

A Conversation with Ted Harris

Edited by Kenneth S. Alexander

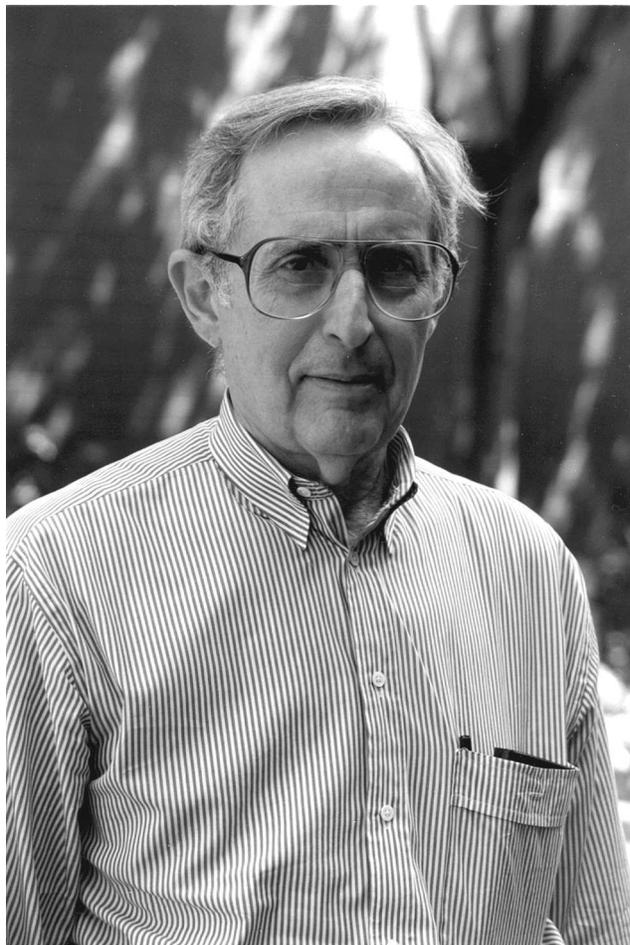
Abstract. Ted Harris was born January 11, 1919, in Philadelphia, Pennsylvania. He grew up in Dallas, Texas, attended Southern Methodist University for two years and completed his undergraduate studies and some graduate work at the University of Texas at Austin. During World War II he served as a weather officer in England in the Army Air Force. He received his Ph.D. in 1947 from Princeton under Sam Wilks. From 1947 to 1966 he was a member of the mathematics department at The Rand Corporation in Santa Monica, California; he headed the department from 1959 to 1965. From 1966 to 1989 he was Professor of Mathematics and Electrical Engineering at the University of Southern California. Since 1989 he has been Professor Emeritus and Lecturer. In 1988 he was elected to the National Academy of Sciences, and in 1989 he received an honorary doctorate from Chalmers Institute of Technology, Sweden. He received an Albert S. Raubenheimer Distinguished Faculty Award in 1985 and a Distinguished Emeritus Award in 1990 from USC.

The following conversation took place in October 1993. The interviewers are Lou Gordon, formerly Professor of Mathematics, University of Southern California, now with The Filoli Information Systems Company, and Sam Genensky, formerly of The Rand Corporation, founder and Director of The Center for the Partially Sighted.

Sam: When you were a child, did you show a propensity for mathematics or science, and what kind of support did you get from your family and the people who were involved in your early education? Each of us can tell a story about this—we didn't come out in a vacuum.

Ted: My parents gave me a kids' science book when I was 8 and a chemistry set when I was 10, and I read junior science books from the library. When I was 12 I wanted to be an astronomer, but, considering the shortage of jobs for astronomers, my father suggested chemistry, which I also found congenial. In those days of the Great Depression, one had to think about making a living.

Lou: You started your undergraduate studies in chemistry, but then became a graduate student in



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point-set topology—a move from the very concrete to the very abstract. How did mathematics attract you?

Ted: Well, I had my first two years of college at SMU and originally intended to be a chemistry major. Then I moved to the University of Texas at Austin, still intending to be a chemistry major, so I took a lot of chemistry, but a lot of math also, more than I needed for a science degree. As the years went on I didn't find chemistry exciting, not even a course in physical chemistry, which I didn't appreciate at the time. On the other hand I had a very interesting course in analytical mechanics with H. J. Ettliger. So it seemed that math was what I wanted. R. L. Moore was then the dominant mathematical influence at UT. I began my graduate work expecting to write a thesis with him in point-set topology. His method of teaching was well known: students were fed the axioms for a "Moore space" and had to prove the basic theorems with no reading of the literature allowed and very few hints from R. L. This bare-handed approach developed self-reliance, but in later years I had to learn the importance of mastering the powerful techniques of the past. The classes were quite competitive. Each student wanted to be the first to show the class the proof of a hard result. If someone else was ready to present it first, you might withdraw from the demonstration so you could get it later on your own. At the same time I took some courses in differential equations with Ettliger.

