9.   MY EDUCATION IN STATISTICS, RALEIGH, N.C., 1946

A six weeks session was organized by Gertrude Cox in Raleigh, North Carolina in 1946. Gertrude was above all an organizer, and a very good one, The DBS decided that I should attend that conference--talk of sampling was in the air and it was felt that someone in the Bureau should at least know what sampling was. From that meeting and what grew out of it I learned whatever little I know of the science of statistics.

Sir Ronald A. Fisher was the star of a dazzling assembly of statistical talent that Gertrude had put together. They included William Cochrane, whom I later caught up with at Harvard; Jacob Wolfowitz, a protégé of Abraham Wald at Columbia, as well as some lesser figures, but all of proven excellence in their research and teaching. Each gave a six-week course.

At that point in his life Fisher was already a legend and I was excited as we waited for him to appear in the class-room. When he did come in he gave us a lecture on genetics of certain characteristics of mice. He would pick up a mouse, hold it 3 inches away from his eyes, and examine it carefully. I speculated on what the mouse was thinking about all this.

His extreme short-sightedness, surprisingly, was the source of his mathematical prowess. For Nature compensated by an almost uncanny power of visualization. He could for example see degrees of freedom, well known in statistics, as dimensions, and when that is possible important propositions of statistics become almost obvious. I remember standing by at a later meeting when Frank Yates asked him about his power to do this. Fisher answered by describing a four-dimensional cube; Yates appeared to follow his description, but I did not; those English words, arranged according to the rules of grammar, were meaningless to me.

Note the title of Fisher's main book: Statistical Methods for Research Workers. The last three words are significant. He regarded himself as the knight who would protect the honest research worker against the wicked mathematician smothering him in formulas and diverting attention from the all-important biological aim of his research. Cramer, the Swedish mathematician, later published painstaking derivations of Fisher's results in a book thicker than all of Fisher's opus together. When asked if he had looked at Carmer's work, I remember Fisher saying ironically "Yes, now I know my formulas are right." He did not have the precious time and eyesight to read Cramer.

He was past the age when he had done his original work, and did not even claim to understand all of it. In one of the lectures to us at Raleigh he got stuck. Cochrane, who had been a student of his at Rothamsted, called out a hint, so he handed the chalk to Cochrane with the words "You can explain this as well as anyone."

I remember talking to Fisher subsequently and put to him what I thought must be a problem for those exploring for oil--a problem in identifying the location of an underground deposit. It costs money to sink a shaft, so one wants do sink as few as possible. I said one would sink a coarse grid pattern, to get a first approximation to the boundaries of the deposit, and then sink further shafts along those boundaries, and perhaps sink a third set along the more refined boundaries. Doing all of this with the minimum number of shafts is a nice problem in optimization. He applauded my (not very original) approach.
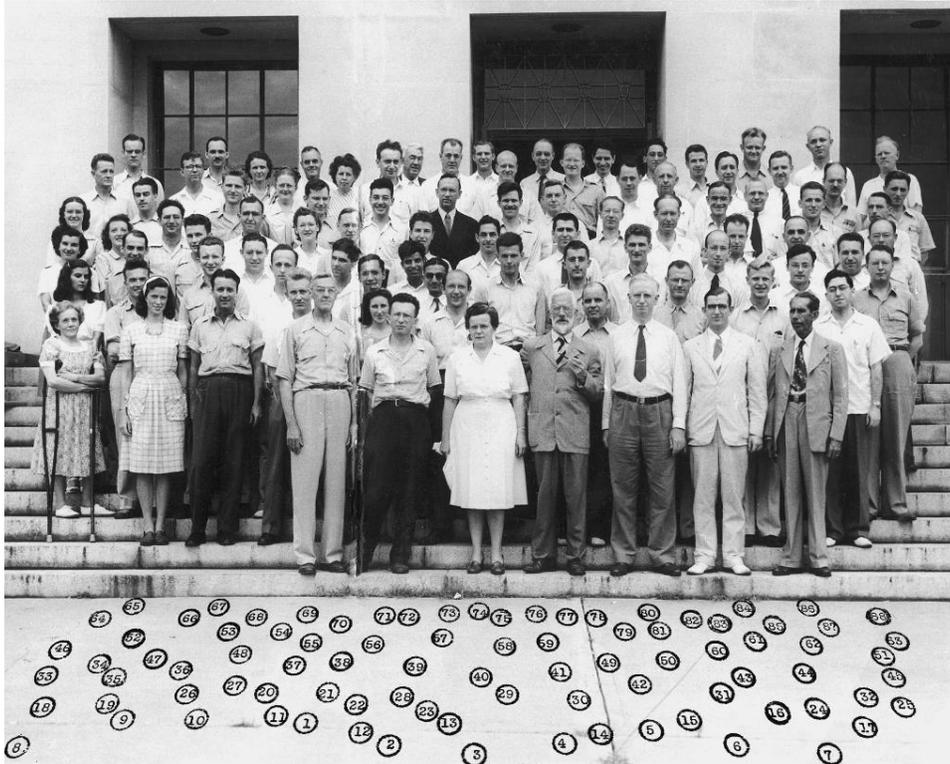
A photo of the North Carolina State University Summer Statistics course in 1946; Nathan Keyfitz is numbered 87, the second row from the back and the second from the right. Sir R.A. Fisher is numbered 4. Source:
http://www.stat.ncsu.edu/information/history/photokeys/figure02.php

