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Milton Friedman and Statistics

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When Milton Friedman came to study at the University of Chicago in 1932, the field of statistics was in the beginning stages of a major change. The correlational methods that Karl Pearson had helped develop around 1900 had become widely spread and reached a stage of relative maturity, but new approaches associated with the names Ronald Fisher, Jerzy Neyman, and Karl Pearson's son Egon were beginning to take root. Milton would have heard little or nothing of this new work at Rutgers, where he had studied advanced mathematics and had his first exposure to economic statistics from Arthur Burns. The situation at Chicago would not have been much different: there, in the Economics department, Milton's future brother-in-law Aaron Director taught a medium-level statistics course that concentrated on the older methods, and Henry Schultz taught a more advanced course on what would come to be known as econometrics, concentrating on time series analysis and mathematical economics, and staying mostly clear of newer work in statistics. But while Schultz (who would die in 1938 in a tragic car crash while traveling with his family) was himself only beginning to learn of the newer work, he was energetic in his correspondence and played a key role in putting Milton in touch with the one person above all others in the US who recognized the power of the new methods: Harold Hotelling at Columbia University.

Hotelling's own career had followed a path that would have been hard to forecast. After training in mathematics and journalism at the University of Washington, he had gone to the Princeton University mathematics department for graduate work, initially intending to focus upon mathematical economics and statistics. But Hotelling discovered that those were unfamiliar topics in that department, and he ended up writing a dissertation far removed from those subjects, in abstract topology. With that background he then took the unlikely step of accepting a position in 1924 at Stanford University in the Food Research Institute, forecasting crop yield among other things. At Stanford, Hotelling blossomed as a researcher in economics and statistics of the first rank. Before he moved to Columbia University's Economics Department in 1931, he had held positions in both the Mathematics and Economics Departments at Stanford, and he had begun a sequence of publications in transportation and resource economics, as well as in multivariate statistical analysis, that were among the most influential of that period. Hotelling became aware of R. A. Fisher's work in statistics as early as 1926, and he understood it probably better and certainly earlier than anyone else in the US. Thus, when Milton moved to Columbia to study mathematical economics with Hotelling for the 1933-34 year, supported by a fellowship Schultz had arranged, Milton was simultaneously going to work with the nation's foremost mathematical economist and its foremost mathematical statistician. The fruits of this proximity were not long in ripening.

In 1936, Hotelling published (with Margaret Pabst) a paper on rank correlation methods that was to prove quite influential. Briefly put, what Hotelling and Pabst investigated was an earlier measure that Charles Spearman and Karl Pearson had proposed, where in studying the correlation of measures of two characteristics, such as the height and weight of a group of people, you might consider replacing the measurements by their "ranks": the smallest height would be denoted "1", the second smallest "2", and so forth, similarly for the weights, and then you would calculate the correlation coefficient for the two sets of ranks as a measure of the strength of the relationship between the characteristics. One virtue of this use of ranks instead of raw measures would be that the analysis would not be sensitive to distributional assumptions (such as that the separate measurements followed normal distributions); another benefit was the relative ease of calculation for statistics based upon small integers in that pre-computer age. Spearman and Pearson had given rough standard deviations for the rank-based measure; Hotelling and Pabst provided a more thorough and accurate analysis that allowed for the use of the rank correlation for small and moderate samples. It was an important contribution to the early study of what came to be known as nonparametric statistical methods.

