

Eugenics and Modern Biology: Critiques of Eugenics, 1910–1945

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Summary

Eugenics in most western countries in the first four decades of the 20th century was based on the idea that genes control most human phenotypic traits, everything from physical features such as polydactyly and eye colour to physiological conditions such as the A-B-O blood groups to mental and personality traits such as “feble-mindedness,” alcoholism and pauperism. In assessing the development of the eugenics movement—its rise and decline between 1900 and 1950—it is important to recognise that its naïve assumptions and often flawed methodologies were openly criticised at the time by scientists and nonscientists alike. This paper will present a brief overview of the critiques launched against eugenicists’ claims, particularly criticisms of the American school led by Charles B. Davenport. Davenport’s approach to eugenics will be contrasted to his British counterpart, Karl Pearson, founder and first editor of the *Annals of Eugenics*. It was not the case that nearly everyone in the early 20th century accepted eugenic conclusions as the latest, cutting-edge science. There are lessons from this historical approach for dealing with similar naïve claims about genetics today.

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Introduction

The *Annals of Human Genetics* was born out of the conviction that understanding the nature of human heredity would make it possible to improve the biological quality of the human population. Launched as the *Annals of Eugenics*, the journal evolved squarely out of the eugenics movement of the early 20th century. The journal’s founder and first editor, Karl Pearson (1857–1936), was the protégé of Francis Galton (1822–1911), the originator of eugenics as a concept, and the man who coined the term itself (meaning well, or truly born). While eugenicists were interested in all aspects of human heredity, they were particularly concerned with social and personality traits such as intelligence, “feble-mindedness,” criminality, alcoholism, pauperism and mental disorders such as schizophrenia and manic depressive insanity. Most eugenicists believed that such traits were to a large degree, if not exclusively, genetically determined. The perception was widespread that these conditions were increasing

at a rapid rate in modern industrial society, and that since “low-grade” individuals with these traits were having more children than “high-grade” individuals, eventually good traits would be swamped by bad and society would deteriorate. The answer, according to eugenicists, was to control reproduction by scientific means in order to increase the number of children born to high-grade, and reduce the number of children born to low-grade individuals and families. To this end, Pearson launched the *Annals of Eugenics* as an outlet for up-to-date research that would provide information on what traits were inherited and what their patterns of inheritance were (Pearson, 1925, pp. 1–4).

Eugenics, like genetics itself, was an international movement with many variants (Adams, 1990). Two of the most important movements were those in Britain and the United States. While sharing many characteristics, the two movements differed in various ways, reflecting to some important degree the orientation of their respective leaders: Karl Pearson in Britain and Charles B. Davenport (1866–1944) in the United States. Pearson’s work was cautious, biometrical, statistical and highly sceptical of the newly developing field of Mendelian genetics. Pearson’s approach is clearly represented in the contents of the various issues of the *Annals of Eugenics* under his editorship. Davenport’s approach, on the other hand, was vociferously Mendelian, and while he

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understood statistical approaches better than many of his contemporaries, he made little use of biometry in his eugenical work.¹

In many ways, Pearson and Davenport are comparable figures in their respective eugenics movements. Both had strong academic backgrounds and were among the most prominent advocates of eugenics. Pearson was the Galton Professor of Eugenics at University College, London (UCL), and a Fellow of the Royal Society while Davenport was a one-time faculty member at Harvard and the University of Chicago and a member of the U.S. National Academy of Sciences and the National Research Council. Both also headed their own eugenics research units: Pearson was head of the Galton Laboratory at UCL, while Davenport was Director of both the Station for Experimental Evolution (funded by the Carnegie Institution of Washington from 1904 onwards), and the Eugenics Record Office (hereafter ERO) (funded initially by the Harriman family of New York in 1910, and after 1916 by the Carnegie Institution) at Cold Spring Harbor, New York. Pearson founded his own eugenics journal (*Annals of Eugenics*), while Davenport founded *Eugenical News* in 1910, with his Superintendent at the ERO, Harry H. Laughlin, (1880–1943) as editor. Like the *Annals*, *Eugenical News* published papers on genetics in various organisms alongside papers on human heredity and analysis of family studies and correlations among relatives. Unlike the *Annals*, *Eugenical News* published articles on aspects of race, immigration, and sterilisation, reflecting the more strongly politicised and racialised nature of the eugenics movement in the United States.

Reading through the tables of contents or the pages of either the *Annals of Eugenics* or *Eugenical News* today's readers would never realise there was a good deal of potent criticism by scientists and nonscientists alike of many, if not all, of the eugenicists' claims. In this paper, I will systematically summarise by category (use of genetics or statistics and general methodology) a range of criticisms brought against eugenics in its own day. This will serve three main purposes: (1) It will make clear that it is not just in hindsight that eugenics claims seem naïve and racially/socially biased from the outset. (2) It will provide a summary, in one place, of the range of criticisms launched against eugenics in its own day. Almost all of the criticisms discussed in this paper have been discussed by historians of eugenics in existing publications. However,

¹ Davenport had spent part of a sabbatical year (1899–1900) in London with Galton and Pearson learning the principles of biometry, and shortly afterwards became one of the editors of the new journal that Galton and Pearson founded, *Biometrika*. Later, differences erupted and Davenport eventually resigned from his editorial post, but went on to publish a two-volume handbook on biometrical methods.

to bring all of the objections together in one place will emphasise the extent and quality of the critiques that may not be otherwise apparent. (3) It will help us understand how to approach claims put forward today about a strong genetic basis for complex social traits such as I.Q., criminality, manic depression, sexual orientation and religiosity (to mention only a few).

I have divided the discussion of criticisms of eugenics into several categories: criticisms of the basic genetic concepts, criticism of eugenicists' methodologies (sampling, data collection, analysis including statistical problems), criticism from a sociological and political perspective, and finally, criticism of the very moral and ethical basis on which eugenical ideas were based.

