



## Embedding Ethics

*Edited by*

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## Your Body, My Property: The Problem of Colonial Genetics in a Postcolonial World

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Until its annual meeting in 2003, the American Association of Physical Anthropologists was the only major professional anthropology organization that lacked a code of ethics. The specifics of its reluctance to adopt such a formal code may be obscure, but there is a considerable extent to which it may be at least understandable. As the anthropological field that self-consciously represents the scientific approach to human studies, biological anthropology would seem to be ideally situated to take the lead in establishing ethical standards to help establish the Baconian promise of better living through science. Yet obviously that is not occurring.

Biological anthropology is not methodologically unified—as archaeology and cultural anthropology are unified by excavation and ethnography, respectively. Rather, it borrows methods from biomedical research and cross-cultural ethnographic, ecological, and historical fields in studying the evolution and natural diversity of our species. Consequently, the development of an ethical code would necessarily reflect the extraordinary methodological diversity practiced under the banner of biological anthropology. But, such a practical matter aside, the field sits in a historical and philosophical trajectory that merits consideration.

Writing in the *American Anthropologist* in 1900, George A. Dorsey reviewed the progress that had been made at his institution, the Field [Columbian] Museum in Chicago, shortly after the Columbian Exposition in 1893:



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Naturally much osteological material of great ethnic value was procured, along with many of the collections donated by the Exposition, as well as with many of the collections obtained by purchase. As a result the department was in possession of skulls and skeletons from Alaska, the Northwest coast, and several of the Plains tribes; from Ohio, New Jersey, and Arkansas mounds; from prehistoric graves in Costa Rica, Colombia, Peru, Bolivia, and Chile; and through the Boas and Ward purchases many specimens from America, Europe, Asia, Africa, and the Pacific islands.

A few years later, "From the Blackfeet, Bloods, and Haida was also collected a large amount of osteological material, while a small amount of similar material was obtained from the Kootenay, Tlingit, and Tsimshian." For 1897-98 he reports, "In the division of physical anthropology more than 150 skeletons were accessioned, the most important single collection being one of fifty-two Papuan skulls from Gazelle peninsula, New Britain, received in exchange from Dr Parkinson."

Over a century later, I read these words in conjunction with a project I was undertaking for the American Anthropological Association's Centennial Commission involving the field's history. What jumped out at me was the sterility of the descriptions: the material was "procured," "obtained," "collected," and "accessioned." No description was given of where the bones actually came from or how they were acquired. Of course, it is not hard to figure out. As Franz Boas famously wrote to his sister from the field in 1888, "It is most unpleasant work to steal bones from a grave, but what is the use, someone has to do it" (Freed, Freed, and Williamson 1988; Starn 2004). Boas, it is worth noting, indeed made his initial mark as a collector of skeletal remains, such that his faculty appointment at Columbia in 1897 was as a physical anthropologist.

Thus, the dirty little secret of modern physical anthropology: it began as little better than the metaphorical hunchbacked assistant supplying anthropological science with the gory body parts it used for data. At least this was something of an advance over the premodern version of the field, which was most famously represented by the proslavery polygenism of Samuel George Morton, Josiah Nott, and George Gliddon (Hrdlička 1914; Stanton 1960) and also based on the "collection" of skulls. As science, the field developed a sterile vocabulary to conceal the macabre nature of its work. Following the medical model (whose early founders, such as Vesalius, were dogged by accusations of grave-robbing but nevertheless worked ultimately toward the noble goal of healing the sick), physical anthropologists lexically transformed erstwhile people into specimens, materials, and parts of collections. The

transformation was sufficiently complete that some contemporary practitioners are able to deny, with an apparently straight face, that their intimate study of dead people's parts is gruesome and rather distasteful to ordinary sensibilities (Killion et al. 1999).

The passage in 1990 of the Native American Graves Protection and Repatriation Act, 25 U.S.C. 3001 *et seq.* (NAGPRA), which deals with both cultural and biological remains, can be seen as the culmination of half a century of progress in biomedical ethics. Although the Hippocratics enjoined each other to "first do no harm," the twentieth century saw the development of a formal concept of responsibility on the part of the scientist to the subject. This of course came in reaction to the excesses of the Nazis in the name of science (the notorious Auschwitz camp doctor Josef Mengele had a doctorate in physical anthropology from Theodor Mollison in addition to his medical degree [Muller-Hill 1988; Robert Proctor, personal communication]) and was formally articulated as the Nuremberg Code in 1947.

