

Human Biodiversity as a Central Theme of Biological Anthropology: Then and Now

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The late 1990s see biological anthropology in a malaise.

On the one hand, the field has abstracted itself from contemporary racial discourse, which tinges the field as being inherently trivial in the popular mind; and on the other hand, those very issues which dominated biological anthropology for generations have been re-appropriated by cognate fields, trivializing biological anthropology within the academic sphere. Within the field itself, one still commonly hears that biological anthropologists must become better biologists or better scientists—but only rarely that they must become better scholars, better humanists, and better anthropologists.

The four papers which follow result from a symposium at the 1995 meetings of the American Association of Physical Anthropologists.¹ Their theme involves using *The Origin of Races*, written in 1962 by Carleton Coon, as a focal point for discussion of key conceptual issues in the history, epistemology, and teaching of human variation in physical anthropology. A glance at the professional or popular literature shows readily that many issues we thought were old, are in fact new. They are getting a new wind, and whether that reflects our own incompetence in getting our message across, or in even figuring out what our message is, or whether there is indeed something new that requires our scrutiny and appraisal, are possibilities upon which we need to reflect. To some extent they bear on the status of physical anthropology as a possibly more-than-just-marginal intellectual endeavor.

At one extreme we encounter the popularity of the para-scientific bestseller, *The Bell Curve*, crudely invoking innate differences between large segments of the human species to account for differences in economic and social status (Herrnstein and Murray 1994). Few academic works have ever matched the breadth of attention it generated in the mass media, although one such comparable work was Coon's 1962 book. At the other extreme we encounter the elitism of the hyper-scientific Human Genome Diversity Project, crudely calculating that "ancestral Europeans are estimated to be an admixture of 65% ancestral Chinese and 35% ancestral Africans."

¹ Three of the four papers which follow are directly derived from the symposium. Appreciation is due to the other participants (Gene Giles, Bernice Kaplan, Ken Jacobs, Amos Deinard, Vince Sarich, Milford Wolpoff and Rachel Caspari, Trudy Turner and Mark Weiss, and Loring Brace) for their oral presentations. The paper by Ms. Silverman is drawn from her award-winning senior thesis in the History of Science department at Harvard University.

The Bell Curve invokes an unprovable innate basis to justify a reactionary political stance—decreasing the quality of life for those with the lowest quality of life already—insensitive at best, evil at worst. But it is harder to account for the Human Genome Diversity Project's proclamation, which certainly exists without any such overtly malicious political goal. To assert that their genetic statement is false would be to imply that we know the ratio to be more like 80:20 than 65:35, when what is screwy here is a fundamentally archaic ideology—one which takes for granted that Asians and Africans are genetically primordial, homogeneous, and dipolar entities; and that Europeans are somehow bio-historically less pure than they, the result of an ancient racial martini mix. It would be understated to characterize such ideas merely as being wrong.

The problem here is only partly, though significantly, comparable to that of *The Bell Curve*. It lies principally with the construction and use of history. It represents a statement of classic pseudo-biological history, benign to the extent only that it implies no social agenda directly. History, however, is inevitably political; it confers identity. And to the extent that biological anthropology is the scientific study of human biological history, the "65:35 gene pool mix" reflects the substitution of a folk ideology for anthropological knowledge and is consequently very retrogressive. The indirect consequences, of course, revolve around the biological reification of folk-constructed categories.

This situation is not without its precedents. In 1962 Carleton Coon was a leading spokesman on anthropological issues and a widely admired intellectual with many popular and scholarly books to his credit. He had earned his doctorate at Harvard and taught there for some twenty years before moving to the University of Pennsylvania. *The Origin of Races* was titled somewhat pretentiously to invoke Darwin (its original title was *On the Origin of Races*), and it was self-consciously relevant to issues of integration and civil rights which were being debated at the time.

The book made four major claims. First, that the human species was divisible into five fundamental constituent subspecies or races. Second, that these taxonomic entities had a deep presence in prehistory, being discernible as equivalent subunits of *Homo erectus*. Third, that these evolved into *Homo sapiens* at different times. Fourth, that the economic and political dominance of contemporary western Europeans and their descendants (and secondarily, east Asians and their descendants), was simply a consequence of their longevity as members of the species. Thus, "it is a fair inference" that whites "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" (Coon 1962a: ix-x).

