Eugenics

Looking at the Role of Science Anew

Among the most troubling phenomena of the last century, one finds the political, moral and scientific issue of eugenics. As is well-known, eugenics became a movement mostly within the framework of public health throughout the Western industrial countries, especially Britain, the United States, the Scandinavian Countries and Germany. The underlying notion was that the endangered or already degenerated heriditary stock could only be improved by the control of individual reproductive behavior and/or the reform of social institutions held to be counterselective (cf. Kevles 1985). Thus eugenists focused on human reproduction and its institutions, notably on the choice of mates and marriage (cf. Schallmeyer 1918). The selection-oriented social analysis was translated into a comprehensive scheme of social reform. In Germany, this was carried to extreme measures, and ultimately became associated with the atrocities of the Third Reich (cf. Weingart et al. 1988).

For obvious reasons, eugenics has become the center of a long-standing debate engaging scientists and the public alike. In particular, historians and sociologists of science inquire, among other things, into the rise and fall of an overwhelming ideology, the change of values connected to it, as well as into the role of scientists, professions, and politicians involved. Thus part of the history of eugenics is the history of scholarly attempts at understanding it. The question is: What is the role of science if it comes produce, obscure, and/or enlighten eugenics as a powerful tool of reasoning and intervention that regulates the behavior on the level of individuals and populations? Thus far, historians of science tended to tell stories that 'make things straight': In these stories, science played a significant role in both the waxing and waning of eugenics.

Only recently, scholars began to doubt that story, among them Diane Paul and Peter Weingart. Both authors stress the idea that by importing ideas or instruments from other disciplines dealing with related phenomena one makes use of a rich heuristic base from which on may look at certain stories (in science studies) anew. Diane Paul, for instance, suggests to employ specific statistical tools that reckon with methodological concerns similar to the historians', such as the issue of sampling data or the independence of evidence. In fact, she calls those statistical terms a "set of productive metaphors." Treating concepts as metaphors is precisely the approach Weingart uses when he, for instance, looks into the fate of Darwinian terms, or metaphors, such as "struggle for existence." From this perspective, one can observe the specific ways in which various disourses make use of a term thereby expanding and changing both its meanings and pragmatics over time (Weingart 1995). Statistics or metaphors are but two tools that help to regain the distance necessary to avoid (probably all too pleasing) short-cuts in science studies, notably in history of science. Diane Paul's historical account of the role of science in eugenics, guided by statistical concepts, is a powerful case in point.

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A Statistical Viewpoint on the Testing of Historical Hypotheses: The Case of Eugenics

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Introduction

This essay represents an exercise in what might be called a non-speculative, non-abstract philosophy of history. Most historians seek to establish causal connections. Since statisticians' stock in trade is distinguishing cause from correlation, I suggest that we might glean fresh historiographic insights from their work. I do not mean by this that historians should become more quantitatively-oriented, much less that they ought to make use of specific statistical tools. Indeed, no parametric (and for most historians, few if any non-parametric) statistics are applicable to their domain. But statisticians do reckon with methodological issues – involving sampling, replication, independence of evidence, and so forth – that are deeply germane to the historians' task. I argue that a sensitivity to statisticians' concerns and methods might suggest a new vocabulary, new ways of framing issues, and a new set of productive metaphors. The point is not to (further) scientize history, but to propose a potentially useful heuristic for thinking about problems of ascribing causation to events in the human past.

To illustrate its value, I reanalyze a long-standing debate in the history of eugenics. Both Peter Weingart's work and my own involves a critique of the view that scientific advances played a central role in eugenics' decline. I begin with a synopsis of the received view, followed by an account of the arguments that have convinced most historians that this view is unsatisfactory. Taking a 'reflexive turn,' I then reevaluate aspects of the critique in light of methodological considerations prompted by a recent encounter with the field of biostatistics.

