
THE PROBLEMATICS OF "RACE" IN CONTEMPORARY BIOLOGICAL ANTHROPOLOGY

Alan H. Goodman

ABSTRACT

Race is an "under-problematized" and "under-theorized" construct in biological anthropology despite the fact that 50% of polled biological anthropologists in 1984 believed (or did not believe) it to be a "valid and useful concept." A brief history of human race is reviewed in which it is maintained that "race" became reified by constant use, it became conflated with human variation, and it was (and is) politically useful to those in power. Arguments for the scientific and epidemiological problematics of race are summarized, and especial attention is paid to its use in skeletal biology/forensic osteology, nutrition and medicine, and the new human genomics. The assertion is made that there is no scientific reason to continue using the concept of race in biological anthropology.

KEY WORDS:

Race, Population Variability

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INTRODUCTION

"Current-day anthropologists recognize at least three major races, namely, Caucasoid, Mongoloid, and Negroid, which differ from each other with respect to skin color, facial structure, hair form, body proportions, and morbidity and mortality patterns" (Hinkes, 1993:48)

"Because most associations between disease and race have no biological basis, race—as a biological concept—is not useful in public health surveillance" (MMWR # 42, 1993:12).

Biological anthropologists under-problematize race; they do not give much thought to how the use (or lack of use) of race as a key concept reflects influences of power and ideology, is related to one's social position and training, and how the use of the concept is a political statement. Furthermore, little effort is put toward critical analysis of the idea of race and how one's relationship with the idea further influences the choice of research questions and the results of one's research. That race is under-problematized and "undertheorized" is appalling, especially in a discipline, biological anthropology, which is specifically about the nature of human VARIATION.²

In addition to under-theorizing race, I contend that even more obvious is the fact that biological anthropologists, myself included, have simply avoided discussion of our conceptual models of human variation.³ How must this look to the world beyond the professional biological anthropologist? On one level, it must look like what it is purported to be—a non-problematic. But, I suspect to a keener observer it may appear as a sort of *laissez faire* schizophrenia. Race is believed to be real by some but not by others; it is useful and used in some branches of biological anthropology, but not in others.

This schizophrenia of belief and use could simply be a healthy diversity of opinion and a realization that some concepts work well in some areas and not so well in others. Unfortunately, however, for heterogeneity to be healthy one would expect a great deal of discussion and argument about what is proper use of a concept. Witnessing the heated debate between proponents of competing models for the origin of *Homo sapiens* (a topic referred to below), makes obvious that biological anthropologists are not shy about conflict. They do not shrink from challenging each other on many counts from description and interpretation of fossil remains to theoretical models. Yet, when it comes to contemporary human variability, it is as if we are in a Kuhnian period of "normal science." Research goes on as usual, when, in fact, we are in the exact middle of an incomplete revolution between two conflicting world views. For a discipline that tries desperately to be taken as a science this ambiguity is problematic. More important still, the lack of a developed consensus, or even of a clear sense of points of disagreement, damages the public image of biological anthropology.

Evidence is frequently cited that the "non race" world view is now dominant, at least in cultural anthropology. This is documented clearly in the scholarly research of Lieberman and colleagues (1989) who show a progressive decline in belief in the salience of the concept of race among successive generations of cultural and biological anthropologists. How-

ever, what is not frequently focused upon is the result that exactly half of the polled biological anthropologists in 1984 believed that the concept of race was valid and useful. Furthermore, I have suggested elsewhere that the first reversal of the trend toward a decline in the use of race may be occurring in the early 1990s (Goodman and Armelagos, ms). The revolution is incomplete. Given this backdrop, given that the concept of race has been so intimately linked to biological anthropology, and given that how biological anthropologists use and theorize race has such important sociopolitical and economic ramifications, a serious discussion of the use and relevance of race seems to be long overdue.

It is hoped that this paper is one step in opening up this discussion. In the following I first briefly review the historical development of the idea of race and its continuation in natural history and anthropology. I then provide a capsule summary of arguments for the scientific and epidemiological problematics of race. Following Goodman and Armelagos (ms) I then outline trends suggesting a resurrection of race in biological anthropology in context of broader trends in the sciences and society. Diverse areas of biological anthropology that are focused upon include: (1) studies in skeletal biology and forensic osteology; (2) biological anthropology studies in nutrition and medicine; and (3) the new human genomics, including efforts to understand human evolution using mtDNA and more inclusive and exhaustive trends in genomics, chief of which is the Human Genome Diversity Project (HGDP). I end by asserting that there is no scientific reason to continue using the concept of race in biological anthropology. It should be eradicated from our scientific vocabularies. Thus, the re-emergence of race in biological anthropology must be tied to extra-scientific issues. I do not expect everyone to agree with these conclusions, especially without further discussion. Rather, I write this paper in hopes of opening up discussion among biological anthropologists and others who may offer insights.

A BRIEF HISTORY OF RACE

"A popular political statement now is "there is no such thing as race." The visible differences between different populations tell everyone that there is something there" (Brues, 1993:76, emphasis in the original).

"The tendency has always been strong to believe that whatever received a name must be an entity or being, having an independent existence of its own and if no real entity answering to the name could be found, men did not for that reason suppose that none existed, but imagined that it was something peculiarly abstruse and mysterious" (John Stuart Mill [1806-1873]).

The concept of race developed from the Greek idea of a great chain of being and the Platonic notion of ideal types (Lovejoy, 1936; Stepan, 1982). For Plato, the observed world included objects that differed from ideal and pre-ordained types of objects. Western Christianity combined the notions of ideal types and the "great chain of being." Christian Europeans unambiguously occupied a step closer to angels and god (at the top of the chain) than non-Christian, non-Europeans. The world of this time, and the developed concepts,

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were static, idealist, and non-evolutionary. This worldview provided the ideological basis for the concept of race—that humans are divisible into a discrete number of fixed and unchanging human types.

