Perspectives

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The Genetical Theory of Natural Selection

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> So melancholy a neglect of Darwin's work suggests reflections upon the use of those rare and precious possessions of man—great books. It was, we believe, the custom of the late Professor Freeman to warn his students that mastery of one great book was worth any amount of knowledge of many lesser ones. The tendency of modern scientific teaching is to neglect the great books, to lay far too much stress upon relatively unimportant modern work, and to present masses of detail of doubtful truth and questionable weight in such a way as to obscure principles. . . . How many biological students of today have read *The Origin*? The majority know it only from extracts, a singularly ineffective means, for a work of genius does not easily lend itself to the scissors; its unity is too marked. Nothing can really take the place of a first-hand study of the work itself.
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> R. A. Fisher and C. S. Stock (1915)
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> [Fisher was 25 years old and his student friend Stock 27 in 1915. Professor E. A. Freeman was Regius Professor of Modern History at Oxford, 1884–1892.]

ON May 14, 1929, R. A. Fisher wrote to Oxford University Press, "I should call the book something like *The Genetical Theory of Natural Selection*." Within the year Fisher's (1930) book had been published, and in May 2000 we can celebrate its seventieth birthday. Last year Oxford republished in facsimile the 1930 edition as a Variorum Edition, edited with a foreword, notes, and appendices by Henry Bennett. Once again, the book, which J. B. S. Haldane (1932) described as "brilliant" and Sewall Wright (1930) as "certain to take rank as one of the major contributions to the theory of evolution," is in print.

In the present panegyric I assume some familiarity with *The Genetical Theory*. The indefatigable scholarship of Professor Bennett has left us with two detailed descriptions of the book, his Variorum Edition foreword and an earlier essay in his volume of Fisher's biological correspondence (Bennett 1971–74, 1983), but, like *The Origin of Species*, *The Genetical Theory* cannot be appreciated second hand. The book is not long; the main text of the 1930 edition has 265 pages of 13-point lines. ("Fairly large print is a real antidote to stiff reading" wrote Fisher to his Oxford editor—all quotations from Fisher's correspondence are from Bennett 1983.) Bennett opens his foreword with the following paragraph:

> *The genetical theory of natural selection* is celebrated as the first major work to provide a synthesis of Darwinian selection and Mendelian genetics. Its publication in 1930 marked a turning point in the development of evolutionary thought, contributing fundamentally to the renaissance of Darwinism following a long period of neglect. It is a work remarkable for what it reveals of Fisher's creative genius and his insight into many of the problems facing evolutionary biologists today.
>
> Bennett (1983)

In this *Perspectives* series, Crow (1990a) summarized the main themes of *The Genetical Theory* ("arguably the deepest and most influential book on evolution since Darwin"), and in another celebration of the Fisher centenary (Crow 1990b), he gave this succinct account:

> [In addition to enunciating the "fundamental theorem of natural selection," Fisher] developed a totally novel way (now standard) for determining the probability of survival of a mutant gene. He worked out the partial differential equation for gene-frequency change, using a trigonometric transformation that made the variance independent of the allele frequencies. He generalized Haldane's formula for the probability of survival of a mutant gene, making it possible to treat deleterious as well as favorable mutants. The formula, further improved by Malécot and Kimura, plays a crucial role in the analytical treatment of the neutral theory of molecular evolution. He worked out the stationary distribution of allele frequencies. He gave us the first quantitative theories of sexual selection, mimicry, polymorphism, evolution of recombination rates, and supergenes. He explained why the sex ratio is nearly 1:1 even in highly polygamous species—a problem that baffled Darwin and that provides one of the best illustrations that natural selection does

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Figure 1.—R. A. Fisher, about 1932.

> not always do what is best for the species. In doing this, Fisher introduced the concept of parental expenditure, thereby seeding a cloudburst of ecological literature. Rarely have so many new and deep ideas been put into a single book.
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> Crow (1990b, p. 270)

I discuss the problem of the sex ratio below, as well as mention further themes in *The Genetical Theory*, such as kin selection.

