the remains were culturally unidentifiable. The review committee eventually found Spirit Cave Man to be culturally affiliated with the Paiute-Shoshone (Minthorn 2002: 17463), but the Bureau of Land Management (BLM) made what would be the final agency decision and agreed with the Nevada State Museum. The Fallon Paiute-Shoshone later filed suit against the BLM. In 2006 the U.S. District Court of Nevada agreed that the BLM had not fully considered the tribe's evidence for affiliation and remanded the matter back to the BLM for further consideration (*Fallon Paiute-Shoshone Tribe v. United States Bureau of Land Management*, 3:04-cv-00466-LRH-RAM).

contexts where they address varied research problems or assist in the reconstruction of past behavior (e.g., Larsen 1997). First, osteology students are taught to interpret anatomical features, evidence of disease and stress, and particular morphologies in the context of an individual or group's lived experience (Baadsgaard, Boutin, and Buikstra 2011; Kakaliouras 2008*b*). The estimation of sex and age places a skeletal individual in a larger community or even a familial setting when mortuary contexts are available. A periosteal lesion on a bone, denoting some kind of infectious or traumatic process, marks that individual as someone who had experienced some kind of stress (Buikstra and Ubelaker 1994). Each step, for that matter, in the process of osteological data collection builds an osteological person; compiling "osteobiographies" has been a conscious and successful research method in bioarchaeology for over 30 years (e.g., Saul 1972). Some bioarchaeologists have gone even further, constructing fictional but materially contextualized life-history narratives from osteological and mortuary interpretations (Boutin 2008).

Second, when these osteological people are grouped together and generalized in space and time as populations or samples, they act in concert to answer or pose questions about past human lifeways. For example, burial populations with a given prevalence of caries (tooth cavities), evidence of iron-deficiency anemia and parasitism, and craniofacial shortening can index a group of people practicing organized agriculture (Cohen and Armelagos 1984; Larsen 1997). The transition from hunting and gathering to agriculture across the world has been documented by bioarchaeologists and osteologists through the interpretation of changes in bony markers. Numerous other examples, from documenting violent conflict to tracking certain diseases to evaluating kinds and intensities of labor, can make this point: the whole field of bioarchaeology depends on these osteological subjects as individuals or groups created through the interpretive skills of researchers and maintained through the publication of research results.[11]

These osteological "subjects" bear little resemblance to those referred to in the repatriation literature, especially those from the nineteenth and early twentieth centuries that were put into the service of constructing racial taxonomies (i.e., Gould 1981); like all subjectivities, they have changed over time. Still, osteological subjects are generally incommensurable with Native ones especially because their construction often requires excavation and sustained physical contact, a situation of disturbance and disrespect perceived as dangerous to many Native people (e.g., Blom, Petersen, and Wiseman 2006:88–90). For example, when Vermont Abenaki and anthropologists from the University of Vermont worked to repatriate remains from an ancient cemetery disturbed by development, Abenaki representatives made it clear that the exhumation and curation of their ancestors' bones "can cause illnesses involving both mind and body (generally called 'bone disease' by some Abenakis and their advocates)" (Blom, Petersen, and Wiseman 2006:83). Here, the authors explained the Abenaki perspective without judgment and used this experience to more respectfully and quickly carry out future repatriations (Blom, Petersen, and Wiseman 2006:89).

The abject reaction that some indigenous people have to the idea of interacting with the dead as skeletal remains is comparable to that experienced in the West in the presence of recent corpses (Buchli and Lucas 2001:10). Dry bones in a research or archaeological context have been largely desensitized in American and European culture; bones in other contexts, however, such as in mass graves or in places where they are not conceived to belong, may still produce distress and even horror. But supposedly bones do belong in museums, on lab tables, and at the end of an archaeological brush, as over a century of Western fascination with archaeology attests.

Osteological subjects also bear little resemblance to the imagined or remembered lives of indigenous ancestors. The past lives that osteological subject creation illuminates tend to be morphological, populational, and adaptational. They do not typically resonate with contemporary Native American concerns with the past because, in short, they were not meant to (cf. Reinhard et al. 1994). When they have crossed paths with each other—these indigenous and osteological subjects—they have usually done so in contested terrain, such as with Kennewick Man/The Ancient One. Osteological subjects serve a conception of a distant past that is filled with population migrations, cultural transitions, disease and skeletal trauma histories, and other features that tell the past in physical anthropological terms. Further, these subjects have been created and mobilized in similar ways in multiple research contexts (cf. Baadsgaard, Boutin, and Buikstra 2011; Reinhard et al. 1994); osteological analytic skills have typically been seen as transferable and not requiring a career-long single-region focus (Buikstra and Beck 2006). It is only recently that indigenous ancestors as subjects—those embodied in or synonymous with the same remains osteologists wish to study—have gained similar resonance in a larger public and of course legislative sphere. Native American people have been struggling for decades to discursively reclaim their ancestors as people, not just as collections, specimens, or data. The successful emergence of this perspective is most recently evident in the scholarly work of people such as Chip Colwell-Chanthaphonh (2009), Eric Hemenway (2010), and Clayton Dumont (2011). Hemenway, for example, richly describes both the practicalities and deep emotional trials of the repatriation work he performs for his tribe, the Little Traverse Bay Bands of Odawa Indians:

> NAGPRA was created to see Indian peoples' beliefs as equal to others. When *people* are returned, sacred items repatri-

11. It should also be obvious that these are the same things so intriguing to Western publics in general. Cable television networks such as the Discovery and Learning channels regularly showcase what bones can tell "us" about the past and how scientists with special skills interpret the features on skeletal remains to illuminate the tales bones tell, so to speak.

> ated, and Indian burials protected, it recognizes modern day tribes' beliefs about who they are and where they come from—their identity. (Hemenway 2010:173; my emphasis)

So although cooperation between Native Americans and anthropologists continues, as each reaches to claim or reclaim what they perceive as their rightful subjects, more struggles may emerge as well.

Like conceiving all repatriatables as similar things, picturing repatriatables as subjects is a generalization I am willing to make to highlight historical and contemporary differences and possible symmetries between indigenous and scientific anthropological ontologies. Repatriatables have effected cultural change and have created new identities as they have moved from hand to hand. Further, because repatriation has brought Native people and scientists together in many instances, new relationships between osteologists, indigenous people, and remains are emerging. To reprise the example above, Blom, Petersen, and Wiseman (2006) detail how the Abenaki people of Vermont they were working with reacted negatively to the scientific language used to refer to the skeletal remains eventually repatriated to them. In this instance and others (e.g., Appadurai et al. 2008;[12] Larsen and Walker 2005; Panich and Schneider 2006), anthropologists took the chance to reflect on their own disciplinary culture, and Native people allowed claimed remains to exist as osteological subjects—at least for a little bit longer—making the bridge between them that much more tangible.

Conclusion: Future Worlds of Repatriatables

Repatriation and repatriatables have forever changed relationships between physical anthropologists and Native Americans in the United States. Likewise, Canadian First Nations and Australian aboriginal peoples and anthropologists have been on similar trajectories. The systematized nature of repatriation in these countries stands in stark contrast to most of the rest of the world, a situation that seems particularly notable considering the last few decades of attention to and concern for global indigenous heritage and knowledges (e.g., Sillitoe 1998).[13] To conclude, I will offer a few suggestions about the future of repatriation and repatriatables outside of the English-speaking settler nations in the North.

Although I have presented a very U.S.-centric picture of repatriation in this discussion, repatriation is also a concern for indigenous peoples and nations worldwide (e.g., Endere 2002; Layton 1989). For example, the World Archaeological Congress has since its 1989 Vermillion Accord (Zimmerman 2002) been supportive of repatriation efforts in diverse countries. A good deal of this activity—and non–North American and non-Australian repatriation efforts on the whole—is concerned with the return of material cultural heritage from foreign museums (usually European and U.S.) to home countries, particularly in Africa and South America. For instance, in 2005, Peru (in the persons of the former president Alejandro Toledo and his anthropologist spouse, Eliane Karp) requested the return of artifacts and human remains from the monumental Incan site of Machu Picchu that are currently in the possession of Yale University (Lubow 2007). Although the Peabody Museum had initially claimed ownership, documentation was later found clearly establishing Peru's title to the artifacts. Yale later reached an agreement with the Peruvian government to share use of the collection.

Repatriation of material heritage to a nation rather than to specific and politically sovereign indigenous peoples presents a whole sphere of different cultural as well as political concerns and challenges. After all, repatriatable objects/subjects will likely have different meanings and capacities in different cultural worlds. Archaeology, though, has long been a fruitful terrain for the promotion of nationalism, in particular in the history of European states (i.e., Kohl and Fawcett 1995). In cases such as the Benin Bronzes and the Rosetta Stone, though, it was the colonizing powers of Europe that collected the material pasts of their Others, either to demonstrate their power to do so or to assimilate other traditions into their own, respectively.[14] Additionally, in Latin America, where many indigenous people were incorporated into their colonial states—as opposed to the separation and later establishment of sovereign dependencies in Native North America—indigenous identity (though not as *indio*) itself has often become synonymous with the nation (de la Cadena 2000; Stern 1982; Yannakakis 2008). In the United States, repatriation sets up sovereign and eligible tribal "individuals" to receive cultural property.[15] In places where material heritage is conceived as national, it may be that disempowered indigenous or ethnic minorities will become further marginalized as the past is retold with repatriated remains and artifacts.

12. In this conversation with Arjun Appadurai, Chris Witmore, Ian Hodder, and others discuss the productive "hybridization" (Appadurai et al. 2008:213) of archaeological and Native ritual practices accomplished by Otis Parrish of the Kashaya Pomo in Northern California and the University of California, Berkeley, and other California universities conducting these sorts of integrated archaeological field schools. This relationship, further elaborated in Panich and Schneider (2006), however, soured when the Pomo and other Native groups were not invited to take part in negotiations over the continuation of Berkeley's NAGPRA committee (http://nagpra-ucb.blogspot.com/2008/06/statement-by-otis-parrish.html [accessed December 26, 2009]). This example testifies to the continuing contestations between anthropologists and Native people "on the ground."

13. http://www.unesco.org/new/en/natural-sciences/priority-areas/links (accessed February 5, 2012).

14. http://www.britishmuseum.org/the_museum/museum_in_london.aspx (accessed December 22, 2009).

15. Repatriatables also, and interestingly in this context, are markers for the economic systems in which they travel. In the United States, federally recognized Native American tribes and nations are treated like good neoliberal individuals, interacting and competing in the marketplace as they are able. NAGPRA, as legislation, supports a neoliberal perspective on cultural property, where preferably individual owners are identified and, if I may, compensated with the return of their property.

Alternatively, as is the case in Bolivia, new constitutional formations may bring indigenous ontological worlds directly into regulatory and legislative spheres (Van Schaick 2009). This may be welcome news to Condori, who 20 years ago said

> The message of both archaeology and history in Bolivia is clear: the evidence of our past, the age-old historical development of our societies and the Indians are for them only prehistory, a dead and silent past. Prehistory is a Western concept according to which those societies . . . *have no history*. This fits perfectly into the framework of thought typical in Western culture. (Condori 1989:51; emphasis in the original)

Prehistory (Kehoe 1998)—a land of anthropological objects and perhaps subjects, or a term belonging to a certain worldview where the past is best understood under the lens of science—is increasingly under revision. After decades if not centuries of Western institutional ownership, the lives of past peoples have been regaining their power in the present as repatriatables and as indigenous rather than scientific subjects. Perhaps it is a brand new world, at least momentarily.

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Ethical Issues in Human Population Biology

by Trudy R. Turner

Standards of ethical practice in any profession change over time. In this paper I examine the philosophical underpinning of the ethical norms of biological anthropology and how these norms have influenced the practice of the discipline over the past 60 years. Particular attention is paid to bioethics, a special branch of applied ethics concerned with human health and human-subjects research. Codification of bioethics began after World War II with the Nuremberg Code, and there currently are multiple documents that define the relationship between researcher and subject for anyone working with human populations. The evolution of these documents and their application is examined. All of these codes emphasize the centrality of voluntary consent and set forth criteria that must be met before any research can be conducted. Biological anthropologists often work with identified and stressed populations in a complex, active, long-term relationship. This relationship presents multiple ethical challenges, including individual and group consent for research projects. Changing ideas concerning the actualization of consent in particular are presented. Emerging topics—such as compensation, data sharing, biobanks, and large databases and collections—are discussed. These topics will generate continued discussion of ethics in human biology research.

Standards of ethical practice in any profession evolve and change over time. What may seem the norm at a particular time can be regarded as well outside the standard of practice at another point in time. Even though ethical standards may change, the practice of ethical behavior is central to all work in a discipline. An examination of the logic and origins of ethical standards can help researchers balance the competing demands and responsibilities they face as they engage in their work. Biological anthropologists often work with disadvantaged or stressed populations in a complex, active, long-term relationship. This relationship can be ethically demanding. In this paper I will examine the history of ethical practice in biological anthropology and also examine some of the newest ethical issues faced by practitioners of the discipline.

Emphasis on ethics in professional life has grown over the past 30 years. This increasing momentum is visible in many ways—there are ever more professional organizations that have defined codes of ethics; there is an increasing number of journals devoted to ethics in various professions; federal agencies supporting research, such as the National Institutes of Health, have extensive coverage of ethical topics available online;[1] and there are many other new extensively detailed Web sites that "give policy makers and the public access to legislation, policy, guidelines and recommendations of government and nongovernmental organizations worldwide" (Avard and Knoppers 2000:102). The Center for the Study of Ethics in the Professions at the Illinois Institute of Technology currently has a library of over 850 codes of ethics for various professions.[2] Many professional societies have ethics modules online. Courses on ethics or ethics training are recommended parts of graduate curricula. New codes and revisions to older codes are usually a response to public scrutiny of some sort of ethical infraction or are a response to advances in science and technology that require new interpretations of ethical guidelines. While it is well known that medicine, law, engineering, and business have ethical standards and codes, the scientific community also shares a set of guiding principles that have been codified into codes of ethics for research and practice.

Each academic discipline also has its own set of standards and principles because each discipline has its own history and its own ethical dilemmas. All anthropologists face a variety of ethical issues as they engage in their research with both people and with animals. Ethical dilemmas have been prevalent in anthropology since its earliest days. Franz Boas was stripped of his membership in the American Anthropological Association (AAA) because of a letter he wrote to the *Nation* in 1919 accusing some anthropologists of spying for the U.S. government. Decades later, in the 1960s, anthropologists were accused of covert activities in foreign countries (Weaver 1973). Many of the issues faced by anthropologists are also faced by other social scientists. Some of these issues, especially those that deal with human-subjects research, can be subsumed under the general term of "bioethics." What makes them

Trudy R. Turner is Professor of Anthropology in the Department of Anthropology, University of Wisconsin–Milwaukee (P.O. Box 413, Milwaukee, Wisconsin 53201, U.S.A. [trudy@uwm.edu]). This paper was submitted 27 X 10, accepted 9 IX 11, and electronically published 16 II 12.

1. http://bioethics.od.nih.gov/; http://www.humgen.org/int/.
2. http://ethics.iit.edu/.

 0011-3204/2012/53S5-0019$10.00. DOI: 10.1086/662661

relevant for all anthropologists is that they concern the protection of human participants in research. The protection of human participants in research crosscuts all of anthropology because cultural anthropologists, archaeologists, and biological anthropologists all work with modern and ancient human populations. Biological anthropologists in particular concentrate on the biological basis of human behavior, diversity, and evolution.

Codes of ethics exist for each discipline because every individual practitioner faces choices. Codes provide a framework for making informed choices in situations where there are conflicting obligations and responsibilities. The codes provide a common consensus or framework of general principles for discussion and choice. Because no code can anticipate each unique situation, discussion and reflection are vital to anticipate situations that may require quick decisions. Anthropologists (as evidenced in the AAA, the American Association of Physical Anthropology, and the Society for American Archaeology codes of ethics)[3] recognize a series of responsibilities—to the people with whom they work and whose lives they study, to scholarship, to science, to the public, to students and trainees, and to employers and employees. With these multiple levels of responsibility, it can be difficult to determine which takes precedence in a given situation. In this paper, I will briefly review the history of professional ethics and then focus specifically on bioethics (after Turner 2005*a*, 2005*b*, 2010). This will entail a review of the various codes that have been established over the past 60 years, and I will discuss some of the newest ethical dilemmas and practices confronting researchers. A fuller treatment of the many ethical issues faced by biological anthropologists can be found in Turner (2005*a*).

The Philosophical Bases of Professional Ethics

Professional ethics are considered to be a branch of normative or applied ethics based primarily on either the late-eighteenth-century utilitarian/consequentialist theories of Jeremy Bentham and John Stuart Mill or the deontological theories of Immanuel Kant (Beauchamp and Childress 1989; Mappes and DeGrazia 1996). An ethical theory provides an overall perspective and moral principles that can inform an ethical problem (Ridley 1998). Utilitarian theories are consequence based. "Any ethical theory that claims the rightness and wrongness of human action is *exclusively* a function of the goodness and badness of the consequences resulting directly or indirectly from that action" is a utilitarian theory (Mappes and DeGrazia 1996:6). Utilitarian theories are either "act" or "rule" theories. "Act" utilitarian theories suggest that a person should act in such a way as to produce the greatest preponderance of good over evil. The interests of everyone associated with the act should be weighed. An act that results in the greatest good for the greatest number is ethically good. "Rule" utilitarian theories state that a person should act in accordance with the rule that if generally followed would produce the greatest balance of good over evil, everyone considered. Act utilitarian theories are situational, while rule utilitarian theories are not. Rule utilitarian theories envision a mediating step, the moral rule, between an individual action and an ethical principle. "According to the rule utilitarian, an individual action is morally right when it accords with the rules or moral code established on a utilitarian basis" (Mappes and DeGrazia 1996:13). Deontological theories, on the other hand, consider some acts to be obligatory no matter the consequences. The foremost proponent of the deontological theory, Immanuel Kant, argued that the single fundamental ethical principle was the categorical imperative. The first and second formulations of the categorical imperative state "Act only on that maxim through which you can at the same time will that it should become a universal law" and "Act in such a way that you always treat humanity, whether in your own person or in the person of any other, never simply as a means, but always at the same time as an end" (Mappes and DiGraza 1996:17). Particular duties and obligations are derived from these formulations and form the basis of deontological theory. Notable perfect duties, based on a respect for persons, include the duty not to kill an innocent person, the duty not to lie, and the duty to keep promises (Mappes and DiGraza 1996:18).

Both utilitarian and deontological theories have been used as the basis for discourse and discussion of personal and professional life, although recently ethical theories such as pluralism, social justice, virtue, and relativism have been used to define professional ethics. Professions are characterized by the scientific competence of their members as well as a collective ideal of service and duties that the members share (Bayles 1989). At the center of a profession is a collection of skills or competencies. Professional ethics are concerned with the rules and decisions concerning the practices, methods, policies, and research of various professions (Appelbaum and Lawton 1990) and are derived from ethical theories and principles. The first code of professional ethics in the United States was that of the American Medical Association in 1847 (Konold 1962). The first two decades of the twentieth century saw a boom in ethical codes when many professional societies organized and adopted their first statement of ethics. The past 30 years have witnessed a second boom in codes of ethics. In addition to an increase in the number of recognizable professions, many of the original codes have been reevaluated in light of new moral problems.

Bioethics and Belmont

Bioethics, a special branch of applied ethics, can be defined as the study of the ethical and moral implications arising from biological and medical research. Bioethics is concerned with

3. http://www.aaanet.org/profdev/ethics/; http://physanth.org/association/position-statements/code-of-ethics; http://www.saa.org/AbouttheSociety/PrinciplesofArchaeologicalEthics/tabid/203/Default.aspx.

human health and human-subjects research and sets forth standards and principles that have become the model for work in medicine and research. Formal bioethics began after World War II, in the wake of Nazi experimentation, with the Nuremberg Code.[4] This code explicitly sets forth the principle of voluntary consent and requires, among other things, that the person doing the experiment define the nature, duration, and purpose of the experiment; the method by which it will be conducted; and any hazards that might occur. The code's 10 clauses list criteria that must be met before any experimentation can be done on human subjects, including that the experiment must yield fruitful results for the good of society that cannot be obtained in any other manner and must avoid unnecessary physical or mental injury. In addition, the subject of the experiment may end the experiment at any time that it reaches the limits of his or her physical or mental endurance. In the decades following Nuremberg, several ethical codes were enacted by the U.S. government, the National Institutes of Health (NIH), the World Medical Association, and the Department of Health, Education, and Welfare (DHEW). In 1953, the NIH issued a policy for its clinical centers that was the first code to establish protections for subjects in U.S. government facilities. In the early 1960s the U.S. Congress passed legislation regulating the drug industry in part as a result of the thalidomide births.[5] The law required that researchers inform subjects of a drug's experimental nature and required that consent be obtained for participation in a clinical trial.

In 1964, the World Medical Association formulated the Helsinki Code (updated six times, most recently in 2008) that distinguished between therapeutic and nontherapeutic research. Nontherapeutic research is purely scientific and does not imply diagnostic or therapeutic value to the subject of the research. The Helsinki Code reaffirmed the basic principles of Nuremberg in that biomedical research should be based on clearly formulated experimental protocols and scientific principles and should be conducted by qualified persons. It also reaffirmed that subjects of experiments must be informed of the aims, methods, benefits, and hazards of the experiment.

The Helsinki Code became the model for all subsequent ethical codes on medical experimentation,[6] including the Council for International Organizations of Medical Sciences (CIOMS) International Ethical Guidelines for Biomedical Research Involving Human Subjects, 2002, and the Indian Council of Medical Research Ethical Guidelines for Biomedical Research on Human Participants (Puri et al. 2009). Since its first edition, Helsinki always recognized the role of the physician to promote and safeguard the health of the patient as the prime directive.

In 1966, U.S. institutions receiving federal funding were required to provide peer review of research to consider the rights and welfare of subjects, the appropriateness of methods, and the balance of risks and benefits. However, the review was entrusted to local institutions, and there was little oversight (Turner 2005*b*). Despite these various codes, multiple infractions of bioethical principles occurred. The most egregious examples were the 1963 case at the Jewish Chronic Disease Hospital in Brooklyn, where 22 elderly and debilitated inpatients were experimentally injected with live cancer cells, and the Tuskegee syphilis study,[7] which came to light in 1972 (Bulger, Heitman, and Reiser 2002). Beginning in 1971, Congress responded to these various allegations with Institutional Guidelines of the Department of Health, Education, and Welfare and the 1972 Patients' Bill of Rights (see Beecher 1970; Childress, Meslin, and Shapiro 2005; Coughlin and Beauchamp 1996; Doyle and Tobias 2001; Faden and Beauchamp 1986; and Gray 1975 for a fuller discussion of the history of bioethics).

In 1974, Congress enacted the National Research Act, which mandated an institutional review board (IRB) review for all Public Health Service–funded research and authorized the establishment of the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. The commission produced a document known as the Belmont Report. The Belmont Report articulated three ethical principles based on a compromise of utilitarian and deontological ethical theories: autonomy, or respect for persons; beneficence; and justice. These principles are usually understood as do no harm, apply the rules of justice and fair distribution, do not deprive persons of freedom, and help others.

The Belmont Report has been codified into federal regulations and is used by IRBs in their analysis of research protocols. These IRBs are local and found at institutions conducting or supporting human-subjects research. IRBs are

4. http://ohsr.od.nih.gov/guidelines/nuremberg.html.

5. Thalidomide, a drug developed in Germany in the late 1950s, was prescribed to pregnant women to relieve the symptoms of morning sickness. During the next several years, thousands of infants in over 48 countries were born with severe birth defects, often affecting limb growth. In 1961 the link between thalidomide and the birth defects was made. Before this, it was believed that the fetus was protected from maternal drug exposure. Annas and Elias state that thalidomide "stands for all the deformities and 'monsters' that can be inadvertently or negligently created by modern medicine" (Annas and Elias 1999:98). Thalidomide was removed from the market; however, recently, some clinical uses of the drug have been discovered.

6. 2008 update; http://www.wma.net/en/30publications/10policies/b3/index.html.