The Nuremberg Code assumes that the research in question is medical and invasive and that there is a common cognitive framework shared by the scientist and subject that permits the scientist to assume the responsibility of obtaining the "voluntary consent" of the subject. It assumes further that the only relevant party aside from the scientist is the subject. It is, therefore, and was intended to be, a first step. We now recognize, for example, that a geneticist has a responsibility not just to the patient with a diagnosed genetic disease but also to other family members who are at risk for it. Thus, in the 1990s, successful medical malpractice lawsuits established a "duty to warn" for people whose personal health decisions had been adversely affected by virtue of not having been informed about a close relative's genetic condition (Dolgin 2001).

The recognition of scientific responsibility beyond simply the experimental subject, when combined with the Nuremberg Code's appreciation of "mental suffering and injury" as meriting consideration, implies that NAGPRA is very harmonious with modern biomedical sensibilities when it protects Native Americans from the callous disposition of their dead relatives. Thus, when an archaeologist conservatively objects to the repatriation of Ishi's brain on the grounds that Indian remains "care no more what happens to them than a dead stick off a tree" (Whittaker 2000:4), the point is correct but moot. Other parties do care profoundly what happens to Ishi, and the scientific value of the brain may indeed need to be balanced against their rights. The scientifically valueless brain of the famous California Indian ("the last

of his tribe”), long in the possession of the Smithsonian Institution, was ultimately repatriated under the National Museum of the American Indian Act to the Yana-speaking Redding Rancheria and Pit River tribes in August 2000 (Starn 2004).

### The Human Genome Diversity Project

My own interest in ethics stems from my involvement with the Human Genome Diversity Project, a consortium of population geneticists who proposed a scheme for collecting genetic material from indigenous people on a grand scale in 1991 (Cavalli-Sforza et al. 1991). One might anticipate that a proposal for establishing a repository of biological materials from indigenous peoples might merit some intense prior ethical reflection about the collection and disposition of such materials, especially coming just a year after the passage of NAGPRA. Yet it did not: virtually all the relevant considerations were reactive, articulated by critics, and only subsequently addressed by the HGDP (Marks 2003).

And yet NAGPRA and the HGDP are obviously linked. The issues that stimulated the passage of NAGPRA—the existence and management of osteological repositories of indigenous peoples—certainly are paralleled in the proposal to establish a DNA repository of indigenous peoples, in the questions of just how such “specimens” are to be “acquired,” the discomfort promoted by having strangers take and control powerful, magical, tabooed things, and the ease with which political or economic power can be exercised to obtain and control valuable objects over the reservations of their rightful owners.

An anecdote may serve here: A distinguished senior anthropologist took me aside a few years ago to express his mystification at the ethical issues being raised against the HGDP. He explained that many years ago he had brought blood back from the field for a physical anthropologist to analyze and that it was simply a matter of course. I asked him whether “his people” had taboos about their blood, and he assured me that they did—and how! So how did he manage to get them to give up their blood? Simple, he told me; they traded it to him for penicillin, and if they didn’t give him blood they didn’t get their antibiotics. Suffice it to say that the blood was acquired in a highly coercive fashion. And while I would not presume to judge this senior colleague’s behavior with the aid of several decades of hindsight any more than I would judge Boas’s grave-robbing over a century ago, I can certainly speculate that the same sympathies that lay behind NAGPRA would hold sway in a debate over the ownership of the blood.

Indeed, the ties between blood and bones for physical anthropologists are very intimate. Both are studied as markers of ancestry, and both are—or have been—considered as causes or at least physical manifestations of behavioral tendencies. Earnest Hooton, for example, carried out the skeletal analysis of Pecos Pueblo (recently repatriated from Harvard’s Peabody Museum) in 1930, and less than a decade later he published the extraordinary work *The American Criminal* (Hooton 1939), purporting to discover physical differences between criminals and volunteer firemen that he believed underlay the development of the criminalistic tendency. Hooton believed that he was discerning a constitutionally inferior human type that could only be dealt with eugenically, through segregation or extirpation.

