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A Conversation with Stephen E. Fienberg

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Abstract. Stephen E. Fienberg is Maurice Falk University Professor of Statistics and Social Science at Carnegie Mellon University, with appointments in the Department of Statistics, the Machine Learning Department and the Heinz College. He is the Carnegie Mellon co-director of the Living Analytics Research Centre, a joint center between Carnegie Mellon University and Singapore Management University. Fienberg received his hon. B.Sc. in Mathematics and Statistics from the University of Toronto (1964), and his A.M. (1965) and Ph.D. (1968) degrees in Statistics at Harvard University. He has served as Dean of the College of Humanities and Social Sciences at Carnegie Mellon and as Vice President for Academic Affairs at York University in Toronto, Canada, as well as on the faculties of the University of Chicago and the University of Minnesota. He was founding co-editor of *Chance* and served as the Coordinating and Applications Editor of the *Journal of the American Statistical Association*. He is one of the founding editors of the *Annals of Applied Statistics*, co-founder and editor-in-chief of the new online *Journal of Privacy and Confidentiality* and founding editor of the new *Annual Review of Statistics and its Application.* He has been Vice President of the American Statistical Association and President of the Institute of Mathematical Statistics and the International Society for Bayesian Analysis. His research includes the development of statistical methods, especially tools for categorical data analysis and the analysis of network data, algebraic statistics, causal inference, statistics and the law, machine learning and the history of statistics. His work on confidentiality and disclosure limitation addresses issues related to respondent privacy in both surveys and censuses and especially to categorical data analysis. He is the author or editor of over 20 books and 400 papers and related publications. His 1975 book on categorical data analysis with Bishop and Holland, *Discrete Multivariate Analysis*: *Theory and Practice*, and his 1980 book on *The Analysis of Cross-Classified Categorical Data* are both citation classics. He served two terms as Chair of the Committee on National Statistics at the National Research Council (NRC) and is currently co-chair of the NAS-NRC Report Review Committee. He is a member of the U.S. National Academy of Sciences, and a fellow of the Royal Society of Canada, the American Academy of Arts and Sciences, and the American Academy of Political and Social Science, as well as a fellow

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MS: So, Steve, how is it that you came to become a statistician?

SF: It's actually a long story, because when I was in high school and entering university, I didn't even know that there was such a field. I was good at mathematics and I went to the University of Toronto, which was in my hometown—that's where the best students went if they could get in. I enrolled in a course called *Mathematics*, *Physics*, *and Chemistry*. It was one of the elite courses at U of T, and during the first year, as I went through my chemistry labs, I never succeeded in getting the right result when I mixed the chemicals up in the beakers; I realized chemistry wasn't for me, and so the second year I did only math and physics. Then there were the physics labs, and I could never quite get the apparatus to work properly to get what I knew was the correct answer. I still got an *A* in the physics lab, because I could start with the result and work backward and figure out what the settings were and things like that; but it was clear to me that physics wasn't for me as a consequence. So that left me with mathematics, and it was in the second year that we had a course in probability. So I was being gently introduced to statistical ideas. Then in my third year there was a course in statistics that was taught by Don Fraser, and he was terrific. His course was a revelation, because I didn't know anything about statistics coming in. Don followed the material in his *Introduction to Statistics* book and he began with probability theory and he brought into play geometric thinking throughout. When he got to inference, it was like magic. Of course, in those days Don did what was called "fiducial inference"—he called it "invariance theory" and later "structural inference"—where you went suddenly from probability statements about potential observables given parameters to probability statements about the data. I recall the old cartoon by Sydney Harris that people like to reproduce of the two scientists pointing to a blackboard full of equations, and one of them points to an equal sign and says, "And a miracle suddenly occurs here." That's sort of what happened in Don's class. He was a great lecturer, he was friendly with the students, and it was very clear that statistics was a really neat thing to do. Thus, in my fourth year I took three classes involving statistics

FIG. 1. *Miron Straf, Steve Fienberg and Judy Tanur at the University of Connecticut, October,* 2009.

FIG. 2. *Steve as a Toddler in* 1940*s in Toronto.*

FIG. 3. *Steve at Camp Tamarack, near Bracebridge, Ontario in 1952.*

and probability and then applied to graduate school in statistics. The rest, as they say, is history.

MS: So it was mathematics by elimination and statistics by revelation. Let's go back a bit. When did you discover that you had an aptitude for mathematics and statistics? In elementary school? Or high school?