WORLD WAR II

But then World War II came along and the Air Force came to the campus advertising for people with bachelors' degrees in math, science or engineering to go into the Air Force meteorology program. Meteorology looked like just the thing and sounded exciting—eight months training at Cal Tech (one of five universities in the program) and then active service. At the end of the training period we were given a certificate in meteorology, commissioned as second lieutenants and given our assignments. Mine was to England, where I went in January 1943 and served with various units of the Eighth Air Force until June 1945.

Sam: Is there any one assignment you had that stands out in your memory?

Ted: My most interesting assignment was from October 1944 to June 1945 when I was one of the station weather officers at Eighth Air Force Headquarters near London. Each afternoon one of us conducted a scrambler phone conference with five other forecasting stations, to prepare the forecast for the

next day's operations. Then the commander's own staff weather officer, who worked with us, briefed him about the forecast. We had plenty of data from surface stations, plane flights and ships, from which we constructed synoptic weather maps. Upper air data (winds, temperature and humidity as functions of height or pressure) were radioed from unmanned balloon ascents and were used to construct thermodynamic charts essential for predicting thunderstorms and other hazards to flying. Extrapolation formulas for forecasting weather-front movements were known but not practical without computers; extrapolation by eye, modified by various rules of thumb, was used. Probability and statistics weren't used, except for a little climatology. Our crucial forecasts were for 24 hours or less; long-range forecasts were not taken seriously.

PRINCETON

Lou: After the war you went to Princeton.

Ted: When the war in Europe ended, I came back to the States and went on terminal leave in September 1945. By that time I was 26 years old and had to consider whether to go back to graduate school or to get a job as a meteorologist. I went to the Weather Bureau and talked to people in the research department. One man there, Dr. C. F. Sarle, was very enthusiastic about applying statistics to weather forecasting. As I recall, he was predicting the weather for, say, the area around Washington, D.C. 24 hours ahead, using regression equations involving meteorological variables the day before at Washington and other places. He told me they were having very good luck that way and were doing as well as the standard methods of forecasting. He suggested that I get a Ph.D. in statistics to have a basis for working in meteorology. That sounded good. I don't remember wondering if I should do it—I just decided to do it. I talked with Ettliger and he approved of the plan, but whereas Sarle had suggested Columbia, Ettliger said, "You really should go to Princeton. We know Sam Wilks and he is the best person in the country to work with." So I applied to Princeton, where John Tukey was also active in statistics, and was admitted to the Graduate School in time for the 1945 fall semester. Shortly after I arrived at Princeton I was in the tea room one afternoon before I had a chance to learn who people were. A polite gentleman stepped up and asked what I had been doing (I was still wearing my uniform). I said "Weather forecasting." He said "What do you use?" I mentioned weather maps and thermodynamic charts. I thought he looked a little vague when I mentioned thermodynamics and asked if he knew what it was.

He still looked vague so I gave him a brief explanation. I learned later that he was the distinguished physicist Eugene Wigner. Some of my fellow students reminded me from time to time how I had explained thermodynamics to Wigner.

Lou: Who were your fellow students?

Ted: Princeton graduate students who, then or later, worked in probability or statistics included Dick Bellman, Kai Lai Chung, Paul Meier (who introduced me to my future wife Connie), Sam Karlin, Gilbert Hunt (whose erudition impressed all of us), Luis Nanni, Mel Peisakoff (still an undergraduate then and later my brother-in-law), Bernard Sherman and David Votaw, Jr. Visitors—long and short term—included Ted Anderson, Niels Arley, George Brown, William Feller, Merrill Flood, Paul Hoel, Alex Mood, Fred Mosteller and Henry Scheffé. Harald Cramér visited during 1946–47 and gave elegant lectures on stochastic processes. The afternoon teas in old Fine Hall were a high spot. Besides the math faculty and graduate students, there were usually some physicists from next door. Solomon Lefschetz, then chairman of the Mathematics Department, always impressed us by his mastery of his tea cup and saucer with his artificial hands. Once, a few months into my first semester, I heard him complaining that the graduate students were bookworms who spent all their time studying. I reminded him that in his welcoming remarks he had warned us that our places in the Graduate School were coveted by many others. We were fond of him anyway! I benefited a lot from the weekly statistics seminars—not only the talks but the discussion by Tukey, Wilks and others. After a couple of months of the Princeton environment, I forgot about meteorology and planned to become a statistician. After preparing for the general exams in the spring of '46, I attended a outstanding summer session in statistics at North Carolina State College, organized by Gertrude Cox. Visiting faculty included C. I. Bliss, W. G. Cochran, R. A. Fisher, G. W. Snedecor and J. Wolfowitz. Perhaps most important for me were the lectures of Wolfowitz on sequential analysis, then rather new. I still have a group picture showing the faculty and students, including many budding young statisticians. In the fall I returned to Princeton.

