CONTINGENCY TABLES

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In biological and medical experiments many contingency tables arise that cannot be analyzed by the chi-square test because of low cell frequencies. A method for treating such cases is presented. Although illustrated with one four-fold universe with two marginal totals fixed, besides the total number in the sample, the general principle can be stated for v cellular universes with not necessarily equal numbers of cells, and with L totals remaining fixed, including the v totals of the size of the subsample from each universe.

Consider now the sample of N from a four-fold universe having two characters A and B, with probabilities $p_1 = p_{AB}$, $p_2 = p_{\alpha B}$, $p_3 = p_{A\beta}$ and $p_4 = p_{\alpha\beta}$. If n_i is the number in a sample of N having the attribute associated with p_i, then the probability of observing n_1, n_2, n_3, n_4 is given by

$$P = \frac{N!}{n_1!n_2!n_3!n_4!} p_1^{n_1} p_2^{n_2} p_3^{n_3} p_4^{n_4}.$$

We restrict our further attention to those tables that have the following totals fixed: $n_1 + n_3 = (A)$, $n_1 + n_2 = (B)$ and $N = n_1 + n_2 + n_3 + n_4$, and also satisfy the condition that the probability of their occurrence shall not vary from table to table by virtue of the values of p_1, p_2, p_3 and p_4.

These conditions are sufficient to determine an associated universe that represents the appropriate null hypothesis. It can also be shown that the probability of an observed table arising from a universe satisfying the null hypothesis is

$$\left[\sum \frac{1}{n_1!n_2!n_3!n_4!}\right]^{-1} \frac{1}{n_1!n_2!n_3!n_4!},$$

where the summation is over all samples which could arise, satisfying the fixed totals.

The rule can now be made that the significance of a table is to be determined by the sum of the probabilities of the table and of all other tables no more probable.

The condition that the probabilities shall not vary from table to table, and the rule just stated, will give a test of significance.

Statistical Flowers Caught in Amber

Paul A. Samuelson

Since I remember well the war-time MIT seminars in statistics now being reproduced in abstract form, I am happy to accept the editors' invitation to reminisce about those times.

Chance alone turned up these Abstracts in the University of Chicago libraries. Although it was my secretary (and Harold Freeman's), Eleanor Prescott Clemence, who typed up these mathematical abstracts, all of us had forgotten they were ever compiled. With probability not minute, Harold Freeman would have sent a copy of them to our friend W. Allen Wallis, who with certainty approaching unity throws away nothing. (The initials W. A. W. on the manuscript Stephen Stigler stumbled upon in the Chicago archives are in the unmistakable schoolboy hand of the Honorable W. Allen Wallis.)

Actually, with faculty blessings, this seminar series was conceived and executed by two graduate students: Lawrence Klein, who was to become MIT's first Ph.D. in Economics and our first home-grown Nobel Laureate; and Joseph Ullman, then studying economics but in the course of the war's windup in Europe later to be enticed into a career in mathematics by Gabor Szegö. Laurie and Joe both as introducers of the speakers; Harold Freeman and I would both cringe and delight in the unpredictable algebraic felicities of their unrehearsed introductions. (Sample: when the illustrious Richard von Mises was to be presented, his many fames as a pioneer had not run ahead of him; so our student impresario left it at, "Although I don't know why, our speaker is supposed to be a very famous scholar.")

It is amazing that, in this epoch after Pearl Harbor, when faculty was dispersing to various war-time labs and graduate student bodies were shrinking to a small core of transients and women, two active students could still attract without stipends so brilliant a group of speakers. Most were

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locals who came of course by foot or, in those days of rationing, by streetcar or at worst by rail from Rhode Island; two lecturers traveled from New York and one from Washington. It is a disappointment to find no women on the list: at the least Hilda Geiringer might have been invited, along with Richard von Mises.

The quality of the papers has high variance. Mine, for example, is a mere finger exercise in curve fitting, excusable only in someone young who volunteers to help keep the local pot boiling. On the other hand, the Haavelmo piece is a gem, which contains the essence of his contribution to the problem for which he quite properly got his 1989 Nobel Prize, and which was later to stimulate the important Cowles Commission works at Chicago by Koopmans, Marschak, Rubin, Hood, Hurwicz and many others.

In alphabetical order I shall recall some personal features of the baker's dozen of speakers.