The above notwithstanding, there was seemingly little officially sanctioned and scientific concern over differences in humans, and especially in thinking these differences to be innate. This changed when Europeans began to develop trade routes and expand markets. At this point the idea of race became useful as a means of justifying European capitalist expansion and the natural science of human diversity became an enterprise. With it was born a theory of racialism⁴, the belief that humans are and always have been divided into a fixed and discrete number of human races (types). The goals of this science, racist human variation, were to describe these types and demonstrate their unique behavioral and biological characteristics.

This science starts with efforts at classification and explanation by Buffon in 1749 in his *Histoire Naturelle* and Linnaeus in 1758 in the tenth edition of his *Systema Naturae*. It extends through the writings of other French, German, English and eventually United States natural historians, who tinkered with these classifications and began to consider why variation existed. For Linnaeus, race explained⁵, or at least correlated with, systems of government and psychological characteristics. In addition to classifying a "monstrous" species of *Homo* with various subspecies types, and a wild man (*ferus*) type of *Homo sapiens*, Linnaeus noted four racial types — *americanus*, *europaeus*, *asiaticus*, and *afēr*. He decreed that *americanus* is regulated by custom and paints himself with fine red lines; *asiaticus* is haughty and covetous and governed by opinion; *afēr* is indolent, anoints himself with grease, and is governed by caprice, and *europaeus* is genteel, acute, inventive, and governed by law. These clearly prejudiced convictions were excluded from subsequent human taxonomies, surely so as to make them appear biological and objective. Yet, Linnaeus's initial descriptions are illustrative, not of showing his particular prejudices, but the prejudices of the time during which the biological concept of race developed.

From this time on race became widely used by scientists and politicians, and became a popularly recognized idea, so much so that it was taken to be real. It filtered into languages and etched itself on the minds of eighteenth to twentieth century Europeans (Stepan, 1982; Smedley, 1993; Todorov, 1993). While the exact processes by which a scientific idea becomes a "thingified" popular reality is surely variable from person to person and place to place, it is clear that ideas that are useful to the ruling class (with control of legislation, access to information, etc.) tend over time to be accepted as doubtless, certain, natural, real.

The concept of race was and remains a typological and non-evolutionary concept. It holds on today despite the fact that the notion of fixed, ideal types should logically have been replaced well over 100 years ago with the advent of Darwinian and evolutionary theory in biology and anthropology. I suggest three interrelated reasons for the continued use of race: (1) race became reified by constant use, (2) it became conflated with human variation, and (3) it was (and is) politically useful (Fields, 1990).

ROOTS: RACIALISM IS ANTHROPOLOGY, ANTHROPOLOGY IS RACIALISM

Biological anthropology has had a long and close association with racialism. Franz Boas being the main exception, until the Second World War anthropology was the study of race. An example of this close relationship is J. Deniker's (1904) general anthropology text. By its title, *The Races of Man: An Outline of Anthropology and Ethnography*, the conflation of race and anthropology/ethnography is transparent. In fact, nearly everything that is conventionally considered anthropology—comparative and evolutionary approaches to biological variation as well as human custom, religion, myth, political institutions, and language—is subsumed under the study of races. It should be noted that Deniker identified ten races in Europe alone.

While others, later in the first half of the century, might not so widely conflate racialism and anthropology, it would be extreme revisionism to not acknowledge the importance of race in anthropology. The sway of racialism, and more broadly of biological determinism, is almost always clear. For example, E. A. Hooton, the advisor to a generation of biological anthropologists at Harvard, while decrying the obvious racism of the great chain of being, continued to use race as a taxonomic tool and continued to make generalization on differences among races, including that "we are fairly safe to assume that the Australian is far less intelligent than is the Englishman" (Hooton, 1946:158). What perhaps is most evident in the writing of Hooton and other major figures in biological anthropology between the 1920s and 1940s, was their lack of comfort with typology and problems in fitting the data to typological notions. Nevertheless, few saw beyond typology, and how they were constrained by the reification of race (Blakey, 1987; Brace, 1982). One who did and became an outsider in his own discipline, Ashley Montagu, called race a myth, man's most dangerous myth, and the phlogiston of his time (Montagu 1962; 1963; 1964).⁶

A few notable exceptions aside, Carleton Coon (1962) being chief amongst them, race, as Lieberman and colleagues (1989) demonstrate, began to leave the anthropological and scientific lexicon after World War II. However, this survey work, as well as a perusal of the literature, makes clear that the idea of race, injured as it may have been, never disappeared. Today, the popularity of *The Bell Curve* (Herrnstein and Murray, 1994) and the thesis that intelligence and behavior is explained by race (Rushton, 1995), is a clear reminder.

More serious is the fact that Rushton's research and writings are just the extreme, the tip of the iceberg of racialism; at least Rushton's position on race is clear. What may be more important is the ease with which the concept of race is able to hang on in both popular and scientific discourse. Muir (1993) has recently made the distinction between "mean racists" and "kind racists." Mean racists aim to hurt; kind racists do not, but they continue to define humanity in racial terms. By doing so, kind racists play into the hands of mean racists by maintaining, perhaps unwittingly, the ideological grounds for mean racists.

THE SCIENTIFIC MYTH OF RACE

The decline in popularity of the concept of race is commonly held to be due to changing politics, including an influx of liberal, Jewish and women anthropologists after the Second

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World War (Barkan,1992). True as this may be, there are also profound and fundamental scientific reasons that speak to the scientific invalidity and epidemiological problematics of race.

From the outset it should be clear that there is no reality of race. Thus, when Brues says "there is something there" (see quote above), it should be clear that what is there is diversity, "good old human variation." Race is "only" a melded social and scientific construct, a worldview and means of conceptualizing (and explaining) human variability. We tend to think of race as a reality only because we live in a society in which race matters (West, 1993) and in which race has become reified by its constant use and lack of questioning of its underlying reality.

Because race is a worldview, and because worldviews differ, definitions of race are varied and protean. For example, some classifications are based on geographic origin (with some assumed biological concordance), others are based on clusters of traits, and others still are based on bureaucratic and social definitions (again, with an assumption of a biological basis). Nobody has yet agreed on a definition (Brace,1982). In fact, they range all over the place. Furthermore, all efforts at a scientific (widely accepted, reliable) definition have proven to not be implementable. Brace captured the assumptive and protean nature of race when he commented on racialist research:

"The connection between the biology discussed and the races named at the end is never clearly spelled out, and in fact the attentive reader cannot discover, from the information presented, just how the racial classification was constructed—other than the fact that this just seems to be the way anthropologists have always done things" (Brace 1982:21).

