

Editor's Introduction

Genetics, Eugenics and Evolution

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On 7 May 1988 a commemorative conference for Bernard Norton was held at the University of Leicester, sponsored by the British Society for History of Science and organized by W. H. Brock and myself. The papers commissioned for that conference have been rewritten for publication in this special issue of the *British Journal for the History of Science*. The historians of biology and medicine whose work appears here share a respect for Bernard Norton's work, and many of us were fortunate to have known him personally as well. His unexpected death in 1984, therefore, provoked a sense of loss, both personal and professional.¹ With this volume we pay tribute to him.

Those familiar with Norton's work will recognize that 'Genetics, Eugenics and Evolution' does not quite do justice to the breadth of his range. For his interest in the Galtonian tradition required him to look in detail at early attempts to measure that 'trait' so esteemed by (Anglo-American) eugenics movements: intelligence. In one of our last conversations, as I recall, he expressed particular interest in the work of the American IQ-tester, Lewis Terman, and at the time of his death he was working on a book entitled *Biology and Social Class*. For pragmatic reasons, however, it proved impossible to incorporate this psychometric stand into the current volume. Nevertheless, the majority of Norton's published work illustrates the manifold connections between biometrical genetics, eugenics and evolutionary theory, one of his last papers showing how all three were intertwined in the work of R. A. Fisher.²

FROM DARWIN TO THE EVOLUTIONARY SYNTHESIS

Will future historians of biology regard Mayr and Provine's *The Evolutionary Synthesis*³ as a historiographical turning point? From our vantage point in 1989, it certainly seems so. For one thing, the book made plain the alarming gaps in our knowledge of the

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1 W.H. Brock, 'Bernard John Norton: 1945–1984', *British Journal for the History of Science* (1985) 18, pp. 342–344.

2 Bernard Norton, 'Fisher's Entrance into Evolutionary Science: the Role of Eugenics' in M. Grene (ed), *Dimensions of Darwinism*, Cambridge, 1983, pp. 19–29.

3 Ernst Mayr and William Provine (eds), *The Evolutionary Synthesis: Perspectives on the Unification of Biology*, Cambridge Mass, 1980.

development of evolutionary theory since Darwin. Subsequent books such as Peter Bowler's *The Eclipse of Darwinism* or the collection edited by Marjorie Grene, *Dimensions of Darwinism*, have begun to alleviate that ignorance.⁴ At the same time, however, they have borne out the most important point of Mayr and Provine's volume: that the path from Darwin to the modern synthesis was anything but linear and progressive. Instead the credibility of natural selection declined between the late nineteenth century and the First World War; even the 'synthesisers' of the 1930s and 40s did not regard selection as the sole sufficient mechanism; and some biological communities outside the English-speaking world (e.g. in Germany) remain to this day cautious about selection. Finally, when *The Evolutionary Synthesis* appeared, it was generally accepted that the divergence of natural historical and experimental traditions within the life sciences had accelerated in the late nineteenth century with the 'revolt from morphology'.⁵ But in sketching the highly uneven distribution of Darwinian and Mendelian concepts among different specialities, Mayr and Provine's authors reminded us just how fragmented the biological community had become by the 1920s. It is thus with good reason that we refer to 'the modern *synthesis*' and all the more challenging for us to explain how, under such circumstances, a synthesis was possible at all.