ARNOLD

MIT never lived up to its promise in statistics but the mathematics and economics departments had a smattering of statisticians. George Wadsworth, an applied mathematician, consulted widely for the armed forces and for various private interests. Probably Kenneth J. Arnold was connected with the Wadsworth group, as Albert Bowker would have been.

One interesting feature of Arnold's analysis was its inversion of the maximum-likelihood procedure. Instead of seeking to show, as R. A. Fisher did in Gauss's wake, that the arithmetic mean of a Gaussian sample is the estimator that maximizes the sample's likelihood, one can ask in the Gauss and 1911 Keynes fashion: "For what family of distributions is the *median* the maximum-likelihood estimator? For what, the *harmonic mean*?" And so forth.

Gauss himself, in a demonstration he ceased to be proud of, had purported to prove that the Normal Curve was the "most-likely" probability law. Had he gone whole hog, and asked what probability distribution was truly most likely to have given rise to the observed sample $(x_1 \leq x_2 \leq \cdots \leq x_n)$, he should have given as his answer the stepfunction that has jumps at those points of size proportional to the frequency of each such x_j value—a case of absurdly close curve fitting.

Arnold takes notice of a curve procedure: to find characterizing properties of the Normal Law. Examples are:

a. Its max-likelihood central tendency is the sample mean (Gauss-Fisher).
b. Its joint (m, σ) sampling distribution involves independent probabilities (Kaplansky).
c. For its normalized density, $N'(x) = p(x)$, $p'(x) = xp(x)$ (K. Pearson).
d. For joint (x, y) samplings from it, if x and y are independently distributed, any orthogonal rotation of them leaves independence invariant (Herschel, Maxwell).

Actually, as Stephen Stigler (1980) might have more strongly emphasized, the de Moivre "reproductive" property of the Normal Law is by all odds its important singularity:

e. It is the only law with finite moments for which the mean of a sample obeys the same law as each item does after rescaling (by $1/\sqrt{\text{sample size}}$). This guarantees that *it* is the asymptote when a valid Central Limit Law applies.
f. As Arnold explicitly noted, it is defined by the Edgeworth-Bachelier-Einstein-Folker-Planck partial-differential (heat Fourier) equation of radiating probabilities.

Arnold himself, who served in later years a tour of duty as Secretary of the Institute of Mathematical Statistics, left MIT for the Wisconsin and Michigan State mathematics departments. Good Americans when they die go to Paris; lucky American professors retire to Cape Cod where Kenneth Arnold resides.

BOWKER

Harold Freeman had several statistics students who made their mark. The late Jack Kiefer is a case in point—at Columbia, Cornell and Berkeley. So is Albert Bowker who tied up with Wallis' wartime Statistical Research Group at Columbia. His subsequent career as Stanford Dean, Berkeley Chancellor, CUNY Head and University of Maryland executive extenuates some loss of concentration on Latin Squares.

FELLER

Willy Feller was then at Brown's strong war-time group in mathematics assembled by Dean Richardson. Later he was a mainstay at Cornell and Princeton. He had arrived in America from Yugoslavia by way of Harald Cramér's Stockholm seminar. Feller had not yet published his classic textbook, but already he was known to be a superb lecturer. Stochastic processes (the name traces to Bachelier c. 1900) were as old as Adam, but were just on the verge of a self-conscious renaissance.

Within economics, Markov processes were also about to take off. (Before 1938, I had to work out for myself that the transition-probability matrix for grandchildren of the elite was the square of the first-generation matrix—such was the state of economics education of the time.) The integral equation of renewal was a hot topic in business-cycle demographics and in Lotka population analysis. Feller had already shown the need for rigor when one claims to give infinite series of exponentials as exact solutions. Later, Ansley Coale and his pupil A. Lopez were able to defend demographer's heuristics since the inability of the very old and the very young to have children guaranteed the completeness of the infinite-series representation. And it was a refugee economist in New Zealand, Harro Bernardelli, who first pioneered the Leslie matrix version of Lotka's demographic integral equations. Much later, Nathan Keyfitz was able to show how the first term(s) in the Lotka infinite series enable calculating the penultimate transition timing to a population's maximum. When I wrote for the 1980 Merton *Festschrift* on R. A. Fisher's economic calculus of "reproductive value," I was delighted to see how his heuristics had touched against those of Keyfitz.