Sam: What impact did your fellow graduate students have on you?

Ted: I was impressed by the mathematical erudition of some of the Princeton students. We learned a lot from each other in conversations and informal student seminars. There was no separate statistics department, and statistics majors mixed freely with the other math students. From my point of view

this was a very good thing. On the nonmathematical side, coming from the apolitical environment at U. of Texas, I benefited from the range of political and social views among the Princeton students.

Sam: You've talked a lot about your graduate years. What can you tell us about the writing of your thesis?

Ted: Wilks had suggested a statistical problem on which I had made no progress. One day he dropped into my office and showed me what we now would call the Bienaymé–Galton–Watson model for branching processes, and the iterative relation for the generating functions. I'm not sure where he came across the problem—perhaps from Fisher's book *The Genetical Theory of Natural Selection* (which I heard about later from Tukey), or perhaps from the nuclear fission analogy. Wilks introduced me to John Wheeler of the Physics Department, who suggested several people he thought might have worked on branching processes at Los Alamos or elsewhere, including Stanislaw Ulam and Richard Feynman. Feynman wrote that his work was still classified. But Ulam sent me a paper by himself and David Hawkins which at first had a discouraging effect, because I had obtained a few results and this paper had them all and more. However, I set to work and had some additional results in a month or so. One was a proof that the limiting distribution of a standardized supercritical branching process is absolutely continuous, except possibly at one point. This was not in the paper of Ulam and Hawkins, but I thought I'd better phone Feynman to ask him if he had gotten it; it surely couldn't be classified. I was relieved when he said, "It's all yours. Physicists don't give a damn about absolute continuity." I kept working and by April thought I had enough for a thesis. Fortunately Wilks and Ulam both attended a mathematics meeting in New York at that time. I got them together and heard Stan assure Sam that I had some results not in the Hawkins–Ulam paper. That was my informal oral exam, although I had a regular one soon after. Then of course there was the question of a job. I had always supposed I would go into academic work, but early in 1947 I got a letter from John Williams, who was organizing what later became the Mathematics Department at the Air Force's Project Rand, then a subsidiary of the Douglas Aircraft Company, later the nonprofit Rand Corporation. At first I wasn't interested, but both Wilks and Mosteller thought well of Williams. Soon after, I talked with Williams in New York. His picture of Rand was tempting. I felt it would be a place to do good science and math. I had been in the Air Force three and a half years and felt comfortable about defense work. So