The contributors to Part I of this issue all address one or other facet of this historiographical debate. The opening paper by *Jonathan Hodge* articulates a historiographical shift which is evident in recent work on the nineteenth century. Tidy distinctions between natural historical and physiological traditions in nineteenth-century biology, Hodge argues, will not do. From Darwin to Dobzhansky, evolutionary thinkers have often devoted a great deal of attention, not only to heredity and variation, but also to the problems of vitality and mortality. Thus Hodge and Phillip Sloan have made a case for the young Darwin as a 'generation-theorist'. At the same time students of nineteenth-century hereditary thought have argued that Mendel's work was less an attempt to outline a theory of inheritance than an investigation of theories of descent by hybridization.⁶ Hodge applies the same perspective to August Weismann, a figure often cited for his prescience in anticipating modern ideas of heredity and evolution, demonstrating the continuity of his generation-theoretic concerns (although not his solutions) with Darwin's. While not denying the fragmentation of biological theory in the twentieth century, nonetheless, Hodge finds the perpetuation of Darwin's and Weismann's broad synthetic ambitions in H. J. Muller and Sewall Wright. Recent work suggests, furthermore, that although Muller and Wright may have been rather isolated figures in the American genetics community, related 'synthetic traditions' continued to flourish elsewhere.⁷ Although aimed primarily at historians of nineteenth-century evolutionary theory, there-

4 P. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900*, Baltimore and London, 1983; M. Grene (ed) op. cit. (2).

5 Garland Allen, *Life Science in the 20th Century*, New York, 1975; 'The New Experimental Biology', special issue of *Journal of the History of Biology* (1981), 14, no. 1.

6 Robert Olby, *Origins of Mendelism*, 2nd edn, Chicago, 1985.

7 Jonathan Harwood, 'National Styles in Science: Genetics in Germany and the United States between the World Wars', *Isis* (1987), 78, pp. 390–414; Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics*, New York and Oxford, 1987.

fore, perhaps Hodge's historiographical suggestion also merits the attention of historians of genetics.

Much of the work so far on the 'revolt from morphology' has concentrated on the rebels: the younger generation of experimental embryologists who rejected the phylogenetic aims and descriptive methods of their teachers. *Peter Bowler's* essay in this collection, in contrast, focuses upon the old guard, showing the diverse ways in which they responded to the evident difficulties confronting evolutionary morphology. All of them were more interested in constructing phylogenies than in establishing evolutionary mechanisms, and the extent to which they accepted Darwinian principles varied greatly. Even E. Ray Lankester, arguably the most 'Darwinian' of the group, retained elements of Haeckel's recapitulationism, envisioning a phylogenetic mainstem which displayed progression from simple to complex, and whose sidebranches arose through adaptation. The failure to address the question of evolutionary mechanism in detail, however, enabled stubborn defenders of the Haeckelian tradition, like E. W. MacBride, to interpret Lankester's work in neo-Lamarckian terms. But other evolutionary morphologists were quicker to abandon ship; F. M. Balfour, Adam Sedgwick and William Bateson became critical of the recapitulationist assumptions underlying their early work, although only Bateson succeeded in making the transition to the new experimental study of variation.

As the literature on the biometrician–Mendelian debate in Britain has demonstrated, the experimental and statistical analysis of variation at this time was simultaneously the study of evolutionary mechanism. Just *why* Bateson chose discontinuous models of heredity and evolution while the biometricians opted for continuous variation and selection, however, was the subject of debate during the 1970s. In his contribution to this volume, *Robert Olby* reopens the historiographical discussion with a critique of Donald MacKenzie and Barry Barnes' sociological interpretation of the biometrician–Mendelian controversy. While not wishing to ban sociological explanation *per se*, Olby proposes a shift of emphasis in several respects. The key biometrician, he argues, was not Karl Pearson, as in MacKenzie and Barnes' analysis, but W. F. R. Weldon. Searching for relevant differences between Bateson and Weldon which could account for the controversy, Olby is struck less by class differences than by those of professional skill, location within the zoological community, and external contacts (e.g. with groups of practical men in fishing *versus* horticulture). Lastly Olby points out a methodological tension in MacKenzie and Barnes' account which is common to many social structural explanations: that they rely heavily upon a small number of individual scientists as exemplars of particular groups and yet are unable to rule out psychological explanations for these individuals' thought-patterns. Attempting to exploit this tension, Olby claims Weldon as an important anomaly for the MacKenzie–Barnes thesis and argues that what appear to be class-based differences in outlook between Weldon and Bateson may actually be psychological in origin.