FREEMAN

Harold Freeman was the true leader of MIT's scientific activity in statistics. I always thought of him as Ibsen's Peer Gynt: Peer Gynt with touches of Thorstein Veblen. A perpetual student, Harold never took a Ph.D. degree. MIT didn't care; why should God?

A picture of him on his sickbed in 1936 showed what Jesus would have looked like. When Harold arrived here as a freshman a few years before the 1929 Crash, he topped six feet but weighed less than 100 pounds. He was too weak to carry the molten steel then required at the Forge Lab—even though 20 years later he led our pickup teams to league victory in basketball.

Although he once resembled Jesus, he told stories like Baron Munchhausen. I never heard him describe an event as it happened. Usually his accounts were better than the real thing. Harold was responsible for my coming to MIT. Harold was responsible for Bob Solow's coming here. When emeritus he wrote a bestseller on the evils of American capitalism. Joan Robinson wrote to him in the following vein: "How can a pearl like you coexist with swine like Paul Samuelson and Bob Solow?" He replied: "Easily. They are my best friends."

During the Korean decade, Harold asked me how he could invest a small inheritance so as not to benefit from any war activity. It was a tough question in Leontief input-output networking. In the end I had to cheat him by not mentioning that Gillette and International Harvester did have some Pentagon contracts. During World War II he refused fees for consulting on quartermaster and ordnance matters. He did claim his travel expenses as tax deductions. The local IRS agent said: "Nix. You can be a good guy. But not at our expense."

Every single day from September 10, 1927, to November 3, 1943, Harold ordered a chicken pie at the Walton Cafeteria outside MIT's main gate. By Laplace's Law of Succession, November 4, 1943, had an all but certain outcome. But never since has he eaten chicken.

Once I asked him: "If the Devil promised you a theorem in return for your immortal soul, would you accept the bargain?" Without hesitation he replied, "No. But I would for an inequality."

In his eighties I salute Harold Freeman. *Hail Apollo*!

HAAVELMO

Trygve Haavelmo began as Ragnar Frisch's favorite assistant at Oslo. World War II caught Haavelmo in America when Hitler's *Blitzkrieg* took over Norway. He was prepared to row back to Europe but that proved to be impractical. So there was naught for him to do but work on his own in Cambridge and Chicago, achieving a breakthrough in connection with identifying stochastic relationships in interdependent systems; he was to join up with the exiled Norwegian Shipping and Trade Mission. His MIT Lecture is not at all a routine review. Rather it is a first revelation of what was to be a major stimulus for the Chicago Cowles group already mentioned.

From time series data on price and quantity of, say, wheat, $[p_t, q_t]$, the economist wishes to estimate the best slope and intercept of wheat's linear *demand* curve. But every such point lies as much on wheat's supply curve. What meaning can we give to a least-squares fit of a line as a demand curve? Frisch, Haavelmo's master, had already struggled with this identification problem and had noticed that a fitted line would have its slope biased toward some mean of the two curves' respective dq/dp slopes.

Haavelmo chose to tackle in this lecture the Keynesian macro scenario called the multiplier-accelerator. I note in 1991 that he chose to use the 1939 version that had brought me an excess of youthful fame. Before Haavelmo, we would have regressed Consumption_t on Income_t or Income_{t-1}; and we'd have regressed $\text{Capital}_t - \text{Capital}_{t-1}$ on $\text{Income}_t - \text{Income}_{t-1}$ or have regressed Capital_t on Income_t. Haavelmo shows how wrong this can be (and indicates when it might be a correct thing to do). Econometrics was never quite the same after his famous *Supplement*.

Being a scholar of no fuss, Trygve had simply typed up his big manuscript and ran off copies on the purple ditto process of the day. He sent copies to the few interested experts and would probably not have published it in a special Supplement to *Econometrica* had it not been for insistence by Jacob Marschak and Ragnar Frisch.

It was a coup for Klein and Ullman to have persuaded Trygve Haavelmo to lecture on his new work red hot off the griddle.

HOTELLING

Today most people understand the term econometrics to cover the part of statistics useful in economics. But the original meaning of the term back in 1930 when the Econometric Society was founded (by Joseph Schumpeter, Irving Fisher and Ragnar Frisch) was the triple combination of mathematics, statistical theory and economic theory mobilized for the scientific measurement of economic reality.