The Received View and its Critics

On the once-conventional view, support for programs of selective breeding flourished at a time when the science of genetics was in its infancy and eroded as the science became more sophisticated, exposing eugenics' technical flaws.[1] Thus in the first flush of enthusiasm for Mendelism, geneticists attributed many mental, moral, and temperamental traits to heredity, and often to single Mendelian factors. If individuals with bad heredity could be prevented from breeding, it seemed that the problems of 'feeblemindedness,' pauperism, sexual promiscuity, and crime would quickly be controlled. According to Charles Davenport, then America's leading geneticist, after only a single generation of segregating mental defectives and anti-social individuals from society during their reproductive years, "the crop of defectives will be reduced to practically nothing," as would the host of social problems they create (1912: 286).

However, by the 1920s it had become evident that most traits, and certainly behavioral ones, were influenced not just by one but several factors, now called 'genes.' Moreover, these genes acted in complex ways with each other and the environment, so there was no unilinear relationship between a gene and a character. Most important, discovery of the Hardy-Weinberg principle (which makes it possible to calculate the frequency of carriers when the frequency of the gene is known) destroyed the hope that selection against undesirable traits could eliminate or even greatly reduce them. It is an implication of that principle that, when a trait is recessive and rare, most of the deleterious genes will be hidden in apparently normal carriers. Since the affected individuals represent only the tip of an iceberg, their sterilization or institutional segregation would do little good. To appreciably reduce the incidence of a trait, it would be necessary to prevent the numerous heterozygotes from breeding. But even if this were politically possible, there was no way to identify them. Thus the scientific facts made increasingly clear that such a policy was unrealistic, and support for eugenics – at least in its more interventionist forms – therefore faded. Although many geneticists continued to voice eugenic ideals, they no longer endorsed practical measures to control human breeding. Geneticists had been leaders of the early eugenics movement, which was crippled by their withdrawal of support. Or so goes the customary interpretation of eugenics' rise and fall.[2]

It continues to inform much popular writing on genetics and eugenics. Textbook authors seem to find it especially appealing, perhaps because it short-circuits inconvenient ethical and political debate. If eugenics were based on technical errors, which have long been exposed, it is a historical curiosity, not a matter of contemporary concern. It is also attractive to geneticists, who like scientists generally, are naturally disposed to view "science as a progressive social factor which enlightens and dispels prejudices" (Roll-Hansen 1988: 295). As geneticists, they have a particular incentive to attribute eugenics' decline to increasing scientific sophistication. The troubling history of their field is constantly invoked by critics of contemporary human and medical genetics. It is surely comforting and convenient to assume that geneticists, who once enthusiastically supported eugenics, also exposed its shortcomings. It shows that they were ultimately reliable and, by implication, that their heirs are worthy of trust.

In more sophisticated versions, this interpretation has also appealed to historians of science with a strong rationalist/realist orientation. Thus, in a series of important papers, Nils Roll-Hansen has defended the view that science has been a socially-progressive force, in the history of eugenics as elsewhere. While not ignoring the significance of ideological and political factors, particularly the post-World War II emphasis on individual rights, Roll-Hansen has consistently stressed that increasing knowledge of human heredity made clear the inefficiency of eugenical selection, and hence was largely responsible for the decline of eugenic thinking in the 1940s and 1950s (1989: 343–345). In his rightly influential history of eugenics, Daniel Kevles similarly acknowledges the role of non-scientific factors, while according scientific developments an important role in eugenics' decline. Kevles distinguishes 'mainline' from 'reform' eugenicists; the former tended to be socially and politically conservative, infected by racial and class bias, and scientifically naive, while the latter tended to be liberal or left in their thinking, reject racial and class bias, and understand that too little was known about human heredity to justify coercive measures. In Kevles's view, an important factor in turning the reform geneticists, such as J.B.S. Haldane, Lancelot Hogben, Julian Huxley, and H.S. Jennings, against mainline eugenics was "the rapidly advancing field of genetics" (Kevles 1985: 124–125).