While Hooton’s work sold so poorly (because of its statistical naïveté, among other things) that Harvard University Press declined to publish its successor volumes, there is remarkable intellectual continuity between his work and modern investigations into the neurobiology or genetics of criminality (Allen 1999). The technology changes and the methods change, but the basic questions, assumptions, and approaches remain: studying the body in one era was a means of probing deeper constitutional pathologies, as in another era studying the genes is.

In other words, the bones and the blood are connected by scientific epistemologies stretching across the decades. And although the architects of the HGDP were not themselves concerned with behavioral genetics, one need hardly look far to find its relevance. Dean Hamer, discoverer of the (apparently non-existent) homosexuality gene on Xq28 (Hamer et al. 1993; Rice et al. 1999) tells us that “since the discovery of the gay gene, my lab has gone on to find genes for two other personality traits: novelty seeking and worry” (Hamer and Copeland 1998:11). And naturally “there is still such great variation in the [novelty-seeking] gene in modern day humans. . . . Different ethnic and racial groups, who evolved under different environmental circumstances, have noticeably different frequencies of the different variations” (Hamer and Copeland 1998:49).

The point is simply that the accumulation of genetic material at the turn of the twenty-first century entails a research program similar to the accumulation of osteological material at the turn of the twentieth century—biobehavior and biohistory—that was judged inadequate to justify the human rights violations now perceived in the collection of the bones. (To these research programs we may add the medical knowledge that grew as a result of these skeletal collections, which is paralleled in the genetic research as well, even though in both cases that is not what the materials were principally collected for.)

The offspring of human genetics and anthropology, anthropological genetics exists in the shadow of the eugenic legacy of one parent and the colonial legacy of the other. For all its muddled and inflammatory allegations, *Darkness in El Dorado* raises a key issue about the control of biological remains of a sacred nature when it observes (Tierney 2000:51):

[James Neel, for the Atomic Energy Commission] purchased twelve thousand Yanomami blood samples, dispensing a steel gift for each vial of blood. . . . Today, those vials are located in an old refrigerator at Penn State University, where Chagnon once taught, and are the property of the Human Genome Diversity Project of the U.S. government.

They are actually in the custody of a former protégé of Neel's at Penn State who was involved in trying to start up the HGDP—which is itself of questionable status and in any event is certainly not a part of the federal government. But there are indeed vials of blood collected years ago under circumstances that would not pass bioethical muster today, and if one wishes to do a genetic study of the Yanomami one need not contact any Yanomami or any representative of the Yanomami. Some-how that just doesn't seem right.

The HGDP became aware of these issues shortly after it coalesced, and it attempted to deal with them by developing an idea of "group consent" (Greely 1997). Since the project was formulated as population genetics, it was interested in people specifically as group members. The idea of requiring the consent of the group as well as that of the individual would serve both to create the appearance of a heightened concern for ethics and to reify the group as a natural, bounded unit. Juengst (1998) challenges the value of group consent on the basis of the fluid and hierarchical structure of human populations. Additionally one may well ask whether the consent of the group undermines the ability of the individual to give consent voluntarily.

The most critical issue faced by the diversity project, however, is one that osteologists were spared: the value of the biological object itself as a commodity (Cunningham 1997). (Here, of course, pothunters and other looters of cultural artifacts would have a common ground with the geneticists.) Patent law in biotechnology strongly favors scientists, as the unsuccessful cases of John Moore (Greely 1998) and the descendants of Henrietta Lacks (Jackson 2000) showed (these plaintiffs were denied the right to share in the profits made from cell lines derived from their bodies). In the case of the blood of indigenous people, the National

Institutes of Health applied for patents for cell lines derived ultimately from the blood of a Hagahai (New Guinea), a Solomon Islander, and a Guaymí (Panama).

When confronted with possible financial issues arising from the ownership of indigenous people's blood, the HGDP replied meekly that it was interested in the blood solely for science's sake ("The Project is not a commercial enterprise" [<http://www.stanford.edu/group/morrinst/hgdp/faq.html>]). And yet, the people to whom they were avowing this were well aware of the development of "bio-colonialism" on the part of agribusiness (see Stone, this volume). Availing themselves of indigenous people's knowledge freely given, large agricultural corporations were making considerable profits in which the people whose knowledge they needed were not sharing. If there is economic value in the blood of indigenous people (why else would biotechnology companies be so acutely interested in it?), then what is a fair price?

