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The History of Statistics in 1933[1]

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I wish to place before you this evening an unusual proposition, namely, that Mathematical Statistics began in 1933. There are two reasons I describe this as an "unusual" proposition: first, it is unusual because of its precision in specifying a single year—claims in intellectual history are seldom so specific; and second, I expect you will be surprised by the lateness of the year I give, 1933. Indeed, some of you with a knowledge of the history of statistics may think this proposition is absurd, that it cannot be true. You might argue, after all, that many of the most important intellectual landmarks of our subject, such as the Gauss–Markov theorem and the central limit theorem—or even the Cauchy distribution (which raises havoc with both of these)—date back to more than a century before 1933 (Stigler, 1986). And I admit that if my proposition is stated so baldly, it is an absurd claim, so let me qualify it somewhat. First, in placing the birth of Mathematical Statistics in 1933, I mean this only as a "point estimate"—to be more accurate, I should place a confidence interval around that date, or a credibility interval, depending upon your statistical persuasion. But the interval would not be a large one, so I shall speak of 1933 without emphasizing its imprecision. And second, and more important, I do not refer to the birth of all the *concepts* that make up modern Mathematical Statistics as we now know it. I could not mean that—some of these go back centuries, and we are still in a stage of exhilarating development. In that sense Mathematical Statistics is not even fully born yet! I refer instead to the birth of Mathematical Statistics as a separate field of study, as a set of goals and standards, of problems and techniques that were no longer simply a subset of those of the communities of mathematicians or statisticians generally. In short, the birth of Mathematical Statistics as a *discipline.*

Now, even with this qualification—that I am speaking of Mathematical Statistics as a distinct field, as a set of shared pursuits of a group of scholars—I still expect some unease about my proposed birthdate of 1933. Why 1933? Was there a single magical event that year that can be cited to justify the choice? Well, there were in fact several magical events that year, but I would not cite any of them as more than secondary supporting evidence for my proposition. For examples I could note that in 1933 Neyman and Pearson published the most important of their series of papers that was to lead to one of the most prolific and influential schools of Mathematical Statistics, "On the problem of the most efficient tests of statistical hypotheses" (Neyman and Pearson, 1933). And it was the year of Kolmogorov's *Grundbegriffe der Wahrscheinlichkeitsrechnung* (Kolmogoroff, 1933), that is, *Fundamental Concepts of Probability Theory* (but sometimes translated as *Foundations of the Theory of Probability*). Surely these works would be a part of the New Testament, or even the Gospels, of Mathematical Statistics? These were truly remarkable works, but I will not make that claim. Mathematical Statistics had no *Principia.* We should not ignore these works, but we should not present them as sacred texts.

Indeed, it is hazardous to judge the growth of disciplines by a few great works, works necessarily judged as great at a historical distance. Contemporary judgments of such works were often quite different—for example, Bayes's paper was not a great paper until the 20th century. And even if we adopt our modern perspective of these works, we will miss a lot by focusing on them exclusively. I suggest we could liken the history of statistics to a geological terrain. From a distance we may only see a few mountains. Seen from different vantage points, we can form contradictory views as to which features loom largest. For example, depending upon where you are, the landscape may be dominated by Mount Neyman, by Fisher's Peak, by the Tukey Range or even by the Snedecor Plains. But from any vantage point we cannot easily judge the height of the plateau from which the peaks rise or the depth of the valleys between them. Did you know, for example, that there were by 1933 a large number of attempts at an axiomatic foundation of probability theory within the framework of set and measure theory? In the same year as Kolmogorov's masterpiece, Erhard Tornier published the forgettable "Grundlagen

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der Wahrscheinlichkeitsrechnung" ("Foundations of probability theory") (Tornier, 1933), and there were others of the era, such as by the remarkable Russian, Serge Bernstein. Probability has many foundations—not all equally solid. Tonight I want to explore a little of that less visible terrain by way of making my case.

As background for my brief exploration of this terrain, let me remind you in outline of our—the IMS's—institutional history, and then try to explain through one extended anecdote why what happened, happened when it did.

Mathematical Statistics as a field is not identical with the Institute of Mathematical Statistics, but they are coterminous and highly correlated. The story of the IMS is, at least in broad outline, moderately well known (see particularly Hunter, 1996; also Craig, 1978). The University of Michigan mathematician Harry C. Carver founded the *Annals of Mathematical Statistics* in 1930, loosely under the aegis of the American Statistical Association (ASA), with modest financial support from the ASA. The preface to the first issue was written by the Secretary–Treasurer and future President of the ASA, Professor Willford I. King of New York University. (Remember that name, Willford I. King; he will return later in this story.) In that preface, King boldly claimed that the ASA had been in the vanguard for 91 years, and in order to remain there, they needed to include the increasingly complex mathematical techniques that were then being introduced. Willford King stated, "For some time past, however, it has been evident that the membership of our organization is tending to become divided into two groups—those familiar with advanced mathematics, and those who have not devoted themselves to this field. The mathematicians are, of course, interested in articles of a type which are not intelligible to the nonmathematical readers of our Journal" (King, 1930). King predicted that the *Annals* would help serve both groups, and he expected it to include both theory and applications.

