Chapter 9

THE RESURRECTION OF RACE:
THE CONCEPT OF RACE IN PHYSICAL ANTHROPOLOGY IN THE 1990s

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Introduction

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 and quotation marks 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).

At the Sixty-Second Annual Meeting of the American Association of Physical Anthropologists (Toronto, April 1993) a resolution was put forward to ratify an update of the second (1952/1966) UNESCO Statement on Race (Montagu 1964). The UNESCO statement, despite the work of Ashley Montagu, a signee and key framer, upholds the validity of the concept of race as a classificatory devise. It is not a particularly radical document. Similarly, the update is not a polemic against racialist science. Indeed, as is discussed below, it is not nearly as radical as the recent Centers for Disease Control's "state of the field" paper on the problematic use of race in public health research (MMWR 42, 1993). Yet, at the meeting of physical anthropologists the resolution received vigorous opposition. Why was this so? Does a core or majority of physical anthropologists still hold to the validity of the concept of race? Has race been resurrected?

The following paper documents signs and trends in this re-emergence and attempts to explain the resurrection of race in the context of trends in the sciences and society. We examine the recent use of race in three main areas of physical anthropology: (1) studies in skeletal biology and forensic osteology; (2) physical anthropology studies in nutrition and medicine; and (3) the new human genomics, in which we include 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).
The key question is "How, despite ambiguities in the meaning of race, do forensic scientists learn not to problematize race?" Said in somewhat opposite form, "How does one reify race and its assumption?" 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, France Casting, a company that provides reproduction casts of a variety of human and nonhuman features for use in teaching and research.

France Casting's catalog (Fall, 1992), now twenty pages long, includes descriptions and costs of casts for sex and age discrimination (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.

What does France Casting 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 physical 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 letters "Negroid Male." The description of the skull states that it "illuminates racial traits very well...this is a wonderful cast!" The skull below it is labelled "Caucasoid Female," and its description is similar: "illuminates racial traits very well...This is in excellent condition!" 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 description, in such a poor state of preservation: separated squamosal and sagittal sutures, a cracked and separated malar, brokenzygomatic 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 us, 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 and clearly highlighted is of an "American Indian Pueblo Male." It is described as illustrating scalping marks. We assume that France Casting 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 we believe even more so from the use of the naïve students 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 comforting simple story: there are old and static ideal types, and with a minimum of training one can play the game of judging the cranial 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 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 is officially recognized (St. Hoyme and Iscan 1989). But, bureaucratic races change and they may have little to do with biology (Lee 1993). Interestingly, Bures (1993) apparently believes that the devotion to just providing race is scientific and non-political, whereas belief there are no races is a political statement. Interestingly, Lee (1993) cogently makes the opposite argument in documenting the political pressures that go into becoming an "official race" in the U.S. national statistics. The "I'm just doing my job" approach is political, and suggests strongly that the ideology is accepted from above.

Is race an essential for forensic anthropology? We suggest, despite the long history of attempts at assignment (see Galloway, et al 1993, Gill and Rhine (eds.) 1990; Gordon (ed.) 1993), that race is not essential. The rhetoric of racial types could easily be changed to continental or geographic ancestry without affecting law enforcement efforts. More importantly, the charge to forensic anthropology is to describe as well as possible how individuals looked and other aspects of their biology. 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). Perhaps the best we can do is to go 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.

Race and Medicine

Current-day anthropologists recognize at least three major races, namely, Caucasian, 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).
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 medical studies?

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, we often are faced with the double leap of faith that a disease in question is genetic in etiology and then 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 due to genetics 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 answer? One paragraph is devoted to environmental 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 shows weak correlations between genetic markers and diabetes rates, not at all questioning the correlative nature of the research.

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 .2-.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 cut-off 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 years) is estimated to be reduced "on paper" from twenty to ten percent (Pan and Habicht 1991).

Has it been proven that the "race" difference in iron metabolism has a genetic basis 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? Has it been proven that the difference is pan-racial? The answer to both questions is a resolute "No!" Moreover, the issue is one of a theoretical one: separate cut-offs lead to profound health implications when one considers some of the fundamental consequences (in learning, work, and immunological capacity) of low hemoglobin values in ranges near anemia cut-off values (Scrimshaw 1991).

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. We agree with the CDC finding that this approach is extremely problematic (also see Hahn, et al 1992; Hahn 1992). 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; HCHS 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 continue 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).

According to Daniel Koshland (1987), the editor of *Science*, the nature-nurture debate has ended and nature has won. Keller (1992), Nelsen (1992) and Hubbard and Wal (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). No wonder governments are all for the human genome project and many physical anthropologists are joining in.

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. The HGP has and will dramatically change the course of biological research. With legislative supports and the go ahead to fund the HGPin the United States and HUGO, the compatriot organization in Europe, as the first big biology project, 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 1992). As big as the project is though, we need make clear that it is 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 (Robert 1991). This project is played out as the “politically correct” HGP, because it acknowledges variation and has a strong conservative 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 increasing due to intermarriage and genocide (Roberts 1992). Roberts, in reporting on a HGDP conference, quotes a key participant 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 is 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 we have heard 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, work 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, not only the set, but an additional set.