Opposition to the Scientific (Genetic) Basis of Eugenics

General Criticisms

One of the most generally damning evaluations of eugenic work coming out of Davenport's ERO was that it was carelessly and sloppily conceived and executed, and lacked any semblance of normal scientific rigour. Such wide-ranging criticism appeared early in the history of the movement in 1913 and came from the Galton Laboratory, under Pearson. The particular studies that provoked the criticism were a series of papers on the inheritance of mental defect, or "feeble-mindedness" by Davenport (Davenport, 1912a, b, c) and Henry H. Goddard, author of the highly popularised study of the Kallikak family in New Jersey (Goddard, 1912). The criticisms came in a series of three papers by David Heron (1913), Pearson & Jaederholm (1914) and Pearson (1925). All three investigators were strong eugenicists in their own rights, and felt that poorly conceived and executed work would "cripple the progress of eugenics" by making it seem slipshod and unscientific (Spencer & Paul, 1998: p. 443). Indeed, Heron claimed that "those of us who have the highest hopes for the new science of Eugenics in the future are not a little alarmed by many of the recent contributions to the subject which threaten to place Eugenics with the older 'social science' and much modern sociology – entirely outside the pale of true science" (Heron, 1913: p. 4; quoted in Spencer & Paul, 1998: p. 443).

Of the three critiques coming from the Galton Laboratory, Heron's was the most bold and forthright in its criticisms. Heron's most general criticism was the lack of critical approach that went into Davenport's and others' work on feeble-mindedness. He felt that papers were based on data that "has been collected with a decided bias in favour of a

particular theory of heredity [Mendelism];² that it is presented with extraordinary carelessness; that it is, on internal evidence, repeatedly contradictory; that it is not treated in any adequate statistical manner, and that the conclusions reached are not justified by the data” (Heron, 1913: p. 12). Heron went on to criticise the assumption of a Mendelian explanation, when the data should have been collected from an unbiased perspective and then tested against various theoretical frameworks. He also argued that Davenport’s assumption of feeble-mindedness as the result of a Mendelian recessive gene (expressed in the homozygous state) as too simplistic to be believable. In many cases, the genetic characteristics of ancestors were assumed with no corroborating evidence. For example, if two apparently normal parents produced a feeble-minded child (ff), they were assumed to be heterozygous for feeble-mindedness (Ff), and investigators then searched the family history for other ancestors who could be classified as feeble-minded, often reading into whatever descriptions were available characteristics that could make them appear to have the trait. There were also instances of what appeared to be sloppy research and analysis. In some cases, text descriptions did not match the case numbers given in the data tables (Spencer & Paul, 1998: p. 444). In some instances, heterozygotes were described as normal while in others they were labelled as intermediate between normal and feeble-minded. Moreover, ratios were claimed to follow Mendelian expectations when the data showed they did not (all summarised from Spencer & Paul, 1998: p. 444).

It was problem of this sort that plagued the hardline Mendelian eugenics work in the United States and was apparent (to anyone who looked with any sort of critical eye) from the start, and which a variety of investigators criticised for the next 30 years.

We turn now to more specific categories of criticism of eugenic work in the United States.

Problems in Defining Phenotypes

The most generally pervasive criticism of the genetic basis of eugenics, as exemplified by Davenport’s interpretation of feeble-mindedness, concerned the tendency to oversimplify. One form of oversimplification lay in the definitions of behavioural or personality phenotypes themselves. What is “feeble-mindedness,” “criminality,” or “manic depressive insanity”? A British writer, K.L. Kenrick claimed as early as

² Some lingering suspicion of Mendelian genetics, despite its advances by 1914, may have coloured Heron’s evaluation of the ultra-Mendelian approach taken by Davenport, though Heron does not seem to have played off biometrical against Mendelian work so much as what he considered to be the simplistic and uncritical claims made by Davenport and other eugenicists in the United States.

1914 that conditions like “feeble-mindedness” are so vague and subjective as to be meaningless (Kenrick, 1914: p. 72). T.H. Morgan made much the same point a decade later when he pointed out:

The case most often quoted is feeble-mindedness that has been said to be inherited as a Mendelian recessive, but until some more satisfactory definition can be given as to where feeble-mindedness begins and ends, and until it has been determined how many and what internal physical defects may produce a general condition of this sort, . . . it is extravagant to pretend to claim that there is a single Mendelian factor for this condition (Morgan, 1925: pp. 200–201).

The same can be said for “intelligence” or “insanity.” Morgan pointed out that intelligence is not a single entity, but that there are many kinds of intelligences:

The main difficulty is one of definition. It is commonly assumed that there is one, and only one, criterion of intelligence – that we are speaking always of the same thing when we use the word. In reality our ideas are very vague on the subject . . . Accurate work in heredity can only be attained when the diagnosis of the elements of a situation is known (Morgan, 1932: pp. 208–209).

Morgan went on to say that

The inheritance of derangements of the mental faculties of man is a very difficult problem, partly because there are few, if any, parallels in other animals that can be experimentally tested, partly because the diagnosis in man is often uncertain, and partly because the environment is a complicating agency . . . (*Ibid*, p. 207).

It is important to note here that Morgan emphasised that the precondition for accurate work in genetics is a clear definition of the phenotypes being investigated. A similar variation of this argument is that voiced by social psychologist J.B. Eggen, who pointed out that terms like “feeble-mindedness” or “insanity” covers a multitude of specific components and are not a single entity:

The term [insanity] designates a wide variety of functional disorders, and not by the widest stretching of the imagination can they all be grouped together and considered as an entity . . . [mathematical ability, musical ability, immorality, alcoholism] are not single definite things, which can be inherited as a unit. They are complex and variable factors, not one of which can ever be thought of as a unit or entity (Eggen, 1926: p. 885).

Abraham Myerson, a neurologist at Tufts College Medical School and his colleagues from the Neurological Association of America also made this same point over and over again in their 1936 evaluation of eugenical sterilisation, pointing out that the relationship between various psychoses “is not close enough to warrant the postulate of any widespreading

unitary trait back of all psychoses . . . Consequently, a great deal of the work that has been done is entirely invalid and has only historical significance” (Myerson et al., 1936: p. 88). The authors go on to name Davenport and the Cold Spring Harbor investigators in particular, as having done work that was so vague and sloppy as to be worthless.

It is not necessary to multiply examples endlessly to underscore the fact that many critics found the very first step in a genetic study—precise definition of phenotype—seriously wanting in much eugenic research.

