mechanism, including its structural part.

Here again we are confronted with remarkable achievements of Fisher. While these concern several domains of science, including earth magnetism, Fisher's preference seems to have been genetics and evolution, both out of my usual bailiwick. The general impression I formed from occasional reading is that the modern discipline of population genetics, including such authors as S. Karlin, O. Kempthorne, M. Kimura, R. G. Lewontin, G. Malécot, K. Mather, P. A. P. Moran, and a number of others, is a development that grew out of the works of essentially only three scholars of the earlier generation: R. A. Fisher, J. B. S. Haldane, and Sewall Wright. The provenance of the ideas that underlay the population genetical studies of these three research workers is likely to be quite complex and, probably, very different. However, the relevant works of Karl Pearson (*21*) and, perhaps unexpectedly, a little note by G. H. Hardy (*22*), one of the purest of pure mathematicians, seem to have been a common inspiration.

In his very interesting book (*23*), Moran refers to 28 contributions to population genetics by R. A. Fisher, either alone or with some coauthors, extending from 1918 (*24*) to 1943, and probably this list is not complete. There is no doubt in my mind that in this domain also Fisher's role was that of the founder, at least that of one of the founders, of a fruitful novel domain of human thought and inquiry.

## Concluding Remarks

As stated at the outset, the present appreciation of Fisher's scholarly work is subjective. Also it is one-sided. Both the strict subjectivity and one-sidedness are intentional.

The subjectivity of my account of Fisher's work depends on my personal scientific past and on my personal perspective. No doubt, other scholars will view the same developments differently. Also, I rather expect that Fisher himself would have disagreed with my views on a number of points. One example is the connection between Fisher's own work on experimental tactics, on the one hand, and Wald's work on experimental strategy, on the other. In fact, soon after the appearance of Wald's book, Fisher published an article emphasizing his view that Wald's theory of decision functions has no relation with Fisher's designs of experiments. In a sense, I agree. Wald's work was original work on his own, not on Fisher's problems. My point is that, if Fisher's theory of experimentation did not exist, then, probably, Wald's theory of statistical decision functions would not have been developed as it was developed. As stated by Wald himself, his thinking was stimulated by Fisher's.

Another point on which Fisher is likely to have disagreed with me is my calling him a "descendant" of Karl Pearson. Here a few comments might be useful. A "descendant" does not necessarily mean either a follower or even a student. What I mean here is that, in the early phase of his scholarly activities, Fisher was preoccupied with problems immediately suggested by Karl Pearson's writings. In fact, Fisher seems to have picked up where Karl Pearson left off, and for the history of human thought, it is this link that is significant, not the feelings that the two great scholars had for each other.

The one-sided character of the present article results from my opinion as to how an individual's scholarly activity should be judged. In several earlier writings I have pointed out that certain of Fisher's conceptual developments, not mentioned here, are erroneous. Lest there be a misunderstanding on this point, I emphasize that I continue to maintain this view. However, to err is a part of human nature and I feel that a scholar's activity should be judged by his positive achievements and, particularly, by the influence he exercised on subsequent generations. The purpose of the above outline of Fisher's work is to emphasize my personal views on his record, which is second to none.

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# Footnote by William G. Cochran

In adding a few notes to Neyman's summary and appraisal of Fisher's contributions, I would like to present an impression of my own about Fisher's outlook, and to give some personal reminiscences of Fisher.

The subject matter of statistics has been defined in various ways. I believe that Fisher thought of statistics as essentially an important part of the mainstream of research in the experimental sciences. His major books, *Statistical Methods for Research Workers, Design of Experiments*, and the Fisher-Yates *Statistical Tables* (*1*) were addressed not to statisticians but to workers in the experimental sciences. The 1925 preface to the first edition of *Statistical Methods* opens as follows (*1*): "For several years the author has been working in somewhat intimate co-operation with a number of biological research departments; the present book is in every sense the product of


The author is professor of statistics at Harvard University, Cambridge, Massachusetts. This is the text of an address delivered at the December 1966 meeting of the AAAS in Washington.

this circumstance. Daily contact with the statistical problems which present themselves to the laboratory worker has stimulated the purely mathematical researches upon which are based the methods here presented." In those days the principal departments at Rothamsted, from which Fisher was writing, were soil chemistry, soil physics, bacteriology, microbiology, entomology, insecticides, botany, and plant pathology. Except possibly for botany, he contributed to the work of every one of these. His series of papers on distribution theory, which Neyman has described, were undertaken to provide working scientists with a battery of new tools to guide them in analyzing their data.

Neyman has reminded us of the occasion when Fisher was invited to present his ideas before the Royal Statistical Society. He entitled his paper "The logic of inductive inference," but hastened to tell his readers that the title might just as well have been "On making sense of figures"—inserting this homely alternative, I believe, in case his main title might suggest a rather rarified discussion remote from the real task of handling scientific data.