This, of course, implied a flip side, that the oppression of the dark-skinned peoples of the world was a consequence of their ephemerality as members of *Homo*

sapiens. In other words, it implied that there was essentially a biological and naturalistic explanation for what appeared to be the facts of social and political history.

The central fallacy of Coon's book is that the products of social history have social explanations, not biological ones. They are the results of cultural historical processes and not of microevolutionary natural processes. Franz Boas had established this persuasively by 1911. While often misrepresented as taking a simple "environmentalist" position, Boas certainly appreciated genetics and human diversity. He explicitly denied that cultural forms were the products of biology—arguing instead that they were the products of history and that they needed to be studied with biology as a constant—and he documented this position convincingly.

While we accept it now, at the time this argument was somewhat counterintuitive, like the idea that the earth goes around the sun, rather than vice versa, or that the form of a species can change over time. In fact, during the same year as Boas's first major exposition of these ideas in *The Mind of Primitive Man* (1911), the geneticist Charles Davenport published *Heredity in Relation to Eugenics* which purported to explain largely the same phenomena by recourse to the distribution of the recessive allele for feeble-mindedness. And a few years later his friend Madison Grant—both were founders and executives of the American Eugenics Society—would publish *The Passing of the Great Race* (1916), explaining history in terms of more diffuse constitutional factors.

The differentiation of biological history from social history, and thus the inadequacy of biology as an explanation for the variation in social forms, was as much a revolution in the social sciences as the Copernican, Newtonian, and Darwinian revolutions were in the physical and natural sciences. It reflected a new and fundamental change in the way we scientifically analyze a certain class of data, and it implied conclusions that were to some extent counterintuitive and opposed to folk views about the stability and immutability of differences in thought and action among groups of people. To that extent, Coon's book a half-century later was at best anachronistic and at worst reactionary when it sought to locate the causes of economic and social exploitation and oppression in the biology of the victims.

Coon certainly had the constitutionally-guaranteed right to articulate those ideas. On the other hand, his status as a scientist—as an *expert*—gave those ideas greater credence than they would have merited had they been attributed to a bus driver or bank teller. And they thrust physical anthropology into what we can call, not without some trepidation, its post-modern phase: when the science was revealed to be not just a simple-minded quest for knowledge but also an acknowledged means of validation for social ideologies, with attendant responsibilities on the part of its advocates.

To the extent that science can be invoked to validate ideologies, those particular scientific statements must be held up to higher standards of scrutiny and accountability than other classes of scientific statements. In the case of *The Origin of Races*, Coon was challenged to repudiate the citation of his work by segregationists. His response was that he had produced merely a value-neutral, apolitical work of science (Dobzhansky 1968; Coon 1968).

Coon could plead that he was simply observing the facts of nature and interpreting them as objectively as he could, but that was no longer adequate. He was, after all, of a generation in which American scientists had argued for immigration restriction and involuntary sterilization, on the basis of the quality of the germ plasm, as a scientific solution to the social problem of urban poverty (Mehler 1988). And if the Americans embraced that eugenic science only ambivalently, the Germans later certainly embraced it more wholeheartedly and more tragically.

By 1962, the year which, coincidentally, also saw the publication of Thomas Kuhn's *The Structure of Scientific Revolutions*, the scientific playing field of anthropology had shifted underneath Carleton Coon. Where scientists were now held responsible for the political implications of their pronouncements, it became just an obsolete non-sequitur to respond that political implications were not *intended* to be part of the theories.

Whether they were intended or not, they were present, and the issue became: What do you as author have to say about it? If you really do believe that your scientific research validates racism, it implies that racism is natural and justified and presumably acceptable to you as policy; so why hesitate to endorse it? And if you believe it *doesn't* validate racism, then the abuse of your work to oppress people should concern and offend you as a scholar, indeed as a citizen. And why on earth wouldn't you want to prevent that?

With the hindsight of history it is now clear that Coon's work was not only value-laden, but that he kept up a correspondence with the political activists who were citing his work. Carleton Putnam's 1961 anti-integrationist tract *Race and Reason*, originally titled *Warning to the North*, blamed the new philosophy of egalitarianism on a conspiracy of communists and Jewish anthropologists. According to Putnam:

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 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 through anthropologists but through 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.” [Putnam 1961: 50]

This statement from a “distinguished scientist” is a slightly rewritten quotation of a letter written to Putnam by Carleton Coon on 17 June 1960 (Carleton S. Coon Papers, National Anthropological Archives), and it is eerily prophetic of the furor which would surround the publication of *Sociobiology* (1975) fifteen years later.