With differences in definition, and no clear criteria for what is and is not a race, it is not surprising that there is also no agreement on the name and number of races. As seen in forensic anthropology, the powerful act of naming and numbering has been left to politicians and others without knowledge of human variation (Lee,1993). Thus, St. Hoyme and Iscan (1989) justify the work of forensic anthropologists as just identifying a John Doe's "bureaucratic race." The inability to define race, the inability to agree on how many there are, and the inability to agree upon what biological criteria make a race, shows that this concept is slippery at best, making for problematic politics and biology. The following is a further outlining of why race is not a useful shorthand for human variation.

1. Race as type is inconsistent with evolutionary theory; whereas race is a static concept, evolution is ongoing. While the pace of natural selection might be slow, marriage and mating patterns are rapidly changing the structure of the human gene pool. Thus, what may have been true in the 1800s may not be true today. Moreover, because race is unstable, results based on race are unstable. Thus, a study of racial differences in bone mass from Cleveland, Ohio in the 1920s may have little to no applicability in the 1990s in Cleveland or in any other location. Worse still, because the criteria for assigning race are seldom provided in a repeatable way, we cannot know whether they are applicable.

2. Most traits are *continuous varying* and clinally distributed (Livingstone, 1962). That is, frequencies, averages, and other methods of describing traits change in a multitude of increments from one predefined group to another. In fact, there are typically no clear borders between when one group begins and another ends. It is like classifying individuals based on their weights. If we determined that there was to be two groups, then where would one make the division between heavy and light people? It could be at 75 kilograms, or 170

pounds. The "cutoff" point is arbitrary, a matter of convenience, and those near the cutoff are more like each other than they are like others in their group.

Similarly, it is impossible to fix boundaries between races. There are no natural gaps. The division point is arbitrary and up to the whim of the classifier. Thus, what might be classified as European" or "white" at one time and place is classified as "mixed," "Hindu," "quadroon," "octoroon," "colored," "mulatto," "mestizo" or "black" at another time and place. Moreover, this classification of a continuous trait into discrete units diminishes the complexities and true nature of human variation. Over thirty years ago Livingstone (1962) noted that the concept of race was nearly totally replaceable by study of evolutionary processes and more accurate methods for describing human variation (such as geographic clines).

3. Most trait pairs are *nonconcordant*, that is, traits tend to vary in different ways. The significance of this fact is that knowing the distribution of one trait can rarely explain or predict the distribution of a second. For example, knowing skin color provides no insight into height or any other anthropometric attribute. Why should it? These traits are under different evolutionary pressures. They are not packaged together. This is why race is only skin deep.

4. Within-group variation is much greater than between group variation. There is so much variation within any purported race, about 94% of total diversity based on Lewontin's (1972) calculations from blood group frequencies', that extrapolation from the group to the individual is essentially meaningless. Because of nonconcordance and within group variation, race has little explanatory power. If we know race, we know little more. It tells us little about the processes governing human variation and it has trivial predictive value for knowing something about individuals.

5. The last and most important criticism of race is that its use, as an implied quasigenetic measure, encourages the conflation of genetic with plastic influences. This is perhaps most evident in studies of morbidity and mortality, reviewed below.

Realizing that race is not a useful concept for study of human biology, and that human variation is not reducible to a fixed number of types, does not preclude the importance of ethnicity or other social classifications. In fact, it is clear that race is such a classification. What I wish to raise concern about is not the classification itself but the implied genetic basis of the classification. As should be clear from discussion on the use of race in medical research, the conflation of the social and genetic in race-based medical statistics is a great deterrence to research. Finally, the decoupling of race as a proxy measure of genetic influence from social groupings does not imply that oppression and racism do not exist. African American babies die at a rate that is over twice the white baby rate (Polednak, 1991; Kempe et al., 1992) not because of bad genes, but because of institutional and other forms of racism. Over a century ago Darwin said "if the misery of our poor be caused not by the laws of nature, but by our institutions, great is our sin." So too, we need to be clear as to whether race differences in morbidity and mortality are due to biology or institutions.

RACE IN CONTEMPORARY BIOLOGICAL ANTHROPOLOGY

FORENSIC ANTHROPOLOGY⁸

One of the fundamental goals of skeletal biology and its daughter field of forensic anthropology is accuracy with regard to the demographic characterization of individuals and

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groups. Parts of this characterization include the assignment of race, among other key biological facts. Perhaps unlike other areas of biological anthropology, the idea of race never came close to dying out in forensic anthropology. A bibliography of the field through 1992 includes over 150 references on racial affiliation (Galloway et al., 1993). Without surprise some of these date to the 1800s. Conversely, many have been published in the last three years.

While forensic anthropologists lament that due to migration and intermixture it is more difficult to assign race now than in the past, with the possible exception of Sauer (1992, 1993) the reality of races seems never to have been seriously questioned. In most cities in the U.S. the dichotomy of white and black is no longer as obvious as it once seemed. Asians, Native Americans, and various Hispanic groups make less certain the work of assigning race to a skeleton. The American "melting pot" makes the job of assigning race harder (St. Hoyme and Iscan, 1989). In all of this the validity of the paradigm of human races is unquestioned. Meanwhile, Brues (1993) exhorts that to believe that race is not real is just a political position.

Forensic research articles on the determination of race are relatively uniform—race is known from some form of documentation (such as a researcher's observations or death registration) and multivariate techniques are used to discriminate among two or more racial groups (Thompson, 1982). Forensic reports typically involve completing a series of measurements and observations and then estimating race based on fit to a formula. Often a set of measures and observations works relatively well at predicting race and invariably, if so tried, the technique is shown to work somewhat less well on an independent sample (Brues, 1992; Sauer, 1992). Whether a particular set of measures statistically discriminates groups because of inherent differences, environmental factors, or gene-environment interaction is infrequently addressed (Iscan, 1983).

