Did R.A.Fisher's interest in eugenics stimulate his interest in both statistics and genetics?

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In June 2020 Gonville and Caius College in Cambridge issued a press announcement that its College Council had decided to 'take down' the stained-glass window which had been placed in its Hall in 1989 ready for the centenary of Sir Ronald Fisher the following year. The window depicted the colourful Latin-Square pattern from the jacket of Fisher's 1935 book *The Design of Experiments*. The window was one of a matching pair, the other commemorating John Venn with the famous three-set 'Venn diagram', each window requiring seven colours which were the same in both (Edwards, 2002; 2014*a*). One of the arguments advanced for this action was Fisher's interest in eugenics which 'stimulated his interest in both statistics and genetics'*.

In this paper I challenge the claim by examining the actual sequence of events beginning with 1909, the year in which Fisher entered Gonville and Caius College. I show that the historians of science who promoted the claim paid inadequate attention to Fisher's actual studies in statistics as part of his mathematical education which were quite sufficient to launch him on his path-breaking statistical career; they showed a limited understanding of the magnitude of Fisher's early achievements in theoretical statistics and experimental design, which themselves had no connection with eugenics. Secondly, I show that Fisher's knowledge of natural selection and Mendelism antedated his involvement in eugenics; and finally I stress that the portmanteau word 'eugenics' originally included early human genetics and was the subject from which modern human and medical genetics grew..

Fisher's introduction to statistics

R.A.Fisher came up to Gonville and Caius College as an Entrance Scholar in mathematics in October 1909. He had established himself as a fine mathematician already when at Harrow School, winning the Neeld Gold Medal in Mathematics at the age of sixteen, though it was open to boys of any age (Box, 1978, p.15). In describing her father's mathematical precocity Box notes 'the beginning of his lifelong interest in astronomy' (Box, 1978, p.13).

One of the Fellows of Caius responsible for teaching mathematics was the young astronomer F.J.M.Stratton, an Assistant in the University Observatory. In the Easter Term 1911 he had lectured on *Calculation of Orbits from Observations*, and during the next academic year on *Combinations of Observations* in the Michaelmas Term (1911), the first term of Fisher's third and final undergraduate year. It is very likely that Fisher attended Stratton's lectures and subsequently discussed statistical questions with him during mathematics supervisions in College, and that he wrote his 1912 paper 'On an absolute criterion for fitting frequency curves' as a result (Fisher, 1912). The paper advocates the method of maximum likelihood, though not under that name. Fisher defined *likelihood* nine years later (Fisher, 1921) and coined the phrase *method of maximum likelihood* the year after (Fisher, 1922*a*)' (Edwards, 1997; for further information see Edwards, 1974).

*A draft of the present paper was submitted to the College Council with the request that it withdraw this statement. On 9 November 2022 it agreed to adopt the alternative '*The relationship of Fisher's interest in eugenics and his scientific research remains a matter of scholarly debate*'.

We can gauge the content of Stratton's lectures because David Brunt, in the preface to his book *The Combination of Observations* (Brunt, 1917) wrote 'I have to acknowledge my indebtedness to Mr F.J.M.Stratton … to whose University lectures I owe most of my knowledge of the subjects discussed in this book, and upon whose notes I have drawn freely'. The book is incidentally a wonderful reminder of the state of statistical estimation theory on the eve of the Fisherian revolution. The 1912 paper was Fisher's first publication, in April whilst still an undergraduate. In it he 'acknowledges the great kindness of Mr J.F.M.Stratton, to whose criticism and encouragement the present form of this note is due' (the order of Stratton's initials is incorrect). It is the harbinger of all that was to come in Fisher's development of the theory of statistical estimation (Fisher 1920, 1922*a*,*b* 1925*a*, and beyond).

