DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES

CALIFORNIA INSTITUTE OF TECHNOLOGY

PASADENA, CALIFORNIA 91125

GENETICS IN THE UNITED STATES AND GREAT BRITAIN 1890 to 1930: QUERIES AND SPECULATIONS

Daniel J. Kevles

CALIFORNIA INSTITUTE OF TECHNOLOGY · THE TRUTH SHALL MAKE YOU FREE ·

HUMANITIES WORKING PAPER 15

December 1978

According to recent scholarship in the early history of genetics, by the 1890s many younger biologists were growing restless with phylogenetic morphology and embryology, the traditional descriptive approaches to the much-debated problems of evolutionary theory. Eager to break away from these approaches, a number of these biologists -- and some older ones such as Alfred R. Wallace -- called for programs of experimental research in evolution addressed in particular to the problems of heredity and variation. "No problems in the whole range of biology," Charles O. Whitman of Woods Hole typically said, were of "higher scientific interest or deeper practical import to humanity." [1] In England Francis Galton inspired one of the more important experimental research programs -- W. F. R. Weldon's statistical analyses, developed in collaboration with Karl Pearson, of variations in large populations. Another important departure was the program of hybridization experiments exemplified in the research of William Bateson. Pearson and Weldon helped establish the field of heredity studies known as biometry. The research of Bateson and others paved the way for the rediscovery in 1900 and then vigorous advocacy of the Mendelian paradigm.[2]

Mendel's ideas did not gain rapid acceptance in all biological quarters in either the United States or Great Britain, In England, the biometricians Weldon and Pearson hotly disputed the validity of Mendel's results, the merits of his conceptual scheme, and even the integrity of his British advocates, especially Bateson. Bateson on his part decided to have as little as possible to do with the biometricians; he found it "impossible . . . to believe that they have made any honest attempt to face the facts" and doubted that they were "acting in good faith as genuine seekers for truth."[3] In the United States, while not a biometrician, Edwin Grant Conklin and Thomas Hunt Morgan found a number of compelling reasons, notably the equality of sex ratios and echoes of preformationism, to doubt the Mendelian theory. In England the angry dispute between the biometricians and the Mendelians is said to have diminished considerably after Weldon's death in 1906, and in America Morgan was converted to Mendelism after 1909/10, when he began his celebrated research with Drosophila. Further in the view of recent scholars, during the few years bracketing Morgan's conversion, the work of William Castle, H. Nilsson-Ehle, and Edward East, along with Morgan's, laid a solid experimental foundation for the reconciliation of Mendelism with Darwinian evolution, particularly by showing how Mendelism allowed for small heritable variations liable to selection. Eventually, through the work of Ronald A. Fisher, Sewall Wright, and J. B. S. Haldane, the Mendelian and biometrical paradigms were formally demonstrated to be mutually complementary, a reconciliation close to completion by 1930, when Fisher published The Genetical Theory of Natural Selection.[4]

Illuminating as the prevailing historiography is, with some exceptions it does not go much beyond a treatment of the principal actors and the principal conceptual developments of the field. It leaves unexplored, certainly for the United States and Great Britain, the history of the discipline defined as the history of the overall community of men and women -- the scientific commoners -- who came to practice genetics. As a result, important questions even in the history of genetic ideas remain unanswered. For example: What were the scientific backgrounds of the people who became geneticists in the United States and Britain? What were the intellectual routes in late nineteenth century biology by which they came to the study of heredity? How, if at all, did differences in intellectual journey affect the kind of genetics they did? Further, was Thomas Hunt Morgan typical of American biologists in his early skepticism about Mendelism? On the one hand, Bateson was celebrated when he visited the United States in 1904, but on the other W. J. Spillman, an agricultural experiment station agrostologist in Washington state, observed that year: " . . . a large proportion of our biologists and a good many of our practical plant breeders refuse to accept [Mendelism], and a good many of those who admit that it seems to be true ask in a sneering way how it is going to affect practical breeding operations."[5] Who were the early converts to Mendelism in the United States, and how is their conversion to be accounted for?

Considerably more attention to scientific commoners also seems called for in the celebrated dispute between the biometricians and Mendelians. Various historical analyses have yielded certain key points of interpretation: In the most general treatment of the subject, William Provine has argued that the issue turned on personal conflicts between the principals in the two camps. He has also concluded that this personal animosity delayed the reconciliation of Mendelism and biometry for a full fifteen years.[6] Without question public exchanges between the two sides were usually heated, and correspondence among the principals was replete with personal gibes. Weldon had made an enemy of Bateson with an unfavorable review of his monumental Materials for the Study of Variation in 1894. Bateson was not atypically intemperate in his Mendel's Principles of Heredity: A Defense. And Pearson, in pointing out errors, tended to use "the ugliest means possible," the American geneticist Herbert S. Jennings observed, and to hit offenders "over the head with a club."[7]

Without question, too, the conflict among the principals put scientists who attempted to reconcile biometry and Mendelism at a disadvantage. The most formal expression of biometry was the "law of ancestral heredity." Fathered by Galton and modified by Pearson, this "law" essentially apportioned various fractions of an organism's phenotypical expression to a distribution of ancestral influences. The suggestion as early as 1902 of the British statistician G. Udny Yule that Galton's law and Mendelism actually complemented rather than contradicted each other was ignored by Pearson.[8] A. D. Darbishire, a pupil of Weldon's at Oxford, after graduation commenced a course of research on inheritance in mice. As a result of this work, Darbishire, who began as an anti-Mendelian biometrician, gradually came to a qualified acceptance of Mendelism. The qualifications displeased Bateson and his allies, the Mendelism annoyed Pearson. "Darbishire," it has been observed, "in trying to be objective and circumspect, pleased no one."[9] Similarly, when Ronald A. Fisher submitted his groundbreaking paper reconciling biometry with Mendelism in 1918 to the Royal Society, Pearson and Bateson's disciple Reginald C. Punnett were called in as referees. Both recommended against publication in the Proceedings.[10]

Yet Garland Allen has observed that Provine's stress on personality tacitly assumes that Bateson, Pearson, and Weldon "were not representative of a broader and more profound set of traditions. The further suggestion that the controversy largely ended because Weldon died, does considerable injustice to any understanding of how historical change comes about."[11] Other students of the dispute have argued that profound scientific traditions did indeed lie behind the personal conflict. Philosophically, Bernard Norton has stressed, Pearson vigorously insisted upon avoiding the postulation of such unobservable entities as Mendelian factors. A positivist by conviction, Pearson preferred to describe, in a theory-free fashion, relations among phenotypic observables.[12] Adding fuel to the controversy was the difference between the two sides over how biology was to be done -- mathematically or traditionally.

