

A New History of Anthropology

Edited by Henrika Kuklick

Race Across the Physical–Cultural Divide in American Anthropology

Jonathan Marks

The first generation of American physical anthropologists, known worldwide as the “American School,” functioned as apologists for the Confederacy. Southern politicians greatly appreciated the works of Samuel George Morton, Josiah Nott, and George Gliddon (Hrdlička 1914; see also Glick in this volume). After the Civil War, the field all but disappeared. It was resurrected at the end of the nineteenth century by Franz Boas at Columbia, and slightly later by Aleš Hrdlička at the Smithsonian Institution and Earnest Hooton at Harvard. Boas came to Columbia as a physical anthropologist, an expert on measuring schoolchildren and collecting Eskimo skeletons. On his recommendation, the Smithsonian hired Hrdlička in 1902; Hooton came to Harvard in 1913.

At Columbia, Boas promoted liberal scientific humanism, as had his mentor, Rudolf Virchow. In practical terms, this meant that the study of biological differences and processes was sharply differentiated from the study of cultural-historical differences and processes. Virchow had found Ernst Haeckel’s politically inflected Darwinism to be unscientific and repugnant (Massin 1996); Boas would likewise find Charles Davenport’s politically inflected Mendelism to be scientifically valueless (Boas 1916).

Eugenics and the Anthropological Divide

The intellectual struggle between Boas and Davenport has been little discussed, although they were both founders of their academic disciplines in America – anthropology and human genetics, respectively. Both were New York-based. Both had doctorates in science. And both wrote paradigmatic works in 1911, reaching opposite conclusions about the same issue: the relationship between primitive and civilized folk. Davenport’s *Heredity in Relation to Eugenics* argued for a Mendelian basis of cultural supremacy. Boas’s *The Mind of Primitive Man* argued that cultural dominance rested on historical contingency.

Davenport made genetics the key science of the modern world: it both explained observed phenomena and promised to provide solutions for social problems. His friend

Madison Grant endorsed practical strategies for curing social ills – eugenics – based on the assumption that populations with inferior germ-plasm were clearly identifiable: the poor should be sterilized, and immigration from southern and eastern Europe should be curtailed. Grant's *The Passing of the Great Race* was well received both by geneticists, such as MIT's Frederick Adams Woods, and by politicians as diverse as Theodore Roosevelt and Adolf Hitler.

In January 1926 the informal "Eugenics Committee of the USA" was succeeded by a formal organization, the American Eugenics Society. Within it, a large "Advisory Council" of scientists, clergy, physicians, philanthropists, reformers, and politicians served under a Board of Directors that included Madison Grant. Its first president was the Yale economist Irving Fisher. Davenport wanted Grant to be its second president, but Fisher objected that "our committee is criticized more for his membership than anyone else, I think and it seems to me that it would be bad policy to make him President especially at the start" (June 10, 1926, CDP).

Evidently, Grant's Nordicism, racism, and xenophobia were controversial, despite his strong reputation among many politicians and academics. One of his most vocal critics was Boas, who wrote a damning review of *The Passing of the Great Race* for *The New Republic*. Hrdlička, the founding editor of the *American Journal of Physical Anthropology*, asked Boas to review the second edition of the book, saying that "what you said was so good that I hoped you would write another similar review." Unfortunately a review was never published, and Blakey (1987) makes a case for Hrdlička opportunistically playing off both sides.

In 1926, the AES Advisory Council included the geneticists Castle and East (Harvard), Conklin (Princeton), Guyer (Wisconsin), Holmes (Stanford), Jennings (Johns Hopkins), Walter (Brown), Woods (MIT), Wright (Chicago), and Little (Michigan; later, Jackson Laboratories). Notably missing was Thomas Hunt Morgan, the pioneering *Drosophila* geneticist, who worked in the same building as Boas at Columbia. In deference to Henry Fairfield Osborn, President of the American Museum of Natural History, Morgan did not confront the eugenicists, but published some oblique criticisms in the mid-1920s (Allen 1978; Kohler 1994). The first geneticist-defector from the eugenics ranks was Herbert Spencer Jennings, who reanalyzed the eugenical data presented to Congress (purporting to show a gradient of criminality among the populations of Europe from northwest to southeast) for the social work periodical *The Survey* in 1923. The most newsworthy defector was biologist Raymond Pearl (also of Johns Hopkins), whose widely publicized 1927 critique in H. L. Mencken's *The American Mercury* ultimately cost him a position at Harvard (Glass 1986).

America's two most prominent physical anthropologists also sat on the AES Advisory Council. As early as 1923, however, Hrdlička had complained to Irving Fisher, "During the last five years by mischievous publications such as Grant's and others, a great deal of mal-information has been spread and intolerance aroused among our own people" (June 7, 1923, HP). By contrast, Hooton remained an outspoken advocate of the eugenics program well into the 1930s. When Madison Grant sent Hooton his *Conquest of a Continent* in 1933, Hooton wrote to him, saying, "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" (November 3, 1933, HP). Not until 1936 did Hooton write to the Executive Secretary of the AES to have his name removed from the Advisory Council.

Hooton was nevertheless anxious to differentiate his own physical anthropology from that of the Nazis. He tried "emphatically to dissociate the finding of his science from the acts of human injustice which masquerade as 'racial measures' or 'racial movements' or even 'racial hygiene'" (1936: 512). More significantly, Hooton worked with Boas to draft a resolution condemning the Nazi Nuremberg Laws, circulating it to seven prominent human biologists; but only Hrdlička signed it (Barkan 1992: 314). Indeed, until the 1939 meetings of the American Anthropological Association, the members of the AAA had declined to pass a resolution condemning the Nazis, largely on the grounds that it was perceived to be sponsored by Boas, presumably an interested party as a person of German Jewish origin. Ultimately, the AAA passed such a resolution, officially written by Hooton and presented by Fay-Cooper Cole of the University of Chicago.