Oversimplification of Genetic Models: The Unit-Character Concept

Equally problematic was the tendency, especially of the American school, to postulate simple Mendelian factors to explain complex behaviours—that is, they worked under the *unit-character concept* of early Mendelism (Jennings, 1924; Morgan, 1932; Ludmerer, 1972: p. 62). The unit-character concept is based on the assumption of a one-to-one relationship between a phenotypic character and a Mendelian gene. Thus, there is a gene for eye colour, another for stature and yet another for feeble-mindedness. Herbert Spencer Jennings (1868–1947), a Zoologist and protozoan geneticist at Johns Hopkins University, was particularly critical of this concept by the late 1920s, arguing that most knowledgeable geneticists had rejected it 10–15 years previously:

“ . . . from the fact that the ‘unit characters’ changed when a single gene changed, it was concluded that in some ill-defined way, each characteristic was ‘represented’ or in some way condensed and contained, in one particular gene . . . There is indeed no such thing as a ‘unit character’, and it would be a step in advance if that expression should disappear . . . The doctrine is dead.” (Jennings, 1924: p. 237)

Similarly, Myerson et al. state that

“There is now on record a considerable body of evidence to show that often a few and sometimes many genes in the residual hereditary background of an individual affect the expression of a single gene being studied . . . [In terms of fluctuating characters] there is little doubt but that this fluctuation is due to modifying factors in the residual heredity of the individual . . .” (Myerson et al., 1936: pp. 73–74).

That Davenport could, as late in the development of Mendelian genetics as 1919, publish an account of *thalassophilia* (love of the sea) as a Mendelian, sex-linked recessive found in the families of naval officers only underscores the height of oversimplification embodied in the unit-character concept still rampant in American eugenical thinking.

Oversimplification of Genetic Models: Gene–Environment Interactions

Moving beyond the gene, the problems of definition are only compounded at the next step of eugenical investigation, namely the collection of data on members of families and their organisation into pedigrees. Much of the data collected was itself anecdotal, making any further use of it questionable. L.C. Dunn and the Visiting Committee to the ERO at Cold Spring Harbor in 1929 and again in 1935 made this point abundantly clear. After the first visit, the Committee criticised Laughlin’s and Davenport’s reliance on so much subjective evaluation of cases by their fieldworkers (Allen, 1986: p. 251). The second report was more direct and scathing:

“The records, upon which so much effort and money have been expended, have to date been extremely little used, to judge by the number of publications based upon them. Thus the Office [Eugenics Record Office] appears to be accumulating large amounts of material, and devoting a disproportionately great amount of time and money to a futile system for indexing it, without certainty, or even good probability, that it will ever be of value.” (quoted in Allen, 1986: p. 251).

This report, along with Davenport’s retirement and Laughlin’s declining health, sealed the fate of the ERO, whose funding ceased as of December 31, 1939.

The individual pedigrees constructed from eugenic family studies were also of questionable value. Morgan noted that pedigrees confounded effects transmitted biologically with those transmitted culturally, and by themselves proved nothing about the nature of inheritance of behavioural, personality, or mental traits. Speaking of pedigrees of so-called degenerate or “cacogenic” families, Morgan wrote:

“The pedigrees that have been published showing a long history of social misconduct, crime, alcoholism, debauchery, and venereal diseases are open to the same criticism [*i.e.*, conflating biological and social heredity] from a genetic point of view; for it is obvious that these groups of individuals have lived under demoralizing social conditions that might swamp a family of average persons. It is not surprising that, once begun from whatever cause, the effects may be to a large extent communicated rather than inherited.” (Morgan, 1925: pp. 201–202).

Given that Davenport himself had published detailed family pedigrees showing “inheritance” of pellagra (a niacin, or vitamin B1 deficiency) in 1916 only makes Morgan’s point more obvious (Davenport, 1916). As Morgan pointed out, families share social practices, including diets, as well as genes.

It was obvious that pedigrees do not separate genetic from environmental effects, and eugenicists themselves were well aware of the problem. Early on, A.M. Carr-Saunders in Britain, in an exhaustive critique of much eugenics work

up to 1914, pointed out that in their zeal to show biological heredity many eugenicists overlooked the role of environment (Carr-Saunders, 1914). As a neo-Lamarckian, Carr-Saunders observed that it was theoretically impossible to ever separate rigorously the effects of heredity from those of environment in any individual case. In addition, by such zealous emphasis on heredity eugenicists were showing an insensitivity to the value of changing social practices in improving the lot of their fellow human beings. Major Leonard Darwin (Charles Darwin's son) sounded the call for eugenicists not to give the appearance of ignoring all social or educational reform, lest they lose all credibility:

“What I am desirous of suggesting on this occasion is that the keenest advocates of eugenics are in danger of urging their views in such a way as to produce false impressions; for by perpetually harping on the vastly greater importance of heredity as compared with environment, a false belief may, and I think, at times has been created that they are careless concerning many reforms intended to improve the lot of human beings by improving human surroundings.” [And, quoting from Galton, he continues, eugenicists must] “state from time to time in the most definite manner possible that we do not deny the great influence of environment, and that we ‘acknowledge freely the great power of education and social influences in developing the active powers of the mind’.” (Darwin, 1916: pp. 93–94).

To Leonard Darwin and the more thoughtful eugenicists, heredity *and* environment worked together and all social programs should be grounded in that realisation.

In the interwar period, gene–environment interaction became an increasingly prominent theme among critics of eugenics in the United States. T.H. Morgan was one who repeatedly echoed this sentiment. Although a leader in the development of the atomistic gene concept, Morgan was always an embryologist at heart, and recognised the importance of environmental input into the development of any phenotype. After reviewing the “best case” evidence for the inheritance of mental traits (e.g., Huntington's chorea, considered a form of degenerative insanity at the time), Morgan pointed out the grave difficulty in trying to separate genetic from environmental influences in human beings:

“If these ‘best cases’ are so far from being established on a scientific footing, it is not particularly profitable to discuss the many claims that have been set up for other mental traits . . . The important point, however, to be urged is that the ‘mental traits’ in man are those that are most often the product of the environment which obscures to a large extent their inheritance, or at least makes very difficult their study.” (Morgan, 1925: p. 203).