While Pearson was a brilliant and Weldon an able mathematician, Bateson, who had suffered through mathematics while a Cambridge undergraduate, was mathematically inept. In 1902, though appreciating the value of Yule's idea on the basis of biological intuition, he was mathematically incapable of following it up. "Yule's paper," he noted, "is about the last word on the relation of M[endel] to G[alton], and in future [sic] G's law will cease to be treated as a <u>physiological</u> statement at all and [will] merely become a statistical summary of the expectation as to the composition of a promiscuously breeding population. I tried to do the analysis he has carried through, but it was beyond me."[13] Pearson was outspokenly intolerant of Bateson's inability -- Pearson would have said refusal -- to appreciate the techniques of correlation and regression. Still, many biologists agreed with the American biometrician and admirer of Pearson who remarked that r [illegible] for the most part Pearson had no first-hand knowledge of biological matters; he was "apt to take a rather absurd position sometimes in regard to biologically obvious things."[14]

In the vein of the social determination of scientific ideas, Donald MacKenzie has taken the lead in arguing that Bateson possessed a "conservative," in the sense of Karl Mannheim, intellectual temperament; he was predisposed to analyze the particular case -- the individual organism -- in contradistinction to Pearson, a socialist, who had a high propensity to see the world and nature in terms of collective populations. Bernard Norton has joined MacKenzie in arguing, similarly, that the commitment of Pearson and his allies to biometry was strongly reinforced, possibly even determined, by their embrace of eugenics; the line between eugenics and biometry, in their claim, ran through the eugenicist's eagerness to improve the quality of entire populations, no matter what the cost to individuals. In contrast, it was consistent for Bateson, the Mendelian, to be an anti-eugenicist; that position, like Mendelism, derived from a pro-individualist temperament.[15]

However suggestive, some post-Provine accounts of the dispute seem open to challenge. If Pearson knew no biology, his collaborator Weldon knew a great deal. In terms of intellectual temperament, Weldon was much closer to Bateson than to Pearson. Sharing Bateson's esthetic sensibility, Weldon traveled often to Italy, loved classical and Renaissance art, frequently attended the opera. While Pearson tended to draw social meaning from literature, Weldon was attentive to individual character and circumstance.[16] In social terms, while

Pearson was indeed an ardent socialist and eugenicist, Weldon was neither. He was also decidedly alive to the role of environment in biological development. Weldon aside, both in Britain and America, many eugenicists were Mendelians, and some biometricians were anti-eugenicists. One is hard put to find even a high correlation, let alone a necessary connection, between eugenic attitudes and position in the biometrician-Mendelian dispute.[17]

Whatever the flaws of these interpretations, one may note that they tend to complement rather than to contradict each other. One may also note that for the most part they have been applied to the British case and not to the American. Perhaps the reason is that Thomas Hunt Morgan, unlike Pearson, eventually embraced Mendelism. Nevertheless, it is curious that the opposition to Mendelism of Weldon or Pearson seems to require special analysis, but that of Thomas Hunt Morgan does not. If Morgan's skepticism is perfectly explicable on scientific grounds, perhaps Weldon's at least is, too. In fact, Weldon's extensive correspondence with Pearson in the Pearson Papers at University College London suggests that a great deal more can be said about Weldon's scientific objections to Mendelism. In 1901 Weldon, after subjecting Mendel's results to the chi-squared test, did not see that the results were "so good as to be suspicious," but he did have doubts about the seeming difficulty of reproducing Mendel's results with further pea experiments. Weldon concluded that Mendel was "either a black liar or a wonderful man." "If only," he remarked to Pearson, "one could know whether the whole thing is not a damned lie!"[18]

Most important, all these post-Provine accounts, like Provine's own, address only the principal actors -- Weldon, Pearson, and Bateson. It is difficult to understand how conflict among these three alone could have delayed the reconciliation of Mendelism and biometry for a full fifteen years. More generally, to account for the dispute mainly by scrutinizing the principal actors is to make a set of unexamined assumptions about the significance of a few key people in the course of scientific progress, about the role of those people in determining the research and outlook of their particular scientific community, and about the intellectual commitments of the community's scientific commoners. The relative importance of the various considerations advanced in these analyses remains unknown for the biometrical-Mendelian conflict among geneticists at large. Hence, before deciding to center an explanation on personal conflict, scientific tradition, philosophical preference, mathematical capacity, or intellectual temperament, one must first deal with certain essential questions. Did the angry sharpness of the dispute between Weldon and Pearson on the one side and Bateson on the other extend throughout the British genetics community? More important, did the dispute arise with similar acerbity in the United States?