SF: Not at all. In those days statistics never showed its face in the K-12 curriculum—this was before *Continental Classroom*.[1] Actually it was K-13 in Toronto where I was born and raised. They got rid of grade 13 only decades after I was in school. At any rate, although my mother thought I was genius—don't all mothers think that about their children—I don't have any memory of being anything other than just a good student. I was very good at what passed for mathematics, but even through high school I don't think I was truly exceptional, and, besides, we did pretty elementary stuff—algebra, Euclidean geometry, and then in grade 13 we had trigonometry. As I reflect on those days, I was good at mathematics, but certainly not precocious and I only took standard high school math and with a heavy component of rote and repetition. By the time I got to grade 13 I was at the top of my class, however, and in the province-wide exams at the end of the year I was No. 2 in my school. But I also played oboe in the orchestra and band, and drums in the marching band, as well as participating in several other extracurricular activities. So math wasn't much of a preoccupation and I didn't know what statistics and probability were all about at all.

FIG. 4. *Steve with Don Fraser and Nancy Reid at a conference on the occasion of Don's* 75*th birthday, June* 2000.

[1]*Continental Classroom* was a series of television "course" broadcasts by NBC on a variety of college-level topics in the early 1960s. Fred Mosteller taught the course on Probability and Statistics during 1960–1961

JT: So that explains your broad early work in math, physics and chemistry as a kind of omnibus course rather than going directly into math or statistics. So after your undergraduate work at the University of Toronto, you applied to graduate school; where did you apply and where did you end up going?

SF: Well, at the University of Toronto there had actually been many people to go into Statistics from MP&C. Don Fraser was perhaps the first, but then there were Ralph Wormleighton, Art Dempster and David Brillinger—they all went, by the way, to Princeton. The year before me there was John Chambers, and John had gone to Harvard. I knew John pretty well, and I asked him how it was at Harvard. He seemed pleased with what he was doing and I did apply to Harvard and was admitted. I also applied to Princeton, and in their wisdom they didn't think that I should carry on the tradition from the University of Toronto, and that made the decision easier for me.

MS: Were you disappointed about not being admitted to Princeton?

SF: Clearly at the time I was. This was my first rejection, and it prepared me in a way for what was to come when I submitted papers for publication to major journals! But Sam Wilks, who was the key person at Princeton with whom I had hoped to work, died in the Spring of 1964, before I would have arrived.

JT: By the time you went to Harvard you were already married, is that right?

SF: No, I had met my wife Joyce at the University of Toronto when we were both undergraduates. I was actually working in the fall of 1963 in the registrar's office, and on the first day the office opened to enroll people, Joyce came through. And one of the benefits

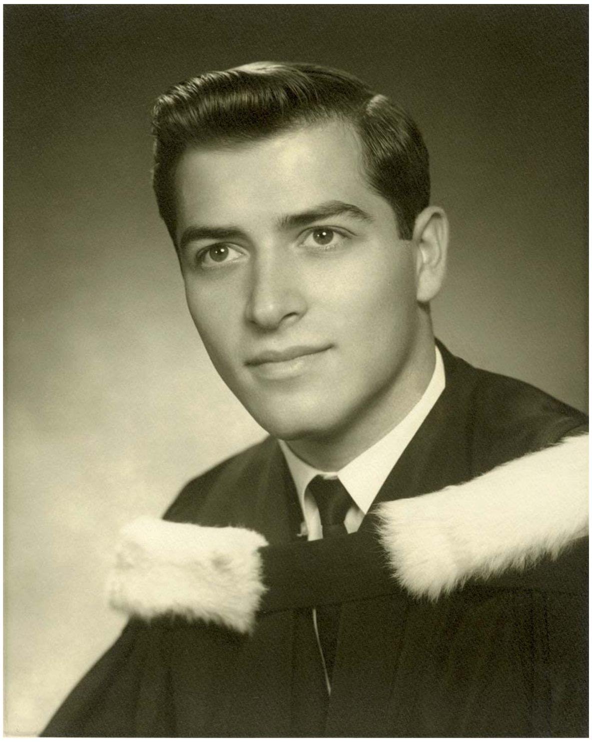

FIG. 5. *Graduation portrait from the University of Toronto*, 1964.

about working in the registrar's office, besides earning some spending money, was meeting all these beautiful women students passing through. That first day I made a note to ask Joyce out on a date. The next day she came through again, this time bringing through another young woman who turned out to be the daughter of friends of her parents. And I thought this was a little suspicious, but auspicious in the sense that maybe I would succeed in getting a date when I asked her. And the next day, she came through again! This time with her cousin! Then I knew that this was really going to work out. And it did. We got engaged at the end of the summer of 1964 after I graduated, but we weren't married when I went away to graduate school. In fact, yesterday I was talking to one of the students at the University of Connecticut who was a little concerned about graduate school; it was wearing her down, and I told her I almost left after the first semester because I wasn't sure if I was going to make a go of it, in part because I was lonely. But I did survive, and Joyce came at the end of the first year; we got married right after classes ended, and we've been together ever since.