Hooton's actual views, however, were hard to specify. He stood nearly alone among American physical anthropologists in criticizing Nazi physical anthropology, but also sponsored the British botanical geneticist and racist R. R. Ruggles Gates, as well as William R. Sheldon, who sought to correlate body form with personality traits. Gates was an unapologetic polygenist, insisting that humans should be seen as comprised of several different species because the interbreeding criterion for the demarcation of species was irrelevant; he was held in utter contempt by the geneticists J. B. S. Haldane (1962) and Theodosius Dobzhansky.¹ In his Foreword to Gates's 1948 book, Hooton coyly disavowed the conclusions that followed; and privately to Robert Yerkes, while not praising the work, Hooton characterized Gates far too mildly as having "not fallen under the influence of the Boasian school of anthropology which insists upon discounting racial differences" (July 12 1949, YP). And many were perplexed by Hooton's sponsorship of Sheldon, whose *The Varieties of Human Physique* (1940) was dedicated to Hooton; Hooton's own students saw little merit in the entire endeavor.

The Postwar Years

Madison Grant died in 1937. The Eugenics Record Office was closed by its chief patron, the Carnegie Foundation, in 1940. Franz Boas died in 1942. Aleš Hrdlička died in 1943. Charles Davenport died in 1944, as sitting president of the American Association of Physical Anthropologists (having been nominated by Hooton). Only Hooton was left to represent the "old guard," and Hooton himself, like many Americans, had migrated politically to the right (which may partially explain the sentiments expressed in the correspondence quoted above). A new generation of American cultural anthropologists emerged to be the antagonists of the scientific racists.

On the political scene, however, the Nazi menace had been replaced by the Communist menace. Prior to World War II, the Nazis were not principally the enemies

of American democracy, but of Communism – and, indeed, the Nazis had directed their strongest political invective against the Communists. It was thus reasonable for American intellectuals who came to maturity in the 1930s, and who opposed both the racism at the core of Nazi ideology and the racial inequalities in America, to gravitate toward the Communists, the strongest enemies of Nazism. Of course, much Communist sympathy among American intellectuals dissipated with the non-aggression pact signed by Hitler and Stalin in 1939.

During and after World War II, the intellectual burden of anti-racism was picked up by the heirs of Boas in cultural anthropology. Notably, two cultural anthropologists, Ruth Benedict and Gene Weltfish, co-authored a pamphlet called “The Races of Mankind,” which had been solicited by the USO for distribution to American soldiers, and which was intended to inform soldiers about the ideological issues dividing the Allied and Axis powers. It was withdrawn, however, after the chair of the House Military Affairs Committee, Andrew J. May of Kentucky, judged its strident egalitarianism un-American; ultimately, he had it declared subversive. As early as 1944, the cultural anthropologist Ralph Linton (who was not in the Boasian intellectual lineage, and famously hated Ruth Benedict) reported to the FBI that Boas had been a Communist, and had been surrounded by Communists, including Weltfish (Price 2004: 111). Weltfish’s career was to suffer in the anti-Communist frenzy of the postwar period; in 1953, two weeks before she was forced to testify before Senator Joseph McCarthy’s committee, she lost the job she had held at Columbia since 1936.

Certainly the most vocal and influential exponent of anti-racist anthropology was the enigmatic Ashley Montagu (Sperling 2000). Born Israel Ehrenberg in London’s East End, he began by studying physical anthropology informally with Sir Arthur Keith, and later studied cultural anthropology formally with Bronislaw Malinowski at the London School of Economics. At this time, he reinvented himself as Montague Francis Ashley Montagu. In 1931, he emigrated to the United States, writing a letter introducing himself to Hooton at Harvard, in which he cavalierly misrepresented his credentials:

I am twenty-six, educated at Cambridge, Oxford, London, Florence, and Columbia. M.A., Ph.D., etc. fifteen anthropological publications. Recommended very generously by Sir Arthur Keith, who has furnished me a too-glowing testimonial which you may see if you wish. Sir Arthur once told me that I can always say that he will speak for me, so I may as well mention this too, for if you hold him in as great respect as I do, this should be impressive. (December 28, 1931, EHP)

In fact Montagu had not matriculated at either Cambridge or Oxford. He would not earn a PhD for several years, and it would be in cultural anthropology, under the supervision of Ruth Benedict. Nevertheless, he got a job teaching anatomy to dental students through Hrdlička. In 1941, he launched his first attack upon the central concept of physical anthropology – race – combining the Boasian approach with the arguments advanced in Britain by the biologist Julian Huxley and the anthropologist Alfred Cort Haddon in their *We Europeans* (1936). Montagu maintained a cordial

correspondence with Hooton, whose sponsorship (or at least benign neglect) would be needed for any advancement in physical anthropology.

Meanwhile, in the wake of revelations of Nazi horrors UNESCO's president, Julian Huxley, sought to have a formal statement issued about race. An international panel of anthropologists was assembled under Arturo Ramos, a Brazilian anthropologist, who died suddenly, leaving Montagu acting as "*rapporteur*" (Barkan 1996). The resulting UNESCO Statement on Race was issued in 1950, and left the "old guard" biologists and physical anthropologists sputtering about the divide between cultural and physical anthropology. To them, it was evident that the Statement had been drafted principally by cultural anthropologists – and authored by its *rapporteur*, Montagu (Stewart 1961). As one of the angered physical anthropologists wrote, the original statement

was drawn up by eight men, one each from seven countries with Ashley Montagu as *rapporteur*. Only one, save the *rapporteur*, is a physical anthropologist – Juan Comas of Mexico. The United States was represented by a Negro sociologist, E. Franklin Frazier; France by Claude Levi-Strauss, a Jewish sociologist, the UK by Morris Ginsberg, profession unstated.² Not a single expert on race had anything to do with it. There were no Germans or Austrians.