Today, the view that eugenics was unable to withstand its encounter with scientific fact, or was even seriously undermined by it, is no longer fashionable in history or social studies of science. It cuts against the grain of constructivist accounts of knowledge, which as Barbara Herrnstein-Smith notes, stress "the *participation* of prior belief in the perception of present evidence," as opposed to realist and rationalist accounts, which "insist on the possibility of the *correction* of prior beliefs by present evidence" (Herrnstein-Smith 1991: 140). But even historians not inclined toward constructivism have come to doubt whether the undoubted advance of science played a significant role in the waning of eugenics. The doubts arise from the following considerations:

1. The scientific developments conventionally said to have eroded support for eugenics occurred in the 1910s, long before eugenics became disreputable. The multiple-factor explanation of continuous variation was first suggested by the English biometrician Karl Pearson in 1904, followed by the Swede Herman Nilsson-Ehle in 1908–09 and by the American Edward M. East in 1910, while R.A. Fisher's famous paper, "The correlation between relatives on the supposition of Mendelian inheritance," appeared in 1918. Discovery of the Hardy-Weinberg principle and recognition of its implications also occurred early. The work of G. H. Hardy and (independently) Wilhelm Weinberg dates to 1908; its consequences for eugenics were first noted – by East – and refined by R.C. Punnett in 1917.
2. A related consideration is that a thorough critique of eugenics long predated its decline. Thus, the methodological problems that vitiated much of the work that issued from the Eugenics Record Office at Cold Spring Harbor were identified by the biometricians Karl Pearson and especially David Heron in *Mendelism and the Problem of Mental Defect,* published in 1913–14. Heron in particular identified numerous technical flaws in the work of Charles Davenport and other American Mendelian eugenicists in a series of very well-publicized critiques (Spencer / Paul 1998).
3. The researchers responsible for the growth of relevant knowledge and correction of error were themselves eugenics enthusiasts. They included Nilsson-Ehle, East, Pearson, Heron, and even Punnett (who is often misconstrued as a critic [cf. Paul / Spencer 1998: 122]).
4. Although the Hardy-Weinberg theorem undermined claims for the rapidity of selection, it did not demonstrate that eugenics was futile. As noted earlier, it had often been said in the 1910s that one or two generations of eugenical selection would be enough to eliminate the problem of mental defect. But as the implications of the theorem were absorbed, it became evident that this claim was untrue, and by the early 1920s, the nature of the mistake was well-understood.

However, the new understanding seems not to have led anyone committed to eugenics to abandon their stance. Nor is there any reason it should have. For one thing, mental defect was not considered rare. Indeed, the *raison d'être* of the eugenics movement was fear of the normal population being swamped by the 'feebleminded.' As R.A. Fisher showed, on standard assumptions about the incidence of the trait, substantial progress would result from a few generations of eugenical selection. And, as Fisher also noted, the point of eugenics was never to eliminate the last defective individual. Moreover, even if selection did work very slowly, most eugenicists would continue to support policies to prevent mental defectives from breeding. In their view, even a small reduction in the trait was better than none. H.S. Jennings, who did much to make the implications of the Hardy-Weinberg theorem clear to the public, reflected a common view when he wrote: "Even though it may get rid of but a small proportion of the defective genes, every case saved is a gain" (Jennings 1931: 207).

5. In at least some respects, eugenics gathered steam in the 1930s. If science exposed the futility of eugenical selection, how can we explain the fact that between 1933 and 1940 compulsory sterilization laws were adopted by all of the Scandinavian countries, Germany, and Japan (among other states), while existing laws were more rigorously enforced? In the 1910s and 1920s, there was considerable opposition to compulsory sterilization on the grounds that the statutes were ineffective, rested on unfounded assumptions about the heredity of the targeted defects, promoted sexual promiscuity, and were biased in their application. In the U.S., most eugenicists favored segregation. But opinion shifted in the 1930s. Prominent foes of sterilization now came to think it sensible. The turn from segregation to sterilization was presumably a response to the world-wide economic depression. Sterilizing institutionalized individuals, who could be returned to the community, reduced the burden on the public fisc.

That is not to claim that science was superfluous. Indeed, it clearly mattered in at least two ways. First, it prompted eugenicists to modify their arguments. The claim that feeblemindedness could be eliminated in a generation or two was effectively abandoned. Moreover, the Hardy-Weinberg theorem provided resources for critics. Both in- and outside the genetics community, the claim that selection was too slow to justify the effort was frequently employed by critics of segregation or sterilization. What it did *not* do was convince proponents of these practices to change their mind. Here as elsewhere, prior beliefs were remarkably stable in the face of apparently contradictory evidence. Individuals who thought that mental defectives should not be permitted to breed had very little trouble accommodating the Hardy-Weinberg principle.

