



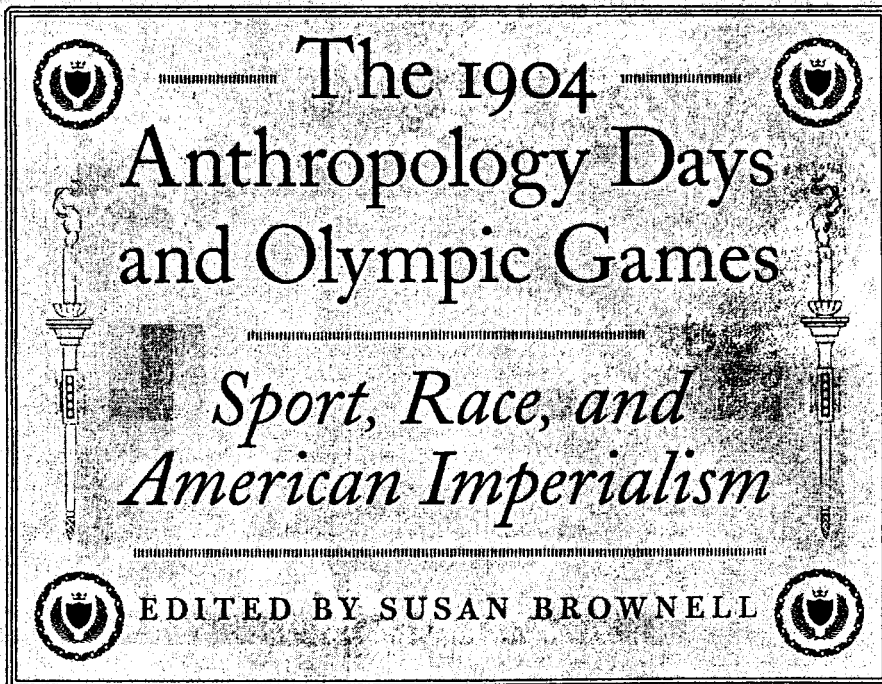
Chapter II. The Growth of Scientific Standards from Anthropology Days to Present Days

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Anthropology is by its very nature a reactive science. It arose as an academic specialty largely in opposition to late-nineteenth-century racism and Social Darwinism—whence E. B. Tylor’s assertion, from the last page of *Primitive Culture* (1871), that anthropology is “a reformer’s science.” A generation later in America, Franz Boas established academic anthropology largely in opposition to hereditarian thought, publishing *The Mind of Primitive Man* the same year as Charles Davenport’s *Heredity in Relation to Eugenics* (1911). Davenport’s book was the first major post-Mendelian text of human genetics in America, and proceeded to explain class, civilization, and individual intelligence in terms of the global distribution of genetic factors, with particular reference to a major gene for “feeble-mindedness.” Boas would wage a decades-long intellectual war to establish anthropology in the face of such powerful scientific opposition.

Anthropology in the early twentieth century existed in a small handful of universities; to the extent that it was acknowledged as a field of scholarship, it was located in museums. The most intellectually progressive museums were in Germany, but the museums with the most ready access to the materials of “savage man” were in America, where the indigenous peoples had been “pacified” for a generation and could now be examined as the objects of dispassionate scientific study.

Franz Boas entered the American anthropological scene in the 1880s, one



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of a few practitioners equipped with a doctorate—since the advanced degree was still quite rare in America. His experience with the German conception of museum anthropology clashed with the established practice of American anthropology, and Boas leveled a sharp criticism of that practice in the pages of the journal *Science* in 1887. Boas had found the Smithsonian's collections nearly useless for his interest in peoples of the Northwest Coast, not because the museum lacked materials, but because the materials were organized according to their degree of advancement in relation to similar objects, and not by what we would now call "cultures." The Smithsonian's senior anthropologist defended the scheme on the grounds that cultural evolution proceeds everywhere similarly, since "like causes produce like effects," but Boas argued that this approach was unhistorical, since commonly "unlike causes produce like effects."¹

Boas was employed at the time as the geography editor for *Science*. Later thwarted by the Smithsonian's securing an anthropology position for one of their own in Chicago in 1894, Boas would only find permanent academic employment at Columbia in 1896. He was appointed lecturer in physical anthropology (his research expertise lay in "collecting" Eskimo bones and measuring schoolchildren).