7. The Tuskegee syphilis study was conducted between 1932 and 1972. Six hundred African American sharecroppers in Macon County, Alabama participated in the study; 399 had syphilis at the beginning of the study, 201 did not. The participants were offered free medical care, meals, and burial insurance. Although penicillin was shown to be effective against syphilis in the late 1940s, participants were not offered the drug. They were also not told they had the disease or given any form of counseling. The Tuskegee experiment was the longest-running nontherapeutic medical experiment conducted on humans and was only terminated when the methodology was exposed. "It has come to represent not only the exploitation of blacks in medical history, but the potential for exploitation of any population that may be vulnerable because of race, ethnicity, gender, disability age or social class" (Corbie-Smith 1999:5–8).

responsible for the review and approval of research activities involving human subjects. Their primary mandate is to protect the rights and safeguard the welfare of human research subjects. In 1981, final DHEW approval was given in 45 CFR 46, Subparts A, B, and C (Title 45 Public Welfare, Code of Federal Regulations, Part 46 Protection of Human Subjects, 1991). On March 18, 1983, Subpart D was added to the regulations, providing additional protections for children who are subjects in research. Initially, the Department of Health and Human Services (DHHS; the agency that replaced DHEW) regulations applied only to research conducted or supported by DHHS. However, in June 1991, the United States published a common policy for federal agencies conducting or supporting research with human subjects. That policy, which is known as "the Common Rule," extended the provisions of 45 CFR Part 46 to 14 other federal agencies; it now governs most federally supported research. The composition and operation of each university or institution IRB must conform to the terms and conditions of 45 CFR Part 46.[8] Human-subjects research must be overseen by local IRBs. Funding by federal agencies will not be approved without IRB oversight and approval. In multi-institution or multinational projects, more than one IRB may be involved. Because every institution in this country has its own IRB and every country may have its own regulations, approval to do research can be cumbersome. But as Long states, "As a general rule, investigators should simultaneously meet the highest standards of both our own culture and those of the research subjects' culture" (Long 2005:278).

Since the establishment of the IRB system, other federal commissions—including the National Research Council, the National Bioethics Advisory Commission, and the President's Council on Bioethics[9]—have continued to examine issues concerning human subjects and to prepare updated guidelines.

The Rise of Principlism

In 1976, T. L. Beauchamp and J. Childress published the first volume of *Principles of Biomedical Ethics*, a work that would become the cornerstone of the emerging field of bioethics. At the same time, Beauchamp, working for the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research, was asked to give shape and substance to the principles that had emerged at a commission meeting held that year (Beauchamp 2003). That document became the Belmont Report. Although the Belmont Report defines three principles and the book by Beauchamp and Childress defines four, the two schemes are similar in word and meaning. The four principles defined by Beauchamp and Childress, reflecting norms embedded in public morality (Beauchamp 2003:28), are respect for autonomy, nonmaleficence, beneficence, and justice. The Belmont Report blends respect for autonomy and nonmaleficence under autonomy. The three Belmont principles apply to a set of tangible ways to ensure the ethical treatment of research subjects. The principle of autonomy or respect for persons applies to informed consent, beneficence, risk-benefit assessment and justice, and the selection of subjects (Beauchamp 2003:21). The first principle, autonomy, refers to personal self governance: "personal rule of the self by adequate understanding while remaining free from controlling interferences by others and from personal limitation that prevent choice. . . . [It] means freedom from external constraint and the presence of critical mental capacities such as understanding, intending and voluntary decision making capacity" (Beauchamp 2003:24). The principle of autonomy is the basis for informed consent.

Philosophers, ethicists, historians of science, and attorneys have all written excellent reviews about the history of informed consent (see, e.g., Beauchamp and Childress 1989; Childress, Meslin, and Shapiro 2005; Gert, Culver, and Clouser 1997). This principle of autonomy or respect for persons, articulated as voluntary or informed consent, was of primary importance in the shaping of the Nuremberg Code and presented an ideal for dealing with human subjects. However, the particulars of the application of this ideal to real-life situations was not well articulated until the Belmont Report. The importance of informed consent and understanding risks has special meaning when conducting research on children, prisoners, the incapacitated, or even those whose culture is different from that of the researcher. Protections, codified in 45 CFR 46, are in place to guard the autonomy of individuals who are judged to have a diminished capacity to understand any of the risks that might be associated with a research program (Stinson 2005).

The second principle, beneficence, "requires that relevant positive efforts are made to secure the well-being of persons and protect them from harm" (Bulgar, Heitman, and Reiser 2002:119). The probability and magnitude of harms and risks must be balanced against the anticipated benefits, and risks should be reduced while benefits should be maximized. Justice, the third Belmont principle, requires that there not only be fair procedures and outcomes in the selection of research subjects but that the benefits of the research be equitably distributed (Bulger 2002).

Working with Non-Western Populations

Bioethics codes have been established in countries worldwide, and international organizations have developed codes. The United Nations, working primarily through UNESCO and the World Health Organization (WHO), has a history of declarations and covenants concerning human rights and autonomy.

The Universal Declaration of Human Rights was adopted by the General Assembly of the United Nations in 1948. To

8. NIH Human Research Protection Program; http://www1.od.nih.gov/oma/manualchapters/intramural/3014/.

9. http://www.bioethics.gov/.

> give the Declaration legal as well as moral force, the General Assembly adopted in 1966 the International Covenant on Civil and Political Rights. Article 7 of the Covenant states "*No one shall be subjected to torture or to cruel, inhuman or degrading treatment or punishment. In particular, no one shall be subjected without his free consent to medical or scientific experimentation.*" It is through this statement that society expresses the fundamental human value that is held to govern all research involving human subjects—the protection of the rights and welfare of all human subjects of scientific experimentation. (CIOMS 2002; italics in the original)

But before most of these covenants and declarations were in place, the WHO convened a working group that met in 1962 and 1968 to discuss studies of "long-standing, but now rapidly changing, human indigenous populations" (Neel 1964). Two reports were produced, both authored by James Neel (1964, 1968). These reports detail the relationship and ethical obligations of researcher to study population. Neel particularly emphasized six factors of special importance: (1) the privacy and dignity of an individual must be respected, and anonymity of subjects must be maintained; (2) satisfactory, but carefully considered, recompense should be given for participation in a study; (3) the local population should benefit from the study by medical, dental, and related services; (4) attempts should be made to maintain congenial social relationships with participants; (5) learned individuals from the local population should be consulted; and (6) there should be the utmost regard for cultural integrity of the group.

These principles, which are clearly in line with both Nuremberg and Helsinki, were in place during the heyday of studies conducted under the Human Adaptability Section of the International Biological Program (Collins and Weiner 1977). While these practices may have been fully acceptable and even farsighted at the time research was conducted, they do not completely meet standards in place today. Several years ago, a controversy occurred surrounding James Neel and his research among the Yanomami, an indigenous population in Venezuela and Brazil. The controversy erupted a short time before a book by Patrick Tierney, *Darkness in El Dorado* (2000), was published. In proofs of the book, Tierney had accused Neel of starting a measles epidemic by injecting local villagers with a virulent measles vaccine. These charges were withdrawn before the book was published because of a huge outcry by the scientific community about the validity of these claims. But controversy continued, with some researchers claiming Neel and his team did not do all they could to alleviate the measles epidemic among the Yanomami. Several professional organizations, including the AAA, set up task forces to review all materials. Within the anthropological community the controversy quickly came to concern the seeming conflict between obligations to science and humanitarian efforts. Those members of the task force charged with reviewing the Neel material (Turner and Nelson 2005) found that Neel worked very hard to alleviate the measles epidemic he found in Venezuela. However, the question in the Neel study and in many studies conducted before the implementation of the Belmont Report is, how informed was informed consent? How well-articulated were the goals, methods, and consequences of the research? While these questions are important when dealing with relatively informed Western English-speaking individuals, how were they handled with non-Western, non-English-speaking indigenous populations?

Informed consent was sought, but not in the ways it is now sought. In a survey of researchers working with indigenous populations during the 1960s and 1970s, Turner and Nelson (2002, 2005) found that every study had individuals in the populations who did not participate. Voluntary consent was therefore assumed. Researchers either had government or local permission to conduct their studies. In every case, researchers gave some explanation of the motivation for the study. But some of these explanations were not necessarily complete. Researchers felt that perhaps local populations might not understand precisely the questions they were pursuing. Scientists who were part of the Yanomami expedition in the late 1960s have stated that the Yanomami were told that the researchers were going to look for diseases of the blood. This was true, but there were other things that were researched as well. Some Yanomami that have spoken to outsiders after the publication of the Tierney book have stated that there was an expectation of greater medical benefit from the work (Turner 2010; Turner and Nelson 2005).

Since the early work by Neel and his colleagues, UNESCO, CIOMS, and the most recent version of the Declaration of Helsinki have addressed issues related to working with indigenous populations. UNESCO has a long history of international covenants on human rights, indigenous rights, and the human genome and human rights. UNESCO has recently (2005) issued a statement on bioethics and human rights.[10] This statement affirms the basic principles of Belmont and also discusses respect for different cultures and the relationship between states in research and shared resources. UNESCO is the umbrella organization for the International Bioethics Committee (IBC) and the Intergovernmental Bioethics Committee, which were formed in 1998 and have 36 member states. The IBC is charged with promoting reflection on ethical and legal issues raised by life-science research and encourages the exchange of information and action to heighten awareness of bioethics issues. The CIOMS, an international nongovernmental organization established jointly by WHO and UNESCO in 1949, has available on its Web site the 2002 edition of International Ethical Guidelines for Biomedical Research involving human subjects (the first edition was written in 1982). Again, the three principles of Belmont are prominent in the document. Beginning work in the late 1970s, they produced guidelines for research involving human subjects. One of the questions they addressed was whether

10. http://www.unesco.org/new/en/social-and-human-sciences/themes/bioethics/bioethics-and-human-rights.

the principles articulated in Belmont were universal or whether they were culturally relative. The standards that they designed did not violate what they regarded as universal ethical standards (the three Belmont standards) but stated explicitly that cultural values needed to be considered. In addition, they include explicit discussions of research in areas where either individuals or populations are dependent and in need of protection. To this effect, researchers are cautioned to refrain from practices that could worsen unjust conditions, to ensure that they leave areas of low resources better off, and to engage in capacity building for areas of limited resources.

Beyond Individual Autonomy

Scholars using nearly three decades of experience with Belmont are reevaluating some of the language of the principles. Emanuel and Weijer (2005) suggest that Belmont, through the principle of respect for persons or autonomy, embraces an atomistic view of a person and does not adequately address community. While they have suggested that community be added as an additional principle, others (Childress 2003) suggest that community fits into the original principles as part of an expanded notion of respect for persons. Other suggestions for the consideration of community have subsumed it under beneficence or justice. However, Emanuel and Weijer argue that placing community within the other principles "does not adequately account for the possibility of conflict between individual and communal choice" (Emanuel and Weijer 2005:168). The importance of community and the positioning of the individual within a larger context, noted early by feminist ethicists (Lebacqz 2005; Wolf 1996), can have profound implications for autonomy. A recent textbook on bioethics by Singer and Viens (2008) addresses this idea explicitly. In a section on religious and cultural perspectives in bioethics, they discuss the ways in which consent can be obtained from members of aboriginal, Buddhist, Chinese, Hindu, Sikh, Islamic, and other individuals from an array of populations. They note that in some cases, cultural practices require that respect for individual consent is really familial or community consent. They credit the input of anthropologists in bringing about this sensibility.

Researchers are conditioned to think about the impact of research on an individual—on his or her health or psychological well-being. It is also important that the researcher think about the impact of the research on the study population. While Belmont protects individual participants in research projects, many anthropological studies are population based, and the findings of these studies can affect whole populations. Consultation and group consent is now sought from populations. But group consent leads to a new suite of questions (enumerated by Juengst 1999; Turner 2005*a*). Who speaks for the group? If the group is nested within a larger group, who represents the original group? What are the limits of the group? What is the relationship between expatriate groups and the community of origin? Does permission from a national government to conduct research have meaning for the community being studied? How does one obtain informed consent from an individual or a group whose members have little understanding of the project or the risks involved? How can the culture of the population be taken into account in the design or implementation of the project? What are the implications concerning the disclosure of the identity of the group? Can consent be withdrawn sometime in the future? How? Can samples be withdrawn sometime in the future? How? Are there appropriate benefits for the population under study? This series of questions must be asked by every researcher engaged in research with human populations. However, the most current CIOMS guidelines caution that

> In some cultures an investigator may enter a community to conduct research or approach prospective subjects for their individual consent only after obtaining permission from a community leader, a council of elders, or another designated authority. Such customs must be respected. In no case, however, may the permission of a community leader or other authority substitute for individual informed consent. In some populations the use of a number of local languages may complicate the communication of information to potential subjects and the ability of an investigator to ensure that they truly understand it. Many people in all cultures are unfamiliar with, or do not readily understand, scientific concepts such as those of placebo or randomization. Sponsors and investigators should develop culturally appropriate ways to communicate information that is necessary for adherence to the standard required in the informed consent process. Also, they should describe and justify in the research protocol the procedure they plan to use in communicating information to subjects. For collaborative research in developing countries the research project should, if necessary, include the provision of resources to ensure that informed consent can indeed be obtained legitimately within different linguistic and cultural settings. (CIOMS 2002)

In discussing research with Australian indigenous communities, Dunbar and Scrimgeour (2006) discuss the repositioning of indigenous peoples from subjects to active participants in all aspects of the research activity. Community-managed organizations can be approached for local assistance and can identify the appropriate stakeholders. However, some institutional support from universities and funding agencies is necessary to facilitate full participation. One of the main issues remains the flow of benefits from research to indigenous people and indigenous ownership of cultural and intellectual property. Hudson and Russell (2009) discuss the relationship-building process with the Maori, which includes partnership, participation, and protection. The main issue remains respect for indigenous rights and control over research processes and the realization of equitable benefits for the communities. The authors suggest a value-based as opposed to rule-based consultation. The important values are respect for groups as sovereign entities, indigenous control, and reciprocity and mutual benefit. These values have been codified in a modification of the treaty with the Maori.

An example of group consent and consultation can be found in the work of O'Rourke and colleagues, who have been engaged in ancient DNA research with several populations (O'Rourke, Hayes, and Carlyle 2005). Each of the populations O'Rourke has worked with necessitated an individualized approach for access to samples. Some communities requested in-person meetings; others did not. Different communities had different restrictions on the size or weight of the samples. Working with Paleo-Indian remains for ancient DNA or skeletal biology studies in this country requires adherence to the Native American Graves and Repatriation Act regulations. This may mean that some studies cannot take place. On the other hand, some scientists (Larsen and Walker 2005) have been able to open discussions with native peoples on the study and disposition of remains (Turner 2010).

Physical anthropologists and human biologists frequently study aspects of human variation that allow them to return again and again to local identified communities. Some researchers have had multidecade relationships with their study populations. Over the decades, standards of what is included in informed consent have changed. Friedlaender (2005) gives a detailed account of his 35-year relationship with groups in the Solomon Islands and the changing standards of informed consent that he has implemented in his work. In the 1960s and 1970s studies were conducted without written individual consent or formal government approval. The current standard is full disclosure of the research project and the risks and benefits. This includes returning to the population for additional consent if samples might be used for a related but not identical project. The best way to describe the current paradigm is in terms of an ongoing relationship between subjects and researchers, with subjects as active participants in research design and implementation.

Other Issues

Compensation

The most recent version of CIOMS allows for the reimbursement of lost earnings, travel costs, and other expenses incurred while an individual is a participant in a study. Free medical services are also allowed. Also, individuals, especially those not obtaining any direct benefit from the research, may be compensated; however, none of these payments or services should be so great as to interfere with their ability to decline to participate. There is often a huge differential between the researcher and the participant in studies in education, socioeconomic status, and access to resources. The differential may influence rapport and trust between the investigator and the participant. In medical studies in this country, compensation may take the form of some level of medical care. However, what are appropriate compensations for research studies conducted with non-Western identified populations? If a study includes medical personnel, some level of medical care may be given to the participants. But this is not necessarily the type of care individuals need. Certainly there are some conditions where antibiotics or analgesics can be useful and even life saving. What if a person is identified as diabetic? A single visit from a medical professional will not be sufficient to help this person. Referrals to more long-term care facilities may be in order. In the past, researchers have given many items as compensation (Turner 2010). Researchers usually select these items in consultation with those familiar with the culture. Food items, photos, tools, machetes, and cash have all been given as compensation. Other items have been given to the group or community. Some compensation has involved technology transfer and training of individuals to use this technology (Bamshad 1999; Jorde 1999). Guideline 20 of the CIOMS mandates a much more direct engagement by stating that "external sponsors and investigators have an ethical obligation to contribute to a host country's sustainable capacity for independent scientific and ethical review and biomedical research." This may take the form of strengthening research capacity, training medical personnel, and educating the community from which research subjects are drawn (CIOMS 2002).

Risks and Benefits

Distinctions are made in the current standards between biological and behavioral research. In biological or medical research, risks can often be more clearly identified than in behavioral research. However, behavioral research can cause emotional, psychological, or social harm (Stinson 2005). Embarrassment or social stigma can be real consequences of participation in a research project. An individual might find questions embarrassing or face social consequences if his or her answers to questions become known. One of the most important risks to an individual is disclosure of identity. IRBs are very aware of the risks of this disclosure and will look closely at the ways in which identity can be safeguarded.

Winston and Kittles (2005) describe the challenges to perceived identity that were sometimes generated by the African Ancestry Project. The project was designed to use genetic, historical, and cultural data to provide a bridge to the past and to answer the question of "who am I" for Americans of African ancestry. DNA-based testing was used to determine which of several indigenous African maternal and paternal lineages are present among African Americans. The project had a database of over 9,000 individuals available for comparison. There were both positive and negative psychological effects of this project; these effects were the result of either confirming or not confirming a particular cultural identity. They conclude that researchers must ensure confidentiality, prevent discrimination, and fully disclose all risks, including psychological risks. Williams (2005) also discusses some disclosure issues faced by descendants of Thomas Jefferson after a study of DNA from descendants of Sally Hemings and the Jefferson family. Again, individual identity and group membership were sometimes challenged by genetic results. Stigmatization is a risk that can also occur at the group level. This may be especially true for marginalized or identified populations. Members of a group might be stigmatized by having their circumstances discussed. How can one avoid this situation? Researchers feel that a frank and full discussion of this risk can lead to a negotiation between subject and researcher on the presentation of the results and the naming of the group as participant. Williams (2005) also suggests that constant vigilance during the planning and execution of the project be paramount.

Data Sharing and Ownership

Circular A-110 of the U.S. Office of Management and Budget stipulates that data collected through grants awarded by federal agencies such as NIH and the National Science Foundation (NSF) are public. Federal agencies, reflecting the scientific ideal of an open community of scholars pursuing novel ideas and avenues of research, encourage the broad and rapid dissemination of information throughout the scientific community. But how is "data" defined? The current NSF Web site for physical anthropology states that NSF "expects investigators to share with other researchers . . . the data, samples, physical collections, and other supporting materials created or gathered in the course of the work."[11] Samples collected by physical anthropologists, however, are often unique and difficult to obtain. It may not be possible to obtain second sets of blood or saliva samples or measurements or interviews from members of identified communities. Individual and group consent and confidentiality become major issues if samples are shared. Specific questions about data sharing range from the definition of "data" to fair use for the individual collecting the data (Turner 2005). Even though the physical anthropology program of NSF has a data-sharing requirement, the design implementation of this requirement is up to the individual researcher. It must, however, go beyond publication of results in a scientific journal. Questions related to the ethics and the requirements of data sharing are just beginning to be answered in the biological anthropology community (Turner 2010).

While data sharing may be relatively new to physical anthropology, it is not new to research groups involved in genetics and population genomics (Caulfield et al. 2008; Knoppers and Fecteau 2003; Knoppers and Joly 2007; Wallace, Lazor, and Knoppers 2009), biobanks (Cambon-Thomsen, Rial-Sebbag, and Knoppers 2007), and stem cell research (Isasi and Knoppers 2009). Major complex electronic databanks already exist for genetic information. Sequence data are shared via the International Nucleotide Sequence Database, which includes GenBank, the DNA DataBank of Japan, and the European Molecular Biology Laboratory. Population biobanks containing actual biological materials have been defined by the Council of Europe as a collection that (1) has a population basis; (2) is established or has been converted to supply biological materials or data derived therefrom for multiple future research projects; (3) contains biological materials and associated personal data that may include or be linked to genealogical, medical, and lifestyle data and may be regularly updated; and (4) receives and supplies materials in an organized manner (Wallace, Lazor, and Knoppers 2009:15) These banks collect and store materials for future unspecified projects and are considered to be strategic resources with enormous potential for collaborative research projects. There are at least two kinds of biobanks. Smaller biobanks contain thousands of samples. These samples were usually collected by an individual or small group of researchers and are usually related to a single medical condition or a single population. In contrast, the major biobanks may contain hundreds of thousands or even millions of samples and are meant to be multipurpose and longitudinal. These banks arose during a time when the ethical, legal, and social implication of scientific inquiry were being considered in parallel to the scientific merit. The ethics of these collections were discussed before and during the initial stage of acquisition. However, given all that has been previously said about informed consent, confidentiality, and returning to an individual or a population to request consent for new projects, return of results, and data sharing, what are the ethical considerations that allow the existence of biobanks that are designed to share samples? Consent issues are probably the most contentious, although research on biobank participants indicates that confidentiality rather than consent is of paramount importance (Cambon-Thomsen, Rial-Sebbag, and Knoppers 2007). Several new initiatives have been launched to promote collaboration. One of these, the Public Population Project in Genomics (P3G), is an international consortium with members from 25 countries that is "dedicated to building a worldwide collaborative infrastructure, including a repository of tools and information so as to foster interoperability between studies in human population genomics (Knoppers et al. 2008:664). The P3G Observatory is the heart of the project and provides access to biobanking tools. Biobanking projects from around the world with more than 11 million participants are registered with the P3G. These voluntary participants are viewed as altruistically contributing to the future health benefits of the world. The principles of the project include working for the common good, responsibility, respect, transparency, and accountability. Consent for participants in the biobanks is broad because it is assumed that future research projects are planned. To ensure privacy, there is tight data security and a highly developed governance structure.

P3G is built on successful models such as the HapMap project and the single nucleotide polymorphism consortium. These models and their ethical underpinnings seem radically

11. http://nsf.gov/funding/pgm/_/summ.jsp?pims/_/id=5407&org=BCS&from=home.

different from the failed early Human Genome Diversity Project (HGDP). The HGDP grew out of the realization that the sequenced genome did not represent all the people of the world. A group of researchers, led by L. L. Cavalli-Sforza, conceived of the HGDP to study human genomic variation (Cavalli-Sforza 2005). Four planning symposia took place between 1991 and 1994. The HGDP met with broad opposition from indigenous populations (MacIntosh 2005). The legal, ethical, and cultural issues of genetic research among indigenous groups were not adequately addressed by this project. Lists of populations to be sampled were designed by researchers with little or no community input. Issues of access, ownership, and benefit sharing were paramount and were not originally addressed. In the intervening decade and a half, new international codes have been implemented that help define benefits for local communities and ownership of resources. Greater numbers of individuals are contributing to large databases. Ethical considerations are being addressed early in the process. Agencies such as P3G philosophically "adhere scrupulously to its Charter of Principles. Founded on promoting the common good, responsibility, mutual respect, transparency, accountability and proportionality, these principles span critical boundaries across cultures and among legal systems. . . . Large scale public funding of biobanking mandates a move away from overemphasizing the needs of the individual toward promotion of a free exchange of ideas, data sharing openness for the benefit of all" (Knoppers et al. 2008: 664–665). On the other hand, however, Greeley points out that anonymity may not be able to be guaranteed to those who contribute to these biobanks and that there may be a moral obligation to alert individuals contributing to the biobanks of genetic risks (Greeley 2007). A recent study by Capron et al. (2009) indicated no real consensus among individuals with expert knowledge of biobanks on the important questions related to databases and biobanks: who owns the samples, what are the regulations of researchers who use the samples, and what is expected in terms of benefit sharing and remuneration for participants? Ideas of ownership of samples ranged from exclusive control to custodianship to a system where the participants own the samples. No matter what the philosophy of ownership, all researchers agreed that data generated from any studies involving these biobanks are always considered public domain. The most common way to get access to material in biobanks is through a material transfer agreement with a provision that findings will be shared. Second-party use is not allowed. While there is a clear recognition that respect for participant groups is essential, group consultation instead of group consent seems to be the norm. Benefit sharing is primarily group based, and individual participation is not compensated.

