

# The Origins of Anthropological Genetics

by Jonathan Marks

Although we often date the conflict of “molecules and morphology” in biological anthropology to the 1962 Wenner-Gren conference “Classification and Human Evolution,” the roots of the conflict extend considerably deeper. In the first half of the twentieth century, two established research traditions applied genetic data to problems in physical anthropology: racial serology and systematic serology. These had a tense relationship with the more mainstream areas of racial anthropology and primate taxonomy. Both produced conclusions that were often difficult to reconcile with traditional physical anthropology but that laid claim to the authoritative voices of genetics and evolution. They were also less relevant and less threatening to general anthropology than the other movement for the application of genetics to anthropological problems—eugenics—had been. I discuss the relations of genetics to anthropology as manifested in the areas of eugenics, race, and primate taxonomy in the early twentieth century and the field’s transformation into anthropological genetics in the 1960s.

## Introduction

There is a mythic history of the intersection of genetics and anthropology. One half concerns the zoological relationships of humans as a species to other species. In this story the crude similarity of human blood (and presumably therefore genes) to ape blood was noted at the turn of the twentieth century but largely ignored until the 1960s, when Morris Goodman finally correctly apprehended the phylogenetic intimacy of humans and African apes. Thus, after Nuttall’s work in the early 1900s, “Nothing much happened for the next sixty years, except perhaps that people tended to forget the genetic intimacy between humans and the African apes” (Lewin 1987: 106; see also Goodman and Cronin 1982).

The other half of the mythic history concerns the use of genetic data to study the products of human microevolution. In this story, cultural anthropologists were naturally averse to genetics (which is, after all, science), and aside from work during World War I, anthropology generally ignored genetics, again until the early 1960s, in this case led by heroic figures such as James Neel and Luca Cavalli-Sforza (e.g., Pollitzer 1981). Thus, in the introductory chapter of a recent textbook (Crawford 2007) of anthropological genetics, we read that

Ludwik Hirschfeld and Hanka Hirschfeld (1919), during World War I, demonstrated that military personnel of various so-called “racial groups” or ethnicities differed in the frequencies of the ABO blood groups. In the few decades

that followed, additional blood group systems . . . were shown to vary in human populations. (4)

Unfortunately, until the 1950s, there were few anthropologists with adequate training in human genetics. The reason behind this paucity was that most physical anthropologists were traditionally trained in morphology and racial classification based on typology. (7)

Both of these mythic histories are notable for what they omit. In particular they omit the active research programs of systematic serology and racial serology, the vexed conclusions often drawn by practitioners, the troubled state of human genetics before World War II, and the intellectual shifts in the 1960s and 1980s that resulted in the Human Genome Project and the unprecedented privileging of genetic data. These are the issues I will address in this paper.

## Early Human Genetics as the Unanthropology

In the early twentieth century, Franz Boas transformed American anthropology in large part by infusing it with the German liberal humanism of Rudolf Virchow. Virchow was distrustful of naturalistic theories of human history, in particular those of his former assistant Ernst Haeckel (1876 [1868]). Haeckel’s evolutionary theory held the “Indo-Germanic” branch of the Mediterranean (i.e., Caucasian) race to be the highest form of life and aggressively dehumanized the rest of the human species in Darwin’s name:

Of course the relative number of the twelve species [of people] fluctuates every year, and that too according to the law developed by Darwin, that in the struggle for life the more highly developed, the more favoured and larger group of forms, possess the positive inclination and the certain ten-

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dency to spread more and more at the expense of the lower, more backward, and smaller groups. Thus the Mediterranean species, and within it the Indo-Germanic, have by means of the higher development of their brain surpassed all the other races and species in the struggle for life, and have already spread the net of their dominion over the whole globe. (324)

The various branches of the Indo-Germanic race have deviated furthest from the common primary form of ape-like men. . . . [It is the Germans and the English] who are in the present age laying the foundation for a new period of higher mental development, in the recognition and completion of the theory of descent. (332)

Virchow, on the other hand, demonstrated empirically in the 1870s that various European peoples could not reliably be categorized by a single skull type and that the Aryan or Teutonic appearance was present in only a small minority of Germans. Nor did he embrace the thesis of Darwinian racial superiority with the vigor of Haeckel (the subtitle of *The Origin of Species* was *The Preservation of Favoured Races in the Struggle for Life*, after all). Indeed, the knowledge of “how to differentiate between exclusively national politics and universal human science” was specifically what Virchow (1872) had mocked French anthropology for lacking after the Franco-Prussian War (Manias 2009). Virchow then notoriously called for evolution, or at least whatever Haeckel was speaking on behalf of, not to be taught in schools. In this he found an unlikely ally in Thomas Huxley (1879), who harbored his own ambivalences about the need to teach evolution:

It is not that I think the evidence of that doctrine insufficient, but that I doubt whether it is the business of a teacher to plunge the young mind into difficult problems concerning the origin of the existing condition of things. I am disposed to think that the brief period of school-life would be better spent in obtaining an acquaintance with nature, as it is; in fact, in laying a firm foundation for the further knowledge which is needed for the critical examination of the dogmas, whether scientific or anti-scientific, which are presented to the adult mind. (xvii)

Thus Virchow's protégé Franz Boas emigrated to the United States with an intellectual inheritance of (1) an appreciation for the racial type as an empirical fallacy, (2) a distrust of biologicistic explanations of human social difference, and especially (3) a distrust of the invocation of Darwin in support of the doctrine of racial inequality. In New York a generation later, Boas would play Virchow to the geneticist Charles Davenport's Haeckel. Davenport, the leading human geneticist in America, published his major work (*Heredity in Relation to Eugenics*) the same year as Boas published *The Mind of Primitive Man* (1911). Where Boas made it clear that history is not driven by the gene pool and is not explained by racial

endowments, Davenport made quite the opposite claim, that human groups and social classes differed in their fundamental genetic worth, which in turn explained their political and economic status.