How do forensic anthropologists learn to not problematize race? In order to consider the act of learning to see human variation in terms of typologies we take as an example the catalog of a widely used educational service that provides reproduction casts of a variety of human and nonhuman features for use in teaching and research.

Their most recent catalog (Fall, 1992), now twenty pages long, includes descriptions and costs of casts for sex and age determination (2 pages), a human crania series (4 pages), pathologies, abnormalities and miscellaneous (4 pages), and primates (4 pages). From the size of its catalog and its presence, usually with a prominent display table at meetings such as the annual meetings of the American Association of Physical Anthropologists, France Casting appears to be prospering and expanding.⁹

What does this company choose to cast and what do they say about their material? What is the theory implied in the message to be disseminated to new students, those who casually examine museum displays, and those who wish to go on to do work in the knowledge industry of biological anthropology? What can we infer from an examination of a few entries into the crania series?

The first skull listed on the second page of the skull section is labelled in bold capital letter "NEGROID MALE." The description of the skull states that it "illustrates racial traits very well." The skull below it is labelled "CAUCASOID FEMALE" and its description is similar: "illustrates racial traits very well." The reinforcement of racial types is unambiguous. Essential information is how well each of these skulls fits the ideal racial type of negroid and

caucasoid

The message implied in the description of other casts is superficially more opaque. Two skulls on the first page of the crania series are labelled "PROBABLE NEGROID MALE" and "PROBABLE NEGROID FEMALE" and a skull on the opposite page is listed as "PROBABLE CAUCASOID MALE." The probable negroid male was "excavated from James Island, on the coast of South Carolina", and is "thought to be derived from a Guinea, West African population." ... "the cast nicely illustrates regional traits from this area of the World." Apparently there is no documentation for this skull, thus the "probable."

Why, with thousands of better documented skulls, was one chosen without documentation and that is, according to its description, in such a poor state of preservation: separated squamosal and sagittal sutures, a cracked and separated malar, broken zygomatic arches, and a missing mandibular condyle? Is it difficult to find these ideal racial traits in a documented skull? Could it be that if one decides that certain features are "typically negroid" then the skull must be negroid; what is decided to be negroid in appearance is therefore negroid? The documentation, either social or biological, is then secondary in importance. Is it true, as a forensic anthropology colleague has told me, that "one can just tell (race)?"

The essentialism of characteristics extends from the biological to the sociopolitical. The first presented crania, the only one with a picture on the first two pages, is of an "American Indian Pueblo Male." It is described as illustrating scalping marks. I assume that the company thinks this will be of great interest to students. But is this not just playing even more deeply into stereotypes? Are scalp marks the most important thing we need to know about an American Indian skull?

What is the take-home message? From our reading, and I believe even more so from the naive student's reading and use of this material, human variation is reduced to how well it fits ideal types. Worth noting is how well individual crania fit what is assumed to be typical for their racial type. Students do not learn about the continuous and nonconcordant nature of human variation. They learn nothing of the complexities of biology. Rather they are sold a comfortingly simple story: there are old and static ideal types and with a minimum of training one can play the game of fitting the crania to the ideal type. In spite of contestations of belief in evolutionary theory, this is solid typology. Plato would be proud.

This is in fact the game that is played today by forensic anthropologists because that is what law enforcement agencies want (St. Hoyme and Iscan, 1989). It is the forensic anthropologists' goal to provide "bureaucratic race," that which is officially recognized (St. Hoyme and Iscan, 1989). But, bureaucratic races change and they may have little to do with biology (Lee, 1993). Interestingly, Brues (1993) apparently believes that the devotion to just providing race is scientific and non-political, whereas belief that there are no races is just "a popular political statement" (see quote above). Conversely, Lee (1993) cogently makes the opposite argument in documenting the political pressures that are involved in becoming an "official race" in the United States. As well, Lee (1993) documents the instability of what is considered to be an official race and the extreme lack of concordance with biology. Clearly, the "I'm just doing my job" approach is political, and suggests strongly that the ideology is accepted from above.

I suggest, despite the long history of attempts at assignment (see Galloway et al., 1993; Gill and Rhine, 1990; Gordon, 1993), that race is not an essential concept for forensic anthropology. The rhetoric of racial types could easily be changed to continental ancestry without

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affecting law enforcement efforts. More importantly, the applied goal of forensic anthropology is to describe as well as possible how individuals looked and other aspects of their biologies. To think one has done this by plugging data into an equation and degrading the information to an estimate of racial affinity is wrong and misleading. What is meant by race varies from individual to individual, from institution to institution, and over time (Lee, 1993), thus leaving interpretations open to uncontrolled generalizations. Perhaps we can do better by going back to description—to describe facial and postcranial architecture and other identification keys—and to keep it at this without need to add on a racial category that everyone interprets differently. Clearly, as well, we need to develop more coherent theories to explain skeletal form. Finally, to acquiesce to law enforcement needs — without educating them — misses a key educational moment.

RACE AND MEDICINE

“When race is used as a variable in research, there is a tendency to assume that the results obtained are a manifestation of the biology of racial differences; race as a variable implies that a genetic reason may explain differences in the incidence, severity, or outcome of medical conditions” (Osborn and Feit, 1992:275).

Racial differences in morbidity and mortality are hot topics. In the last few years the National Institutes of Health inaugurated a new “Research Center on the Psychobiology of Ethnicity” to study how different groups respond to medications (Holden, 1991), and a journal titled *Ethnicity and Disease* was launched to foster the study of and spread of information on aspects of the intersection of human variation and disease (Cooper 1991). The *American Journal of Human Biology* recently featured a symposium on “Ethnicity and Disease” (Szathmary and Siervogel, 1993). Nearly every leading medical journal, including *JAMA*, *Lancet*, *American Journal of Public Health*, *American Journal of Epidemiology* and *New England Journal of Medicine*, has recently highlighted articles on racial disparities in health. Concerned about the use of race and ethnicity in medical research, the Centers for Disease Control and Prevention (CDC) recently convened an expert workshop on the “use of race and ethnicity in public health surveillance” (MMWR, #42 1993). Is race a useful way to think about human biological variation in studies of morbidity, mortality and health care?