GENETICISTS AND THE EUGENICS MOVEMENT

Over the last two decades our understanding of the eugenic movements in various countries has steadily grown. In the wake of the predominantly American literature of the

1960s and early 70s, the focus shifted to the British movement where increasing consideration was given to eugenics as an ideology peculiar to the professional middle-class.⁸ Gradually, too, historians broadened their conception of the movement's impact; no longer was eugenics seen primarily as a movement whose distortion of science brought us compulsory sterilization and ultimately genocide. Instead Bernard Norton and others showed how eugenic motives had prompted fruitful innovations in statistics and population genetics.⁹

In the 1980s interest has shifted once again, this time to the German racial hygiene movement.¹⁰ This work is most welcome, not only because it illuminates the origins of the Holocaust, but also because it offers valuable perspective on the British and American movements. As *Paul Weindling's* paper in this volume demonstrates, much of the interest in German racial hygiene has been directed at the question of continuity: to what extent was Nazi eugenic policy prefigured by developments in the racial hygiene movement prior to 1933? While consensus has not been reached on this issue, Weindling argues that it is wrong to treat the German movement essentially as a perpetuation of late nineteenth-century racist anti-modernism, leading directly to National Socialism. This neglects both the political heterogeneity of the movement prior to 1933—which attracted support even from social democrats and communists¹¹—and substantial shifts in the movement's direction, triggered by political dislocations between 1890 and 1945. Without denying certain distinctively German features of the race hygiene programme, Weindling shows how it was perceived as relevant to social policy and public health during the Weimar Republic. If, therefore, 1933 constituted a watershed for German eugenics, it will be important in future to conduct comparisons of the German, British and American movements prior to 1933. The exercise may well be unsettling for those who have heretofore assumed that 'it couldn't happen here': the policies of all three movements were similar, and in terms of legislative impact the German movement was probably the weakest. At the Nuremberg War Crimes Trials, Allied lawyers succeeded in

8 On the United States see Mark Haller, *Eugenics: Hereditarian Attitudes in American Thought*, New Brunswick, New Jersey, 1963; Donald Pickens, *Eugenics and the Progressives*, Nashville, 1968; and Kenneth Ludmerer, *Genetics and American Society*, Baltimore, 1972. On Britain see Lyndsay Farrall, *The Origins and Growth of the English Eugenics Movement, 1865–1925*, Ph.D. thesis Indiana University, 1970; G.R. Searle, *Eugenics and Politics in Britain, 1900–1914*, Leyden, 1976; and Donald MacKenzie, *Statistics in Britain, 1865–1930*, Edinburgh 1981. For a comparative analysis of movements in both countries, see Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity*, Harmondsworth, 1985.

9 Ruth Schwartz Cowan, 'Francis Galton's Statistical Ideas: the Influence of Eugenics', *Isis* (1972), 63, pp. 509–528; Norton, op. cit. (2) and 'Karl Pearson and Statistics: the Social Origins of Scientific Innovation', *Social Studies of Science* (1978); 8, pp. 3–34; Donald MacKenzie, 'Statistical Theory and Social Interests: a Case Study', *Social Studies of Science* (1978), 8, pp. 35–83.

10 Benno Müller-Hill, *Murderous Science*, Oxford, 1988; Sheila Weiss, *Race Hygiene and National Efficiency: the Eugenics of Wilhelm Schallmayer*, Berkeley, 1987; Robert N. Proctor, *Racial Hygiene: Medicine under the Nazis*, Cambridge Mass., 1988. These books also provide a guide to the extensive German literature on race hygiene.