Five years later, Davenport's friend, the naturalist Madison Grant, published a best seller, *The Passing of the Great Race*, which explained human history in terms of the racial superiority of the Nordics and called for the immediate sterilization of the American unfit, “extending gradually . . . and perhaps ultimately to worthless race types” (Grant 1916:47). In 1916, Boas not only published a comprehensive critique of Davenport's cherished eugenics program but also a devastating review of Madison Grant's book in the *New Republic*. Nevertheless they all served together on the National Research Council, vying to control the intellectual direction anthropology would take. And even as British eugenicists vilified Davenport's work and ideas in scholarly and public forums as early as 1912, Davenport retained the power and authority as the leading researcher in American human genetics (Spencer and Paul 1998).

The point I am trying to make is that in the early decades of the twentieth century, the antiracist anthropology that Boas was attempting to establish was being aggressively counterbalanced by a racist anthropology predicated on a fanciful view of heredity and nevertheless promoted by the leading authorities and spokespeople for human genetics. With the principal exception of Columbia's fruit-fly geneticist Thomas Hunt Morgan, most geneticists followed the lead of Davenport and Grant, serving under them on the American Eugenics Society and reviewing their work favorably in scientific forums.

It was not at all clear that human genetics was relevant to or even compatible with the scholarly study of the human species. It was little more than a scientific instrument to oppress the poor and marginalized, as Clarence Darrow came to realize during the Scopes Trial (Darrow 1925, 1926). Lawyers and anthropologists were able to see the poverty of reasoning that suffused the field of human genetics far more clearly than the geneticists could.

## Physical Anthropology and Racial Serology

Physical anthropology was only slightly more welcoming to human genetics than was cultural anthropology. Aleš Hrdlička and Earnest Hooton were among those who served below Madison Grant on the American Eugenics Society, which successfully represented itself as having a scientific biological solution to America's social problems. While Hrdlička complained privately about Grant, he was willing to accommodate himself to Grant so long as Grant financially supported his interests in professionalizing physical anthropology. The vagaries of history are such that when it became clear that Grant would not underwrite the *American Journal of Physical Anthropology*, Hrdlička booted him off the founding editorial board and replaced him with Boas (Spiro 2009). Hooton, for

his part, remained an avid eugenicist long after it fell out of fashion. He wrote Grant in 1933 to thank him for a copy of his latest book and added “I don’t expect that I shall agree with you at every point, but you are probably aware that I have a basic sympathy for you in your opposition to the flooding of this country with alien scum.”<sup>1</sup> But after Grant’s death, Hooton (1940) mocked him:

Madison Grant had a vivid personality and a long head, but, as I remember him, rather a swarthy complexion. I was curious about his conception of Nordicism; so I tackled him on the subject of my own racial type. I said, “Mr. Grant, I have a round head with a cephalic index of 85, brown hair, mixed eyes, a moon face and a blobby nose—all these attractive features going with a muddy complexion. How would you classify me as to race? I should call myself a mixed Alpine.” He asked, “Are you not of purely British ancestry?” I replied, “Yes, my father is an Englishman and my mother is a Scotch Canadian.” He said, “Then, damn it, you’re a Nordic.” That is the only occasion when I have been so classified. (184)

Charles Davenport, on the other hand, remained in high repute within physical anthropology. Although his work had been publicly ridiculed by British eugenicists in 1912 and his eugenical ideas had precipitously fallen out of favor within the American genetics community by the mid-1930s, Davenport could still be elected president of the American Association of Physical Anthropologists—a position he held at the time of his death in 1944, on Hooton’s nomination.

The research in human genetics that most interested the leaders of physical anthropology was, however, of a different sort. Physical anthropology’s primary research question was race, but the most pressing methodological question was the choice of inherited features by which to identify it. The ABO blood group, discovered at the turn of the century, afforded such a stably inherited Mendelian feature. The variation across human populations was studied during World War I by Ludwik and Hanka Hirschfeld (Hirschfeld), initiating the study of racial serology (Schneider 1995).

The problem faced by the field of racial serology as it gained momentum in the 1920s was that the entities it identified were not recognizably racial. Using the frequencies of the blood-group phenotypes, the Hirschfelds managed to divide the human species—based on its principal combatants—crudely into European, Intermediate, and Asio-African. A few years later, using allele frequencies, Laurence Snyder (1926) partitioned the human species into European, Intermediate, Hunan, Indo-Manchurian, Africo-Malaysian, Pacific-American, and Australian. These genetic divisions, however, were exceedingly arbitrary, sometimes self-contradictory, and difficult to relate to the general racial groups with which physical anthropologists were familiar.

By the late 1920s, physical anthropologists were beginning to throw up their hands in despair at the data of racial serology (Mendes-Corrêa 1926; Young 1928). Hooton’s 1931 textbook (Hooton 1931) reviewed the area and concluded that “the fact that some of the most physically diverse types of mankind are well nigh indistinguishable from one another [serologically] is very discouraging. At present it seems that blood groupings are inherited quite independently of any of the physical features whereby we determine race” (490). Likewise, Alfred Kroeber’s (1933) general textbook: “It is clear that we have in these blood-group occurrences an astonishing set of data which may yet profoundly modify the current ideas of race relationships, but which for the present are more provocatively puzzling than illuminating” (12).