The debates that appear to have gone on at HGDP conferences and 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? These are interesting and important issues. Some concerns focus on the late Alan Wilson’s idea of placing a grid over the world and selecting samples based on locations on the grid. This method does not consider the world to be divided into discrete populations, or whether discrete populations should be selected and sampled. Wider support seems to have been garnered for a more traditional method of selecting known “anthropological” populations. This method, championed by Cavalli-Sforza, however, clearly leads to the reification of population differences; the sampling methodology prejudices for finding population differences. That the data are so desperately wanted now, regardless of sampling strategies, is implicit in Cavalli-Sforza’s answer to why select 50 individuals per group: “One person can bleed 50 people and get on the airplane in one day” (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, or who we are, and of our predispositions to disease? This much, however, 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 a 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 environment. In fact, there is no discussion of contextual information that would make this possible. Eventually sequencing strings of DNA will 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 differences. 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 racialist, that is, use and rely the concept of race? To this we are very uncertain. As stated above, how the data are collected will provide fuel variously for clinical 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 non-racial 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.

In essence then, three 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 in hand to map "lines of decent 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 us is not competition for research funds, although this is an issue in a lightly funded discipline. Rather, what most concerns us is the construct of human variation that the project might embody and reify, and the type of training and socialization that the project will provide for young physical anthropologists. Where will the anthropology be in mapping and comparing thousands and thousands of DNA samples? How will this research train a young physical anthropologist to appreciate the complexities of human biology and of biocultural interactions?

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).

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 physical anthropology opposite to (as opposed to near) forensic anthropologists. These real differences aside, all areas need to come to terms with models and ideas 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. We draw the following conclusions from our inquiry into the current state of racialism in physical anthropology.

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 physical anthropology should be explaining biocultural complexity.

2. We have entered a phase of glorification of the gene; one in which simple biological solutions to complex problems are being sought. This does not lead directly to racialism or racism. However, if we can use the past as a gauge then we see the extreme likelihood of such connections. Moreover, the physical anthropology of the 1990s looks suspiciously like the physical anthropology of the 1920s and 1930s.

3. Race is an idea of paradigmatic magnitude. It is a worldview. For us, it is so flawed a view as to be useless in all realms of physical anthropology. We challenge all physical anthropologists to find a place where the concept of race is essential, where research would be hindered if the concept was not used.

4. Racist anthropology is not an unfortunate episode of the past, easy to see and critique. Racism is not something only practiced by politicians and "non-scientists." We cannot keep shrinking from discussions and analysis of how ideas affect 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. As well, others will use (and abuse) this knowledge. Theories are inseparable from knowledge. Facts do not speak for themselves.

5. 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.

6. Whether the new physical anthropology is a retreat to an older physical anthropology, or an advance, is open to further debate, as is whether it will promote a re-emergence of racist ideology. 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 physical 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.

7. Any mildly self-aware scholar knows that race is a dominant construct through which we see the world. Yet, we still have not come to grips with how this affects our science. We submit that we are in trouble when Pat Shipman, an astute journalist/physical anthropologist, pens an article titled, "On the Origins of Race" (1993), based on current evolutionary models. Physical anthropologists need to talk about race and theories of human variation.

8. 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
than it was just a few years ago (Lieberman et al. 1989). We submit that the future of the subdiscipline of physical anthropology will be in serious jeopardy if we do not take seriously our models of human variation. Conversely, if we do, we could again be at the forefront of an empowering science.7

NOTES

1. Here we are 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).

2. Muir (1993) has recently made the distinction between “mean racists” and “kind racists.” Mean racists aim to hurt whereas kind racists do not. However, kind racist, or racists, continue to define humanity in racial terms. By doing so, kind racists leave the door open for mean racists.

3. We want to make clear at the start that classification of races is just one of many things that forensic anthropology does. The following critical comments do not apply to other research directions in forensic anthropology.

4. During the 1991 annual meetings of the American Association of Physical Anthropologists (April, Las Vegas, Nevada) France Casting occupied a table directly at the entrance to the main meeting area. France Casting has since modified its catalog in a manner that corrects some of these problems.

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

6. We credited the term “salvage biological anthropology” to Jonathan Marks.

7. Our initial research in anthropology focused on the idea of race, with particular appreciation to the writing of Ashley Montagu. The current re-emergence of race in anthropology has drawn us back to this topic. B. Bogin (Michigan, Dearborn), E. Hammonds (MIT), L. Lieberman (Central Michigan), J. Marks (Yale), D. Martin (Hampshire), and A. Swedlund (Massachusetts) provided useful comments on an earlier version of this paper, which was read at the 13th Annual Meetings of the International Congress of Anthropological and Ethnological Sciences, Mexico City, July 31, 1993.

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