The answer to the latter question seems to be: No. Between 1900 and 1915 about 25 percent of the 230 articles published in the *American Naturalist* concerned biometry, including statistical studies of variation. Of course, much of this research was of less than compelling importance. Pearson remarked that in certain quarters biometry was becoming "fashionable, and that to measure anything and throw a few figures together is considered biometrical research. In France, Italy, and America this type of biometry based on no adequate study of statistical mathematics is becoming unfortunately more and more common."[19] Nevertheless, the United States certainly had able biometricians, including Raymond Pearl, who for a time was a collaborator of Pearson's, J. Arthur Harris, and, at least early in his career, Charles B. Davenport, the noted eugenicist and director of the Carnegie Station for Experimental Evolution at Cold Spring Harbor. And the American biometricians did face professional difficulties. In 1903 Pearl spoke of the "prejudice in many quarters against biometrical work, and . . . the small chance a young man without reputation stands to get the claims of biometry recognized." Nevertheless, as Sewall Wright has recalled, while "Mendelism was, indeed, ridiculed by most biologists, including those especially interested in evolution, . . . no such debate ensued as in England." The Harvard geneticist W. E. Castle told a British colleague that he had "no doubt about the ultimate victory of the Mendelians, but we must not so worship our pet theory as to become its slave. The deplorable results of such a course are seen in what you term the 'Oxford opposition,'" meaning, of course, Weldon.[20]

Why was there no dispute in America with the acerbity to match that in England? One is tempted to say: Because, like the American Mendelians, the biometricians in the U. S. were not slaves of their paradigm either. At Cold Spring Harbor, Davenport aimed to foster "cooperation among all biometrical workers and students of heredity," which included Mendelians while he remained a biometrician (and biometricians even after he embraced Mendelism). American biometricians like Pearl came out of a biological tradition and were engaged in experimental research. Pearl typically told Herbert Jennings, who was experimenting with paramecium: "How do you reconcile the sort of facts which you are getting in P. and I am getting in egg production, and the plant breeders are getting with their selection work with the 'law of ancestral inheritance?'" Soon afterwards Pearl chided Pearson for refusing to accept the results of Wilhelm Johannsen's pure line experiments, which seemed to contradict Pearson's conviction that evolution must proceed by the selection of small variations. Pearl likened Pearson's denial of the existence of pure lines to someone's "attempting to defend the thesis that black is white. If you could see, as I have repeatedly seen, acres of ground covered with pure line pedigreed cultures showing all the characteristics which Johannsen describes for such pure lines, I am sure that you could not make such a statement as you do."[21]

But Weldon, Pearson's ally in biometry, was an experimental biologist too, and a first-rate one at that. Clearly the grounding in biological tradition of the American biometricians does not fully explain the lack of an acerbic dispute in the United States. Certainly it fails to account for the vitriol generated by the debate in England. The interpretive stress on personal conflict does seem to get at the bitter and impassioned quality of the dispute between Bateson on the one side and Weldon and Pearson on the other, but it can scarcely account for the division into hostile camps -- if such division existed -- of other British students of heredity. To pursue a more generally applicable explanatory hypothesis, viewing the Mendelian-biometrician battle against the background of the more common characteristics of British intellectual life, one thinks immediately of the relatively close-knit nature of British science and scholarship. More specifically, one thinks also of its relatively limited institutional base.

Compared to the American, the institutional base of British genetics was decidedly limited. The American authors cited in the bibliography of Thomas Hunt Morgan et al's Mechanism of Mendelian Heredity, published in 1915, were employed at one time or another in twenty-six different institutions, nine of which had two or more authors. For the period 1916-1930, the number of institutions represented by American authors in the journal Genetics totaled thirty-nine, with fifteen employing two or more such researchers and seven, four or more. These statistics may be matched against the number of institutions represented by British authors who published in the Journal of Genetics for the entire period 1910-1930. The total number of institutions was twenty, about half the American figure; the number with two or more authors only five, about a third of the comparable American figure. More significant, almost all the authors in Britain were concentrated at only two institutions, Cambridge and John Innes, while in the United States genetics authors were spread widely through the research system, save for slight concentrations at Cold Spring Harbor, Harvard and Columbia.[22]

These institutional characteristics suggest the following hypothesis: that the biometrical-Mendelian dispute never reached a level of vitriolic intensity in the United States because members of both camps operated in an environment of sufficient institutional opportunity to adopt a posture of intellectual tolerance towards advocates of the contrary paradigm. But in Britain the temperature of the dispute rose so high because the institutional situation encouraged members on both sides to see themselves in beleaguered positions. Both were seeking to establish a new field in an institutional environment so limited as to encourage the disputants to believe intellectual victory was required for satisfactory professional survival.

Lyndsay Farrall has noted that the principals in the dispute behaved as though they were in a state of siege, and so they did, even after Weldon's death. Bateson, often at odds with traditional biologists as well as with Pearson, denounced zoologists as "nincompoops" -- "their ignorance and bigotry is beyond belief" -- when a critically important paper on the inheritance of eye color by his collaborator C. C. Hurst was refused publication by the Royal Society. When in 1911 his colleagues Reginald C. Punnett and Rowland H. Biffen were denied election to the Society, Bateson pronounced the outcome "disgraceful" and took it as a setback for "all the Mendelian fraternity."[23] To Pearson, on his part, attacks on biometry by Bateson marked the "general tendency of the biologists here to discredit if possible the whole movement." Pearson intended to do nothing which would "give these folk an opening." Pearson believed that his papers failed to receive a fair evaluation from the referees for the Royal Society, especially Bateson. In 1901, together with Weldon and a small group of fellow guarantors, he launched Biometrika. While ostensibly prepared to publish Mendelian materials, Pearson generally ran the journal with an iron intellectual hand. In 1903 he went so far as to discourage Davenport, a member of his editorial board, from publishing an article favorable to the (pro-Mendelian) mutation theory.[24]

In 1910, shortly after Davenport and Raymond Pearl, also a Biometrika editor, published comments favorable to Johannsen's pure line theory, Pearson summarily removed them from the editorial board by heavy-handedly abolishing the board altogether. "It is a disadvantage to the Journal and the cause I have at heart," he explained to Pearl, "to be told that the subeditors of the Journal are opposed to the principles for which it was founded." Davenport he bluntly -- and wrongly -- accused of being "no longer in sympathy with biometric methods and results." Pearl, who regarded Pearson as a friend and mentor -- he had spent a year learning biometry in Pearson's laboratory -- was hurt and angry. He snapped to Pearson: You seem to want "no one associated with you in the editorship of Biometrika who does not think exactly as you do on the questions of theoretical biology." Herbert Jennings, Pearl's close friend, consolingly commented on Pearson's "incredible" behavior: "Whom the gods will destroy they first make mad."[25]