Worse yet, in addition to blood-group data—whose data were real, even if cryptic—there were other sorts of blood studies whose data were equally cryptic and less real. Thus, one of the biological rages of the late 1920s involved a Russian hematologist who claimed to be able to tell male blood from female blood by adding chemicals, shaking it up, and observing what color it turned. The Manoilov Blood Test was discussed in major scientific forums, worked just as well on plants (in spite of their lack of blood), and could also be adapted for the determination of race and sexual preference in humans (Marks 1995; Naidoo, Štrkalj, and Daly 2007). Through the intervention of Charles Davenport’s assistant, the geneticist Harry Laughlin, Hrdlička published the work in the *American Journal of Physical Anthropology*,<sup>2</sup> explaining the procedure by which Manoilov’s laboratory in Leningrad could distinguish the blood of races such as Russians, Jews, Poles, and Latvians from one another. Here, the results were not so much uninterpretable as impossible: to Hooton, these blood tests “do not inspire confidence. . . . The test seems to prove too much. It is inconceivable that all nationalities, which are principally linguistic and political groups, should be racially and physiologically distinct. Jews, for example, are not racially pure, but extremely heterogeneous; so are Russians, Poles, and Letts” (Hooton 1931:491).

Hooton’s skepticism in his 1931 physical anthropology textbook can be profitably contrasted with the naïveté expressed in a textbook of genetics published the same year: “According to Manoiloff, the oxidizing process in a certain blood reaction occurs more quickly in Jewish blood than in Russian blood; tests of race based on this difference proved correct in 91.7 per cent of cases” (Shull 1931:299). Knowing a bit about the nature and composition of human groups turned out to be useful for gauging the reliability of the Manoilov Blood Test, decimal point or no.

Blood, a metaphor for heredity itself, was indeed a very

1. Hooton to M. Grant, November 3, 1933, E. A. Hooton Papers, Peabody Museum, Harvard University.

2. Harry H. Laughlin to Hrdlička, June 15, 1926; October 5, 1926; October 19, 1929, Aleš Hrdlička Papers, National Anthropological Archives, Smithsonian Institution.

special juice.<sup>3</sup> Physical anthropology was on the horns of a dilemma—it wanted stable hereditary markers for the discrimination of race, but the most stable hereditary markers available produced racial nonsense. Moreover, the sense and nonsense produced by genetic analyses were often inseparable from one another, and the geneticists themselves seemed either unwilling or incapable of making that distinction. It would not be for several decades until the constructedness of race itself would be appreciated and would explain the lack of fit between genetic patterns and racial patterns (see below). That was not, however, how that lack of fit would be understood within racial serology, that is to say, by the first generation of human population genetics.

They reasoned instead that their own data superseded all others. Of course, it was rarely if ever articulated so baldly, but the message came through. J. B. S. Haldane and Grafton Elliot Smith debated the hegemony of genetic data following Haldane's presentation at the Royal Anthropological Society in 1932. It arose again in the pages of *Science* in 1946 on the placement of Oceanic peoples among the Mongoloids (Montagu 1946; Wiener 1946). And a few years later, it resurfaced in the *American Journal of Physical Anthropology* (Birdsell 1952; Stewart 1951; Strandskov and Washburn 1951) in response to a proclamation by the serological geneticist William C. Boyd (1947) that "it would seem hardly too much to say that serology (or rather, genetics), is destined to oust craniometry and anthropometry as the main tool of racial anthropology" (46).

This was, to some extent, turf patrolling, but the turf was the epistemic ground separating narrow and reductive, if trendy, research from scholarly and comprehensive, if hoary, knowledge. Moreover, not only was there something vulgar in the geneticists' uncritical self-promotion, but also they were not even actually extracting race from their data—they were superimposing race on their data and merely describing the results (Rowe 1950).

The field of racial serology effectively died off with Boyd's (1963) review in *Science*, which identified thirteen serological races—one African, two Asian, five European, one American, and four Oceanic. The cultural edifice that underlay seeing one kind of African but five kinds of European became a bit more obvious during the era of the civil rights movement.

In sum, the use of genetic data to address questions in physical anthropology had a long if not particularly distinguished history spanning half a century before being reinvented as human population genetics. The problem was that aside from self-interested rhetoric, genetic research did not seem to have anything to add to the corpus of physical anthropology that was either not obviously false or manifestly useless aside from documenting additional differences among human populations. If there was a lack of enthusiasm within physical anthropology for genetics, it was not for lack of in-

terest; the meaning of the work for understanding what physical anthropologists were primarily interested in was simply very unclear.

## Physical Anthropology and Systematic Serology

Cultural anthropologists were put off genetics for its social program and unhistorical interpretations of history; physical anthropologists were put off genetics for its simultaneous meaninglessness and claims to transcendence. There was another anthropological question where genetic data might prove valuable: that of what Huxley called "man's place in nature," or more generally, of the relationships of primate species to one another.

As noted earlier, anthropologists were familiar with George H. F. Nuttall's work on the blood reactions of different species, including humans. (His sister Zelia was a respected archaeologist of Mexico.) The fact that human and chimpanzee bloods appeared to be more similar to one another than the horse and donkey bloods was brandished at the time of the Scopes trial as evidence of our kinship to the apes (Hussey 1926).

What is sometimes lost in the mythic history of molecular anthropology is the fact that the close relationship of human and ape was long known and was consequently not particularly threatening. A Roman poet named Quintus Ennius had commented around 200 BC on "how similar we are to the monkey, the most horrid beast" (*simia quam similis turpissima bestia nobis*); the remark was preserved by Cicero (*On the Nature of the Gods*, 1.35) and quoted in Francis Bacon's *New Instrument* (*Novum Organum*, 1620) and Carl Linnaeus's *System of Nature* (*Systema Naturae*, 10th edition, 1758), both widely read and highly influential works. The similarity of human and ape is no surprise; what is surprising is that anyone could deny their differences. That denial would have to wait for the emergence of molecular anthropology in the 1960s and from the same kinds of data as the systematic serologists used in the previous decades.