It was sent to about 90 scientists, including myself. Darlington, Sir R. Fisher, Genna (Italy) and I "are frankly opposed to the statement". (Carleton Coon to Sarah Dees [undated], CP)

Darlington and Fisher were notable names to drop, being well-known for their conservative political views. It was not surprising, however, that there were no Germans or Austrians. Consider that the leading German physical anthropologist was Eugen Fischer, who had joined the Nazi Party in 1940 after years as a collaborator. In *Science*, the expatriate Franz Weidenreich declared that Fischer merited trial as a war criminal (1946; see also Goldschmidt 1942). But the conservative backlash against the 1950 statement was powerful, particularly in England. The British journal *Man* published a long series of critical comments on it; and in response, a second UNESCO Statement on Race was drafted in 1951. Anxious lest the meeting be dominated by "out-and-out racists," which would result in a "pretty sad" statement (Dobzhansky to Montagu, February 24, 1951, AMP), the anti-racist scholars arranged to have the liberal geneticist L.C. Dunn serve as *rapporteur*. The second statement emphasized the biological aspects of debates about race – and principally the indeterminacy of many key issues, such as intelligence. Even so, many senior physical anthropologists and biologists took exception to this statement. Their criticisms were solicited and published as *The Race Concept: Results of an Inquiry*, by UNESCO. Notable among the few physical anthropologists and geneticists who failed to respond to UNESCO's solicitation was Hooton.

Montagu had successfully undermined the concept of race, central to physical anthropology, but at enormous professional cost. Untenured at Rutgers, he was a prime target for the McCarthyites. Succumbing to political pressure, Rutgers summarily fired him, and he found all other academic avenues blocked. He was forced to earn his living as a lecturer and writer.

Mainstream physical anthropologists, Hooton's former students, were less intrigued by any possible deterministic relationships between the head and the mind than Hooton had been. None elaborated his facile (if industrious) contributions to criminology (Hooton 1939a, 1939b). One (Carl Seltzer) kept somatology alive; and another (Carleton Coon) kept racial studies alive. The others explored diverse issues, from local variation in skull form to population genetics, and the malleability of the human body. The student who epitomized the generational change in the field was Sherwood Washburn, who wrote a thesis in 1940 on the anatomy of monkeys, and then moved to New York to take up a position at Columbia University's Medical School.

At Columbia, Washburn befriended the evolutionary geneticist Theodosius Dobzhansky. Together, they organized a major conference at Cold Spring Harbor on "The Evolution of *Homo sapiens*," bringing together leading physical anthropologists – among them a goodly number of Hooton's former students, as well as Hooton himself. Their objective was to effect a major shift in theoretical and empirical focus from abstract races to real populations. Washburn recalled that Hooton took him aside, to say "Sherry, I hope I never hear the word 'population' again!" (personal communication).

In the following year (1951), Washburn published his most famous paper, describing the shifts that had taken place within his cohort – from the race to the population, from categorization to function, from typology to plasticity, from static classification to evolutionary dynamics, from speculations about heredity to genetic studies, and from humanity's discrete divisions to the underlying common history of the species (Haraway 1988). This was a "new physical anthropology." Adopting the biologists' Evolutionary Synthesis, physical anthropologists subtly but crucially reconceptualized race. It was now understood as a mega-cluster of populations, not an essentialized form. This facilitated the study of race genetically, as it was, for example, by Boyd (1963) and Cavalli-Sforza, which meant identifying people as parts of races, rather than races as inhering within people.

Hooton had employed a fundamentally Platonic idea of race, "a vague physical background, usually more or less obscured or overlaid by individual variations in single subjects, and realized best in a composite picture" (1926: 79). But much rethinking was required when a race was considered to be a cluster of people, rather than an underlying abstract essence they shared. Claims such as "some persons who appear to be white show definite negroid or mongoloid skeletal features" (Hooton 1926: 78), or that a prehistoric skull from Colorado might be "pseudo-Australoid" (Hooton 1930) – became nonsensical, once race was redefined in terms of ancestry and geography. The specialist now focused on adaptation and relatedness, no longer on racial diagnosis and identification.

Indeed, to focus on the issue of race now reduced simply to assuming there were natural global divisions of people, then asking how many there were and where to draw the lines among them. Frederick Hulse (1962) argued compellingly that race was no more than a transient pattern of the gene pool. And, of course, in the real world of migration and admixture, to focus on race effectively required reimagining the racial world of 1492 (or perhaps of 1492 BCE), rather than analyzing the racial world of 1960.

The Racial World of 1960

With the accession of the Nazis and their troubling ideology, Hooton and Hrdlička had laid claim to the “real” scientific study of race, although their writings were permeated with the popular prejudices of the day. For example, Hrdlička (1930: 170) summarized the aptitudes of the races in tabular form, noting that “musical ability” was in blacks “well represented, but not of high intellectual order” (he was apparently not much of a jazz fan). By the 1960s, however, the racial issues of interest to the public were civil rights in America and decolonization in the rest of the world. Those anthropologists best able to contribute to consideration of these movements were cultural anthropologists, and consequently discourses of race were replaced by discourses of ethnicity and nationalism (Baker 1998).