Not mad, but, in the case of Pearson as well as Weldon and Bateson, perhaps professionally frustrated. Between 1894 and 1906 none was situated in professional comfort. Weldon, appointed Linacre Professor of Zoology and a fellow of Merton College at Oxford in 1899, at first found the university "the place for a civilised man to live in, although men do talk about final causes after hall." Soon, however, he discovered that the museum men were "rank morphologists who prefer speculating about the pedigree of animals to any other more serious inquiry." The museum funds were all tied up and he was unable to persuade the fellows of Merton to use the surplus college income -- it amounted to 4,000 pounds per annum -- for academic purposes rather than to invest it in new estates. "We are therefore," Weldon lamented, "removing from our possible means of helping knowledge a sum equal to the whole government grant for scientific research every year." No less infuriating to Weldon was the unsympathetic attitude toward science which permeated the entire university. If a boy was found wanting in Greek, he was turned next to mathematics and, then, after learning "the anatomy of the frog, and a shoddy hypothesis about the pedigree of animals," as a last resort given a science scholarship. Weldon had failed to get "one man to care for any thing I say outside a textbook! Their tutors all tell them one is an amiable crank, useless for the Schools [examinations] except when one says certain definite things. . . ."[26]

At University College in Gower Street, Pearson was burdened by teaching -- at least sixteen hours a week of lectures -- and he also lacked financial support for research until grants from the Drapers Company started in 1903; even then he had to battle the University, which wanted to reduce his department's general budget support by an amount equal to half the Drapers grant. When Raymond Pearl spent 1905 at Pearson's shop, he found conditions "something of a disappointment," particularly in terms of inadequate laboratory space. "The great biometric laboratory of University College is all comprised in one room with two windows [and] with six or seven other people, one of whom is Dr. Alice Lee, whose most settled conviction is that the proper temperature of a room is not over 58°. I nearly freeze. . . ." Pearson was always at loggerheads with the university, too, over the purpose of the institution; while he wanted to transform the University of London into a genuine institution of research, the administration insisted that it remain fundamentally a teaching college. Pearson lamented to Weldon: "I wish we had both been born Germans; we should have established a new 'discipline' by now and have had a healthy supply of workers. . . ."[27]

Bateson, not so fortunate even as Weldon or Pearson, earned his livelihood as Steward of St. Johns' College, Cambridge. To finance his research, he had to make do with small grants from the Evolution Committee and the British Association for the Advancement of Science. He also drew heavily on his own pocket and on collaborators in research, many of them women, all of whom earned their living by other means. Bateson's collaborator Punnett recalled, likely with considerable exaggeration, that early in the century the leading journals refused to publish the contributions of Mendelians; they had to depend on the reports of the Evolution Committee and the Proceedings of the Cambridge Philosophical Society until Bateson established the Journal of Genetics in 1910.[28]

At the turn of the century Pearson, dissatisfied with his post at University College London, applied four times unsuccessfully for different professorships at Oxford and Edinburgh. "I fear . . ., you are the only part of the scientific public, which takes the least interest in my work," he told Galton plaintively. "The mathematicians look askance at anyone who goes off the regular track, and the biologists think I have no business meddling with such things." On his part, Weldon rued the day that he had left University College for Oxford, "the greatest danger to England" that he knew. "I hate it, and I hate myself because I have sold myself to it for money, instead of sticking to good old Gower Street, where there are live people who can be made keen." Like Pearson, Bateson time and again offered himself for professorial posts at Cambridge. Rejected even after his election to the Royal Society and receipt of the Darwin Medal, he lamented to a friend: "I have failed with my contemporaries; with posterity I hope to be more successful."[29]

Of course Bateson was soon successful with his contemporaries. Appointed to the new, five-year chair of genetics at Cambridge in 1908, he achieved personal and professional security when he was made the director of the new John Innes Institution in 1910. The professional situation eased somewhat for Pearson after 1911, when a bequest established an endowed professorship for him and support for the Biometric and Galton Laboratories. To the degree that the dispute between the leading biometricians and Mendelians diminished in intensity after 1906, the reason was probably not simply that Weldon died. Quite possibly it was also because two of the chief contestants acquired more secure institutional status in the tightly knit world of British science for themselves, their paradigms, their research, and their training of students.

The celebrated dispute aside, it seems generally agreed that by 1915 the center of Mendelian research was rapidly shifting from Britain to the United States. As early as 1907, British Mendelians were already avidly reading the American Naturalist and Science. And in 1921, when Bateson was elected a foreign associate of the National Academy of Sciences, he enthused to Pearl: "In our line American opinion is the best attainable, so I really for once feel like somebody!"[30] The shift to American supremacy occurred, it has been suggested, because of Bateson's disbelief in the central role in genetic transmission that the Morgan school assigned to chromosomes. Yet in 1922 Bateson, fresh from a visit to the states, told Hurst: "It seems practically essential that some try at the cytology should be made. . . . It does tell an amazing lot as to the significance of genetical problems."[31] Bateson did hire a cytologist at John Innes. Still, whatever his attitudes toward cytology and the chromosomal role in heredity, little can be said about the effect of his beliefs without knowing a good deal more about his influence after the very early years on the course of genetics research in Britain. A number of British geneticists were on the staff of the John Innes Institute, which Bateson directed from 1910 until his death in 1926. What effect did his antichromosomal views have on the Innes research program? And, going beyond the Innes, what role did they play in the governance of genetic research in Britain through such institutional mechanisms as journals, the Royal Society, the Genetical Society, and the universities? More generally, what were the attitudes of British geneticists at large towards the chromosomal theory of inheritance?