Most of the time, the blood data showed relationships among species that paralleled those derived from classical anatomy. Thus, Nuttall (1902) confirmed the evolutionary distance of the Platyrrhini but was stumped by how distant the lemurs appeared. He coyly suggested that perhaps the lemurs ought to be removed from the order Primates, but of course this simply recapitulates the practice of the racial serologists, assuming that their data transcend all others. The problem, however, is a significant epistemological one: when the blood/genetic data are harmonious with the traditional/anatomical data, we accept them both; but when they are discordant, how do we know which to believe? There have to be checks and balances for the genetic data (Gregory 1917). In fact the blood tests were not at all simple to execute or to interpret and often required extensive hermeneutics. Apparently tarsier blood also failed the test (Hartman 1939); should

3. Goethe, *Faust*, line 1740. This line, spoken by Mephistopheles, was used as an epigram by Nuttall (1904) and Boyden (1951).

tarsiers also be removed from the Primates, the testimonies of their bodies notwithstanding?

The most respected practitioner of systematic serology from about 1930 to 1960 was Alan Boyden. Boyden maintained cordial relationships with morphological systematists and was frank about the limitations of serology, being “no simple guide to animal relationship. The very complexity of the problem demands the use of all possible pertinent data. The data of systematic serology, where comparable methods are used, are as valid as those of systematic morphology, and the two methods of analysis should be considered complementary to each other” (Boyden 1942:141–142).

Hooton’s revised edition of *Up from the Ape* (Hooton 1946) invoked the serological data to help position the human species in the natural order. In particular, Hooton presented the work of Christian von Krogh of Munich, who had pursued the study of the serological intimacy of human and ape. Hooton went on: “The weak similarity of the orang to other species suggests a lengthy process of separate development for this animal and its early branching off from the stock of chimpanzee and man” (45). This is noteworthy in two ways. First, this specific inference would be highlighted as a radical and unanticipated discovery of molecular anthropology a generation later. And second, it implied nothing to Hooton about the classification of the primates: “The differences between man and the great apes are enough to justify us in recognizing a separate family for man, the Hominidae” (47).

## Disciplinary Transformations

After World War II, the fields of human genetics and physical anthropology were in disrepute and needed to be reinvented. Hooton (1936) had struggled in vain to differentiate good American physical anthropology from bad German physical anthropology; his student Sherwood Washburn (1951) would proclaim a “new physical anthropology” focused on evolutionary process, human adaptability, and nonhuman primates. In parallel, James Neel would help construct a human genetics that focused on medical rather than social pathologies; that was oriented toward helping the family, not the race; and that exposed patients to optional services, not coercive surgery.

“Molecular anthropology” was coined at a Wenner-Gren conference organized by Washburn in 1962, “Classification and Human Evolution.” Two significant claims were raised at this conference held in Burg Wartenstein, Austria. First, Emile Zuckerkandl (1963) argued that because the protein sequences of human and gorilla hemoglobin were so similar, we ought to privilege “the point of view of hemoglobin” and regard humans and gorillas themselves as slight variants of one another. Second, Morris Goodman (1963) serologically rediscovered the genetic intimacy of human and chimpanzee and the greater evolutionary distance to the orangutan, and he argued to reclassify them on that basis.

Neither claim was particularly well received. Paleontologist

Louis Leakey, for example, “at times found it hard to be patient with the views of some of my colleagues.” Primate anatomist Adolph Schultz acknowledged that “some of my comments on tentative conclusions may have sounded rather critical.”<sup>4</sup> G. G. Simpson (1963, 1964) could not imagine privileging the point of view of hemoglobin over that of the hallux, ilium, or gall bladder, nor privileging the genetic similarity of human and chimpanzee over the ecological difference. Under the existing principles of animal taxonomy, which he had recently summarized in a book called *Principles of Animal Taxonomy* (Simpson 1961), the optimal scientific product was one that best encapsulated the diverse glimpses afforded by different approaches and data sets. Thus genetics, and more generally phylogeny, was simply a piece of a puzzle, the puzzle of representing evolutionary relationships, which subsume both descent and divergence, and encoding them in a simple linguistic framework. This was a tenet of what Julian Huxley had called “the new systematics,” for which Simpson had emerged as the principal spokesman. The new systematists had recently repulsed a challenge from the numerical taxonomists (Hull 1988), who were at least biologically competent, if philosophically at odds with contemporary practice. But in privileging genetic data over all others (notwithstanding the crass self-interest in doing so), particularly data in which the differences between human and ape are not readily visible, and arbitrarily privileging phylogenetic relationships over all other kinds of relationships, Zuckerkandl and Goodman were seen by the synthetic theorists as simply biologically incompetent and best left to their biochemistry.<sup>5</sup>

Goodman later accused Simpson of rejecting his classification for reasons of anthropocentric and antievolutionary prejudice (Goodman 1996; Hagen 2009). Zuckerkandl wrote it off to a prejudice against genetics (Aronson 2002; Dietrich 1998; Sommer 2008; Suárez-Díaz and Anaya-Muñoz 2008). In fact, it was the arrogance and ignorance behind the claims themselves that put the systematists off molecular anthropology. Alan Boyden was no less dismissive of Goodman’s interpretations than Simpson was (Boyden 1973; Hagen 2009).