The author: Ronald Aylmer Fisher was born in London, England, in 1890, and died in Adelaide, Australia, in 1962. In 1928–1929, when he was writing *The Genetical Theory*, Fisher was head of the statistics department at Rothamsted Experimental Station, already taking over from Karl Pearson the mantle of Britain's leading statistician. His *Statistical Methods for Research Workers*, which was to revolutionize the application of statistics, had appeared in 1925 and was now in its second edition (Fisher 1925). By mid-century, when his collection of papers *Contributions to Mathematical Statistics* (Fisher 1950) was published, it was clear that Fisher was the leading world figure in statistics, and as the century turns it is now generally accepted by historians of the subject that his contributions place him alongside Laplace (1749–1827) and Gauss (1777–1855). Hald (1998), the foremost historian of mathematical statistics, writes, "Fisher was a genius who almost single-handedly created the foundations for modern statistical science. . . ."

Like Laplace and Gauss, Fisher also made outstanding contributions to science outside the fields of probability and statistics. While Laplace and Gauss contributed to the physical sciences such as astronomy and geodesy (not to mention pure mathematics itself), Fisher used his mathematical gifts to till the fertile ground of biology. C. G. Darwin (1930)—Sir Charles Darwin, physicist, grandson of Charles Darwin—opened his review of *The Genetical Theory* plaintively with the words, "When a man reveals himself as a master of two very different subjects it is hard to find a critic of his work." How did Fisher, the equal of Laplace and Gauss in statistics, become "the greatest of Darwin's successors" (Dawkins 1986) as well?

Joan Box in her superb biography of her father (Box 1978) tells a story about Fisher's biology teacher at Harrow School, Arthur Vassall, and the influence he had on the boy. Vassall once told E. B. Ford, in answer to his invitation to name the 10 or 12 cleverest boys he had taught, that it would be difficult to do so, but in terms of sheer brilliance he could divide them into two groups, Fisher in one and all the rest in the other. Writing to Vassall in 1929, as *The Genetical Theory* approached completion, Fisher said, "The fact is that nearly all my statistical work is based on biological material and much of it has been undertaken merely to clear up difficulties in experimental technique."

Fisher's early interest in natural history is reflected in the books chosen for school prizes, leading up to, in his last year, the complete works of Charles Darwin in 13 volumes. "He went up to Cambridge in possession of volumes he was to read and re-read with loving care throughout his life" (Box 1978).

There was some uncertainty as to whether Fisher should try for a Cambridge scholarship in mathematics or in biology, but in the end mathematics won. When he arrived at Cambridge with a scholarship in 1909, the University had just celebrated the Darwin centenary and the fiftieth anniversary of the publication of *On the Origin of Species* (Darwin 1859). In connection with the centenary, an anonymous benefactor had endowed a Professorship of Biology at Cambridge "to be devoted to that branch of Biology now entitled Genetics (Heredity and Variation)," and William Bateson, who had coined the very word "genetics" only 3 years previously, had been elected to it in 1908. Bateson's (1909) book *Mendel's Principles of Heredity*, containing an English translation of Mendel's paper, and Francis Darwin's edition of his father's unpublished essays of 1842 and 1844 (Darwin 1909) were being printed by Cambridge University Press at one end of King's Parade just as the young mathematician-biologist entered Gonville and Caius College at the other end. Fisher bought a copy of Bateson (Fisher 1952), later remarking, "The new school of geneticists using Mendel's laws of inheritance was full of activity and confidence, and the shops were full of books good and bad from which one could see how completely many writers of this movement believed that Darwin's position had been discredited" (Fisher 1947).