In this extended sense of the term, Harold Hotelling qualified in the years 1925-1960 as America's leading econometrician. Trained in mathematics at the University of Washington and Princeton, he came to Columbia from Stanford. And, after 1946, he helped lead the statistical initiatives in North Carolina. At a time when R. A. Fisher's statistics was just beginning to affect American students, Hotelling at Columbia trained many of the nation's teachers. (Later, Wilks at Princeton and Neyman at Berkeley contributed much, and it was Hotelling who found for Abraham Wald a permanent home at Columbia.) Besides being a transmitter of statistical knowledge, Hotelling was also a creator, contributing to canonical correlation, principal components and much else, as well as forging computational algorithms for accelerated convergence of matrix inversion and matrix *eigenvectors*.

In economic theory Hotelling did not go up to bat often, but every article he wrote was a home run. Early on (1925, 1931) he worked out the variational conditions for optimum replacing of a machine and exploitation of exhaustible resources. Parallel to, and independently of, the contributions to demand theory of Slutsky (1915–1953) and Hicks-Allen (1934), Hotelling (1932, 1935) definitively established integrability and curvature conditions for budget unconstrained and budget constrained demand decisions, founding duality theory along the way. Parallel to, and independently of, the new welfare economics of Pareto, Lerner, Kaldor and Hicks, Hotelling (1938) demonstrated that equality of prices and marginal costs were a necessary condition for both Pareto-optimality and Bergson-optimality. Hotelling (1929) enriched permanently the literature of duopoly theory in its spatial aspects: sellers on a line move too close together; ciders are too homogeneous; Republicans and Democrats, sure of the stalwarts at their extremes, vie for the shiftable voters at the center.

Hotelling was an amiable leader with many admiring students. To have helped shape one Kenneth Arrow is fame enow. He was also something of a character. Thus, in the present MIT Lecture, delivered after Pearl Harbor when the Allied cause looked dark, Hotelling announced: "Have no fear. I have surveyed the statisticians possessed by Germany and Japan. They cannot compare with those in Britain, America and Russia. All will be well." He was serious.

Columbia's great physicist, I. I. Rabi, received the Nobel Prize while he was the assistant director of MIT's war-time Radiation Laboratory. Rabi asked me one day if I knew a Harold Hotelling; I replied that of course I knew so great an economist and statistician.

"Is he all there?" Rabi asked.

"Why would you ask that?"

"Well, at a Columbia faculty meeting Hotelling declared that the land the University was on was too valuable for that purpose and we should move elsewhere. 'What new location do you have in mind?' I asked.

"Hotelling replied, 'Seattle'."

I am sure Hotelling was serious.

It is a pity that Hotelling died in 1973 before being awarded the Nobel Prize he richly earned.

KLEIN

Lawrence Klein was too young to vote when he came to our graduate school, fresh from the Berkeley of Jerzy Neyman, Francis Dresch and William Fellner. After 15 months of course work he qualified for MIT's first Ph.D. in Economics, fast work even in war-time. His thesis became a classic, *The Keynesian Revolution*, and it gave the name to an epoch. His Nobel Prize traced primarily to his innovations in econometric macroeconomic forecasting, first at the University of Michigan and then later with the Penn Wharton model.

Klein has been a leader in modern econometrics. He was one of the important Cowles circle, in its Chicago existence. Along with Theodore Anderson he was a Post-Doc in Scandinavia: Klein at Ragnar Frisch's Oslo, and Anderson with Harald Cramér in Stockholm. Klein had a tour of duty at the Oxford Institute of Statistics. Many present-day chair holders call him Master and through him I have grandchildren aplenty.

SAMUELSON

At Chicago and Harvard I had the best economist training available in the 1930s. Therefore, the statistical education, or miseducation, of Paul A. Samuelson would tell something about the scene 50 years ago.

Often I ask couples exactly how they met. They rise to that fly. Where did I first meet the Normal Curve—often called the Gaussian or Laplace-Gaussian curve, but obviously best called de Moivre's Law? I was luckier than I deserved. The Chicago botanist Coulter, in an exposition during the Biology Survey I had to take in my sophomore autumn of 1932, explained simple Mendel laws of peas pink-and-white and smooth-or-curly. When he made tallness depend on the sum of independent genes that could be T_j or t_j, he hinted at a limiting bell-shaped "normal curve" for heights: not bad to get de Moivre's Central-Limit Theorem for the binomial as one's first introduction.