The late 1960s brought the great triumph of molecular anthropology, Sarich and Wilson’s demonstration that leading paleontologists had grossly misrepresented the significance of the fossil *Ramapithecus* to human evolution, for *Ramapithecus* was about three times as ancient as the human lineage was. This discovery did not necessarily have any bearing on either the value of the viewpoint of hemoglobin (the similarity of the blood and the intimacy of the biological history implied by that similarity were familiar but newly quantified; and Goodman himself rejected the molecular clock) or the ne-

4. L. S. B. Leakey to Lita Osmundsen, July 24, 1962; Adolph Schultz to Lita Osmundsen, July 23, 1962, Wenner-Gren Foundation for Anthropological Research.

5. Ironically, the field of numerical taxonomy had little interest in the molecular anthropological work. It was predicated on the analysis of bodies, not biomolecules, and undervalued phylogeny.

cessity of reclassifying the apes (aside from *Ramapithecus*) on that basis. The viewpoint of hemoglobin, however, would come to be increasingly privileged in the 1980s, in lockstep with the rise of the Human Genome Project and its attendant public relations campaign, which came to be known as “genohype” (García-Sancho 2007; Holtzman 1999). By 1992, Jared Diamond could parlay Zuckerkandl’s inability to tell an ape from a human genetically into the central argument of his best seller, *The Third Chimpanzee*. And the rise of phylogenetic systematics (Eldredge and Cracraft 1980; Hennig 1965), radically revising the premises of classificatory practice in biology, would make Morris Goodman into a prophet, as opposed to having simply misunderstood the principles of contemporary systematics as they existed in the 1960s.

The point is that the rise of molecular anthropology in the 1980s had less to do with discoveries and data and far more to do with changing epistemic assumptions within evolutionary biology (Marks 2009). In particular, the decade of the Human Genome Project came with a higher privilege accorded to genetic data and relations (not to mention a higher privilege accorded to genetic explanations for human behavior; see below). The simultaneous privilege accorded to cladistic classification—that is to say classifying only by descent with no attention given to divergence—also placed a premium on genetic data, which tend to preserve a retrievable record of descent moreso than of adaptive divergence.

The 1960s saw the decline of racial serology in parallel with the ascendance of Washburn’s “new physical anthropology”—refocusing human biology on the common themes of being human at the expense of the old pseudotaxonomic divisions (Haraway 1988; Marks 2000). Patterns of human variation had come to be seen differently, with the human species “constituting a widespread network of more-or-less interrelated, ecologically adapted and functional entities” (Weiner 1957), which began to call into question the very ontology of race. Adaptation was biocultural and local, and higher-order clusters of people were ephemeral and united as much by economic and political histories as by gene flow (Hulse 1962). Moreover, geographical variation in the human species was seen to be patterned principally clinally (Livingstone 1962). This tended to make the entire racial enterprise, centered on the pseudotaxonomic question of how many basically different kinds of people there are, seem nonsensical. Thus, the major reviews by Campbell (1962) and Boyd (1963) passed largely unnoticed, marking an intellectual dead end. Indeed, the study of race itself began to acquire a distinctly unappealing flavor in the 1960s; human differences were not nearly as important as equality and fairness, which were issues of social justice, not biology. Moreover, those with the most intense scientific interest in race sometimes seemed unsettlingly the most committed to its use as a social weapon, as Carleton Coon’s *The Origin of Races* (purporting to show that blacks had become *Homo sapiens* 200,000 years after whites) was brandished by the segregationists, with the author’s private blessing (Jackson 2001). By the 1970s, major texts on

human variation could casually get by without the word “race” even appearing in the index (Johnston 1973; Underwood 1979). And further, the geneticists were reinventing the problem.

Richard Lewontin’s 1972 study “The Apportionment of Human Diversity” (Lewontin 1972) is generally taken as a landmark, showing that race “is a myth” or “doesn’t exist.” But the race concept had been under criticism as a natural structure of the human species for decades and had undergone a significant transformation. Into the 1920s, race was considered to be an essential property of the body transmitted genetically (although according to cultural rules apparently quite distinct from those that geneticists had been formalizing), and where ambiguous, it was to be diagnosed as a physician diagnoses a disease (Hooton 1926). In other words, it was a part of you. A convergence of population genetics and the rise of the racialized Nazi state stimulated a series of publications that reconceptualized race not as something that was a part of you, but as something that you were a part of—that is to say, as a population (Boyd 1950; Dobzhansky 1937; Huxley and Haddon 1935; Montagu 1942). This transformation was effectively completed at the 1950 Cold Spring Harbor Symposium on genetics and physical anthropology, organized by Dobzhansky and Washburn, during which the elderly Hooton told his former student, “Sherry, I hope I never hear the word ‘population’ again!” (S. L. Washburn, personal communication).

If people were now considered to be parts of gene pools rather than embodiments of distinct types, the question remained, just how discrete were these gene pools? Certainly a dedicated racial theorist, such as Carleton Coon, could navigate readily between the two concepts—race as embodied type and race as gene pool—assuming that the gene pools in question were considerably different from one another. Lewontin showed that human gene pools were not very different at all; thus, even if one conceptualized race as a gene pool, and even if one compared the most divergent populations, there was still considerably more overlap than difference.<sup>6</sup>

A similar finding had been made by Luca Cavalli-Sforza, who adopted the new statistical and computational methods of numerical taxonomy to construct trees of human populations from their minor genetic differences. The relationship between these trees and human history was never particularly clear, however. Hooton (1946) knew that the “racial” history of the human species involved so much admixture that he drew it literally as a capillary system, with reticulating networks of diverging and converging “blood streams.” Twenty years later, the population geneticists could produce bifur-

6. The observation that the ranges are far broader than the mean differences among human racial groups was a familiar one and is explicit in the second (1951) UNESCO Statement on Race. Genetics now could quantify that observation, and indeed it has held up with many kinds of genetic markers. It ignores the possibility of focusing specifically on the differences between the most divergent populations, characterizing them and redefining that as race, however.

cating trees, but appreciated that human history was not in fact a series of cladistic events. The trees represented similarity, reduced from multiple dimensions to two dimensions, but could not legitimately purport to represent the history that produced the pattern of similarity (Marks 2002).