The ethical discussion surrounding biobanks and their interpretation as for the greater public good is still relatively new. It will be crucial to see what happens in the next several years as public reaction continues to unfold and the ethical principles underlying these banks are tested.

Collections: An Emerging Issue

All biological anthropologists deal with some sort of collected materials. The collection can be skeletal or material remains, repositories of DNA or cells, or field notes. A recent issue of *Anthropology News* (March 2010), the newspaper of the AAA, devoted the "In Focus" section to repatriation. The articles begin to focus our attention on collections and what anthropologists do with them, how they maintain them, and who has access to them. There are examples of repatriation efforts that have been successfully accomplished (Cast, Gonzalez, and Perttula 2010; Young 2010) and other examples of work yet remaining, such as with culturally unidentified items (Colwell-Chantaphonh 2010). Repatriation of native material has been in process for years; other types of collections are currently being examined. Of particular interest were discussions of collections held by large museums (Fiskesjö 2010) and individuals' field notes (Nicholas et al. 2010). At this point in time, there seem to be two different poles regarding the ethics of collections. On the one hand, collections of some material remains are subject to the legal requirements of repatriation and have been the subject of considerable ethical debate. On the other hand, large databases of genetic information are being deliberately collected in such a way as to attempt anonymity and obviate any possibility of return. An examination of the ethics of collections, writ large, is perhaps the next challenge for biological anthropologists. In framing the ethics of collections, biological anthropologists could engage with other groups who have a history of engagement with collections, including museums, libraries, film and video archives, and other groups that deal with information technology. These discussions are beginning with the formulation of principles that will govern the collection and stewardship of materials and include a clearly defined agreement between the principal investigators and the curators of the collection, among other things (see particularly O'Brien 2009). The work by ethicists on ownership of materials in biobanks may be particularly useful to this discussion.

Conclusion

Bioethics is in a sense a work in progress. Since the initial articulation of the principles of autonomy, beneficence, and justice, there have been multiple revisions and additions to the existing understanding of the principles and their application to real-world situations. This changing understanding has been reflected in the changes in federal and international codes of ethics that define behavior. But the heart of bioethics remains the same—autonomy, beneficence, and justice. The notion of consent has been broadened beyond the individual. Our understanding of an individual's place in a community means that the researcher must engage at a different level with his or her subjects. Engaging the community in the planning of a project and then ensuring that the community is empowered are now parts of the process. And in some senses, with biobanks, community is the world community, and research is regarded as a public good. This continued engagement with the principles and the codes are designed to foster a sense of trust between what is now viewed as a research partnership.

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"Your DNA Is *Our* History"

Genomics, Anthropology, and the Construction of Whiteness as Property

by Jenny Reardon and Kim TallBear

During the nineteenth century, the American School of Anthropology enfolded Native peoples into their histories, claiming knowledge about and artifacts of these cultures as their rightful inheritance and property. Drawing both on the Genographic Project and the recent struggles between Arizona State University and the Havasupai Tribe over the use of Havasupai DNA, in this essay we describe how similar enfoldments continue today—despite most contemporary human scientists' explicit rejection of hierarchical ideas of race. We seek to bring greater clarity and visibility to these constitutive links between whiteness, property, and the human sciences in order that the fields of biological anthropology and population genetics might work to move toward their stated commitments to antiracism (a goal, we argue, that the fields' antiracialism impedes). Specifically, we reflect on how these links can inform extralegal strategies to address tensions between U.S. and other indigenous peoples and genome scientists and their facilitators (ethicists, lawyers, and policy makers). We conclude by suggesting changes to scientific education and professional standards that might improve relations between indigenous peoples and those who study them, and we introduce mechanisms for networking between indigenous peoples, scholars, and policy makers concerned with expanding indigenous governance of science and technology.

> *What I'd like you to think about with the DNA stories we're telling is that they are that. They are DNA stories. It's our version as Europeans of how the world was populated, and where we all trace back to. That's our songline. We use science to tell us about that because we don't have the sense of direct continuity. Our ancestors didn't pass down the stories. We've lost them, and we have to go out and find them. We use science, which is a European way of looking at the world to do that. You guys don't need that.* (Wells 2003)

In this remarkable excerpt from the PBS film *The Journey of Man: The Story of the Human Species* (Wells 2003),[1] Spencer Wells, population geneticist and leader of the National Geographic Society's (NGS) Genographic Project, responds to Australian Aboriginal painter Greg Singh. Singh does not accept Wells's suggestion that his Aboriginal ancestors trace back to Africa, insisting instead on the veracity of Aboriginal origins. Interrupting expectations, Wells does not disagree with Singh. He does not represent the human sciences as occupying the authoritative realm of truth while relegating indigenous knowledge to the realm of culture and mere belief. Instead of this dominant epistemological hierarchy, Wells attempts to level the playing field. He represents himself as part of a people ("European" people) who, like Singh, are interested in telling stories about their origins. In a further reversal of expectations, it is Wells who is at the disadvantage. As Singh explains to him, "We know our stories. We know about creation. We know we come from here." It is Wells and the Europeans who are still searching.

However, what at first looks like a new mode of engagement between indigenous peoples and those who wish to study them quickly reverts back to old tropes. After the scene's end, Wells laments, "This really isn't going very well. Tradition rarely sits well with cutting-edge science." Aboriginal songlines may say that humans originated in Australia, but DNA analysis of the blood of Aborigines tells him a different story.[2] A white Land Rover is seen speeding away on a dirt road. Wells gazes out to sea: "Let's go see if we can make history." He is off to India. With his cutting-edge genetics, Wells is literally back in the driver's seat, headed for new lands and new discoveries of fundamental truths, leaving Singh behind

Jenny Reardon is Associate Professor of Sociology at the University of California (College 8 Faculty Services, 1156 High Street, Santa Cruz, California 95064, U.S.A. [reardon1@ucsc.edu]). **Kim TallBear** is Assistant Professor in the Department of Environmental Science Policy and Management, University of California (130 Mulford Hall, Berkeley, California 94702, U.S.A. [kimberly.tallbear@berkeley.edu]). This paper was submitted 27 X 10, accepted 2 IX 11, and electronically published 21 II 12.

1. Wells is also the author of the related book *The Journey of Man: A Genetic Odyssey* (2002).
2. Also see Priscilla Wald's essay in which she analyzes how population genetics research, including the Genographic Project, deploys "stories" in ways that are inseparable from science (Wald 2006). She discusses the *Journey of Man* film at length.

 0011-3204/2012/53S5-0020$10.00. DOI: 10.1086/662629

in the outback of Australia, sitting under a tree by a rock, with his old traditional beliefs.

In this scene, what began as a potentially new mode of understanding genetics—as a storytelling practice—ends by enacting an old story: in the interest of promoting "European" knowledge, moral claims to access indigenous lands and bodies get made. This time such claims are not made on the ground that European culture and practices are superior but on the grounds of justice. You have your stories, we just want ours, Wells says to Singh. In so doing, he presents genetic studies of indigenous DNA as part of what it would take for both indigenous peoples and Europeans to know their origins. In this flip of the usual narrative, it is the indigene who potentially takes away. Singh, for example, can deny his DNA, a resource without which Wells and his European people will lose their past. Through constituting this new injustice, Wells retains the old and familiar position of a European making a moral claim on the natural resources of indigenous peoples.

To date, most biological anthropologists and genome scientists who currently make claims to indigenous DNA miss these deeper histories of relations between Europeans and indigenous peoples. We suggest that this is because these histories are associated with race and racism, and most practitioners of human genetics and anthropology believe they abandoned race as an object of study and racism as a practice. Instead, they argue that their efforts will undermine biological conceptions of race and thus counteract racism. Wells and leaders at the NGS, for example, have sold the Genographic Project partially on the grounds that it will show race has no biological meaning, and thus we are all one people.[3] They are not alone in making this claim. It is a common belief among human population geneticists and biological anthropologists who use genetic techniques to study human origins and evolution that if you undercut race as a biological category, you also undercut racism (Human Genome Diversity Project 1993; Reardon 2011).

However, as we demonstrate below, there is no necessary link between antiracialism—that is, opposing racial categories—and antiracism.[4] Specifically, we show how in many instances it is in the name of being against "race" that contemporary scientists continue to make claims to control Native peoples and own their resources. Indeed, as Wells implicitly argues in the *Journey of Man*, indigenous peoples should give their DNA in order to support Europeans' new civilizing project: the cosmopolitan antiracist world promised by genomics.[5] While in the nineteenth century, Europeans sought to tame American wildernesses and the "savages" that inhabited them—a so-called civilizing project now generally viewed as racist—in the twenty-first century, self-proclaimed Europeans continue to make a claim to indigenous peoples and their resources, only this time they do so in the name of the civilizing project of antiracism.

In short, while biological anthropologists and geneticists commonly state desires to build an antiracist future, often they do so on conceptual and material terrains that leave intact old links between whiteness and property. Wells, for example, argues for access to "nature" (in the form of human DNA) in order that he might transform it into something of value and use: knowledge about human evolution. This enacts old understandings of Native peoples, nature, and subjectivity that position Native peoples as having no similar ability or desire to transform nature (in this case, their own natures) into value. They thus lack subjectivity, becoming mere repositories of DNA. These understandings and performances reflect a very old order of things in which whiteness figures as a rational civilizing project that creates symbolic and material value of use to all humanity. As a formation that brings good things to all, whiteness itself becomes a thing of value that should be developed and defended—in this case, Wells argues for the right to study Europeans.[6]

Below, we describe the deeper histories of these relations between whiteness, property, and the human sciences. In particular, we show how the American School of Anthropology enfolded Native peoples into their histories and claimed knowledge about and artifacts of these cultures as their rightful inheritance. Drawing both on the Genographic Project and the recent struggles between Arizona State University (ASU) and the Havasupai Tribe over the use of Havasupai DNA, we then describe how similar enfoldments continue today despite most contemporary human scientists' explicit rejection of hierarchical ideas of race.

We seek to bring greater clarity and visibility to these constitutive links between whiteness, property, and the human sciences in order that the fields of biological anthropology

3. See, e.g., *National Geographic*, "Geographic and IBM Launch Landmark Project to Map How Humankind Populated the Planet," press release, April 13, 2005 (http://www-03.ibm.com/press/us/en/pressrelease/7611.wss [accessed February 15, 2012]). Also see Rediff interview with Spencer Wells (http://www.rediff.com/news/2002/nov/27inter.htm [accessed February 15, 2012]).

4. Our research findings resonate with David Theo Goldberg's recent observation that "antiracialism, it turns out for the most part, is whiteness by another name, by other means, with recruitment of people of color to act as public spokespersons for the cause. . . . Antiracialism is about decategorization, a gesture necessarily by the racially dominant towards those they racially suppress. Antiracism, by contrast . . . seeks to remove the condition not indirectly through the removal of the category in the name of which the repression is enacted. Rather, it seeks to remove the structure of the condition itself" (Goldberg 2009:22).

5. In making the film, Wells explicitly sought to generate support for efforts to collect DNA from indigenous peoples in order to conduct human population genetics research. Wells would go on to lead one such effort, the NGS's Genographic Project.

6. Of course, the antiracialist Wells does not understand the category "European" to be a racial category but rather a geographic one. Yet such a distinction would be lost on many, including human population geneticists. For example, Leslie Clarence Dunn, known as one of the fathers of human population genetics, is cited as arguing that geographic isolation "is the great race maker" (UNESCO 1952:33). These strong links between race and geography highlight the thin lines between antiracialism and racialism and begin to suggest why antiracialism rarely serves as an effective instrument of antiracism.

and population genetics might work to move toward their stated commitments to antiracism (a goal, we argue, that the fields' antiracialism impedes). Specifically, we reflect on how these links can inform extralegal strategies to address tensions between U.S. and other indigenous peoples and genome scientists and their facilitators (ethicists, lawyers, and policy makers). We conclude by suggesting changes to scientific education and professional standards that might improve relations between indigenous peoples and those who study then, and we introduce mechanisms for networking between indigenous peoples, scholars, and policy makers concerned with expanding indigenous governance of science and technology.

Whiteness, Property, and Genomic Science

> *Whiteness as property has carried and produced a heavy legacy. It is a ghost that has haunted the political and legal domains in which claims for justice have been inadequately addressed for far too long.* (Harris 1993:1791)

Connections between whiteness and property have long been recognized as central to the constitution of the American nation and its legal system. Racial theories central to the construction of the United States positioned whites as rational agents capable of intervening in and transforming nature into productive property, thus justifying the taking of Native lands (Harris 1993). In political documents, doctrines, and scientific papers, settlers from Europe represented Native peoples as existing either outside of or at an earlier point in civilization and thus lacking the moral qualities needed to advance civilization through transforming raw natural resources into things of value for humans (Declaration of Independence, 1776; Dippie 1982; Morgan 1909 [1877], 1965 [1881]). The right to constitute "modern" or "European" (to use Wells's descriptor) natural and moral orders found support in these racial ideologies that construed Native peoples as incapable of developing the modern industrial state and its productive citizen, the property-owning individual.

Thus, property and race developed close and strong links in the American context. Indeed, as Cheryl Harris argues in her groundbreaking 1993 *Harvard Law Review* article, "Whiteness as Property" (Harris 1993), whiteness and property became so strongly linked that whiteness in effect became a form of property. If whites alone could construct and possess property, then whiteness itself became a valuable thing. Indeed, Harris describes it as a "treasured property" that accords those who own it rights and privileges that the American legal system defends (Harris 1993:1713). Not surprisingly, these rights and privileges include the right to control the legal meaning of group identity, including the identities of others (e.g., blacks and Native Americans) whose racialization and subordination as "other" is necessary to solidify the exclusive parameters of whiteness. If whiteness and the property and privileges that it encloses are to be effectively defended, its owners must also claim the right to define the others who are not white and who therefore should not access its privileges.

While in recent decades scholars have made evident these connections between whiteness and property as they play out in American law, much less is known about the relations between whiteness, property, and technoscience. Yet as technoscience, particularly the biotechnosciences, continue to rise in importance in societal development strategies, its relations to race and property will only increase in importance. We live in times where for many, the relevant "civilizing" project that shapes their lives is the development of the "knowledge society" in which knowledge is a primary source of wealth.[7] Thus, we suggest that if we are to understand contemporary relations between race and property and their role in constituting contemporary political orders and subjectivities, then sites of knowledge production must come into our critical view.

As a hub for the production of knowledge and conceptions of race in contemporary societies, the life sciences and the genome sciences in particular promise a particularly important vista. We suggest that the often surreal interactions provoked by biological anthropologists and population geneticists attempting to gain access to Native American DNA might provide an exemplary case. In recent decades, Native American DNA has emerged as a new natural resource that Native peoples possess but that the modern subject—the self-identified European—has the desire and ability to develop into knowledge that is of value and use to all humans. As we already noted, while many biological anthropologists and human population geneticists may formally reject "race" as a legitimate scientific object, they continue to value studying and understanding whiteness (e.g., Wells's quest for European origins) and believe that a study of "redness" is a constitutive part of this project.[8]

How did this happen? How did anthropologists and later human geneticists come to understand Native cultures and then biologies as part of the proper inheritance of whites and thus that which scientists had the right to control and study? In what ways have these understandings been challenged? In what ways do they persist? Through an examination of both historical and contemporary cases, these are the questions that we seek to answer.

Constructing "Red" as the Inheritance of Whites

Legal concepts of property and anthropological concepts of human evolution may appear to have lived entirely separate

7. As the science and legal studies scholar Sheila Jasanoff explains, "knowledge has become the primary wealth of nations, displacing natural resources, and knowledgeable individuals constitute possibly the most important form of capital" (Jasanoff 2005:4).

8. Although that is not to say that all reject the reality of race. Some genetic ancestry and health disparities research is certainly racialist in its study of genetic aspects of race while also aiming to be antiracist. See n. 30 for an example.

intellectual lives. However, they are joined by a common concern: inheritance. As Yael Ben-zvi (2007) notes in her important contribution to critical race theory and the history of anthropology, "In both the biological and economic sense, inheritance connects individuals or generations within particular groups so that biological and material properties are transferred from the deceased to the living members of the same group" ("Where Did Red Go?"; Ben-zvi 2007:213). Thus, the law and the human sciences confront the same fundamental question: what constitutes "members of the same group" for the purpose of understanding the transfer of properties? In law, one must determine who is a group member in order to determine who inherits (material) property. In the human sciences, the order of cause and effect is reversed. Who inherits (biological) property determines who is a group member.

As historians of science and critical race theorists document, both American law and science have historically drawn on race to order human beings into groups (Foner 1999; Harris 1993). Scholars commonly focus on the racial line between black and white that is crucial for understanding the operation of race and power in the United States. But it begs an important question: what is the place of other racialized groups in this black/white picture? In particular, why are Native Americans sorely neglected in the majority of analyses of race and power in the United States? Ben-zvi provides a revealing answer: this elision is the result of dominant nineteenth-century anthropological theories that turned Native Americans into the "vanishing ancestors of their presumably white heirs" (Ben-zvi 2007:213).

Analyzing the central works of Lewis Henry Morgan—perhaps the most influential American anthropologist of his time—Ben-zvi demonstrates that anthropological theories of cultural evolutionism positioned Native Americans in a period of human evolution that preceded and made room for whites. For Morgan, "human progress" proceeded through different "ethnical periods": savagery, barbarism, and civilization.[9] To understand this "progress," he argued passionately for the need to study the "aborigines" who represent these periods and relevant subperiods.[10] As Morgan explains in his last work, *Houses and House-Life of the American Aborigines*,

> The progress of mankind from their primitive condition to civilization has been marked and eventful. Each great stage of progress is connected, more or less directly, with some important invention or discovery which materially influenced human progress, and inaugurated an improved condition. For these reasons the period of savagery has been divided into three subperiods, and that of barbarism also into three; the latter of which are chiefly important in their relation to Indian tribes. The Older Period of barbarism, which commences with the introduction of the art of pottery, and the Middle Period, which commences with the use of adobe brick in the construction of houses, and with the cultivation of maize and plants by irrigation, mark two very different and very dissimilar conditions of life. The larger portion of the Indian tribes fall within one or the other of these periods. (Morgan 1965 [1881]:xxv–xxvi)

Not only do "Indian tribes fall within one or the other of these periods," Morgan argued that "in no other part of the earth were these two conditions of human progress [the Older Period and Middle Period] so well represented as by American Indian tribes" (Morgan 1965 [1881]:xxiv). Morgan believed that "knowledge of the culture and state of arts of life in these two periods was indispensable to understanding human progress" (Morgan 1965 [1881]:xxiv–xxv). Therefore, the human sciences could not progress without a study of the American Indian (Morgan 1965 [1881]:xxiv–xxv). Through such study, he argued, "we may recover some portion of the lost history of our own race" (Morgan 1965 [1881]:xxv).

To illustrate, Morgan describes American Indian family structures as open and inclusive, ready to admit new members (i.e., whites, but not blacks, who were out of place in Morgan's evolutionary paradigm; Ben-zvi 2007:217). The adobe bricks that make up Indian homes are similar in shape and material to those in American homes, but without the finished, polished nature of American bricks. In these representations, white people did not violently colonize Native peoples. Instead, whites represented a more evolved form of the same people: Americans. Indeed, Morgan went further. He believed that American Indians represented all of "mankind" in an early stage of evolution. Writing in perhaps his most influential work, *Ancient Society*, he argues

> Since mankind were one in origin, their career has been essentially one, running in different but uniform channels upon all continents, and very similarly in all tribes and nations of mankind down to the same status of advancement. It follows that the history and experience of the American Indian tribe represent, more or less nearly, the history of our own remote ancestors when in corresponding conditions. Forming a part of the human record, their institutions, arts, inventions and practical experience possess a high and special value reaching far beyond the Indian race itself. (Morgan 1909 [1877]:vii)

Morgan therefore urged Americans to enter "this great field and gather its abundant harvest" before the American Indian cultures "perish[ed]," lost to "the influence of American civilization" (1909 [1877]:vii).[11] Pace Spencer Wells, Morgan made many of Wells's same riveting claims 130 years earlier.

9. For Morgan's explanation of his theory of "ethnical periods," see Morgan (1909 [1877]:3–18).

10. In particular, Morgan felt that the study of American aborigines, or "American Indians," should "command as well as deserve the respect of the American people" (Morgan 1965 [1881]:254).

11. Also see Bieder (1986) for an account of Morgan's contributions and his study of American Indians prompted by a sense of urgency that the Indian would inevitably disappear, erasing not only Indian history but the history of the rest of mankind.

The Genographic Project and the Persistence of Nineteenth-Century Anthropological Imaginaries

Although Morgan's theories of cultural evolutionism fell from anthropological favor long ago, the idea that Native Americans and others (i.e., "Africans") represent an earlier period in human evolution and thus can help modern humans understand themselves persists in contemporary anthropological imaginaries. Today such imaginaries manifest themselves not through the study of the building blocks of homes but through the analysis of what many human scientists consider the building blocks of bodies: DNA. Genetic evolutionism has eclipsed Morgan's cultural evolutionism. A rich contemporary example of this can be found in the NGS's Genographic Project.

This self-described "landmark DNA quest to decipher our distant past" seeks to collect the DNA "of very special people living today" in order to tell the story of "the human journey" (Wells 2007:45). Genographic Project organizers have tried very hard to steer this project clear of accusations of racism and have instead presented it as an initiative that unites humans (Reardon 2009; TallBear 2007). However, careful inspection of the "our" of "*our* distant past" and the "very special people" who hold the secrets to this past reveals a parsing of human beings into evolutionary stages not so different from Morgan's theories of cultural evolutionism.

Let us begin with the "very special people" that the Genographic Project seeks to sample. Who are these people? Spencer Wells, human population geneticist and leader of the Project, provides the following answer.

> Ideally, they would be living in the same place as their ancestors did centuries ago. They should have been relatively isolated from immigration from surrounding groups who have moved into the region recently. They also should retain some of their ancestors' ways of life, be it language, marriage patterns, or other cultural attributes. In other words, what we want are *indigenous* people. (Wells 2007:45; italics in original)

Both Morgan's aborigines and Wells's indigenous people represent similar things: "our" ancestors; human beings from whom "we" can learn important lessons about ourselves. In both cases, the "we" that constitutes the knowing subject is differentiated from the objects of study. In Morgan's case, the "we" are made up of members of "modern" society, and their objects of study are "aborigines." Wells tries very hard to avoid this hierarchical language and the racist legacies it invokes. Yet implicit, indeed explicit, in the Genographic Project is the notion that some people—namely, what the project leaders call "indigenous people"—live in "remote" places, closer to the origins of humanity. Wells does not use the now-loaded language of "savage" or "barbarous," but he does describe indigenous people as having genomes that are simpler to decipher and that leave a "clearer trail" (Wells 2007:4). This makes them different from those whose genomes evolved after what Wells describes as "the mobility revolution" (Wells 2007:48). It is these latter individuals—individuals whose genomes became more complicated over the course of human evolution—whose secrets are locked up in the more clear genomes of indigenous peoples. It is the journey of these individuals that the Genographic Project and genetic scientists more broadly seek to document.