In 1902, the Smithsonian bypassed WJ McGee as the successor to John Wesley Powell as the head of the Bureau of American Ethnology, a position for which McGee had been groomed for nearly a decade. Boas published a strong letter in *Science* in protest, to no avail. McGee, however, became the first president of the American Anthropological Association that year, and shortly thereafter directed the anthropology exhibit at the Louisiana Purchase Exhibition. The paper Boas composed for the occasion was published in 1904 as "The History of Anthropology," and somewhat notoriously situated anthropology as an outgrowth of German philosophy, entirely ignoring the intellectual contributions of the American practitioners. And although those same American practitioners had instituted the simultaneous study of physical form, material remains, activities, and languages as the basic

constituents of anthropological research, Boas's 1904 paper is one of the earliest documents to articulate in a formal way the constitution of anthropology as "the biological history of mankind in all its varieties; linguistics applied to people without written languages; the ethnology of people without historic records, and prehistoric archaeology."² At the time Boas wrote, the American Anthropological Association was but two years old, and the dominant figures of its first generation were all recently deceased (Lewis Henry Morgan, John Wesley Powell, and Daniel Garrison Brinton). It was an opportune time to write a mythic history for the discipline, which already had little theoretical coherence.

Boas was an innovator in using local history and intellectual integration as an anthropological framework. This was tied to his innovation of using "culture" as a plural noun, and to his suggestion "that civilization is not something absolute, but that it is relative, and that our ideas and conceptions are true only so far as our civilization goes."³ Nevertheless, the discipline was still not very far removed from crude racism. Scarcely a few decades earlier, American anthropology was principally represented as the craniological studies of Samuel J. Morton, and of Josiah Nott and George C. Gliddon, intended to justify slavery through the demonstration of distinct cerebral physiologies and separate origins of the races. The first generation of American physical anthropology essentially became obsolete after the Civil War.

One can easily perceive the tensions in nascent anthropology shortly before the turn of the twentieth century. In the pages of *Science*, Brinton had lamented a pendulum swing among European anthropologists "[to] deny the existence of any such things as racial or ethnic traits, tendencies, or capacities." Brinton categorically denied that other peoples possessed the same level of intelligence as Europeans. "The mental traits of races and peoples," he wrote, "are as much their peculiar characteristics as are their bodily idiosyncrasies, and are just as impossible to change by any quick process."⁴

A year later, D. K. Shute, a Washington-based physician, read a paper before the Anthropological Society of Washington, subsequently published

in the *American Anthropologist*, that presented a roster of ostensibly apelike or “simioid” features possessed differentially by human races. “Measured by these criteria,” he wrote, “the Caucasian stands at the head of the racial scale and the Negro at the bottom.” This analysis and conclusion provoked some heated discussion, principally from the Washington-based anatomist Frank Baker, the sitting editor of the *American Anthropologist*. Baker observed that the physical differences reported among the races were overblown and “misunderstood.” And to the extent that there are racial characteristics by which whites differ from blacks, “as a matter of fact there does not seem to be adequate ground for the conclusion that his racial peculiarities are remarkably simian.” Baker drew on first-hand dissecting-room experience of blacks to support his contention that “ape-like characters are no more common among them than among whites.” The next commentator, however, agreed more with Shute and asserted that “the negro is an example of retarded or arrested development.”⁵

WJ McGee, Boas’s new ally in 1904, held ideas about human diversity that were somewhat unsophisticated, but were still a far cry from those of the racist physicians who still comprised much of the anthropology community. He found popular racial classification to be typological and unrealistic, pointing to “the objection that certain peoples hardly fit any one of the five classes.” At the same time, however, he argued that the separate origins of different peoples was still an open question and envisioned biocultural evolution as a set of intertwined phenomena, which yielded a four-stage developmental series no matter what was being analyzed, with its European expression at the summit.⁶ Thus, “[w]hen the world’s peoples are classified by culture-grade, or in terms of progress from the lowest to the highest stages, it at once becomes manifest that they are arranged . . . in accordance with general physical development, including strength, endurance, and viability.” And finally, he believed that race mixture was a good thing (a contentious point at the time), with “the world’s strongest blood being the world’s most-mixed blood,” which would seem to place melting-pot America above all other nations.⁷

He saw the Louisiana Purchase Exhibition as an opportunity to bring “the field” home, and to study indigenous peoples without the inconvenience of having to travel to them.⁸ Torn from any relevant context or environmental setting, these people could nevertheless hopefully be the objects of study in somatology (bodily form), psychology, arts and industries, languages, law and society, faiths and philosophies, general ethnology, general anthropology (“the comparative study of primitive and advanced peoples in an unexampled assemblage of race-types and culture-grades”), and as subjects for scientific, photographic, or artistic record.