Milton's earliest and still his most influential article in mathematical statistics was directly inspired by the Hotelling-Pabst publication. The article appeared in the December 1937 issue of the Journal of the American Statistical Association under the title "The use of ranks to avoid the assumption of normality in the analysis of variance." There Milton described how the idea of replacing numerical observations by ranks could be extended to the two-way analysis of variance, a computationally intensive set of methods R. A. Fisher had introduced a decade earlier for comparing a set of mean values in the presence of other sources of variation. Consider a two-way table of measurements, cross-classified in rows and columns; for example, a table of British deathrates classified by year (columns) and county (rows). The statistician might wish to test for the existence of year-to-year variation, but any analysis that ignored the important county differences in rate due to demographic factors would find that county variation would swamp the milder year-to-year variation and make the year effect undetectable. Fisher's analysis of variance was essentially a generalization of the two-sample paired t-test: he would subtract each county's mean from the rates for that county, and then compare the variation among years in these adjusted rates with an estimate of the variation due to chance alone, under the hypothesis that the rates were distributed according to a normal distribution. Milton's procedure separately ranked the data within each county and then combined these ranks into a simple statistic that was easily computed, easy to assess for statistical significance, and free of the assumption of normality.

Milton's article was and is a model of clear statistical exposition. The use of his test spread widely across the social and behavioral sciences. Even today, when the question of computational ease is not a serious issue for most statisticians, "Friedman's Test" is a part of nearly every major statistical computing package in the world. In a 1939 correction note in the same Journal (Volume 34, page 109) he pointed out that a square root sign had been dropped by the printer at an important place on page 695 of the original article, and in a 1940 article in the Annals of Mathematical Statistics he studied the comparative performances of his test and some competitors, but otherwise there has been little advance upon his original treatment in the past seven decades, a remarkable feat.

Milton's interest in statistics found other outlets during that period. For example, when Jerzy Neyman visited the United States in April 1937 to give a set of influential lectures at the Graduate School of the U.S. Department of Agriculture in Washington D.C., Milton was not only in the audience, he also assisted W. Edwards Deming in preparing the lectures for publication, and as that publication shows, contributed some of the most perceptive of the audience's questions. With this background, it is not surprising that in 1942 he was recruited to help in the war effort as part of the Statistical Research Group at Columbia University.

Harold Hotelling and W. Edwards Deming had played key roles in the formation of the Statistical Research Group, and Hotelling was nominally the Principal Investigator, but to all intents and purposes it was run by the man Hotelling recruited as Research Director, W. Allen Wallis. Allen knew Milton from Chicago, and Milton joined the SRG shortly after its formation as one of a group of Associate Directors. Wallis (1980) tells the story of the SRG and Milton's important contributions. In terms of publications, Milton's largest effort was devoted to writing a major portion of the sampling manual the SRG produced and published after the war (Freeman et al, 1948). In terms of time on war research, Milton emphasizes in the memoir Two Lucky People the efforts he devoted to helping to develop a successful proximity fuse for anti-aircraft fire.

Milton's work on the proximity fuse and related problems led to three of the articles included here, all published in the postwar compilation Eisenhart et al (1947). All are involved with the design of experiments of the sort that arise in industrial research and production. The first of these is today chiefly notable for bringing cost into statistical planning. This was not entirely novel – C. S. Peirce had written generally on the economy of research a half century earlier – but the particular problem and the details of implementation considered were new. The second [6] treated a problem that was quite old, involving a method used by Fechner in 1860 in experimental psychology that is now usually called probit analysis or quantal response, and as Milton notes in the article it was widely used in medical research. But as a technical problem in experimental design it had not been previously considered, and because it was a nonlinear design problem, it did not fit the class of problems R. A. Fisher had treated in his 1935 classic book on the subject. Although the analysis Milton presented treated a narrow formulation of the problem and has since been superceded, the exposition's clarity still has much to recommend it to present day readers. The third of these, written jointly with Jimmie Savage, is really a philosophical discussion and an outline for a vastly important area of industrial experimentation. It introduced and argued for the idea of sequential experimentation for production optimization, and there was to be a huge literature to follow on this with new methods of implementation, much of it the work of George E. P. Box.