With regard to even more vague characteristics such as “intelligence,” Morgan was doubly sceptical:

“The difficulty, of course, is – aside from our inability to define what is meant by intelligence – that we do not know here how much is due to nature and how much to nurture.” (Morgan, 1932: p. 209)

Indeed, Morgan goes on to speak of “*the two-fold method of human inheritance . . .*,” meaning, of course, cultural/social and biological inheritance, and of the difficulty in distinguishing between them.

Franz Boas (1858–1942), Professor of Anthropology at Columbia University and a strong opponent of eugenics from the start, wrote in *Scientific Monthly* as early as 1916, that it would seem the first obligation of the eugenicist ought to be to determine what traits are truly inherited and which ones are not. “Unfortunately,” he wrote, “this has not been the method pursued,”

the battle-cry of the eugenists, ‘Nature not nurture,’ has been raised to the rank of a dogma, and the environmental conditions that make and unmake man, physically and mentally, have been relegated to the background.” (Boas, 1916: pp. 471–478).

Eugenicists, Boas complained, simply *assumed* that most traits were inherited (which was, of course, the point to be proven in the first place) and then proceeded to argue about *what the pattern of inheritance was*.

Physiological chemist Oscar Riddle (1877–1968), who had carried out much work on endocrine function, particularly the role played by hormones in growth and development,³ was sensitive to the apparent disregard most eugenicists had for the role of the environment in the development of human mental and behavioural traits. In his view, genes were not rigid determiners, but their output, whatever it was, could be significantly influenced by external factors:

“It has become clear that the specific *conditions* under which a gene or factor operates and develops have an equal value with the germinal factors in the appearance of anything that can be called heredity.” (Riddle, 1928: pp. 62–71).

Riddle would have agreed with modern statements to the effect that there can be no gene expression without an environment in which that expression takes place, and in which the gene's expression is not affected in some way by that environment.

Meanwhile, in their lengthy critique of eugenics from a neurological perspective, Myerson et al. repeatedly speak of

³ Riddle was a distinguished endocrinologist who discovered the hormone prolactin responsible for stimulating the production of milk in the mammary glands. He was a member of the National Academy of Sciences, USA and, ironically, a long-time researcher (1913–1945) at Davenport's Station for Experimental Evolution at Cold Spring Harbor, the parent institution of the ERO.

the environment as a *releasing agent* for the genes, thus demonstrating their recognition of the gene–environment interaction that is the precondition for development of all traits. To extend their analysis further, Myerson et al. point out that the role of genetic modifying factors made it extremely unlikely that any behavioural or social traits are controlled by single genes, thus making any sort of detailed genetic analysis (without breeding experiments) theoretically impossible (Myerson et al., 1936: p. 74).

Among the most prominent and thorough-going criticisms of the unit-character, or single-gene effect was H.S. Jennings. Jennings approached the problem of heredity from a more holistic and integrative position than most of his contemporaries, and hence to him the oversimplifications of the eugenicists seemed particularly outmoded and dangerous. In a thorough and insightful analysis of Jennings' biological and philosophical background, Judy Johns Schloegel has argued that Jennings approached eugenics from both a strong social-political and pragmatic platform (Schloegel, 2006). Strongly influenced by both John Dewey's pragmatism and C. Lloyd Morgan's concept of emergent *evolution*, Jennings understood better than most of his contemporaries that: (1) the whole (an organism, for example) is greater than the sum of its parts and (2) the interactions among the parts give rise to new, or emergent, properties that could not have been predicted from knowing only the individual components. Jennings was trying to fashion a materialist biology that avoided the (to him) meaningless alternatives of either mechanism or vitalism. In 1924, Jennings published a critique of eugenics, "Heredity and Environment," derived in part from his written testimony to the House Committee on Immigration and Naturalisation, which he had prepared to counter Harry H. Laughlin's claims about the genetic inferiority of southern and eastern Europeans. In this article, Jennings pointed out that gene–environment interactions are crucial to understanding the nature of heredity:

"[N]ot only what the cell within the body shall become, but what the organism as a whole shall become, is determined not alone by the hereditary materials it contains, but also by the conditions under which those materials operate. Under diverse conditions the same set of genes will produce very diverse results. It is not true that a given set of genes must produce just one set of characters and no other. . . . It is not true that what an organism shall become is determined, foreordained, when he [*sic*] gets his supply of chemicals or genes in the germ cells, as the popular writers on eugenics would have us believe. The same set of genes may produce many different results, depending on the conditions under which it operates." (Jennings, 1924: pp. 232–233)

Jennings went on to say that "what the body as a whole shall become – depends not alone on what it contains – its

heredity – but also its relation to many other conditions; on its environment." (Jennings, 1924, pp. 230–231). Proceeding on an antireductionist, developmentally oriented line of reasoning, Jennings had occasion to point out that genes were not destiny:

"The characteristics of the adult are no more present in the germ cells than are those of an automobile in the metallic ores out of which it is ultimately manufactured." (Jennings, 1925; quoted in Myerson et al., 1936: p. 77).

Jennings had an appreciation for the process of embryonic development, akin to the process of manufacturing a car. In development, all genes acted against a background of other genes plus input from the environment. The blueprint for a car or the genes for an organism only materialises in the context of materials supplied from the outside, and the nature of these can affect in critical ways, the overall outcome (structure of the car or phenotype of the organism). No matter how precise the blueprints (genetic elements inherited from the parents) the ultimate and detailed form of the product (the offspring's phenotype) was always strongly influenced by environmental inputs.