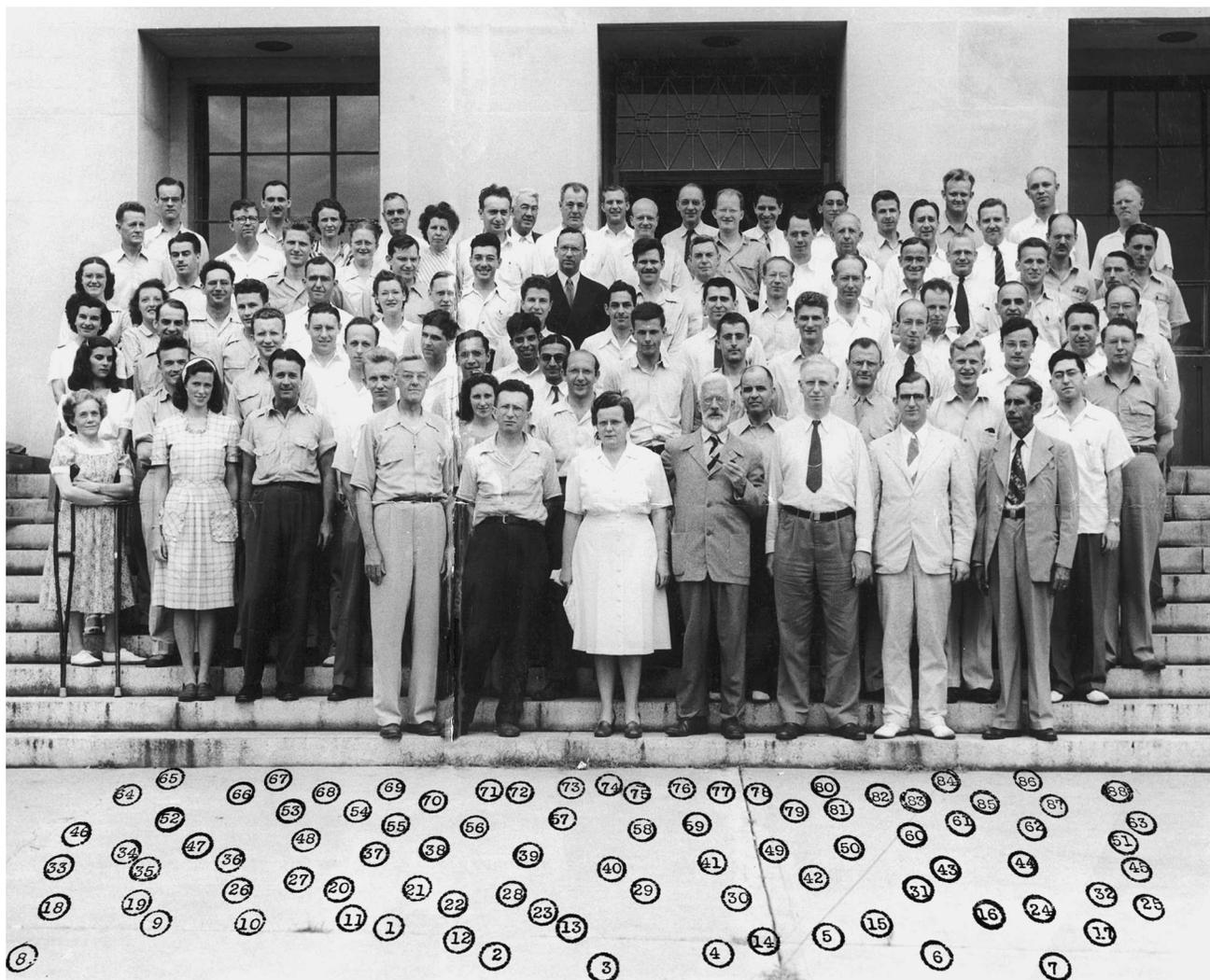


FIG. 1. *Statistical Summer Session, North Carolina State College, Raleigh, 1946*: (1) G. W. Snedecor; (2) J. Wolfowitz; (3) Gertrude M. Cox; (4) R. A. Fisher; (5) C. I. Bliss; (6) W. G. Cochran; (7) T. Casanove; (8) Huldah Bancroft; (9) Jeanne Freeman; (10) A. R. Mangan; (11) H. F. Robinson; (12) Sarah Porter; (13) P. M. Neurath; (14) F. M. Wadley; (15) W. L. Deemer, Jr.; (16) R. P. Ament; (17) M. L. Norden; (18) Doris Hiers; (19) H. A. Salmela; (20) Jay T. Wakeley; (21) R. L. Anderson; (22) J. M. Batista; (23) V. Divatia; (24) P. Gutman; (25) W. T. Walker; (26) F. J. Verlinden; (27) R. J. Monroe; (28) G. R. Seth; (29) F. S. Acton; (30) T. E. Harris; (31) D. B. Duncan; (32) H. J. Smith; (33) Victoria Rossetti; (34) Eleanor B. Donohue; (35) R. A. Porter; (36) J. M. Cameron; (37) Elizabeth R. Bowker; (38) J. F. Crow; (39) W. J. Angulo; (40) G. E. Noether; (41) G. E. Nicholson, Jr.; (42) E. A. Radsliff; (43) J. C. Neill; (44) H. O. Hetzer; (45) H. L. Bush; (46) Julie J. Gegner; (47) J. H. Weatherspoon; (48) Max Astrachan; (49) A. L. Tester; (50) Halvdan Astrand; (51) G. C. Ashton; (52) A. E. Paull; (53) B. G. Greenberg; (54) Carol M. Jaeger; (55) A. H. Bowker; (56) Robert Bechhofer; (57) B. H. Schneider; (58) M. E. Terry; (59) R. I. Piper; (60) K. S. Dodds; (61) C. A. Bridger; (62) J. F. Kubis; (63) D. B. W. Reid; (64) J. H. Watkins; (65) C. P. Mook; (66) P. T. Bruyere; (67) Douglas C. Hill; (68) Gertrude W. Diederich; (69) Roy L. Roberts; (70) Harriet J. Kelly; (71) Milton Sobol; (72) Frank Parker; (73) P. J. Rulon; (74) A. L. Finkner; (75) H. L. Lucas; (76) F. W. Sherwood; (77) R. M. Harding; (78) D. O. Price; (79) D. W. Parvin; (80) Herman Chernoff; (81) H. L. Thomas; (82) B. M. Graham; (83) B. B. Migicovsky; (84) P. J. Blommers; (85) Walter Leighton; (86) P. E. Lewis; (87) Nathan Keyfitz; (88) Guy Stevenson.