Consistent with this view was his assertion that decision theory was in no sense a generalization of his ideas. He writes (*2*): " . . . the Natural Sciences can only be successfully conducted by responsible and independent thinkers applying their minds and their imaginations to the detailed interpretation of verifiable observations. The idea that this responsibility can be delegated to a giant computer programmed with Decision Functions belongs to the phantasy of circles rather remote from scientific research." These fighting words might suggest a blanket disapproval of decision theory, but elsewhere he writes of decision theory as the correct approach to a different kind of problem, acceptance sampling (*3*): "The procedure as a whole is arrived at by minimising the losses due to wrong decisions, or to unnecessary testing, and to frame such a procedure successfully the cost of such faulty decisions must be assessed in advance; equally, also, prior knowledge is required of the expected distribution of the material in supply."

In his own researches he took little interest in the problem of collecting nonexperimental data, as in sample surveys, or in the more perplexing problem of making sound inferences from uncontrolled studies, although such data abound even in the experimental sciences. It is true that his late pamphlet *Smoking—The Cancer Controversy* dealt with the pitfalls in drawing conclusions about cause and effect from nonexperimental data (*4*). However, his main contribution in that pamphlet, as I see it, was to claim that there were two alternative hypotheses, both reasonable and neither implicating smoking as the culprit, that might explain the available data on the relation between cigarette smoking and lung cancer death rates. Until data had been gathered that disproved both these alternatives, there was no justification in his view for diatribes or action against cigarette smoking. But as indicative of a rather half-hearted interest in the matter, he presented no detailed analysis to support the claim that these alternatives were in fact consistent with all the observed data, nor did he attempt to outline the types of data that would be needed for a crucial comparison among these hypotheses.

His concept of statistics may explain some of the things that irritated him—for instance, the teaching of a test of significance as a rule for "rejecting" or "accepting" a hypothesis. Like others, I have difficulty in understanding exactly what Fisher meant by a test of significance: he seems to imply different things in different parts of his writings. My general impression is that he regarded it as a piece of evidence that the scientist would somehow weigh, along with all other relevant pieces, in summarizing his current opinion about a hypothesis or in thinking about the nature of the next experiment. A passage in the seventh (1960) edition of *Design of Experiments*, inserted in order to clarify this point, reads as follows (*3*, p. 25):

> In "The Improvement of Natural Knowledge," that is learning by experience, or by planned chains of experiments, conclusions are always provisional and in the nature of progress reports, interpreting and embodying the evidence so far accrued. Convenient as it is to note that a hypothesis is contradicted at some familiar level of significance such as 5% or 2% or 1% we do not, in Inductive Inference, ever need to lose sight of the exact strength which the evidence has in fact reached, or to ignore the fact that with further trial it might come to be stronger, or weaker.

In this connection I wish that Fisher had given more advice on how to appraise "the exact strength of the evidence." I have often wondered, as I suppose does Neyman, why Fisher seems not to have regarded the power of the test as relevant, although he developed the power functions of most of the common tests of significance.

He was also unhappy, particularly later in life, at seeing statistics taught essentially as mathematics by professors who overelaborated their notation (in order to make their theorems seem difficult, in his opinion) and who gave the impression that they had never seen any data and would hastily leave the room if someone appeared with data.

Although not disagreeing, I perhaps rate Fisher's positive contribution to estimation theory more highly than does Neyman. Given the probabilistic model by which the data were generated, the concept that a specific sample contains a measurable amount of information about a parameter, the delineation of cases in which a sufficient statistic exists, the notion of the efficiency of an estimate and the development of a technique for measuring efficiency—these, although not all original with Fisher, were great steps forward. It was soon evident that these concepts were oversimplifications, applicable to only a limited range of problems, and Fisher, like others after him, struggled hard to find ways of extending their range. But despite the mass of solid and difficult research that has been done since his work, it is noteworthy how often the methods actually used nowadays in data analysis are Fisherian, or are fairly straightforward extensions of his methods.

To turn to some reminiscences, the first is intended to illustrate Fisher's ingenuity in computations, of which he did a great deal with what would now be considered very inferior equipment. When he left Rothamsted in 1933 to become professor of eugenics in London, his assistant, Frank Yates, was appointed head of a one-man department of statistics at Rothamsted, this being the middle of the depression. On the land of the Duke of Bedford at Woburn, barley had been grown in a long-term experiment on the same plots for 50 years. The director of Rothamsted, Sir John Russell, was writing a book on this experiment. In preparation for this, he engaged a young lady to do statistical calculations, but soon afterward she resigned to get married and I was appointed, partly to finish these calculations and partly to assist Yates.

Since the young lady was on her honeymoon, I had first to discover what her calculations were about. For each year she had the barley yield, $Y$, per acre and six variates $x_1 \ldots x_6$ representing the amount and seasonal distribution of rainfall. To each rainfall variate she was fitting a fifth-degree polynomial in time (years), and finding the residuals from this regression. No time trend was being fitted to the barley yields.