I suggest that the same guidelines as govern the disposition of osteological specimens should govern genetic specimens as well. If bones belong to people rather than to science, then blood and its derivative products should as well. The collection and use of genetic samples have been flying under the radar for a long time, but the same principles of dignity and respect for indigenous rights should govern them. The market value of blood or DNA makes it even more crucial to recognize ownership and control of these materials.

#### Four Concerns of Human Genetic Research

Contemporary ethical concerns in human genetics generally focus on four issues: autonomy, beneficence, nonmaleficence, and justice (from the WHO Meeting on Ethical Issues in Medical Genetics, Geneva, December 15–16, 1997). These are directly applicable to anthropological genetics and raise significant issues about whether and how they can be met in a cross-cultural context.

Respect for the autonomy of persons centers on the ability of an individual to make a free and informed decision about participating. In particular, it is a safeguard for those with diminished autonomy, notably children and impaired adults. The spirit of this guideline surely extends to people whose diminished autonomy is a result of their isolation from the communities of modern science and business.

Beneficence is a requirement to ensure that scientific research delivers some good to the people participating. While there are theoretical benefits that may accrue as a result of population genetic research—a

cure for diabetes in Native Americans is sometimes brandished in this context (e.g., Kidd, Kidd, and Weiss 1993)—it is a far-fetched proposition given the way in which the project was designed. Its objective was to collect samples for microevolutionary studies—hence the desire to include some Hopis, some Navajos, some Inuit, and so on. In order to utilize the genetic sample, a researcher interested in, say, diabetes among the Navajos would have to be able to correlate the phenotype, the trait, with the genetic variant. In other words, the researcher would have to know which samples came from people with diabetes. Further, if the samples had in fact been collected by a researcher who recorded whether they came from diabetics or not, making it possible to use them to study the possible genetic etiology of diabetes, they would be of use to the next researcher, who might be interested in the genetic etiology of gallstones, only if there was a record of which samples came from people with gallstones. Thus, these samples would be of only exceedingly minor benefit to the people they came from unless they were accompanied by comprehensive medical histories. Benefit to the communities of interest was never a goal of the project or even a sidelight; it was always for the benefit of the scientists themselves. Indeed, other people's blood has commonly become a commodity among genetic researchers, a medium of exchange and a locus of reciprocal obligation (Anderson 2000).

The third concern, nonmaleficence, is the safeguard against the callous or cruel scientist. There is certainly no doubt that the architects of the HGDP had no intention of inflicting harm or pain on others. And yet of course harm and pain come in many forms. In a "puff piece" for *Time* on the project we learn the following (Subramanian 1995:54):

On one occasion, when Cavalli-Sforza was taking blood from schoolchildren in a rural region of the Central African Republic, he was confronted by an angry farmer brandishing an ax. Recalls the scientist, "I remember him saying, 'If you take the blood of the children, I'll take yours.' He was worried that we might want to do some magic with the blood.

Ignoring the problem of the diminished autonomy of the schoolchildren-subjects, we are still faced with the geneticist's causing obvious distress to the local people. And while this anecdote seems to invite us to ridicule the ignorance of the ax-wielding savage, we might look at it instead as exemplifying two other problems about the scientist. First, the fact that the farmer is concerned about the possible magic indicates

that the scientist has not fulfilled his responsibility to explain the research to the subjects so as to obtain their full consent. Second, obviously the scientist is the source of some emotional unease; whether the farmer was a lone pathological actor or simply articulated a more widespread feeling we cannot say. However, there was clearly the feeling of harm and pain—spiritual, perhaps, but clearly no less real to the farmer—of which the scientist was the source.