And yet, as the civil rights movement advanced, there were still many who saw egalitarianism as Communistic, and, indeed, as un-American. Among them was a wealthy textile magnate called “Colonel” Wickliffe Draper, who had established a philanthropic scientific endowment named “The Pioneer Fund” in 1937, with Charles Davenport’s protégé, Harry Laughlin, as its first president. The previous year, Laughlin had been awarded an honorary degree from Nazi-controlled Heidelberg University, for his inspiration in drafting model eugenic sterilization laws. The possibility that he might accept it in person was scandalous even to Davenport (Kevles 1985).

With Draper’s financial support, Davenport undertook work in the 1920s to show that miscegenation in Jamaica had produced an inferior hybrid race; even other eugenicists judged this research to be egregiously flawed (Castle 1930; Pearson 1930). By the 1950s, Draper was using the Pioneer Fund to channel financial assistance to opponents of integration and civil rights within the academy, and even to scholarly non-academicians. In 1959, some of these activists incorporated the International Association for the Advancement of Ethnology and Eugenics (IAAEE), and shortly thereafter began publishing *Mankind Quarterly*, with Draper’s backing (Tucker 1994, 2002; Winston 1998). Prominent among these activists were Columbia University psychologist Henry E. Garrett, UNC anatomist Wesley Critz George, and a sometime historian and former president of Delta Airlines, Carleton Putnam.

Garrett had testified for the defendant in 1953 in the famous *Brown vs. Board of Education* trial. In the first volume of the *Mankind Quarterly*, he ranted against “the equalitarian dogma,” crediting it not to Thomas Jefferson but to the Communists, anthropologists, and Jews, and particularly to Franz Boas. Under the editorship of an obscure Scottish nobleman named Robert Gayre, *Mankind Quarterly* listed Garrett and the geneticist Ruggles Gates (see above) as associate editors.

The journal immediately ignited a controversy in the scientific community. The British physical anthropologist Geoffrey Harrison (1961: 163) reviewed it angrily in *Man*: “Few of the contributions have any merit whatsoever, and many are no more than incompetent attempts to rationalize irrational opinions.” Likewise, the Mexican physical anthropologist Juan Comas (1961) wrote an extended critique in *Current Anthropology*. Santiago Genoves (1961) brought *Mankind Quarterly* to the attention of the readers of *Science*.

The Yugoslav anthropologist (and Dachau survivor) Božo Škerlj resigned from *Mankind Quarterly's* editorial board upon discovering its ideological stance, and criticized the journal in *Man*, only to be sued by Gayre and Garrett for associating them with Nazi ideologies.

Carleton Putnam had become an active spokesman for segregation in the late 1950s. His National Putnam Letters Committee took out large pro-segregation advertisements in major newspapers, often written as open letters to the president – for example, in *The New York Times* on January 5, 1959. In 1961, the Public Affairs Press released Putnam's short book, *Race and Reason: A Yankee View* (originally titled *A Warning to the North*), which not only made the argument against integration, but also followed Henry Garrett in laying the blame for the idea at the feet of anthropologists, Communists, and Jews (even more explicitly than Garrett had), and reviling Franz Boas in particular. But Putnam went further, and discussed the qualifications of different types of anthropologists to issue authoritative pronouncements on race. Anthropologists came in two varieties, cultural and physical. The equalitarian movement had been led by the cultural anthropologists, although it was the physical anthropologists who were the experts on racial biology (1961: 51–2).

Apparently, Putnam had at least one ally within the anthropological community, tutoring him on the state of the field, at least as that ally saw it. And Putnam acknowledged as much:

Besides intimidation there has, of course, been a false indoctrination of our younger scientists, although some hope on this score may be found in the following statement in a letter to me from a distinguished scientist younger than I am, a scientist not a Southerner, who is a recognized international authority on the subject we are considering: "About 25 years ago it seemed to be proved beyond a doubt that man is a cultural animal, solely a creature of the environment, and that there is no inheritance of instinct, intelligence or any other capacity. Everything had to be learned and the man or race that had the best opportunity for learning made the best record. The tide is turning. Heredity is coming back, not primarily through anthropologists but through the zoologists. It is the zoologists, the animal behavior men, who are doing it, and the anthropologists are beginning to learn from them. It will take time, but the pendulum will swing." (1961: 50)

To Putnam, the proposition that different groups of people had roughly equivalent intellectual endowments, and were entitled to equal opportunities and rights, was radical. It was not radical, however, for most anthropologists, whether cultural or physical. The American Anthropological Association quickly passed a resolution introduced by its outgoing president, archaeologist Gordon Willey, condemning Putnam's book (Margolis 1961). The following spring, the American Association of Physical Anthropologists passed a similar resolution, introduced by Stanley Garn.

The president of the AAPA was Carleton S. Coon of the University of Pennsylvania, who had been Hooton's second doctoral student. He later recalled the events of that business meeting:

I was involved in that I was related to the author through both the Carletons and the Putnams. He had sent me a copy, and I had read it. I had seen nothing actionable in it

...

I asked the audience how many had read Carleton Putnam's book? Just one. What did he think of it? Not much. How many had heard of it before? Only a few hands were raised. Yet these people were prepared to vote to censure Carleton Putnam. . . .

There they were, some of them old and trusted friends, apparently as brainwashed as Pavlov's puppies, or as most of the social anthropologists . . .

I told my fellow members that I would no longer preside over such a craven lot, and resigned the presidency. I would leave them to vote whatever resolution they wished, but not in my name. (1981: 334-5)³

Coon's story was more than a little disingenuous, however. His resignation was not accepted. While he was still president of the AAPA, he published his *magnum opus* on race. Originally pretentiously titled *On the Origin of Races* to evoke Darwin, Coon's book lost its initial preposition at some point along the way, and was published in October 1962. Its central premise was that the five types of humans (Caucasoids, Mongoloids, Australoids, Congoids, and Capoids [i.e., those from the Cape of Good Hope]) evolved into modern *Homo sapiens* from *Homo erectus* at different times; and, further, that this provided a deterministic, naturalistic explanation for the different "levels of civilization" each had attained.