Those early *Annals* appear today a bit quaint, filled for the first few years mostly with review articles (all with handwritten formulas), unending pages of formulas for moments and semiinvariants of various statistics and a few reprinted articles from other sources. The original articles that did appear were a curious mix. Articles that we recognize today as of great historical significance, like Harold Hotelling's 1931 *Annals* paper, "The generalization of Student's ratio" (Hotelling, 1931), were exceedingly rare—in fact, I have just named them all. More typical was a cute little 1933 simulation study by Selby Robinson, "An experiment regarding the χ^2 test" (Robinson, 1933). Robinson's simulation (based on coin tosses) verified for a simple example that Ronald Fisher had indeed been correct in his correction of Karl Pearson regarding the degrees of freedom when parameters are estimated.

If all had been allowed to develop naturally, I can imagine that the *Annals* would have gone on to be, roughly, the equivalent of *JRSS Series B*, a separately published Theory and Methods Section of *JASA*. In that event, it is a matter of conjecture whether or not there would be a discipline of Mathematical Statistics today. But as often happens in such stories, fate stepped in, this time in the form of the Great Depression.

In 1933 the ASA came under the same overriding concerns for budget that have recurrently plagued it ever since, and in December of that year the same Willford I. King (who you will recall had endorsed the *Annals* in 1930) led the move to strip the *Annals* of its meager ASA subsidy. King had done an early form of a spreadsheet analysis and claimed that half the cost of producing the *Annals* was being subsidized by non*Annals* subscribers—as he put it, "members, most of whom are not specialists in mathematics, and hence find the articles in the *Annals* not particularly adapted to their needs" (Hunter, 1996). In fact, King's bookkeeping was faulty—his budget included a salary for the *Annals* Editor when none was being paid (nor, as a matter of principle, has a salary ever been paid to an IMS Editor), and he assumed there would be no loss of membership in ASA with the demise of the *Annals*. But the hero of the day was Editor Harry C. Carver, who in January 1934 took over the *Annals* at his own expense and maintained it without institutional base or support.

By October of 1934, Carver had evolved an idea for an association of mathematical statisticians as a base for the *Annals*, and despite his earlier experiences he approached the ASA again, to see if he could arrange for such an association within the ASA. The ASA was interested, but in the end the interest was insufficient. On the one hand, the ASA did not want a new organization dedicated to statistics to start without their involvement, nor, on the other hand, in the words of their President Frederick C. Mills, an economist at Columbia University, did they want to encourage the establishment within the ASA of "a movement which [might] tend towards the disintegration of the Association" (Hunter, 1996). Given his past experience with ASA, Carver was reluctant to pursue an affiliation further, and he and a number of like-minded mathematical statisticians, particularly the University of Iowa's H. L. Rietz, moved forward on their own. The IMS was officially organized at a meeting at Ann Arbor on September 12, 1935, with H. L. Rietz as President, Walter Shewhart as Vice-President, Allen T. Craig as Secretary–Treasurer and the three original voting Fellows—a sort of membership committee—being Burton H. Camp, Arthur R. Crathorne and Harold Hotelling. They designated the *Annals* as the official journal of the Institute. Later, in 1938 the IMS took over full financial responsibility for the *Annals* from Carver.

That, in a nutshell, is the institutional history. It tells what happened, it shows that indeed 1933 was a crucial year—it was the year that ASA moved to cut their affiliation with mathematical statistics. But it does not explain "why." Well, I am afraid that the time available tonight is too short to tell you the full story of "why." If we had more time, I would tell you about events that were happening far away from Ann Arbor, Michigan, that played a crucial role—about how in 1933 Karl Pearson retired from his professorship, Ronald A. Fisher was appointed as Pearson's successor and Jerzy Neyman wrote to Fisher asking for a job. Fisher sent a cordial and encouraging reply (Bennett, 1990). And I would tell you in careful detail how at this time Egon Pearson came upon the work of Walter A. Shewhart on quality assurance and, becoming immersed in Shewhart's philosophical approach, was led to think of testing problems, and how, when Egon was set up in a separate University College London department as a rival to Fisher, he found himself, unlike Fisher, with a vacancy that he could offer to Neyman (Box, 1978). And perhaps I would tell you of the quite consequential rebuff that Fisher delivered to Sam Wilks when Wilks, then studying in England, tried to publish his paper on the independence of the sums of squares in the analysis of variance in the Royal Society's *Transactions* (Bennett, 1990). In 1938 Wilks succeeded Carver as Editor of the *Annals* and appointed a stellar editorial board, consisting of Fisher, Neyman, Cramér, Hotelling, Egon Pearson, Darmois, Craig, Deming, von Mises, Rietz and Shewhart. Sam Wilks edited the *Annals* for a dozen years, and he transformed the *Annals* into the most influential statistics journal in the world. And I would remind you of the flurry of activity in other statistical capitols, such as the fact that P. C. Mahalanobis founded *Sankhyā* in 1933. But most importantly, I would discuss the growing importance of statistics in science and industry, and how these demands put intolerable strains upon the creaky methodology of the past, leading to questions that only mathematically trained statisticians could answer, and exposing nonmathematical statisticians to unanticipated folly.