6. Revelations of Nazi atrocities did produce widespread revulsion against genetic explanations of individual and group differences in general, and eugenics in particular. In the 1950s, "cultural determinism" reigned (cf. Nelkin / Lindee 1995: 34–37). But this development obviously had nothing to do with science. As Weingart notes,

> the crucial factor in the [post-war] loss of legitimacy of eugenics and race-biology, in conjunction with the overwhelming moral indictment, was a shift in political values, i.e. the restoration of the rights of the individual and not, as is often claimed by the scientific community, the prevalence of 'good science' – the new genetics – over 'bad science' – German race-hygiene – and / or the end of the 'abuse' of science by corrupt political regimes – the Nazi's suppression of genetics (Weingart 1999: 173).

Moreover, the rejection of eugenics was very uneven. Many scientists did not share the new enthusiasm for cultural explanations of human differences; their misgivings were reflected in resistance to the first version of a 1949 UNESCO statement asserting that all races were genetically equal (Provine 1986). As early as the mid-1950s, a backlash against the dismissal of eugenics was evident. Prominent molecular scientists, perhaps emboldened by the discovery of the double-helical structure of DNA and unraveling of the genetic code, argued that recent medical and military developments necessitated what they explicitly called 'eugenics.' In the view of these scientists, advances in medical treatment were allowing many individuals who would in the past not have reproduced to enjoy near-normal fertility. At the same time, expanded medical and military uses of atomic energy, especially atmospheric nuclear testing, were increasing the load of deleterious mutations. Moreover, many commentators assumed that a perceived population explosion would anyway require restraints on human breeding. If it were necessary to control population quantity, they reasoned, why not also control population quality?

In the 1950s and 1960s, scientists such as H.J. Muller, Bentley Glass, and Linus Pauling in the U.S., Francis Crick, Julian Huxley, and N.W. Pirie in Britain, and Hans Nachtsheim in Germany (as well as the American theologians Paul Ramsay and Joseph Fletcher), argued that there was an urgent need to replace the current laissez-faire system of reproduction. In 1952, Nachtsheim even attempted to resurrect the Nazi sterilization law of 1934 (Weingart, Kroll, and Bayertz 1988; Weingart 1999: 175; Paul and Falk 1999). A number of commentators thought they saw a trend, with eugenics again becoming fashionable (cf. Paul, in press). They could hardly have been more wrong. By the mid-1970s, eugenics was definitely in disrepute, at least among those who came to speak for the public in the realm of reproductive genetics.

This turn of events seems most plausibly explained by the social turmoil that began in the 1960s. The anti-war and civil rights campaigns challenged established authority, a trend reinforced by the patients' rights and womens' movements that followed in their wake. A series of scandals involving experiments on human subjects undermined the assumption that physicians could be trusted to act in their patients' best interests. At the same time, women demanded control of their economic resources, their life decisions, and especially their own bodies. 'Autonomy,' 'choice,' and 'self-determination' became feminist dictums, and the concept of 'reproductive responsibility' was replaced by 'reproductive rights.' In the new perspective, reproduction was an entirely private matter, in which the state had no business meddling. These are the main elements in the case against the conventional view that attributes the decline of eugenics to the progress of genetics. To summarize: The scientists responsible for the developments said to have undermined eugenics were themselves eugenicists, whose discoveries occurred too early to provide plausible explanations for shifts in attitudes that began only in the post-World War II period. The most oft-cited scientific discovery – the Hardy-Weinberg principle – was not after all inconsistent with advocacy of eugenical selection. Nor can scientific discoveries explain the sudden resurgence of eugenic discourse in the 1950s and 1960s and its equally rapid fading.