Odd as that may sound today, it was only a few years earlier that Boas himself had asked Robert Peary to bring back some Eskimos from Greenland, to be studied in New York. The outcome was tragic, as were the personal histories of Ota Benga and (under quite different circumstances) Ishi.⁹ Without cultural context, however, the people and their possessions turned out to be largely valueless anthropologically, and the psychosocial and biomedical consequences of transplanting people were generally overlooked or naively rationalized at the time.

The people brought to the Louisiana Purchase Exhibition would nevertheless be the objects of scientific study, as closely as one could approximate the dispassion of a chemist studying the properties of boron. Natural science would afford the most appropriate role model for the study of “man,” and the differences in kind between their subject matters would be minimized as far as possible.¹⁰

Today, such an idea seems ludicrous for its internal contradiction: To pretend humans are not cultural in order to study them “scientifically” is to begin by denying the most salient natural fact about the human species. Moreover, it is now commonplace in genetics to acknowledge that since a phenotype is the product of a genotype expressed in a specific context, there can be no phenotype independent of the environment that produced it. In the case of humans, obviously, that context is biocultural—with complex environmental, motivational, and experiential components contributing to the development

of human bodies and behaviors. Consequently, it is meaningless to try to regard human bodies and behaviors—their states, performances, and products—independently of the circumstances in which they arise, perform, and produce. Those were clearly premodern times in anthropology.

The Return of Racial Pseudoscience

Flash forward a hundred years. We pass the elaboration of cultural relativism by Boas's students after World War I; the vain struggle of Earnest Hooton to differentiate good American racial studies from bad German racial studies between the wars; Sherwood Washburn's reinvention of physical anthropology in the 1950s.¹¹ We pause by the British physical anthropologist J. S. Weiner's characterization of the biological structure of the human species "as constituting a widespread network of more- or-less interrelated, ecologically adapted and functional entities"—implying that the constituent units of the species are local populations, not para-continental clusters.¹² We note the elucidation of the cultural aspects of race—from studies of immigrants (pioneered by Boas), cultural history, the submerging of former racial identities (e.g., Irish, Jewish), the elaboration of new ones (Latino, Middle Eastern), and the invention of the racial category "multiracial."

And yet, along with the erosion of the idea that race constitutes a natural, biological human category, a backlash develops. In the early 1960s, the Columbia University psychologist Henry Garrett, anatomist Wesley Critz George of the University of North Carolina, and businessman-author Carleton Putnam published articles and books arguing that science supported segregation.¹³ Moreover, they maintained that the emerging consensus about race was simply an antiscientific political doctrine imposed by a conspiracy of Jewish communist anthropologists, led by Franz Boas.¹⁴ Similar sentiments were even held by the prominent anthropologist Carleton Coon, who was circumspect enough in public, but would share the Jewish-communist-anthropologist theory in correspondence. Coon, as sitting president of the American Association of Physical Anthropologists in 1962, was clandestinely corresponding with, and aiding, the segregationists.¹⁵

Indeed, in a 1960 letter to his cousin Carleton Putnam, Coon assured him prophetically, "The tide is turning. Heredity is coming back into fashion, but not through anthropologists. 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 later quoted that letter anonymously in his notorious *Race and Reason*.)¹⁶ And fifteen years after Coon wrote those words, *Sociobiology* proved him right.

But actually heredity and sociobiology are non sequiturs, if the question at hand is race. How do they converge? The hereditarian believes that genetic variation underlies behavioral or mental variation. The racist believes that humans are divisible into a number of natural groups, each with different endowments. The interests of the hereditarian and the racist coincide if the behavioral or mental variation under consideration is specifically that which differentiates groups from one another.

This may seem like an obscure point, but it gets to the heart of many contemporary misunderstandings involving racism, genetics, and evolutionary psychology (i.e., sociobiology, version beta). The question, Does genetics influence behavior? has a trivial answer. So does the question, Does culture influence behavior? And so, too, does the question, Does personal experience influence behavior? To address the relationship between genetics and human behavior at all rigorously requires that we examine their fundamental patterns of variation. That variation is not random; it has structure.