In order to make this imaginary work, the Genographic Project organizers define "indigenous people" in a very particular way. As Wells explains in the passage above, "indigenous people" are "relatively isolated from immigration from surrounding groups." Although technically the Genographic Project does allow for self-identified indigenous populations to come forward and ask to be included in the study (Genographic Project 2005:12; TallBear 2007; Zhadanov et al. 2010), standard practice is that scientifically interesting populations must conform to long-standing criteria of genetic distinctiveness conventionally associated with geographic isolation.[12] To ensure that an indigenous person conforms to this criterion, organizers specify that "the participant will need to have grandparents who were members of the population in question." This, they explain, will minimize admixture and "assure that the genetic lineages we find are as representative of the ancient history of the population as possible" (Genographic Project 2005:12).[13]

12. A tribe from Massachusetts, the Seaconke Wampanoag, did come forward of their own accord to participate in the project at the very moment when the Genographic Project was encountering trouble recruiting U.S. indigenes. The Genographic Project thus received positive press for collaborating with a Native American tribe (TallBear 2007). The Genographic Project's scientists and tribal historians coauthored a scientific article published in the *American Journal of Physical Anthropology* detailing the results of sampling (Zhadanov et al. 2010). Interestingly, this article is unlikely to contribute to scientific understandings of ancient migrations to the Americas. The Seaconke Wampanoag who were sampled largely trace to European and African populations. Indeed they were shown to have no "maternal Native American lineages" and only one "Native American" paternal haplotype in an individual with known Cherokee male ancestry (Zhadanov et al. 2010:586). Instead, the article is notable for its insightful treatment of nongenetic Wampanoag history and the fact that it was coauthored by Genographic Project scientists and Wampanoag tribal members.

13. How many grandparents must be a member of the population is not further specified. However, the ideal in genetic studies of human evolution is to sample individuals with four grandparents from the same population. In Luca Cavalli-Sforza et al.'s *History and Geography of Human Genes*, aboriginal populations with "25% or more admixture" are excluded (Cavalli-Sforza, Menozzi, and Piazza 1994:24). Smaller-scale studies are even stricter, ranging from 0% alleged admixture in individuals (four endogenous grandparents; Lorenz and Smith 1994; Torroni et al. 1993*b*) to populational admixture rates of ≤5% (Callegari-Jacques et al. 1993; Neel 1978; Torroni et al. 1992), 8.7% (Torroni et al. 1992), and 12% (Torroni et al. 1993*a*). "Admixture" is calculated according to the presence in populations of haplotypes or genetic lineages that are tied to non-American geographies. Two respectable anthropological genetics texts (Crawford 1998; Relethford 2003) also completely miss discussing how populations or individuals are chosen/constituted as "American Indian" (or "Eskaleut," "Nadene," or "Amerind") for sampling. Other key articles about Native American migrations also skip discussions of criteria

These acts of construction rarely gain recognition. Genetic scientists such as Spencer Wells and Luca Cavalli-Sforza, for example, do not believe that they play any role in constructing "indigenous people"; instead, they believe that "indigenous people" and their genomes exist as real phenomena in the world that they simply sample and study. This belief, we argue, is the product of an anthropological imaginary that dates back to at least the late nineteenth century—one that posits indigenous peoples as distinct from modern humans and as representatives of an earlier stage of human evolution.

It is this same imaginary that leads human population geneticists to assume their right to study indigenous DNA. If indigenous people represent modern humans at an earlier point in evolution, then indigenous DNA is part of modern humans' inheritance and, thus, property. This implies the further right to study that DNA. Specifically, the nineteenth-century imaginaries that animate contemporary human population genetics make it possible to imagine indigenous DNA as constitutive elements of contemporary "white" bodies and thus part of the property that those who can claim a white identity rightfully control.[14] Because concepts of whiteness tie closely to ideas of modernity and rationality (Dua, Razack, and Warner 2005; Goldberg 1993; Kempf 2002; Puar 2001; Said 1979 [1978]) and then to science (Goonatilake 1998; Harding 2008; Subramaniam 2001), genetic scientists not typically considered "white" who work on projects such as the Genographic Project can partake of the privileges and power that whiteness in collusion with rationality offers up. They too can claim property rights in indigenous DNA.[15]

ASU versus Havasupai: Our Interests Are in Your Interest

We argue that this right to study indigenous DNA for purposes not sanctioned by indigenous peoples is clearly at issue in the recent legal dispute between the Havasupai and ASU over the use of Havasupai DNA samples. This case has been widely represented as a matter of deceitful scientists who failed to inform their research subjects about use of their samples (e.g., Bommersbach 2008; Shaffer 2004).[16] While certainly an egregious case of failed informed consent, we contend that the problems are at once less tractable and more fundamental. The deeper structural problem is the relations between whiteness and property this case enacts—specifically, the way in which Native peoples once again become folded into the long-standing goals of "Europeans" to transform nature into useful products and to create knowledge that will be of use and benefit to all people.

At issue in this case—in which both the tribe and 52 individual members of the Havasupai Tribe sued ASU for $50 and $25 million, respectively, for misuse of their blood samples—is whether ASU researchers distributed DNA samples collected from members of the Havasupai Tribe in the early 1990s in a manner that violated the tribe's informed consent rights.[17] What is notable for our analysis is not that population geneticists distributed Havasupai DNA to non-ASU researchers without informed consent but that once this was pointed out, researchers still adamantly defended their right to engage in this practice.[18] Most of the researchers involved justified the distribution of samples for research that did not directly relate to diabetes—the research the Havasupai explicitly approved—on the grounds that it advanced science.[19] As Therese Markow, the researcher who originally collected the Havasupai DNA, explained to the independent investigators hired by ASU and the Havasupai, "'Knowledge is power,' and the more one knows, the better off one is from a research perspective" (Hart and Sobraske 2003:87).

Markow has described the Havasupai project as a broad one of "medical/genetics" within which the most pressing problems of Havasupai would be investigated: diabetes and schizophrenia (Dalton 2004:500; Hart and Sobraske 2003:83). She has also argued that her understanding of the research purpose broadly was to study Havasupai "behavioral and medical disorders," and that is what they were consented for (despite missing documentation and graduate student claims

used to determine who constitutes "the Pima" or "the Papago" (e.g., Wallace and Torroni 1992) or "Native Americans" (Santos et al. 1999) for sampling purposes. Those who rely on data sets from older studies are especially vague in discussing their inclusion criteria for samples (e.g., Torroni et al. 1992, 1993*a*, 1993*b*). It would appear that the authors believe that group boundaries and sampling decisions are self-evident.

14. Moreover, as genetic concepts of indigeneity become normative and accepted as those that signify the truly "indigenous," different—and often more inclusive—biological and social criteria used by indigenous peoples themselves are abstracted from view. As a result, the idea of indigenous governance and citizenship is implicitly challenged (TallBear 2013).

15. This is one of many examples in which the phenomenon of whiteness does not map easily onto "white people." Thus, as George Lipsitz points out, "opposing whiteness is not the same thing as opposing white people" (Lipsitz 2006:viii).

16. Also see Hart and Sobraske (2003), the lengthy investigative report commissioned by ASU, which presents an ultimately critical picture of scientists involved in the case. Dalton (2004) paints a less damning picture.

17. *Tilousi et al. v. Arizona State University et al.*, CV 2004-0115 (Ariz. Superior Ct., Coconino County). Also see *Havasupai Tribe of Havasupai Reservation v. Arizona Board of Regents*, 204 P.3d 1063 (Ariz. App. Div. 1, 2008).

18. Few dispute whether researchers at ASU distributed Havasupai DNA in a manner that moved beyond what tribal members understood would happen. What is at issue is whether ASU had gained legal informed consent to engage in this form of distribution. Resolution of this point is not imminent, as the lead researcher, Therese Markow, claims a moving company lost the relevant informed consent forms when she moved to Arizona University (Hart and Sobraske 2003:85).

19. Markow and her co-principal investigator also used data gathered from the Havasupai for diabetes-related research (as the tribe understood it) and data gathered in previous cultural anthropology studies for schizophrenia-related research that the tribe claims not to have approved. In Markow and Martin (1993), they calculate "inbreeding coefficients" for tribal members.

to the contrary). In the middle of the controversial and ongoing dispute about whether Havasupai were properly consented for anything but diabetes, Markow continued to defend research to which the Havasupai objected (i.e., research on ancient human migrations by biological anthropologists at other institutions; Hart and Sobraske 2003:87). She did so on the grounds that once such basic scientific research is published, it becomes public and serves as a foundation on which other researchers can do basic science that might benefit subjects medically in the future. For example, when researchers compared data from research subjects identified as Pima and Havasupai, they concluded that not all Native Americans are genetically alike. Such knowledge, Markow argued, can lead to a better understanding of disease within the population and "can be helpful in counseling and providing guidance to improve treatment" (Hart and Sobraske 2003:85–87, 59).[20]

Dr. Stephen Mack, a researcher at Roche Molecular Labs, made similar assertions. In 1996 he published his University of California, Berkeley, dissertation, a population genetics study that addressed how long Native American populations had been in the Americas (Mack 1996). The thesis was based on research that used purified DNA samples (not original Havasupai specimens, but DNA collected from the cell lines made from the original specimens) received from Dr. Henry Erlich of Roche Molecular Labs in Berkeley. In using Havasupai data to this end, he argued, along with Theodosius Dobzhansky, that "nothing in biology makes sense apart from evolution" (Erlich quoting Dobzhansky 1973). In other words, while his research was not a medical study per se, it did contribute to understandings of the biological context in which such medically related studies could proceed (Hart and Sobraske 2003:78). Implicit in Mack's assertion is that if the Havasupai wanted to understand biologically what made them more susceptible to diabetes, they would have to subject themselves to studies of human evolution and human migrations. For both Markow and Mack, despite the Havasupai's misgivings, all the research ultimately works to further the tribal goal of understanding the biological underpinnings of diabetes.[21]

Indeed, the value of all genetic research, and the right of researchers to study Havasupai DNA in whatever way they deemed appropriate, went unquestioned by most who studied the Havasupai DNA. The report produced by the independent investigators (known as the Hart Report) indicates that none of the researchers who received Havasupai DNA asked for documentation that the Havasupai had given their consent for the distribution of their DNA (Hart and Sobraske 2003: 72, 77, 74). Nor was any documentation of the transfer of Havasupai DNA maintained. When samples were sent to other labs or collaborators, Markow notes that it was done "with a phone call" (Hart and Sobraske 2003:82). It would appear that most simply assumed they had the right to study the Havasupai DNA.

Further, one scientist explicitly resisted the very idea that he should have to account for his use of the Havasupai samples. The Hart Report describes this scientist—the noted population geneticist from Stanford, Dr. Peter Parham—as "at best marginally cooperative." He would not meet with investigators in person. They note, "When asked, for example, whether he had or would produce any documents concerning the samples and the work performed in his lab, Dr. Parham responded: 'Obtaining this information would consume a lot of my time. Could you please provide an explanation of why I should expend this time and effort?'" (Hart and Sobraske 2003:71–72). Speaking through Stanford University general counsel, Parham defended his use of samples for three reasons. First, he did not receive tissue or cells that were present in the original Havasupai blood samples. Rather, he received transformed cell lines that were made by ASU, "just descendents" of cells taken from Havasupai tribal member bodies (Hart and Sobraske 2003:71). Second, the existing cell lines had no individual identifiers, and Parham received no genealogies (Hart and Sobraske 2003:72).[22] For these first two reasons, Parham noted, his research with the material was exempt from IRB approval.[23]

Parham's third reason, though, is perhaps most revealing. Like Markow and Mack, he argued that it was appropriate to work with Havasupai samples because he was producing something that would be of medical value to them. In particular, the study he did reassessed the accuracy of Markow's previous human leukocyte antigen (HLA) class I typing of Havasupai samples. This reassessment, he contended, was of indirect medical relevance because diabetes can lead to kidney failure and the need for a transplant. His study results could inform and help in the process of identifying a suitable HLA-matched kidney donor for any Havasupai who might eventually need a kidney transplant (Hart and Sobraske 2003:72).

There are indications that other geneticists may have been sent samples, but the documentary trail is unclear (Hart and Sobraske 2003:8, 70, 82). In all, three non-ASU scientists obtained possession of cell lines descended from Havasupai tis-

20. One presumes that Markow is imagining genetic counseling within the confines of medical care down the road when any of these findings have actually translated into innovations in medical care. See Manolio (2010) and Wade (2009) for arguments about the as yet unfulfilled promises of such claims and worries about to whom such innovations, when they do come, will be available or not. Charis Thompson explains that African American men are overrepresented in criminal forensic genetics databases, while wealthy whites are overrepresented in personalized medicine databases (personal communication with author, May 2011).

21. As Lewis Henry Morgan argued over a century ago, the study of Native peoples proved central to the study of human evolution. Mack and Markow merely made this argument work in the reverse direction: to understand Native peoples means understanding human evolution.

22. The investigative report, however, points out that there were identifiers—"I.D. numbers employed in Dr. Markow's identification system" (81 tied to tribal member names in her secure records) and that these are listed in the publication (Hart and Sobraske 2003:149).

23. Markow also defends the individual privacy protections as sufficient in the Havasupai research as she only ever used group identities in her research and publications (Hart and Sobraske 2003:84).

sue specimens for human migrations research (Hammer et al. 1998; Hart and Sobraske 2003:70, 128; Karafet et al. 1997, 1999).[24] In addition, multiple scientists coauthored papers based on Havasupai genetic data while they were never in possession of actual cell lines, with many subsequent papers citing them (Hart and Sobraske 2003:136–139).[25] None obtained any direct consent from the Havasupai for these secondary uses. The Hart report demonstrates that involved scientists acted as if they owned—that is, they had the right to possess and control—the Havasupai's DNA.[26]

Property Interest Cloaked in Color Blindness and Claims to Neutrality

When genome scientists view their science as neutral—that is, in the interest of all (including groups such as the Havasupai)—they miss this assumed property interest. When Therese Markow explained to Stephen Hart, "Knowledge is power," presumably she meant that knowledge is power for all, but is this presumption accurate? Will, for example, the knowledge that University of Arizona population geneticist Michael Hammer claims to have produced about Bering Strait migrations using Havasupai samples—research that Markow viewed as his "obligation" to publish—empower tribes (Hart and Sobraske 2003:131, 89)?

There are indications from the courts that the answer to this question is a resounding "no" and that tribes justifiably fear that this kind of knowledge might be used against them. With claims to land and governance rights at stake, the state relies on anthropological and historical evidence to determine whether applicants meet a "socially constructed" image of the Indian (McCulloch and Wilkins 1995). Genetics is increasingly important to anthropology and the (re)construction of human history. Ben-zvi and others (e.g., Bieder 1986; Deloria 1988 [1969]) have shown the considerable influence of anthropologists historically in constituting Nativeness. There is every reason to expect that the state will avail itself of *biological* anthropological evidence in order to determine who or what is Indian.

Indeed, in the controversial Kennewick Man case, in which tribes claimed the 9,000-year-old remains for reburial and scientists claimed them for further study, the state ordered the extraction of DNA in order to determine the "cultural affiliation" of the bones. DNA amplification was unsuccessful because of bone mineralization (Kaestle 2000), but that will not always be the case. Tribes have much at stake when science asserts intellectual authority over and alters the parameters of indigeneity. Because of lack of conclusive (scientifically mediated) physical evidence of Kennewick Man's cultural affiliation with living tribes, the remains were deposited with the Burke Museum at the University of Washington, a "court appointed neutral repository."[27] Scientists were granted the right to study them.

The view that genetic knowledge of human evolution is an objective neutral good that benefits all and not a particular kind of knowledge that fits within a particular way of living and enacting the world in effect denies indigenous people such as the Havasupai the right to control their own genomic resources and identity. While, as Cheryl Harris notes, whites are granted the right to use and enjoy their reputation as "white" people, indigenous people, such as the Havasupai, would not appear to have a similar right to control the construction of their identity as it would impinge on the right—even obligation—of scientists to do research. Indeed, countervailing claims by Native Americans to a property interest in their own biological materials and history often are viewed—much as Harris describes white resistance to affirmative action—as an obstruction of "the original or current distribution of power, property, and resources [that are] the result of 'right' and 'merit'" (Harris 1993:1778). In this case, the scientists argue that they had the right to study Havasupai DNA on the grounds that there was scientific merit to their research. Markow recently defended herself to the *New York Times*: "I was doing good science" (Harmon 2010). These arguments not only negate Native American claims, they also position Native peoples as acting in a "politically motivated" manner that threatens science.[28]

Within the life sciences, these sets of ideas and positions are supported by an ideology of color blindness. As Cheryl Harris explains, by the early 1990s, many dominant social institutions had replaced the old definition of race, one that "created a false linkage between race and inferiority," with a new "color-blind" one that "denies the real linkage between race and oppression."[29] The new definition, like the old explicitly racist definition, maintains white racial domination over other races—this time by denying the "historical context of white domination and Black subordination" (Harris 1993:1768). Color blindness does not recognize, yet simultaneously

24. These included Henry Erlich of Roche Molecular Labs, Michael Hammer of the University of Arizona, and Peter Parham of Stanford.

25. These included Stephen Zegura of the University of Arizona; Tatiana Karafet of the University of Arizona and the Laboratory of Human Molecular and Evolutionary Genetics, Institute of Cytology and Genetics, Novosibirsk, Russia; and their multiple coauthors (Karafet et al. 1997, 1999).

26. Indeed, the legal regime seems to support scientists exercising a property right in samples even when informed consent is in question.

27. See http://www.washington.edu/burkemuseum/kman/ (accessed June 9, 2011).

28. In a similar manner, Diversity Project organizers positioned indigenous critics of their project as "politically motivated" while they continued to view themselves as neutral scientists working on behalf of all humans (Reardon 2005:113).

29. Central to the norm of color blindness is "the assertion that race is color and color does not matter" (Harris 1993:1768). We can attribute to the color-blind ideology both the racialist and antiracialist positions that we described earlier. Whether race is biologically real or not, it should not matter socially, i.e., it should not be used to discriminate against or in favor of any race.

supports, long-standing property regimes in favor of whiteness.[30]

The biological sciences have similarly transformed. Today only the rare scientist would invoke biological or genetic data to make a claim about racial inferiority or superiority. Like the law, contemporary biological science holds that race and ethnicity—whether they are biologically real or not—should not matter for the purposes of ordering society or determining the rights or worth of human beings. Indeed, this has been a central claim of scientists involved in genetic ancestry research.[31] Instead, it is commonplace to believe that if one is doing "scientific" work, then it will benefit all humans. It is not the norm to suggest that practices must be responsive to the possibility of causing social harms.[32] Thus, many scientists may simply assume a right to study and control Native American DNA and maintain no practices of accountability (e.g., no records of where the Havasupai DNA was shipped), thus upholding long-standing regimes of property and whiteness.

Reconceiving Genomics and Property

On April 10, 2010 the Arizona Board of Regents (ABOR) settled out of court with Havasupai tribal members for $700,000 (ABOR 2010), a fraction of monetary damages claimed in the original lawsuits.[33] The settlement also provided for tribal member scholarships and collaborations between ABOR and the tribe in "health, education, economic development, and engineering planning" (ABOR 2010). Perhaps the most important aspect of the settlement is that it provided for the repatriation of blood samples. On April 22, 2010, a delegation of Havasupai arrived at ASU. As they sang ceremonial songs, a freezer was unlocked by a university official. The Havasupai were there to claim long stored blood samples—some from individuals now dead—for burial in the floor of the Grand Canyon, the Havasupai home (Harmon 2010; Kiefer 2010).

Because the case was settled out of court, there is no legal precedent for future cases in which researchers and institutions violate research subjects' rights. However, the settlement is important because ABOR and the university acknowledged misconduct serious enough to award monetary damages.[34] The settlement could also "affect plaintiffs' and attorneys' views of litigation opportunities," and it could lead researchers to work to understand and consider more thoroughly subject "perspectives" on the nature of research being performed on their biological materials (Mello and Wolf 2010:2–3).

Within Native and indigenous communities, these perspectives increasingly include those who claim more comprehensive rights to govern research activities and to have a greater say in the constitution of knowledge about their bodies, populations, and histories (Mariella et al. 2009).[35] In particular, invoking sovereignty discourses, indigenous peoples themselves increasingly make ownership claims on their genetic resources and their genetic heritage (Mariella et al. 2009; Mead and Ratuva 2007).[36] Such claims extend not only to biological samples and derived data but also to their histories that can be known in part through DNA. However, to date, these claims alone have proven largely ineffective in securing increased tribal control of samples and data. We conclude by considering why this has been the case, what this reveals about the genomic constructions of race and property, and what might constitute a more responsive and constructive approach to property in the domains of genomics and biological anthropology.

The Limits of Existing Legal and Regulatory Frameworks

In the Havasupai case, the attorneys issued six charges: breach of informed consent, infliction of emotional distress, fraud, negligence, violation of civil rights, and most importantly for our topic, "conversion" of Havasupai blood samples for the

30. Harris further explains that defining race as nothing more than color "and therefore meaningless . . . is as subordinating as defining race to be scientifically determinative of inherent deficiency" (1993:1768). While the old definition of race linked it with hierarchy and notions of inferiority, "the new definition denies the real linkage between race and oppression under systematic white supremacy. Distorting and denying reality, both definitions support race subordination" (Harris 1993:1768).

31. Consider DNAPrint Genomics, a prominent DNA ancestry testing company until they declared bankruptcy in early 2009. Their popular technology, the patented AncestryByDNA test, was quickly licensed to DNA Diagnostics Center. DNAPrint asserts that the recent move to describe race as "socially constructed" is oversimplified and that there is a "genetic component of race" that can be measured by their AncestryByDNA test. But they also argue that this genetic component of race is not socially or politically relevant, i.e., a racial*ist* position that decries rac*ism*. As the company explains, "DNA has no recorded history of your political, social, personal or religious beliefs." See "What Is Race?" (http://www.ancestrybydna.com/welcome/faq/#q1 [accessed October 21, 2006]), now available through the internet archive at http://web.archive.org/web/20060709021118/http://www.ancestrybydna.com/welcome/faq/#q1 (accessed February 15, 2012).

32. That said, author conversations and early ethnographic work with often younger critical scientists suggest changing attitudes toward scientific property claims in indigenous and other biological samples (e.g., TallBear's National Science Foundation award SES-1027307, "Constituting Knowledge across Cultures of Expertise and Tradition: An Ethnographic Study of Indigenous Genome Scientists and Their Collaborators"). How the broader academic field and the law will respond to openings for ethical paradigm changes is yet to be seen.

33. See n. 15.

34. Personal conversation by authors with Rebecca Tsosie, director of American Indian Legal Program, Arizona State University.

35. Also see http://www.ihs.gov/Research/index.cfm?module=hrpp_irb for an incomplete list of IRBs and tribal IRBs as well (accessed June 9, 2011).

36. The Mataatua Declaration on cultural and intellectual property rights of indigenous peoples, June 18, 1993, in *Pacific genes and life patents: Pacific indigenous experiences and analysis of the commodification and ownership of life*, Aroha Te Pareake Mead and Steven Ratuva, eds., 197–200 (Wellington: Call of the Earth Llamado de la Tierra and the United Nations University Institute of Advanced Studies, 2007). The Mataatua Declaration was passed by a plenary of delegates from Ainu (Japan), Australia, Cook Islands, Fiji, Indian, Panama, Peru, Philippines, Surinam, the United States, and Aotearoa (New Zealand).

scientists' benefit. The complaint asserts that "The Blood Samples are Plaintiffs' tangible property and Defendants' activities . . . constitute a substantial interference with plaintiffs' possession or right thereto, as well as the defendants' wrongful exercise of dominion over Plaintiffs' personal property rights in the Blood Samples." The samples are described as "unique living substances and properties" that "would provide defendants with competitive, commercial, and scientific advantages" (*Tilousi et al. v. Arizona State University et al.*, CV 2004-0115 [Ariz. Superior Ct., Coconino County, 11]). They sought a total of $25 million in compensatory and punitive damages from ABOR, ASU, and individual scientists. In this, as in many other cases, indigenous groups failed to secure property rights to their biological samples and data derived from them. In its motion to dismiss the case, the federal district court explicitly dismissed three of the charges, including the conversion (or property rights infraction) charge.