Finally, by justice is intended the fair distribution of whatever benefits may accrue as a result of the research. There is little to add to the preceding discussion of the way in which contemporary patent law favors scientists. The issue with respect to indigenous people came to a head in 1996 with the filing of a patent claim on the cell line originating from the blood of the Hagahai man mentioned above. The sample had been collected by a medical anthropologist not affiliated with the HGDP who had actually attempted to broker an agreement that would have had the Hagahai comprehending what they were participating in and sharing in any financial benefits. In the wake of protests, the National Institutes of Health withdrew its patent application. But the story does not end there (Harry and Marks 1999:304):

Dr. Pauline Lane . . . has been working with the Hagahai to find out about their impressions of being subjects of a genetic patent. She tells us that there have been no long-term benefits to the community. . . . There have been no additional medical services or supplies provided to the Hagahai. With regard to community understanding, Lane further tells us, "The community members I spoke to were confused. They feel that Jenkins had helped their community, but they also felt that maybe they had been cheated out of some money for their blood. They did give informed consent for blood to be taken for diagnosis but NOT to be taken out of the country for research. They suggested that they would NOT trust researchers again."

Who could fault them for concluding that once researchers had what they wanted (HTLV-1 sample), then the Hagahai were no longer of interest? The Hagahai cell line is now available to the public at the American Type Culture Collection as ATCC Number: CRL-10528 Organism: *Homo Sapiens* (human) for \$216.

The HGDP fared exceedingly poorly using the ethical criteria generally in place for medical genetics. It is hardly a surprise, then, that a blue-ribbon panel concluded that while "a global assessment of the extent of human genetic variability has substantial merit and warrants

support" it nevertheless "foresees numerous ethical, legal, and human-rights challenges in the prosecution of a global effort" (National Research Council 1997:1-2).

### The Rights of Scientists?

The HGDP was designed by and for the scientific community. Its subject was one that the scientists were interested in: microevolution. The same panel acknowledged up front that the merit of the project was specifically that "the insight the data collected could provide into the origin and evolution of the human species" (National Research Council 1997:2). Or, as one of its advocates gushed during a meeting in 1996, "We're going to tell these people who they really are!" I am still stunned by the presumption that nonscientists do not know who they are and that whatever ideas they may have about their own identity and descent are somehow trivial and irrelevant.

This exposes another paradox in the HGDP. In order to muster initial support for the project, its proponents wrapped themselves in the liberal humanism of "no-race" rhetoric. In their summary document of 1993 (<http://www.stanford.edu/group/morrinst/hgdp/summary93.html>) they tell us:

The HGD Project will also provide the scientific data to confirm and support what is already clear from population studies—that, in biological terms, there is no such thing as a clearly defined race. . . . Most importantly, therefore, the results of the Project are expected to undermine the popular belief that there are clearly defined races.

This goal was very quickly criticized in two ways: first, if we already knew this (and we did), it was hardly a compelling justification for undertaking such a large-scale project; and second, the proponents' own work was often presented sloppily, lending itself to an interpretation that there were indeed real races, which could even be color-coded: Africans yellow, Australians red, Mongoloids blue, and Caucasoids green (Cavalli-Sforza, Menozzi, and Piazza 1995; Marks 2002).

More recently, the issue has been revitalized with the publication by the remaining participants in the HGDP, now working with a genetic repository based in France, of a paper in *Science* on the computerized genetic clustering of human populations (Rosenberg et al. 2002). When asked to divide the human genetic samples into two groups, the computer program dutifully did so, and likewise with three and four

groups. When asked to divide the world into five, it came out with Africa, Eurasia, East Asia, Oceania, and America, and when asked to divide the world into six, it separated out from those five the Kalash of Pakistan. On the face of it, this would seem to lend no support to popular ideas about race: the Kalash are hardly equivalent to the Africans, and there is nothing racially commonsensical about juxtaposing Eurasia–Africa with East Asia–Oceania–America. And yet, the take-home lesson of the *New York Times* was "Gene Study Identifies 5 Main Human Populations," and the HGDP's spokesman, Marcus Feldman, "said the finding essentially confirmed the popular conception of race" (Wade 2002).

Given that the "popular conception of race" has been disconfirmed many times over (Montagu 1963; Marks 1995) and that the article in question did not actually purport to confirm it, what is going on here? Perhaps it needs to be understood in the context of another widely publicized study, one claiming that race is genetically real and biomedically useful (Risch et al. 2002) and that the previous half-century or so of the study of human diversity is just so much fluffy political correctness. And yet, epidemiology seems to be roundly rejecting the genetic hypothesis for racial differences in health (Goodman 2000; Kaufman and Hall 2003). A cynical (realistic?) look at these geneticists' attempt to revitalize race might take note of the desire to rationalize target markets for the pharmaceutical industry (Duster 2003) now that federal funds seem to have dried up for the HGDP.