My second introduction came when I accompanied a girlfriend from the Social Service School to an afternoon lecture by the sociologist Ogburn. (He turned out to be the old boy who played tennis at the Quadrangle Club each mid-day. It was he who picked for the new Social Science Research Building its wall motto by Kelvin about scientific knowledge coming only when you could measure it.) Ogburn drew a symmetric unimodal curve on the board and asserted that, within what he called two standard deviations centered on its middle, 95% of any sample would have to fall. I saw that needn't be so: Alas, he didn't mention that, for something called the Normal Curve, it would be true.

I suppose it was soon thereafter that I happened to find on the Social Science Reserve shelf a little statistics primer by the psychologist Thurston. It defined mean, mode, median, geometric and harmonic means, standard deviation (not variance), mean-absolute-deviation and percentiles. It gave histograms for grouped data and may even have prattled about Sheppard's Corrections. A chapter dealt with the Normal Curve; and, I believe, an Appendix may have given Gauss' purported proof that the Normal Curve was the most probable (symmetric) one to have produced the specified sample (under the proviso that for some reason the sample mean had to be the estimator of the parameter a in the density function $p(x - a)\,dx$). What was not gratuitous was to recognize that only for $f(x - a) = (2\pi)^{-1/2} \exp[-\frac{1}{2}(x - a)^2]$ would the maximum likelihood estimator for a be the mean of a random sample $[x_1, \ldots, x_n]$.

I think Thurston described 2-variate linear correlation à la Karl Pearson. But in reading for a nature-versus-nurture term paper, I encountered Pearson's detailed description of Galton's regression-toward-the-mean analysis of parents and children. Later I learned from Ezekiel's treatise all about multiple regression.

Where were my classroom teachers in all this? Aaron Director—who was my first-quarter teacher in beginning economics and later in labor economics—taught me statistics. He relied on a pedestrian text and Mitchell's 1927 *Business Cycles*. I ended up knowing that people used the mean and standard deviation because they were "mathematically tractable." The median and mean-absolute-deviation, though intractable, had the saving grace of not being too much affected by "extreme deviations." Multiple regression I learned on my own, computing on automatic Monroe desk calculators Gauss-Doolittle least-squares equations. I learned about collinearity and ill-conditioned matrixes the hard way, grinding out approximations to 0/0 by ϵ_1/ϵ_2 expressions incident to roundoff errors in connection with a Paul Douglas production-function model based upon labor and capital inputs that grew in the *same* proportion. (Later from Ezekiel I learned the "free-hand curvilinear correlation" techniques of Louis Bean: knowing our data points lovingly, in those days we could almost feel the free play of collinear independent variables. My students in the post-computer age never got introduced to their time-series data points, except through the chaperonage of their summarizing product moments.)

A shocking undergraduate education in statistics at America's second-best university? Yes. But it was worse at the first-best university; at Harvard there were only hand calculators, and honors students learned virtually nothing. By the time I was a graduate student there, I was one out of 20 who discovered the small and exclusive E. B. Wilson seminar given every other year. Maybe at Iowa State things were better for undergraduates lucky enough to be majoring in agricultural economics. Maybe, but don't give 2-to-1 odds on it.

One advantage of a miseducation was much independent reading. Like a drunken sailor I staggered randomly through the many derivative works on least squares in astronomy and geodesy, all tracing back to Gauss. (A typical passage: "In triangulating the Lake Superior region, we solved 75 simultaneous equations between April, 1898 and September, helped by the sparse pattern of nonzeroes in our array of coefficients.") Not until Wilson steered me to Whittaker and Robinson did I learn about characteristic functions ("Fourier integrals") or that some linear sum of variates each normally distributed might *not* itself be normally distributed. I was bothered where I should have been bothered, as to read in R. A. Fisher that an over-small chi-square might be as significant as an over-large one—this from the genius who declared Neyman–Pearson to be totalitarian idiots.

One who did not live through the *ancien régime* would not believe how spotty it could be!