But I do not have time to tell you all those things. And so I will settle for one story, one episode from 1933 that tells much about both the plateau from which the edifice of Mathematical Statistics rose in (or about) that year, and about the intellectual tensions that contributed to its construction.

This brings me to Horace Secrists. I expect that not many of you know much about Horace Secrist. He was born on October 9, 1881, so when 1933 began he was 51 years old, at a peak of ambition and international fame. Secrist is an appropriate figure to memorialize at this year's meeting in Chicago. In the fateful year 1933, he was a Professor of Statistics at Northwestern University, well supported by that University as head of their Bureau of Business Research. He was also the ASA representative to the program committee for the summer meeting that year.

For 10 years prior to 1933, Secrist and his associates had been at work on a study of business conditions, a study that had taken on new urgency with the arrival of the Great Depression. You can well imagine his excitement. In a time of national—even international—economic calamity, he was in a unique position to diagnose the ailment and prescribe a cure. And, as luck would have it, he *did* make a phenomenal discovery, a law of economic activity that traced the nation's problems to the very fact of unfettered economic competition. Naturally, he moved to publish this momentous discovery, in a book that appeared in 1933—with a preface dated January 1, 1933. Secrist's book was scholarly and immensely detailed. Its 468 pages included 140 tables and 103 charts, all carefully documented and clearly explained. Yet for all this statistical structure, there is still a sense of passion to the work—restrained passion, but passion nonetheless. Like Darwin in his *Origin of Species,* Secrist had assembled a huge body of evidence, all supporting a surprising general law, one hitherto unknown. And like Darwin, Secrist restrained his claims, while still letting the reader know that the author was not at all uncertain about the scope and importance of his accomplishment. Even the physical size of Secrist's book is about the same as Darwin's. Why then, you might ask, is Darwin revered today as a great scientist, while Secrist, if known at all, is likely to be thought a fool?

Secrist's book was titled *The Triumph of Mediocrity in Business* (Secrist, 1933), and in it he announced and documented a startling discovery about the behavior, over time, of human economic activity. He stated his fundamental conclusion as follows: "Mediocrity tends to prevail in the conduct of competitive business .... Such is the price which industrial freedom brings" (Secrist, 1933, page 7). Not only does mediocrity prevail, he found that things were getting worse—American business was actually converging toward mediocrity! Secrist's evidence consisted of a large number of industrial time series. For example, he had data on 49 department stores' profits over the decade 1920 to 1930, where his measure of profit was the ratio of net profit or loss to net sales. He traced the fortunes of the stores over time, to see how those fortunes responded to initial economic success or failure. Accordingly, he divided the 49 stores into four approximately equal groups: the 25% stores with the highest 1920 profits, the 25% percent stores with the lowest 1920 profits and the two intermediate quartiles.

He then followed these groups—or rather the group averages—over time, and found a remarkable tendency for convergence toward the overall average—toward mediocrity. He looked at the data in every way he could think of—the phenomenon was not a 1920 phenomenon; there was a tendency toward mediocrity in each group from any initial year, and a steady decline in the variance of the group means. Those stores with higher than average profits showed a decline, those with lower than average profits initially showed an increase, and the more extreme groups showed the greater movement. Secrist cited Francis Galton and expressed this conclusion in terminology Galton had applied to processes of inheritance: "Both expenses and profits approach the mean, or to use Sir Francis Galton's expression, "'regress to type'" (Secrist, 1933, page 3).