Meanwhile Fisher's academic work flourished. He took a First Class in Part I of the Mathematical Tripos in 1911 and another in Part II in 1912. A postgraduate year at Caius followed with the award of a Wollaston Studentship for 1912-13 in which he studied at the Cavendish Laboratory under James Jeans for statistical mechanics and quantum theory, and under Stratton for more theory of errors. Advanced study of these topics had a profound influence on Fisher's subsequent researches in both theoretical genetics and mathematical statistics. During this year he published his second paper, a long essay 'Application of vector analysis to geometry' (Fisher, 1913).

The statistician W.S.Gosset, who famously published under the name "Student", was known to Stratton through his visits to Cambridge and its School of Agriculture. He noticed in Fisher's 1912 paper a discrepancy between his and Fisher's 'formula' for the estimated standard deviation of a sample from a normal distribution and mentioned this to Stratton, who suggested that Fisher should write to Gosset directly, which he did. On 12 September Gosset, who had spent a year studying under Karl Pearson, sent Fisher's letter to Pearson seeking his advice, adding that he (Gosset) had replied to Fisher justifying his original solution. 'To this he [Fisher] replied with two foolscap pages covered with mathematics of the deepest die in which he proved, by using *n*-dimensions that the formula was, after all' what Gosset had said (Box, 1978, p.72).

Thus did the twenty-two-year-old Fisher prove the distribution function for what came to be known as Student's *t*, inaugurating the concept of a sample-space and initiating his fundamental contributions to the mathematics of statistical distribution theory starting with *t* and the correlation coefficient (Fisher, 1915). This is not the place to give further details of such a well-known story, which is amply covered in numerous sources (*e.g.* Box, 1978, E.S.Pearson, 1990, Hald, 1998). Nor is it necessary here to describe Fisher's continuing fundamental advances across the whole breadth of mathematical statistics, statistical inference and the design of experiments. The point to be stressed is that at no stage from its initiation in 1912 up to what might be considered its zenith in 1935 with the publication of *The Design of Experiments* (Fisher, 1935*a*) is there any mention of eugenics, any connection with eugenics, or even any application to eugenics (as distinct from human genetics).

It is therefore not true that Fisher's interest in eugenics stimulated his interest in statistics. Norton and Pearson (1976) wrote 'It may be noted that, in 1912, Fisher published his paper 'On an absolute criterion for fitting frequency curves' in the *Messenger of mathematics*, which foreshadowed the basis on which his statistical philosophy was to be erected'. Yet it was Norton (1978*a*,*b*) who later stressed that it was eugenics (see below).

Fisher's introduction to natural selection and Mendelism

Fisher arrived in Caius College in October 1909. In June the University had celebrated Charles Darwin's centenary and the half-centenary of his *Origin of Species*. Fisher was already no stranger to the book, or to its sequel *The Descent of Man*, for during his last year at Harrow he had chosen the complete works of Darwin in thirteen volumes as a school prize. Already fascinated by the theory of natural selection and its application to Man he stepped into an atmosphere not only Darwinian but now Mendelian, for Cambridge was the home of William Bateson, the champion of Gregor Mendel's long-neglected paper which had resurfaced in 1900. The new century was facing a scientific revolution and in Britain Cambridge was its base. Bateson (1906) had introduced the name *genetics* for the new subject of the physiological basis of inheritance. In London he had presided over *The International Conference on Hybridisation and Plant Breeding*, which changed its name in the course of the Conference to the *Third International Conference on Genetics*.

Direct evidence is so far lacking about Fisher's knowledge of Mendelism before he came to Caius. But he will have stayed in the College for his 'Previous Examination' earlier in 1909, and the records show this to have taken place on June 22-26, with Fisher placed in Class II of the four classes in both Part I and Part II and in Class I for Mechanics. His five-day stay coincided with the Darwin centenary on June 22-24 and he will have had the opportunity to visit bookshops where books relevant to the centenary were certainly on display, such as R.C.Punnett's *Mendelism* (1905, 1907). (For Punnett see Edwards, 2012). This was the first text-book on the subject, small and cheap, written by a Fellow of Caius, printed and published in Cambridge. Also published was R.H.Lock's (1906) remarkable *Recent Progress in the Study of Variation, Heredity, and Evolution* with a chapter on 'The theory of natural selection' and two on 'Mendelism'. (For Lock see Edwards, 2013.) Lock was also a Fellow of Caius and his book was likely to have been on display as well. Punnett's *Mendelism* was even published by the bookshop opposite Caius, Bowes and Bowes. Capping all was Bateson's big volume from Cambridge University Press *Mendel's Principles of Heredity,* as recorded by Fisher (see below). Fisher might also have come across Bateson's earlier book with the name *Mendel's Principles of Heredity: A Defence* (Bateson, 1902*)*.