At least two fundamental problems repeatedly arise when assuming that the measured race differences in disease rates are biological and can be generalized to a racial propensity or predisposition. First, the environment is rarely controlled for. Second, the results once assumed to be genetic are reduced to the equating of genetic with pan-racial. Thus, one is often faced with the double leap of faith that a disease is genetic in etiology and that genetic equates with a racial-genetic predisposition.

A recent paper titled “Transitional Diabetes and Gallstones in Amerindian Peoples: Genes or Environment?” (Weiss, 1991) illustrates this problem. By the title, “... Genes or Environment?” the author makes clear that he purports to test whether high rates of disease are the result of genes or environment. Of course the dichotomy of genes or environment is a false one, and the author is surely using this for pedagogical purposes. Yet, aside from this point of simplification, how balanced is the analysis? One paragraph is devoted to environmen-

tal etiology and ends with the sentence "Many potential confounding factors make these results difficult to interpret" (Weiss, 1991:111). The author proceeds, having dismissed environmental etiology, to discuss at length and in very optimistic tones preliminary research that show weak correlations between genetic markers and diabetes rates, not at all questioning the correlative nature of the research. Data notwithstanding, the notion that disease such as diabetes, gallstones, and obesity are high in Native Americans because of a genetic predisposition is becoming reified by the development of the term "New World Syndrome" (Weiss et al., 1984).

Research on race and anemia provides a further example of the extreme public health implication of assuming group differences are real and pan-racial. In the 1970s Garn and colleagues presented data on the distribution of hemoglobin levels in blacks and whites in the United States. They reported an approximate 1.0 g/dl mean difference (blacks less than whites; Garn et al., 1974, 1975; Garn, 1976). Following this work the suggestion was made to institute separate cut-offs for anemia for blacks and whites, a suggestion that is still widely supported (Pan and Habicht, 1991).

Robert Jackson (1990; 1992; 1993; R. Jackson and F. L. C. Jackson, 1991) has reexamined some of these same data and has introduced new data. He has endeavored to control, as much as is possible, for obvious environmental factors such as iron intake, and to eliminate from analysis low hemoglobin values that may be related to genetic anemias. What Jackson and Jackson find is that the mean hemoglobin difference between blacks and whites is reduced to the 0.2-0.3 g/dl range when these obvious environmental factors and hemoglobin variants are controlled for. Furthermore, inconsistent variation between black and white infants, for example, where black infants had higher hemoglobin values before six months and white infants higher values after six months, does not suggest genetic etiology (Jackson, 1993).

Despite these data even very knowledgeable researchers such as Pan and Habicht (1991) continue to call for separate hemoglobin cut-offs for classification of anemia in blacks and whites. What are the policy implications of separate cut-offs? If the black cutoff is reduced just .5 g/dl, from 12.0 g/dl to 11.5 g/dl, half the difference proposed by Garn et al. (1974), the prevalence of anemia in nonpregnant, nonlactating black women (18-44 yrs) is estimated to be reduced "on paper" from twenty to ten percent (Pan and Habicht, 1991).

Yet still, separate cut-offs are supported despite the fact that the purported "race" difference in iron metabolism has no known genetic basis, especially one that suggests that blacks are uniformly more efficient than whites in their metabolism of iron, or that they somehow do just as well on .5 g/dl less hemoglobin. Nor has it been proven that the difference is pan-racial. Moreover, the issue is more than a theoretical one: separate cutoffs lead to profound health implications when one considers some of the functional consequences (in learning, work, and immunological capacity) of low hemoglobin values in ranges near anemia cut-off values (Scrimshaw, 1991).

The CDC's report on the use of race in medical and epidemiological research makes clear that the conflation of race with genetics, as well as the lack of clarity over whether race differences are reflective of genetic or nongenetic factors, is a serious constraint to public health (MMWR #42, 1993). Among its conclusions are that "Because most associations between disease and race have no biological basis, race—as a biological concept—is not useful in public health surveillance." (MMWR 1993:12). Further, racial categories are too

broad to be meaningful, there is no clear definition of race, the OMB Directive 15 (which delineates the racial categories for federal agencies) has no scientific basis, distinctions between race and ethnicity are unclear, concepts of race change over time, and their meanings differ among individuals. The CDC report goes further still in its conclusion that emphasis on race in public health reinforces stereotyping and racism and diverts attention from underlying socioeconomic factors (MMWR, 42, 1993:12-13). Somewhat conversely, it may be useful to maintain race as a social construct and as a means to monitor the health consequences of racism.

In summary, there are conflicting reasons for studying "race" in medical research. One group of scholars finds race to be a convenient shorthand for human (genetic) variation. This approach is old and established. Witness the fact that Medline, part of the National Library of Medicine, still uses subject heading such as "Negroid Race," "Caucasoid Race," and "Racial Stock." Witness as well the fact that 64% of all research articles involving human subjects published between 1920 and 1990 in *American Journal of Epidemiology* had some reference to "race" (Jones et al., 1991).

I agree with the CDC finding that this untheorized use of race is extremely problematic (also see Dressler, 1993; Hahn et al., 1992; Hahn, 1992). The implications of the undertheorizing are that differences are due to genetics, and this approach reinforces a form of victim blaming. Yet, the 2.4 fold relative risk of infant mortality of black babies versus white babies in the U.S. cannot be explained by genetic predisposition (David and Collins, 1991; Hogue and Hargreaves, 1993; NCHS, 1993). The CDC report suggests that racism, in both its material and ideological components, is more real than race. Racism and socioeconomic factors undoubtedly have more of an effect on health and biological welfare than race as biology. Unfortunately, the mixed messages of what race differences in morbidity and mortality signify continues to confuse the public, and many researchers. Perhaps the only way to clarify the message is to change the language.

RACE, RACISM AND HUMAN GENOMICS

"We used to think our fate was in the stars, now we know, in large measure, it is in our genes" (James Watson, *Time*, March 20, 1989).

"These mapping stories are dramatic, their messages compelling and their promises seductive" (Lippman, 1992:1469).