Rethinking the Critical Case

For all these reasons, the conventional account seems to fail. But its critics are also vulnerable at several points. Thinking about statistical methodology directs our attention to the following problems in both the received and more recent views:

1. Sampling bias: *Whose* views are to be sampled? I have looked primarily at geneticists. But I could have selected a different focal group, such as doctors, social workers, home economists, or politicians. Moreover, it is possible – even probable – that none of these are reflective of the views of the public(s). As Martha Nussbaum notes, all cultures involve conflict over norms, and 'what most people think is likely to be different from what the most famous artists and intellectuals think' (Nussbaum 1997: 127–128). In general, norms are articulated by elites. What looks like a general cultural shift may instead reflect shifts in thinking among elites or even the replacement of one set of elites by another. In this case, it is relevant that beginning in the 1970s, bioethicists began to replace scientists as the primary spokespersons on social and ethical issues in genetics. During the 1950s and 1960s, most books on genetics-related issues were authored by distinguished scientists, and it was to scientists that journalists and conference-organizers typically turned for guidance on such issues. But by the 1970s, that discourse was dominated by bioethicists. Having emerged as a distinct intellectual discipline in the 1970s, bioethics was inevitably affected by the patients' rights and feminist movements, and it embraced as its core value the principle of respect for autonomy. Thus underlying norms about reproduction may in fact have been much more stable than we would be led to believe if our evidence were limited to statements by professionals.
2. Ascertainment bias: This related kind of bias, in which skewed results arise from the way in which cases come to our attention, vitiated much work in eugenics itself. For example, Heron pointed out that some data collected by the Mendelian eugenicists was tabulated and analyzed only when at least one child in each family was mentally defective, thus creating an excess of defectives. But ascertainment bias is equally a problem for historians. My work, like that of many others, considers (a handful) of individuals, who

came to attention because they were famous and published in leading journals. If they constituted an unrepresentative sample, adding more instances would not help. The result would be what statisticians call "pseudo-replication."

3. Multiple endpoints: What counts as evidence for the truth of our hypothesis? The problem of deciding what to measure is notoriously severe when the subject has fuzzy boundaries and is hard to define (Gilovich 1991: 59). The term "eugenics" has protean meanings. Some definitions are extremely broad, incorporating virtually any activity in the realm of human breeding. For example, today prenatal diagnosis is sometimes considered eugenics on the grounds that it involves selection of fetuses. But it is more often excluded on the grounds that the technology serves individual rather than social purposes and/or that the means employed are voluntary rather than coercive.

How eugenics is defined has political implications. Critics of contemporary genetic testing generally prefer an expansive definition, thus associating testing with disfavored practices of the past. Supporters, on the other hand, tend to favor a narrow definition, thus divorcing testing from those same practices. However, the choice of definition also matters greatly for any thesis about eugenics' decline. When an earlier generation of historians claimed that eugenics fell into disrepute in the 1930s, they referred not to the general idea of improving human heredity through selective breeding but to the specific beliefs about class and race superiority and specific practices associated with the 'mainline' movements. That is why these histories characterize geneticists such as J.B.S. Haldane and H.S. Jennings as critics of eugenics, notwithstanding views about who should and should not breed that would, on the broader definition now (implicitly) adopted by most historians, mark them as proponents. Underlying disputes about continuity or discontinuity are often disguised conflicts over definition.

Even if we could agree on the meaning of eugenics, it is not obvious what the best indicators of its waxing and waning would be. Passage of laws or other concrete policies? If so, which? Attitudes? If so, whose? I have used passage and enforcement of the sterilization laws as a measure of support for eugenics. But there is considerable evidence that advocates of such laws were generally indifferent to the cause of mental defect; in their view, it was irrelevant whether feeblemindedness was attributable to heredity or environment. What mattered was that institutionalization was expensive and that mental defectives made bad parents. On this line of reasoning, there is no inconsistency between increasing support for sterilization and declining belief in the power of genes to shape mentality and behavior. Thus changes in respect to sterilization may correlate poorly with hereditarian beliefs or other conventional markers of eugenics.