Nevertheless, historical inferences were precisely what the human population geneticists began to derive, and like the early racial serologists, they saw their results as confuting the anthropologists. In particular, the issue Cavalli-Sforza chose was, which two of the three major races are most closely related? The esoteric statistical analysis of serological data suggested Europeans and Africans; a similar analysis of anthropometric data suggested Europeans and Asians. As the serologists had done decades earlier, Cavalli-Sforza simply concluded that the genetic inference was correct and the anthropometric data were somehow misleading (Cavalli-Sforza 1974; Cavalli-Sforza and Edwards 1964). Other population geneticists with other statistics managed to retrieve the ostensibly “anthropometric” tree (Nei and Roychoudhury 1972, 1974), and it would not be until the wake of “mitochondrial Eve” that Cavalli-Sforza would acknowledge how dodgy these conclusions actually were:

Blood groups and enzyme polymorphisms gave different results with respect to the location of the root, with blood groups still showing greater similarity between Africans and Europeans than between Europeans and East Asians. . . . With enzymes and proteins, however, Europeans were closer to East Asians than to Africans. . . . With some contradiction [new DNA data] tend to confirm the African-non-African split, but they are affected by biological or statistical weaknesses. (Cavalli-Sforza et al. 1988:6002)

Actually, the emerging consensus following the mitochondrial Eve work was “none of the above.” If the African gene pool is ancestral to the European and Asian gene pools and subsumes them, then it cannot be used as a contrast to them. It is rather like asking whether Rodentia are more closely related to Primates or to Mammalia; Mammalia subsumes the other two categories, thus rendering the answer produced by the computer largely meaningless, because it depends entirely on which particular specimens of Mammalia are chosen to represent that group. While the structure of the tree itself is sensitive to demographic histories such as migration, amalgamation, and population expansion, and to the assumptions built into the clustering algorithm, it is also sensitive to the choice of samples and what they are intended to represent. Population “splits” as cultural-historical events might indeed be there, but it is unclear just how to identify them from a tree of genetic similarity.

It is worth noting that race never left the forefront of this research in human population genetics (Reardon 2004). The geneticists, however, utilized it in diverse ways. In some hands race was negated (Cann, Stoneking, and Wilson 1987; Lewontin 1972, 1974); in others it was adopted as an unproblematic analytical category (Nei and Roychoudhury 1974);

and in still others it was—somewhat paradoxically—simultaneously both mythologized and reified (Bowcock et al. 1991; Cavalli-Sforza, Menozzi, and Piazza 1995).

## Big Science and Corporate Science

In the 1980s, human genetics came to recrystallize around the goal of sequencing a human genome at the cost of several billion taxpayer dollars. Bolstered by the geno-hype (Holtzman 1999) mobilized to secure popular interest and federal funding, the purple prose and hyperbolic inanities (“We used to think our fate was in the stars. Now we know, in large measure, our fate is in our genes,” crowed James Watson epigrammatically [Jaroff 1989:67]) of the Human Genome Project fertilized other nearby fields as well. Hereditarian political philosophy took old concepts and repackaged them pseudogenomically with considerable public fanfare (Herrnstein and Murray 1994). Another beneficiary was the reborn field of human behavioral genetics, regularly finding (and subsequently losing) genes for homosexuality, alcoholism, aggression, depression, and other nonnormative behaviors, or brandishing curious anecdotes of identical twins separated at birth (Holden 2009) as if they represented unproblematic scientific data.

Once it was observed that the Human Genome Project seemed to be rooted in a naively Platonic view of the genome (Walsh and Marks 1986), human population geneticists created an opportunity for themselves. A Human Genome Diversity Project (HGDP; Cavalli-Sforza et al. 1991; Roberts 1991) could augment the Human Genome Project and be a boon to human population genetics, but it would require a rhetorical justification for the public expenditure. Tellingly, that justification would be drawn from antiquated views of anthropology, which left anthropologists ambivalent about the project in spite of its own population-level geno-hype (Diamond 1991; Kidd, Kidd, and Weiss 1993; Roberts 1992; Weiss, Kidd, and Kidd 1992).

Blood collection and analysis had become an anthropological staple since it was first carried out by Carleton Coon in 1922 in the wake of the Hirsfelds’ work to see whether the Rif in Morocco possessed racial blood traits that matched their racial physical traits. By the 1970s, following the work of James Neel in Amazonia and Cavalli-Sforza in central Africa, collecting blood samples had become commonplace in anthropology, although it was carried out on a small scale and a largely ad hoc basis. That, however, permitted it to fly under the bioethical radar, so to speak. By shining a bright light on the field, the diversity project inadvertently began to call into question the crucial data-collection practices of human population genetics in an era of heightened sensibilities about the property rights of indigenous peoples. Why did they need to make a collection of the DNA of the world’s human populations?

First, they invoked the tropes of “salvage anthropology,” namely, the imminent extinction of indigenous peoples, which

they complemented with discourses of isolation and purity (Barker 2004). It is worth noting in this context that half a century earlier the serologist William C. Boyd was challenged for genetically reifying his Navajo samples: he said they were “pure,” but the anthropologist Clyde Kluckhohn knew the ethnohistory of the community and knew that they were not at all “pure” (Kluckhohn and Griffith 1950). The Hopi geneticist Frank Dukepoo (1998) made the same point to the HGDP: “My father (a “Hopi”) is a mixture of Hopi, Ute, Paiute, Tewa and Navajo; my mother, on the other hand, (a “Laguna”) is a mixture of Laguna, Acoma, Isleta, Zuni and Spanish. Members of other tribes share similar admixture histories as our ancestors raided, traded or kidnapped to ensure survival of their numbers. . . . [I]t is reasonably safe to surmise the same situation for members of other ethnic groups” (242).