I accepted a job at Rand. Connie and I were married in June and I joined Rand in Santa Monica in August, expecting to stay a few years and then go to a university. Our children Steve and Marcia soon came along, and later Steve and Ruth gave us grandsons Dave and Mark. Rand was set up as a reservoir of civilian scientists to study problems

of national defense. Williams was a remarkable man who believed strongly that the scientists and mathematicians should be given maximum freedom while keeping their basic military mission in mind—a difficult balance to strike. The problem still shows up whenever government or corporate funds are involved.

RAND CORPORATION

Sam: Rand had a military mission. How did mathematicians at Rand satisfy that mission, as you interpret it?

Ted: Let me give you my own experience in my early days there. Edwin Paxson and others at Rand did systems analyses—bigger and better operations research, applied to problems of national scope, mainly military, with consideration of as many factors as possible: not only military but economic and political factors. I recall Jack Hirshleifer's strong support for giving due weight to economic factors. I think I had a role helping firm up and improve some of the mathematical models. I soon learned to beware of "errors of the third kind"—that is, giving the right answer to the wrong question. (I don't recall who coined the phrase.) One should understand the background of the question. Also I continued to work on branching processes, in line with the basic policies of the department. We felt better if our mathematical work had some visible potential for applications. Linear programming, dynamic programming, game theory and control theory flourished, and statistics and probability were recognized as essential. But if what interested you most was not only unapplied but inapplicable as far as anyone could see at the time, your stay at Rand was likely to be limited. The Air Force seemed reasonably satisfied with the mixture of applied and basic work done by Rand as a whole. You, Sam, were a valuable link between the mathematicians and the rest of Rand.

Lou: Wasn't it about this time that you wrote your famous papers with Bellman?

Ted: At that time, Dick Bellman was at Stanford, but visited Rand occasionally as a consultant. He had been on my doctoral dissertation committee and was familiar with my thesis. I knew that some Galton–Watson processes extend in a natural way to continuous-time Markov branching processes. I remember saying to Jimmy Savage, also a consultant, "If we apply this Markov process to bacteria, they have exponential life lengths. Are they really that way?" I knew Jimmy had some biological background. He said, "Not at all. I've watched them under a microscope and you'll see the bacteria sit there a while and then, after a fairly definite period of time they'll split. It's variable, but still it's nothing like exponential." That raised the question how to model a continuous-time branching process with arbitrary life lengths instead of exponential life lengths. It turns out that the generating function satisfies a nonlinear integral equation, and the mo-

ments satisfy renewal equations. From the discrete-time case I knew pretty well what to expect—this or that should have an almost sure limit, and so on. On the other hand Dick knew how to handle the functional equations. So we produced our papers (Bellman and Harris, 1948, 1952) on age-dependent processes, a lot of fun. At the same time David Kendall had a different approach to the problem of nonexponential life lengths. I also did some work with Herman Kahn, later a global systems analyst but then a physicist, who was applying Monte Carlo techniques to neutron shielding. (Others, including John von Neumann and Stanislaw Ulam, were also working on this then-hot topic.) The idea was to use a Monte Carlo method to estimate the probability that a neutron doing a random walk gets through a slab. Since the chance that it does so might be one in a billion, you couldn't do it by straightforward sampling because the computers of those days weren't fast enough. Herman knew about importance sampling in statistics and had some very ingenious ideas about applying it to Monte Carlo. We worked together for some time; I supplied the formal statistical techniques to implement his ideas. The work was reported in *The Monte Carlo Method*, a symposium proceedings (Harris and Kahn, 1951).

Sam: Who else was at Rand then?

Ted: One good thing about Rand was that there were many very fine consultants around for a summer or shorter periods. Ken Arrow and Jack Marschak were among them. Von Neumann was there from time to time—working, I think, with people who built the "Johnniac" computer. I saw little of him but was intrigued when he recommended Bayesian inference (which I had thought was obsolete) for solving a certain statistics problem. Later, after learning about the Bayesian role in the "complete classes" of Wald's decision theory, I saw that von Neumann had a point. From Savage I acquired additional pro-Bayesian sentiments. Other consultants with whom I had beneficial contacts included David Blackwell, Samuel Karlin and H. F. Bohnenblust. I worked with Arrow and Marschak on a dynamic model for inventory problems. We studied what Marschak called $s - S$ policies: you wait till the inventory gets down to size s and then order up to size S . I thought that was a good example of work appropriate for Rand. Our work seemed to start a cascade of papers: in particular, Dvoretzky, Kiefer and Wolfowitz wrote a deep paper proving some optimality results for $s - S$ processes.