Coon and Putnam: A Family Affair

Since the time of Madison Grant, the Boasians had successfully repudiated explanations of "civilization" that involved nature rather than history – so Coon's thesis seemed uncomfortably retrogressive. Although Hooton had always been a little uneasy with the Boasian wall separating social history and biological evolution, he had generally kept his doubts to himself.

Carleton Putnam, however, had devoted considerable energy in 1962 to editing and publishing a pamphlet by the anatomist Wesley Critz George on the intellectual inferiority of blacks, commissioned by the Governor of Alabama, distributed by the Putnam Letters Committee, and underwritten by the Pioneer Fund. He now actively promoted the work, in another open letter advertisement in *The New York Times*, this one addressed to President Kennedy and published on October 3, 1962. Cultural anthropologist Morton Fried of Columbia wrote in to quote both the AAA and AAPA resolutions against Putnam, but was blindsided by the response published in the *Times* on October 24 by Garrett and George.

[I]n his new book, "The Origin of Races," published Oct. 15, Carleton Coon, one of the foremost physical anthropologists, presents evidence indicating that the white race passed from the stage of *Homo erectus* to *Homo sapiens* 200,000 years before the Negro and is therefore 200,000 years ahead of him on the ladder of evolution.

One need look no further than the introduction to "The Origin of Races" to find these words: "It is a fair inference . . . that the subspecies which crossed the evolutionary threshold into the category of *Homo sapiens* the earliest have evolved the most, and that the obvious correlation between the length of time a subspecies has been in the sapiens state and the levels of civilization attained by some of its populations may be related phenomena.

Although Garrett and George's letter was published on October 24, it was dated October 14, actually the day before the publication of the book they were quoting. Coon himself was soon besieged by colleagues and journalists wanting to know what he had meant, and how the segregationists had been able to pounce upon what he had said so quickly.

The truth was uncomplicated. Putnam and Coon had been corresponding since 1960, when Coon began assisting Putnam in the preparation of the latter's *Race and Reason*. Putnam's extended quotation from a "recognized international authority" was taken from a letter from Coon dated June 17, 1960 (CP), and deliberately disguised. On May 17, 1962 Putnam had sent the anatomist George "sheets of quotations from Coon's book"⁴ (George to Putnam, May 19, 1962, GP). And just a few days later, Putnam told George that he wanted "to go over *The Biology of the Race Problem* [i.e., George's pamphlet] with you paragraph by paragraph. I now have the Colonel's comments as well as Coon's." With the publication of *The Origin of Races*, and Coon's help with both Putnam's *Race and Reason* and George's *The Biology of the Race Problem*, the segregationists had good reason to count Coon an ally.

Moreover, following Ruggles Gates's death, Coon was invited to join the editorial board of the *Mankind Quarterly*. There were affinities between Coon and Gates: Coon's racial taxonomy (which split sub-Saharan Africans, and lumped Americans with Asians) was identical to the one presented by Gates in *Human Ancestry*, but at a lower taxonomic level; and Gates had acknowledged Coon's assistance in reading and commenting on his 1948 book (see Eckhardt 2000). But Coon politely declined *Mankind Quarterly's* invitation, although not because the journal was an appalling disgrace to scholarly studies of human variation, but because it would look bad: "I will be very glad to get your monographs and also your magazine, to which I would be happy to subscribe," he wrote, "but I fear that for a professional anthropologist to accept membership on your board would be the kiss of death, here in the so-called land of the free and home of the brave" (Coon to Gayre, November 6, 1962, CP).

Coon's book received favorable notices from Hooton's successor at Harvard, William Howells, from the distinguished biologists Ernst Mayr and George Gaylord Simpson, and from many others. Coon even retrofitted his earlier work to accommodate *The Origin of Races*. He revised *The Story of Man*, reducing the number of races identified in it from six to five, and changing the order in which they evolved, upon which his new book's radical conclusions were predicated. Coon had bluntly written in the first edition that "The Mongoloids are probably not as ancient as the Negroids" (1954: 198); this flatly contradicted the thesis articulated in *The Origin of Races*, and he deleted it in the new edition.

The rumors about Coon's connections to the segregationists had been swirling for months in the academic community. Stanley Garn, who had worked with Coon at Harvard, had co-authored *Races* (1950) with him, and had also put forward the AAPA motion to condemn Putnam's work, noted that "Carleton Putnam, racist pamphleteer and a cousin of Carleton Coon, leaked cryptic hints well before the Mississippi insurrection" [in the fall of 1962, when a black student named James Meredith tried to matriculate at the University of Mississippi] (Garn 1963).

As a full-time propagandist, Putnam certainly was not going to keep his newly acquired scientific arguments secret. Putnam had written privately months earlier, "When the President of the American Association of Physical Anthropologists, a magna cum laude graduate of Harvard and a native of New England, states that recent discoveries indicate the Negro is 200,000 years behind the White race on the ladder of evolution, this ends the integration argument" (Putnam to James A. Moss, June 4 1962, CP). Putnam's public addresses and pamphlets led Sherwood Washburn, the president-elect of the American Anthropological Association, to write to Coon, "I certainly hope that there is some way you can answer the weird political distortions which he makes from your evolutionary views. The less Putnam appears to be scientifically respectable, the better for all of us" (August 8 1962, CP). Coon would not repudiate Putnam publicly or privately, however. While the segregationists were already citing his forthcoming work, Coon adopted a self-protective posture of scientific detachment in his reply to Washburn: "We have no business getting involved in domestic social and political issues, particularly as an association" (August 18 1962, CP).