Genetic variation—approximately 85–95 percent of it—is principally found within groups, that is, as polymorphism.¹⁷ Paradigmatic is the ABO blood group system, in which virtually all human populations have some A, some B, and some O—and only differ from one another in the relative proportions of each allele. Only a small proportion of genetic variation exists as alleles that one group has but another does not.

Behavioral variation in the human species—defying comparable quantification—is principally found between groups, that is, as culture. Regardless of the analytic problems with the concept of culture, we traditionally take

it to refer to group-level differences in behavior. That certainly is what the Boasian concept of culture was intended to convey—locally normative ways of experiencing the world, existing in it, and making sense of it. And yet, obviously, the differences between cultures have a basis entirely in social history. If cultural difference constitutes the principal structure of human behavioral diversity between groups, and if those differences are not genetic, then it follows that the great bulk of between-group human behavioral variation must have a nongenetic etiology.

Can genetic differences in mind and act be found? Of course, but they cannot constitute any significant part of “the big picture.” These genetic differences will be polymorphisms—like the great bulk of genetic variations are—and thus will be identifiable from person to person within a population. In addition to factors like life experiences and familial traditions, there may well be genetic reasons for why one person tends to think or do one thing and another person tends to think or do another—as long as we understand that we are talking about mental and behavioral differences *within a single population*. In other words, human behavioral variation and genetic variation are structured so differently that the latter cannot reasonably be considered a significant cause of the former. The great bulk of human behavioral/mental variation occurs *between* groups and is the product of historical forces; the great bulk of genetic variation occurs *within* groups, and may indeed comprise part of the causal nexus of someone’s life trajectory.

It is consequently anthropologically trivial to discover a genetic variant that influences thought, mood, or deed. Anthropology is concerned principally with the different things that different *groups* of people do; psychology, perhaps, is the science concerned with why people in the same group do different things. Consequently, the discovery of genetic variations in neurotransmitters and receptors is trivial in the scope of human behavior. Imagine a Yanomamo and a Harvard professor, who share an allele that makes them a bit happier (or unhappier, or smarter, or more violent) than their peers. There is no reason to think that their lives, experiences, or perceptions would converge

significantly as a result of having the allele. One would still be living the life of an Amazonian horticulturalist and the other would be leading the life of an urban American intellectual. Each would be a little happier (or unhappier, smarter, or more violent), but the genetic similarity they share would be completely submerged by the cultural difference they do not share.

The reason for this digression is that without the analytic partitioning of behavioral diversity into between-group (i.e., cultural) and within-group (i.e., psychological) domains, the hereditary factors that affect the latter may be improperly posited to explain the former as well. This distinction is crucial in genetics. Consider two identical plots of soil. A handful of seed (from the same source, so there is no difference between the handfuls) is scattered in each. One plot receives plentiful sunlight and water; the other receives only a little. In a few weeks, each plot has plants of varying heights. In one, the plants vary a bit, but are generally tall and robust; in the other, the plants also vary, but are generally short and stunted. Genetic differences are certainly at work dictating the range of variation of plants in either plot, *but the big difference is between the two plots and is entirely nongenetic.*¹⁸

Looking at the variation within either set of plants, one can calculate a statistic called heritability, which will estimate the extent to which the variation in plant height is related to variation in genes. But that statistic is inapplicable to understanding the difference *between* the two plots, which is considerably larger and has no genetic basis. Yet that is precisely what hereditarians, from Arthur Jensen through Philippe Rushton, have purported to do: use a study of within-group variation to explain differences between groups.¹⁹

The Human Genome Project seemed to bring a new legitimacy to hereditarian explanations for human behavior, which the human genetics community was itself slow and somewhat reluctant to criticize.²⁰ In the 1990s, the “genotype” that helped rouse public support for the Human Genome Project subtly became conflated with the scholarly analysis of heredity itself. As possible niche markets for pharmaceutical companies, races are being actively reinscribed by a strange new breed of epidemiologists and population

geneticists.²¹ Evolutionary psychology appropriates the authority of Darwin to analyze imaginary cognitive modules as if they were biological imperatives.²² Thus, old-fashioned racism—the idea that human groups are natural divisions, and are possessed of unequal talents—is back as well.

