The long shadow

Davenport's Dream: 21st Century Reflections on Heredity and Eugenics

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The earliest human geneticists were wrong about the most basic meanings of their work—so bombastically and shamefully wrong—that we all now have to work exceedingly hard to emerge from their shadow nearly a century later. At the head of the list was Charles B. Davenport, the most respected human geneticist in America for the first few decades of the twentieth century.

Davenport's Dream is two books in one: first, a collection of essays about Charles Davenport and his work and second, a reprint of Davenport's influential 1911 textbook, *Heredity in Relation to Eugenics*. Why reprint the book? Because it was the first full-length exposition of human genetics published in America, and it authoritatively explained to readers scientific facts like, "Germans are, as a rule, thrifty, intelligent, and honest. They have a love of art and music, including that of song birds, and they have formed one of the most desirable classes of our immigrants" (p 214).

It is hard not to get defensive when confronted with such a history. The most obvious defensive posture is "that was then; this is now," the one adopted in this volume most notably by Maynard Olson. Olson is critical of the over-hyping of genetics in business and the media but wants to distinguish Davenport's science from Davenport's ideology, which he can now detect with the aid of a century of hindsight. "In contrast," he adds, "most of what I and my colleagues think about these issues remains unspoken, at least in any public forum" (p 89).

The reason for that should be obvious: when geneticists do articulate them, the results are all too commonly still embarrassing. Olson thus misses the crucial issue: you cannot reasonably bracket any aspect of human activity apart from cultural ideologies. Anything done by conscious humans is *ipso facto* cultural. In order for science to be a noncultural activity, it would have to be carried out by animals or robots; it would have to be effectively a nonhuman activity. The problem, consequently, is far more subtle and interesting than Olson's mundane observation that Davenport "commingled science and social values." So does he, so do I, and so do you. The problem is, how do we broaden the intellectual scope of what constitutes an education in human genetics, so that practitioners can be more attuned to those social values and more reflective and knowledgeable about them?

Such breadth indeed might have spared Olson some embarrassing paragraphs on the subject of race. For the record, race is an intersection of (objective, measurable) difference and (subjective, impressionistic) otherness. Race is thus not about difference but about meaningful difference; and as such, it is not discovered but constructed.

Where Lewis Wolpert invokes Mary Shelley's *Frankenstein* only to disparage moralizers, Philip Reilly's essay concludes with an oddly relevant suggestion: that perhaps geneticists really ought to be responsible for deciding who shall live and who shall die. Combining the two, a question from the eugenics era reemerges for me—why do geneticists so badly want to run other people's lives, and what on earth makes them think they have the wisdom to do it? Indeed, the book's first contributor (a more recent director of the laboratory founded by Davenport) is on record elsewhere, to wit: "People say it would be terrible if we made all girls pretty. I think it would be great." Thank goodness for nondirective genetic counseling!

Several of the contributors attempt to identify something that Davenport was prescient or correct about, although the value of such an enterprise is obscure. Right or wrong, he was believed, he was followed, he was read, he was written up, he was funded, he was feared and he was admired for decades. Those are the facts, and they require explanation. Whether, say, redheads really do undergo negative assortative mating, as Davenport thought (p 33), is simply trivial both biologically and intellectually.

Elof Carlson, who has grappled with the history of genetics more bravely than most practitioners, contributes a most thoughtful essay on the scope of Davenport's work. Sadly, though, nobody cares to wrestle with the big, scary questions about science that emerge from Davenport's career. Once you become aware that David Heron, a protégé of Karl Pearson at the Galton Laboratory in London, called Davenport a more or less flat-out quack in the pages of his monograph, the journal *Science* and *The New York Times* in 1913–1914, you have to ask, "Why did it take so long for Davenport's scientific empire to fall?" The Eugenics Record Office wasn't shut down until the end of the 1930s, and when Davenport died in 1944, he was the sitting President of the American Association of Physical Anthropologists. How does one amass and retain scientific clout that is so extraordinarily impervious to criticism?

Sad to say, David Heron makes no appearance here. Neither does Davenport's friend Madison Grant, author of the 1916 proto-Nazi best-seller *The Passing of the Great Race*. In 1926, Davenport touted Grant to be the second President of the American Eugenics Society and had to be talked out of it by the first President, the Yale economist Irving Fisher. Once the connection between Davenport and Grant is established, the issue of scientific racism, not merely genetic overenthusiasm, must be confronted. Without Madison Grant, though, Davenport's nose looks that much cleaner.

We can now readily see that Davenport's major failing was to read the prejudices of his culture, era, class, and self-interests into his science and then to read them back out. But that error is timeless and is effectively impossible to avoid. If you need me to specify any recent examples, you should be doubly embarrassed. And that is why we need historians.

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