Naturally, I plotted each variate against time. The yields had marked trends in time; the rainfall variates showed no sign of any time trend. Her objective was now clear. The time trend in yield was being interpreted as due to slow changes in the soil, not to weather. Consequently, she wanted to remove the effect of this trend in yields by a fifth-degree polynomial on time, before regressing the residuals of yield on the rainfall variates. "But," I said to myself, "she's all mixed up. She solemnly removes the trend from the rainfall variates, which don't have any trend, and doesn't remove it from the yields which do. I shall have to start over again and do it right."

Fortunately, it was then lunchtime. During lunch I asked Yates if he had shown the lady how to set up her calculations or if she had done this herself. The answer was: neither. Before she started, Fisher had given her detailed computing instructions. When I returned from lunch I thought "Perhaps we should not condemn this young woman too hastily." A little algebra showed me the well-known regression result which I should have learned in college. The yields $Y$ have a regression on $x_1 \ldots x_6$ (rainfall) and $x_7 \ldots x_{11}$ (time). If one does not want to invert an $11 \times 11$ matrix, the correct regression coefficients $b_1 \ldots b_6$ can be obtained in two ways: (i) Regress the yields and the rainfall variates on time, and then regress the residuals of yield on the residuals of rainfall. (ii) Regress the rainfall variates on time (whether they have a real trend or not) and regress the direct yields on the residuals of rainfall—this was Fisher's method. Regressing the yields but not the rainfall gives the wrong answer.

Why had he done it this way? Although there was only one set of rainfall variates, there were ten yield variates, from plots with different fertilizer treatments. By Fisher's two-step process, removal of the time trends involved dealing with only a single $5 \times 5$ matrix. Actually, no matrix inversion was required for this step, since Fisher had constructed fifth-degree orthogonal polynomials to do the job.

A second reminiscence might be entitled "Fisher explaining a proof." In one of his lecture courses, he quoted without proof a neat result for what appeared a complex problem. Since all my attempts to prove the result foundered in a maze of algebra, I asked him one day if he would show me how to do the proof. He stated that he had written out a proof, but after opening several file drawers haphazardly, all apparently full of a miscellaneous jumble of papers, he decided that it would be quicker to develop the proof anew. We sat down and he wrote the same equation from which I had started. "The obvious development is in this direction," he said, and wrote an expression two lines in length. After "then I suppose we have to expand this," he produced a three-line equation. I nodded—I had been there too. He scrutinized this expression with obvious distaste and began to stroke his beard. "The only course seems to be this" led to an expression four and one-half lines long. His frown was now thunderous. There was silence, apart from beard stroking, for about 45 seconds. "Well," he said, "the result must come out something like this" and wrote down the compact expression which I had asked him to prove. Class dismissed.

My third experience concerns our joint project. When I went to Cambridge as a student in 1931 my supervisor, Wishart, instructed me, at the request of Fisher, to compute a table of the 1 percent levels of $z = \log_e F$ to seven decimal places for a large panel of different pairs of degrees of freedom. Fisher was doing a corresponding table of the 5 percent levels. For those who think that graduate students nowadays are exploited by their professors, I might mention that Wishart told me he expected me to be working on this table 3 hours a day, 6 days a week, and that the labor was unpaid.

My contacts with Fisher on this project went through three stages. At first, when we met, he would ask about my progress: he had started sooner and was well ahead. Then came a period when he didn't ask, so I would ask him how he was coming along: I was gaining and towards the end of this period I was ahead. The third stage is easily foreseen. I would ask him, and he would hastily change the subject. I can take a hint as well as the next man. I believe that we last mentioned the project sometime in 1936. The fourth incident exemplifies Fisher as the outraged professor. When he was professor of genetics at Cambridge I called on him one spring morning at his working quarters, Whittinghame Lodge. I was told that he had just received some upsetting news, and was walking in the garden to calm himself. The news was a report from the university committee that was to approve Fisher's proposed teaching program in genetics, to the effect that they had not yet completed their study of his proposal, and there would be a further postponement of a decision, for the seventh time as I recall it, until their next meeting in October. "Cambridge University," said Fisher, "should never appoint a professor who is older than 39. If they do, then by the time his proposal for his teaching program has been approved by the university, he will have reached retirement age."

Finally, there is Fisher, the applied geneticist. We were standing at the corner of Euston Road and Gower Street in London, waiting to cross the road on our way to St. Pancras Station. Traffic was almost continuous and I was worried, because Fisher could scarcely see and I would have to steer him safely across the road. Finally there was a gap, but clearly not large enough to get us across. Before I could stop him he stepped into the stream, crying over his left shoulder "Oh, come on, Cochran. A spot of natural selection won't hurt us."

The experience of a period of association with a genius is so exhilarating that I wish every young scientist could have it. I don't know that it helps one to become a better scientist, because relationships and results that we can discern only with great effort, if at all, seem to come in a flash to someone like Fisher. But a glimpse of what the human brain at its best can do encourages a spirit of optimism for the future of *Homo sapiens*.

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