Many of the reviews of Coon's book, while favorable, noted that segregationists were invoking him, and politely (often condescendingly) averred that they were doing so improperly or inappropriately – as indeed Washburn had put it to Coon. *The Saturday Review* asked the distinguished evolutionary geneticist Theodosius Dobzhansky to review the book, and Dobzhansky wrote a polite, albeit critical, review, sending an advance copy to Coon as a professional courtesy. Coon, who had written obsequiously to Dobzhansky a few months earlier that the latter's ideas were very similar to those expressed in his own forthcoming book (May 26 1962, TDP), now read his own insecurities into the review and reacted vituperatively: "You accused me of 'mischievously' altering my style so as to provide easy quotes for political people. That is libel" (Coon to T. Dobzhansky, October 25, 1962, CP). Coon prevailed upon the editor of *The Saturday Review*, Norman Cousins, to pull Dobzhansky's review and to publish an excerpt from his own book (Coon 1981, 353).⁵ Dobzhansky's review came out in *Scientific American* and *Current Anthropology*.

Dobzhansky was in fact an ideal person to help relieve the pressure on the anthropological community, being an interested outsider (his research was on the evolutionary genetics of the fruitfly, *Drosophila*), and of great scientific stature. In his homeland, Dobzhansky had been deeply affected by the politicization of biology through the agency of Stalin's geneticist, Trofim D. Lysenko. He had begun his career in America working with Thomas Hunt Morgan, the only geneticist of prominence to have resisted the lure of the eugenics movement, and had become involved with the anthropological community in New York, working on projects with Ashley Montagu, Margaret Mead,

and Sherwood Washburn. Being a "real" biologist, non-Jewish, and an émigré from the Soviet Union, he was particularly immune from the segregationists' charge of undue influence of a Jewish/Communist/Cultural-Anthropological conspiracy.⁶

This was a juncture at which the solidarity of biological and cultural anthropology was crucial, and it was fortunate that the incoming president of the American Anthropological Association was a biological anthropologist, Sherwood Washburn. Washburn had been Coon's teaching assistant at Harvard, and had a testy relationship with their common mentor, Hooton. Hooton had been irked by Washburn's first paper on race, and could hardly have missed the fact that the "new physical anthropology" was effectively a repudiation of his own work.

Washburn, as a non-Boasian, was also well suited to chide Putnam for "greatly exaggerat[ing] the role of Boas in American anthropology" (Washburn to Putnam, December 5 1961, GP). Putnam fumed back, "It is not sociologists, nor cultural anthropologists, who are best qualified to speak on this subject, but physical anthropologists and geneticists" (Putnam to Washburn, December 12 1961, GP) – apparently unaware of the academic specialty of his correspondent! And obviously, if one of the central issues was the comparison and relative evaluation of the accomplishments of different groups of people, then the cultural anthropologists were indeed the most appropriate experts (Diamond 1962; Steward 1962).

Dobzhansky had already published a thunderous review of Putnam in the *Journal of Heredity*. Between Washburn and Dobzhansky, then, the two most highly respected "physical anthropologists and geneticists," invoked by Putnam, were in fact actively repudiating his work, even before the publication of *The Origin of Races* tied it to Coon's.

As a respected professional colleague, Coon had to be handled differently from Putnam, and Washburn agreed to address the "race issue" in his 1962 AAA presidential address (Washburn 1983: 19; DeVore 1992: 422). Washburn's address, subsequently published in the *American Anthropologist* (Washburn 1963), provided the community essentially with a "position paper" written by a leading physical anthropologist, thus undermining both the possibility that Coon spoke authoritatively for the subfield, and the argument that the AAA represented only cultural anthropologists, who differed from, and erred against, physical anthropologists in their understandings of race.

A tradition developed among Coon's friends that Washburn's presidential address had been a vehement personal tirade against Coon, and that the published version was much more polite (Shipman 1994). Washburn vigorously denied this rumor, but did not keep a copy of his address. But not only would that have been counterproductively indecorous for an issue of such delicacy, it would also have been unnecessary. Washburn's strategy was simply to define Coon and his work out of modern anthropology, as he had, in effect, been doing for over a decade.

At the annual meeting of the American Association for the Advancement of Science in February 1963, Dobzhansky acted as discussant in Stanley Garn's symposium on race, and

closed by referring to the adverse use of Coon's theory by racist propagandists. He remarked that this demonstrates that scientists can no longer remain in ivory towers, unconcerned

with the impact of their pronouncements, and that, indeed, it is naïve and irresponsible for them to pretend they can. (Leacock 1963)

In private, Coon and Dobzhansky had been conducting a heated correspondence for months (Jackson 2001). Coon finally responded publicly in *Science* to Dobzhansky's "charge, repeated many times since my book came out, that I wrote it irresponsibly, naively, or mischievously. I wrote it without evasion or provocation and as truthfully as I was able, fully aware of the abuse that would follow, but not expecting an officious rebuke from a man of Dobzhansky's stature as a scientist" (Coon 1963).

It is worthy of note that Coon did not attempt to protect himself under the shield of "academic freedom" – as professors subsequently associated with the *Mankind Quarterly*, Pioneer Fund, and their satellite organizations, would do. Coon's argument was that he could not be held accountable for whatever use people made of his work. But that position had become untenable, not least after J. Robert Oppenheimer's widely-quoted comment in 1947 that "physicists have known sin" for their development of nuclear weapons.