Any analysis of the relative vigor and quality of genetics research in Britain and the United States must take into account the institutional environment, or what Charles Rosenberg has called the "ecology" of the discipline. In Rosenberg's view, the swift rise to prominence of the American school of genetics was made possible by certain important institutional developments. One derived from the rapid enlargement after the 1890s of opportunities for research and graduate training in American universities. Between 1900 and 1915, doctorates awarded in America in botany and zoology more than doubled. Between 1915 and 1930 the prominent American geneticists William E. Castle and Edward M. East, both at the Bussey Institution of Harvard University, each trained twenty Ph.D.'s. At Columbia between 1910 and 1930, Morgan produced a comparable number, including of course the graduates of the famed Fly Room, C. B. Bridges, A. H. Sturtevant, and H. J. Muller. In contrast, the total of advanced students in British grant-aided universities during 1913/14 was 172.[32] Another institutional advantage of importance to American genetics, Rosenberg has stressed, was the natural interest in the discipline of farmers and breeders who, though not always sympathetic to basic genetic research, nevertheless generally supported its prosecution at the newly established agricultural experiment stations attached to the land grant colleges and universities. Bateson caught the significance of the gathering institutional power of American biology when early in the century, stimulated by the establishment of Davenport's richly funded installation at Cold Spring Harbor, he remarked: "We had read your vast programme of work with wonder and admiration. How any decent competition is to be kept up on our side I scarcely know!"[33]

Over the years the Cold Spring Harbor station actually produced less respectable genetics than it might have, but the agricultural experiment stations, especially after the passage of the Adams Act in 1906, provided an abundant source of fine genetics research.

The Maine Experiment Station took advantage of the funds made available by the Adams Act to hire Raymond Pearl, for the purpose of mounting a thorough investigation of inheritance in poultry, particularly with regard to Mendelian phenomena. Pearl was delighted with the salary, facilities, budget, assistance, and the degree, as he told Pearson, to which he would have a free hand. "I am under no restrictions as to giving the work a practical turn. On the contrary I am expected to work exactly as if I were taking up the study of heredity for my own purely scientific ends." Save for occasional lapses, the administrators of the station followed through on the promise. In 1909 Pearl mused how he was protected at the station "like a valuable piece of furniture." [34]

Experiment station scientists who had to concern themselves more than Pearl with practicality also contributed significantly to genetics. Particularly notable was the work of William J. Spillman, the plant agrostologist at Washington State College who in 1901, responding to the needs of local farmers, began to develop a variety of true winter wheat hardy enough for local conditions. Spillman knew nothing of Mendel, but he sensed that a useful route to follow might be hybridization. Smart and observant, Spillman recognized that the variations he observed in the F_2 generation were not fortuitous but the result of possible combinations of characters in the two hybrid parents. Calculating the percentages of plants displaying various characters, he found what he regarded as an astonishing regularity in these distributions over his different plats of wheat. Put on to Mendel's papers in 1902, Spillman realized that his results were entirely explicable on Mendelian theory. Upon reading Spillman's first wheat paper, published before he had yet heard of Mendel, C. C. Hurst exclaimed: "As I read the copious facts given in the tables, the paper is biologically of the greatest importance and in the large numbers with which it deals is in my opinion the most valuable confirmation of Mendel published since his day, indeed in some respects it gives more facts than did he."[35]

Yet if in the opportunities it gave to a Morgan, Pearl, or Spillman, the United States had a numerical institutional advantage over Britain, it did not have any advantage in institutional setting or type. Research was gaining a stronger foothold in British universities after the turn of the century. Moreover, in the 1890s, anticipating Davenport, Francis Galton had joined leading English biologists to advocate the establishment of an experimental farm -- it was to be located at Down, the Darwin family estate -- to study variation and heredity. The effort had failed, but by the early twentieth century the agricultural utility of genetics research stations was under discussion in Britain. In 1904 the Cambridge geneticist R. H. Biffen founded the Journal of Agricultural Science, declaring: "The problem of heredity is going to be of such importance to agriculture that we propose to lay ourselves out for publishing it."[36] The degree to which English breeders and horticulturalists were willing to lay themselves out is unclear. Older breeders of plants, horses, and pigeons seem to have been skeptical; nevertheless, as a member of the City Columbarian Society, a London group of pigeon fanciers, told Hurst: "It is the younger members who are gradually taking interest and disposed to breed on Mendelian lines."[37] In 1909 Hurst himself transformed his family nurseries at Burbage in Leicestershire into the

Burbage Experimental Station, which enjoyed considerable publicity when members of the British Association for the Advancement of Science, while meeting in nearby Birmingham in 1913, toured the facilities.[38] The British Army, alive to the uses of genetics, engaged Hurst and Cossar Ewart of Edinburgh in 1911 as scientific experts in a program for breeding superior hunter-type horses for the military. By 1914 the British government was supplying funds to agricultural experiment stations at various universities.

In any event, the American institutional advantage did not make necessarily for higher quality science. American physics operated in an institutional setting similar to that of genetics, yet American physics did not rank with British before World War I, when just two institutions, the Cavendish Laboratory and Manchester University, were enough to put Britain in the first rank. To account for the comparative quality of genetics in the United States and Great Britain, it would seem that the institutional situation must be considered in the total context of the discipline. Perhaps Mendelian genetics was somehow particularly well suited, in a way that physics was not, to the American scientific tradition or environment. Given the elitist nature of British academia, perhaps the loss in World War I of key younger geneticists was criticially important to the fate of the discipline in England. Perhaps genetics in Britain was also affected by the fact that there, much more than in the United States, a large proportion of the people working in the discipline seem to have been breeders and horticulturalists, like C. C. Hurst, rather than professional scientists in universities. In 1924 the British Genetical

Society had 108 members, 42 of whom were private individuals and plant or animal breeders.[39] Also, perhaps the fate of genetics in the two countries was determined less by the relative number of institutional opportunities and more by the way those institutional cpportunities were used for the training of students and the prosecution of research programs.

The use was undoubtedly a partial function of the external forces that shaped them. In both the United States and Britain, economic expectations helped create the institutional environment for genetics research -- at agricultural experiment stations and probably in university departments. Economic expectations aside, the pursuit of genetics was also, it seems, affected by social forces, notably the eugenics movement.