Lou: Rand was a hotbed of statistical decision theory and the theory of games. Did that change your way of thinking about things in your life and/or your work?

Ted: I didn't work in these areas myself but picked up something about them from David Blackwell, George Dantzig, Mel Dresher, Abe Girshick and Lloyd Shapley. The early days of Rand were part of an exciting period when people were learning the connections between game theory and decision theory and linear programming, all of which relate to convex sets. Learning about them gave me some background that was useful later when I taught statistical decision theory at USC. In the early 1950's, I dropped branching processes for a while and got interested in recurrence and invariant measures for Markov processes. The main stimuli that I remember were a talk by Mark Kac on recurrence times for the Ehrenfest model, a talk by Feller on the Erdős–Feller–Pollard results when they were yet new, Hopf's book on ergodic theory; Chung's notes on Doeblin's work; a talk by Herbert Robbins and Gopinath Kallianpur on their work; and Cyrus Derman's thesis. My main result on invariant measures was in a paper in the 1955 Berkeley Symposium. About 1955 J. L. Doob asked me to write a book on branching processes, which kept me busy for some years. (I first heard of martingales in a postcard from Doob, advising me that a standardized branching process is one.) I had to learn some Russian, since there weren't as many translations then as now. I found that after learning a few hundred Russian words, you can read Russian mathematical text pretty well.

Lou: It is unusual for a book to be standard in its field for as long a time as your book on branching processes. What features of the book do you attribute this to?

Ted: I worked hard on the exposition, made the book as elementary as possible and included many applications. Several people gave me very helpful comments on the early drafts. I admit it was pretty carefully written.

Sam: What other kinds of projects were you involved with at Rand?

Ted: I was also involved at that time in a military operations research project at Rand. Indirectly it led to some good applied mathematics. We were studying rail transportation in consultation with a retired army general, Frank Ross, who had been chief of the Army's Transportation Corps in Europe. We thought of modeling a rail system as a network. At first it didn't make sense, because there's no reason why the crossing point of two lines should be a special sort of node. But Ross realized that, in the region we were studying, the "divisions" (little administrative districts) should be the nodes. The link between two adjacent nodes represents the total transportation capacity between them. This made a reasonable

and manageable model for our rail system. Problems about the effect of cutting links turned out to be linear programming, so we asked for help from George Dantzig and other LP specialists at Rand. Eventually this led to the book *Flows in Networks* by Ray Fulkerson and Lester Ford, which has applications to assignment problems, minimum cost flows, warehousing problems and others—an example of how applied problems can lead to good mathematics.

MOVING TO USC

Sam: In 1960 you replaced Williams as head of the Mathematics Department at Rand. [Williams moved to a higher administrative position.] How did this affect your work and your future decision to move to USC?

Ted: The administrative work still left me considerable time for research. I think the reason I moved was not connected with being department head. I felt the best thing to do was to continue Williams' policies, which I admired and had no inclination to change. However, during the 1960s, things were changing at Rand. In the early days its support had been almost completely from the Air Force. As time went on, more and more support came from other sources, some non-military. Although this was a good thing in some ways, it reduced the possibility of doing work not immediately applicable to real problems. For instance, a city funding a two-year contract to improve its fire department wouldn't be enthusiastic about supporting basic research in combustion. Partly for this reason and partly because I had always had the feeling I would go into academic life, I was delighted when the USC Mathematics Department offered me a position. At that time, late 1965, the mathematics department at USC was expanding with an institutional grant from NSF. Sol Golomb, Dick Bellman (who had moved from Rand to USC), Paul White (then Chair of the Math Department), Al Whiteman and Zohrab Kaprielian (then Chair of Electrical Engineering) were very helpful in my coming. I was glad I had had the years at Rand, and also glad to make the change. I have certainly had a great attachment to the USC Mathematics Department, and also have felt it to be a very lively place from the point of statistics and probability.

Lou: What kind of research were you doing around the time you moved?

Ted: In the middle 1960s, I wrote my paper on Brownian collisions (Harris, 1965), which was my own entry into infinite particle systems. As far as I can remember, this came just from idle curiosity.