Second, in a post-NAGPRA era, one could hardly fail to take note of the complexities associated with making collections of blood as museums were being obliged to return their collections of bones. Issues of informed consent, financial interests, and the responsibilities of the researchers were raised reactively, if at all. Worse still, the organizational meetings pointedly spoke about indigenous peoples but not to them. The HGDP seemed to be recapitulating the colonial science of an earlier era (Cunningham 1997).

Third, the issue of consent itself in a cross-cultural context was complicated by the possible use of the samples against the wishes or interests of the subjects. In a civil case filed in 2004, the Havasupai sued researchers from Arizona State University in part on the grounds that, had they known that their DNA samples were going to be used to build scientific narratives and undermine their own narratives of autochthonous origins, they would not have given the samples.<sup>7</sup> But even more problematic is the use of the DNA samples for work that is manifestly racist. In 2005, geneticist Bruce Lahn purported to find a genetic deficit in two brain genes of the peoples of Africa (Evans et al. 2005; Mekel-Bobrov et al. 2005; Regalado 2006) using the HGDP (now the HGDP/CEPH) DNA collection.<sup>8</sup> One suspects that if the people were made aware of the use to which their blood samples were being put, they might be inclined to reconsider consenting.

And fourth, the HGDP appropriated to itself the cultural authority of science in matters of ancestry and very casually delegitimized any other ideas about kinship and descent (Atkinson, Bharadwaj, and Featherstone 2006; Egorova 2007; Tutton 2004). With an uneven track record, it was never clear that the HGDP could deliver on this promise, and it is not clear just how reliable the claims to historical accuracy are. In many cases, the genetic patterns are exceedingly subtle or may even be statistical reifications (Moore 1994; Novembre and Stephens 2008; Templeton 1998). Nevertheless, the scientific authenticity of their narratives of ancestry would be the

principal product marketed by its successor, the Genographic Project. But when the Genographic Project acknowledges that they only study “a small fraction of the genome—less than 2%” (Wells and Schurr 2009:184), it is hard to know how they could produce a picture of an individual’s ancestry that is either comprehensive or accurate.<sup>9</sup>

The Genographic Project was initiated in 2005 as a privately funded venture in human population genetics supported principally by National Geographic and IBM and hoping to transcend the issues that had undermined the HGDP. Once again, they were quickly burdened by ethical questions (Harmon 2006; Nicholas and Hollowell 2009) centered on consent and exploitation. A 2007 solicitation from the Genographic Project invited wealthy patrons to participate in a \$50,000 “Journey of Man” tour in a “VIP-outfitted Boeing 757” to visit exotic subaltern people and have the head of the Genographic Project personally analyze their DNA and establish fictive kin relations for them (Marks 2007).

The innovation of the Genographic Project was to identify a product to market, namely, ancestry (Wald 2006). The HGDP had been criticized for its interest in indigenous people and its lack of interest in populations it considered to be admixed, notably the urban and acculturated peoples of the world, which is most of the world. The Genographic Project would use those peoples to subsidize the study of the indigenous peoples. For \$99.95, I (the least indigenous person I know) can purchase either a mitochondrial DNA test or a Y-chromosome test and have my own haplotype matched to those of “global populations.” Their Web site explains:

To be clear—these tests are not conventional genealogy. Your results will not provide names for your personal family tree or tell you where your great grandparents lived. Rather, they will indicate the maternal or paternal genetic markers your deep ancestors passed on to you and the story that goes with those markers.

Once your results are posted, you will be able to learn something about that story and the journey of your ancestors.<sup>10</sup>

But because their mtDNA test would only be examining one of my eight great grandparents, it is therefore not analyzing ancestry in the familiar sense of the term; nor do they discuss the complexity of what ancestry actually means in reference to lives lived and journeys made hundreds of generations ago, when the number of my genetic ancestors was astronomical.<sup>11</sup>

9. The 2% value given includes analyzing the Y chromosome along with mtDNA. For women, only mtDNA is studied, which reduces the value by several orders of magnitude.

10. <https://genographic.nationalgeographic.com/genographic/participate.html> (accessed September 12, 2009).

11. Conservatively assuming four generations/century, my ancestors 20,000 years ago are two to the eight hundredth power, or a number with 240 zeroes after it. That would be 233 orders of magnitude greater than the number of people alive at the time if there were 10,000,000 people alive at the time. Many of those are common ancestors (i.e., I

7. The case was settled out of court in April 2010 (Marks 2010a).

8. Of course, the claim has not stood up.



The novelty here is the commodification of DNA information—the identification of a market, the construction of a demand for the information—and the continuity is provided by the acquisition of the comparative database from the bodies of indigenous people, guided by ethical considerations (or the lack thereof) of several generations past, but now unconstrained by the need for government approval or oversight to be funded. The funding is already in place.

The allure of the market and the creation of wealth through the production of genomic information has stimulated the development of corporate human genetics internationally, most notably in Iceland (Pálsson 2007). The most significant innovation of deCODE in Iceland was to dampen the criticism that the construction of a comparative database replicated colonial relations; Icelanders would be studying their own gene pool for the advancement of knowledge and, it is hoped, profits. Indeed, the growth of corporate science has stimulated historian Steven Shapin (2008) to argue that the corporate model is an alternate normative model of scientific knowledge production rather than simply an aberration of an idealized pure form of academic science. Nevertheless, even millennia ago, it was widely appreciated that when truth and wealth are concurrent goals, truth invariably suffers as a result (Matthew 6:24).