While Fisher is considered the founder of modern statistics, his range was limited. All he would approve was using a test of significance to find the probability of a result occurring by chance. If the probability was less than one in twenty the paper disclosing the result was worth publishing--if only one paper in 20 in a journal contained a non-significant result the journal would be better than most. Of course for Fisher the object of research was not to publish papers, but to produce a result that research workers, farmers and other honest people could use.

What Fisher didn't want, and carried on a lifelong campaign against, was any application of Bayes' theorem. The theorem itself is mathematically correct, a tautology. What is wrong, he said, is to use it to bring in any expectation one had in advance of doing the experiment (a

priori). Does that not affect the meaning of the test of significance? The Fisherian answer was "no". Practically all other statisticians think "yes". Fisher outweighed all the others and until he passed away Bayes' theorem stayed underground. Now it is in widespread use.

From what I knew of him Fisher's unspoken fear was that admitting Bayes' Theorem would open the door to some difficult mathematics. If he could no longer understand his own earlier work, what would he make of such a powerful generalization as statistical decision theory?

I came into the argument in a small way. At the Raleigh conference Jack Wolfowitz was prominent, and he could be taken as representing the opposition to the Fisherian view. I had the simple-minded hope that bringing Fisher and Wolfowitz together might help solve the controversy, and arranged a lunch in downtown Raleigh. It was a huge un-success. Fisher would not listen, was not even polite. In fact he said at one point, rudely and irrelevantly, that these East Europeans can't be trusted. Contrary to what one might expect, the man of East European ancestry outdid in courtesy the high-born Englishman.

Fisher was not unconscious of the honors he received. I remember a reception at which the irreverent Bill Hurwitz, side-kick of Morris Hansen, saying something like "What is this nonsense about Sir" and was overheard by Fisher. Most people would act as though they hadn't heard such a remark, but not Fisher. He charged in with a sharp rebuke.

Another way in which the Raleigh Conference set me off on a new track was meeting Sam Wilks. Wilks was writing a book on mathematical statistics, that would be the very first, and I was able to procure a type-script copy, typos and all. This book, whose mathematics required diligent reading but not any more background than I had, occupied my evenings and week-ends for more than a year after. One of its benefits came some years later: it underlay the theory on which I based my dissertation.

On a later occasion Fisher visited Canada, passing through Ottawa, and we invited him to have tea at our house at 5 Bristol Avenue. Barby, then aged about 2 and precocious, went up to where he was sitting, and climbed onto his knee. She said "I like you. You look just like my bear." Fisher, having five daughters, knew how to ingratiate himself with little girls. I made a good color picture of Fisher, with his signature beard, but alas it has been lost.

In the last phase of his life, Fisher took a job at the University of Western Australia. Figuring that the University of Toronto (arguably Canada's premier institution of learning and research) could more than compete with Western Australia, Dan DeLury, Chair of the U. of T. Statistics Department, invited Fisher to teach there. I applauded Dan; Fisher would raise the image of the U. of T. worldwide. The negotiations were well under way when the news came that Fisher had passed away at the age of 64.