All four of these research areas—behavior genetics, population genetics, evolutionary psychology, and scientific racism—tend to cite the same core works. These include Derek Freeman’s claim to have refuted Margaret Mead’s conclusions about Samoan adolescence—and by extension, the importance of culture in human behavior; Napoleon Chagnon’s claim to have identified a reproductive bias for Yanomamo warriors, and by extension, for prehistoric warriors; Daly and Wilson’s claim that stepparents are more prone to infanticide than genetic parents, thereby rooting human kinship in natural relations, rather than in cultural forms; and David Buss’s claim to have identified species-wide propensities in human mate choice.²³ That none of these claims has held up well under scrutiny, and the fact that none is taken seriously in mainstream anthropology does not seem to matter. Like the footprints of human and dinosaur by the Paluxy River in Texas that creationists still cite to confute anthropology, these works are brandished at face value for the sole purpose of—well, confuting anthropology.²⁴

Certainly the most bizarre work in this arena is that of Canadian psychologist J. Philippe Rushton, who argues that Africans have evolved high reproductive rates and low intelligence, Asians have evolved low reproductive rates and high intelligence, and Europeans have struck a happy balance of both; and uses brain size, crime rate, sex drive, and penis length, as surrogate measures of intelligence and reproductive rate. Rushton’s work was cited by Richard Herrnstein and Charles Murray in *The Bell Curve* (1994), along with a preemptive appendix defending its seriousness. Rushton’s work is also cited favorably by Sarich and Miele (2004).

Jon Entine’s work complements the lot, and is cited with admiration by Rushton (2000) and Sarich and Miele (2004). Entine’s book, *Taboo: Why Black Athletes Dominate Sports and Why We’re Afraid to Talk About It*, argued for innate

racial differences in athletic ability, and even revived a variant of the old Jewish-Communist-anthropologist conspiracy as his explanation for “why we’re afraid to talk about it.” In fact, many of us working in human variation talk about it all the time. What we say is something like: (1) human achievements arise in cultural environments and cannot be separated from them; (2) quality of achievements is not a reliable indicator of the quality of innate abilities; (3) professional over-representation is not a reliable argument for the existence of special endowments; (4) the qualities of the population cannot be inferred from the qualities of its most extreme members; and (5) sources of within-group variation are not reliable explanations for differences between groups.

This reflects a scholarly, indeed a scientific, consensus: that there is no rigorous scientific basis on which to infer the presence of group level endowments. To argue in a modern intellectual context that group-level endowments exist requires controlled data (anecdotes and life-histories do not suffice); a means of separating from the analysis the traditions, expectations, and stereotypes that track people into certain venues; and the statistical recognition that one cannot generalize about large populations from their most outstanding members.

As I explained to Jon Entine when he sent me the manuscript, the issue at hand is scientific evidentiary standards. Either you meet them, in which case a dialog can proceed, or you don’t—in which case, why are you even bothering to pretend to raise a scientific issue? The fact is that the pseudoscientific arguments for black athletic superiority directly parallel the pseudoscientific arguments for black intellectual inferiority. Entine expressed indignation at my lumping him with the authors of *The Bell Curve*, went on to disparage my skepticism in the book itself, and then actually wound up as an adjunct fellow at the American Enterprise Institute, Charles Murray’s professional base. Small world!

Science and Humanity

The argument that racial endowments are at the root of racial achievements is beset by an epistemological problem. Humans are biocultural animals; everything we do, or think, or say, or achieve is brought into existence and

rendered meaningful in a cultural context. There is no human thought or behavior external to culture. Culture indeed precedes our species, which means that every human being that has ever been born has been born into a cultural environment. One cannot analytically remove humans from culture any more than one can analytically remove the eggs from a cake. One can talk no more sensibly about humans without culture than one can talk sensibly about pigeons without feathers.

Why are some “races” over-represented in some sports? The prominence of blacks in modern track and field is paralleled by the prominence of Latin Americans in baseball. Moreover, the prominent Latin Americans in baseball run a wide “racial” gamut, since the very category “Latin American” itself cross-cuts “race.” Do blacks in track and field and Latin Americans in baseball require two different explanations, or will the same one suffice?

When a demagogue like Entine generalizes about “the body of the black athlete,” we are minimally obliged to wonder what black athletic body he has in mind. Basketball guard Kobe Bryant’s wiry body? Baseball slugger David Ortiz’s enormous body? Speedy Carl Lewis? Lanky Jerry Rice? Stocky Joe Frazier? Unless we can identify a common biological thread linking them (aside from pigmentation), we are obliged to consider the possibility that the physically diverse group of prominent black athletes are united more by the social filters that tracked them into professional sports than by the possession of a common biological gift.