STRUIK

Dirk Struik in his tenth decade is certainly MIT's senior professor, and he must be one of the world's oldest mathematicians. Trained in Leiden, his specialty was differential geometry and he was coauthor with Schouten of a work on tensors. As a departmental volunteer he taught MIT's course on probability for many years. (I seem to remember that he had a cute axiomatic basis for the Poisson distribution.) As a writer on the history of mathematics, Struik attained world fame.

America has gained much from immigrants and Struik was a decade ahead of the avalanche to our shores propelled by Hitler. Struik took an active interest in a teacher's union for universities. In the witchhunt days of Senator Joseph McCarthy, Struik came under various attacks. Although Marxism can hardly infiltrate students' notes on quaternions, Dirk Struik had to sit out his pre-retirement years on a leave of absence with pay—a sad reflection of an ignoble epoch and a definite loss to the community of scholarship. Survival is one form of revenge and it lifts spirit to see the Struik couple striding vigorously as erect nonagenarians.

VON MISES

Richard von Mises indeed had many claims to scholarly fame. He was the kind of pure mathematician who worked in many applied areas of physics, engineering and statistics—a type like von Karman, not rare in Europe and almost nonexistent in America. During World War I he built Austria's first military airplane. With Einstein's friend and biographer Philip Frank, who was also brought to war-time Cambridge, Massachusetts by Hitler's fascism, von Mises edited a famous treatise on partial differential equations.

Von Mises was perhaps best known to mathematicians for his attempt to build the foundations of probability, not on Laplace–Kolmogorov definitions of measure and generalized notions of equally-likely sets, but rather on the concept of an infinite series whose successive terms lacked all order. The approach never much caught on. Some considered it circular; its consistency and rigor were questioned (even though Wald wrote a paper giving it some support). Hilda Geiringer, long von Mises' good friend and ultimately his wife, wrote and lectured in the von Mises vein.

I remember von Mises in a different statistical connection. R. A. Fisher had in those days made inverse and Bayesian probabilities dirty words, with only Harold Jeffreys and von Mises keeping that old faith alive among working mathematicians. A von Mises piece, I think in an early issue of the *Annals of Mathematical Statistics*, was my first source for the important theorem, that as one's sample of new data grows indefinitely, the sensitivity of one's inferences to one's *prior* probabilities goes to zero.

The occupancy problem von Mises treated in this lecture series arose from a famous argument among actuaries in a Vienna insurance firm, who had arrived at inconsistent answers. They called on von Mises, who resolved the quarrels by recourse to elementary combinatorics relevant to Boltzmann's probability-entropy. Cocktail parties still are entertained and surprised by von Mises' demonstration of how few must be the assemblage if no two are likely to have the same birthday in this year's calendar.

A few personal remarks about von Mises. The conservative economist Ludwig von Mises was Richard's older brother, but their views were opposed. Ludwig believed in *a priori* truths; Richard wrote a trenchant book on *positivism*. Ludwig hated mathematics and deplored any attempts by economists to use natural science methodology. Both were strong minds and personalities. (Out of disapproval of Hitler, Richard for a time dropped his "von" and wrote his signature as Richard de Mises. Like John von Neumann he could not, in America, quite give up his honorific—even though both had titles of fairly recent family origin that were unconnected with ancient feats of military glory and were tainted by either bureaucratic merit or financial lobbying!) I remember as an instance of von Mises' high quality a war-time discussion in one of the Harvard Houses on *turbulence*, in which he touched on notions now recognizable under the categories of chaos, bifurcation and computer Monte Carlo explorations.

WALD

Abraham Wald was simply our best. Fisher, Neyman and Wald were the top trio for statistics in the twentieth century. Abe was solid, deep and wide—completely without flash. Virtually self-taught in the boondocks of Hungarian Romania, he sought to study pure mathematics in Vienna. Karl Menger (the mathematician son of Carl Menger the economist) recognized his potential; but prospects for an outlander Jew in depression Austria were minimal. To survive, the racehorse harnessed himself to the cart of economic statistics. Oskar Morgenstern and the banker Karl Schlesinger scratched up bare financial support. Wald's proof of the existence and uniqueness of a Walrasian equilibrium owed its origins to Schlesinger; along the way Wald made a significant contribution to modern index number theory, and useful additions to seasonal adjusting and to Slutsky cycle analysis. Economics, he discovered, was fun and but for his accidental death in a 1950 Indian Air crash, he would have innovated much in theoretical economics along with continued work in statistics and pure mathematics.