It is important to point out that most eugenicists claimed they realised that environmental factors played a role in determining phenotypes, especially for human personality and social traits. In his article, "Crime, Heredity and Environment" in 1928, for example, Davenport acknowledged that what is considered criminal varies from culture to culture: "Crime is always a *relative* thing, having relation to the mores or to the behavior that is expected of one. In one country bigamy is a crime; in another it is the conventional mode of life." (Davenport, 1928b: p. 307). He goes on to acknowledge that crime is not a simple phenomenon: "We are dealing with a broad field of action or rather of interaction between persons and their surrounding world, involving numerous stimuli and varied response." (Davenport, 1928b). After reviewing the claims of both sociologists, who argued that criminality is due solely to environment, and criminal anthropologists of the Lombroso school,⁴ who argued that it is all biologically innate, Davenport takes what seems like the reasonable position that the causal factors vary from case to case and

⁴ Cesare Lombroso (1835–1909), an Italian psychiatrist and founder of the field of "criminal anthropology" in the 1890s, argued that criminals are a degenerate subset or "race" of the human species characterised by visible, atavistic features (what he called "stigmata") that allowed detection of the criminal, with correlations even between particular stigmata and particular types of crime. Although Lombroso's claims were largely rejected by the 1920s, he is credited with shifting criminology from a purely legalistic to a more scientific, some have said, more "humane" approach. Davenport did not accept Lombroso's findings but did appreciate his attempt to understand criminals as "born" rather than made.

probably both are involved to some extent in all cases (Davenport, 1928b: p. 308). However, the remainder of the article is devoted to examining cases of *hereditary crime*, on the grounds that since most people have inherited good social and altruistic instincts and thus do not commit crimes, the phenomena to be explained are the exceptions, the habitual or hereditary criminals. Davenport's tactic here is fairly representative of many eugenicists: they acknowledge or pay lip service to the role of environmental factors, or of gene–environment interactions, but then proceed to discuss only the genetic side of the equation.

Methodological Problems: Experimental Design, Analysis of Data, Mode of Reasoning

A number of critics picked up on eugenicists' poor design of studies, circular reasoning, poor use of statistics, and a tendency to draw sweeping conclusions that went far beyond what the available data allowed.

In the process of data gathering, a number of critics noted that eugenicists gathered largely anecdotal evidence, by self-administered questionnaires or other means whose uniformity or accuracy could not be verified (Heron, 1913; Jennings, 1924; Morgan, 1925). Laughlin sent out hundreds of eugenic questionnaires to college students asking about family backgrounds, conditions known to exist in their families and the like—data that were wholly dependent on the recipient's reliability, accuracy and ability to gain information about family members. In an era before the routine preparation of medical records, much of the information on which family studies were made was gained by hearsay and guesswork. Even the trained field workers from the ERO often had to accept family and community rumours (i.e., gossip), or make their own subjective assessments of the family members they met. For example, Anna Wendt Finlayson, an ERO field-worker who studied the Dack family, descendants of Irish immigrants in western Pennsylvania, gave the following descriptions of William Dack, whom she had not met:

“William died almost fifty years ago, but he is remembered by a few of the oldest settlers of the locality as a peculiar, silly old fellow who drank a good deal, stole sheep and household valuables from his neighbors, and did not seem to be very intelligent... William's second wife a Mary Murphy... An old resident of Bushville, now deceased, once stated to a woman who was interviewed by the writer [Finlayson] that William and Mary were first cousins.”

And of James Dack, whom she did meet:

“James Dack was commonly known as ‘Rotten Jimmy’, the epithet was given because of the diseased condition of his legs,

which were covered with chronic ulcers, although the term is said to have been equally applicable to his moral nature. He was a thief and a general good-for-nothing, but neither shrewd nor cunning. His conversation quickly revealed his child-like mind.” (Finlayson, 1916: pp. 6–7).

On the basis of such information, family pedigree charts such as the one for the Dacks shown in Figure 1, were constructed and genetic hypotheses proposed to explain the individuals' phenotypes in Mendelian terms.

There were also problems of sampling error that greatly skewed results (usually in favour of eugenicists' arguments). One example will suffice, again from the ERO. To demonstrate that immigrants from different countries had hereditary differences in their tendency towards crime, in 1921 Harry Laughlin sent questionnaires to the superintendents of prisons across the country asking them to list their inmates by nationality. Laughlin then compared the percent of each nationality incarcerated against the percent of that nationality in the general population. A rating of 1.0 would be what was expected for any nationality if there were no differential hereditary tendencies to criminality from one national group to another. The results Laughlin compiled and presented to the House Committee on Immigration and Naturalization (as Expert Eugenic Witness) showed what eugenicists in the United States had long claimed: northern European countries showed a level at or below 1.0, meaning they were *under* represented in prison, while southern and eastern European, as well as Mediterranean, Balkan and Russian populations were well above 1.0, meaning they were *over*represented in the prison population (Laughlin, 1923: Chart 3, opposite p. 740). The results supported the initial assumption that northern Europeans (read “Nordic”) were genetically more socially responsible while southern Europeans and Slavics had a greater inherited tendency towards criminal activities.

Laughlin's data and conclusions were analysed and strongly criticised by Joseph Gilman, a Professor in the School of Business Administration at the University of Pittsburgh (Gilman, 1924). Gilman noted that Laughlin had not corrected his sample for age or sex, especially important for comparisons since incarcerated populations tend to be largely male, between the ages of 20 and 30, while the general population includes both males and females and all ages (Gilman, 1924). It was also well known, Gilman pointed out, that immigrant populations tend to include more males, since men come first to find work and establish themselves before bringing over their families. For the year 1916, Gilman noted, the number of inmates in state and federal prisons contained 12 times more men than women, yet only if the number of males in the general immigrant population were also 12 times greater, would the difference between immigrant and nonimmigrant groups be significant (Gilman, 1924: p. 40). Laughlin