4. 'Optional Stopping' (or 'Variable Windows'): This is an analogue to the problem of multiple endpoints, involving shifting timeframes rather than kinds of evidence. When does one stop counting? Roll-Hansen has noted that it may take considerable time for the implications of some scientific developments to be recognized and incorporated into practice. He is right. Perhaps the discovery of multifactorial inheritance and the complexity of gene action had long-delayed effects. But without specifying a timeframe in advance, the temptation will be to stop at the point that the hypothesis is confirmed.
5. Particularity: History is always the single realization of a process. This problem of having one sample path of course unites studies of nature and society: paleontology, historical geology, evolutionary biology, systematics, the study of the origin of life, perhaps even cosmology – as well as the histories of science or, for that matter, of printing, or peasant revolts, or changing styles of dress. There was one Cambrian explosion – or only one we can study – just as there was only one Copernican and one industrial revolution, and one world-wide Depression. Only Emile Durkheim founded French sociology – and only once.

   Of course to test historical hypotheses, we can sometimes make use of 'natural experiments.' One form of natural experiment is the comparative method (cf. especially Adams 1990). The industrial revolution occurred in multiple places, as did the eugenics movement. But use of the comparative method to look for fundamental commonalities is fraught with difficulty. In the history of eugenics, there are such a such a small number of cases that we quickly use up the 'degrees of freedom.' Moreover, these cases are not independent of each other: The eugenics movement in Germany was influenced by eugenics in the United States, which was influenced by eugenics in Britain, and so forth.

6. Interaction: R.C. Collingwood enjoined historians to get inside of an event, into people's heads, to rethink the thoughts and relive the experience of historical actors. But as even scholars sympathetic to Collingwood have noted, this approach can take us only so far, since historical events and states of affairs occur 'over the heads' of the participating individuals (cf. Dray 1989: 9). Charles Rosenberg has made a similar point in arguing the need for etic as well as emic approaches in the history of science on the grounds that the larger conceptual, social, and material structures in which their work is embedded "are often opaque to the objects of one's research" (Rosenberg 1988: 566).

The need to take account of woods as well as trees (in Rosenberg's phrase) means that we face the problem of sorting out interacting factors, a task that is particularly daunting when there are a large number of weak interactions, and so small contributing causes. In biology, effects may be masked – or exacerbated – depending on the company they keep. While there can be an effect of A regardless of B, and of B regardless of A, whether A matters may also depend on the presence and level of B. Even the direction of A can reverse depending on B.

In the realm of parametric statistics, the analysis of variance, for all its limitations, provides a tool for untangling the effects of multiple factors, each having not only "main" effects of their own, but also effects only in conjunction with other factors. Of course we can not use anova to sort out complexly interacting factors in history. But we can be attentive to the fact that particular scientific developments may be causally efficacious only when linked to specific social events. That point is illustrated by the history of the Hardy-Weinberg theorem. In the 1920s, it seemed to most people to provide a better reason to expand eugenic efforts (by identifying the hidden carriers) than to abandon them. By the 1970s, the same theorem seemed instead to provide self-evident proof of eugenics' futility. What changed were our values. When individual rights came to be held in high esteem, the theorem came to carry quite different implications than it had previously.

In short, the study of statistics directs our attention to the importance of replication and independence of evidence, to the dangers of pseudo-replication and of sampling and ascertainment bias, to the temptations of optional stopping, and in general to the difficulty in concluding that a factor is causally efficacious in the world. Historians are of course well aware of these issues, which have all been described under other labels. But employing the language of another discipline, particularly one focused on causes, can bring some of their features into sharper relief. It can also help us to be more genuinely reflexive about our own work.

Acknowledgements

Two years ago, I was persuaded to develop a freshman seminar in quantitative thinking. A less logical choice to teach such a course would have been hard to find. I had no background – having slept through two cookbook-style statistics courses in graduate school – and even less interest in matters quantitative. However, faced with the daunting prospect of having to teach the subject myself, if only at a baby level, I bought a small library of books and also sat in on yet another course. That this time it clicked is attributable mostly to the extraordinary skill of the instructor, Richard Lewontin, and the section leader, Andrew Berry (who helped me think through the implications for historiography). I am grateful to Hamish Spencer and to Mark Solovey as well for useful comments on a draft of the essay.

Notes

1 For a more detailed account of the history of this argument cf. Paul and Spencer (1998).

2 According to Buchanan et al., earlier historians believed that: "Eugenics was abandoned as the science of genetics progressed, leaving genetic scientists increasingly dubious of the factual claims of the movement" (Buchanan et al. 2000: 39).

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