Once Wald got to America, places like Harvard could have had him for a song but the mathematicians and administrators of my days in the Harvard Yard were tone deaf to his kinds of quality. It was Harold Hotelling who engineered his appointment at Columbia, where he lived happily ever afterward.

I seem to recall as the interesting feature of this Wald lecture at MIT the fact that Wilks' nice analysis of tolerance intervals, which was distribution-free in the 1-variable case, simply did not generalize to the case of 2-or-more-variables. With all Wald's ingenuity, he had to treat the variables asymmetrically to get anywhere with a generalization. At that time we may not have realized that soon Wald was to encounter and solve definitely the important problem of sequential analysis.[1]

More than 40 years ago a good friend of Wald told me the following charming story. It is secondhand hearsay but has some ring of truth.

Wald to Wolfowitz: Funny thing, I'm proving more theorems than ever but somehow I don't seem to be as happy as I used to be.

Wolfowitz to Wald: Maybe you ought to get Menger's advice at Notre Dame in South Bend.

Wald to Menger: Funny thing, ...

Menger to Wald: Maybe what you need is to get married?

Wald to Wolfowitz: Menger says maybe what I need is to get married.

Wolfowitz: Of course, what a fool I've been not to see it. And I have a cousin . . . .

The rest was history, happy history, but all too stochastically short.

WIENER

Much has been written about Norbert Wiener as prodigy and character. His own two autobiographies, *Ex-Prodigy* and *I Am a Mathematician*, give his version of reality. One biographer has grouped in a book Wiener as Mr. Clean and John von Neumann as the Lucifer who put his genius at the service of the bad guys. We shall not see Norbert's like again soon, but at the MIT of 1922-1964 he was ever-present. I always felt he might have accomplished twice as much if his restlessness and neuroses did not keep him so much in other people's offices. Those who were not themselves geniuses may even have lost a theorem or two because of time occupied with Norbert.

Dozens of stories were told about Wiener, many undoubtedly apocryphal since one had heard similar tales about local characters at colleges ranging from Ripon, Wisconsin, to Cambridge, England. An instance (that might even be true) can illustrate the genre. When the Wieners moved from one Belmont, Massachusetts house to another, Norbert drove near-sightedly back to the old home. It was empty and the door was locked. In perplexity he asked an urchin playing in the street where the Wieners lived. "Mama sent me to take you there, Father. Come along." Wiener's lack of skill at bridge and chess was awesome. A businessman studying at the MIT School of Management was amazed to be able to trounce him at a Faculty Club chess game. "How, Professor Wiener, can you be such a genius in mathematics and such an idiot at chess?" For once, Wiener's answer was on the mark: "In mathematics you're as good as your best move. In chess you're as bad as your worst." The wonder is that one who could play his chess would.

Along with Birkhoff, von Neumann, B. O. Koopman, Hopf and some of the great Russians, Wiener

[1]Remarkably, what Wald did was done in record time, reckoned in days and not months. To go much beyond this initial killing seems still to be frustrating. (It is well-known that preoccupation with priorities of discovery was virulent among the shipmates on the Wallis Statistical Research Group cruise: What did Milton say to Allen in the presence of Major Something-or-other at 10:03 a.m.? Was Jack Wolfowitz paranoid in his concerns for Wald's priorities? Before the last octogenerian is gone and history embalms myths, I ought to preserve in the record the fact that Walter Bartky of the University of Chicago Astronomy Department circulated widely for the Bell System around 1929 one well-specified formulation of the sequential-analysis problem that went beyond the Dodge-Romig procedures and included a complete solution for the binomial case. In July of 1940 at the Cowles Colorado Springs Conference, in the presence of Wald, Samuelson, Haavelmo, Flood and many others, Bartky lectured on this matter. Robert K. Merton as sociologist of science will not be surprised that straight-arrow Wald would have forgotten this event when Wallis threw at him the sequential-analysis challenge. Only one thing matters: It was Abraham Wald who did for the first time the general analysis that it is possible to do. And it is a matter for regret that subsequent generations have been so powerless to advance significantly beyond where he had arrived.)

was a major contributor to ergodic theory, then in the flush of its first decade. His lecture in this series was, as I remember it, purely expository but he began with the solemn warning:

"Gentlemen, we are at war. And nothing you hear in this room must be repeated outside these walls lest it give aid and comfort to the Enemy."