According to Daniel Koshland (1987), the editor of *Science*, the nature-nurture debate has ended and nature has won. Keller (1992), Nelkins (1992), and Hubbard and Wald (1993) all show that we are living in a time where there is a pull toward simple, biological explanation, e.g., it is in the genes. The "it" in question is most anything from why black babies are smaller and black women are more anemic to sexual prowess, athletic ability, homosexuality, criminality, and homelessness. Never mind that there might be some societal and environmental etiology; it is much easier and perhaps more satisfying to have a simple, guilt-free explanation. This approach to the human condition—find a responsible gene and go on to the next problem—is something legislatures can relate to (Watson, 1992).

The Human Genome Project (HGP) is the embodiment of a rise in thinking that human nature is profoundly gene-based (Lewontin, 1991). It is a very big thing with profound social and scientific implications. The HGP has and will dramatically change the course of biological research. With legislative support and plentiful funding for the HGP in the United States (and HUGO, the compatriot organization in Europe), and rhetoric such as the above by influential editors and Nobel prize winners, it is not hard to see why legislative and financial support has been garnered and why anthropologists are hopping on the genomics bandwagon (Lewin, 1993; Lewontin, 1991; Roberts, 1992a, b). As big as the project is however, it is really only a symptom, a result of deeper currents that flow between and within biomedical research and society.

In anthropology, genomics takes on a unique twist because anthropologists control the human variation knowledge industry. The anthropological spin on the HGP is the Human Genome Diversity Project (HGDP). As it was envisioned by population geneticist Luca L. Cavalli-Sforza and colleagues, the HGDP was to be a project that would rectify an important limitation of the HGP, which was to look at a single genome, not the diversity among human genomes (Roberts, 1991). This project is played out as the "politically correct" HGP, because it acknowledges variation and has a strong conservation biology rhetoric. As seen by Mary-Claire King, this work is only possible now because we finally have the know-how and diversity is rapidly decreasing due to intermarriage and genocide (Roberts, 1992). Roberts, in reporting on a HGDP conference, quotes a participant as saying "What changed is the availability of thousands of genetic markers, scattered around the genome" (1992:91). The informant continues to say that just as the new markers and other techniques are becoming available, populations are disappearing. In the same *Scientific American* article in which he starts by declaring that the data to reconstruct human history are at hand, Cavalli-Sforza ends by saying that "Anthropological fieldwork must catch up ... with the rapidly disappearing data. Priceless evidence is slipping through our fingers as aboriginal populations lose their identity" (1991 :110).

Of course, this is neither the first time that scientists have bemoaned the disappearance of "priceless evidence" nor the first time that one hears the argument that we must do research because we have the means. Over a quarter century ago Garn wrote: "Now, quite suddenly we are in a position, as many investigative fields come to maturity, to answer the fundamental questions that will lead to a more complete understanding of the different races of mankind" (Garn, 1965:10). A perusal of human biology literature would certainly yield a variety of such calls to "salvage human biology," extending back to the 19th century.

In operation the HGDP is designed to involve the collection of blood samples from a large number of humans, some 10,000 or more. These samples will then be taken to major research facilities (most likely in the U.S.A.) where they will be immortalized in cell cultures (Roberts, 1991). After this initial blood gathering, it will then be possible to amplify and read gene sequences and make comparisons across individuals and groups. For example, if one accepts the notion of a biological clock (that mutations take place at a relatively constant rate), and if a large number of genes from different Native American populations or other groups are compared, then this data could provide further information on the date that these groups fissioned. It would provide an additional set of data upon which to test hypotheses (note: not the only set, but an additional set).

The debates at HGDP conferences that have reached the pages of *Science* focus on sampling strategies (Roberts, 1992). How many individuals will be sampled, how will they be selected, and how will groups for inclusion be identified? The late Allan Wilson favored a uniform sampling strategy, in effect placing a grid over the world and selecting samples based on locations on the grid. However, wider support seems to have been garnered for a more traditional method of selecting known "anthropological" populations. However, this method clearly leads to the reification of population differences; the sampling methodology prejudices in favor of finding population differences. That the data are so desperately wanted now, regardless of sampling strategies, is implicit in Cavalli-Sforza's answer to the questions about the scientific rationale for selecting 50 individuals per group: "One person can bleed 50 people and get on the airplane in one day" (In Roberts, 1992:1205).

Although issues of sampling are important ones, they may also deflect attention from the more fundamental questions of theory and utility of the project. What will be the likely scientific and humanitarian payoffs of the project? Will it be racist science and even lead to racism? Or will it increase our understanding of the invalidity of race, who we are, and what are our predispositions to disease? This much is certain: much of what the project becomes needs to result from thoughtful discussion, not just doing something because it is doable.

The HGDP, as it is envisioned, has the marking of reductionist science with a very mechanistic approach to human biology. There is no built in effort to examine interactions between genes, or between genes and the environment. In fact there is no discussion of contextual information that would make this possible. Eventually sequencing strings of DNA can lead to the view that the person is the string (Lewontin, 1991). Without contextual information, which will surely slowdown and make the project more expensive, it is hard to envision how the project will do more than provide additional data on small and trivial polymorphic differences¹². It is repeatedly promised that the project will provide keys to understanding susceptibility to disease (see for example, Kidd et al., 1993), but how can it if all we have is genes without contexts? The pronouncement of King, one of the organizers of the HGDP, that the project will tell us "who we are as a species and how we came to be" (Roberts, 1991:1204) seems to be a just slightly overblown soundbite.

Is the project bound to be racist, that is to use and reify the concept of race? Of this I am very uncertain. As stated above, how the data are collected will provide fuel variously for clinal versus populational approaches. Perhaps, in the right hands, the data will prove once and for all that races are abstractions, that, as Mary-Claire King (1993) says, we will find so much within-group variation that the project will be a key to a nonracial science. But, we have known for at least twenty years that within-group variation is so much greater than between-group variation (Lewontin, 1972). We do not need a very expensive data collection exercise to show this. The data are already at hand. The often heard contention that the study of the pattern of human variability is being hindered because some scientists are afraid of the results is a canard. The results are known (Cavalli-Sforza, 1991).