This was a remarkable discovery, and its implications for the international economy were obvious. But I can hear you saying "perhaps this is just a statistical accident? Or a statistical artifact?" These questions occurred to Secrist as well, and, careful investigator that he was, he looked into both. First, this was not simply a phenomenon involving department stores: Secrist investigated a grand total of 73 separate series from as varied enterprises as groceries, hardware stores, railroads and banks, and the results were always the same: regression to type was a universal rule for American business! But what about statistical artifact? Could it be that all series—not just economic series—behave in this manner? Secrist considered this possibility too, and here too the answer was negative. He looked at a 10-year series of average July temperatures in 191 American cities, grouped in just the same way as the economic data, and found stability—no regression. He wrote, "Despite the relatively wide dispersion of the [temperature] rates at a given time and high positive correlation of them in the first and second years, regressive tendencies do not prevail" (Secrist, 1933, page 426). His conclusion was that regression only held where competitive forces held sway, and the outcomes were under human control.

The initial reactions to Secrist's book were favorable. The Royal Statistical Society published a short synopsis of the findings and added, "One cannot withhold a tribute of admiration for the author and his assistants for the enthusiasm and pertinacity with which they have carried to the end an extremely laborious task" *(J. Roy. Statist. Soc.*, 1933, **96** 721–722). The *Annals of the American Academy of Political and Social Science* (Riegel, 1933) praised the work as "unusually careful," and added that "The results confront the business man and the economist with an insistent and to some degree tragic problem." The *American Economic Review* (Elder, 1934) called the work "thoroughly scientific," praising Secrist's objective statistical approach, commenting that "Such an approach is a welcome change from the frequent theoretical discussions which pile one unproved assumption upon another." And Willford I. King (you do remember him!) reviewed the book for the *Journal of Political Economy,* stating that Secrist's charts and tables "establish conclusively the validity of all the findings," adding that "the book reflects in a most creditable manner the painstaking, long-continued, thoughtful and highly successful endeavor of an able statistician and economist to strengthen our knowledge of the facts and theory of competition" (King, 1934).

But Secrist's complacency was shattered when he read the review in the *Journal of the American Statistical Association*. That review was by Harold Hotelling. Like Secrist, Hotelling had Chicago connections—Hotelling had studied at the University of Chicago in 1920, and he was to return there 35 years later to receive an honorary degree.

Hotelling was polite, but he pulled no punches in what must be described as a devastating review. He gave a lucid explanation of the regression phenomenon, a phenomenon that Secrist clearly did not understand despite his citation of Galton and his adoption of Galton's terminology. Hotelling noted of Secrist's conclusion, namely that business tends toward a stable mediocrity, that "if true in the sense in which the reader naturally interprets it, would be of immense importance." But it was not true in that sense. He wrote, "If the [business] concerns were arrayed according to the values taken by the variable in the last year of the series [instead of the first year of the series], the lines would diverge. Thus from the same data one may demonstrate stability