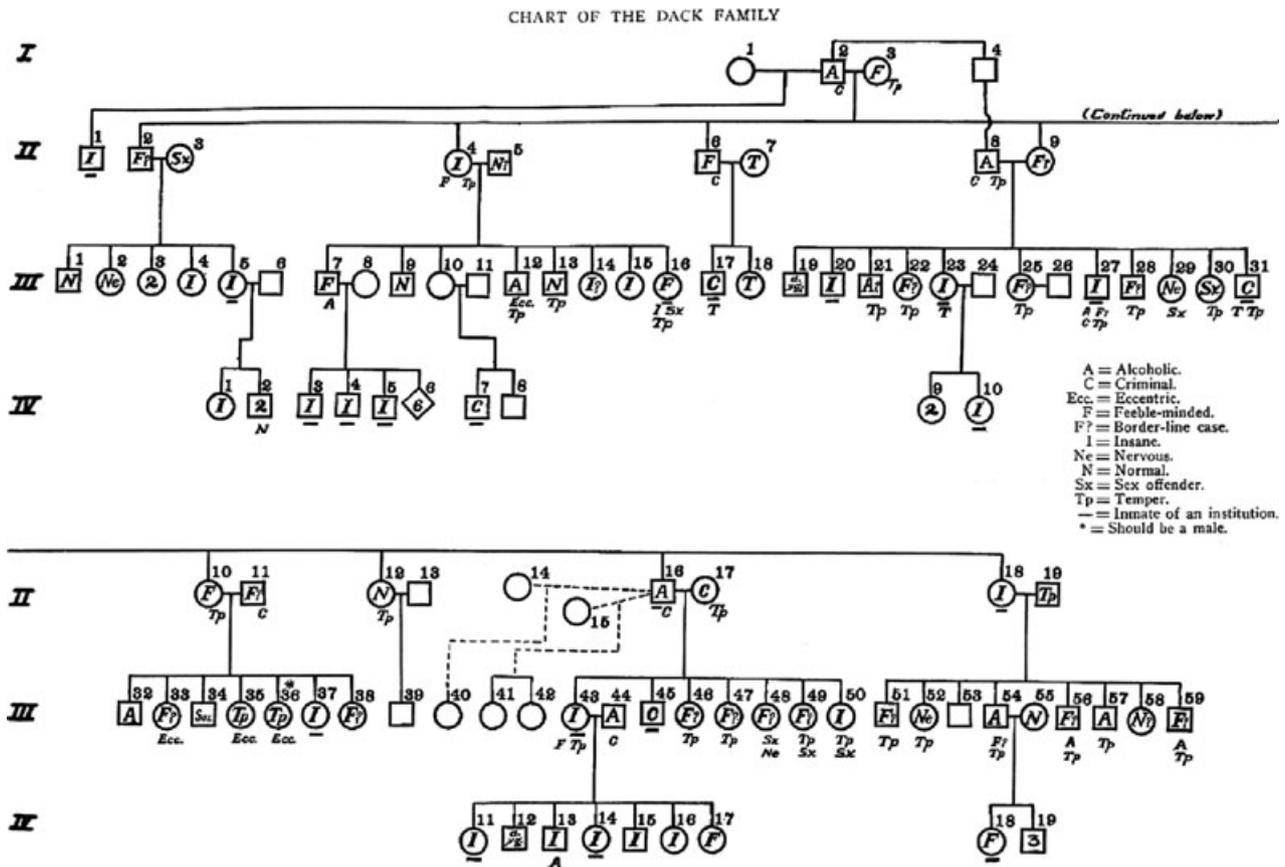


Figure 1 Pedigree of the Dack family of Rural Pennsylvania (Finlayson, 1916).

had not collected data on the percent of males in immigrant populations, thus rendering any comparison problematic.

In an even greater difficulty, Gilman noted that Laughlin's survey of prison populations was carried out in 1921, whereas the general population figures for comparison were taken from the census of 1910 (rather than 1920). Since general immigration rates were increasing between 1910 and 1914, and shot up dramatically after the war ended (1919), immigrant groups were *underrepresented* in the 1910 compared to the 1920 census, artificially inflating the percent of the 1921 prison figures. Laughlin actually noted this problem and claimed that he used the 1910 census to correct for what he thought was a "lag factor," that is, there would usually be a lag time between an immigrant's arrival and the chances they would be arrested and imprisoned. Thus, if he had used the 1920 census, Laughlin claimed, it would have artificially lowered the institutional percentages (Laughlin, 1923: pp. 828–830). Among the other factors that skewed Laughlin's results, Gilman noted, was that the response rate of prison authorities to Laughlin's questionnaire was much higher from the northeast than from any other part of the country. Since immigrants from Europe were more concentrated in the northeast, the prison num-

bers reflected the prison populations in that geographic area, while the census, of course, reflected the population of the country as a whole. Laughlin had either not noticed the geographically skewed returns, or had decided not to comment on them (Allen, 2001).

Another methodological issue is the failure of eugenicists to consider seriously, and test, alternative explanations for the causes of mental and social behaviours. Although not a biologist, journalist and essayist Walter Lippmann (1889–1974), took on the eugenicists broadside in the 1920s over everything from immigration and sterilisation to I.Q. tests and education. Lippmann pointed out that the results of the acclaimed Army Intelligence Tests administered during World War I by Robert M. Yerkes of Harvard (and a committed eugenicist) started with the assumption that intelligence was inherited, and collected test data on recruits (1,700,000 of them). When the data were analysed by Princeton psychologist Carl Brigham in 1923, he found a startling correlation between length of residence of immigrants in the United States and their performance on the tests. Brigham's conclusion, with which Yerkes agreed, was that the quality of immigrants had been declining for at least several decades (Brigham, 1923: pp. 110–111).

However, Lippmann found that a far more compelling correlation existed between scores on the tests and number of years of *schooling* (Lippmann, 1923: p. 97). Furthermore, arranging group scores by region of the United States, and by availability of schools (per capita) in these regions, yielded another more significant correlation. Could not education, Lippmann asked, account for a large amount of higher scores compared to lower scores on the tests? Lippmann was sophisticated enough to realise that correlations do not prove causality, but his point was that the psychologists such as Brigham, who were also staunch eugenicists, had not bothered to make such comparisons themselves. They simply proved what they started out believing.

There were also problems with the use or misuse of statistics. In Laughlin's crime data presented to Congress, for example, Joseph Gilman noted that while Laughlin reported the standard error (SE) for his calculations of percent incarcerated, he made no use of the data in his analysis, a particularly glaring omission when the SE was large. On a similar note, in the study on race-crossing in Jamaica, Davenport and Morris Steggerda, made much of what they claimed to be the greater variability of traits in "browns," that is offspring of one white and one black parent, than in offspring of white parents or black parents. In making this argument Davenport presented variability as standard deviation of 52 physical traits that Steggerda had measured in Jamaica. But physical anthropologist Wilson D. Wallis argued that Davenport should have used the *coefficient of variation* (standard deviation divided by the mean of each population), since the populations under consideration (whites, blacks and browns) all had different means for the traits that were examined (Wallis, 1938: p. 686, 690, 692; see also Altink, 2007: pp. 69–70). As Wallis pointed out, using standard deviation alone *did* appear to support the claim of greater variability among hybrids, but when coefficient of variation was calculated, all but a few traits (nasal breadth, for example) showed *less variability* in browns than in either blacks or whites (Altink, 2007: p. 692, and Table 7). The generalisation that hybrids between two populations always show greater variability seemed to Wallis to be, at best, a moot point; at worst, it was simply untrue. It was apparent that Davenport used the statistic that would give him the results he wanted, since he had argued that race-crossing was "disharmonious" and that greater variability meant greater disharmonies (Davenport, 1928a; 1929).