Fisher was later to reminisce about the Cambridge atmosphere:

> I first came to Cambridge in 1909, the year in which the centenary of Darwin's birth and the jubilee of the publication of *The Origin of Species* were being celebrated. 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).
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> The fiftieth anniversary of the publication of *The Origin of Species* was being celebrated, apart from other things, by the publication of Bateson's book *Mendel's Principles of Inheritance'* [actually *Mendel's Principles of Heredity*] (Fisher, 1959).
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> In 1909 [Bateson] published his *Mendel's Principles of Heredity*, a book that I bought that year as a mathematical freshman. It includes a translation of Mendel's paper on "Pisum" (Fisher, 1951).

He soon received as a College Prize *Darwin and Modern Science* 'a remarkable collection of able essays assembled by Professor A.C.Seward' (Seward, 1909) as a 'Memorial Volume' in connection with the Darwin centenary (Fisher,

1959). There is no doubt that Fisher's interests in natural selection and genetics were well advanced before there is any evidence of an interest in eugenics.

On 10 November 1911 Fisher read a paper 'on "Heredity" (comparing methods of Biometry and Mendelism)' to the Cambridge University Eugenics Society which showed how extremely well-informed he was. He mentions Weismann, de Vries, Mendel, Bateson, Darwin, Karl Pearson and Galton, and 'should certainly have included' Johannsen 'if I could have got at the original papers'. Not published until by Norton and Pearson (1976) from an uncorrected copy preserved in the Eugenics Society archives, Bennett (1983) reprinted the lecture from Fisher's own corrected copy.

This now-famous paper starts with a lecture on Mendelism. 'The simplest case is that of the blue Andalusian hen' - he seems to have been reading Punnett (1905, 1907). Soon he is showing a human pedigree for brachydactyly from Bateson (1909). Then comes a description of Pearson's biometrical approach. 'It has been shown by Karl Pearson, on whose mathematical work the whole science of biometrics has been based, that a number of pairs of Mendelian allelomorphs scattered at random in a population would serve as the independent arbitrary causes which biometricians require'. Lock's book (1906, 1909) has a section 'Mendelism and biometry: researches of Yule and Pearson' which is right up-to-date: 'The question naturally arises as to how far the Mendelian rule of inheritance agrees with or contradicts those estimations of hereditary values which have been arrived at by the labours of the biometricians'. No wonder the mathematics undergraduate just starting his third year was so well-informed.

The scientific scene is now set for Fisher's attempt to answer the question that Lock had so clearly raised. He worked on his solution in his spare time from 1915 until its completion the following year. Published in 1918 (Fisher, 1918*a*) in circumstances that have often been described, it starts with a very full account of the work by Yule and Pearson. Neither there nor in any of its 26 sections does it make any reference to eugenics. But it does end 'Finally, it is a pleasure to acknowledge my indebtedness to Major LEONARD DARWIN, at whose suggestion this inquiry was first undertaken, and to whose kindness and advice it owes its completion'. Once again, as well as the introduction to the paper, Box (1978) is essential reading, supplemented by the later Bennett (1983). As to the paper itself, it is recognized as the foundation of biometrical genetics, quantitative genetics, animal and plant scientific breeding and, in statistics, the analysis of variance (and the word 'variance').