FIG. 6. *Joyce and Steve in Portugal for a conference on privacy and confidentiality*, 1998.

MS: And where were your children born?

SF: Ah, *conceived* in various places, born in others. We believe that Anthony, my older son, was actually conceived in Scotland, on the vacation we took just after I graduated from Harvard. He was born in Chicago, where I had my first academic appointment, and, indeed, as we traveled across the country, from Boston to Chicago, Joyce began experiencing morning sickness (all day long), which didn't make for such a great trip. Then Howard was born in Minnesota just after we had moved there and I had joined the University of Minnesota faculty.

JT: Tell us more about what happened when you first arrived at Harvard.

SF: Well, one of the reasons I went to Harvard is that they not only gave me a fellowship, but also a research assistantship to work with Fred Mosteller. The day after I arrived, I went into the department because I didn't quite know what a research assistant did, and I went to see Fred (at the time he was Professor Mosteller, of course—I didn't learn to call him Fred until later). Fred was busy, but his assistant, Cleo Youtz, said he would like to have lunch with me. So I came back for lunch, and we went to the Harvard Faculty Club. Fred was being very courteous, and he suggested I order the horse steak, a special item on the faculty club menu at the time. And the horse steak came—I'm not sure if you've had horse steak—it's not *quite* like the kinds of steaks we normally order, it's a *little* bit tougher. I cut my first piece of horse steak, I put it in my mouth and started to chew. And then Fred began to describe this problem to me. It was about assessing probability assessors. I didn't understand a thing, and he's talking away, and I'm chewing away. Then Fred asked me a question, and I'm chewing away. At this point, he pulled an envelope out of his pocket and on the back of it there were these scribbles. He handed it to me, and I'm *still* chewing because you really can't eat horse steak except in very small bites. It turned out that the scribbles were notes

FIG. 7. *Steve dining with Fred Mosteller at ISI meetings in Paris*, 1989.

from John Tukey about this problem. In fact, this was a problem that John and Fred were working on for some larger project, and my job was to translate the chicken-scratches on the back of the envelope into something intelligible, when I didn't know anything about what was going on. I worked at it for a while, and then Fred slowly told me what John's jottings meant, and the key idea was that for assessing probability forecasts, you have to look not just at the equivalent of means, or the bias in them (known technically as calibration), but also at the equivalent of variability (how spread out the forecasts are). Actually, that was a very important lesson, although I didn't have any clue about it in my first months at Harvard.

Over the course of my first fall at Harvard, I discovered a paperback book called *The Scientist Speculates: An Anthology of Partially Baked Ideas*, edited by Jack Good, with whose work I later became very familiar. In it was a short essay by Bruno de Finetti on assessing probability assessors, and de Finetti's ideas went into the technical report I wrote up on the topic with Fred and John. Fifteen years later, at the Valencia I Bayesian meeting, Morrie DeGroot and I began to work on the problem and ultimately wrote three papers on the topic of calibration and refinement of probability forecasters, heavily influenced by that first research exercise with Fred.

MS: I wanted you to talk about Fred. Fred has been a very influential person in your career, and not just during your thesis. Maybe you want to tell us a little bit more about how he influenced your lifek and also how you came to go from Harvard to Chicago.

SF: Well, during that first year I worked on several problems with Fred and I wrote up some memos, but they never quite moved into papers at the time. Fred was pretty busy, and I got interested in Bayesian inference and multivariate analysis. I had begun to take an interest in Bayesian methods, having participated as a first year student in a seminar across the river at the business school run by Howard Raiffa and Bob Schlaiffer. At the time, Art Dempster was the person who seemed to be most involved in these Bayesian things and multivariate analysis, so I began to meet with him. In the process of working with Art, I met George Tiao, who was visiting the Business School with George Box for the year. As a consequence, George and I wrote a paper together on Bayesian estimation of latent roots and vectors but it just didn't look like it was going to be a thesis problem.

The next summer, Fred ran into me in the hall and said he had some problems that I might like to work on. Fred had become deeply involved in the National Halothane Study at the NRC and, unlike most NRC studies, he and others—Tukey, John Gilbert, Lincoln Moses, Yvonne Bishop, to name a few—were actually analyzing data and creating new methods as they went along. The data essentially formed a giant contingency table and Fred got me working on a few different problems that ultimately came together as the core of my thesis. In the process I collaborated on separate aspects of the work with John Gilbert, Yvonne Bishop and Paul Holland. I did most of the work in 1967 and that was the summer of "The Impossible Dream," when the Boston Red Sox won the pennant. I would work into the wee hours and go to Fenway Park and sit in the bleachers for the afternoon games. Professional sports where cheap in those days. We also used to go to Boston Gardens for Bruins and Celtics games. Fred was also a Red Sox fan and he actually got tickets for some of the 1967 World Series games. I was envious, but when I returned to Boston in 1975 on sabbatical we both were able to get World Series tickets. I got tickets for game 6 and Fred got them for game 7!