In essence then, three slippery goals of the HGDP have been expressed by various proponents: (1) it will be a key to showing the invalidity of race, (2) it will provide data to reconstruct human history, and (3) it will help provide information on genetic patterns of disease susceptibility. However, we already have the data to show that race has little explanatory value and, according to Cavalli-Sforza, sufficient genetic data are already in hand to map

"lines of descent of populations of the world" (1991:104). Furthermore, there is no reason to believe that the new data that might come from the HGDP will lead to obvious or statistically less ambiguous phylogenetic trees. Finally, the methodology is not robust for studying disease etiologies, the third and remaining goal. At best the resulting data will provide preliminary associations between gene frequencies and disease. Thus, a real concern is that the project's intellectual payoffs will continue to be overstated and this will eventually turn public support away from science and anthropology.

What most concerns me is not competition for research funds, although this is an issue in a lightly funded discipline. Rather, it is the under-theorized and under-problematized idea of human variation that the project might embody and reify, as well as the type of training and socialization that the project will provide for young biological anthropologists. Where will the anthropology be in mapping and comparing hundreds and thousands of DNA samples? How will this research train a young biological anthropologist to appreciate the complexities of human biology and biocultural interactions?

GENES, FOSSILS AND HUMAN EVOLUTION

One area where a genomics approach to anthropology has been challenged is in the study of human origins. Unfortunately—and this may be a first measure of where we are as a field—the challenge may be more typological than the genomics model. Two "models" have been put forward to explain the evolution of *Homo sapiens*. The "out of Africa" or "African Eve" model is aligned with genomics because it was developed from mtDNA data from the lab of the late Allan Wilson, a leading proponent of the HGDP, and more fundamentally because it rests on common assumptions of neutral (non-Darwinian) evolution. This model, which has recently been shown to lack robusticity with regards to dates of separation of groups or the point of origin of the tree (Templeton, 1993), purports to show that humans evolved in Africa about 200,000 YBP and evolved from an African woman (Cann et al., 1987; Vigilant et al., 1991).

The contending alternate model is labelled either the "regional continuity" or the "multi-regional" model (Thorne and Wolpoff, 1992; Wolpoff, 1989). This model is purported to be the fossil-based, as opposed to genetic-based, model of human evolution. The authors contend that rather than showing replacement of populations from outside of Africa, the skeletal data supports long term biological continuities within regions. Thus, Frayer and colleagues (1992), while acknowledging exchange of genes between regions, suggest that evolution toward modern humans has taken place in parallel in separate regions.

What is disconcerting is that in some regards the alternate scenario has fallen into a hole of supporting a model that may be more typological than that which derives from mtDNA. Although this I hope is unintended, it is disquieting to find an astute journalist/biological anthropologist writing a popular article on the regional continuity model that is titled "On the origins of races" and with a byline "Fossil skulls and ancient remains are providing important clues about the evolution of human races" (Shipman, 1993). As well, a recent review of the controversy in *Annual Review of Anthropology* also considers the multiregional model to be one that supports the long term separate evolution of major races: "This alternate view postulates further that human races are ancient, possibly predating the emergence of the species, and that they arose as adaptation to local conditions" (Long, 1993:252). Intended or not, the regional model raises the idea of separately evolving races.

CONCLUSIONS

"I very strongly object to anybody who says knowledge is dangerous ... Notice I said knowledge and not theories spouted off" (Kenneth K. Kidd, cited in Horgan, 1993:29).

"Nothing handed down from the past could keep race alive if we did not constantly reinvent and re-ritualize it to fit our own terrain. If race lives on today, it can do so only because we continue to create and re-create it in our social life, continue to verify it ..." (Fields, 1990:18).

There are as many differences as similarities in the three subfields reviewed. Older, typological ideas are more obvious in forensic research. There is perhaps the most progressive thinking in some corners of public health and medical research, and those working in the new genetic anthropology may see themselves as occupying an end of biological anthropology opposite (as opposed to near) forensic anthropologists. These real differences aside, all areas need to come to terms with models and implied theories of human variation. Moreover, the use of race, or even of atomistic thinking, in one area provides tacit support for the use of race in another area. One thing that is certain is that the distinction between "knowledge" and "theory" as proposed by Kidd, a leading proponent of the HGDP, is rarely obvious to nonscientists. Knowledge to one society (Jews are an inferior race and the cause of Germany's economic problems; African Americans are unable to govern themselves...) is exposed as politically expedient theory in another.

A last appraisal of where we are today is provided by the debate held at the 1993 annual meetings of the American Association of Physical Anthropologists (Toronto, April) on a resolution to update the UNESCO Statement on Race, 1952 (revised 1966). The update, which was felt to be timely because of an upsurge in racist biology (the work of nonanthropologists, primarily Phillippe Rushton, is often cited), is an effort to provide an update on how physical anthropologists view race. The draft update, like the initial UNESCO statement, is not a polemic against racist science. Rather, it takes a middle ground, in for example naming major human groups as races and generally supporting the notion that race is of very little importance aside from its social meaning. It is not even as radical as the CDC document which was drafted mainly by physicians, anthropologists, and public health workers (MMWR, 1993). The UNESCO revision seems to be the result of an effort at compromise of differences of opinion within the discipline.

The update was sent back to committee. The exact reasons why can only be conjectured. Those who commented on it picked apart some of the writing and others quite clearly voiced disagreement with the "nonracial" character of the documents. Conversely, others voted against the document because it was not a bold enough statement on the myth of race.

Regardless of reason, it is clear that there is no consensus and great confusion exists in the discipline with regards to race. A great deal of uneasiness and testiness is evident when one tries to discuss the validity of the concept with a group of biological anthropologists (this was evident before, during and after the discussion of the resolution). Biological an-

thropologists do not need to table discussion of race and documents do not need to be sent back to committees, to be revised and acted on in private. Biological anthropologists need to talk about race.

From an inquiry into the current state of racialism in biological anthropology, I draw the following modest conclusions

1. The denial of race is not a denial of human diversity. Rather it is a stance that suggests that human diversity is too complex to be explained by types. Similarly, human biology is more than strings of beads and mechanics. Humans are not comprised of replaceable parts. A goal of biological anthropology should be to explain biocultural complexity.