It may not have mentioned 'eugenics' but was it of any use to the development of eugenics? It surely discussed the correlation between *human* relatives, but that was because the theory of such correlations was the subject-matter of the literature from which it sprang. How many references to this paper are there in the later eugenics literature? Even Fisher's (1930) own *The Genetical Theory of Natural Selection* with its last five 'eugenics' chapters does not reference it. Fisher (1918*b*) did write an immediate explanatory paper in the *Eugenics Review* which started 'The great service which the modern development of statistics has rendered to eugenics is that it supplies a definite method of measuring and analysing variability' but that was a nod to the readers of the journal, and the word 'eugenics' could equally well have been 'science'. Neither of the Royal Society's referees for the main paper, Karl Pearson and Punnett, mentioned eugenics in their reports, though both were heavily involved in the subject (for the reports see Norton and E.S.Pearson, 1976.)

Norton (1978*b*) wrote 'We should see it [Fisher 1918*a*]... as a stunning contribution to eugenics'. Once again, we can replace 'eugenics' by 'science'. It is no

more true that Fisher's interest in eugenics stimulated his interest in genetics than that it did in statistics. It was his youthful scientific knowledge of natural selection and Mendelism that engendered his eugenic concerns for the future of the British population and not the other way round.

Finally we may note that Fisher's eugenics (as opposed to his human genetics; see below) bears no obvious relation to his genetical research with animals and plants while Arthur Balfour Professor of Genetics in Cambridge (1943-57). Edwards (2003) has provided a summary of this, some of which Fisher had started while he was at the Galton Laboratory.

Fisher's introduction to eugenics

Evidence is absent for any interest in eugenics by Fisher before he arrived in Caius. However Punnett and Lock were both members of the London-based Eugenics Education Society. Lock presented the second edition of his book to the College Library in July 1909. Chapter X 'Eugenics' was new for this edition. It and the last part of the 'Concluding Chapter' XI give a remarkable account of contemporary views of eugenics promulgated by Francis Galton, Karl Pearson, Sidney Webb and Bernard Shaw. Their language is no longer regarded as acceptable, but 'The past is another country; they do things differently there', and we have a duty to allow for that.

In Chapter X Lock explains that eugenists seek to confront the 'dangerous differences in the incidence of the birth-rate' among the groups identified by Pearson, but quotes Thomas Huxley: 'Who is competent to do the necessary selecting?' and Lock himself adds another objection 'The ruthlessness necessary for the carrying out of the method of deliberate selection is in itself so unsocial a quality that, if it were ever to arise, society would probably be far worse off than before'. So what is to be done? Lock describes some suggestions, but 'It must not be supposed that the writer is a special advocate of all or any of the suggestions which have been mentioned above'. There was plenty for Fisher to think about. (For evidence that Fisher read the College copy see Edwards, 2013.) Bennett (1983) thought that Whetham and Whetham's (1909) *The Family and the Nation* 'seems also to have come under Fisher's early scrutiny'.

It is likely that Punnett will have told Fisher about the Society and perhaps even floated the idea that a Cambridge society might be started. Fisher and some student friends lobbied senior members of the University with the idea and in 1911 the Cambridge University Eugenics Society was formed, with substantial support among the dons. One of them was the Regius Professor of Physic Sir Clifford Allbutt, like Punnett and Lock a Fellow of Caius. Professor A.C.Seward, the Professor of Botany, was President, Fisher was the undergraduate chairman and the young John Maynard Keynes the Senior Treasurer.

Fisher's third publication was his first on eugenics. At the end of his Wollaston studentship year 1912-13 he repeated a paper 'Some hopes of a eugenist' to the Eugenics Education Society (on 2 October 1913, presumably in London) which he had given to the Cambridge branch in November 1912. It was published in *Eugenics Review* the following year (Fisher, 1914). This must be the paper which the mature Fisher (1930) was to call 'ephemeral' in *The Genetical Theory of Natural Selection*. In it he had written 'From the moment we grasp, firmly and completely, Darwin's theory of evolution, we begin to realise that we have obtained not merely a description of the past, or an explanation of the present, but a veritable key of the future'.