Once again, though, putting aside the question of whether human population geneticists have an inside track to truth or a good track record pursuing it (and I don't think they do), there is a more fundamental question underneath the scientific arrogance. Anthropology is a unique field in that it relies fundamentally on the goodwill of the subject matter toward the scientist. A chemist does not have to worry what about boron thinks of him. Without the goodwill of the people involved, however, there can be no anthropology, whether it consists of having people agree to be measured or bled, to have their ancestors excavated, or to be visited.

One of the most extraordinary suggestions to emerge from the Kennewick Man fiasco of the past few years (in which physical anthropologists took legal action to prevent repatriation of a ninety-five-hundred-year-old skeleton in Washington that they claimed was not the ancestor of Native Americans because they are Mongoloid and he was Caucasian [Preston 1997; Chatters 2001]) is the notion that the rights of scientists were thereby being violated. The Kennewick skeleton

was taken out of the hands of the scientists only after they had made plans to transport it from Washington, where it was the subject of a NAGPRA claim, to the Smithsonian, where it would be easier to retain control. Anthropologists at the Smithsonian had purchased airline tickets for the transportation of the bones away from the tribes claiming them and from the Army Corps of Engineers (Chatters 2001:74)—a point virtually never noted in popular renditions of the story. When that plan was thwarted, however, the scientists filed suit, arguing that “their civil rights were being violated” (Thomas 2000:xxi). In an opinion issued June 27, 1997, U.S. Magistrate John Jelderks noted that “the . . . plaintiffs filed suit . . . to enforce what they contend is a legal right to study the remains.” I certainly cannot blame the Native American groups who wished to bury Kennewick Man without letting science study him, when the scientists’ initial plans were to transport him to the Smithsonian without any permission so that he could be studied in comfort and repatriated with difficulty.

This is the issue on which the HGDP and Kennewick Man converged for me: Do scientists have the right to study whatever they want, without regard to the wishes or sensibilities of the relevant people? That question has, of course, been answered for all time in the wake of World War II. They do not. The advancement of science is important, but it must be weighed against the encroachment upon basic human rights. And today there is a fundamental human right under constant threat: *the right not to be a scientist.*

Science in the modern world can be a ruthlessly ethnocentric and intolerant ideology, many of whose most prominent representatives believe that they have a Victorian destiny to supplant religion. But being a scientist or doing science does not necessarily entail evangelism for science. After all, teaching or doing art history does not involve intellectually cloning art historians; it simply involves instilling an appreciation for what art historians have done and for the way they think.

A Smithsonian anthropologist defended the lawsuit on *Nova* (“Mystery of the First Americans,” February 15, 2000) with the thought that “a clear and accurate understanding of the ancient past is something that the American public has a right to know about,” as if he had it to offer! What links the HGDP to Kennewick Man is the cavalier fashion in which scientists and, in both cases, self-righteous, self-interested, self-proclaimed, and slightly paranoid advocates for science could rewrite origin narratives and identities of other peoples on the basis of partial, ambiguous, or dubiously interpreted evidence. In both cases, skeletal and genetic anthropology, the current generation faces a different social

and political landscape for the production of scientific knowledge from its predecessors’.

In the case of the HGDP one could legitimately ask why people would wish to participate in a project designed to denigrate and contradict their own ideas of who they are and where they came from. It is significant in this context to note that the peopling of the New World is now known, on the basis of genetic studies, to have occurred from Asia in *one* migration (Merriwether, Rothhammer, and Ferrell 1995; Kolman, Sambuughin, and Bermingham 1996), *two* migrations (Karafet et al. 1999), *three* migrations (Greenberg, Turner, and Zegura 1986), and *more than three* migrations (Szathmari 1993; Torroni et al. 1994).