To the extent that native differences in “abilities” exist, they must be patterned like most genetic variation: that is to say, principally within group. Some people will indeed have the eyesight, coordination, and reflexes to be able to hit a fast-moving ball with a piece of wood more reliably than others can. But the unlikely constellation of genes that makes such a feat possible will not be greatly over-represented in one population relative to another. Moreover, like all other human endowments, the genes involved will be expressed in a highly specific context.

To argue, then, that a specific athlete is “naturally” endowed is trivial. To

argue that a group is “naturally” endowed, with any degree of rigor, simply requires a lot of well-controlled data. Without those data, the argument is sophistry, not science—and sophistry with an incredibly bad track record. And if those data are impossible to collect, that means that the question itself was not framed scientifically in the first place. Science, after all, is not so much about asking questions, as it is about asking questions that *can be answered*. Posing a question that cannot be answered rigorously, and then pretending that it can be, is pseudoscience.

Except in rare cases, unfortunately, pseudoscience is only identifiable as such in retrospect. But every recent scholarly generation has been saddled with combating the idea that somehow social inequalities or hierarchies are merely expressions of natural hierarchies, or racial endowments. This stretches from the Social Darwinists of the 1890s through the eugenicists of the 1920s, Nazis of the 1930s through ’40s, segregationists of the 1950s and ’60s, and their inheritors today.

There is no conspiracy of silence on the study of human diversity—although that suggestion, originally made by the segregationists, can still be identified in the writings of some modern scientists, who should know better.²⁵ The intellectual progress we have made in the study of human diversity over the last century has involved the development of standards for pronouncing scientifically on the nature and existence of human groups. They were needed because of the muddled thought, ignoble goals, and conflicted interests that have pervaded the scholarly and popular literature. Intellectual standards, however, are ultimately what permit a science to mature.

Notes

1. Boas, “The occurrence of similar inventions”; Mason, “The occurrence of similar invention.”
2. Boas, “History of Anthropology.”
3. Boas, “Museums of Ethnology.”
4. Brinton, “Current Notes on Anthropology.”
5. Shute, “Racial anatomical peculiarities.”

6. WJ McGee, "Current Questions in Anthropology."
7. Provine, "Geneticists and the Biology of Race Crossing"; WJ McGee, "Anthropology at the Louisiana Purchase Exposition."
8. WJ McGee, "Opportunities in Anthropology."
9. Bradford and Blume, *Ota Benga*; Starn, *Ishi's Brain*; Harper, *Give Me My Father's Body*; Kroeber and Kroeber, *Ishi in Three Centuries*.
10. Mason, "Scope and Value of Anthropological Studies"; Topinard, *Anthropology*.
11. Hooton, "Plain Statements about Race"; Haraway, "Remodeling the Human Way of Life"; Marks, *Human Biodiversity*.
12. Weiner, "Physical Anthropology."
13. J. P. Jackson, *Science for Segregation*.
14. Garrett, "Equalitarian dogma"; George, *Biology of the Race Problem*; Putnam, *Race and Reality*.
15. Marks, "Human Biodiversity As a Central Theme"; J. J. Jackson "In ways unacademical."
16. Letter of C. S. Coon to C. Putnam; Putnam, *Race and Reason*.
17. Lewontin, "Apportionment of Human Diversity"; Barbujani et al., "Apportionment of Human DNA Diversity"; Rosenberg et al., "Genetic Structure of Human Populations."
18. Lewontin, "Race and Intelligence."
19. Jensen, "How Much Can We Boost IQ?"; Rushton, *Race, Evolution, and Behavior*; Rushton and Jensen, "Thirty Years of Research on Race Differences."
20. Herrnstein and Murray, *Bell Curve*; Andrews and Nelkin, "The Bell Curve: A Statement."
21. Kahn, "How a Drug Becomes 'Ethnic'"; A. F. Leroi, "A Family Tree in Every Gene," *New York Times*, March 14, 2005; Koenig et al., *Revisiting Race*.
22. Pinker, *Blank Slate*; Rose and Rose, *Alas, Poor Darwin*.
23. Freeman, *Margaret Mead and Samoa*; Chagnon, "Life Histories"; Daly and Wilson, *Homicide*; Buss, *Evolution of Desire*.
24. Wrangham and Peterson, *Demonic Males*; Hamer and Copeland, *Living with Our Genes*; Pinker, *Blank Slate*; Sarich and Miele, *Race*.
25. O. Judson, "The Subject is Taboo," *New York Times*, June 28, 2006, <http://judson.blogs.nytimes.com/>.