Fisher the mathematician developed and retained the highest opinion of biologists' work. In the Preface to *The Genetical Theory* he remarks:

> The types of mind which result from training in mathematics and in biology certainly differ profoundly; but the difference does not seem to lie in the intellectual faculty. It would certainly be a mistake to say that the manipulation of mathematical symbols requires more intellect than original thought in biology; on the contrary, it seems much more comparable to the manipulation of the microscope and its appurtenances of stains and fixatives; whilst original thought in both spheres represents very similar activities of an identical faculty.
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> Fisher (1930, p. viii)

Nevertheless, mathematics was Fisher's undergraduate subject and he distinguished himself at it in the hard school of the Cambridge tripos, obtaining a "first" in both parts. Awarded a studentship in physics, he was able to return for a graduate fourth year and study, at the Cavendish Laboratory, the theory of errors under F. J. M. Stratton and statistical mechanics and quantum theory under James Jeans. It is hard to imagine a more complete biological and mathematical preparation for the young man who was simultaneously to revolutionize statistics and bring mathematical and statistical thinking into evolutionary theory. "Just as James Clerk Maxwell had conjured a theory of gases out of the motions of individual molecules so Fisher, trained in the same school of science, had conjured a theory of evolution out of the survival of individual genes" (Edwards 1988). Perhaps Fisher even dared to think that he bore to Darwin the same relationship as Maxwell did to Faraday (see Fisher 1932).

Before leaving the question of mathematics, we should pause to record (with the author's permission) D. G. Kendall's remarks when he unveiled a plaque on the building formerly occupied by Fisher's Cambridge Department of Genetics, on March 22, 1990 (see also Kendall 1990):

> We have come here today to pay honour to Fisher the great geneticist, rather than to Fisher the greatest of all statisticians. But I should also like to say a word about Fisher the great mathematician—a figure some of you may not have met.
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> He has many claims to that third title. For example he was one of the few who anticipated to some extent the creation of the Theory of Games, which swept economists off their feet and which has more recently begun to play a role in biology.
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> But the chief claim rests on his contributions to stochastic diffusion theory. It will suffice to present this through two anecdotes.
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> We usually think of stochastic diffusion theory as originating in Kolmogorov's great paper of 1932. But hear what Feller said in a Princeton lecture: "If Kolmogorov had never written, the whole theory would have developed in much the same way starting from Fisher's book *The Genetical Theory of Natural Selection*, published in 1930."
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> Some years ago at a small mathematical seminar in the Black Forest, devoted to the analytical aspects of branching processes, . . . Kolmogorov referred to "das wundervolle Buch von R. A. Fisher," and I heard two American statisticians sitting near me whisper to one another: "Well, it *can't* be the R. A. Fisher *we* know." And they were right: they had never before encountered R. A. Fisher the geneticist, much less R. A. Fisher the mathematician.
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> So in unveiling this plaque, a tribute to his memory, let us for once recall the full sweep of his genius.

Leonard Darwin: In his second undergraduate year, Fisher was instrumental in setting up the Cambridge University Eugenics Society with the support of senior members of the University such as R. C. Punnett (soon to be the first Professor of Genetics), John Maynard Keynes, and Horace Darwin (the youngest of Charles's surviving children). Galton, in his Presidential Address to the Eugenics Education Society of London in 1909, had remarked, "It cannot be too emphatically repeated that a great deal of careful statistical work has yet to be accomplished before the science of eugenics can make large advances" (quoted in Forrest 1974). Although this might have been Fisher's motto for the work ahead, what cannot be seriously sustained, as it has been by some historians of science, is that Fisher's youthful enthusiasm for eugenics was the engine for his advances in statistical and evolutionary genetics (see Bennett 1983). Nevertheless, his involvement was of vital influence in another way, for it led him to meet Galton's successor as President of the London Society, Major Leonard Darwin, Horace's immediately elder brother, "surely the kindest and wisest man I ever knew" (Fisher in 1943; quoted by Bennett 1983).

