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R.A. Fisher: A Memoir

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Since my mathematical statistics, such as they were, are the greatest thing in my life, R.A. Fisher, their finest exponent, was the greatest man in my life, though I had little personal contact with him. There were very few of us around in Fisher's early research days but all we had to do was to keep up with him. I still recall my pride at H. Hotelling's numbering me amongst some half dozen "disciples of R.A. Fisher". I have long since ceased to read, still less study, mathstat papers. There are thousands nowadays with which the conscientious young researchers have to try to cope. But my colleagues amongst them tell me that there has been no break-through comparable with that of Fisher's; this is also evident from work in applied statistics, since most of the functions still used derive from Fisher, so familiar that his name has ceased to appear in the references, a fate reserved for many of old. After the great exponents of the mathematical theory of probability (the Bernoullis, Laplace, Gauss, etc., Laplace the greatest from the statistical point of view), there was Bravais (the correlation coefficient), Karl Pearson (chi-squared and most else up to his time) and Fisher (the largest stride forward since the mathematicians of probability).

There will be no citation here of Fisher's papers, with most of which I was once very familiar. Lately, Mrs Box, Fisher's daughter, was in touch with me for information about W.S. Gosset ("Student") about whom she is preparing

*This biographical note on R.A. Fisher was written by Dr Geary in November 1982, at the suggestion of Professor John Spencer. It is not clear whether or not Dr Geary intended to publish the note, but we have decided that, after some editing and annotating, it is a natural complement to Professor Spencer's appreciation. – Eds.

a memoir. Gosset was a close friend of Fisher's. He intuitively discovered the frequency distribution of normal t, subsequently proved rigorously by Fisher, whose development of t-theory is probably still the most practically useful part of his vast output. Mrs Box has written a life of her father, even dealing bravely with his mathematical work. Also, in my time there was published a compendium of Fisher's papers edited by himself.[1] I no longer have my copy (the fate of all books one values) but I recall the reproduction of the original papers with marginal corrections by Fisher in his own hand. I do not know if a later version ever appeared.[2] Certain I am that a complete edition of Fisher's papers, possibly with a critical analysis of main trends, would still be inspirational for young researhcers, as they were for me. The social sciences desperately need a break-through and the mathematical aspect of this might well inspire young researchers. A new mathematical evangelist with something like Fisher's genius may be waiting in the wings.

The point of the preceding paragraphs is that there will be no need to deal with Fisher's life and work generally, but only with my own contacts therewith that still remain in a capricious memory.

When I was young, I had the idea (backed by some friends) of competing for the vacant chair of mathematical physics ir UCD. Though I did not know him personally, I wrote to Fisher asking for a reference in the hope that he had seen my few papers. He complied promptly and in very kind terms. I did not proceed with my candidature, for, at a late stage, I discovered that the university had an extraordinary regulation (I hope it has since disappeared) that a candidate for a chair in UCD, not already on the staff, had to accept a reduction of 25 per cent in salary to start with, reaching full salary only by annual increments after five years. I, then a married civil servant, could not afford this condition, so I withdrew. Fisher had been so kind, I thought I should explain to him. I recall a passage in his reply: "In English universities we don't believe in Bedlington terriers". I made enquiries to discover that this breed was highly inbred! He was far from this himself, starting in London, moving to Cambridge, before becoming the major property of the statistical world.

The first time I met Fisher was in his laboratory in Cambridge. I still recall the very pungent (but not unpleasant) aroma of thousands of mice for, on the applied experimental side, Fisher was a distinguished geneticist. Later, for a meeting of the International Statistical Institute at Rio, we were both guests of the Brazilian Government. We met at Rome airport for a flight

1. The work referred to here is *Contributions to Mathematical Statistics*, Wiley (1950).

2. A later version – *The Collected Papers of Ronald Aylmer Fisher* – was in fact published in five volumes by University of Adelaide Press, between 1971 and 1974.

across the South Atlantic in a Pan Am Do Brazil plane. When this arrived opposite, and very close to, the waitingroom where we were seated, a great sheet of flame ascended thirty feet into the air from one of the engines, while most of us dived under the tables. For some hours we watched in trepidation engineers on ladders repairing the engine. Presently the pilot entered the waitingroom to announce that the plane was ready for flight and to invite us aboard. There were several Italian members of ISI waiting for the flight but most of them (including Corrado Gini), understandably nervous, decided to await a plane days later – Gini arrived just on the last day of the meeting! Fisher, without a word, joined the pilot. I went with him, announcing loudly, "I have always been a follower of Dr Fisher". We had an entirely uneventful flight, except for the exchange of words described below. We sat together.

In one aspect, Fisher was what D.B. Wyndham-Lewis used to call "a great urban tease". No doubt in that spirit, near the end of the flight, he remarked, "Geary, you don't hold with those who attach importance to testing for normality, do you?" I could have rejoined "But some of your own important work was in this field". Still less was I tempted to add that there was a reference to the topic in his great book, *The Design of Experiments*, about which I felt a slight grievance.[3] During the time it went through five or six editions, I had published several papers on testing for normality but my name did not appear on the index of any edition. Saying nothing, I wrote on the back of an envelope

> "Samples (not necessarily normal) of n_1 and n_2, variances s_1^2, s_2^2, large sample variance of $\frac{1}{2}\log(s_1^2/s_2^2) = \frac{1}{4}(\beta_2-1)\left(\frac{1}{n_1}+\frac{1}{n_2}\right)$.
>
> The test is more one of kurticity than of normal population variance equality"

and handed the envelope to Fisher. (Curiously, he himself had given the foregoing formula for the normal case of $\beta_2 = 3$). After some minutes' study he handed back the envelope: "What time do we arrive in Recife, Geary?" A modest revenge for that ignoration!