Reviewing *The Origin of Races*, geneticist Theodosius Dobzhansky sent Coon a draft of his article, solicited by *The Saturday Review*, which observed that Coon “states some of his conclusions in a way that makes his work susceptible to misuse” and that the book contained some “semantic mischief” (Dobzhansky 1963). Coon jumped all over it: “You accused me of ‘mischievously’ altering my style so as to provide easy quotes for political people. That is libel” (Coon to Dobzhansky, 29 Oct 1962, Carleton S. Coon Papers, National Anthropological Archives).

Dobzhansky is quite taken aback:

If you “mischievously altered” your “style so as to provide easy quotes for political people,” I was unaware of that. No such allegation is contained in my review. Should I then offer you apologies for what I did not write? What I did write is that you got yourself “into semantic mischief,” and this makes your “book usable as grist for racist mills.” [Dobzhansky to Coon, 29 Oct 1962, Carleton S. Coon Papers, National Anthropological Archives]

The Saturday Review ultimately declined to publish the review, which instead came out in *Scientific American* and *Current Anthropology*. Privately, however, the heated correspondence between Coon and Dobzhansky continues over the next five months, until finally Coon writes to him:

On the advice of an eminent jurist whom I consulted on this matter I am writing to ask you to end your campaign of defamation against me, and am sending a copy of this letter to the director of your institute....

You accuse me, time and again, of “mischievously” slanting my style so that protagonists of racial segregation can quote me easily. That is a complete falsehood....

When you accuse me of irresponsible writing you forget your own irresponsibility in exposing a fellow scientist to what I have had to undergo as a result of your own actions.

Why have you done this?

When will you stop? [Coon to Dobzhansky, 25 Feb 1963, Carleton S. Coon Papers, National Anthropological Archives]

And Dobzhansky very formally responds:

Sir:

I never wrote, or said, that you are "slanting my style so that protagonists of racial segregation can quote me easily", or that you are "altering my style so as to provide easy quotes for political people". These phrases are yours, not mine. You can repeat them as many times as it pleases you, but their authorship is not attributable to myself. [Dobzhansky to Coon, 28 Feb 1963, Carleton S. Coon Papers, National Anthropological Archives]

Coon, however, was actually projecting the ugly truth onto Dobzhansky's criticism. Not only had he indeed been a source of ideas for Putnam's venomous treatise, but they had actually discussed the ways in which Putnam could cite Coon and yet conceal that it was Coon he was citing. Putnam wrote to him:

I must find some way of keeping the quote while disguising the source. There are various ways of doing this. Suppose I cut out the "prize-winning" and the "physical" and the "international reputation" and simply referred to the writer as a "Northern anthropologist," would you let that pass? Suppose I referred to him simply as a "distinguished scientist, younger than I am" (since one of the issues is out-of-date doctrines), saying nothing about anthropologist or North or South?

As to recognizing you by your style, I can re-write the quote in a completely different style. In fact I can camouflage the whole thing so thoroughly that I won't be really quoting you at all, but an imaginary correspondent never to be disclosed, my conscience being cleared by the recent knowledge that the substance of what is quoted is correct. Please let me know which of these various stages of remoteness would satisfy you. [Putnam to Coon, 1 Sept 1960, Carleton S. Coon Papers, National Anthropological Archives]

Hiding behind a public veneer of scientific objectivity, Coon was actually supporting one side of a social issue clandestinely and arming it with scientific data and arguments.

Sherwood Washburn, sitting President of the AAA, wrote 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." (Washburn to Coon, 8 Aug 1962, Carleton S. Coon Papers, National Anthropological Archives.) But when Putnam shortly thereafter published a letter citing Coon's work in support of segregation, Coon would not repudiate him.² And he received so much correspondence about it that he began privately responding with a form letter:

Dear.....

Thank you for your inquiry. In reply let me say that, in his published letter entitled *Evolution and Race: New Evidence*, Mr. Carleton Putnam accurately quoted portions of the revised edition of *The Story of Man* (Alfred A. Knopf & Co., New York, May 1962). He did not, however, include what I said on the subject of race on page 214. His interpretation of my book is his own. At the risk of appearing to promote sales, may I suggest that you read all 425 pages of the book yourself and form your own opinion of my intentions?
Sincerely yours
Carleton S. Coon

The Story of Man had been first published in 1954, and it merited no headlines. There was no suggestion of its six identified "races" being rankable by the sequence in which they became transmuted into *Homo sapiens*, nor of civilization being a consequence of it. In fact, Coon had written quite bluntly, "the Mongoloids are probably not as ancient as the Negroids" (1954:198), which would represent the very opposite of the thesis expressed in *The Origin of Races*.