This outcome should not surprise. Genome scientists as well as the law commonly position knowledge and even molecules as separate from the bodies of donors and therefore as not the property of donors. For example, as we noted above, the Hart Report claims Peter Parham of Stanford argued that "the cells he received no longer contained any of the cells that were present in the original blood sample, but are just descendants" (Hart and Sobraske 2003:71). Therefore, Parham concluded that he was free to use the samples as he pleased. In *Moore v. Regents of California* (1990), the California Supreme Court similarly argued that once tissues leave an individual, the individual does not retain property rights.[37] Although biological anthropologists' and population geneticists' uses and claims about DNA have been allowed, when nonscientists such as tribes or individual research subjects assert claims and the right to control DNA extracted from their bodies, these claims are disallowed. In the Havasupai case, connections are severed between blood cells collected from indigenous people and the cell lines made from them.

We argue that this is because there is a much deeper property issue at play, the one our paper describes: the property of whiteness and the role genomics plays in maintenance of this form of property. As exhibited by the exchange between Spencer Wells and the Aboriginal artist in *The Journey of Man*, both scientists and indigenous peoples make claims to narrate history and determine identity. However, while indigenous peoples explicitly assert their right to narrate their own histories and identities, Euro-American nation-states and scientists usually need not do so because these histories and identities are recognized and upheld in dominant systems of law and science. As Harris and Ben-zvi show, the United States sets and enforces the parameters of whiteness by drawing on legal and scientific discourses of property and ownership that are pervasive but not officially recognized.

In short, Euro-American law and science operate within and act to enforce dominant social formations. Further, power inequities exist between these formations and indigenous peoples. As a result, indigenous peoples' efforts to reclaim rights to their resources and identities through dominant legal and regulatory mechanisms are likely to continue to fail. These mechanisms are mediated at every turn by power relations shaped by histories of racism and colonialism, and it is these relations that must be addressed if we are to recognize and respond to the problems created by the constitution of whiteness as property by both the law and the life sciences.

Intercultural Justice

Although human genetic variation language and practices regularly ignore the already social and political nature of research, there are ideas brewing in critical research communities that outline how the future of anthropological and genomic research and the power relations between those who study and those who are studied can be different. Of these, we believe most promising are those that are not free of state authority but rather combine the pragmatic advantages of tribal and indigenous regulation with efforts to transform our philosophical and ethical landscapes.

American Indian law scholar Rebecca Tsosie proposes shifting the basic theoretical and legal framework within which we evaluate indigenous and scientific claims to one guided by a framework of indigenous genetic resources. Specifically, Tsosie calls for the development of "intercultural justice." Such a framework would entail a "restructuring [of] the legal relationships among Native nations and the United States and its non-Indian citizens to alleviate the historical and contemporary grievances and harms that continue to affect Native communities" (Tsosie 2007:498). Such a framework would draw on tribal and international law to better protect "intergroup equality and fundamental human rights" (2007:397). As Tsosie explains, Euro-American values of property and privacy see all resources as capable of being owned, "efficiently" used, exploited, and therefore transferable to ensure their more productive use (Tsosie 2007:397). Many indigenous groups, on the other hand, understand property to be collective or communal in nature, believe that these rights are coupled with responsibilities to protect the resource, and recognize that property can have spiritual value and should not threaten group privacy (Tsosie 2007:397–398). An intercultural framework would not only account for the historical exploitation of indigenous peoples as research subjects, but it would also consider and address these differences in approaches to property.

Technically, consideration of these differences should already be ensured by tribal rights of self-determination. However, when challenged, the enforcement of tribal sovereignty

37. *Moore v. Regents of California*, 51 Cal. 3d 120 (1990). However, the recent *Association for Molecular Pathology v. USPTO* decision in which U.S. District Court for the Southern District of New York declared invalid some of the patents of Myriad Genetics related to the breast cancer susceptibility gene 1 and 2 indicates that the Moore decision is under reconsideration. See Conley and Vorhaus (2010) for a description of this decision.

falls to mediation in state and federal courts, where it is adjudicated by non-Natives and thus non-Native cultural conceptions, values, and law. Tsosie reminds us that tribal law and institutions—such as tribal institutional review boards (IRBs)—are better positioned to respond effectively to tribal values about research and knowledge. Thus, mainstream collaboration with tribal court systems and the development of tribal IRBs can provide a more effective governance structure for overseeing knowledge production that is not damaging to tribal interests (Tsosie 2007:408–409).

Indigenous Control of Biological Specimens

In recent years, tribes and First Nations have put forward promising mechanisms for direct tribal control of biological samples, although they are not without enforcement challenges. The Alaska Area Specimen Bank is Alaska Native controlled. Located in Anchorage on the Alaska Native Health Campus, the bank is managed by the Alaska Native Tribal Health Consortium (ANTHC). Nine tribal health organizations make up the ANTHC. During the last 50 years of biomedical research, tribal people served by these health organizations have contributed nearly a half million specimens to the bank (CDC 2009). To access specimens, investigators must present research plans in communities whose samples they want to access. After securing community approval for new research, the Alaska area Indian Health Service (IHS) IRB must also grant its consent for research on bank specimens (Terry Powell, "Genomics, Tribes, and Indigenous Peoples" workshop 2008).[38] The bank is housed in a Centers for Disease Control (CDC) facility as part of a longtime cooperative research arrangement. Together, tribal health leaders and the CDC developed bank policies and procedures to maximize health benefits to Alaska Natives from any research conducted with samples while protecting their privacy (CDC 2009).

A second mechanism for tribal control of biological samples is the DNA on loan concept developed by geneticist Laura Arbour and Canadian Institutes of Health Research (CIHR) official Doris Cook (Arbour and Cook 2006). "DNA on loan" means simply that a researcher is considered only a temporary steward of blood and tissues he/she accepts for research. The community or individual retains ownership and control over the future handling and uses of the samples (Arbour and Cook 2006:155; Couzin-Frankel 2010:1218). The researcher cannot conduct secondary research on the samples without first securing consent for the new research. Anonymized samples, too, are retained as the property of community and individual donors. As long as written consent is obtained stipulating that the samples are "on loan," legal adherence by researchers is required. This model encourages researchers to maintain regular communication and ongoing relationships with communities if they want to make use of samples as new questions and technologies of investigation arise. This is opposed to the "helicopter research" that indigenous peoples lament in which researchers drop in for samples and then leave, never to be heard from again. The DNA on loan concept is the default property arrangement promoted by the CIHR in their "Guidelines for Health Research Involving Aboriginal People."[39]

Changing the Cultures of Genomics and Property

These and other promising legal models are being investigated by others. However, we contend that for legal institutions to undertake such change, scientific institutions will have to adopt more inclusive cultural frameworks in their governance of genomic and other research. The work cannot be left only to tribal IRBs and to the courts.

In collaboration with an interdisciplinary group of scholars,[40] we have suggested the need to develop an international research network and clearing house that could do some or all of the following to facilitate the creation and adoption of more "intercultural" frameworks.[41] Ideas include promoting both indigenous and international governance of genomic research by using the United Nations Declaration of the Rights of Indigenous Peoples as a baseline governance principle for governance of genomic research (United Nations 2007).[42] Our group also suggests the international sharing of model codes and contracts,[43] some crafted for use in the United States but revised for potential use among non-U.S. indigenes.[44] We also think it is important to highlight the work of critical scientists who are developing new approaches to sampling and genetic resource governance such as the DNA on loan concept and

38. The workshop funded by the National Science Foundation and hosted by ASU's law school and its American Indian Policy Institute, November 6–7, 2008, consisted of conversations and strategizing about indigenous genomics and representation, sovereignty, and property. Participants in addition to the authors included legal scholars and practitioners Philip (Sam) Deloria (American Indian College Fund), Nadja Kanellopoulou (Oxford), Pilar Ossorio (Wisconsin), Brett Lee Shelton, and Rebecca Tsosie (ASU Law); science studies scholars Paul Oldham and Brian Wynne (Lancaster University, UK); geneticists Laura Arbour (University of British Columbia) and Nanibaa' Garrison (Stanford); and Native American IRB expert Terry Powell (Alaska Area Indian Health Service). The workshop was the first in a series of meetings and related projects that will explore opportunities for expanding indigenous governance of genomic research.

39. See http://www.cihr-irsc.gc.ca/e/29134.html (accessed June 9, 2011).

40. See n. 38 for names of collaborating scholars.

41. Genomics, Governance, and Indigenous Peoples Workshop, Tempe, Arizona State University College Law, November 6–7, 2008 (http://cnr.berkeley.edu/tallbear/workshop/index.html [accessed June 9, 2011]). For a description of this workshop, see http://cnr.berkeley.edu/tallbear/workshop/participants.html (accessed June 9, 2011).

42. Article 31 of this declaration specifies indigenous peoples' right to control and protect various cultural resources and "human and genetic resources" (United Nations 2007).

43. See American Indian Law Center (1999) and Indigenous Peoples Council on Biocolonialism (2000) for such models.

44. See http://indigenousgenomicsgovernance.org/ (accessed June 9, 2011).

the tribally controlled biobank. In particular, we want to call attention to emerging research in which scientists are rethinking their research questions such that they reflect not only a "European" view of historical events (including genomic events) and values about which knowledge is important to produce but also which address a broader array of standpoints, thus resulting in a broader array of "truths."[45]

Another avenue of change is supporting educational and advocacy initiatives with national and international scientific associations in which changes are suggested to professional ethics guidelines and curricula. This can make disciplines more responsive in their research and teaching to differing concepts of property and relationships to knowledge. Federal funding agencies can also be targeted. We take inspiration from the CIHR guidelines that are "contractual" and "voluntarily assumed by the researcher in return for the funding provided by CIHR." Among other directives, the guidelines instruct researchers to respect not only aboriginal "worldviews" as those pertain to notions of collectivism and sacredness of knowledge and specimens but also to respect aboriginal jurisdiction over research, precisely the collaborative move that Tsosie calls for.[46]

Supporting and creating these (inter)national and intercultural networks and initiatives can help facilitate changing the culture of human genomics and biological anthropology such that it is the norm to recognize and respond to the power relations at play in these vital areas of research. It is through making these changes that we may begin to address the "heavy legacy" of whiteness as a form of property that both law and science have inherited and move the fields of biological anthropology and genetics from antiracial to antiracist futures.

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45. We are currently collaborating with a biological anthropologist colleague to plan a critical sampling practices workshop in which a select group of biological anthropologists and other genome scientists, social scientists, and genome policy experts working in different parts of the world will come together to engage in a facilitated dialogue. The goal will be to share, document, and strategize about ways of researching that engage race concepts more critically and that respond better to indigenous priorities and challenges.

46. See http://www.cihr-irsc.gc.ca/e/29134.html#4.13 (accessed June 9, 2011).

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Old Bones, New Powers

by Jean-François Véran

In the 2006 Wenner-Gren symposium volume edited by Ribeiro and Escobar and titled *World Anthropologies: Disciplinary Transformations in Systems of Power*, the central question focused on the ways in which cultural anthropology was being challenged and reshaped by "transformations within systems of power." In this essay, I will explore two propositions: first, that the question can usefully be reversed to examine how the anthropological field is itself a key transformer of those systems of power; and second, that the idea of "world anthropologies" presented in Ribeiro and Escobar can be challenged by expanding it to biological anthropology. In doing so, I suggest that the stimulating pluralization of scientific production can be combined with the (re)construction of a shared world anthropology.

When George Herbert, fifth Lord of Carnarvon, announced the discovery of the tomb of Tutankhamen in 1922, he negotiated an exclusive agreement with the *Times* of London to break the story. The local Egyptian media could not even cover the event. In 2010, the results of the analysis of Tutankhamen's DNA were announced by Zahi Hawass, the head of the Egyptian Department of Antiquities of Cairo. Nowadays, only the Supreme Council of his department is empowered to communicate archaeological discoveries in Egypt, even when these findings are the result of work by foreign teams, and systematic publications have to be made in Arabic. Silencing decades of Western scientific speculation over the pharaoh's death, Hawass was handing back to the Egyptians the right to talk on their own about their own history.

However, the "truth" about the pharaoh's death was not that easy to repatriate. In an overwhelmingly Muslim country, the cosmology and polytheism of ancient Egypt have constituted serious obstacles to the celebration of the past. But the main difficulty was the strong popular reluctance to submit Tutankhamen's mummy to DNA analysis. Hawass had to proceed with great care. That is why he created in April 2007 and June 2009 two laboratories dedicated to the study of ancient DNA led only by Egyptian researchers. The fear was that DNA samples analyzed abroad by foreign scientists would be manipulated so that a Jewish origin could be attributed to the pharaohs. This fear was itself based on previous attempts by some Egyptologists to equate the figure of Akhenaton—promoter of a form of monotheism—with that of Moses.

This story is about a new balance of power where former colonized people repatriate not only mummies, artifacts, bones, and symbols but also the right to produce knowledge of their own about their own history and their own legacies. But the story also suggests something else, that this new distribution of scientific authority does not obliterate the relation between knowledge and power. This is particularly true for physical/biological anthropology. Given the profound engagement of the discipline with former colonial enterprises and with colonial systems, the issues of power raised now in a new rebalanced anthropology mirror those that guided the discipline earlier.

In the 2006 Wenner-Gren volume edited by Gustavo Ribeiro and Arturo Escobar and titled *World Anthropologies: Disciplinary Transformations in Systems of Power* (Ribeiro and Escobar 2006), the central question focused on the ways in which cultural anthropology was being challenged and reshaped by "transformations within systems of power." Our work at the meeting at Teresópolis raised related questions regarding physical/biological anthropology. In this essay, I will explore two propositions: first, that the question can usefully be reversed to examine how the anthropological field is itself a key transformer of those systems of power; and second, that the idea of "world anthropologies" presented in Ribeiro and Escobar (2006) can be challenged by expanding it to biological anthropology. In doing so, I suggest that the stimulating pluralization of scientific production can be combined with the (re)construction of a shared world anthropology.

From Raciology to New Physical/Biological Anthropology

In a famous article, Gayatri Chakravorty Spivak (1988) raises the question, can the subaltern speak? She studies the practice of sati—that is, when a widow immolates herself by publicly throwing herself on the funeral pyre of her husband in a ritualized way—in India's colonial history. While many na-

Jean-François Véran is Professor of Anthropology, Institute of Philosophy and Social Sciences, Federal University of Rio de Janeiro (Largo do São Francisco de Paula, 1, Centro, 20051-070, Rio de Janeiro, Brazil [jfveran@gmail.com]). This paper was submitted 27 X 10, accepted 19 VIII 11, and electronically published 11 XI 11.

 0011-3204/2012/53S5-0021$10.00. DOI: 10.1086/662384

tive, colonial, and scientific meanings have been produced to justify, condemn, or explain sati, Spivak shows that the women themselves have been denied the meaning of their own death. They remain voiceless. Subalternism is then conceptualized as the dialectic position of those reduced to a voiceless or silenced body.

The history of resistance to slavery has shown that one option left when one cannot speak is to deny others the use of one's body by committing suicide. An expression widely known in the Americas to refer to that practice was that the slaves "swallowed their tongues," and sometimes it was literally the case (Bammer 1994). Some would also consume clay and dirt, a practice historians have linked to West Africa, which could lead to disease and death.[1] Obviously, suicide was not the only form of resistance, and often, from Haiti to India, colonized people did find ways to raise a collective voice. The point is that from the French "Black Code" defining slaves as "furniture" to the Brazilian legislation defining them as "items of property which come in the order of goods, with no will nor legal personality," slavery has been probably the most radical of the many attempts through modern history to reduce people to bodies that cannot speak.

Physical anthropologists, among others, used these bodies, dead or alive, to produce the knowledge confirming that they were indeed inferior and primitive. That is, when the bodies finally spoke, it was through the voice of science, and this science confirmed that the beings that carried these bodies could not say much anyway. Physical anthropologists were what Donna Haraway (1988) has called the ventriloquists, the ones who made the mute and silenced bodies of those being controlled speak about natural order. Just as the non-Greeks were barbarians—as Lévi-Strauss (1987 [1952]) suggested, "barbarous etymologically refers to the confusion and inarticulation of birdsongs, opposed to the significant value of human language" (19)—so, too, centuries later physical anthropologists could scientifically explain why the primitives were these "runners behind in the field of civilization" described by Fouillée (1905). In a scientific domain obsessed by the "facial angle" (roughly the angle of the line from the nose to the forehead with the horizontal line formed by the jaw), de Gobineau (1853) situated Negroes between the whites and the monkeys. Based on the assumption that there was a narrow relationship between the development of intelligence and brain size, Paul Broca (1861) explained why some races were indeed inferior to others. Inspired by him, his followers tracked the precocious suture of the cranial box, a detail used to suggest that Negroes had a lower brain volume.

Eventually, eugenics would add its share to the demonstration of inferiority. The polygenic theory proposed that human races descend from different biological species. Certain that human ability was hereditary, Francis Galton (1869) invented and defended eugenics as a way to preserve and improve superior races. In sum, from anthropometry to serology and craniology to morphology, physical anthropology's early agenda reflected a hypothesis of global biological determinism—namely, race—that would explain origins, evolution, and even morals or cultural diversity. The quest for this biological determinism was of course rooted in the positivist episteme that hard sciences provided hard facts. The anthropologists were indeed committed to give evidence with a consistency proportional to the sophisticated anthropometric techniques they developed. However, raciology had a much stronger political force. As the Teresópolis Wenner-Gren conference showed in a clear-cut way, from Norway to Germany, Portugal to Brazil, and Japan to the United States, scientists have been required—and committed themselves—to conform to the political dreams of nation or empire. Anthropological evidence of purity could be used to suggest a glorious past. A nation's pure ascendancy could be invoked to explain ancestry, war, eugenics, or colonialism, and experts in physical anthropology were often caught in the tautology of confirming by nature what men were doing by politics. To some extent, one might wonder whether the spectacular shift within anthropology after World War II from biological to cultural determinism did not proceed from the symmetric projection of the same tautology. Anyway, let us remember that physical/biological anthropology does bear a heavy responsibility for having produced a raciology that scientifically endorses the division of humanity into biologically distinct varieties unequal in their cultural performance and profoundly alien to one another. In sum, it is through anthropology that dominated bodies produced racial narratives, that is, evidence of their own inferiority and the legitimacy that justified their domination.

Of course, biological anthropology has changed a great deal in recent decades. After the ideology of race fed into nationalism and imperialism, proving to be the most disastrous combination possible, and in the ruins of war, philosophy wondered whether man could still think after Auschwitz. It was a challenge that led anthropologists to rethink its paradigms for the study of man. Franz Boas's early-twentieth-century four-field approach—which brought together in a single frame physical anthropology, linguistics, archaeology, and cultural anthropology—already questioned the relevance of anthropometrics, stressed the weak heuristics of race or the dead ends of social Darwinism, and went beyond the predicaments of cultural evolution by theorizing "cultural relativism." Already in the thirties, Europe had gone through a similar transformation of the field, as the racial-typological approach lost ground both to a "new" physical/biological anthropology addressing the question of evolution in a more independent way and to social anthropology or ethnology. After World War II, the discipline was quite ready to welcome the evolutionary synthesis started in 1936 and first presented in 1942 by Julian Huxley in *Evolution: The Modern Synthesis*

1. The medical anthropologist Dennis Frate, while noting that the consumption of dirt can lead to disease, suggests that slaves were more likely to be dying of malnutrition (http://www.nytimes.com/1984/02/13/us/southern-practice-of-eating-dirt-shows-signs-of-waning.html).

(1942). It is noticeable that in over 400 pages, Huxley's book does not include or mention anthropology's relevance to the field of evolution. Not even once is Franz Boas mentioned. Huxley's synthesis does stress the importance of field studies with real human populations, and his agenda clearly calls for a unification of several branches of biology, including genetics, cytology, botany, morphology, ecology, and paleontology. But no mention is made whatsoever of anthropology and its anthropometry or craniology, these scientific practices being clearly relegated to the prehistory of science. Nevertheless, the evolutionary synthesis became a foundational intellectual event for the "new physical anthropology" as proposed by Sherwood Washburn in his 1951 report (Washburn 1951). The discipline "must change its ways of doing things to conform with the implications of modern evolutionary theory" (Washburn 1951:303), Washburn proposed. And so it did in many ways, from Washburn's functional adaptive complex to modern genetics and human biology to molecular anthropology. The discipline is now committed to strong ethical codes (see Turner 2012) and is all the more eager to claim its ideological independence, given that its epistemic legitimacy has suffered so much from political involvement in the past.

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The eagerness to defend and give actual consistency to the old/new frontier has had consequences for the discipline itself. Deeply concerned to distance itself from its dubious past and regain its epistemic legitimacy, biological anthropology has been tempted to promptly bury in the collective fosse of "bad science" those of its ancestors that have become undesirable. But the voices of the former subalterns, claiming their own ancestors whose remains were stored as museum collections or objects of scientific study, keep the old anthropology in play. It has been impossible to bury this past, and it has become obvious that in spite of claims about its scientific irrelevance, the heritage of raciology cannot simply be dismissed, at least in its political consequences and continuities.

This brings us back to the Tutankhamen story and more globally to the repatriation issue that has reconfigured many constituted collections. As shown by Ribeiro and Escobar, a new balance of power is being produced, and the "hard evidences" of yesterday—the bones, skulls, and blood samples—are perceived in ways that mirror the political and colonial dynamics that were implicated in their original collection in the field. Each skull, each bone "coming home" has a forceful symbolic efficiency. Using Bruno Latour's (1999) expression, they constitute a "parliament of things" redefining ethics, rules of power, and even laws of empowerment.

This is what the Native American Graves Protection and Repatriation Act of 1990 is about, as the American Indians and Native Alaskans are recognized as having "the right to determine the destiny of their ancestral remains." The National Museum of Australia has responded to similar legislation, passed in 1996, involving the ancestral human remains and secret and sacred objects that have been returned to Aboriginal and Torres Strait Islander people. New Zealand has a repatriation program called Karanga Aotearoa, literally "the call from homeland." From Indonesia to Mexico, Peru to Norway, and Paris to Bamako, repatriation issues multiply and bring unrest to the well-ordered universe of museums, biobanks, and archives.

This "call from homeland" cannot of course be reduced to an issue of power politics. That would be disregarding the deep identity, symbolic, and moral issues involved as much as the genuine concerns for a more ethical science. But it is at least as much about the situation and the process as it is about the objects themselves. The meanings of the remains might sometimes (unsystematically) reflect local cosmologies or myths or a traditional order. They are, however, always resignified dialectically and densified until saturation, with issues of recognition, legitimacy, powers of all kind, and even sometimes instrumental reason and "business as usual." This is why in spite of some museums' strong voluntarism and commitment to repatriation, tensions and conflicts still eventually arise. This is what happened, for example, with the repatriation by the Smithsonian Institute of the brain of Ishi, known as the last Yahi Indian. Facing critics for alleged insensitivity and slow process, the Smithsonian had to defend itself and prove its goodwill. This is also what occurred with the Kennewick Man case, where a U.S. federal court had to settle that the remains would be handed over to scientists before their restitution: some communities have not hesitated to push their rights and advantage in cases with rather improbable kinship and to provoke a clash with a sometimes skeptical scientific community. That is, in a very Maussian perspective, an object restituted too easily looses its transactional value and unloads the eventually useful political charge it contains. Ethical voluntarism can indeed be perceived as a new form of paternalism depriving the groups involved of a fair and perhaps desired (necessary?) confrontation. Political correctness is sometimes regarded as counterproductive to the outbursts of a necessary conflict. This dynamic configures some kind of a paradox. On the one hand, the scientific community has never been so committed to ethically aware practices, as a guarantee that past mistakes can finally be left behind, the pages turned, the book closed. On the other hand, some ethnic groups have never been so eager to keep the book open and the pages visible, for this is exactly where the margins of negotiation are drawn and recorded at present.