Leonard Darwin was to the young Fisher what J. S. Henslow, Professor of Botany at Cambridge, had been to the young Charles Darwin. In 1836 Charles wrote on his return from the *Beagle* voyage, "My dear Henslow, I do long to see you; you have been the kindest friend to me that ever man possessed." In his *Autobiography* (Darwin 1893) Charles recorded his debt: "I have not yet mentioned a circumstance which influenced my whole career more than any other. This was my friendship with Professor Henslow." When Henslow died, Charles concluded his eulogy with a sentence that Fisher could so well have written of Leonard: "Reflecting over his character with gratitude and reverence, his moral attributes rise, as they should do in the highest character, in pre-eminence over his intellect" (quoted by Gardiner 1999).

Leonard was a powerful influence on Fisher in many ways, and the direct link he provided with Charles Darwin is of profound significance. The dedication of *The*

Genetical Theory is "TO MAJOR LEONARD DARWIN in gratitude for the encouragement, given to the author, during the last fifteen years, by discussing many of the problems dealt with in this book." From 1915 to the publication of *The Genetical Theory* and beyond, the two men were in regular contact, much of it fortunately preserved in their correspondence, where we find Leonard urging Fisher, as early as 1922, to write his great book: "I hope to stir you up to write a great work on the mathematics of evolution." In 1928 he wrote, "I am glad you are at work on your evolution book. . . . How about your new statistical work? I hope they can go on simultaneously. *Don't* hurry evolution, but *do* go on with it." And simultaneously they did go on, with Fisher working at Rothamsted during the day and dictating *The Genetical Theory* to his wife in the evenings.

The Eclipse of Darwinism: It is difficult for us, as the century turns, to imagine the scepticism that surrounded Darwin's theory of evolution by natural selection 70 years ago and astonishing for us to recall that Mendelism itself was regarded in some quarters as antithetical to it. In *The Eclipse of Darwinism*, Bowler (1992) describes in detail the anti-Darwinian arguments 100 years ago—Lamarckism, orthogenesis, and the mutation theory—and Bennett (1983) opens his Introduction with a description of the doubts still prevailing in 1920–1930. (See also Mayr 1988; Olby 1981; Turner 1985; and Fisher 1932, 1947, 1954.)

The old criticism of Darwin's theory, that chance events themselves could never produce the marvelous adaptations that nature exhibits, also refused to lie down. It had originally appeared as an immediate response to *The Origin of Species* when John Herschel had commented that Darwin's theory was "the law of higgledy-piggledy." "What exactly this means I do not know," snorted Darwin, "but it is evidently very contemptuous." Herschel added a footnote in the second edition of his *Physical Geography* in 1861 in which he said:

> We can no more accept the principle of arbitrary and casual variation and natural selection as a sufficient account, *per se*, of the past and present organic world, than we can receive the Laputan method of composing books (pushed *a l'outrance*) as a sufficient one of Shakespeare and the *Principia*. Equally in either case, an intelligence, guided by a purpose, must be continually in action to bias the directions of the steps of change—to regulate their amount—to limit their divergence—and to continue them in a definite course.
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> Herschel (1861, p. 12)

Lord Kelvin, then Sir William Thomson, told the 1871 meeting of the British Association in his Presidential Address that Herschel's objection to the doctrine of natural selection, "that it was too like the Laputan method of making books," seemed to him to be "a most valuable and instructive criticism." The references are, of course, to Swift's *Gulliver's Travels* (1726) and his description of the activities of the "projectors in speculative learning" in the Grand Academy of Lagado in Laputa, a satire on the Royal Society of London.

Against this background hostile to Darwinism, Fisher set out to show that evolution by natural selection operating on Mendelian characters was a viable theory not only capable of explaining the observations that had led Darwin to his revolutionary views, but also rich in hidden consequences that offered explanations for evolutionary panoramas as yet unimagined. By applying the techniques of advanced, often stochastic, mathematics to Mendel's genetical theory, Fisher was able to give an account of the power of natural selection.