11 For example, Georg Lilienthal, "Rheinlandbastarde", Rassenhygiene und das Problem der rassenideologischen Kontinuität', *Medizinhistorisches Journal* (1980), 15, pp. 426–436; P. Weindling, 'Die Verbreitung rassenhygienischen/eugenischen Gedankengutes in bürgerlichen und sozialistischen Kreisen in der Weimarer Republik', *Medizinhistorisches Journal* (1987), 22, pp. 352–368.

prosecuting German physicians for their role in the 'euthanasia' programme and experiments on concentration camp inmates, but no case was filed against those involved in the compulsory sterilizations of 1933 to 1939, allegedly because these were also permitted by law in most American states.¹²

As other countries' eugenic movements come under scrutiny, among the most interesting will be those in Scandinavia. As *Nils Roll-Hansen's* paper in this volume indicates, the Scandinavian movements are yet another reminder that German eugenics was not unique in several respects. Nordic racism, for example, was also endorsed by several Scandinavian eugenicists and most of the Scandinavian countries passed (voluntary) sterilization laws even after 1933.

From the beginning historians have been aware, not only of diversity within eugenics movements, but also of a trend toward moderation in their policies which had become evident by the early 1930s.¹³ More recently Daniel Kevles has devoted particular attention to this trend, and his distinction between 'mainline' and 'reform' eugenic factions has been adopted by others (e.g. the contributors to Part II of this issue).¹⁴ Although the programmes of mainline and reform eugenicists probably differed to some extent from one country to another,¹⁵ nevertheless, there is some agreement on the issues which divided these two camps. 'Mainliners' tended to advocate compulsory sterilization and/or segregation while reformists supported voluntary sterilization;¹⁶ mainliners were more concerned with race differences than were reformists;¹⁷ reform eugenics drew attention to the effects of environment upon eugenically important phenotypes;¹⁸ and in general reformists criticized the mainline tradition for neglecting the complexity of genes' effects upon phenotypes.¹⁹

Just why this shift should have occurred, however, is currently the subject of debate. At issue are the roles played by scientific advances and ideological change in undermining

12 Proctor, *op. cit.* (10), p. 117.

13 Haller, for example, distinguishes between the racist 'pessimistic' faction which dominated American eugenics during the 1920s and a 'moderate' minority which came to prevail during the 1930s (*op. cit.* 8, chapter XI and pp. 122, 179–183). Although G.R. Searle does not define the term, he refers to a small minority of 'reform eugenicists' in Britain before 1914 (*op. cit.* 8, p. 27 and *passim.*) which extended its influence during the 1930s (Searle 'Eugenics and Politics in Britain in the 1930s', *Annals of Science* (1979), 36, pp. 159–169). Garland Allen argues that a more moderate and scientifically defensible 'new' eugenics began to displace the 'old' eugenics in the United States from the mid-1920s (*viz* 'The Misuse of Biological Hierarchies: the American Eugenics Movement, 1900–1940', *History and Philosophy of Life Sciences* (1983), 5, pp. 105–128; and 'The Role of Experts in Scientific Controversy' in H. Tristram Engelhardt and Arthur L. Caplan (eds), *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology*, Cambridge, 1987, pp. 169–202.

14 Kevles, *op. cit.* (8), chapter XI and *passim.*

15 For example, while Roll-Hansen (this issue) notes that Scandinavian reformists were sceptical of positive eugenics, Kevles (*op. cit.* 8, pp. 178ff) suggests that British and American reformists often advocated positive eugenics. Similarly, according to Searle (*op. cit.* 8, p. 44), race differences were less central to the British mainline tradition than, for example, in the United States.

16 Roll-Hansen, this issue; Kevles, *op. cit.* (8), p. 167.

17 Roll-Hansen, this volume; Allen (1987) *op. cit.* (13), pp. 172–173; Kevles, *op. cit.* (8), pp. 106, 127, 174.

18 Roll-Hansen, this volume; Allen (1987) *op. cit.* (13); Kevles, *op. cit.* (8), chapters IX and X.

19 For example, Allen (1987) *op. cit.* (13), pp. 172–173.

the credibility of mainline policies. Since these policies claimed the authority of science, *geneticists'* opinions of mainline eugenics are of particular interest. Kenneth Ludmerer was the first historian to address this issue systematically.²⁰ Examining the attitudes of American geneticists towards eugenics between 1905 and 1935, Ludmerer distinguished three phases. Up to 1915 geneticists were generally enthusiastic about eugenics. From 1915 to 1923, however, they expressed concern in private that the movement's policies had not kept pace with various developments in genetics. From 1924, dismayed by the use of eugenists' testimony in securing passage of the racist Immigration Restriction Act, geneticists began to voice their criticisms in public. And after 1933, 'fearing another Germany, many geneticists . . . completed their renunciation of the eugenics movement.'²¹