Practical Problems of Eugenics

A number of criticisms of eugenics touched on the practical problem of achieving eugenic goals even if those goals seemed desirable. Among the problems were, of course, the social, ethical and legal issues surrounding methods like compulsory

(or even voluntary) sterilisation. But from the biological and genetic perspective, there were problems, too. One was that if most pathological conditions were truly recessive, it would take generations to truly eradicate them from the population. Especially after the rise of population genetics, it was realised that earlier hopes of quick solutions to social problems through eugenic measures were naïve and unrealistic (Kevles, 1985).

On the other side of the coin, critics such as T.H. Morgan pointed out that social reforms would be a far more quick and effective way to deal with many of the problems eugenicists wanted to solve through genetics. As he wrote in 1925:

"Social reforms might, perhaps, more quickly and efficiently get at the root of a part of the trouble, and until we know how much the environment is responsible for, I am inclined to think that the student of human heredity will do well to recommend more enlightenment on the social causes of deficiencies rather than more elimination in the present deplorable state of our ignorance as to he causes of mental differences." (Morgan, 1925: p. 205)

Morgan later (1932) compared the basic method of dealing with mental defects to that with communicable diseases: In the past, we could have bred for greater resistance to cholera or TB, but it was quicker and more satisfactory to clean up the environment and thereby eliminate the pest, than to change the human constitution (Morgan, 1932: pp. 210–211). He concludes with a strong humanitarian plea:

"If within each human social group the geneticist finds it impossible to discover, with any reasonable certainty, the genetic basis of behavior, the problems must seem extraordinarily difficult when groups are contrasted with each other where the differences are obviously connected not only with material advantages and disadvantages resulting from location, climate, soil and mineral wealth, but with traditions, customs, religious, taboos, conventions, and prejudices. A little goodwill might seem more fitting in treating these complicated questions than the attitude adopted by some of the modern race-propagandists." (Morgan, 1925: p. 207).

The gist of Morgan's argument was that even if genetic factors might be involved in leading to certain social or mental conditions, it would make far more sense to search out the social components involved, since those could be changed more readily. It would not require multiple generations to provide better schools, clean up ghettos and improve living conditions (including public health measures), and these would inevitably have an ameliorating effect for everyone.

A considerable number of critiques of specific eugenics social programs—immigration restriction and the various proposals for compulsory sterilisation—were advanced as those campaigns became prominent in the public arena. I will not discuss here the many objections to eugenics on moral,

ethical, legal and religious grounds, as those have been discussed by others and would take us too far afield from the topic of the present paper. This is not in any way to diminish the importance, indeed, the centrality of such arguments in the larger picture of the history of eugenics. They were certainly part of the discussion in their own day. But they all raise questions, which would require a lengthier discussion than is possible here.⁵

Conclusion

In many ways, this paper has only scratched the surface of the variety of scientific arguments brought against eugenics in the first 40 years of the 20th century. What conclusions can emerge from this overview of criticisms of eugenics? How can it help us understand several important questions, both historical and contemporary? Among these questions, the most important seem to me to be: (1) Historically, did the criticisms launched from within the scientific community as well as by informed and influential lay writers, have any influence on the course of the eugenics movement? (2) And, depending on the answer to the first question, what can the history tell us about dealing with similar claims today about the genetic basis of many human social behaviours and mental conditions?

In answering the first question, it is important to note first that although there were various criticisms of eugenics in the first two decades of the century, their number and visibility increased noticeably after 1925. Especially in the United States, I would suggest this may have come as a response to two major public issues in which eugenics was highly influential: (1) the heated, nativist and very public debates surrounding immigration restriction and (2) the U.S. Supreme Court decision on *Buck v Bell* (1927), which upheld as constitutional a Virginia Sterilisation statute of 1924 allowing for forcible sterilisation of institutionalised individuals. As blatant racial and antiethnic biases were aired in the immigration debates, a number of geneticists, as well as others, began to realise that eugenics was not just oversimplified or bad scientific theory, but that it was being used to influence far-reaching and significant political and social policy. It is certainly clear that from the mid 1930s onwards, when the National Socialist government in Germany was making eugenics and race a cornerstone of

their national policy, a number of geneticists, particularly in Europe, began to take notice with more public responses.

What published criticism existed prior to 1920 probably had little influence on the course of the movement as a whole. Indeed, it was in the interwar years that eugenics movements everywhere reached their apogee. In the post-1925 period, however, considerably more criticism was published by well-known geneticists (T.H. Morgan, H.J. Muller, Raymond Pearl and H.S. Jennings in the United States, J.B.S. Haldane, Lancelot Hogben, Lionel Penrose and Julian Huxley in England, and Otto Mohr in Norway, among others), and at the very least this signalled that eugenics did not have the unqualified support of the entire genetics community. All historians agree that by the early-to-mid 1930s, the organisation and propagandistic aspects of eugenics movements in most countries (except Nazi Germany) were in decline, although social programs such as sterilisation were generally on the increase, due in part the economic pressures from the Great Depression (Paul, 1995: p. 134). Several factors may have been responsible for the downward trend of eugenic organising: (1) ironically, the very economic crisis that tended to promote *increased* sterilisations, reduced funding prospects and also underscored the fact that people's economic and social status could be dramatically affected by external conditions; (2) the fact that eugenics as an ideology promoting the interests of its ruling-class financial supporters (providing a biological rationale for a hierarchical view of society) had served its purpose and (3) the scientific criticism itself. With regard to the latter, the question is on *whom*, if anyone, did the criticism have an effect?