While he was with the SRG, Milton, with Allen Wallis, played a vital role in the creation of sequential analysis. As Wallis (1980) tells the story, they were prodded by a question from a Navy Captain, who suggested that some sampling problems could be better dealt with by adjusting the sample size as the sampling proceeded, rather than sticking rigidly to a predetermined sample size, as was then the practice, and as seemed in a superficial analysis to be dictated by statistical theory. Wallis and Friedman took to the idea almost immediately and developed a few ad hoc procedures that indicated it could be done, and then they attempted to enlist some of the more theoretical mathematicians in the group to show that a sound statistical principle could justify a sequential approach. They encountered initial resistance, but soon after they convinced Abraham Wald to take up the problem, Wald managed to find just such a theoretical justification, and he went on to develop the sequential probability ratio test and a new field of statistical analysis was born.

Milton's own work on sequential analysis was at the time limited to contributions to restricted SRG memoranda. However, for a 1960 festschrift for Hotelling, he drew a 1945 result from his files and, working with Ted Anderson, developed it into a paper on a sampling plan that was for the binomial problem in a sense superior to Wald's sequential probability ratio test.

Towards the end of the war, Milton wrote to my father, who had returned to the University of Minnesota and was trying to arrange a position there for Milton. Milton's letter suggested that he would continue, at least initially, with statistical work if he were to go to Minnesota:

467 Central Park W.
New York 25, NY
May 19, 1945

Dear George:

Many thanks for your (no) progress report. Sorry though not too surprised at your report of squabbling. The variability about the mean seems remarkably small - one university faculty, even the best, is pretty much like another.

Two points occurred to me that might be useful to you - though probably neither is since both are on the rational rather than practical level. Both are perhaps directed at Kozelka more than the others.

1. Sequential analysis has, as you know, been declassified. Both theory & applications will appear in some form or other in the near future. Its main application has been in industry, as you know. I would have rather special competence to teach sequential, &, if I came to Minnesota, Minnesota could be one of the first to reveal the secret weapon to an eager public.

2. As you know, I have been working on a manual on sampling inspection for the Navy. In that connection, I've had a chance to learn a good bit about quality control & acceptance inspection. It has occurred to me that business schools have been missing a golden opportunity. Quality control & the like have been monopolized by engineering schools. Largely as a result, I think, it has been very poorly developed along rather arbitrary and simple lines. It seems clear, however, that it pretty definitely involves economic considerations as well as technical considerations. It's a rather nice economic problem to try to figure, only for one example, what kind of an OC curve a business firm ought to buy; or what multiple of σ they should use in setting control limits. Technical considerations do enter in - statistical & engineering. But they are, like in other economic problems, simply given data. The field could accordingly be developed at least as well in a business school as a branch of business management as in an

engineering school. The business school that first takes it up, & gets someone to develop it along economic lines could, I think, make a killing.

I have developed some interest in the problem & would not be adverse to giving a quarter course or so in it. At the same time, it is obviously not something I would have any interest in for any long period. Consequently, I should just as soon not be committed to working in the field.

If you think, however, that it is the kind of consideration that might cut some weight, I would have no objection to your using it.

The first point, on sequential, you might well know of your own accord. The second is a bit more ticklish. If you want to attribute it to me, you might say I mentioned it in conversation - or something like that. You will know better than I how to put it, though.

Nothing much new on the New York front. As you know, a possible fight is brewing between Fry & us over sequential applications.

How's the family? Our's is fine.

Many thanks,
Milton

If we recall the enormous impact the quality assurance process known as "Six Sigma" has had on industrial production over the past decade, there may seem to be a prescience to the sentence in the May 19, 1945 letter that says: "It's a rather nice economic problem to try to figure … what multiple of σ [a business firm] should use in setting control limits." But then, in the spirit of 1945 thinking on such matters, Milton probably would have kept to what is still often a sensible 3σ limit! Hammond and Hammond (2006) reprint this letter and others of that period.