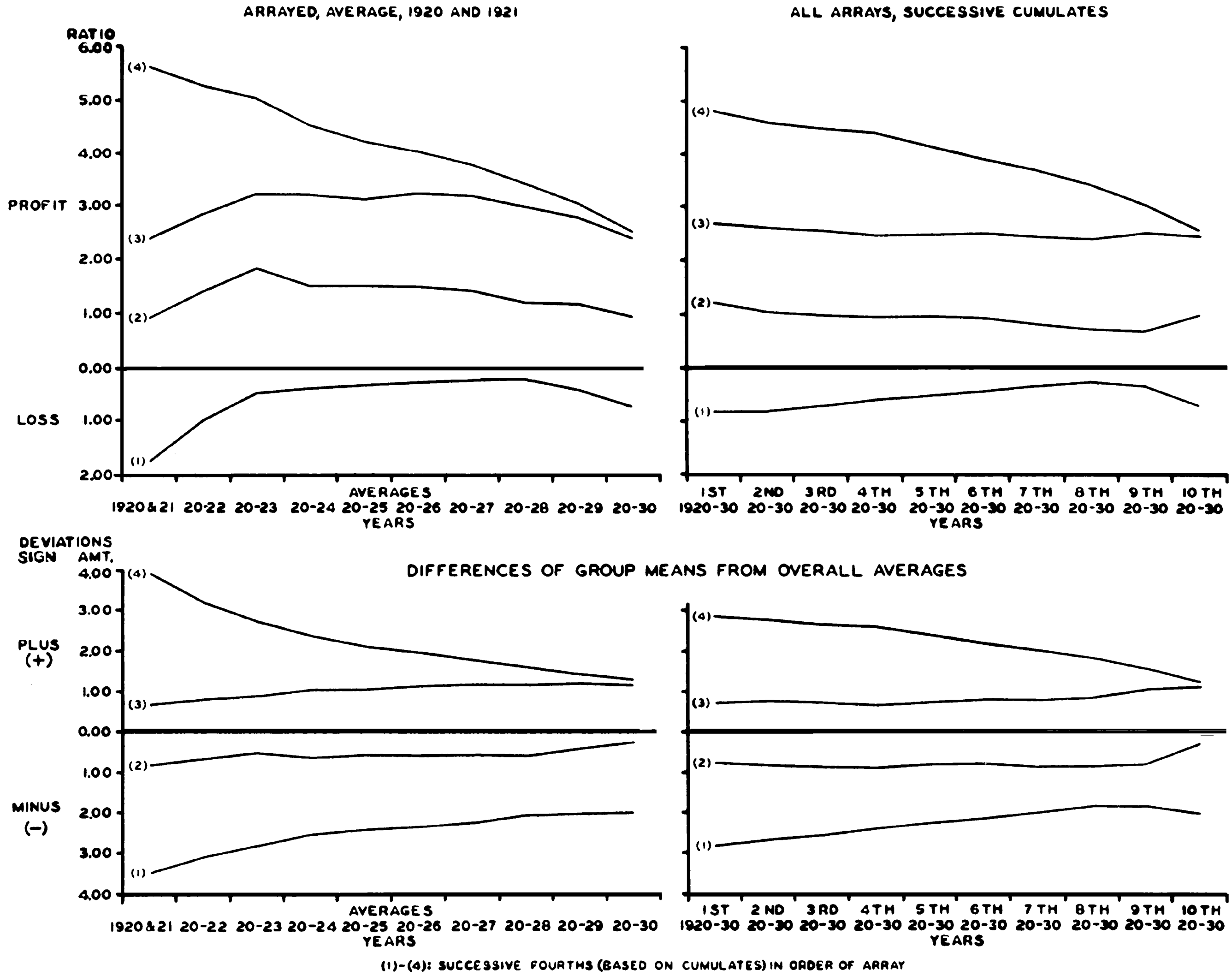

FIG. 1. *One of Secrist's* 104 *charts: the lower left-hand panel shows the trend in group means over time of* 49 *department store's profits, grouped according to which quartile they belonged in 1920, with the overall yearly averages subtracted. Note the regression toward the mean* (*Secrist, 1933, page 176*).

or instability according to taste. The seeming convergence is a statistical fallacy, resulting from the method of grouping. These diagrams really prove nothing more than that the ratios in question have a tendency to wander about" (Hotelling, 1933). What of the lack of regression among the 191 cities's temperatures? Hotelling wrote, "But this means merely that the cities do not move about, while business ratios do." If there were to be a true convergence, Hotelling stated, then the series should show a decrease in variance over time, and this was assuredly not the case.

Hotelling was of course absolutely correct in his strictures. A modern reader might prefer an explanation in terms of components of variance, and we could add that had Secrist selected a group of cities within the same geographical area rather than spread out over the entire country, then the between-year variation in temperatures would not have been dwarfed by the between-city variation, and Secrist would have found much the same convergence for his temperature data as he found for department stores. With the range of climates Secrist included, the correlation of successive temperatures was effectively perfect, and of course there was no regression. But Hotelling was writing for the general American Statistical Association audience of 1933, and technical notions would have been out of place. As it was, the review is a model of clear exposition, and, if you have a taste for such things, economical execution of an offending author.

The review must have been clear to every reader, save one. Secrist wrote a long and passionate letter to the *Journal*, calling the reviewer "wholly mis-