Expressing the aim of imposing through science a certain type of social control upon industrial society, the British and American eugenics movements included many geneticists before World War I and even some afterwards, when in the United States it turned increasingly conservative and racist. In both periods enthusiasts of eugenics seem to have supplied the institutional development of genetics research with certain benefits. At University College London Galton supported a Eugenics Record Office and a Research Fellowship in National Eugenics. Pearson, aching for funds, thought that "the dear old fellow could have spent his money better," but his turn came soon, since it was Galton's will that established his endowed professorship of eugenics.[40] While Pearson's Drapers Company grant was not given for eugenic purposes, it was used to aid statistical studies in heredity, and the Drapers managers regarded the results favorably enough to continue the grant for some thirty years. Eugenic interests also played a role in the establishment of the chair of genetics at Cambridge University. Arthur Balfour, a longtime advocate of strengthening research in British universities and a member of the Eugenics Education Society, persuaded an anonymous donor -- he was one William Watson -- to endow the chair with £20,000.[41]

In the United States, eugenic convictions helped energize the research and scientific entrepreneurial activities of Charles B. Davenport at Cold Spring Harbor. There, in 1910 and with a substantial grant of funds from Mrs. E. H. Harriman, Davenport founded a Eugenics Record Office, the American equivalent of the enterprise at University College. In 1921 the Office, further endowed by Mrs. Harriman, became part of the Carnegie Institution's Department of Genetics, which between 1921 and 1930 spent almost $1.3 million for genetic and eugenic research.

Institutional considerations aside, eugenics seems to have helped recruit young scientists into genetics proper. In 1911 a group of faculty and undergraduates at Cambridge University, including R. C. Punnett, L. Doncaster, and R. A. Fisher, formed the Cambridge University Eugenics Society. In America, some colleges established special courses in eugenics, while many more taught biology, sociology, and psychology with eugenically flavored textbooks. Scarcely a major American university faculty failed to include one or more professors of biology who, espousing the desirability of eugenic goals, no doubt inspired some of their students into careers in genetics research.[42] Recruitment aside, eugenics or proto-eugenic convictions likely drew the attention of important scientists to problems in heredity. It certainly did so for

Galton, Pearson, and R. A. Fisher, whose work in the reconciliation of biometry and Mendelism was from his undergraduate days fueled by his eugenic concerns.[43]

Despite the suggestiveness of the above examples, not a great deal can yet be said with confidence about the role -- or lack of it -- of eugenics or of economic expectations in the development of genetic research. We must therefore set down as still another question to be answered: How did social and economic forces actually affect in genetics the growth of its institutions, the recruitment of its practitioners, and the nature of the research they pursued? Indeed, before much more can be said with confidence about the early history of genetics generally, the scope of the subject must be broadened beyond its traditional definition as a body of knowledge largely produced by disembodied actors, extracted from their personal, professional, or institutional contexts and engaged for the most part in purely rational scientific debate. The subject has already been enriched by the scholarly arguments that philosophical, professional, or ideological predilections helped shape the scientific work of Galton, Pearson, Bateson, and Fisher. It would seem likely to benefit still more if its scope were enlarged to include its social, economic, and institutional dimensions. From that enlargement there is much to be learned, not only about the rise of the genetics communities but also about the as yet dimly perceived arena where the history of ideas and the history of institutions come together.

FOOTNOTES

1. Whitman is quoted in Charles B. Davenport, "A Summary of Progress in Experimental Evolution," p. 5, Charles B. Davenport Papers, American Philosophical Society Library, Philadelphia, Pa. See Alfred R. Wallace to Francis Galton, Feb. 3, 1891, Francis Galton Papers, University College London Archives, file 142/2B.

2. Useful introductions to the early history of genetics are: L. C. Dunn, A Short History of Genetics (New York: McGraw-Hill, 1965); A. H. Sturtevant, A History of Genetics (New York: Harper & Row, 1965); Garland E. Allen, Life Science in the Twentieth Century (New York: John Wiley, 1975). On Bateson, see Beatrice Bateson, William Bateson, F.R.S.: Naturalist, His Essays and Addresses, together with a Short Account of His Life (Cambridge: Cambridge University Press, 1928); A. G. Cock, "William Bateson, Mendelism, and Biometry," Journal of the History of Biology, 6 (Spring 1973), 1-36; Lindley Darden, "William Bateson and the Promise of Mendelism," Journal of the History of Biology, X (spring 1977), 87-106; on Weldon and Pearson, see Karl Pearson, "Walter Frank Raphael Weldon," Biometrika, 5 (1906), 1-52; Lyndsay A. Farrall, The Origins and Growth of the English Eugenics Movement, 1865-1925 (Ann Arbor: University Microfilms, 1970); Farrall, "W.F.R. Weldon, Biometry, and

Population Biology," unpublished manuscript; E. S. Pearson, Karl Pearson: An Appreciation of Some Aspects of His Life and Work (Cambridge: Cambridge University Press, 1938). On other aspects of the subject, see Garland E. Allen, "T. H. Morgan and the Emergence of a New American Biology," Quarterly Review of Biology, 44 (1969), 168-88; J. S. Wilkie, "Some Reasons for the Rediscovery and Appreciation of Mendel's Work in the First Years of the Present Century," British Journal for the History of Science, I (June 1962), 5-17; W. E. Castle, "The Beginnings of Mendelism in America," in L. C. Dunn, ed. Genetics in the Twentieth Century: Essays on the Progress of Genetics during its First 50 Years (New York: MacMillan, 1951); Alfred H. Sturtevant, "The Early Mendelians," Proceedings of the American Philosophical Society, 109 (August 1965), 199-204. See also the entries on Pearson, Bateson, Weldon, and Galton in the Dictionary of Scientific Biography.

3. Bateson to C. C. Hurst, March 24, 1903, C. C. Hurst Papers, Cambridge University Library, Cambridge, England, Add 7955/3/12.

4. For overall treatments of the biometrician-Mendelian controversy, see P. Froggart and N. C. Nevin, "The 'Law of Ancestral Heredity' and the Mendelian-Ancestrian Controversy in England, 1889-1906," Journal of Medical Genetics, 8 (1971), 1-36; William B. Provine, The Origin of Theoretical Population Genetics (Chicago: University of Chicago Press, 1971); Lyndsay Farrall, "Controversy and Conflict in Science: A Case Study -- The English Biometric School and Mendel's Laws," Social Studies of Science, 5 (1975), 269-301.