Separating Human Genetics from Eugenics

The early eugenists (for so they called themselves in Britain) were aware of the importance of a greater understanding of the laws of heredity even before the Mendelian era. The aged Galton himself lectured to the Sociological Society at the London School of Economics in May 1904 on 'Eugenics: its definition, scope, and aims' during which he listed five components of the 'Course of procedure'. First was 'Dissemination of a knowledge of the laws of heredity, so far as they are surely known, and promotion of their further study' (Galton, 1904).

'As early as 1924, when the Rockefeller Institute of Health was founded in London, Fisher had prepared a notice for the *Eugenics Review* "to bring to the attention of the Ministry of Health the urgent desirability of establishing a Chair of Human Heredity in relation to disease, with facilities for training advanced students in methods of research appropriate to the subject"' (Box, 1978 p.202). The previous year a similar statement had been sent by Leonard Darwin, President of the Eugenics Education Society, to the Transitional Executive Committee of the Rockefeller School of Hygiene on the Society's behalf (full statement in *Eugenics Review* **15**, 643-44, 1924).

In 1931 Major C.C.Hurst wrote to a number of the most influential biological and medical scientists in Britain inviting them to a meeting at the London School of Economics on 21 July to discuss the need for an initiative to promote research in human genetics in Britain. The initiative was successful and the Medical Research Council set up its Committee on Human Genetics, chaired by J.B.S.Haldane with further members Julia Bell (of the Galton Laboratory), E.A.Cockayne (Physician to the Middlesex Hospital), Fisher, Lancelot Hogben, L.S.Penrose and J.A.Fraser Roberts. It met on 2 March 1932, which may be taken as the day on which human genetics emerged from the shadow of eugenics in Britain. The next year Fisher succeeded Karl Pearson as Galton Professor of Eugenics at University College London. No-one thought to change Galton's chosen title or that of the *Annals of Eugenics* until after the war, by which time the word 'eugenics' was fatally wounded. For a fuller account see Edwards (2004) and *Blood Relations: Transfusion and the Making of Human Genetics* (Bangham, 2020).

In 1934 when Fisher took over the editorship of the *Annals of Eugenics* from Karl Pearson he replaced the subtitle 'A Journal for the scientific study of racial problems' with 'A Journal devoted to the genetic study of human populations'. A Foreword introducing the change explained:

> The contents of the Journal will continue to be representative of the researches of the [Galton] Laboratory, and of kindred work, contributing to the further study and elucidation of the genetic situation in man, which is attracting increasing attention from students elsewhere. The two primary disciplines which contribute to this study are Genetics and Mathematical Statistics. It will be our task to build up a mutual understanding between experts in these fields, to prevent the perpetuation of one-sided "biometrical" and "genetical" standpoints on human populations, and to provide a medium of publication for bodies of original data, and for technical advances in these two fields, in so far as they contribute directly or indirectly to the better understanding of heredity in human populations. *Annals of Eugenics*, 1934.

In 1935 Fisher lectured to the Eugenics Society on 'Eugenics academic and practical' (Fisher, 1935*b*). He had been given the title, which he did not like very much, and used his time to explain the work of his new department, from which the teaching of statistics had been detached on Karl Pearson's retirement by the establishment of a new Department of Statistics under Pearson's son Egon. In Fisher's

'Galton Laboratory', set to become famous under him and his post-war successor L.S.Penrose, the 'Aims and methods of eugenic research' were to be 'Problems of Mendelian inheritance', 'Linkage and individual prognosis', and 'Studies of serological inheritance'. The title lingered, but the 'research' was all human genetics.

We may note the subtitle of Bangham's book (see above). Two older books also describe the transition to human genetics. Kevles (1985) has a detailed account in Chapter XIII 'The establishment of human genetics'. Unfortunately in the previous chapter he repeated the common mistake that Fisher's scheme of family allowances was a 'state' scheme: 'the government would provide an allowance for each child proportional not to the family's absolute need but to its total earned income' (p.183). On the contrary, the cost was to be born not by the state but as in the pension system in the universities, by employer and employee contributions.