2. We have entered a phase of glorification of the gene. This is consistent with the search for simple biological solutions to complex problems, and it is also consistent with an upsurge in racialism. Genetic reductionism does not lead directly to racialism or racism. However, if one can use the past as a gauge then we see the extreme likelihood of such connections.

3. Race is an idea of paradigmatic magnitude. It is a worldview. I suggest that it is so flawed a view as to be useless in all realms of biological anthropology. Biological anthropologists are challenged to find a place where the concept of race is essential, where research would be hindered if the concept was not used. I propose that if the concept is not essential it should be abandoned.

4. Racist anthropology is not simply an unfortunate episode of the past, easy to see and critique. Racism is not something only practiced by politicians and "non-scientists". We can not keep shrinking from discussions and analysis of how ideas effect our science and interconnect with power. The past is past. We cannot change it but we can learn from it. Hopefully what we take from an examination of the history of an idea in our discipline shows that knowledge is power, and we in the knowledge industry have power. How we decide to use knowledge is far from trivial. Others will also use (and abuse) this knowledge. Theories are inseparable from knowledge. Facts do not speak for themselves

5. Whether the new biological anthropology is a retreat to an older biological anthropology or an advance si open to further debate, as is whether it will promote a reemergence of racist science. This paper has purposely focused on the negatives and the potential for misapplication simply because they are possible. We need emphasize that none of our fellow biological anthropologists come anywhere near Muir's (1993) idea of the mean racist. However, all of us need to consider how, by keeping alive the myth of race, we might maintain and support subtle forms of racism.

6. In the preface to *Race, Science and Humanity* (1963) Montagu assesses that he is unlikely to change the minds of older scientists who have grown up with the "myth of race." However, he hopes that he might convince younger colleagues. Unfortunately, today the situation is more complex. Belief in race may be less generational than it was a decade ago (Lieberman et al., 1989). I submit that the future of the subdiscipline of biological anthropology will be in serious jeopardy if we do not take seriously our models of human variation. Conversely, if we take our expertise and our models seriously, we could again be at the forefront of an empowering science.

7. Racism is more real than race. To deny race does not deny the study of racism. Race (biology) and racism are often confounded in human biological research, especially in studies of group differences in health and nutritional status. What is needed are more studies of the biological consequences of racism. This would be an important corrective.

NOTES

1. Many of the ideas in this paper were developed in Goodman and Armelagos (ms) and Goodman (ms). I am in debt to a large number of colleagues with whom I have discussed ideas presented here, or have labored through readings of prior drafts, and to Noel Boaz and Linda Wolfe for providing this opportunity to write further on race in biological anthropology.

2. An interesting contrast with the lack of debate over the utility of race in biological anthropology is the progressive forcefulness with which the Centers for Disease Control has tackled the utility of race in public health research (MMWR # 42, 1993).

3. The most obvious shrinking from discussion recently occurred at the 1993 annual meetings of the American Association of Physical Anthropologists (Toronto, April 1993). At the business meeting a motion was put forward to ratify an update of the second UNESCO statement on race (signed in 1951, last updated in 1964). After discussion, mainly focusing on minor wording, the update was sent back to committee. The only sense one got from the discussion was that few individuals had taken the time to carefully read the statement and that there was disagreement on its content. Although the business meeting was clearly not the place for a lengthy discussion of the document, its, and underlying points of contention, do need to be discussed.

Race, as Boaz (1993) has recently noted has also been a topic of some discussion on HUMBIO-L, an electronic bulletin board devoted to discussion of issues in human biology. However, once the discussion became somewhat challenging it ceased. In fact, it was I who asked for a definition of race that was not a tautology. A reply has not been forthcoming and general discussion ended.

4. Here I am following Todorov's (1993) definition of racialism, as a doctrine and theoretical point of view, versus racism, which is a term designating behavior. Todorov warns that "the form of racism that is rooted in racialism produces particularly catastrophic results: this is precisely the case of Nazism." (1993:91).

Racialism is a worldview in which human variation is considered to be made up of distinct types (races). Racialism, then, is somewhat broader than what is conventionally defined as scientific racism, the belief in relative superiority and inferiority of different groups (Shanklin, 1994). However, in actual practice the difference may be blurred.

5. There is little evidence that Linnaeus was interested in explanation as we would think of it. His *modus operandi* seemed only to have been to describe and organize what God had created and left on earth.

6. Although this is beyond the scope of this paper, it is worth noting that both Ashley Montagu and Franz Boas, perhaps the two most influential critics of race at the turn of the century and at the midpoint respectively, were born Jewish. Interestingly, one can also see clear statements against biological determinism in the writing of Frederick Douglass (Foner, 1950 [1854]). However, for obvious social reasons, Douglass gained less of an audience.

7. Lewontin's findings have been confirmed and supported by Nei and Roychoudhury (1982), who use a more extensive set of data.

8. It should be clear at the start that "racing" skeletons is just one of many things that forensic anthropologists do. The following critical comments do not apply to other research directions in forensic anthropology.

9. During the 1991 annual meetings of the American Association of Physical Anthropologists (April, Las Vegas, Nevada) this casting company occupied a table directly at the entrance to the main meeting area. It should be noted that racial terminology was eliminated from the catalog presented at the 1994 meetings of the AAPA.

10. We select for illustration this article, not because it particularly shows the extreme biases of hereditarianism, but because Weiss is one of the most influential biological anthropologists of this era and he is, these comments notwithstanding, a brilliant and thoughtful researcher.

11. Jonathan Marks deserves credit for the term "salvage biological anthropology."

12. Perhaps the most serious charge that the project has no significant humanitarian benefit comes from Native Americans. In a letter that made its way onto the public internet, Tododaho-Chief Leon Shenandoah asks Jonathan Friedlaender, head of Biological Anthropology at the National Science Foundation, to cease the project's funding because indigenous peoples have been excluded from decision making. The project is succinctly characterized by Shenandoah as "make-work project for science and anthropology".

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