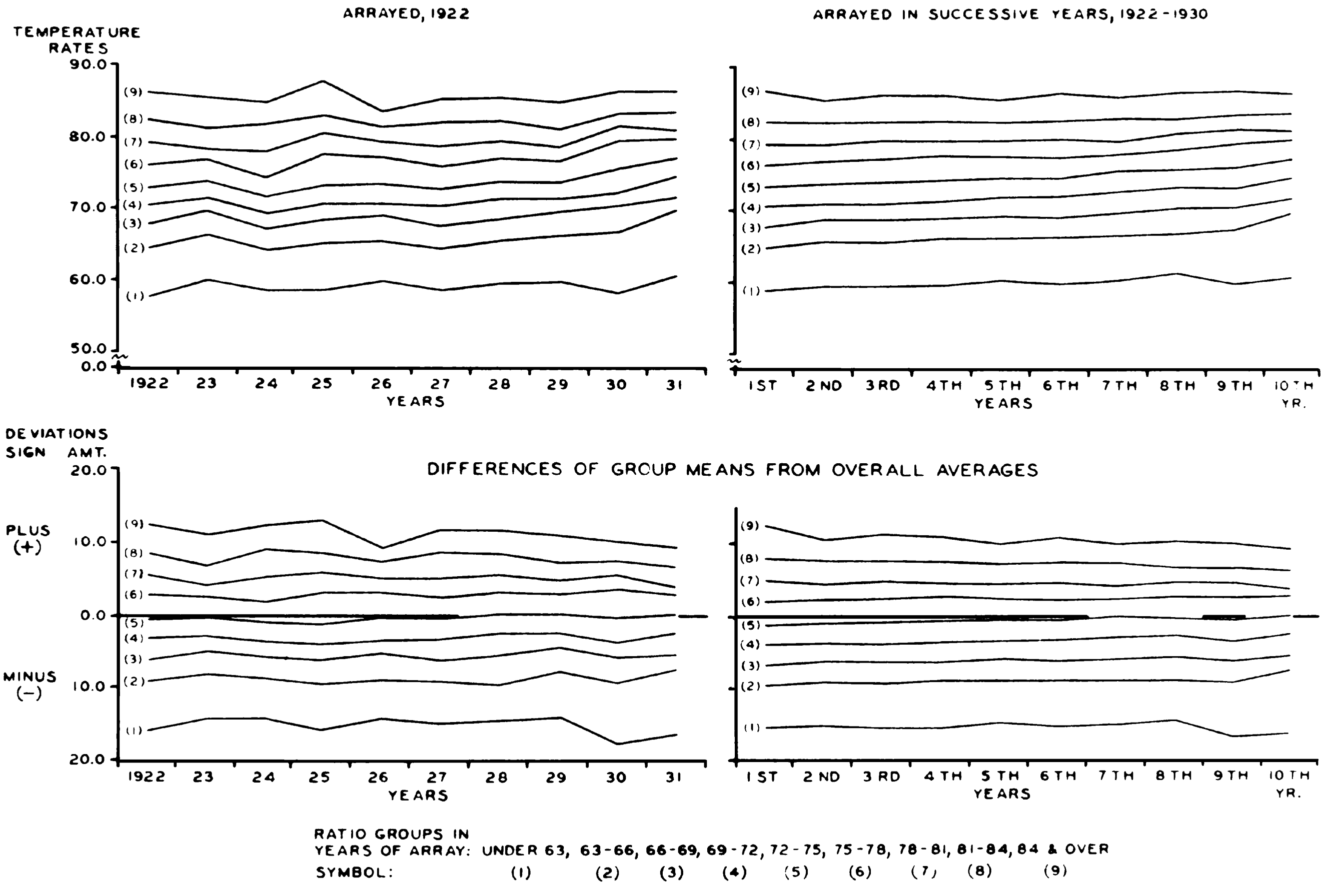

FIG. 2. *Secrist's temperature data: the lower left-hand panel shows the trend in group means over time of average July temperature for 191 American cities, grouped according to which quartile they belonged in 1920, with the overall yearly averages subtracted. Note the lack of regression toward the mean (Secrist, 1933, page 429).*

taken," and accusing him of having examined the book no more than superficially. Hotelling's suggestion, that the (ungrouped) variance of the series be examined, was rejected as "a test of convergence which is unrelated to the phenomenon of regression and one which the author specifically repudiates." Secrist's letter was published in *JASA* with Hotelling's response. (Secrist, Hotelling and Rorty, 1934). Hotelling was not chastened. Hotelling began:

> When in different parts of a book there are passages from which the casual reader may obtain two different ideas of what the book is proving, and when one version of the thesis is interesting but false and the other is true but trivial, it becomes the duty of the reviewer to give warning at least against the false version.

Interpreting Secrist as claiming now—in blatant contradiction to most of his book's text—that he had never meant to imply true convergence of individual firms, only of group means, Hotelling now returned to regression as a statistical artifact. Hotelling was charitable, and he took Secrist as demonstrating only that the variance of the conditional expectations (given the first year's value) was less than the unconditional variance. But, as Hotelling stated, the ratio of these two variances was always the square of the correlation coefficient—and hence less than 1.0. Hotelling wrote:

> This theorem is proved by simple mathematics. It is illustrated by genetic, astronomical, physical, sociological, and other phenomena. To 'prove' such a mathematical result by a costly and prolonged numerical study of many kinds of business profit and expense ratios is analogous to proving the multiplication table by arranging elephants in rows and columns, and then doing the same for numerous other kinds of animals. The performance, though perhaps entertaining, and having a certain pedagogical value, is not an im-

> portant contribution to either zoology or to mathematics.