I met Fisher twice in Dublin, the first time as dinner guest of Peake, a brewer of Guinness and a scholar in his own right; also present were, George O'Brien and W.S. Gosset. Crossing James's Street after dinner, alone

3. More relevantly, the same point holds for Fisher's book *Statistical Methods for Research Workers*. See John Spencer's appreciation of R.C. Geary.

with Fisher I remarked, "Dr Fisher, I have been studying your recent Proceedings of the Royal Society Paper on cumulants but I cannot understand one half page (I specified the point). I would be grateful if you would explain".[4] The only reply I got was, "It came to me on a train to Edinburgh". Later, in Cambridge, I told this story to my friend John Wishart, himself an eminent statistician, who stated at once that this was absolutely true. Fisher was visiting Edinburgh to lecture under Wishart's auspices and, emerging from the train, exclaimed something like "Eureka, I have it". Later again, I told all this to Maurice Kendall, a volume of whose great work (with Allen Stuart), *The Advanced Theory of Statistics*, had just appeared, and contained a chapter on the Fisher theory of cumulant expansion (which had come to be termed The Rules), stating that I had the manuscript paper on the subject, but necessarily omitting that crucial half page I didn't yet understand. "Even so", said Maurice, "could you let me have your paper". I do not know if The Rules have since been proved. I do know that historically these intuitions visit mathematicians of genius and sometimes remain unproved: there is Fermat's Last Theorem, H. Poincare ("it came to me mounting a bus"), Ramanujan and Fisher (as described), among others.

The second time we met in Dublin was at a dinner given by Hilda and A.J. McConnell (then professor of mathematics in TCD, later provost). Fisher, sitting next to me, quite out of the blue and with no preamble (and indeed no sequence), remarked in a low voice to me: "Geary, why don't you have a crack at the economists?" I was amazed. Fisher could not have known that all my life I had loathed one branch of economics, namely what is commonly called textual economics, that kind of elementary logic using the terms employment, output, trade, prices and the rest according to consistent, but not necessarily, accurate, rules with never a figure in their books or papers but page numbers. We econometric statisticians may not yet be able to plan the future but at least we are good at post-mortems! I was excited at inferring that so great a man as Fisher was of our company. Once or twice before, I had launched obscure attacks on literary economics, but, some years after Fisher, the assault became full-blooded in a book I edited, entitled *Europe's Future in Figures*, so vigorous as nearly to wreck the ERI (afterwards ESRI) of which I was director, because of the objections of some economic professors on the Council. There has never been an economics paper in the offensive sense of the term from ESRI.

4. This reference is almost certainly to the 1929 Proceeding of the London Mathematical Society (2), 30, pp. 199-238, a paper which Maurice Kendall describes in his 1963 obituary article, "Ronald Alymer Fisher, 1890-1962", *Biometrika*, 40, pp. 1-15. The rules referred to are the combinatorial rules which express the cumulants of k-statistics in terms of parent cumulants. They are described and proved in chapters 12 and 13 of M. Kendall and A. Stuart: *Advanced Theory of Statistics*, (3rd edition), Vol. 1.

It is so long ago that I do not clearly recall whether I was actually present or heard from someone else that at the meeting of RSS when Fisher presented his great paper entitled "The Logic of Inductive Inference", in which he proved that the estimate of a parameter maximizing the likelihood had the minimum asymptotic variance.[5] (I once knew this paper well for I generalised it to many parameters, substituting the word "generalised" before "variance" and changing "a parameter" to "many parameters".[6] The tone of many of the discussants' comments was rather crudely sarcastic, reminding one that Fisher, for all his universally recognised brilliance was what is termed "a controversial character", of which more anon. As to difficulty, most geniuses write sparingly, giving readers credit for more intelligence than we possess. In my own case I might never have understood Fisher's treatment of the 2 x 2 chi-squared case with 1 d.f. if I had not read Udny Yule's commentary soon after. (I flirted with the idea that situations with 3 d.f. were also conceivable, but *Biometrika* did not agree with me, so I abandoned the attempt.)

I had no personal part in the famous controversies between Fisher and the Pearson school (Karl, his son Egon and J.S. Neyman). I took a great interest in them, however, though very much regretting the unnecessarily vigorous, controversial tone of them. On the earlier number of degrees of freedom issue, we all agreed with Fisher, but most thought unfortunate his assailing so old and eminent a figure as Karl. Indeed, a statistician friend of mine described his obituary article on Karl in the *Annals of Eugenics* as a "jumping on the corpse".[7] I think that it was his spleen against the Pearson school that led Fisher to argue for his mystical substitute for the J.S. Neyman-E.S. Pearson confidence limits of estimate which still rule the roost, while the Fisher approach has long since disappeared.

It was my melancholy duty to announce Fisher's death in Australia at a tripartite statistical conference under ERI auspices in Dublin in 1962.[8] In a short allocution, I described him as "the greatest statistician who ever lived or is likely to live". That is still my opinion.

5. This paper, with discussion, appeared in the *Journal of the Royal Statistical Society*, 98 (1935), pp. 39-82.

6. This paper is referred to as (1942a) in John Spencer's appreciation of R.C. Geary.

7. The article referred to here is R.A. Fisher (1937): "Professor Karl Pearson and the Method of Moments", *Annals of Eugenics*, vol 7, pp. 307-18.

8. The meeting referred to here is the 24th European meeting of the Econometric Society, which was held jointly with the meeting of the Institute of Mathematical Statistics and the Institute of Management Sciences in Dublin, September 3-7, 1962. Dr Geary made the announcement of Fisher's death before reading his paper "Some Remarks about Relations between Stochastic Variables: A Discussion Document" which was published in 1963 in the *Review of the International Statistical Institute*.