Kevles concluded Chapter XIII with this summary:

> From 1930 to 1945, Fisher and Haldane were the most productive pair in human genetics on either side of the Atlantic. Much of their work appeared in the *Annals of Eugenics*, a quarterly journal started by Karl Pearson in 1926, control of which Fisher acquired when he became Galton Professor. Fisher changed the subtitle of the *Annals* – under Pearson it had been a journal "for the scientific study of racial problems" – to a journal "devoted to the genetic study of human populations," and, assisted by a subvention from the Eugenics Society, he published a wide range of articles dealing with various aspects of the subject in a predominantly mathematical fashion. Between 1930 and 1945, the largest cluster of human genetic analysis – indeed, some forty percent of the work published in Britain and the United States combined – saw the printed light of day in the *Annals*. Fisher and Haldane provided intellectual guidance to the Galton staff as well as visitors who came to work at the laboratory, and, more important, through the journal they set a standard of first-class research in human genetics for scientists elsewhere to emulate.

Mazumdar (1992) also describes the emergence of human genetics sympathetically. A preliminary page tells us that 'This outstanding study follows the history of the eugenics movements from its roots to its heyday as the source of a science of human genetics'. Eugenics, so-named by Galton, contained the foetus of human genetics and in criticising it commentators need to recognize this fact.

History of Science

> Historians of science of considerable distinction have retarded the understanding of Fisher's contribution to evolutionary biology by a distracting emphasis on his involvement in the eugenics movement. They seem to have been unable to put themselves in the position of a first-rate young mathematician well read in Darwin and natural history who, just nine years after the start of the Mendelian revolution in genetics, finds himself an undergraduate at the very place where the liveliest discussion of it is taking place, and turns his mind, as Galton did when he read *The Origin of Species*, to its profound implications for mankind. Without a proper study of Fisher's scientific thinking they too readily drew the unwarranted inference that it was driven by social considerations rather than that the unfolding new science was generating the social concern. Mackenzie (1981, p. 189) thought Fisher in 1918 'sought not to *reconcile* Mendelism and biometry, but to use Mendelism to vindicate biometric eugenics' (for a critical review of MacKenzie's book see Edwards, 1981).
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> MacKenzie also quoted Norton (1978*a*) who wrote that Fisher (1918*a*) should be seen 'predominantly as a contribution to the hereditarian social ideology of eugenics' (also quoted by Bennett, 1983, p. 17). Norton thought that 'To understand Fisher's 1918 paper historically, we must surely attempt to uncover the problems that generated it' and 'Until recently, Fisher's decision to become involved in this sort of

work has remained somewhat mysterious'. Unfortunately Norton did not have available the essential evidence contained in Fisher's biography (Box, 1978) and his correspondence with Leonard Darwin together with Bennett's commentary (Bennett, 1983). But he had recently been able to consult the records of the Cambridge University Eugenics Society and in particular had found a typescript of Fisher's 1911 lecture … (Norton & Pearson, 1976). He filled the void with what he had found: 'Recently uncovered documents show that Fisher's problems were ideological rather than biological'. 'From this one might suppose that the motivation for Fisher's (1918*a*) paper was an eugenic one'. Finally, 'We should see it ... as a stunning contribution to eugenics'. It would not have been necessary to dwell on this but for the fact that it has been so influential.

From Edwards (2014*b*).

Bernard Norton, in a paper in the *New Scientist* in 1978, sought to explain 'why Fisher, a mathematician by training, chose to work on such abstruse biological issues as the inheritance of human characters, and the respective roles of genetic and environmental factors in determining those characters? ... Fisher worked as he did *because he was an ardent eugenicist* (author's italics)'. [Norton, 1978*b*]

Paper for Caius College Council meeting on 24 June 2020 from the Senior Tutor and four students of the College Student Union.

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