Ralph Freeman—no kin to Harold Freeman and quite illiterate in mathematics—Head of MIT's Economics Department, attended Wiener's lecture by miscalculation and was trapped into being a captive audience. As the small group disbanded, Ralph whispered to me, "Hell, Hitler and Himmler couldn't get a word out of me even if the speaker had been a coherent lecturer."

WILSON

Edwin Bidwell Wilson was a polymath, Willard Gibbs' last protégé at Yale and my Harvard Master in mathematical economics and statistics. Since I wrote recently about him for the Yale Symposium celebrating the 150th Anniversary of Gibbs' birthday, I cannot do better than paraphrase extensively what I said there.

Wilson's merits are in danger of being lost in the mists of history. His forebears went to Yale until he went to Harvard, graduating in 1899 at the age of 20 with the alleged designation of *summa summorum cum laude*. By miscalculation he went to the Yale Graduate School. Gibbs turned out to compensate for Yale's inadequacies in pure mathematics; this pushed Wilson toward mathematical physics and, in the end, toward many novel fields. He wrote the first studies on stability of an airplane and contributed to the following areas: psychometrics (principal components and generalized Spearman–Thurston factor analysis), population analysis and actuarial statistics, Fisherine mathematical statistics and mathematical economics.

MIT called Wilson from Yale. At MIT he headed mathematical physics, wrote his serviceable and durable *Advanced Calculus* and was drafted to be the acting president in the early 1920s. Then President Lowell called him to be the dean of what is now the Harvard School of Public Health. Each spring he taught for the Economics Department in alternate years a seminar in mathematical economics and mathematical statistics, and I was one of the small contingent of economists who benefited from his broad knowledge and wry wit.

E. B. was the only intelligent man I ever knew who liked committee meetings. He was long Chairman of the Social Science Research Council, and much longer Editor of the *Proceedings of the National Academy of Sciences, U.S.A.* He headed the Watchdog Committee that Harvard set up to monitor its new venture into sociology à la Sorokin, Parsons and Merton.

Statistics at Harvard in the 1930s was a scandal. The course on probability in the Mathematics Department was often not offered; when offered it was usually by Mr. X, who could not escape impressment. In the Economics Department Leonard W. Crum and Edwin Frickey taught bizarre versions of Yule's statistics and a smattering of correlation analysis. (Something close to the Central-Limit Theorem appeared in my notes from Crum as the Law of Large Numbers.) What we all learned was, never trust statistics.

Wilson was a saving remnant. He gave intelligent exposition of Fisherine statistics. He knew R. A. Fisher, admired him but also knew him as unreliable in experimental matters. Both Wilson and Fisher goofed in appraising the evidence against cigarette smoking; both were enlisted by the tobacco industry; but Wilson's follies were misdemeanors of skepticism whereas Fisher's were felonies of stubborn idiocy. Retrospective audits have only worsened Fisher's report card in this matter.

FINALE

Reading these embalmed abstracts is a bit like experiencing the time warp of a visit to Pompeii. It brought the same déjà vu as I experienced in reading the resurrected report by Erik Lundberg of his 1931-1933 sojourn in America as a Stockholm Rockefeller Fellow. Names almost forgotten came back to instant life after one gazed upon the snapshots caught at that time.

Does nostalgia make it seem a time better than it was? Perhaps not. Was there ever a more uncivil pair than Karl Pearson and R. A. Fisher? Alfred North Whitehead told me that he was the only person in England who could stay on speaking terms with both Pearson the Galtonian and Bateson the Mendelian. Pearson in power abused Ronald Fisher as untenured scholar. Beaten children become child beaters, it is said. And that was the alibi admirers of Fisher offered to extenuate his hauteurs. (At the Galton Laboratory of the University of London, when Karl Pearson's kingship was divided into chairs for both Fisher and Egon Pearson, a separate staircase was built so that Fisher would not have to encounter the face of Neyman. Honest Injun? That's what we youngsters were told.)

The years 1935-1950 represent an inflection point in the growth of intensive science. The generation of scholars willed to America by Adolph Hitler were unusually dedicated people and they stoked the furnaces of native American research establishments. I believe this rediscovered MIT war-time seminar catches something of the flavor of those unusual times.

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