While Ludmerer's thesis has found some support, it undoubtedly requires modification in a number of respects. First, as the mainline/reform distinction makes clear, most of the geneticists who spoke out critically from the mid-1920s did not abandon eugenics altogether but merely advocated a more moderate reform eugenics. Secondly, even Nazi racism did not frighten these geneticists away from reformist policies. Contrary to Ludmerer's claim, the 'Geneticists Manifesto' signed at the Seventh International Congress of Genetics at Edinburgh in 1939 did not 'renounce' eugenics; it attacked Nazi racial hygiene while endorsing a version of reform eugenics.²² Nor did post-war revelations of the horrors of the concentration camps persuade reformists that eugenics was fundamentally tainted. Although Kevles writes that eugenics had become a 'dirty word' in the United States after 1945, Diane Paul argues that reformist geneticists in both the United States and Britain continued to make a case for eugenics into the 1960s, as did Hans Nachtshiem in Germany.²³ Thirdly, and most importantly, the extent to which new developments in genetics forced geneticists to modify their views of eugenics is in dispute, as is evident from the papers in this volume by Nils Roll-Hansen and *David Barker*. One of the most striking anomalies for the Ludmerer thesis, as Barker shows, is that even into the 1930s, many eminent British and American geneticists continued to support H. H. Goddard's eugenically influential theory of feeble-mindedness, despite the fact that the theory violated various rules of standard genetic analysis. Furthermore, if the growth of genetic knowledge is supposed to have prompted a rethink, it is difficult to see why geneticists in Germany should have responded so differently from their colleagues elsewhere; I am unable to find a single German geneticist during the interwar period who voiced any reservations—either in public or in private—about the policies of the German racial hygiene movement. Finally, at the most general level, some of the evidence used by historians endorsing a version of the Ludmerer thesis is simply inconclusive. When a

20 Ludmerer, 'American Geneticists and the Eugenics Movement, 1905–1935', *Journal of the History of Biology* (1969), 2, pp. 337–362.

21 Ludmerer, *op. cit.* (20), p. 356.

22 Compare Ludmerer's claim in *op. cit.* (20), p. 358 with 'Men and Mice at Edinburgh', *Journal of Heredity* (1939), 30, pp. 371–373.

23 Kevles, *op. cit.* (8), p. 251; Diane Paul, 'Eugenics and the Left', *Journal of History of Ideas* (1984), 45, pp. 567–590; on Nachtshiem see the paper by Weindling in this volume and F. Vogel, 'Hans Nachtshiem, 1890–1979', *Berichte und Mitteilungen der Max-Planck-Gesellschaft* (1980), Heft 3, pp. 26–29.

geneticist cites scientific arguments in criticizing the eugenics movement, we are not entitled to infer that those arguments were in fact a *cause* of the criticism. That is to forget that reasons are often given to rationalize a decision, not to explain it.²⁴