Neither the HGDP nor the Kennewick Man battle casts science in a particularly favorable light. Both call attention to scientists’ belief they have a right—perhaps even a duty!—to delegitimize other people’s ideas about who they are and where they came from. There is, in fact, no compelling reason to think that Kennewick Man and his contemporaries are any other than ancient Native Americans and the ancestors of modern Native Americans (Eshleman, Malhi, and Smith 2003).

This is not to say that science should not be interested in such things. Bio-history is a patently approachable question scientifically and an entirely legitimate venue of scientific inquiry. As does any scientific program, it advances unsteadily and makes several errors for every achievement (one may recall the many wrong DNA structures, including Linus Pauling’s three-stranded model, before Watson and Crick’s publication, and the reluctance of many early molecular geneticists even to accept DNA as the genetic material when proteins were so ubiquitous and so diverse).

The question is: How, then, does one honestly secure the participation of the people whose ancestors, relics, relations, or blood one wishes to study when the research agenda is constructed to undermine their beliefs? Who would want to participate in such a study, if its purpose were as candidly disclosed to them as it was to the scholarly audience—telling these people who they “really” are?

Presumably securing the participation of people in such a microevolutionary study requires either risking the withdrawal of its most valued subjects by explaining the hegemonic goal of discerning who they “really” are and where they “really” came from, or else simply not disclosing it fully. This is of course a problem, for there is a recognition of some degree of tolerance of and respect for other beliefs and lifeways in this work, but whether it is operationalized in any way or is merely lip service is unclear.

Thus, the HGDP's 1993 "Summary Document" notes under "Field work issues" that "customs and traditions of participating communities must be respected at all times." But this is noted only in the context of procurement. After the substance changes hands and becomes a laboratory specimen, there is no acknowledgment of the need for such respect. The laboratory is a privileged site, it would seem, where the concern for biochemical toxicity and radiation may need to be acknowledged but otherwise the science is unfettered by the apprehensions and ideological conflicts that it might be causing.

### **Creationism and the Right Not to Be an Evolutionist**

Another area in which biological anthropology interacts with public sentiments, often with hostility and authority, is the general presentation of human origins. Obviously the scientific origin narrative, evolution, more accurately represents "how we got here" than other origin narratives. And yet one finds repeatedly in creationist rhetoric a theme that should be familiar to anyone knowledgeable about the anthropology of myths—that there is more to them than simply the details of the story (Numbers 1992; Toumey 1994).

Once again, genetics is at the center of the storm: bodies are built clonally from a single zygote; the zygote contains all the information necessary for the process of development; genes constitute that information; different genes build different organisms; alternative forms of genes (alleles) differ from one another in specific and knowable ways; alleles spread through a population over generations by a small number of fundamental processes; and these processes work to transform a population over long periods of time. All of these are parts of the scientific canon, and all are unimpeachable except in very narrow and highly exceptional senses. With a basic understanding of the processes, of course, we try to reconstruct the details of our evolutionary history—limited by the fossil record and our creativity in making sense of it (Landau 1991).

Yet creationists, by and large, do not want to argue about the stochasticity of genetic drift or the six-million-year-old biped *Orrorin tugenensis*. They are, for the most part, interested in more fundamental issues of whether the universe is governed by a benevolent hand, morality, ultimate justice, good and evil, happiness, and what lies beyond death. These are issues to which science and especially evolution have little to contribute, and yet they are the very issues that all origin myths deal with to some extent. Origin myths are culturally integrated to a far

greater extent than science is. Science's standard operating procedure is to take some aspect of new knowledge and to substitute it for whatever alternative existed before it—generally without looking for or dealing with the broader implications or cultural and symbolic connections (but, for an important exception, see Simpson 1949).

With respect to evolution, there seem to be three responses to this. Some seek spirituality through evolution (Teilhard de Chardin 1959; de Duve 2002); others invoke evolution specifically to reject spirituality as illusory (Dawkins 1995; Avise 1998; Wilson 1998), and still others respond by denying evolution and seeking to undermine it as reliable knowledge (Behe 1996; Johnson 1998; Dembski 2002). It is in this latter group that we generally find the "creationists," who strive to introduce some version of biblical literalism into public school science curricula.