The improbability generator: I once heard Fisher remark, "Natural selection is a mechanism for generating an exceedingly high degree of improbability." It was one of his favorite aphorisms, first reported by Julian Huxley in 1936 and often repeated in Huxley's work (*e.g.*, 1942, 1954) until it finally passed into the language unattributed through the writings of C. H. Waddington, Gavin de Beer, Ernst Mayr, and Richard Dawkins. Although it does not occur in *The Genetical Theory*, Fisher's rebuttal of "the objection which has been made, that the principle of Natural Selection depends on a succession of favourable chances" embodies the thought in characteristic style:

> The objection is more in the nature of an innuendo than of a criticism, for it depends for its force upon the ambiguity of the word chance, in its popular uses. The income derived from a Casino by its proprietor may, in one sense, be said to depend upon a succession of favourable chances, although the phrase contains a suggestion of improbability more appropriate to the hopes of the patrons of his establishment. It is easy without any very profound logical analysis to perceive the difference between a succession of favourable deviations from the laws of chance, and on the other hand, the continuous and cumulative action of these laws. It is on the latter that the principle of Natural Selection relies.
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> Fisher (1930, p. 37)

In 1954 Fisher contributed to *Evolution as a Process*, edited by Julian Huxley, A. C. Hardy, and E. B. Ford, an essay, "Retrospect of the Criticisms of the Theory of Natural Selection" (Fisher 1954). In the words of Huxley's introductory essay, Fisher "very appropriately deals with the main criticisms which have been levelled against the general theory of natural selection, and the main difficulties which its opponents have raised. He points out that all of these have been in principle answered or resolved by recent developments of mendelian or particulate genetics." It is uncertain when Fisher wrote this essay—probably in the 1930s. When he sent it to Ford in 1951 for *Evolution as a Process*, he wrote, "I wrote it a long while ago when the possibility of my bringing out a second edition of the *The Genetical Theory* was in my mind." It amplifies the rebuttal of the "succession of favourable chances" argument in two ways. First, Fisher enlarges on his aphorism:

> . . . it was Darwin's chief contribution, not only to Biology but to the whole of natural science, to have brought to light a process by which contingencies, *a priori* improbable, are given, in the process of time, an increasing probability, until it is their non-occurrence rather than their occurrence which becomes highly improbable.
> Fisher (1954, p. 91)

Second, he emphasizes the importance of not being carried away by improbabilities viewed after the event anyway:

> Consideration of the conditions prevailing in bisexual organisms shows that . . . the chance of an organism leaving at least one offspring of his own sex has a calculable value of about 5/8. Let the reader imagine that this simple condition were true of his own species, and attempt to calculate the prior probability that a hundred generations of his ancestry in the direct male line should each have left at least one son. The odds against such a contingency as it would have appeared to his hundredth ancestor (about the time of King Solomon) would require for their expression forty-four figures of the decimal notation; yet this improbable event has certainly happened.
> Fisher (1954, p. 91)

These considerations, flowing from *The Genetical Theory* in the 1930s, also provide a fascinating background to Fisher's contemporary discussions of the foundations of statistical inference, for the logical weakness of any statistical test of significance is that it argues on the basis of the probability of an event after the event has been observed. Such fundamental questions cannot be pursued here, but the development of Fisher's own concept of *likelihood* is of course part of the response to the dilemma (for likelihood and its history, see Edwards 1992). We may note, however, that the theory of evolution by natural selection has a high likelihood precisely because the ratio of the probability of the natural world, as we observe it, on the hypothesis of natural selection to its probability on the "hypothesis" of pure chance is so enormous.