Notwithstanding the truth in Hotelling's assessment, let me say a word in Secrist's defense. His book was ambitious, and it addressed an important societal problem. Despite his great excitement about his result, he did not rush it into print, but instead, like Darwin, he examined all aspects where he thought he might have gone astray, and he did so with conscientious care and attention to detail. He was well read in the philosophy of science, and he adapted his methodology to the teachings of those philosophers, specifically citing John Dewey and Morris Cohen. He was cautious of too heavy a reliance on theory, stating that he was dissatisfied with a "system of generalization which derives its principles largely or solely from deductive analysis" (Secrist, 1933, page 28). He also worried about too great a reliance upon inductive studies and insisted upon a huge amount of empirical evidence before publication. He specifically cited John Maynard Keynes's *Treatise on Probability* (Keynes, 1921) as support for the general methodology he adopted. Secrist's preface tells us that he solicited comments and criticism from 15 American statisticians and econometricians and 23 European statisticians and economists before publication, although he himself assumed full responsibility for the study. Among the Americans he consulted were not only the ASA officials Willford I. King and Frederick C. Mills whom we have met earlier, but also the *Annals* Editor Harry C. Carver, the biostatistician Raymond Pearl and the polymath E. B. Wilson. Among the English were a number of important economists, as well as statisticians John Wishart and Udny Yule. I cannot help but wonder what the responses of these scholars were; perhaps one or more of them tried to call the regression fallacy to Secrist's attention but only succeeded in getting him to include references to Galton and to add his "confirmation" (via his temperature data series) that his main result was not an artifact. And in fact there are very few statements in Secrist's book that are actually false, even if the aggregate impression is, as Hotelling noted, totally mistaken.

Now if I were given to conspiracy theories, there is one more point that I would make. Willford King and Horace Secrist's careers had been closely intertwined from the very beginning. They had both received their Ph.D.'s from the University of Wisconsin, Secrist in 1911 and King in 1913, and they had taught statistics there together until 1917. Both wrote widely-used statistics texts (King, 1912; Secrist, 1917). Hotelling was in 1933 the foremost mathematical statistician in the country and was closely associated with the *Annals of Mathematical Statistics*. Hotelling's review of Secrist appeared in December 1933; King took his first step to cut off the *Annals* subsidy in December 1933. A conspiracy theorist might then attribute the founding of the IMS to the publication of Secrist's book. But I would not make so bold a claim. What lesson can be drawn from this story? I would not be so foolish as to argue that the existence of a community of mathematical statisticians would have been sufficient to have spared Secrist embarrassment. Indeed, the regression fallacy is extremely subtle, and it can as easily hoodwink the mathematically educated as the nonmathematician (e.g., Friedman, 1992, discusses a recent transgression by two economists). But I do think that Mathematical Statistics is necessary to avoid such traps, that common sense alone is not enough. Indeed, the development of the multivariate techniques that would have permitted a proper and interesting analysis of Secrist's data was intimately tied to the development of a community of mathematical statisticians in the years after 1933. The major lesson, I believe, is that good statistics requires a conversation between scientists and mathematical statisticians.

After 1933 the growth of mathematical statistics was phenomenal. The *Journal of the Royal Statistical Society* published summaries of mathematical work each year, and for the year 1933 they found, for the first time, that a single author was insufficient to prepare such a summary; they needed three. Soon they were forced to abandon the practice entirely. By the time the U.S. entered the Second World War, Sam Wilks had started the program at Princeton, and his *Annals* was established as a premier journal. Jerzy Neyman had, at the invitation of Ed Deming, given his influential lectures to the U.S. Department of Agriculture. The field of Mathematical Statistics had, by 1940, achieved a relative maturity.

This of course did not mean the end of controversy over the proper role of mathematics in statistical investigations, and over the importance of basic research in contrast to applied work. The role of mathematics in statistics—indeed in all of science—has been debated for well over three centuries. It occupied Scottish doctors in the 1690s (Stigler, 1992). It has been the subject of ASA presidential addresses; for example, in his 1926 address, Leonard Ayres complained about the mathematization of the new statistics (Ayres, 1927, quoted by Billard, 1996). The debate goes on today; even within the halls of this meeting you can hear occasional grumbles about too much abstraction, about statistical theory being too