Suffice it to say that introducing creationism into the science curriculum as an equivalent or even relevant alternative to evolution is quite simply fraudulent. And yet there is a different side to this coin—namely, that the comparative study of worldviews and origin narratives is a traditional focus of anthropology. Further, an application of some of anthropology's classic relativistic humanism should imply a modicum of respect or at least tolerance for creationism—*not, of course, as a representation of the content of field of biology* but as part of the anthropology curriculum, whose very reason for existence is the ultimate wisdom that comes from encountering or experiencing the breadth of ideas that people have and the cultural logic that guides them. And if we agree that creationism should be acknowledged and engaged (rather than accepted or combated), then what better locus for it than biological anthropology, a field that self-consciously constitutes the boundary between natural science and systems of thought?

### **Conclusions**

I once flew home from the physical anthropology meetings on a puddle-jumper next to a distinguished senior colleague. I had just given a paper critical of the Canadian psychologist J. Philippe Rushton (e.g., 1995), who argues that black people have small brains and large penises, which he freely translates to mean stupidity and licentiousness; that yellow people are the opposite; and that white people have evolved a happy medium. The physical anthropologist with whom I sat took the liberty of upbraiding me for my paper. He said, "You see, you're too critical, you're too contentious."

I said, "Don't we as scholars and as anthropologists have a moral obligation to combat racism?"

He said, "I don't believe in morality."

*Was I sitting next to Darth Vader?* I asked whether we could begin by at least agreeing on the proposition that racism is evil.

He said, "I don't believe in good and evil. Those are constructions"—quickly becoming a relativist of convenience.

I said, "I agree they are constructions, but that doesn't mean they're the same thing."

He said, "You see, that's your problem, Jon. You're causing trouble by criticizing that work."

I replied, "You've got it backwards. The racist is the one causing the trouble; the problem is racism—not the critique of racism."

He told me I just didn't understand, and at least he was right about that. But there is one thing I do understand: I certainly would not want my children to study with him.

Obviously, a field in which one can have such a conversation with a senior representative is probably unlikely to be a leader in ethical practice. Physical anthropology's finest hour arguably came in 1963, when the field (led at the time by Sherry Washburn) coalesced against the racism inherent in Carleton Coon's (1962) interpretation of the origin of races, which was being invoked by segregationists with the author's blessing (Marks 2000; Jackson 2002). While some of the issues linger, it faces new moral and ethical challenges today.

One of the major recent ethical triumphs in this area has been the repatriation of Sarah Baartman, "The Hottentot Venus," whose genitalia had been dissected by the leading anatomist in Europe and whose body parts had remained for nearly two hundred years in the Musée de l'Homme in Paris. The "old" South Africa was not interested in the remains of Sarah Baartman, but the "new" South Africa was—she constituted both a complex national/racial/political symbol and a real-life person with descendants, relatives, and newly empowered co-ethnics. This was obviously not covered by NAGPRA, nor did it sit well with museum people generally—certainly the Louvre would not want to set a precedent that could result in the Mona Lisa's being returned to Italy. And yet her remains were of no scientific or aesthetic value, nor were they on display—why keep her? Ultimately it required a personal appeal from Nelson Mandela, years of political maneuvering between the parliaments of France and South Africa, and a series of negotiations between the South African physical anthropologist Phillip Tobias and his French counterpart, Henri de Lumley (Tobias 2002). She was repatriated in May 2002.

It seems to me that there has to be more to the relationship between geneticists and indigenous peoples than simply trying to talk them out of their precious bodily fluids (the phrase is from the film *Dr. Strangelove* and curiously appropriate here). Any operative idea of full disclosure should entail not only the scientific issues involved but as well the possible financial benefits to the scientist. And, more to the point, there should be some exchange between the people and the scientist to clarify the value to them of the research question to which they are contributing.

The many physical anthropologists who work in an atmosphere of respect and amity with indigenous groups have been given a black eye by the arrogance and belligerence of the few who re-create exploitative or insensitive relations with local communities. Modern research in human population genetics certainly does little to mitigate the widespread fear among traditional and impoverished people that Americans want their blood or body parts (Scheper-Hughes 1996). For a field that began as little more than simple grave-robbing, this is perhaps not unexpected. But an anthropology that treats people contemptuously—as represented by the high-profile HGDP and the Kennewick Man combatants—will make it hard for the rest of the field to survive honorably into the current century.