This redefining balance of power brings turmoil within anthropology itself as a discipline. The question goes way beyond the loss of collections and the difficulties of getting access to new data. Dislocating collections inevitably brings back the question of how they were constituted and what they were constituted for. It is memory work. Let us recall that sepultures of the Sami people were systematically collected in Norway for museographic collections until the 1920s as part of a search for a specific racial theory (Kyllingstad 2012). Indeed, yesterday's theories have been criticized, and the collections, when not considered useless for modern research, have been resignified. But the collections are still there in one way or another, and they are not "just" still there. Every single collection is the empirical version of a categorical theory. Even kept as the historic testimony of an erroneous past, they still at the very least rely on the implicit epistemic that things can and must be ordered, or better, reordered, and this time in the right way. As Jonathan Friedman (2008:32) remembered, "the basic idea of museum is to impose an order to the world, the question is the one of the limits of this world." These limits are precisely what is being challenged. As the paper of Ann Kakaliouras (2012) in this volume nicely shows, repatriation in itself is limited in transforming anthropology's relationship to indigenous people because of its object-oriented ontological premises. That is, even with the strongest ethical care and deontological commitment, biological anthropology still aims at an ever more problematic objectification of man. Indeed, it is not one or another specific anthropological object, method, or theory that is being challenged here. It is the very idea that there is a logic of the *anthropos* and that it is reducible to scientific objectification. This is what Samuel Sidibé (2008:39), director of the National Museum of Mali, expresses: "Today, the question must be posed this way: how observed societies have the right to intervene in how they are observed. . . . I think this position of observing others is no longer acceptable."

The sentence "observing others is no longer acceptable" captures the vertiginous challenge that anthropology has been facing over the last 20 years. "Observing others" was exactly the core project of the discipline. Concretely, every single anthropologist, physical/biological alike, knows exactly what this means: access to the field is from now on negotiated; it is submitted to all kind of restrictions, legal or informal; it is instrumentalized in all possible ways; and collections are repatriated piece by piece or are impossible to constitute. Scientists travel the world, not in search of the perfect "case" study but of the mildest legislative regulations and the easiest access to data. The data produced by the researcher now have to compete with all sorts of other narratives, such as "oral memories," not to mention "self" or "organic" anthropologies. Indeed, in this context, just "observing" is impossible, and the "others" of the Malinowskian golden ages do not exist anymore as such. Have they ever existed?

Of course, these multiple resistances come from emerging subjects willing to dismiss the "white masks" (Fanon 1952), the "orientalisms" (Saïd 1978), the "ideology of tribalism" (Mafeje 1970), the "ethnophilosophies" (Houtondji 1977), and the other "invention of Africa" (Mudimbe 1988) that past anthropologies have forced on them. Concomitantly, competing perceptions and readings of the world, once considered mere superstitions, are now rehabilitated and equated in value to occidental science by postmodern multiculturalism. Inspired by the poststructuralist call for a necessary deconstruction of logocentrism (Derrida 1979), let us remember here the vast and fascinating debates in postcolonial literature on the existence of a specific African scientificity (Diop 1960), rationality, or philosophy that would be based on alternative epistemic premises. Indeed, reluctance to engage in academic objectification is not only a claim for irreducibility. It expresses sometimes the need to "provincialize Europe" (Chakrabarty 2000) and occidental science and to locate it thoroughly among other systems of access to reality.

Through the negotiation of "old bones," new powers are emerging exactly where they were once denied. Yesterday's dominated were reduced to a silenced body. It is notably through the remains of these bodies that their descendants gain voice today. The symbolic continuum, indeed, is very powerful. The restitution controversies now question the limits of these new powers. Are the epistemic foundations of physical/biological anthropology soluble in the existential, symbolic, and political resistance to objectification? In a provocative way, the question would then be, can physical/biological anthropology still speak? A quick first answer is that in spite of the "revolution of the observed," the anthropological discourse is still very present outside the academic field, in society, within public policies, and even on key geopolitical issues.

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Our thesis is that if access to fieldwork and data has become so tenuous, it is not only for self-affirmation and ethical reasons but also above all because anthropology as a whole is just as power related as it was before. As Amselle (2001) showed in a study on Africa, anthropologists were historically so close to the colonial administration that they were sometimes the same individuals, doing the dual work of "recognizing" one or other supposed ethnic groups and then specifying the administrative frontiers and rules by which these supposed ethnic groups were to be incorporated in the empire. In Brazil, today's anthropologists recognize on a monthly basis the ethnogenesis of new indigenous people or "discover" new *quilombos* (runaway slave communities' descendants) and are mandated by state agencies to delimit legally their territory (Véran 2003). Yesterday, "racial" and ethnic differences were the main justifications for domination, special rights, and unequal treatment. Today, these same differences are celebrated as necessary for "ethnodiversity" in a globalized world, and they give access to special rights and differentiated treatment. What was discriminated against yesterday is "affirmed" today, but in both cases, whether biological or cultural, the anthropologist is a key actor. Just as in the past, he or she often appears as the interpreter of symbolic orders proving the "authenticity" of an indigenous claim, the truth teller about a grave or a bone, the expert at delimiting a territory, the certificate giver of autochthony or kinship. In Latin America, "anthropological expertise" has become the greatest counterpower for agribusiness, real estate projects, and dam construction to a point that some anthropologists have been offered payments to write counteranthropological reports that would conclude that such or such community is not indigenous or *quilombola*. *Istoé* (2008), a Brazilian weekly magazine, has gone so far as to denounce the "anthropological piracy" of national land. A joke circulates in Latin America that the economists once were in power, but they lost it to the anthropologists. Finally, let us remember in the U.S. context the Human Terrain System, which embeds anthropologists with combat brigades in Iraq. The idea is to produce a "Human Terrain Mapping" system to understand the local populations. Indeed, in a changing balance of power, anthropology might be challenged on its epistemic ground, but it is definitely reaffirmed in its historical power of speaking of the others—looking at them—with all political consequences attached.

This observation sheds a different light on why so many power issues are being built through negotiating access to the field and repatriation of remains. We shall now examine three elements in order to try to understand this continuity of the power of anthropology itself.

Objectively, let us remember that the paradigms, objects, and methods of physical/biological anthropology might have changed, but the discipline's central questions still focus on human origins, evolution, and human variation. These questions were at the very core of nation building in the nineteenth and twentieth centuries, when the "imagined communities" of nations were configured through ideas about blood, race, and destiny (Anderson 1991 [1983]). In a world of changing frontiers and powers, new forms of political organization crave answers to the same questions. At least from a narrative and ideological perspective, origin, ethnic difference, and cultural specificity are the exact categories required for political recognition of an "ethnic," "autochthonous," or "traditional" community/people/group. If I may be provocative, it is puzzling how in scaling down from nations to ethnic groups, modern anthropology has reshaped the discourse: ideology has become "myth of origins" or "system of belief"; race shifted to "ethnicity" or "population"; political separatism is now "preservation of diversity"; and domination, hierarchy, and hegemony have gained a semantic lifting as they became "counteracculturative" forms of resistance in a globalized world. From the land reform of South Africa to the one of Mozambique, from the redefinition of citizenship in Cameroon to that of Ivory Coast, from "rebecoming Indian" to "rebecoming African" in the Americas, groups of all kinds in the world get recognition by "self-affirmation" of an ontology, a distinction, and a destiny. That is, the fundamental anthropological question still holds as the key issue for the social and political organization of man. From the nineteenth century to the twenty-first century, there is of course a difference in scale. Against the excesses of nations and nationalisms are now promoted "minority," "autochthonous," "traditional," and "ethnic" groups of all kinds. But to use Lévi-Strauss's (1991:134) structuralist apparatus, this movement from majority ruling to minority rights configures a transformation group involuting around the same structural invariants: ontology, race, and destiny. Of course, this transformation group has also gained a new polarity: the justifications for yesterday's domination (−) has now become the legitimacy for democracy (+). The question is, if, as we know, the anthropological discourse on race and nation was ideologically saturated yesterday, how is it today on ethnicity and multiculturalism? I will suggest later that biological and cultural anthropology have dealt with that question in very different ways. Let us for now register that anthropology does speak a lot to the new balance of power builders.

The second argument to account for the persisting power of the discipline consists of inverting the restitution issue. It is not only about what anthropology took from "them" but also about what "they" took from both physical and cultural anthropology. The discipline indeed revoked some categories, resemanticized others, and eventually shifted from biological to cultural variables. But these categories have crossed over from the academic field and keep on signifying and organizing perception and reality outside the revised anthropological world. The strongest example is race. Based on modern genetics, there is (almost) an academic agreement now that the category is not heuristic to account for human variability. Still, race is overwhelmingly present as a popular belief, still produces racism, organizes state statistics as it does in the United States, and even determines public policies.

This "persistence of race" (Fry 2005) is not an atavism of the past. The category is on the contrary gaining force and influence at (re)organizing social conflict, politics, and policies in the globalized world. It is rather symptomatic that social scientists in twenty-first-century France, where the use of racial category is prohibited by the first article of the constitution, now question whether or not there is a shift "from the social question to the racial question" (Fassin and Fassin 2009).[2] At the same moment, Brazilian intellectuals denounce the "racialization" of the state and the "dangerous divisions" it produces (Fry and Maggie 2007), while others successfully promote a "status for racial equality."[3] Indeed, the political uses of race are usually justified in the postmodern way indicated by the social sciences: race is a social construction, exists as such, and produces real consequences that require pragmatic solutions. However, it is important to remember with Bourdieu (1980) not to confound the theoretical reason with the practical sense. Concretely, this means that it would be a major sociologism to conclude that race is really perceived as a social construction in its popular uses. Race is rather a synthesizer combining in multiple and changing ways biological and cultural elements to qualify human differences. As such, race does involve some kind of popular comparative

2. The thesis is that social conflictuality in contemporary France is less organized along the class line and more along the racial/ethnic line.

3. For details, see http://www.cedine.rj.gov.br/legisla/federais/Estatuto_da_Igualdade_Racial_Novo.pdf.

anatomy and genetics closer to the "old" than to the "new" physical anthropology. As shown by Peter Wade, it still contains notions of blood, sperm, and corporal substances in a somewhat naturalized conception, and it is not consistent with what social scientists objectify as "social construction" (Wade 2002).

"Ethnicity" is another notable example. Invented as a tool for describing a specific form of social organization, the concept has gone through the same deconstruction process, from early essentialism to perspectivism, as did "race" within the academic field (Barth 1969). However, in the meantime, and just like race, "ethnicity" has gone through a large popularization process to such an extent that it is now a native category. In this process, it is clear that it has regained the substantialism that Frederik Barth so strongly argued against. The same demonstration could be done with "symbol," "identity," "myth," and even "culture."

The debate is to what extent the native uses of anthropological categories relate to a "self-anthropology," "strategic essentialism" (Spivak 1987), the ultimate conquest of reflexive modernity, a form of counteracculturative resistance, or whichever combination. Of course the local uses of anthropology are made through adaptation, transformation, and resignification. However, the self-revindicated and promoted "traditional peoples" tend to have a more traditional conception of anthropology than current anthropology itself. One reason is that their recognition is conditioned to the perfect matching with the criteria produced by legislators, themselves embedded in a rather essentialist conception of biocultural difference. Anthropology has changed, but neither the locals nor the NGOs nor the political institutions really care. The Indians have to be Indians, and the anthropologists have to be the kind of anthropologist who certifies how genuinely Indian they are. Rather ironically, the natives have become the guardians, the memory holders, of the solid categories by which they were recognized and controlled in the first place. They hold a mirror in which most anthropologists do not recognize their discipline anymore but are still asked to recognize how native the natives are. This is why so many young anthropologists who participate in land demarcations or provide expertise on the indigenous, autochthonous, or traditional communities have this strong sensation of discomfort: they know how reified and instrumentalized is the material they have to work with. But they still do the job, for they know that as long as the natives are stuck in political otherness in terms of land access and rights, anthropology is stuck with its old categories. The anthropologists, too, are traveling back with the bones and restituted remains.

In sum, the persisting power of anthropology also derives from this strange and paradoxical dialogue between its analytical categories and their popularization and its past and its present. In an international context where "diversity" has become the key paradigm for power redistribution and legitimation, the world has never been so anthropological. After centuries of domination, in a context that finally recognizes the value of minorities and traditional peoples, who wants to hear that tribes were partly a colonial invention (Amselle 2001), that race does not exist, and that there is no such thing as a substantial identity? The globalized world needs to be reenchanted, and anthropologists are called to be among the first reenchanters. However, even adapted to modernity and multicultural democracy, even legitimized by the pragmatism of reverse discrimination, categories such as "race" or "ethnic" do carry, as Ricardo Ventura Santos has nicely remarked in the Teresópolis conference, the signatures of "old" anthropology. Signatures cannot be revoked. They empower. They also create responsibility.

The third argument to account for the persisting power of the discipline is more specific to biological anthropology, though not exclusively. The negotiating force of the natives seems directly proportional to the substantiality of the attributes they bring into the negotiation. The reason is that, as we know, new powers imply the production of a new symbolic order. By definition, a symbol requires a materiality to carry its immateriality. So the new anthropological world mentioned above requires an intense process of materialization. As already mentioned, bones and remains constitute powerful materialities when the important issue of ancestry is politically linked to rights and property. The notion of "immaterial patrimony," now present in many constitutions worldwide, has been so forceful because it ends by bringing some kind of materiality—at least a certificate—allowing the symbolic empowerment of daily life practices (e.g., a cooking recipe, a traditional remedy). The "problem" with the antireification posture of modern anthropology is that it offers very little grounds for materialization. How does one build power with the idea that identity is relational and situational? How does one delimitate a territory with a deconstructive conception of ethnicity? The misunderstanding can be deep, between the "soft" conception of anthropology as mere interpretative discourse and the natives' need for hard facts providing convincing arguments for rights access.

That is why biological anthropology eventually appears to be more resourceful than, say, the situational concept of "self-affirmation." Moreover, it is precisely because the interpretative shift of cultural anthropology led to a critique of substantialism that biological anthropology is sometimes used by ethnic/racial activists as a counterargument to redensify the categories and the discourse. By this logic, a bone is a bone. The harder the evidence, the stronger the arguments. Paul Gilroy tells how, facing the growing resemanticization of race from biology to social construction, militants borrowed from early physical anthropology the thesis of a biological determinism of culture (Gilroy 2000). Melanin was presented as the key explainer for an African sense of rhythm. Diseases prevalent in black populations, such as falciform anemia, were held as proof both for a racial differentialism and a racist discrimination in health access. In Brazil, DNA sequencing has turned out to be a powerful element in the debate over race. It is used by the geneticists who fight against racial laws to prove the biological nonexistence of race. But it is also used to sustain the classical *mestiça* (mixed-race) thesis of the Brazilian people. BBC Brazil organized a large DNA testing of famous Brazilian black personalities. A polemic arose when a young Olympic sportswoman was identified as over 50% white in her genetic origins. In the meantime, black activists make intense use of DNA testing as a way to reconnect genetic origins with a cultural matrix and social roots. A "solid" argument was furnished by mitochondrial DNA sequencing of a representative sample of the population: because in Brazil the average mitochondrial DNA is predominantly of African origins, this would prove that intermixing originated from the large-scale rape of the African woman.

The power of biological anthropology relies on its capacity to provide hard evidence in key contemporary political issues such as origins, ancestry, anteriority, restitution claims, objective biological differentiations, or the impact of racism on human growth. This power is all the stronger in that it is reversible. It can potentially play against the natives' narratives or claims for anteriority and restitution. The popular fear for the misuses of Tutankhamen's DNA is directly proportional to the power that genetics is believed to have in "proving" ontologies against mythologies. What if Tutankhamen's DNA revealed Jewish origins? What if a local myth of origins does not matches the scientific mapping of migratory fluxes? What if "they" decode Jesus? What if the discovery of a new skull challenges the idea of African origins of man? Precisely at a moment when former subalterns are entitled and encouraged to speak on behalf of themselves and tell about their origins, ancestors, and differences, DNA might silence them again by establishing a harder truth on top of native representations. Of course, from a sociological perspective, social reality and scientific truth are two ways to access reality. They can interact and cross, but they keep a relative independence one from another. DNA has no immediate or self-evident meaning and has proved to be rather tautological in its political uses. That is, like ideas about race and biological difference in the nineteenth century, it can be mobilized as a certifier for a specific ideology and ignored when it plays against it. However, in its social uses, biological anthropology does appear as a way to counterbalance the mainstream perspectivism of cultural anthropology. It is hard bones against floating concepts. A new biopower appears all the more potentially strong in that it relies on negotiated (unquestionable) ethics, independence, and accuracy.

I would suggest that biological anthropology is caught in a paradox. As the former subalterns speak, the epistemic foundations of object-oriented science are often denounced as no longer "acceptable." But this rejection is less on behalf of alternative epistemologies than on behalf of an anthropological world in which scientific objects are still also political subjects in quest of emancipation. Because of this lasting political density, it would be counterproductive to call off the agenda and dismiss the field. Indeed, biological anthropology is not soluble in some tropical sadness of natives' transformation into postmoderns or in a metropolitan blues of collections going home, marking the end of an adventure. The fact is that Indians, ethnics, autochthonous themselves, have proved not to be soluble in late modernity. On the contrary, from ethnogenesis to domestic ethnicities, from new racializations to the mainstream celebration of diversity, the twenty-first century is turning out to be a rather ethnic century. In this multiculturalist world, new actors multiply on behalf of claimed differences often presented as ontological, ancestral, or substantial.

If it is true that physical anthropology got caught yesterday in the political excesses of raciology, the question is now to what extent the sacralization of diversities of all kinds does not fall into the political excesses of culturalism. The conflicts were once national; they are now "cultural," "between civilizations," or "ethnic." But just as in the past, facing a growing competition of identities and political claims, groups seek solid frontiers to secure their space. That is where biological anthropology might be brought back to its original premises. Facing the resurging quests for biological proofs of political ideologies, the field is forced to question its old/new frontier. There is no doubt that outside the small circle of its community, biological anthropology is still perceived in the strict continuum of its past, at least in terms of power. After all, has the episteme changed that much? The discipline might dismiss/repatriate old collections, but it still constitutes new collections based on new categorical theories. The tension is blatant: new powers are emerging through the negotiation of old bones, but old powers are being confirmed by the constitution of new collections and technical devices. The fact that these new collections are ethically inflected, co-constituted, geographically bounded to their sites of origin, or shared does not change the fact that power is still associated with the process of classification, collection, and storage. My claim is not about assuming a never-ending past. It is about resisting the political balances and rebalances, uses and abuses, and resolving whether or not there is still an *anthropos*, a *logos*, and an anthropology.

Toward a World-Reunified Anthropology?

The argument so far can be reduced to a simple proposition: thinking from Ribeiro and Escobar's conceptualization of "world anthropologies," it is not only about disciplinary transformations within systems of power but also about how the world's rebalancing of the discipline contributes to the transformations of the systems of power themselves. Precisely whether within its classical academic bounds or exploring a-centered rhizomic reterritorializations, this is once more an anthropological world where the legitimacy for political organization is still sought in some kind of *anthropos*'s logic: a biocultural determinism, a code, an ontology or origin, a destiny or an evolution, a race, regardless of how it is resemanticized by political correctness. That is what the worldwide diffusion of identity or ethnoracial politics expresses;

even though looking bottom-up, politicized ethnicities are more a means for basic rights and access than a fight for identity itself. Let us in fact remember here that traditionalism has indeed little to do with tradition (Bouju 1996) and that the consciousness of identity is not exactly identity anymore (Béji 1997). Anyway, in this dynamic, yesterday's social Darwinism is now replaced by new mutations produced by the same analogy between nature and culture: symptomatically, because biodiversity was proven to be the key to life, then ethnodiversity appears now to be the key to human organization. So multiculturalism has to be the definite answer to mature democracy. This is not to neglect the importance of current works on biocultural coevolution. I am just suggesting that political communities may still be objectively "imagined," but they still do try to "prove themselves" on solid substantial elements produced by whichever combined culturalizations of nature and naturalizations of culture. So, in a pluralized, multifocal, "diverse," emerging world, anthropology as the science of man's diversity is definitely called to be a witness, a provider of proof and evidence. And anywhere in the world there will be anthropologists committed to situational substantialism, if not out of theoretical convictions at least on behalf of political pragmatism. As a famous Brazilian anthropologist told me, "the day has not come when a committed anthropologist will refuse to attribute a certificate of indigeneity."

Finally, Ribeiro and Escobar (2006) are perfectly right when they call for "pluralizing the existing visions of anthropology" (6), as long as we do not fall into a somewhat naive—or optimistic—spatial determinism. If we do rehistoricize the field, as they rightfully suggest, we will surely find out that the decentering process of anthropology did not systematically generate plural anthropologies. On the contrary, in spite of the multiple postcolonial explorations of what could be alternative epistemologies, the results have sometimes been the reaffirmation of the most classical and substantial premises of the field. "Identity" and "otherness," even from multiple locations, are still mostly captured within what Derrida (1978 [1966]) called "the determination of being as presence" (278) and still saturated by the obsession of ontology. Likewise, I can surely agree on the necessity to "provincialize" Europe as long as the old ideas of "center" and "capital" are not imbedded within the concept. And let us not be disingenuous. It is not only about an everlasting "coloniality" that would impeach the former subalterns to really speak on their own. It is also because new powers are being negotiated through old bones along with old concepts from old anthropology.

What I am trying to suggest is that the biggest task of the field is, within its ethic-aware scientific practice, to fully and appropriately address—this time—the political uses and misuses of the concepts it has contributed to create. Obviously, this agenda is nothing new. Biological and cultural anthropology have been dealing with it for the past three decades, but in two different ways. Cultural anthropology has gone through a steady epistemic crisis deriving from a vast deconstruction of the field's own ethnocentrism. This crisis has been partially resolved by integrating within anthropology itself the relativist matrix that the discipline applied so far to the natives and by making perspectivism a "native" category of the field itself. A radical conclusion would be (and sometimes is) that as there is a diversity of cultures, there is a diversity of anthropologies. The "interpretative shift," for example, helpfully downgrades the epistemic status of ethnological narrative by rereading it as a metanarrative. But to what extent would a radical metainterpretation of anthropology lead to misinterpreting the political claims for pluralism? Are not these claims co-constructed by anthropologists inclusive? And therefore do not they fully integrate interpretation in the building process of shared situations and meanings? The "Interstitial Perspective" (Bhabha 1994) of post-postcolonialist anthropology is everything but a radical perspectivism and rather calls for a shared "hybrid" epistemology than for fragmentation. From the Council for the Development of Social Science Research in Africa work exploring hybrid and new forms of cosmopolitism between classical universalism and relativism (Hountondji 2007) to the new generation of Indian's subaltern studies, the former "subalterns" tend to indicate that relativist essentialism is being left behind, if not in the political world at least in parts of the academy. As raised by Achille Mbembe (1999), the point now is to address the challenge of internationalization by "getting out of the ghetto." But again, is mainstream anthropology hearing them all the way? Cultural and minority studies confound at times the necessary pluralization of the objects with a—so far—merely rhetorical pluralization of objectification itself. By doing so, they fall into some kind of "anthropologism," confusing analytical categories with substantial reality. If it is true that yesterday's ethnology "invented" African tribalism, to what extend is some of today's ethnology not inventing modern ethnicities? And after the colonial anthropology of tribalism, does the field also have to invent the tribalization of anthropology as a ritual of affliction to pay off its political debts and deal with its bad consciousness? As a result, most new generations of students are "lost in translation," seem to confuse different points of view with different views of the point, and wonder whether anthropology still has an object. The problem is that while cultural anthropology indigenizes relativism, the former natives indigenize academic essentialism. In a world where bones and identities are politically saturated, my suggestion is that responsibility begins with epistemic steadiness.

Epistemic steadiness is exactly what is claimed by "new" physical anthropology/biological anthropology. Since Washburn's manifesto, the field seems to escape from the crises that roiled its cultural counterpart. Indeed, most of the discussants at the Wenner-Gren conference seemed to agree that there was no crisis at all. As a fact, looking at numbers, biological anthropology appears dynamic, prospective, and ever more collaborative within multidisciplinary contexts not to mention its prominent role in the infatuation with DNA talk and mass-market genomic testing. The question is, as the restitution issue is now making manifest, how much longer can the field pretend to respond to political claims by technical and ethical concerns? Could we be witnessing a kind of shell game where ever more sophisticated technical norms are being constructed as an answer to ever more politicized claims?