Milton did go to Minnesota for one year, and he taught one course in statistics. His active interest in how statistics should be taught in universities had earlier surfaced while he was a visiting Instructor at the University of Wisconsin in 1940-41. There he wrote a report advising the university on the teaching of statistics, commenting on the deficiencies of the existing arrangement. In mid-May 1941, a proposal to promote Milton to Associate Professor was opposed my several members of the faculty, and the controversy boiled over in the newspapers, where the last paragraph of his report was reprinted and described as endorsed by the graduate assistants. In the end, Milton withdrew his candidacy. After the war, he returned to the topic when Hotelling succeeded in getting the Institute of Mathematical Statistics to form a committee on teaching. Milton played a major role in drafting their report in 1948, which had a significant impact in encouraging the formation of a large number of statistics departments at American universities over the following two decades. But in Milton's subsequent work, developing statistical methods was at most a secondary goal.

Milton's major investigation of the monetary history of the United States with Anna Schwartz did yield one long methodological paper in 1962. In the process of studying a vast number of bank records, usually recorded at different, incommensurate times, they had constant need to interpolate values in time series using all available evidence. Based on this experience and a thorough analysis of the properties of different interpolation schemes, Milton put together what amounts to a practical manual for any other investigator faced with similar problems. It remains a clear and well-conceived set of principles, although the huge development in economic time series techniques since 1962 renders them less needed now than then.

Milton's impact upon statistics as a discipline has been significant, but the impact of his knowledge of statistics upon his work in economics has been even greater. Since that work is discussed elsewhere, I shall only underscore one example, also related to Hotelling. When Milton spent the year 1933-34 at Columbia University, it was precisely at the time Hotelling published a review of a book by Horace Secrist entitled <u>The Triumph of Mediocrity in Business</u>. Hotelling's devastating review appeared in December 1933 and the follow-up correspondence with the devastated author was published in June 1934, both in the <u>Journal of the American Statistical Association</u>. I have discussed this episode in detail elsewhere (Stigler, 1999, Chapters 8 and 9), but the outline is as follows. Secrist had gathered a huge amount of data about firms' profitability over the 1920s, and arranged them in a form that seemed to show there was a tendency for both good firms and bad firms to become more mediocre over time, with decreasing variation in profitability to boot. Capitalism was apparently doomed. But Hotelling pointed out that Secrist had been duped by the regression phenomenon discovered in the 1880s by Francis Galton. There was no such tendency at all, and an equally valid but different display of exactly the same data Secrist compiled would have shown movement <u>away</u> from mediocrity and <u>increasing</u> variation. Secrist's entire book was a monumental blunder.

This episode had a deep and lasting impact upon Milton – more so than upon Secrist, who never really got the idea. First of all, Milton's clear understanding of the phenomenon made him acutely sensitive to others' blunders on this over the entire span of his career. In one of his earliest book reviews (Friedman, 1939) he observed an instance of the fallacy, describing the error as having been "frequently pointed out but nonetheless continually being made." Over a half-century later he made a similar observation on the error in work by more recent eminent authors (Friedman, 1992). But second and more importantly, it colored the way Milton viewed variation in virtually all subsequent work. The regression phenomenon is at bottom a simple consequence of a variance component model, where chance variation is viewed as due to two components, one transitory and one persistent. Milton's insight in seeing how revealing this idea could be if applied to the study of consumption was absolutely key to his theory, as Laidler has pointed out using different terminology.

Up to 1945 Milton was both a statistician and an economist, and his work was fairly evenly divided between the two fields. Sometimes he was both in the same article, as in his thoughtful essay with Allen Wallis in 1942 on the challenges in making empirical determinations of indifference curves. But as he wrote to me in November 1976, "My high point as a mathematical statistician was VE Day in 1945." After 1946 he was really simply an economist, albeit one with an acute statistical understanding underlying much of his work. Had he taken a different route in 1946, he could well have become one of that century's major statisticians, although that was apparently never an option he seriously considered. Nonetheless the work he did in statistics, some of it important and influential, was all of a very high quality, and understanding the nature of his work in statistics is essential to an understanding of the development of his major contributions to economics.

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