The new version cited by Putnam, however, had been revised to bring it into line with *The Origin of Races*. The line cited above was deleted, and "Man's Six Geographical Ranges" had been condensed into "Man's Five Geographical Ranges" (although the map remained identical, actually showing six; Coon 1962b: 190-191).

Certainly the racists perceived Coon as an ally. With the death of geneticist R. Ruggles Gates, Coon received a letter (R. Gayre to Coon, 29 Oct 1962) inviting him to join the "honorary advisory board" of the notorious *Mankind Quarterly* (Skerlj 1960; Comas 1961; Lane 1994). Coon responded, "I will be very glad to get your monographs and also your magazine, to which I would be happy to subscribe, but I fear that for a professional anthropologist to accept membership on your board would

²Or *could* not, without risking the exposure of his correspondence with Putnam, which would have undermined Coon's pose of neutrality. This correspondence is not cited by Shipman (1994) in her recent defense of Coon.

be the kiss of death, here in the so-called land of the free and home of the brave” (Coon to R. Gayre, 6 Nov 1962).

We can accept, along with Peter Medawar (1984), the proposition that the object of science is to sift through the many things which might be true, and discern from among them the relatively few things which probably *are* true. But situating science realistically within a matrix of society and politics gives anthropological propositions something more to contend with. For given that there is politically benign and politically odious physical anthropology—in ways that there is *not* politically benign and politically odious organic chemistry—it must fall to authors themselves, and failing that to the community at large, to identify and to repudiate its obsolete or dangerous manifestations.

And so they did. Major critiques of Coon’s work were published by Ashley Montagu (1963), Joseph Birdsell (1963), Derek Roberts (1963), Sherwood Washburn (1963) and others. But if anthropologists had begun to come to terms with these issues, unfortunately biologists had not. Thus, one of the most interesting myths of the era was promoted by Stephen Jay Gould (1984:28): “Leading evolutionists throughout the world reacted to Coon’s thesis with incredulity.” In fact, however, two of the three “leading evolutionists throughout the world” wrote glowing reviews in prominent forums (Mayr 1963; Simpson 1964). Dobzhansky’s criticism is noteworthy precisely because he was *not* trained as an anthropologist and used his stature as a general biologist to attack the work.

Thus it is of significance that the same sorts of arguments were brought into play again with the publication of *Sociobiology* (1975). The scientist would again complain that others were politicizing the arguments over an ostensibly scientific program that was in fact loaded with politics. And it is being replayed today with the promotion of the Human Genome Diversity Project (Gutin 1994).

But how could a community that has been through what science has justified in this century reasonably be asked to evaluate political pronouncements in the scientific literature as if they were *not* political?

It cannot, and indeed it should not. Political statements that pretend to be non-political are falsehoods, and they do no credit to any scientific pretensions. It is at least to the credit of the authors of *The Bell Curve* that they acknowledged its fundamentally political nature.

The matter at hand is the structure of biological variation in the human species and its implications. The issues from all angles—scientific or epistemological, historical, and political—are very much with us today, and they have major consequences for the present and future of anthropology.

Human variation is geographically patterned, but not necessarily taxonomically organized, as scholars of the 18th century recognized. In general, people are similar to those nearby and different from those far away. Whether we refer to races as low-tech impressionistic color-coded subspecies as did Linnaeus, or as hi-tech computerized color-coded “ethnic regions” (Cavalli-Sforza et al. 1994), the act of imposing qualitative differences on them does not help us understand the biological patterns structuring human variation. What the act represents is the Linnaean, essentialist, pre-evolutionary approach to human diversity.

There is no scientific basis—genetic, phenotypic, or eco-geographic—for asserting that a Persian and a Belgian are qualitatively the same, and a Ghanaian and an Ethiopian are qualitatively the same, but a Persian and an Ethiopian are qualitatively different. That is what the natural concept of race would say, and it is false. Further, as history shows, this misconception effectively clouds the biological picture with a good deal of ideology and folk knowledge. That is the lesson we draw from *The Origin of Races*, and to the extent that it has not yet been fully appreciated by our cognate fields, it is our responsibility to get them to appreciate it.

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