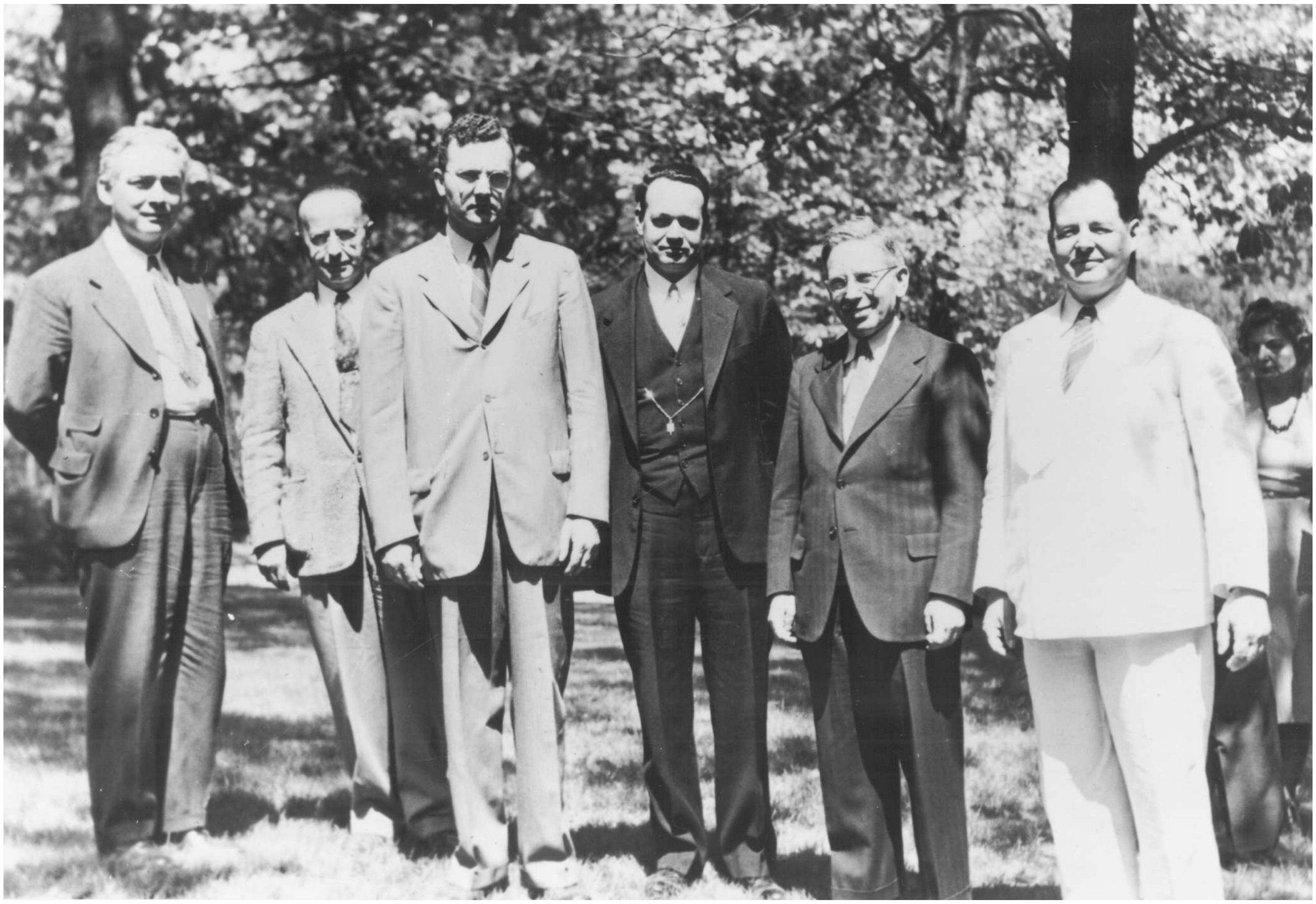

FIG. 3. *A group of Founding Fathers*: (*from left*) *Willy Feller*, *Walter Shewhart*, *Sam Wilks*, *Paul Dwyer*, *Abraham Wald and Harold Hotelling. Photo probably taken in early or middle 1940s.*

removed from applications. Usually the complaints are loudest where the need is greatest. Sometimes these complaints are justified, but they tend to overlook the gains that can only be had from mathematical theory. One of my favorite statements on this was by Francis Edgeworth, writing in 1881 in defense of the use of abstract mathematical reasoning in economics. Edgeworth wrote (Edgeworth, 1881, page 3):

> He that will not verify his conclusions as far as possible by mathematics, as it were bringing the ingots of common sense to be assayed and coined at the mint of the sovereign science, will want a measure of what it will be worth in however slightly altered circumstances, a means of conveying and making it current.

If statistics is to be able to avoid the Secrist phenomenon, if statisticians are to be able to understand the limits and generality of their methodology, its worth in different circumstances and the means of adapting it to others, then it will need more than just Mathematical Statistics, but it will surely not need less. When next you hear doubts raised about our concentration on basic theory, think of Horace Secrist and his 140 tables and 103 charts, and think of Hotelling's analogy to an array of elephants in rows and columns as a proof of the multiplication table. But neither should mathematical statisticians be complacent; above all remember that it is the conversation between theory and applications that is crucially important.

I have only told you the smallest part of the history of Mathematical Statistics in this century. There was no single hero to this or to subsequent history. I have mentioned many of the most important figures; to these should be added many others, such as Abraham Wald and a number of people thankfully still with us. But *that* is a story for another day.

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