The “big science” triumph of molecular anthropology has been the chimpanzee genome, released with great fanfare in 2005. The most interesting claims involve identifying a baseline average level of difference between the DNA sequence of human and chimp and then identifying regions that appear to be “too similar” and presumably vital for survival, and regions that appear to be “too different” and presumably at the root of our adaptive differences from chimpanzees. While possibly valid in some cases, these assumptions have proven epistemologically difficult to sustain at face value (Prabhakar et al. 2006; Shi, Bakewell, and Zhang 2006).

Certainly the oddest results come from combining studies. The peopling of the New World, for example, has been argued on genetic grounds to have occurred in one wave, two waves, three waves, and more than three waves. The root of the genetic tree of human populations is generally taken to lie within African populations (Campbell and Tishkoff 2008), but it has also proven surprisingly difficult to exclude non-African input into the gene pools of the rest of the world (Reich et al. 2010; Templeton 1993).

Most paradoxically of all, the DNA from Neanderthals has been recently interpreted as indicating their sufficient *difference* from modern humans as to be separated from us at the species level, as *Homo neanderthalensis* (Lalueza-Fox et al. 2005). Concurrently, the DNA from chimpanzees has been recently interpreted as indicating their sufficient *similarity* to modern humans as to be separated from us at the species level, as *Homo troglodytes* (Wildman et al. 2003). Yet because

the difference between humans and Neanderthals is considerably smaller than that between humans and chimps, it follows that both of these inferences cannot simultaneously be true. One or the other or both must be wrong. Unfortunately, the molecular geneticists do not seem anxious or willing to explain to the rest of the scholarly community which of them it is. Quite possibly they cannot tell.

## Conclusions

The most basic conclusion from observing the crossroads of genetics and anthropology over the last century is that superficially you see very different patterns when you examine genetic data than when you examine more traditional kinds of data. This is as true when the gaze of hemoglobin is applied to human ancestry as when it is applied to human diversity. In both cases, however, the significance of the genetical viewpoint is strongly inflected culturally. The intimacy of human and chimpanzee bloods was long familiar to students of human evolution without the concomitant inference that that particular bit of knowledge necessitated a different representation of our place in the natural order, that is to say, without the belief that the apparent genetic relations were more “real” than all others. Moreover, within the human species, the genetic data revealed races when they were expected to, negated races when they were expected to, and consequently leave geneticists in disagreement on the subject at present (Koenig, Lee, and Richardson 2008).

This leads to the second conclusion, that human genetics gives out mixed messages about race because it only has access to one component of it (studying difference); anthropology provides the other (studying meaning). Race is not so much difference (because all populations and all individuals are biologically/genetically different); rather, it is meaningful difference (a subjective judgment that certain differences or patterns of difference are more important for classificatory purposes than other kinds and patterns of difference). Consequently, geneticists do not have privileged access to race and never have, because they study only difference. But reducing race to simply measurable difference leads to confusion. Indeed, the ambiguities expressed in the genetic work have led one philosopher of biology to try to resuscitate race as a set of formal naturalistic categories on the basis of a thoroughgoing confusion of genetically produced dendrograms with cladistic events in the prehistory of human populations (Andreasen 2004; Gannett 2004; Marks 2010b).

Finally, molecular anthropology reinforces the conclusions that contemporary historians are drawing about the highly mythologized scientific history of the nineteenth century. Most significantly, the central importance of human diversity in the origins of evolutionary biology has been considerably undervalued. The scientific positions of monogenism (one origin of *Homo sapiens*, most compatible with biblical literalism) and polygenism (different origins of the races, with the biblical story relating merely the most recent creation,

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am somewhat inbred), and many of them overlap with other people's ancestors (i.e., we are all related).

most compatible with geology) correlated with the political poles of abolitionism and slavery, respectively. Monogenism, being more morally defensible, necessitated an evolutionary view—one that saw Adam as the sole progenitor of all the diverse races—but was scientifically problematic, given the increasingly apparent antiquity of the earth and the succession of life. Darwin, coming from abolitionist families, made the monogenist position scientifically respectable by implying a single ancient origin of the human species, with that origin not being Adam, but rather a sort of ape (Desmond and Moore 2009; Livingstone 2008). Being already evolutionary, monogenism could readily adapt the Darwinian position to its ends.

And likewise today, human evolution and human politics are connected. Various political movements attempt to draw legitimacy from molecular anthropology, such as the push for ape rights (Cavalieri and Singer 1993) and more importantly ape conservation; and the molecular similarity of humans and chimpanzees is regularly recruited to help exaggerate their similarities to establish claims on behalf of ape “language” and “culture” (Fouts and Mills 1997; Savage-Rumbaugh and Lewin 1994; Wrangham et al. 1994).<sup>12</sup>

Thus, while the relationship between genetics and anthropology has evolved over the last century, through the emergence of race studies, American eugenics, Nazi race hygiene, the civil rights movement, molecular genetics, postcolonialism, and corporate genomics, we can see some general trends. Normative human genetics is not value neutral and is not disconnected from contemporary social and cultural politics. Indeed, it has commonly been more of an applied science than an abstract theoretical one, while nevertheless rarely if ever confronting its track record as applied science. Consequently, the value of anthropology for contemporary genetics probably resides strongly in helping to explore the cultural assumptions that inhabit the production and interpretation of its data, and that have for over a century.

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12. These usually involve confusing particular properties of language and culture for definitions. In humans, language and culture are seen as intimately connected, as related expressions of human social cognition; but in chimpanzees, the claims for language generally come from captive studies of carefully reared animals, while the claims for culture come from field studies of wild animals, and are largely epistemologically disconnected from one another.
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