Reading *The Genetical Theory*: The author, the editor, the reviewers, Fisher's correspondents, and all more recent commentators agree that *The Genetical Theory* is not an easy book. Seventy years after its publication the custom is now for such a book—if such a book were to be written—to rely to a far greater extent on aids to easy assimilation. Diagrams, pictures, and boxed text relieve the burden of solid reading, and numerous metaphors reflect reasoned argument like the curved mirrors at a carnival. Fisher's book, by contrast, is all argument. Each sentence must be *considered*, from the very first one ("Natural Selection is not Evolution"). "Each rereading of this classic brings something new" (Crow 1990a). Leonard Darwin did his best as he read each chapter in draft: "One idea one sentence is, I think, a good rule," he wrote to Fisher in March 1929. One reader has written, "Not least is my debt to the ghost of Ronald Fisher himself: in this wordy age, the pleasure of working with such closely and acutely reasoned writing is beyond my power to express" (Leigh 1986). The style belongs to Charles Darwin's age, not ours, and it is illuminating to think of *The Genetical Theory* as a kind of mathematical-Mendelian appendix to *The Origin of Species*. Fisher was probably the best-read Darwinian of his generation, and it is not surprising that it affected his style.

Another feature of *The Genetical Theory* that is characteristic of its time is its cavalier way with references. *The Origin of Species*, it will be remembered, contains none at all, on the grounds that it was but an abstract of the much larger work that Darwin had planned: "This Abstract, which I now publish, must necessarily be imperfect. I cannot here give references and authorities for my several statements; and I must trust to the reader reposing some confidence in my accuracy" (Darwin 1859). Perhaps the most astonishing of Fisher's omissions is his own paper (Fisher 1918) "On the correlation between relatives on the supposition of Mendelian inheritance." It is true that the 1918 paper is not primarily concerned with evolution, but what modern writer could resist citing such a path-breaking paper when he came to write his *magnum opus* in the same general field?

Another notable omission from Fisher's list of references has led generations of evolutionary biologists to assume that perhaps the most famous single argument in *The Genetical Theory*, on "Natural Selection and the sex-ratio," was original to Fisher. Although Fisher was not aware that the argument was in the first edition of *The Descent of Man* (Darwin 1871), he was certainly aware of some secondary sources for it (Edwards 1998) and, in particular, a paper by Cobb (1914) in *The Eugenics Review*. Cobb did not refer to Darwin any more than Fisher did to Cobb, and one must suppose that the argument was circulating freely at the time among the small number of people then interested in such things.

Neglect of *The Genetical Theory*: Bennett (1983) has discussed the reviews of *The Genetical Theory*. Those in a position to judge it, especially Haldane and Wright, who both wrote long reviews, thought it a masterpiece. In spite of Wright's (1930) review, the book was not as widely appreciated in the United States as it was in Great Britain, and this was to remain the case until quite recently. Leonard Darwin warned Fisher that its influence would be slow to be felt: ". . . my impression is . . . that it will be slowly recognized as a very important contribution to the subject. But I am afraid it will be slow, because so few will really grasp all that it means. . . ." And Fisher himself observed, on hearing from Oxford University Press in 1931 of better-than-expected sales, "It was so long before I heard from them that I had quite made up my mind that it was one of those books which everybody praised and nobody read, and would have no influence on biological opinion."

In the United States, Dobzhansky (1937) clearly appreciated the significance of *The Genetical Theory*, but in his influential *Genetics and the Origin of Species* he preferred the more metaphorical line of thought established by Wright (1932). Simpson's (1944) *Tempo and Mode in Evolution* was rather more appreciative of Fisher's contributions. Mayr (1942), by contrast, never seems to have come to terms with its importance, either in *Systematics and the Origin of Species* or when writing on the history of biology. In *The Evolutionary Synthesis* (Mayr 1988; first edition 1980), he contributed a "Prologue: Some thoughts on the history of the evolutionary synthesis" without once mentioning *The Genetical Theory* (unless a remark about "the supposedly evolutionary writings of . . . R. A. Fisher" counts as a mention). In his massive *The Growth of Biological Thought*, Mayr (1982) expresses opinions about Fisher's work that can be explained only by an inadequate study of *The Genetical Theory*, and his *One Long Argument: Charles Darwin and the Genesis of Modern Evolutionary Thought* (Mayr 1991) contains nearly 200 references, but nothing of Fisher's at all (or, indeed, of Wright's).