Coon never maintained that Putnam and the segregationists had misquoted or misused his work, which implies that they were drawing what he saw as proper inferences from it. And Coon was happy to send his academic enemies into the hands of the red-baiters. As he wrote to anthropologist Harry Turney-High of the University of South Carolina, "it seems to me that somebody should do [two] things: (1) investigate the communist influence on American anthropology via Bella Dodd [a Catholic lawyer for the Communist Party], Boas, and the Boasians. . . . (2) find out why Ashley Montagu changed his father's name retroactively in *Who's Who* and whether or not he has ever carried a [Communist Party] card."

After Putnam and Coon

The immediate academic reaction to the disputes over Putnam and Coon was that physical anthropology largely abandoned thinking about race as such, replacing it instead with population genetics. Thus, to the extent that race was a "real world" issue that students came to college engaged with, and opinionated about, physical anthropology effectively withdrew and relegated academic racial discourse to the population geneticists on one side and to the cultural anthropologists on the other. Cultural anthropologists' position had been laid out by the Boasians; but the population geneticists' position was naively ambiguous, sometimes denying the very existence of race (Lewontin 1972), and sometimes using race as an unproblematic analytic category (Nei and Roychoudhury 1974). Academic physical anthropologists turned their backs on the race concept, although it remained viable in forensic anthropology, in its "applied" connection to law enforcement – largely disconnected from theory (Brace 1995).

The Pioneer Fund, however, continued to find and support scientists whose work echoed the themes that had aroused Wickliffe Draper's interest in the likes of Charles

Davenport, Harry Laughlin, Henry Garrett, Carleton Putnam, and Wesley Critz George. As late as 1980, the Pioneer Fund gave money to reprint an excerpt from Carleton Coon's work (Lombardo 2002). They now made a subtle shift, however, by supporting work that seemed to affirm the general inheritance of behavior. This would be convergent with the racist agenda on the assumption that there is an easy translation between the reasons that different people act in different ways *within the same group*, and the reasons that *groups of people* act differently from one another. This elision of the sources of within-group and between-group variation became central to the arguments of Arthur Jensen, the Berkeley psychologist, one of the major beneficiaries of the fund in the 1960s. His student, Thomas Bouchard, began a widely publicized study of twins at the University of Minnesota, with the generous help of the Pioneer Fund. *The Mankind Quarterly* remained in print, edited by Roger Pearson (Lynn 2001; Kenny 2002; Lombardo 2002).

By 1970, Arthur Jensen and William Shockley – the Stanford physicist who was also a Pioneer beneficiary – were putting new spins on familiar stories, Jensen arguing for the innateness of IQ differentials, and Shockley for meaningful variation in the overall genetic value of human groups. Their scholarly opponents were principally population geneticists, such as Stanford's Luca Cavalli-Sforza and Harvard's Richard Lewontin (a Dobzhansky student).

In the late 1980s, a Canadian psychologist named J. Philippe Rushton, supported by the Pioneer Fund, made news with a theory that misapplied ideas from population ecology: Human races, he argued, had undergone divergent selective pressures on life-history variables, which could be inferred by IQ, head size, self-reported measures of sexual activity and sexual anatomy, lawlessness, and, of course, civilization. When his work, along with that of several other Pioneer Fund grantees and *Mankind Quarterly* contributors, was cited in the bestseller *The Bell Curve* (Herrnstein and Murray, 1994), it brought the journal under renewed public scrutiny, as well as the fund itself (DeParle 1994; Lane 1994; Littlewood 1995; Marks 2005).

Jon Entine's *Taboo: Why Black Athletes Dominate Sports and Why We're Afraid to Talk About It* (2000) repeated some familiar charges: scholarly discourse on race is stifled by a liberal-Jewish-anthropological conspiracy.⁷ One of Entine's principal sources was Berkeley physical anthropologist Vincent Sarich, whose own book *Race: The Reality of Human Differences* (Sarich and Miele, 2004), presented the same charges. While Sarich and Miele do not mention the Pioneer Fund, they discuss uncritically the work of its modern beneficiaries: Philippe Rushton, Richard Lynn, and Arthur Jensen. Then they speculate on what the "discovery" that indigenous Africans have a mean IQ of 70 might imply about Africans, rather than questioning what it might imply about the quality or character of the science that produced this figure.

There is no little irony in being able to counter their arguments by quoting none other than Earnest Hooton (1940: 107), assessing the implications of controlled adoption studies:

These data might be interpreted as indicative of the fact that intelligence is not innate but environmentally controlled. Or they may reveal that intelligence tests test principally

the environment of the testees and the intelligence of the testers. As Juvenal might have said, "Who shall test the testers themselves?"

... [T]o me the principal result of these fascinating experiments is to confirm a strong suspicion that diagnoses of superior or inferior intelligence cannot be made from intelligence tests (at any rate upon the testees).

Unsurprisingly, these modern neo-racist writers cite one another, acknowledging the complementary nature of their positions. Thus, the "2nd Special Abridged Edition" of Rushton's book – mass-mailed to social scientists – begins with plaudits for Entine. Sarich and Miele also praise Entine, after dismissing a century of anthropological progress – and then proceed to laud Rushton. Political scientist Charles Murray, co-author of *The Bell Curve*, provided blurbs for the works of Rushton and Sarich-Miele. Jon Entine became an adjunct fellow of the American Enterprise Institute – Charles Murray's base – in 2002. The same year, Philippe Rushton assumed the presidency of the Pioneer Fund.