When two elastic particles of equal mass moving on a line collide they exchange velocities. I wondered how you could define collisions for two Brownian particles. There is a perfectly reasonable way to do this, which naturally brings up the question what happens if there are infinitely many of them on a line. What is the inhibitory effect on a tagged particle? Under certain conditions it turns out that the variance goes up not like t as in the Brownian case, but like the square root of t . Around 1970, after doing some work on particle systems viewed as point processes, I became acquainted with the work of Frank Spitzer and R. L. Dobrushin on infinite interacting systems, which I found very appealing. For most of the 1970s I worked on topics suggested by their research or related work of Richard Holley, Tom Liggett, and others. There's a natural path from infinite interacting systems to continuous stochastic flows. A student of mine, Wang Lee, wrote a thesis on stirring processes, a generalization of Spitzer's simple exclusion in the symmetric case. If you're only interested in occupancy numbers rather than individual particles, symmetric simple exclusion is a Poisson process of permutations of the d -dimensional integer lattice. By contrast, Lee's stirring processes are Poisson processes of measure-preserving transformations of the real line. Some time later, wondering what kinds of limits such stirrings may have, I got a limiting continuous flow which, after a while, I realized was of a type studied by Itô and others in connection with systems of stochastic differential equations. The approach via stirrings gives a somewhat different point of view, emphasizing the correlation tensor. Luckily, Peter Baxendale, who had done very fundamental work in flows, came to USC at about that time, and we collaborated on a paper on homogeneous isotropic flows. I wrote a couple of other things on stochastic flows in the 1980s.

RETIREMENT

Sam: You're emeritus now. Was it a big change for you to retire?

Ted: I retired in 1989 after a very gratifying birthday conference hosted by USC which I really appreciated. Since then I've been teaching one undergraduate course a semester at USC. I find it occupies my time quite well. My only paper after retirement has been a mostly expository contribution to the volume honoring Frank Spitzer on his 65th birthday (Harris, 1991).

Sam: As you look back on your long, productive career, do you have any sort of general statements to make or advice or direction to give young people

who might be coming into the field? Not necessarily what to work on, but approaches to working on problems: interacting with other professions, openness of mind, whatever it may be? Actually I'm giving you a chance to be a philosopher.

Ted: I've already traced the stimuli for my work on recurrence; it came from listening to the best researchers or reading their work. The trite moral from this is: get a good deal of exposure to many bright people, whose work is not necessarily closely connected to yours, and some of it will pay off. Having some diversity in what you study and what you look at is very helpful.

Sam: But you have to have the wisdom to take advantage of these opportunities. A lot of people don't have this—the opportunity is there and they don't perceive it as an opportunity.

Ted: That's true. Sometimes luck helps. For example, I first heard about percolation in the late 1950s from a memorandum being circulated by some government agency describing percolation and asking how you find the critical value. I looked at the problem and made no progress. Some time later, after Hammersley and Broadbent had written on percolation, there was a conference on graph theory at Rand. Since it was right down the hall, I attended some sessions and picked up the notion of the dual of a planar graph, which, with a result of Hassler Whitney, gave me a lower bound for the critical value for square plane percolation. Harry Kesten eventually did the difficult job of finding the exact value. What I learned about Moebius inversion from a talk given by G.-C. Rota turned out to be crucial for my paper (Harris, 1976) on set-valued Markov processes.

CHANGES AND DIRECTIONS

Sam: You've seen a great change in universities in the course of your life. What has changed for the better? What's changed for the worse?

Ted: I entered college in 1935. On the positive side, we don't have racially segregated public universities now. Student activism, then unthinkable, has produced some good results, although there have been excesses. The increase of government funds to universities after World War II has given a big boost to mathematics. Comparing calculus then and now, today's first-year courses cover more than I had as a student—I think having labs and TA's today has something to do with it.

Sam: Relative to graduate students, has anything happened or changed in the quality or orientation of graduate students in your time as opposed to today?

Ted: I don't see a difference in quality between the best then and the best now. As to orientation, my personal impression is that today's students are more receptive to nonacademic jobs. Most of my Ph.D. students took nonacademic jobs.

Lou: Has the perception of the fields of statistics and probability changed in the eyes of other mathematicians?

Ted: There was a time when probabilistic arguments weren't recognized as rigorous mathematics. By the time I was a graduate student, Wiener, Kolmogorov and Doob had created rigorous foundations for stochastic processes. However, it took a while for their work to spread. For example, Halmos and Savage, in their 1949 paper on sufficient statistics in *The Annals of Mathematical Statistics*, were careful to define basic measure-theoretic concepts for the readers of that time; this would not be required for today's more sophisticated readers. Nowadays, differential geometry, Lie-group theory and other areas are also important for probabilists.

Sam: Do you feel that the addition of fields such as differential geometry, Lie-group theory and the like add to probability theory in some significant way, or do they just give the field novelty without advancing it very far?

Ted: I think they have enriched the study of diffusion and continuous stochastic flows. The analysis of a flow example that I contributed to Spitzer's Festschrift volume certainly benefited from some prior work of Baxendale on flows on certain Lie groups.

Sam: Do you have any visceral feelings about directions you would like people to explore in the future? Are there areas that you would like to see the next generation of graduate students look into?