I shall argue that by not fully confronting issues of power, biological anthropology is at risk of a multiform new positivism. Indeed, many biological anthropologists have moved resolutely toward the "hard" sciences of the biological laboratory, where the agenda of anthropology as a whole can seem remote and even irrelevant, and the new positivism can facilitate that distance. Past mistakes, from this perspective, can be addressed through technical details that finally establish the real truth. The vague and uninterrogated suggestion that biological anthropology has been "wrong for so long that we're now well situated to know that we're right" is politically convenient in some ways and scientifically comforting. The new positivism might also take the form of policing the boundaries: some kinds of anthropology are one side of the line, and others are safely in the "hard" sciences, where epistemological status is not ambiguous. This new positivism sets up a circumstance in which biological anthropologists can disregard the current turmoil in social anthropology. The debates of those concerned with politics and power and with the sociopolitical embeddedness of science production and uses can be tuned out in the laboratory. A third form of neopositivism would consist in "hiding" behind the pragmatism of human biology, claiming the status of an exclusively applied practice, and therefore forgetting to scale up to the *anthropos* question. In this construction, biological anthropology is biology by another name. This pragmatic biological anthropology would be deviant in Merton's sense of ritualism (Merton 1968 [1949]), which involves strict respect for the scientific means of knowledge production with a simultaneous failure to address the larger questions of the field. There are, then, many ways that biological anthropology can turn to science and practical results in order to turn away from and avoid the crisis.

Conclusion

In sum, the power issue ends up bringing cultural and biological anthropology together in a dialectic tension that is part of the performance of a global irresolution on the epistemic status of the discipline. In the new multiculturalist order, cultural anthropology is so eager to avoid all accusations of positivism that it risks renouncing any claims to explain anything. Symmetrically (and the symmetry here is not trivial), biological anthropology is so willing to build positively on technical capabilities that it risks reperforming the past mistakes as it focuses on bypassing the risk of positivism.

Whether by default or by excess, this epistemic irresolution has a consequence. The field is animated by political agendas just when it should be possible finally to learn from the past centuries that these political agendas are a central part of its original sin. That the political agenda is now committed to pluralist democracy and minority right is less relevant than it might seem. The argument is reversible: if "race," "ethnicity," and "identity" can be "strategic" for pluralizing powers in a democratic frame, they also feed all sorts of fundamentalisms and "tyrannies of identities" (Béji 1997). Indeed, in the "ethnic market," many competing local groups take from the "soft" situational conception of ethnicity the notion that "auto-recognition," "auto-affirmation," and even "self-esteem" are worthwhile community projects. Then, they take from the "hard" evidences of bones, skin, and blood the "proofs" to reconstitute an essentialist backbone. So the questions are, is anthropology soluble in the political urges for multiculturalism, and has not "cognitive pluralism" always been constitutive of the discipline? Or do we really need to proclaim along with the necessary dissemination of knowledge production that knowledge is only relatively and situationally acknowledgeable? Is not relativity even admitted within "hard" sciences?

This irresolution needs to be faced. But for that to happen, the two branches of the field need to face each other in a more systematic and integrative way. "Useless intellectualism" on one side, "heresy" on the other: is it not true that images of reciprocal representation are not always flattering? Getting rid of clichés, we might find that cultural anthropology can deal with its issues by actively helping biological anthropology resolve its own. For example, social anthropology as a discipline can undertake the work of showing, even in the "new" scientific order, that a bone is not exactly just a bone. It can participate in the work of remembering that classifications and categories are "moods" and "tones" (in the words of the nineteenth-century semiotician Charles S. Pierce) rather than the products of objective observation alone. It can play an active role in guiding political interpretations and social uses of technical data and in making it clear that DNA ancestrality does not "prove" any identity and does not account for human cultural diversity. Symmetrically, biological anthropology can help mitigate the excesses of culturalism and relativism by demonstrating what Pálsson (2012) called the "biosocial relations of production" and holding tight to the irreducibility of human evolution to cultural revolutions. Human biology is a fundamental resource for the demonstration that the political ideology of race and all sorts of social essentialisms produce real biological consequences on human growth and health, as in Noel Cameron's project in South Africa. Finally, biological anthropologists can become a part of dissolving the obsession of ontology in the biological reality of global dissemination.

Finally, if I were to point at a message that a more coherent and steady anthropological discipline could in theory deliver, it is precisely that its scientific question does not necessarily bear any political signification. The questions of origin, evolution, and variation of the human species, biologically and culturally, the *anthropos*-logic, is indeed fascinating and scientifically fundamental. But they do not imply that the sociopolitical organization of man is or should be determined by skin tones or by cultural moods.

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Studying Mandela's Children: Human Biology in Post-Apartheid South Africa

An Interview with Noel Cameron

by Joanna Radin and Noel Cameron

In this interview, human biologist Noel Cameron reflects on his work on child growth and development in post-apartheid South Africa. The conversation focuses in particular on Cameron's involvement with a cohort study called Birth to Twenty, which sought to determine the health impacts of apartheid on black children born in the year Nelson Mandela became president. Cameron considers the extent to which human population biology can contribute to the creation of new and potentially improved health realities for marginalized communities in the Global South.

For over 20 years, human biologist Noel Cameron has studied child growth and development in South Africa. He coordinated an ambitious longitudinal study now known as Birth to Twenty, which followed a cohort of children born around the time Nelson Mandela was elected president (fig. 1). During the Wenner-Gren symposium "The Biological Anthropology of Modern Human Populations: World Histories, National Styles, and International Networks"—organized by Susan Lindee and Ricardo Ventura Santos and held in Teresópolis, Brazil, in 2010—Cameron's reflections on his involvement in this post-apartheid project highlighted the role that anthropological interventions can play in documenting the violence of oppressive regimes and providing information crucial for rectifying injustice. Most striking to workshop participants was his observation that while old anthropometric techniques had certainly been used to enforce racial segregation, those same techniques could be used to demonstrate how apartheid's consequences persisted in the bodies of black South African children. It was agreed that a focused discussion of Cameron's interventions in South Africa would be a dynamic complement to the international perspectives on biological anthropology included in this supplement. Joanna Radin, a doctoral candidate in history of science with interests in postwar human biology, conducted the interview on April 21, 2010, at Princeton University, where Cameron was then visiting as professor of public and international affairs.

Joanna Radin is a Doctoral Candidate in History and Sociology of Science at the University of Pennsylvania (249 South 36th Street, Cohen Hall, Suite 303, Philadelphia, Pennsylvania 19104, U.S.A. [jradin@sas.upenn.edu]). **Noel Cameron** is Professor at the Human Biology Research Center, School of Sport, Exercise and Health Sciences, Loughborough University (Loughborough LE11 3TU, United Kingdom [n.cameron@lboro.ac.uk]). This paper was submitted 27 X 10, accepted 30 VIII 11, and electronically published 27 II 12.

Radin: Let's start with an intellectual biography: how did you become interested in doing human biology work?

Cameron: In 1967 I was doing an initial degree majoring in sports science. My second academic interest was biology, and at Loughborough University in the UK, where I was an undergraduate, I studied the relatively new subject of "human biology." Human biology covered a lot of areas that interested me: anatomy, physiology, and, in applied anatomy and physiology, the role of exercise. We also learned about human growth and development. When I finished that bachelor's degree in 1971, I wanted to continue studying human biology. So I applied for a masters degree that, uniquely, was being offered at Loughborough, and I was accepted to start in October 1972. My master's thesis investigated how 17-hydroxycorticosteroids were released when stressed by whole-body vibration, such as that experienced by astronauts—so nothing really to do with human growth and development. However, one day my supervisor, Dr. Peter Jones, asked if I wanted to work with Professor James Tanner in London, who was looking for a research assistant. It was a wonderful opportunity because I'd read all of Tanner's work, I'd studied skeletal maturity, and now I actually had the opportunity to work with *the* guru.

James Tanner is one of the world's leading experts on human growth and development. His name is synonymous with the assessment of human growth and puberty, which is assessed using the eponymous method called "Tanner Scales." His landmark publication was *Growth at Adolescence* (Tanner 1955), which for the first time synthesized the research undertaken, primarily in the United States, between 1920 and 1950 into a volume that dealt specifically with the biology of human growth during adolescence. By the early 1970s he was globally recognized as the European, if not the world, expert

 0011-3204/2012/53S5-0022$10.00. DOI: 10.1086/662573

Figure 1. Birth to Twenty participants waiting for their regular assessment in 1998. These assessments were made at school and involved both anthropometric measurement and questionnaires (copyright Noel Cameron).

on human growth. Tanner's Department of Growth and Development at London University's Institute of Child Health was a mecca for scientists and pediatricians who wished to study both the normal and abnormal growth of children.

Radin: What do you think drove you to want to pursue these interests? Was it your background as an athlete?

Cameron: I'd always had an interest in biology. I had a teacher in high school, Trefor Jones, who sparked my imagination both in terms of sport and science. He loved the fact that I was captain of the school rugby team on Saturday afternoon, was acting as a lead in the school play on Saturday evening, and was in his biology class on Monday morning—enjoying it all. It seemed very natural that I should continue with this link.

The Tanner connection arose out of that initial course of human biology. I guess there was a link between the fact that I was doing sports science and a lot of sports science is about physical activity in children. There was that interest in how kids get better in terms of doing exercise, and so human growth was quite involved with that. Looking back, I think there was a logical progression from doing sports science to doing a master's in human biology to joining Tanner's department to doing human growth and development work to doing what I do now.

Radin: In the introduction to your 2002 edited volume, *Human Growth and Development* [Cameron 2002], you referenced your desire to put theory into practice, a theme that emerged early in your career. I wanted to get you to talk about how you wound up in South Africa and how that was informed by this desire to put theory into practice.

Cameron: Working in James Tanner's lab was quite an experience. Nowadays we tend to work in labs that are fairly specific in terms of being primarily concerned with molecular, biological, behavioral, or clinical sciences. But Tanner's lab at that time was a collection of about 40 scientists who were looking at all of these aspects, at the whole spectrum of human growth and development.

It was a multifaceted department that exposed me to the full breadth of human growth from behavior to biochemistry to endocrinology to statistical analysis and the analysis of human variation to dealing with growth disorders. And I was allowed, and expected, to be involved in all aspects of it. The clinical work involved the analysis of the growth of children presenting at the growth disorder clinics. I would do a complete growth workup for Tanner and his clinical colleagues, and after they had arrived at a diagnosis and treatment was initiated, my job was to follow up with repeat assessments and predictions.

From 1976 to 1983 I was also teaching a course on human growth to biological anthropology students at Cambridge University and working in London at the Institute of Child Health with scientists, clinicians, and community health workers in child health from developing countries. It was wonderful exposure for me to work with these scientists from all over the world. I would talk to them about how to set up growth studies and nutrition studies in their countries, which were mostly developing countries, and I hadn't been to one (apart from Wales!).

After completing my PhD in 1977, I stayed with Tanner for another 6 years. I decided then that I knew a lot of the theory and some of the practice, particularly with regard to growth disorders, but here I was telling people how to do growth studies in South Africa, or wherever, and the basis of my advice was almost all theoretical.

I had often talked about putting my knowledge where my mouth was; to actually work in a developing country and find out firsthand what it was like to undertake research in these circumstances. I was therefore looking for a country that would provide that experience, and for a whole variety of reasons I ended up in South Africa.

Radin: Can you enumerate a few of those reasons, at least what you think the most important ones were?

Cameron: I hadn't been thinking of South Africa as a destination. But a position opened with Phillip Tobias, who is a famous biological anthropologist. He was the successor to Raymond Dart, who had become internationally renowned in the 1920s for finding the Taung child—the fossil of *Australopithecus africanus* that became known as "the missing link." Tobias had been a student of Dart and eventually became his successor as head of the Department of Anatomy at the University of Witwatersrand in Johannesburg. Phillip Tobias had developed an undergraduate course in human biology and wanted a human biologist to take it over. Given the absence of suitably qualified people in South Africa, he had contacted his colleagues internationally and thus spread his search to Europe. Tanner maintained that I was the "first human biologist" because I was the first student that had come through my graduate studies with him as a dedicated human biologist.

But of course this was a time of high apartheid, and I thought, what's being done with blacks in South Africa? Probably very little, but let me find out. So I started reading around, and indeed there had been almost a total absence of work on black children in South Africa. Blacks in South Africa had suffered from 40-odd years of legalized segregation. It was a story waiting to be told.

I went down to Johannesburg for a week in 1983 to find out whether I'd be able to work there. Would I be able to get into the townships; would I be able to approach black children and get information, do the studies I wanted to do; were there facilities that would allow me to analyze such data? Would I be allowed to publish?

And, of course, many people said they wouldn't go to South Africa because if you went there you were supporting apartheid. There were academic sanctions to prevent South African scientists from talking about what was going on. It was felt that merely by having a South African scientist in the room you were in some way supporting apartheid. So a decision to go and work there was one you had to take terribly carefully.

Radin: Were there people that explicitly advised you against it?

Cameron: Yes. But as a scientist interested in what I was interested in, I could only have achieved anything from *inside* the country, actually doing studies of human growth and development, identifying states of undernutrition, identifying the *legacy* of apartheid in terms of child health and growth. And by pinpointing areas where things could be done to improve infant and child health, one could improve human capital. You can only do that from *inside* the country.

In the week I was in South Africa, I got major reassurances from Tobias that "over his dead body" would anybody prevent me from publishing. So I decided to go for 3 years. I took the flight the evening of January 1, 1984.

Radin: And 1984 was also the year you published your book on methods that grew out of the Cambridge lectures [Cameron 1984]. In that moment, you'd drawn together a lot of informal knowledge around human growth and measurement and started to codify it.

Cameron: There had been no manuscript which had dealt specifically with how to measure children, none. There were manuscripts in anthropology on how to measure adults, but there were no measurement protocols for children. Tanner asked me to write a chapter for a compendium of three volumes that he and Frank Falkner edited called *Human Growth*, which came out in 1978 [Falkner and Tanner 1978]. I expanded the chapter into a book published in 1984, when I was in South Africa, in fact.

Radin: At this point it seems you had moved to another level of seeing how these methods could be used in a very different kind of context.

Cameron: The studies we had done were studies of normal UK children. I hadn't had the opportunity to apply those methods in a developing-country scenario with children who were more extreme in terms of their health and well-being. Within 2 years of going to South Africa, I had started two longitudinal studies, both in rural areas, one in a place called Ubombo in Natal, which is now KwaZulu-Natal, the other one near the Botswana border on a farm in a place called Vaalwater. At the time nothing significant on the growth and health of rural children had come out of South Africa, particularly on farm children. It was known that farm workers were, relatively speaking, badly treated. They were, to all intents and purposes, indentured slaves. They didn't earn enough to be able to leave, although they were paid according to a government-agreed amount that was pitifully small. This was an opportunity to get these baseline data about what was happening with rural children.

The contrast for me was dramatic; I'd been working in a center of excellence and sophistication in the Institute for Child Health in London, and a year later here I was sitting on top of a hill in South Africa measuring African children who were small and suffered from chronic levels of malnutrition, *undernutrition*, which nobody had documented.

Radin: And so what was the path from these initial rural studies to the Birth to Ten cohort study?

Cameron: By the time I'd been there a year, I knew I was going to be staying longer than 3 years because there were so many things that were crying out to be done and that I felt I could do with the skills and knowledge that I had. Other scientists in South Africa in the field of community health were pleased to see me because they knew Tanner's work intimately. They described me as a "thoroughbred" from an excellent stable and were keen to suggest potential studies and ask for advice.

I was young and arrogant, and toward the end of 1986 I wrote to the president of the Medical Research Council [MRC], the equivalent to the head of the U.S. NIH [National Institutes of Health], and said that he really ought to give me a research assistant. I informed him that I was the only person doing human growth research in this way and the research would be fundamentally important in terms of child health, particularly as the country comes out of apartheid. By the late 1980s, it was obvious things were changing rapidly. The laws that had kept people apart, such as the mixed marriage laws, were being ignored.

Radin: So petty apartheid was starting to break down, and there was a growing social movement that allowed you to productively intervene?

Cameron: Exactly that—petty apartheid was breaking down, but "grand apartheid," of course, was still there. I thought that my research was fundamentally important in monitoring the effects of sociopolitical change. With regard to my letter to the Medical Research Council, I didn't hear anything apart from an acknowledgment that the letter had been received. Then in February 1987, Phillip Tobias, who was five doors down the corridor, asked me to see him. He told me that the MRC president [Professor Andries Brink] was coming up to Pretoria for a meeting and wanted to stop in Johannesburg for a meeting with Tobias *and me*! I was very concerned that Professor Brink was not pleased about receiving an unsolicited letter from this young upstart!

Radin: You didn't know what the reaction was going to be.

Cameron: Hell no! I thought I was going to get rapped over the knuckles! Research in South Africa was very hierarchical. The president of the Medical Research Council was in charge of all the medical research funding in the country and was thus extremely powerful.

So, I was surprised when Andries Brink told me that the Medical Research Council recognized clearly that the country was going through dramatic change. Urbanization was part of that change, and they were concerned they did not have any information about child health in urban areas. He said that they appreciated that human growth was a core area. He invited me to initiate a study that the MRC would support to investigate child health in urban environments and introduced me to the MRC epidemiologist, Dr. Derek Yach.

Radin: They wanted you to collaborate with an epidemiologist?

Cameron: Yes, Derek Yach was the epidemiologist for the Medical Research Council. He and I immediately decided we would undertake a birth cohort study, based in Soweto. Soweto is literally the SOuthWEst TOwnship, 25 km to the southwest of Johannesburg. We wanted to get children at birth and follow them through to adulthood and learn about them, their mothers, and their families and what was happening in terms of maternal and child health in Soweto. We wanted a representative sample of both black children from Soweto and white children from Johannesburg. We very quickly got together with Lucy Wagstaff, who was professor of community pediatrics at Baragwanath Hospital in Soweto.

Radin: As you're narrating the story about doing this growth study paired with an epidemiologist and a pediatrician, can you draw out what you brought as an anthropologist?

Cameron: It's a question that comes up even now. I constantly find myself in competition, certainly for research funding, with epidemiologists rather than being in concert with them. In the "Born in Bradford" study I'm involved with now [http://www.borninbradford.nhs.uk], the head of the scientific research committee is an epidemiologist. When this birth cohort study was being set up, she and I would debate from epidemiological and biological standpoints. Epidemiologists want to do things on a global scale. They need samples of tens of thousands in order to study disease. We, on the other hand, as biologists interested in human growth, want powered samples in which we can determine "normal." So the differences between us are that epidemiologists, and of course pediatricians, are interested in the "abnormal." They're interested in the disease state or in the unwell, abnormal child. Human population biologists' major tool is specificity, whereas the epidemiologist and pediatrician's major tool is sensitivity, statistically speaking. The debate between us is that I want a powered sample from which I'm going to get my normal data; they want a sample of thousands from which they're going to be able to measure everything; things we know and things we don't know. It's a very different research design scenario. What has happened a lot in the history of birth cohort studies is that you end up with a design in which a large sample of individuals are assessed at specific points like birth, 5 years when they start school, and maybe 10 years just prior to puberty. But a small subset of 1,000 or 2,000 or 3,000 children are then followed much more closely. In the Bradford study we ended up compromising in that we enrolled about 15,000 and we're closely following 1,000.

Radin: That's very interesting.

Cameron: Yes, terribly interesting, but because I want normality and she wants to look at all the things that go toward disease state, in the party of myself and the epidemiologist and the pediatrician, we were bringing different things to the table. In the Birth to Ten study, Lucy Wagstaff was bringing an intimate knowledge of the health of children, and particularly babies, in Soweto—and she'd been dealing with them for 20 or 30 years. Not only did she know the babies and their mothers, she also knew how the health system operated in Soweto, which was very different from the way it operated in Johannesburg. Derek brought this completely *global* epidemiological approach. And he knew how to deal with the Medical Research Council; who to talk to in order to get things done.

Radin: And you were where?

Cameron: I was the chairman, and apart from keeping those two together, my job was making sure we had a viable study. In terms of what we were going to study, what measurements we were going to take, what assessments we were going to do, how often we were going to do them in terms of the research design itself, I was the one doing that. We were all absolutely fundamentally important within that scenario.

At a time of major social upheaval, three *white* people deciding they're going to do a growth study in Soweto was impossible. You couldn't decide you were going to do a growth study in Soweto unless the community representative—the head of their health service—agreed. Lucy Wagstaff arranged a meeting with the three of us and the head of Soweto Health in a clinic in "deep Soweto" to meet this man. We arrived at the place, which had a security fence surmounted by barbed wire around it. We'd agreed beforehand that Derek would be the spokesperson, and Lucy and I would nod and be supportive. He ultimately granted us approval to do the study by the simple act of offering us a drink of schnapps—at 10:30 am. This was a surprise to us all but not to be refused if we wanted our study!

Now of course things would be terribly different, but at that time he wielded enormous power. The way in which health was run in Johannesburg and Soweto was totally different as a result of separate amenities. In Soweto there were 13 clinics and one computer that dealt with maternal and child health for a population the same size as Johannesburg, in which there were 67 clinics and almost as many computers!

Radin: Getting this man's approval was a critical component for beginning the process of engaging social participation.

Cameron: It was getting permission to even approach an idea of doing this. The study was going to be based in Baragwanath Hospital, which is the biggest hospital that serves the black community in Soweto. We could get data from whites, Asians, and the colored community at different hospitals. Word spread by mouth with regard to the fact that Birth to Ten had been approved. At the time, people did not believe you could do a study like this. There'd only been one other birth cohort study done in Africa, in East Africa, in Kenya.

Radin: So "people" being scientists in South Africa or just in general?

Cameron: Scientists, community health workers, and academics. When we started to develop the idea of a birth cohort study, we knew that there was no way myself, Lucy, and Derek could do it on our own. I wanted to build a team composed of experts in different areas. I wanted to sell the study to other groups like community health departments. Time and again, I was told "You've got no chance. This is not going to happen. It's not going to work. Either you won't get the mothers to volunteer, or you won't collect the data, and you certainly won't keep it together for very long because the [sociopolitical] situation's too volatile."

Radin: What made you keep going?

Cameron: Ebullience. This was fundamentally important. I don't know if Derek and Lucy felt this, but I did—that this was *fundamentally* important in terms of describing the human condition with regard to the township. It was certainly important in defining a baseline of the legacy of what had happened over the previous 40 years. It was too important not to do, and at no point did I ever think that I wouldn't do it . . . even when we started, when we didn't have any substantial financial support for the study.

Radin: So who did fund this study?

Cameron: Anybody I could persuade to release money. The Medical Research Council, of course, had funded from the beginning. The Anglo-American Corporation had a chairman's fund for the uplift of black people. I met with the chairman and got almost matched funding. I also got funding in kind. For instance, I attended a Sunday lunch one day, and one of the other guests owned the local franchise for Kentucky Fried Chicken. As a result, the first vehicle we got to transport mothers to the research centers had a big sign on the side saying "Kentucky Fried Chicken—Colonel Sanders supports the Birth to Ten birth cohort." It didn't last long—we only used it for two weeks and then it got carjacked at gunpoint *inside* Baragwanath Hospital! But we got anything we could.

Radin: What was the evidence that as apartheid ended the cities were going to be flooded with people?

Cameron: There was this 3.5% urbanization rate from the latest census, which meant there were going to be 14 million people coming into urban areas by 2000, and Soweto was one of the biggest urban areas with shanty towns both within it and around the outside of it. And yes, people were going to be migrating at the rate of knots, and we had to have some baselines, we had to know what was going on. We had to be able to quantify, as it were, the legacy of apartheid.

Radin: What techniques did you need to innovate in order to deal with the circumstances of being in Soweto and getting people to come back and participate?

Cameron: That was the most difficult part of what we did. The methods of assessment were the same as they'd be anywhere else. But the methods that created and sustained longitudinality were the result of a learning experience because this had never been done in this environment. These were moms living throughout Soweto and throughout Johannesburg, and we had to get to them. We organized through the community health system and well-baby clinics through the fact that the moms visited the clinics and that the community health nurses were on board with us, and if the mom didn't come, the community health nurse would find the mom. Looking back 20 years later, I'm not 100% convinced that at that time the moms knew what a longitudinal study was or even saw themselves as being part of a cohort of mothers with infants who were going to be seen for X amount of time.