If Ludmerer's internalist account is correct, one might expect the growth of knowledge to induce individual geneticists to change their minds about particular eugenic policies. The number of demonstrable 'converts', however, is remarkably small. Ludmerer was able to cite only T. H. Morgan, Raymond Pearl and A. F. Blakeslee as having become more critical of eugenics after 1915.²⁵ Of fifteen geneticists in David Barker's study who at one time endorsed Goddard's theory, only J. B. S. Haldane, F. A. E. Crew and R. A. Fisher had changed their views of the theory by the mid 1930s. Just as commonly, one finds geneticists who were mainliners or reformists from the start and did not change their minds. As Roll-Hansen's paper in this volume shows, Otto Lous Mohr and Wilhelm Johannsen were already critical of mainline eugenics by 1915 while Harry Federley and Hermann Nilsson-Ehle remained tolerant of mainline policies as late as 1930. It might well be more productive to conceptualize the transition from mainline to reform eugenics, not as a 'change of individual minds', but as a shift in the influence or visibility of two groups whose adherence to the mainline or reform programme was fixed by an early date, say 1915. From this perspective the shift from mainline to reform represented less a loss of confidence among mainline geneticists in the *feasibility* of eugenics on technical grounds, than a growth of concern within the reformist camp about the *desirability* of eugenics. Such concern was certainly prompted in part by political reservations. It is well known that many—if not most—of the geneticists normally cited as reformists were sympathetic in varying measure to the left, as can be seen clearly in the 'Geneticists' Manifesto'.²⁶

There are several indications, however, that other geneticists' unease with mainline eugenics was rooted in professional considerations: a close association with a controversial movement could harm genetics' public image. Even before the First World War, for example, Karl Pearson was worried that the Eugenics Education Society's 'unscientific' policies were antagonizing the British medical profession. If the Eugenics Laboratory at University College London got mixed up with the EES, he wrote to Francis Galton, 'we should kill all chance of founding eugenics as an academic discipline'.²⁷ During the war, several American geneticists declined office in eugenic organizations, objected to joint meetings of eugenicists and geneticists, and recommended that journals of genetics should not publish on eugenics.²⁸ Although William Bateson was a convinced hereditarian who accepted the arguments for some eugenic policies, he wanted genetics, nevertheless, to keep its distance:

The eugenicist and the geneticist will, I am convinced, work most effectively without organic

24 I have discussed this issue at greater length in 'The I.Q. in History', *Social Studies of Science* (1983), 13, pp. 465–477 on pp. 473–475.

25 Ludmerer (op. cit. 20) also claimed R.C. Punnett and William Castle as converts, but Barker's evidence (this issue) makes the extent of this conversion doubtful.

26 Op. cit. (22). See also Paul (op. cit. 23) and Roll-Hansen (this issue).

27 Kevles, op. cit. (8), p. 104.

28 Kevles, op. cit. (8), pp. 121–122.

connection, and though we have much in common, should not be brigaded together. Genetics are [sic] not concerned with the betterment of the human race but with a problem in pure physiology, and I am a little afraid that the distinction of our aims may be obscured.²⁹

A year later he wrote to Morgan, opposing geneticists' participation in the Second International Congress of Eugenics on the grounds that 'we might do ourselves great harm'.³⁰ In 1925 he declined an invitation to address the undergraduate eugenics society at Cambridge explaining:

On and off I have definitely tried to keep clear of [eugenics]. To real genetics it is a serious—increasingly serious nuisance, diverting attention to subordinate and ephemeral issues, and giving a doubtful flavour to good materials.³¹

Nevertheless, one of the key features of geneticists' relations to the eugenics movement is that they did not abandon eugenics *altogether* until after World War Two. The 'public image theory' outlined above would attribute this to the fact that, throughout this period, genetics was an emergent discipline which could not afford to alienate potential sources of public support. That the institutionalization of genetics in various countries gained much from its 'eugenic promise' is well documented. In Britain the chair of genetics at Cambridge was endowed by a benefactor with eugenic sympathies, as was the Galton Laboratory and the chair occupied successively by Pearson, Fisher and Lionel Penrose at University College London.³² In the United States genetics and eugenics were institutionally intertwined in the American Breeders' Association, the American Genetics Association and their respective journals before the First World War and in the Carnegie Institution of Washington's Department of Genetics thereafter.³³ In Scandinavia, as Roll-Hansen demonstrates in this issue, Norway's first department of genetics and Sweden's Institute of Race Biology—later a centre for human genetics—were both established with an eye to their eugenic relevance. In view of such extensive dependence upon eugenics for public visibility, it is hardly surprising that the Geneticists' Manifesto concludes its defence of reform eugenics with a plug for genetics:

The effectiveness of such progress [in eugenics], however, would demand increasingly extensive and intensive research in human genetics and in the numerous fields of investigation correlated therewith . . . the study of [human] genetics is beset with special difficulties which require the prosecution of research in this field to be on a much vaster scale . . . than hitherto contemplated.³⁴

One of the merits of the 'public image theory' is that it may be able to account for variations in the ways in which geneticists sought to disengage themselves from mainline

29 Bateson, 'Common Sense in Racial Problems', *Eugenics Review* (1919–1920), 13, 325–338.

30 Cited by Hamilton Cravens, *The Triumph of Evolution: American Scientists and the Heredity–Environment Controversy, 1900–1940*, Philadelphia, 1978, p. 176.

31 Bateson to Michael Pease, 28 January 1925, Bateson papers, sheet no. 1876, John Innes Horticultural Institute.

32 Kevles, *op. cit.* (8), pp. 37–38 and 'Genetics in the United States and Great Britain, 1890–1930', *Isis* (1980), 71, pp. 441–455 on p. 454.

33 B. Kimmelman, 'The American Breeders' Association: Genetics and Eugenics in an Agricultural Context, 1903–1913', *Social Studies of Science* (1983), 13, pp. 163–204; Garland Allen, 'The Eugenics Record Office at Cold Spring Harbor, 1910–1940', *Osiris* (1986), 2, pp. 225–264.

34 *Op. cit.* (22), p. 373.

eugenics. As Garland Allen pointed out several years ago, the fact that American geneticists expressed their initial dissatisfaction with the eugenics movement *in private* may have been expedient at that time since important funding bodies (such as the Carnegie Institution of Washington and the Rockefeller philanthropies) were then supporting both genetics *and* eugenics.³⁵ Similarly, if we want to understand why German geneticists declined to speak out against the racial hygiene movement before 1933, we had better not overlook the fact that genetics was much more weakly institutionalized in Germany than in either Britain or the United States until the 1950s.³⁶

In view of its obvious plausibility, the public image theory deserves to be systematically developed. Of course, the three kinds of explanatory factors discussed above—intellectual, ideological and professional—need not be mutually exclusive.³⁷ All three of these factors may be necessary for a full understanding of the genetics community's changing relations with the eugenics movement. Lest this seem like a recommendation for mindless eclecticism ('it is all very complicated' etc.), let me conclude by outlining a model which tries to integrate the intellectual, professional and ideological facets of the problem. It makes three postulates:

(i) Almost all geneticists recognized relatively early (by World War One) that mainline eugenics was technically flawed.

(ii) Until mainline eugenics became controversial during the 1920s, however, it was professionally inopportune for the genetics community as a whole to draw attention to these flaws or disassociate itself publically from the eugenics movement.

(iii) The only individual geneticists prepared to speak out against mainline eugenics from the mid 1920s (or earlier in some cases) were those with political objections or those working in well-established institutions who could afford to dispense with the visibility which eugenics conferred.

Whether right or wrong, the model illustrates how apparently contradictory interpretations of the genetics community's abandonment of mainline eugenics might in fact be complementary.

35 Allen, 'Genetics, Eugenics and Class Struggle', *Genetics* (1975), 79, pp. 29–45.

36 J. Harwood, *op. cit.* (7).

37 Although they make little of it, even historians who emphasize the importance of intellectual factors have noticed how concerned geneticists were with the discipline's image. Cravens, for example, remarks that reputable biologists

were quite willing to tolerate the eugenics movement and to perhaps even support some of its goals so long as the movement appeared disinterested, scientifically credible, and dignified. But the activities of Laughlin [an American racist who deployed genetic and eugenic arguments in promoting anti-immigration legislation] and others put science—genetics, after all, so far as the public could tell—in a distasteful and ugly light. (*op. cit.* [30], p. 177).

Ludmerer, too, observes that geneticists 'were dismayed by the distorted image of genetics that eugenicists were popularizing' (*op. cit.* 20, p. 354).