In Britain the advocacy of E. B. Ford (*e.g.*, 1938) and Julian Huxley (*e.g.*, 1942) in their numerous writings ensured that *The Genetical Theory* was not entirely forgotten in the early years. Ford wrote to Fisher in 1955: "It has been an amazement to me that the original edition did not sell out long before the War [1939] but, after all, a book is to be judged not by its sale but by its effect upon science, and no book of the century has had a greater effect upon biology than has this one, the ideas spreading out from it through, apparently, a limited number of readers of the original, but that kind of thing is what both you and I are accustomed to find (people like to be given little summaries)." Fisher, although he was Arthur Balfour Professor of Genetics at Cambridge from 1943 until his retirement in 1957, ran a very small department, which had little impact at the time (Edwards 1990), and the leading writer on evolutionary genetics in Britain has been Maynard Smith, a student of Haldane's. Maynard Smith's (1993) *The Theory of Evolution* (first edition 1958) ends with a list of books for further reading, which starts with *The Origin of Species* and includes books by Mayr, Dobzhansky, and Simpson, as well as P. M. Sheppard, R. C. Lewontin, C. H. Waddington, and G. C. Williams, but not *The Genetical Theory* or, curiously enough, Haldane's *The Causes of Evolution* (1932).

The Genetical Theory has not been translated into any foreign language, and outside the English-speaking world perhaps only France has given it serious attention (L'Héritier 1934; Roger 1981).

The main influential topics from the early life of *The Genetical Theory* were "Natural Selection and the sex-ratio" (mentioned above), which came to be seen as the archetypal game-theoretic argument (Maynard Smith 1982), and "the fundamental theorem of natural selection." The latter, apart from having been the original inspiration for Wright's "adaptive surface" approach, has caused a great deal of discussion and indeed controversy, and only now can we understand Fisher's thinking (see my review, Edwards 1994, and for a parallel between the history of the fundamental theorem and that of fiducial probability, see Edwards 1995).

Revival of *The Genetical Theory*: By the time of Fisher's death in 1962, *The Genetical Theory* had thus had only a limited direct influence. Like Mendel's work, its fame now rests more on the influence it should have had, rather than the influence it did have. It lay fallow for just as long as did Mendel's paper. The publication of the second edition in 1958, however, made it more widely available, and the next generation of evolutionary biologists was to discover, or in some cases duplicate, its riches. I make the point by considering the reprint of G. C. Williams's (1996) influential book *Adaptation and Natural Selection*. Williams, together with W. D. Hamilton, has been held chiefly responsible for the modern view that natural selection operating within populations is the primary mechanism of evolution. Williams cites *The Genetical Theory* in the first paragraph of his Introduction, but it is his 1996 account of what influenced him in the writing of his book to which I draw particular attention:

> Two publications that I read that year [1954–55] were also of great importance to me, and may have deterred me from some alternative career. One was Shaw and Mohler's brief article on sex ratio (1953). The other was David Lack's chapter in Huxley, Hardy, and Ford (1954). Shaw and Mohler's superb work went largely unnoticed until it was discovered by Charnov (1982), who made the Shaw-Mohler equation a focal concept for his broad treatment of sex allocation. . . . Right from its opening paragraph, Lack's "The Evolution of Reproductive Rates" was a sublime encouragement. I had found a biologist who believed, as decisively as I did, that natural selection is a real scientific theory. It logically predicts that there are certain sorts of properties that organisms must have, and others, such as adaptations for the "benefit of the species" (Fisher 1958: 49–50), that they could not possibly have.
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> . . . .
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> I may have been afflicted by a common form of megalomania in assuming that I had the uniquely right perspective and was far ahead of my time. Awareness of the work of Shaw and Mohler should have made me doubtful about any such claim. I was really forced to abandon it by the discovery of other publications that anticipated some of what I was writing, especially W. D. Hamilton's 1964 works on inclusive fitness.
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> Williams (1996 p. x)

My point in reproducing this extract is, of course, that the cited works of Shaw and Mohler, Lack, and Hamilton all relate to topics Fisher treated in *The Genetical Theory*. Specifically, Shaw and Mohler (1953) start from *The Genetical Theory* having, like Fisher, consulted only the second edition of Darwin's *The Descent of Man*. [I had myself asked Fisher about natural selection and the sex ratio in 1958, and he had told me to read *The Genetical Theory*, which I did, putting his argument into mathematics with the help of W. F. Bodmer (Bodmer and Edwards 1960), including the effect of parental expenditure, which Shaw and Mohler had omitted]. As we now know, the argument was not original to Fisher, but it was *The Genetical Theory* that made it famous.