Ted: I don't think I can predict or recommend directions. The areas in which I worked still seem lively, and I'd be happy to see them keep flourishing. By the way, branching-process theory has recently been used and further developed in connection with DNA multiplication. PCR (polymerase chain reaction), a type of branching process, has become familiar in court cases involving DNA identification.

Lou: How do you feel about the role of probability theory in statistics in this age of heavy number crunching and heavy computing? Is there still a role for probability theory?

Ted: I think "yes" is a safe answer until computers show they can do things comparable to inventing martingales. But computers may become respected junior partners.

Sam: I think I belong to the school that says you can have a computer, but a computer is never go-

ing to advance our knowledge of the fundamentals of our field. It's going to solve specific problems. I don't want to put words in your mouth, but do you think that in the last analysis the really significant advances are not made numerically?

Ted: So far the computers have been good servants. But in all the fiction stories I know about robots, beginning with Čapek's *R.U.R.*, the robots decide eventually that they're human or better, and conflicts result. Maybe we can work out a *modus vivendi* with them.

Lou: From your vita it's clear that you've never rushed to publish before having something profound to say. How do you feel about the exploding growth in the production of research?

Ted: Maybe I can answer by recounting some history. I was Editor of *The Annals of Mathematical Statistics* from 1955 to 1958. During that period there was a substantial growth of the *Annals*, and when the Council saw my request for the number of pages for the second year, they were a bit shocked at the size. So I spent a week acquiring and analyzing some statistics about recent submissions, acceptances and rejections. The data showed that more papers were being submitted, and the editors felt there were now correspondingly more good papers deserving publication. So the Council went along and increased the budget, and we didn't take this as an opportunity to cut down on the fraction accepted. Somehow we have to figure out how to keep up with the information explosion. Computer network publication may help. Sheer lack of space on bookshelves will keep us from printing too much on paper.

Sam: For quite a long time a lot of research in universities has been supported by the government in one guise or another as something either part of or peripheral to national defense. Now that these funds are drying up, what effect do you think the potential reduction in federal funding is going to have on research in statistics and mathematics in general?

Ted: Sharply diminished support would surely have a bad effect. Moreover, the scientific community is under pressure to orient its activities visibly toward national goals. This could be very harmful if done indiscriminately, but if some people are motivated toward goal-oriented work, I don't think the results will be all bad.

TEACHING

Sam: Ted, I'd like to hear a few comments on your philosophy concerning the teaching of undergraduate and graduate students—separately if you prefer.

Ted: I think we should be sure that graduate students in science and engineering get enough (but not too much) mathematical rigor and that math graduate students get enough applications. The graduate probability and statistics courses that I gave before I retired were populated 50 percent or more by electrical engineering students. Generally speaking they appreciated a rigorous approach, and it helped that this feeling was shared by our EE faculty. Since I became Emeritus in 1989, I've been teaching a section of our undergraduate probability course for business students. One semester of calculus is a prerequisite. The sections are all computerized: the teacher may project computer demonstrations (e.g., simulated random walks) on a screen, and the students do computer exercises using DERIVE or other software. Naturally we give them applications (sampling, opinion polling, quality control, reliability and so on) and from time to time discuss examples from newspapers or other current sources. Such examples help students see mathematics as related to what's going on in the world. For some time I have collected articles from newspapers, the journal *Science*, company reports and other sources, illustrating applications of probability and statistics. Topics include medical trials, correcting the 1990 census, counting the animals and plants of the U.S., aircraft safety, statistics of earthquake aftershocks and so on.

Sam: Do you look with favor upon people who have built fine reputations in their fields, in this case mathematics or statistics, teaching courses to freshmen?

Ted: Yes. A veteran may have some interesting things to tell from his own experience. Moreover, his experience will help him decide which topics in the text should be emphasized and which may be touched only lightly or omitted.

Sam: Ted, did you feel after you completed your graduate work that you were a well-rounded human

being, or did you feel that you needed broadening? This is a loaded question. I once felt I was all physicist and mathematician, and I had to learn other aspects of learning and knowledge in order to be a more complete human being.

Ted: I was fortunate as a kid in having good piano training, which I've kept up. I've always been a reader, and in the last 25–30 years have read seriously on Jewish subjects and acquired a modest reading knowledge of newspaper Hebrew. As to my being a well-rounded human being, my family is still working on that.

Lou: What kind of plans do you have now for enjoying your retirement?

Ted: As long as I continue teaching, that will occupy most of my professional time. Once I hoped to write up some of the newspaper examples I mentioned above. Perhaps I'll find time to do that some day. And I'd like to get better acquainted with my grandsons, who give me a stake in the future.

Sam: I'm sure you'll do all that and more. Thanks, Ted, for taking the time to talk with us.

Ted: Many thanks to you both. I enjoyed our conversation.

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