It is clear that scientific criticism did not have much effect on "mainline" eugenicists, such as Davenport, Laughlin, Morris Steggerda or Madison Grant. Most reacted defensively to criticism and virtually never changed their minds from their earliest to their later works. Notable exceptions were Henry H. Goddard and Carl Brigham, both of whom later recanted their claims about the strong genetic basis of mental ability and the meaning of national differences in performance on mental tests. They did not, however, renounce the underlying basis for eugenics practice, just the current use of mental tests to identify an underlying genetic basis for intelligence. Younger eugenicists in the United States, such as Frederick Osborn, a former New York railroad executive (he retired at 45),⁶ and long-time Secretary of the American Eugenics Society, and later President of the Pioneer Fund, certainly took to heart the criticism of blatant racism and jingoism and tried to steer

⁵ For more detail on critiques of the immigration issue in the United States, see Hascenahl, 1970, Ludmerer, 1972, Chase, 1977, Paul, 1995, and Allen, 2001. Objections to the various sterilisation laws have also been treated extensively by a variety of scholars (Myerson et al., 1936; Robitscher, 1973; Reilly, 1991, especially Chapter 8; Reilly, 1991; Kline, 2001, especially Chapters 2 and 3; Largent, 2008). One of the best discussions of the moral and ethical aspects of eugenic thinking is Troy Duster's *Backdoor to Eugenics* (2003).

⁶ The Pioneer Fund was established in 1937 by wealthy textile magnate Wycliffe Draper, with Laughlin as its first president. Osborn succeeded Laughlin in 1942. He was later replaced by Draper for being too liberal on racial issues. For a detailed and chilling history of the Pioneer Fund, which still exists, see Tucker (2002).

the eugenics movement in a slightly different direction, away from the kind of work done at Cold Spring Harbor. It is not likely that the scientific criticism had a major impact on the educated public in general. In fact, eugenic ideas were still being included in high school biology textbooks well into the 1940s and 1950s (Selden, 1999).

In the United States at least, the scientific criticism may have had its greatest impact on the philanthropic foundations, particularly the Carnegie Institution of Washington and the various Rockefeller entities, giving them cause to reconsider allocation of their funds. As early as 1923, John C. Merriam, President of the Carnegie Institution of Washington had sought the advice of Morgan, Pearl and other geneticists about the quality of all the work (the genetics work in particular) at Cold Spring Harbor under Davenport's direction. All responded that the work was mostly second or third rate and poorly planned with little coordination (Allen, 2004). And as we have seen, in 1929 and 1935 Merriam had convened a special visiting committee to evaluate the eugenics work of the ERO, which was found seriously wanting. It seems clear that the Carnegie Institution was becoming increasingly dissatisfied with the ERO in particular by the late 1930s and that as far as the Carnegie Institution of Washington was concerned, the scientific criticism played a central role (Kevles, 1985: p. 199; Allen, 1986: pp. 250–254; Allen, 2004: p. 199).

Regarding the second question: What can we learn from looking at the history of criticism by scientists of the old eugenics movement that might be relevant today in dealing with claims about a significant genetic basis for mental and social traits, and for issues of reproductive choice? I will focus on only one aspect of this rather large question, and that is the role of open criticism in both the scientific and popular public arenas. Perhaps the most important lesson coming out of the scientific response to the eugenics movement, was that prior to 1925 there was not a great deal of open, published criticism, and what there was remained largely in academic journals. In 1915 Morgan, for example, resigned from the Committee on Animal Breeding of the American Breeders' Association (on the grounds that it was claiming more than it could scientifically justify) but he did so privately in a letter to Davenport (Allen, 1978: pp. 228–229). Others who had doubts also expressed them in private correspondence but did not make their views public. In this way, the general public clearly got the impression that eugenics had the stamp of approval of the scientific/genetics community, especially given that the field was promoted by leading geneticists such as Davenport, and had gained a fair amount of exposure in the popular media.

From this history, I think two important conclusions emerge. The first is that it is important for knowledgeable geneticists to examine claims about the inheritance of this or that trait (especially complex behavioural, personality and mental

traits) when they are publicised today. We have been treated for several decades at the end of the 20th century to a barrage of claims about the genetic basis of a multitude of human complex behaviours, from I.Q. to criminality, aggressiveness, alcoholism, shyness, sexual orientation, manic depression, bipolar disorder and attention-deficit hyperactivity disorder—even “religiosity.” Many of these claims have not held up to careful scrutiny, and all have been criticised for the same faults for which the older eugenic studies were found guilty. Since claims about the genetic basis of such characteristics can have serious medical consequences—pharmacogenomics and drug treatment—it is important that the scientific accuracy of the various claims should be clearly delineated and exposed. This then leads to the second important lesson to come from the history of eugenics: the geneticists who evaluate the newer studies should also make their critiques public, and not restrict their publications only to technical journals. The importance of this became clear in the race and I.Q. controversy in the early 1970s, when Berkeley educational psychologist Arthur Jensen published a highly influential article claiming that the persistent difference in I.Q. scores between blacks and whites in the United States was due to genetics (Jensen, 1969). Because it was based on statistical analyses, Jensen's paper brought high praise from many who immediately saw its policy implications—as it occurred at the height of the bussing campaign for school integration. However, rather quickly population geneticists such as Richard Lewontin at Harvard and psychologist Leon Kamin at Princeton analysed and exposed Jensen's gross misuse of statistics (in particular, the use of heritability estimates) and reliance on older, questionable test score data on identical twins raised apart. The rather persistent attacks throughout the early 1970s by scientists (and historians of biology), along with sociologists, psychologists and statisticians made it clear to the general reading public that at the very least the research promoted by Jensen and his associates was controversial among scientists. I would argue that this open, very public criticism, was responsible for the relatively short lifespan of Jensen's theory (it lasted for only about a decade, though it was revived by Richard Herrnstein and Charles Murray in their book, *The Bell Curve*, published in 1994). So, I think the lesson from the history of eugenics is that those with the background and expertise to expose the fallacies in oversimplified genetic explanations should do so, as should we all try to bring our technical skills to a broader reading public. It is one of the most important ways in which we can insure that the misuse of our science does not occur again.

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