Radin: How was the study explained to them and by whom?

Cameron: The moms came for a checkup at the antenatal clinics in the hospital. There would always be a queue in South Africa, particularly for health care, particularly amongst black people when there weren't enough doctors. And the queues would last for hours. So moms were only too happy that you sat down and talked to them. Not that I would sit down of course because I was a white English male and it would be totally inappropriate. But we had black female researchers who would explain the study to them in their own language.

Radin: So these were other community health workers?

Cameron: We tried to get retired community health nurses. They were the grandmothers who'd worked in community health all their lives, who knew everybody. We got about a 75% enrollment at that time from eligible moms. In later years we were able to see children who were born within the time frame we were working in who clearly should have been part of the cohort and who we'd missed or their moms had initially said no. We had a window of really about 5 to 6 weeks to obtain the sample; that timing was based on pilot studies to look at birth rates and seasonal variation.

Radin: And this was during the period Mandela was freed from prison, right?

Cameron: He was released on February 11, 1990. And we started on April 23, 1990, so we colloquially use this expression "Mandela's children" to talk about these kids who were the first generation in a post-apartheid South Africa. You can't, in any way, diminish the importance of the fact that this was a post-apartheid world. The way in which one felt, thought, talked, and worked was very different after Mandela's release.

Radin: What would a typical visit, a typical kind of session be with a mother say bringing her child in maybe at 5 years? What kinds of measurements and examinations would be done?

Cameron: There'd be all the standard growth measurements like length, height, and some skinfold measurements, and the circumferences of their head and the . . .

Radin: So traditional anthropometric techniques.

Cameron: Traditional anthropometric—I mean taken from my clinical work in England and now applied to these children—the standard measurements to assess the changing morphology of a child. There'd also be a questionnaire from which we'd ask the mother about social circumstances, which we would then translate to socioeconomic status. There were questions about illness and also about the social environment in which the family lived—the number of people who lived in the household, how many rooms in the house [see Richter et al. 2007]. Importantly, this would be done each time the mom came so we had an estimate of what things were dynamic within her life and what things weren't. I had been concerned that people only measured socioeconomic status at one point in time and used the same variables, whether individually or in the form of indices, to describe socioeconomic status or social class. You've got to move away from social class to socioeconomic status so you're talking about what is functional and dynamic within an individual's life.

Radin: Do you think this was your perspective coming out of biological anthropology? What do you think made you focus on this?

Cameron: I think my experience in longitudinal research projects was the primary driving force, the fact that time-series analysis tells you that there are very few environmental factors that have a continuous and constant effect on a biological process, particularly one as plastic as human growth. But it wasn't just me who was realizing that cross-sectional assessments of social class within a longitudinal model were inappropriate. There were departments of social sciences in all the universities in South Africa who clearly knew this was inappropriate. But my issue was that social class changes. The way in which these variables interact changes so that maternal education is recognized as being a major variable that impacts the health of infants. It affects how long she breast-feeds and what sort of food she gives the child, whether the child gets immunized, levels of hygiene, how she takes on board the health messages that are thrown at her all the time. But by the time the child gets to 5 years, it's not about maternal education; now it's about how much money there is in the family to devote to keeping the child in a situation in which he can stay at school and so on. And so now economy becomes the most important variable.

Radin: I'm starting to get a feel for the complexity of the study. I remember you said at the Wenner-Gren conference that you tried to get people to feel like it was their study as well. At what stages did that message start being explicit, and how did you and your colleagues encourage that kind of sense of ownership and participation in the study?

Cameron: I left South Africa in 1997. I'd been there a decade since the initial discussion with the president of the MRC, Andries Brink, and by now Birth to Ten was a big study. We had over 4,000 children, and because it was clearly working, everybody wanted to get on board. The Medical Research Council was giving major backing. It was viewed as a cornerstone of what the South Africa Medical Research Council was trying to achieve, to demonstrate that they hadn't been dragging their feet toward the end of apartheid but actually had been working toward post-apartheid South Africa. It was used very much as a political tool. But the work was still getting done, that was the important thing. And I was leaving the study to go to the UK, which opened up a whole new avenue of funding because I could apply as a UK-based scientist for funding that linked me with a developing-country scientist. And so a whole raft of funding from the Wellcome Trust became available. Some of the biggest medical grants that South Africa had came directly from that association and continue to do so.

In 2000–2001 we appointed a project manager, Dr. Shane Norris, who was a lecturer in the Department of Physiology with strong links with Professor John Pettifor, chair of the Department of Pediatrics at Baragwanath Hospital. Shane was excellent, and he'd learned very quickly about how to manage the study. He was also very good at public relations. He realized that for the study to keep going and to keep our attrition rate minimal, we had to get the participants to take ownership.

Radin: As the children themselves were becoming adolescents.

Cameron: Exactly. So we had to get them to take ownership of the study.

Radin: Can you talk about what he developed in order to do that?

Cameron: We were doing talks, we were having meetings in Soweto, public meetings, in which we'd talk about what was going on and what we were doing and the sort of results we were getting. But he got into developing newspapers.

Radin: Like a newsletter?

Cameron: A newsletter, yes, that would go out to the kids. He developed a Web site, and we changed the logos completely, so now they were three hip adolescent kids as the focus. The participants communicated by text and by Internet and through the Web site. They had a space in which they could talk about what they were doing, like in Facebook. The title was obviously changed from "Birth to Ten" to "Birth to Twenty." And underneath "Birth to Twenty" it said "Your National Heritage." The original logo [fig. 2], which had three children holding hands and playing, had been initially chosen by the moms. They were very childlike drawings of three children holding hands, and the *r* in "Birth" was back-to-front as if a child had written it. We'd had all sorts of things done around that logo, like fridge magnets with a space in them to put the date of the next appointment. Shane Norris changed this logo [fig. 3] and developed the idea that the kids should be able to share with each other the experience of being in a study. So it took the emphasis away from the moms, who had kept them in the study for that first 10 years, to themselves.

The change of title from "Birth to Ten" to "Birth to Twenty" came about when it was clear that we were going to be able to move beyond the first 10 years. That realization came with significant funding obtained in the late 1990s—after I had returned to the UK—to primarily investigate issues relating to historical and current factors that place young people at risk during adolescence and in later life for sexual and reproductive disorders and diseases associated with lifestyle. An eminent South African psychologist, Professor Linda Richter, was the major force behind that aspect of the study [see, e.g., Richter, Norris, and De Wet 2004].

Radin: And ostensibly they're getting information about themselves from the study. What mechanisms were there? Was it in the newsletter? How were they learning about how they were doing?

Cameron: The initial Wellcome Trust funding we'd got

Figure 2. Birth to Ten logo associated with the study from 1990 to 1998. The design of the logo was chosen by mothers of participants (copyright Birth to Twenty).

was to create this substudy to look at bone health. That was an intensive powered study of about 700 children who got feedback on the results we were getting. Shane Norris provided that feedback through these newsletters. You can give individual feedback like that, but you can't provide descriptive statistics on the group because it takes time to analyze the data. One of the biggest problems we faced in South Africa throughout the study was the expertise to deal with the amount of data we had. None of us had appreciated how quickly we'd start accumulating information. We were dealing with literally millions of bits of information.

Radin: That's related to something we talked about a lot in Teresópolis: collections. How did you make sense, begin to even think about interpreting and then maintaining this information?

Cameron: Very rapidly we realized that there was no way we could handle the amount of data that we were dealing with because it grew exponentially. We couldn't find experts, around the world, who knew how to handle this sort of information. All the American longitudinal studies that formed the basis of what we knew about human growth up to and even after the Second World War were all paper based.

The biggest challenge was data handling; how to store, analyze, edit, clean, recomputerize, get it out; and then how to manage its use by other people. So then you get into the question of ownership of data.

Radin: Yes, can you talk about that?

Cameron: Absolutely. Initially, when we started thinking about the study in the 1980s and the 1990s, ethics approvals [for research] were in their infancy in many respects. I didn't come across an ethics committee before the 1980s. The idea of the data being owned by the people from whom you'd got

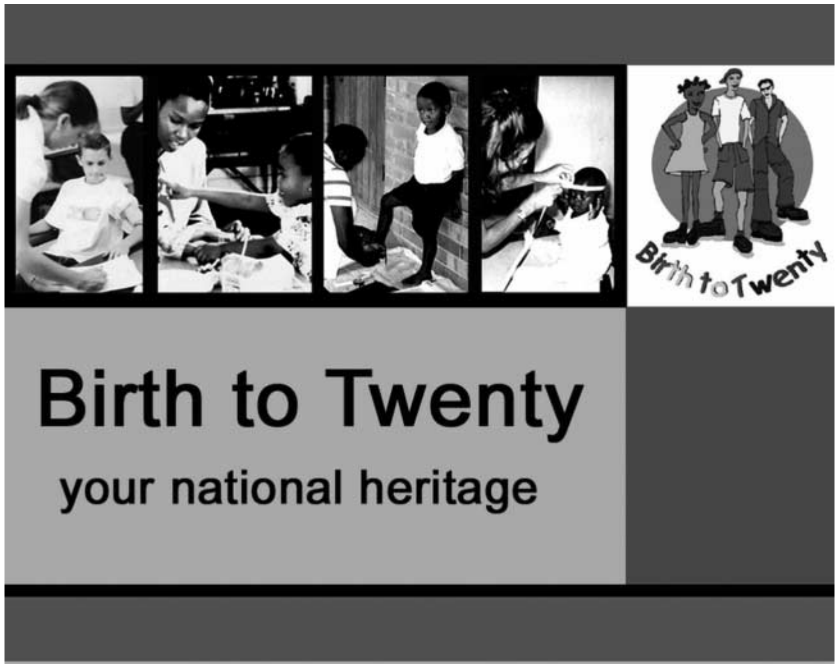

Figure 3. Birth to Twenty logo associated with the study from 1998 onward. Note the logo is more informative and illustrative and implies participant ownership of the study (copyright Birth to Twenty).

it hadn't crossed anybody's mind to some extent. It was only toward the end of apartheid and of a realization of rights of the "participants" as opposed to "subjects" of research that we began to think about who owns the data. This was interesting because before that the scientists would have arguments between themselves about whose data it was.

Radin: Right, data ownership has a lot of registers.

Cameron: Of course it does. I mean if I initiated the study it's "my" data. But you had to do a complete about face and say, actually, "it's got very little to do with me. The data's not my data, the data in some way is owned by the participants. This is information about them." But ideas of data protection didn't exist.

The ownership issues came amidst scientists' realizations in the 1990s of participant involvement in and ownership of scientific research. I will always remember the comments of a South African student of mine who said of our sojourns to collect data in rural areas like Ubombo that it was like the whites were coming and stealing the puberty ratings of the indigenous population. He was, of course, playing on the idea of American cowboys and frontiersmen stealing from the Indians. As if we were Kit Carson and his cronies coming in, getting information, and leaving. He was voicing a very real concern that we were coming once a year to Ubombo, getting this information, and not actually giving anything back to the community. Things changed in many respects around that time. After about the first year, we collected books from the private schools in Johannesburg and set up libraries in the rural schools and got decommissioned microscopes from the anatomy department that were still perfectly functional and set up laboratories in the schools so that we were putting something back into the community.

Plus the organizations giving research funding, for the first time, started requiring us, within our research applications, to say how we would make data available to a wider community. You had to write a whole section on the accessibility of data. But that didn't happen until the mid to late 1990s. That goes back to the ownership issue because a lot of the arguments had to do with, "well it's my data; but why should anyone else have access to it?" That was painful because it took blood, sweat, and tears to get the data. You dedicate part of your life to doing this sort of work, and then someone decides they want your data that you've worked hard on and that you've paid for in one way or another.

Radin: And is it yours even to give?

Cameron: Well exactly; is it yours to give? And at what point do you say no? You've got to go back and ask the participants whether you are a fit person scientifically and in every other way to use their information. These are really difficult questions. Probably every research group comes up with their own way of dealing with them. I'm sure there's no uniformity across the world because the circumstances and the communities with which you are working require different things. We have an access policy. We have a Web site [http://www.wits.ac.za/Academic/Health/Research/BirthTo20], and on that Web site anybody can go in and say, "we'd like to access the data." You apply to do so, you write about what it is you want to do, your bona fides of course obviously have to be appropriate, and then you can go and access a data set restricted to your requirements. The data is, to all intents and purposes, publicly available but only to certain members of the public who are qualified and then only to the data that is of proven interest to them.

Radin: Is this an agreement that was reached in consultation with the participants?

Cameron: No, it was an agreement that was reached in consultation with the funding bodies that paid for the data to be collected. With the participants, certainly, and I may be wrong about Birth to Twenty now, but certainly in the time of Birth to Ten, there was no sort of advocacy group from the community. Because of the historical times, we didn't have a committee that talked about that. In the Born in Bradford study I'm involved with now, we have an advocacy committee made up of community elders and moms and people in the community who we do talk to about all of these issues.

Radin: Of course that model would have to be particular to that society, right? You can't necessarily say, well this works in Bradford so it's going to work elsewhere?

Cameron: Absolutely, because the people who are viewed as being responsible individuals within a society will differ depending on the community. Bradford is 30% Pakistani, and 50% of our sample is Pakistani. The way their society is organized is different from the way in which the European-descended society living next door is organized.

So if you want an advocacy group, you've got to find out from the people who they are, which means, interestingly, that the design of your research group of scientists has got to include people who know the community and are even from the community. And I don't suppose for one second we would set up a study called Birth to Twenty in South Africa now without having a number of black scientists and clinicians involved. Indeed, we do have them on the team now. In South Africa in the early 1990s there weren't these people qualified to help us—apart from the community health nurses, who of course were all black—there weren't any individuals with research experience, knowledge, or desire to work with us. What we did do in Birth to Ten, which was fairly unusual at the time, is that we translated our questionnaires and materials into four different languages. We had English, Afrikaans, Zulu, and Sotho. Zulu and Sotho are the two big languages in Soweto.

Radin: I think this is a nice transition point to ask what have been some of the tangible impacts for public health policy. What are some of the major outcomes of the South African studies?

Cameron: The major scientific findings from Birth to Twenty have really only emerged during the last decade as the data management issues were sorted out and we were able to apply relatively sophisticated statistical modeling procedures to our time-series data. One set of results concerns the development of risk factors for noncommunicable diseases of lifestyle such as Type 2 diabetes mellitus, obesity, and cardiovascular disease. We have been able to demonstrate the emergence of these risk factors in Birth to Twenty children as young as 7 years of age who characteristically have relatively low birth weights and grow extremely rapidly during infancy [Crowther et al. 1998, 2000]. These results demonstrate the need for early intervention to change habitual behaviors relating to diet and physical activity.

We have undertaken focus groups with adolescents to learn about their dietary habits and nutritional intake. We have been able to identify a "nutritional transition," common to the children in other transitional economies, in which traditional high-fiber, low-fat diets are being replaced by low-fiber, high-fat, high-energy diets characteristic of industrialized societies. The result of that transition is a greater prevalence of obesity, particularly amongst girls. Recent research on Birth to Twenty 17-year-olds, for instance, identified that they consumed about eight fast-food items each week and most frequently consumed an item called the "Soweto Quarter." A typical "quarter" consists of a quarter-loaf of white bread, chips [fried potatoes], a slice of cheese, and any number of delicatessen meats and sauces. A macronutrient comparison between a "quarter" and three commercially available fast-food meals demonstrated that the "quarter" provided 5,970 kJ of energy at a cost of about 9 SAR, or about $1 [Feely, Pettifor, and Norris 2009].

But I think the major outcome is almost an intangible one in many respects. In some ways it probably links into the public understanding of science. The study has raised the level of knowledge, interest, and understanding of the importance of child health and child growth in everybody's minds. It makes them topics that are discussed and are understood as being important. Some of the more important things are the stories in South African newspapers about the fact that we're investigating child health, the fact that child growth is important, the fact that immunization is important, the fact that children can recognize the brands of cigarettes by the age of 5 is important in a bad way, the fact that you know moms lie about their children's age to get them into school early.

Radin: So a kind of anthropological information?

Cameron: Well absolutely, because scientific information is for scientists; we publish it in scientific journals using our own language, which is almost indecipherable to a nonscientist. So when we come to write something which is nonscientific, it's really difficult. The Avon Longitudinal Study in the UK has two full-time public relations people, both of whom are ex-journalists. The whole reason being is if you want people to keep participating in the study, you want them to understand it. But the story you have to tell is one that's different from the story you tell scientists.

Radin: That leads me to two branching out questions. One would be how has the data that emerged from this study impacted thinking about human growth and research trajectories in the scientific community? The other branch is who's doing the work of taking the data and putting it into practice at the level of policies?

Cameron: The same message is told in different ways to those different communities. For the policy makers you need to give the information in such a way that they can use the information to make policy. This is done by ensuring that information from Birth to Twenty gets to the right decision maker. A lot of that's done by word of mouth. For example, if there's going to be a major initiative in terms of child nutrition in urban areas of South Africa, the first thing they need to do is identify whether children are malnourished or not. You've got to start doing measurements. The only measurements against which one could compare the findings are the data we have in Birth to Twenty.

Radin: And have there been specific policy decisions in South Africa?

Cameron: As an example, the adolescent fast-food consumption study I mentioned before provides empirical evidence for those wishing to improve the health of all children in South Africa through awareness of poor dietary habits. Residential mobility within the urban environment is another important policy issue with regard to the provision of housing and the distribution of state benefits. Data from Birth to Twenty has been able to demonstrate that by the age of 14 years, 64% of our urban sample had moved house at least once and only one-third of the sample had been stable, with clear implications for the traceability of individuals and households [Ginsburg et al. 2009].

The other ones you'll read about are the things like tobacco and about ages of school entry. But there is a whole variety of factors that are almost hidden effects of having a study like this in that people in policy-making areas that know about the study will think, is there something that Birth to Twenty can teach us about this, published or not published, and will contact Birth to Twenty and say, "what have you got about this information?"

Radin: So there's almost this informal intellectual economy or traffic in ideas happening.

Cameron: Absolutely. I mean the same way in which we do that by reviewing grant applications. The way in which I criticize a grant application depends upon my experience and my knowledge. Part of that comes from studies that I've done and the research that I do and the research I'm interested in. In the UK, we have to include a section on how the proposed project will affect policy. There's an ongoing debate about "blue sky" research, which we didn't have to contend with 20 years ago. Now, I have to start thinking about how my research can not only be published in an international scientific journal for the benefit of the scientific community but also how it can be put to the government to influence policy.

Radin: Are people in other transitional societies in the Global South drawing examples and lessons from this study? Is there a unique role for birth cohort studies to play both in terms of scientific knowledge and societal change?

Cameron: The unique thing about birth cohort studies of course is that they are longitudinal studies of a particular group of children born in a particular historical moment in time and that their growth, because growth is so plastic, is going to reflect what's happening to them now and what's happened to them in the past. They are the only method by which we can gauge or monitor how changes in society are impacting the health and well-being of children.

The major birth cohort studies that have been done in the Global South are the CEBU study in the Philippines, the Pelotas study in southern Brazil, and Birth to Twenty. Certainly they are the three that most people in this field would think of as relevant. We actually believed for a long time that Birth to Ten was invisible to the rest of the world. People mentioned CEBU and sometimes mentioned Pelotas but hardly ever mentioned Birth to Ten/Twenty, and we couldn't understand why. I can remember at one time thinking it was because we were doing it within South Africa.

Radin: In a way you could argue that this project and activities like it have actually altered knowledge practices themselves.

Cameron: Well I think so. I mean the idea of doing a longitudinal study is that these children are born in a particular moment in history. The study may exist for 20, 30, 40 years depending on how long you want to see them during times of changing political and social circumstances. You're getting this record in their health, in their morphology, in their psychological attitudes, in their social interactions that reflect those changes in societies. They're very complex and exciting studies because of that. If you want to look at anything, you've now got a whole history of factors that can allow you to determine at least some of the variants in an attribute. You couldn't do that on a repeated cross-sectional study. You can only do that with birth cohort studies, so they're incredibly powerful from that point of view. One of their problems, of course, is that—and this is the clash between the population biologist and the epidemiologist—unless you have a sample of tens of thousands or at least thousands, you won't get a sample large enough in their 20s to be able to use that information statistically because your attrition rate is so great.

Radin: At the conference, you talked about human biologists having a moral obligation to do work that takes into consideration historical, social, and political contexts. It's very evident how important that is for you. Do you think this sense of moral obligation is shared by others in your profession? As a teacher, do you think it can be cultivated?

Cameron: Do I think it's shared within the scientific community? In my experience I've come across people who are committed to doing research in this area. They've chosen to work, for instance, in a developing country because it's important to them to be involved in improving the quality of life of people who are disadvantaged. It's pretty difficult to work in a developing country if you don't have that degree of commitment because it's a very challenging circumstance. You're going to measure some child in the middle of nowhere and crawl into a tent and . . .

Radin: And touch them.

Cameron: Yes, you're going to be touching other people, and how fundamentally important is that? There's an intimacy and therefore there's an enormous sense of trust that not only will you be kind to them but that you touching them has to do with getting objective information. And also that what you will do with the data is morally just and wouldn't be used to denigrate or to belittle them in any way, shape, or form. That's an incredibly strong responsibility that we take with us, both into the field to do this type of work and away from the field with the data that we have. I would say the vast majority of people I work with have that level of understanding of their responsibility. Those that don't are quickly told that they shouldn't be working in this area because they clearly don't understand what it is they're doing or why they're doing it. How do you get students to understand that? In my experience, you *show* them what it means. Every year I took my students and staff into the field. They came with me into rural areas and measured and got information, talked to the children, gave lessons in class in these schools as part of their giving back to the community. By just being involved, they grew enormously in terms of their attitude. And many of these were white South African students who grew as a result.

Radin: What's interesting about your work is that you've taken certain older anthropological techniques used in creating systems of apartheid and used them to point out the violence and to rectify it. You show that it's not the science that is good or bad but rather how people use that science. But who is supposed to ensure that the tools and products of anthropology are used for "good" in the promotion of health and well-being?

Cameron: A good question. Who is supposed to police it? Of course one individually polices one's own work and that of one's immediate colleagues and students. But learned societies in many respects become the policemen. I was invited in 1990 to give a special lecture at the annual meeting of the American Human Biology Association in Miami about my work in South Africa. At that time I always finished my written papers and my spoken lectures with the statement, "This work is dedicated to a post-apartheid, nonracial, democratic South Africa." Following my talk I was in the business meeting of the association, and a voice from the back of the room proposed a motion that "this Association does not invite South African scientists, or scientists working in South Africa, to the annual meetings."

There was uproar. People turned and shouted at the proposer, "Did you hear what he said? Did you go to his lecture?" The proposer said "No, I boycotted his lecture." And they said, "He spoke against apartheid. How can you say that he shouldn't be here?" The president said he said that a motion had been proposed [about whether or not I should be there] and asked for a seconder. Nobody said a word. And then he said, "There's no seconder to the motion so there's no motion." And that was it. This was a situation where the learned society was policing the moral dimensions . . .

Radin: But not in a way that you would have expected; they were trying to protect those who were seen as doing responsible work.

Cameron: Exactly that. So societies do—and even now at business meetings of the Society of the Study of Human Biology, what people say and talk about is policed and criticized left, right, and center. It's that sort of openness, in critical discussion, of what one says and does that polices one's science.

Radin: What about those who say, "Well I'm a scientist and I do objective research and I can't be bothered. This isn't my role."

Cameron: There were many scientists who did exactly that. They can't be bothered and they are not political. Well you can't avoid taking a stand because by not saying anything you're saying something. You're making a statement that you are so morally devoid of responsibility that you will not say anything when faced with a situation that cries out for you to use your skill to do something. The moral responsibility of a scientist in the situation of South Africa in the mid-1980s was that if you had skills that could be used for the betterment of society, particularly those disadvantaged in societies, that you should damn well get out there and use them. And you're morally bereft if you didn't. I'm talking about the personal responsibility to other people in the global society that you live in. It's man's humanity to man, which is so terribly important.

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