Lack's (1954) influential paper "The evolution of reproductive rates" appeared alongside Fisher's (1954) "Retrospect" in *Evolution as a Process*. In the words of Huxley in his introduction, "Lack advances the view that basic clutch size is adaptive, genetically adjusted by selection. . . ." I can do no better than quote from Burbridge (1992):

> This [explanation] has a long and tortuous history. R. A. Fisher, apparently unknown to Lack, discussed the general problem in his classic book in 1930, hardly an obscure work. Fisher's characteristically incisive comments on fecundity may have been overlooked because they occur in the chapters dealing with human evolution and eugenics, from which many readers avert their eyes.
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> Fisher himself attributed the solution to Major Leonard Darwin, a younger son of Charles Darwin. Leonard Darwin did indeed cover the matter clearly. He thought his treatment of the subject was novel, but he was wrong. Ironically, his father had discussed the problem in the first edition of his *Descent of Man* and given essentially the same solution. But in the second edition the relevant chapter was heavily revised and the passage on fecundity omitted. As later biologists seldom consulted the scarce first edition, Darwin's contribution seems to have been forgotten, even by his own son.
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> As a final complication, Charles Darwin gave the credit for the crucial insight to Herbert Spencer . . .
>
> Burbridge (1992)

As to "W. D. Hamilton's 1964 works on inclusive fitness," Hamilton (1964) had pointed out, with a quotation, that Fisher had invoked the idea of inclusive fitness in *The Genetical Theory*, "Mimicry" (Chap. 8), to explain the evolution of distastefulness in insects. The idea first surfaces in a lecture Fisher gave to the Cambridge University Eugenics Society in 1912 (Fisher 1914; Edwards 1993). It was later taken up by Haldane, and, thanks to the publication of Fisher's correspondence, we now have a succinct statement of it (in a letter to A. G. Lowndes, June 1945): "This does not preclude adaptations which are effectual through the survival of relatives, for these share to a greater or lesser extent the germ plasm of the individual. So the parental instincts, though altruistic, are accessible to improvement through natural selection, and in my book I do discuss how far we may think of the development of nauseous flavours in insect larvae. . . ."

I opened this discourse on *The Genetical Theory* with a quotation from Fisher and Stock (1915) on the importance of a great book. I close it with a quotation from Hamilton:

> This is a book which, as a student, I weighed as of equal importance to the entire rest of my undergraduate Cambridge BA course and, through the time I spent on it, I think it notched down my degree. . . .
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> For a book that I rate only second in importance in evolution theory to Darwin's "Origin" (this as joined with its supplement "Of Man"), and also rate as undoubtedly one of the greatest books of the [twentieth] century the appearance of a variorum edition is a major event. . . .
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> By the time of my ultimate graduation, will I have understood all that is true in this book and will I get a First? I doubt it. In some ways some of us have overtaken Fisher; in many, however, this brilliant, daring man is still far in front.
>
> W. D. Hamilton
>
> (from the dust-jacket of the Variorum *Genetical Theory*)

In the 40 years since Fisher told me to read *The Genetical Theory*, I have discussed it with more colleagues than I can possibly record. I am grateful to all of them. But none of these discussions could have borne fruit without the scholarship of Henry Bennett and the encouragement of James Crow.

Note added in proof: W. D. Hamilton died in Oxford on March 7, 2000, from malaria contracted during a field trip in Africa.

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