For Morgan, see Garland E. Allen's series of articles: "Thomas Hunt Morgan and the Problem of Sex Determination, 1903-1910," Proceedings of the American Philosophical Society, 110 (Feb. 1966), 48-57; "Thomas Hunt Morgan and the Problem of Natural Selection," Journal of the History of Biology, I (spring 1968), 113-39; "The Introduction of Drosophila into the Study of Heredity and Evolution, 1900-1910," Isis, 66 (1975), 322-33. Sure to become the standard biography of Morgan is Allen's Thomas Hunt Morgan: The Man and His Science (Princeton: Princeton University Press, 1978), which includes an extensive bibliographical essay. Other important studies include Elof Axel Carlson, "The Drosophila Group: The Transition from the Mendelian Unit to the Individual Gene," Journal of the History of Biology, 7 (spring 1974), 31-48; Nils Roll-Hansen, "Drosophila Genetics: A Reductionist Research Program," Journal of the History of Biology, 11 (Spring 1978), 159-210.

5. Spillman to Hurst, Jan. 21, 1904, Hurst Papers, Add 7955/4/53.

6. Provine, Origins of Theoretical Population Genetics, pp. 63-64.

7. Jennings to Raymond Pearl, Nov. 15, 1909, Raymond Pearl Papers, American Philosophical Society Library, Jennings file. G. Udny Yule, a friend of Pearson but an appreciator of Mendelism, said of Bateson's Defense that its preface was "turgid and bombastic," its treatment of Weldon "grossly and gratuitously offensive," and its style generally that of the "religious revivalist." Yule, "Mendel's Laws and their Probable Relations to Intra-Racial Heredity,"

New Phytologist, I (1902), 194. See also Farrall, "Controversy and Conflict," pp. 274-76; Froggart and Nevin, "The 'Law of Ancestral Heredity,'" n. 279.

8. Yule, "Mendel's Laws," New Phytologist, I (1902), 193-207, 222-238. In this paper Yule hit upon a very restricted form of the later Hardy-Weinberg law. When in 1908 he learned of Hardy's general expression for the stability of a randomly breeding Mendelian population, he exclaimed: "Absolutely correct & solves the whole difficulty! I am kicking myself for never having seen it. The fact is that in my New Phytologist articles of 1902 I took the population as starting from a single D x R cross, which gives the 1:2:1 ratio as stable amongst the descendants, and it never occurred to me that any other ratio could be stable. It's extraordinary how stupid one can be. Pearson also follows in the same track in his Phil. Trans. paper 'On a generalized theory of alternative inheritance' & so he also gets the 1:2:1 ratio." Quoted in R. C. Punnett to Bateson, April 16, 1908, William Bateson Papers, John Innes Horticultural Institute Archives, Box H. See Pearson, "On a Generalized Theory of Alternative Inheritance with Special Reference to Mendel's Laws," Philosophical Transactions, 203A (1904), 53-86.

9. Froggart and Nevin, "The 'Law of Ancestral Heredity,'" n. 309.

10. Bernard Norton, "A Note on the Background to, and Refereeing of

of R. A. Fisher's 1918 Paper . . . ," Notes and Records of the Royal Society of London, 31 (July 1976), 151-62.

11. Allen, "Genetics, Eugenics, and Society: Internalists and Externalists in Contemporary History of Science," Social Studies of Science, 6 (1976), 111

12. Norton, "Metaphysics and Population Genetics: Karl Pearson and the Background to Fisher's Multi-factorial Theory of Inheritance," Annals of Science, 32 (1975), 537-53; "Biology and Philosophy: The Methodological Foundations of Biometry," Journal of the History of Biology, VIII (spring 1975), 85-93. See also Bernard Norton, "Karl Pearson and the Galtonian Tradition: Studies in the Rise of Quantitative Social Biology " (University College London, Ph.D. thesis, History of Science, 1978), pp. 79-81, 185.

13. Bateson to Hurst, Feb. 8, 1903, Hurst Papers, Add 7955/3/7.

14. Pearl to Herbert S. Jennings, Oct. 8, 1905, Pearl Papers, Jennings file. Pearl also remarked: "I think the impression among biologists that biometry is proving rather a sterile field, comes from the unfortunate fact that (1) Pearson, who has grounded the subject mathematically, hasn't at all the biologist's point of view, and (2) that most of the biologists who have taken it up haven't gone to the trouble (or have been constitutionally unable)to work up thoroughly the mathematical side, so to know how to handle their

data in an adequate way." Pearl to Jennings, Feb. 4, 1906, Pearl Papers, Jennings file.

15. Donald A. MacKenzie and S. B. Barnes, "Biometrician and Mendelian: A Controversy and Its Explanation," University of Edinburgh, Science Studies Unit Paper, September 1974; MacKenzie, "Sociobiologists in Competition: The Biometrician-Mendelian Debate," in The Roots of Sociobiology (London: The Past and Present Society, 1978); MacKenzie, "The History of Statistics in Britain: A Social Interpretation" (Ph.D. thesis, Science Studies Unit, University of Edinburgh, 1978), pp. 13-30, 124-98, 246-308; Norton, "The Biometric Defense of Darwinism," Journal of the History of Biology, 6 (fall 1973), 283-316; Norton, "The Galtonian Tradition," pp. 118-20, 138-9.

16. On Pearson and literature, see Norton, "The Galtonian Tradition," pp. 141-42.

17. Norton raises some of these objections, ibid., pp. 181, 186, 188.

18. Ibid., p. 175; Farrall, "Controversy and Conflict," pp. 292, 295; Weldon to Pearson, [Nov. 1901], Karl Pearson Papers, University College London, file 625.

19. The figures are drawn from my own preliminary statistical analysis; Pearson to the President of the Carnegie Institution of Washington, Oct. 28, 1905, Pearl Papers, Pearson file.

20. Pearl to Davenport, Oct. 5, 1903, Davenport Papers, Pearl file; Wright, "Comparison of the Impact of Mendelism on Evolutionary Thought in England and America," unpublished paper, copy in possession of Bernard Norton; Castle to Hurst, Jan. 18, 1904, Hurst Papers, Add 7955/4/47.