Conclusions

The "four-field approach" had been in place in anthropology for over a century, originally instituted as a means of comprehensively "othering" the now-pacified American Indians. Its call to be "holistic" (i.e., anti-reductive) that emerged in the early 1960s (e.g., Mandelbaum et al. 1963) was to some extent a call to harmonize the polarity of the "physical" versus "cultural" studies that was now being exploited by the segregationists. Paradoxically, Carleton Coon was probably the closest to a generalist anthropologist the discipline had seen since Boas himself.

I think that the history of anthropology bears out the conclusion that consideration of race does not divide anthropology along subdisciplinary lines, and probably never has. Boas's position was nuanced, and evolved considerably during his lifetime, but was part of a liberal German tradition shared, for example, by Felix von Luschan (Smith 2002). Hooton's views were likewise nuanced, and changed during his lifetime.⁸ Ralph Linton, outside the Boasian lineage, discussed race and discussed the anthropological subfields in a 1938 paper, but did not suggest that they mapped on to one another.

The anti-racist Benedict-Welsh pamphlet, which was so threatening to Southern legislators during World War II, assumed the existence of races, but challenged the ranking of their innate intellectual capacities. The modern view, that human variation is clinally structured, and that race is principally a cognitive category rather than a natural one, was still controversial by the late 1960s. And while Carleton Coon had many friends and colleagues who were reluctant to criticize his work, his most devastating critics were nevertheless his fellow biological anthropologists. As the distinguished anatomist Wilfrid E. Le Gros Clark (1963) observed:

It seems that Dr. Coon places too much confidence in the ability of anatomists to distinguish one race from another by reference to single isolated skeletal remains; for

example, he believes that "Any good anatomist can tell the skeleton of human races apart." If this is the case, the present reviewer is certainly not a good anatomist.

Carleton Putnam, likewise, does not seem to have had much support in any quarter of anthropology. Although he may have tried to turn physical anthropologists against cultural anthropologists by claiming that the latter had no expertise on race, he was pilloried by both types.

We are unlikely to return to the days when Franz Boas, Clark Wissler, and Alfred Kroeber could sit on the editorial board of the *American Journal of Physical Anthropology* (in 1918). Biological and cultural anthropology are certainly not converging methodologically or intellectually, but race is not one of the principal elements on which they disagree. To the extent that race is still occasionally taken to be a natural category, it is generally only among narrow classes of applied physical anthropologists.⁹ Physical anthropologists are still more attracted to biologized theories – for example, to sociobiology in the 1970s and to evolutionary psychology in the 1990s – but the issue of race itself is now far less divisive in anthropology than it was to earlier generations. Consequently, at present there may be more agreement among the practitioners of all subfields of anthropology when the subject is race than there is on any other subject (Mukhopadhyay and Moses 1997).

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Notes

- 1 As a botanical geneticist, he was familiar with the problem of applying Ernst Mayr's "biological species concept" to plant species, which are capable of extensive cross-fertilization. Decades later, the distinguished plant geneticist G. Ledyard Stebbins would describe the BSC with considerable justification as "zoocentric" (personal communication). See also Osman Hill (1940).
- 2 Coon's characterizations here are worth noting, given the conspiratorial beliefs of the scientific segregationists (see below). Claude Lévi-Strauss is possibly distantly descended from the Biblical tribe of Levites, but could hardly be described as a "Jewish sociologist" by anyone familiar with his life and work; and Morris Ginsberg was a prominent British sociologist.
- 3 In his autobiography, Gabriel Lasker presents a different picture of events. He recalls Coon denying kinship with Putnam to the AAPA executive committee, and the floor vote on the resolution to have been "something like ninety-one 'aye' and one 'nay'." "[N]obody joined Coon in the vote against the motion, and Coon stormed out of the room" (1999: 148–9).

- 4 This referred to Coon's revised edition of *The Story of Man*, which he adjusted to accommodate his new theory. This volume was published several months before the *Origin of Races*, and the significance of the changes escaped Stanley Garn's (1962) review in *Science*. See below.
- 5 Based on Mead's (1963) attempt to mediate the situation, Jackson (2001) makes a case for the *Saturday Review* pulling Dobzhansky's review for procedural reasons, but this interpretation is supported by neither Dobzhansky's nor Coon's perceptions.
- 6 That did not stop the segregationists, however, from regarding Dobzhansky, at various times, as a crypto-Jew, crypto-Communist, and crypto-Boasian.
- 7 According to Entine, "I think the taboo is a reflection of white racism, liberal white racism. Also partly Jewish. All the thinkers who talk about it, men like Richard Lewontin, Steven [sic] Jay Gould, Jonathan Marks, they're all of my generation, '60s Jews who carry the weight of the Holocaust on their shoulders" (*The Philadelphia Daily News*, February 3, 2000). By coincidence, Entine's book is published by a descendent of the publishing house that handled Carleton Putnam's works: Public Affairs Press.
- 8 The "alien scum" to whom Hooton referred in his correspondence with Madison Grant, presumably subsumed impoverished Russian Jewish immigrants of the early twentieth century, and not the assimilated German Jews. Hooton's first graduate student, Harry L. Shapiro (who ultimately became Curator of Anthropology at the American Museum of Natural History, and remained loyal to his mentor), was in the latter category.
- 9 Forensic anthropologists have tended to be the noteworthy exception, crudely synonymizing the observation of gross skeletal variation and the existence of formal races. Now there are also "corporate geneticists," who market their determination of individual racial affiliations to the public. This is a marriage of high technology and low theory, and its success hinges on the ability to translate complex genetic patterns into simple folk idioms of kinship, including race; see Koenig et al. (2007).

Further Reading

- Barkan, E. A. 1993. *The Retreat of Scientific Racism*. New York: Cambridge University Press.
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- Marks, J. 1995. *Human Biodiversity: Genes, Race, and History*. New York: Aldine de Gruyter.
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