21. Davenport to Pearl, Jan. 15, 1904, Pearl Papers, Davenport file; Pearl to Jennings, Feb. 17, 1909, Pearl Papers, Jennings file; Pearl to Pearson, March 12, 1910, Pearson Papers, Pearl file.

22. The figures are drawn from my own preliminary statistical analysis.

23. Farrall, "Controversy and Conflict," p. 292, n. 93; Bateson to Hurst, June 30, 1907, June 18, 1907, June 27, 1907, March 3, 1911, Hurst Papers, Add 7955/7/ 31;28;29, 7955/11/2.

24. Pearson to Davenport, Jan. 27, 1902, May 23, 1903, Davenport Papers, Pearson file.

25. Pearson to Pearl, Jan. 27, 1910, Pearl Papers, Pearson file; Pearl to Pearson, Feb. 15, 1910, Pearson Papers, Pearl file; Jennings to Pearl, March 5, 1910, Pearl Papers, Jennings file. See also Pearson to Davenport, Jan. 27, 1910, Davenport to Pearson, Feb. 5, 1910, Davenport Papers, Pearson file.

26. Weldon to Pearson, April 12, 1899; May 19, 1899, May 21, 1902, July 11, 1900, Pearson Papers, Weldon file.

27. Weldon to Pearson, July 31, 1903, Pearson Papers, Weldon file; Froggart and Nevin, "The 'Law of Ancestral Heredity,'" p. 12; Pearl to Jennings, Oct. 8, 1905, Pearl Papers, Jennings file; Pearson to Weldon, Oct. 9, 1900, Pearson Papers, file 263.

28. R. C. Punnett, "Earl Days of Genetics," Heredity, 4 (April 1950), 1-10.

29. Pearson to Yule, Oct. 1901, Pearson Papers, Yule file; Pearson to Galton, Jan. 11, 1898, Galton Papers, file 293C (I am indebted to Bernard Norton for this document.); Weldon to Pearson, Oct. 21, 1904, Pearson Papers, Weldon file; Bateson is quoted in Beatrice Bateson, Bateson, p. 93.

30. Bateson to Pearl, n.d., Pearl Papers, Bateson file.

31. Allen, Life Science in the Twentieth Century, p. 57; Bateson to Hurst, Feb. 12, 1922, Hurst Papers, Add 7955/14/8.

32. Rosenberg, "The Social Environment of Scientific Innovation: Factors in the Development of Genetics in the United States," in Charles E. Rosenberg, No Other Gods (Johns Hopkins University Press, 1976), 196-209; Hamilton Cravens, "The Role of Universities in the Rise of American Science, 1890-1930," manuscript version, pp. 31, 33, and n. 50. See his shorter "The Role of Universities in the Rise of Experimental Biology," The Science Teacher, 44 (Jan.

1977); D. S. L. Cardwell, The Organisation of Science in England (rev. ed.; London: Heinemann, 1972), p. 215. On a special aspect of the institutional development of science in Britain, see Gerald L. Geison, Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society (Princeton: Princeton University Press, 1978).

33. Bateson to Davenport, April 25, 1906, Davenport Papers, Bateson file.

34. Pearl to Pearson, Dec. 9, 1906, Pearson Papers, Pearl file; Pearl to Jennings, Oct. 26, 1909, Pearl Papers, Jennings file.

35. Spillman to Hurst, April 16, 1903; Jan. 21, 1904; Hurst to Wilkes, Jan. 21, 1903, Hurst Papers, Add 7955/3/78-81 and 7955/3/54.

36. Biffen to Hurst, Oct. 26, 1904, Hurst Papers, Add 7955/5/58; Christabel S. Orwin and Edith Whetham, History of British Agriculture, 1846-1914 (London: Longmans, 1964), pp. 373-86.

37. C. R. G. Sanfort to Hurst, n.d. [1908], Hurst Papers, Add 7955/8/36.

38. British Association for the Advancement of Science, "Birmingham Meeting 1913, Visit to Burbage, Sept. 16, 1913," pamphlet in Hurst Papers.

39. D. Lewis, "The Genetical Society -- The First Fifty Years," in J. Jinks, ed., Fifty Years of Genetics: Proceedings of a Symposium . . . of the Genetical Society on the 50th Anniversary of its Foundation (Edinburgh: Oliver and Boyd, 1969), p. 1.

40. Pearson to Yule, Oct. 30, 1904, Pearson Papers, Cabinet VI, drawer 5.

41. Farrall, The English Eugenics Movement, pp. 110-11, 126, 130-32; Viscount Esher to the Vice-Chancellor, March 7, 1912, Balfour Professorship of Genetics Guard Book, C. U. R. 39.47, Cambridge University Library; Esher to Balfour, Feb. 15, 1912, Arthur Balfour Papers, British Library, Add 49719, item 214.

42. Kenneth Ludmerer, Genetics and American Society (Johns Hopkins University Press, 1972), p. 82; Geoffrey R. Searle, Eugenics and Politics in Britain, 1900-1914 (Leyden: Noordhoof International Publishing, 1976), pp. 13-14, 34-35.

43. Important work stressing the role of social propensities in the history of genetics and biometry include: Ruth Schwartz Cowan, "Francis Galton's Statistical Ideas: the Influence of Eugenics," Isis, 63 (1972), 509-28; Cowan, "Francis Galton's Contribution to Genetics," Journal of the History of Biology, 5 (fall 1972), 389-412; MacKenzie, "Statistical Theory and Social Interests: A Case

Study"; Norton, "Karl Pearson and Statistics: The Social Origins of Scientific Innovation," both in Social Studies of Science, 8 (Feb. 1978), 35-84, 3-34; William Coleman, "Bateson and Chromosomes: Conservative Thought in Science," Centaurus, 15 (1970), 228-314. See also Norton, "The Galtonian Tradition"; MacKenzie, Statistical Theory in Britain"; Allen, "Genetics, Eugenics, and Society: Internalists and Externalists in Contemporary History of Science."