Fred introduced me to lots of other statistical problems. I was also his TA one year, working with Fred and Kim Romney who was in the Social Relations department at the time. Then the time came to get a job, and Fred said to me, "Where would you like to go?" Things were different in those days, as you will recall from your days at Chicago. We went through the list of the best places in the field, at every one of which Fred had a friend. He called up John Tukey at Princeton, he called up Erich Lehmann at Berkeley, Lincoln Moses at Stanford and Bill Kruskal at the University of Chicago. I either got offers without showing up for different kinds of jobs at these places or I got invited out for an interview. When I was invited to interview at the University of Chicago, it just seemed like a really neat place. All the faculty members were friendly. The temperature in January was really cold, but I liked everything about the university from the people to the architecture; it looked like a university. Leo Goodman was there on the faculty and he had done work that was directly tied to contingency table topics in my thesis. Chicago just seemed like a great place to go to, so I did.

JT: It was there that you first met Bill Kruskal and started being influenced by him?

SF: Bill Kruskal was the department chair at the time, and I barely got in the door before he began talking to me about a slew of different statistical problems...

JT: Without horse steak?

SF: Yes, without horse steak. Bill would just come and say, "What do you know about this?" And one of the first topics we actually discussed was political polls. This was the summer of 1968; there was a lot going on politically in the U.S., and the *Sun Times Straw Poll* was showing up in the newspaper regularly. Two of the key questions were: What was their real methodology? How accurate were their predictions? I began to save the data from the newspaper reports and work on the question of variability and accuracy. Then Bill got me to do a trio of television programs with Ken Prewitt and Norman Bradburn on a special series that aired at 6 o'clock in the morning when nobody ever watched. But right from the beginning, Bill and I interacted; he introduced me to Hans Zeisel in the law school, to people in the business school, in sociology. It was really hard to trail after Bill, because he was interested in everything in the university and outside, and almost everything we discussed seemed pretty neat. So, as I launched my professional career at Chicago, I tried to do something similar—not precisely the same as the way Bill did things—but similar.

MS: Bill was a real Renaissance man, and I presume you were a recipient of his many clippings from newspapers.

SF: Well, the clippings started when I was in my first year—he's the one that started to give me the *Sun Times Straw Poll* clippings. But it wasn't just clippings. Bill would leave library books for me in my box; he would go to the library, which was on the second floor of Eckhart Hall, the building we were in, and he would browse—people don't do that today—the stacks are closed. He would come back, armed with books, and he would share them with his colleagues and get Xeroxes of pages. And this continued up through the 1980s. I would always get packets of different materials from Bill, including copies of letters to somebody else that would say: "I hope you don't mind my sharing this with a few of my closest friends and colleagues." I had this image that he was making hundreds of Xeroxes to send around the world.

MS: And before that, carbon paper. So, tell us a bit about your life after Chicago.

SF: The University of Chicago really was a great place for me to work. I had a second appointment in theoretical biology, which was interesting because I had never taken a course in biology as a student. And actually it was a very formative experience, because it taught me that I could go into an area that I had never studied, never learned anything about, and learn enough for me to make a difference in the application of statistics. I wrote papers on neural modeling, and I wrote papers on ecology; I didn't do a lot of genetics, but I read genetics papers and books because I included that material in the course on stochastic processes that I taught. Unfortunately, Chicago wasn't the safest of places in those days, and Joyce made it pretty clear that she wanted to live in a place where our children could play in the backyard by themselves, not under adult supervision 100 percent of the time. So I began to be receptive to conversations with people from the outside, and soon I was approached by one of my former students, Kinley Larntz, who had just joined the University of Minnesota. They were looking for a chair for the newly created Department of Applied Statistics, as part of a School of Statistics. So after four years at Chicago, I became an administrator as well as researcher and teacher.

MS: Did you work with Seymour Geisser there?

SF: The School of Statistics was an interesting idea. Minnesota had had a statistics department, and it had run into some problems over the years. The university came up with this plan to reinvigorate statistics, and they created the School of Statistics. Seymour was the director, and the School was supposed to have three departments. There was the old statistics department, renamed as the Department of Theoretical Statistics, there was the new applied department that I was chairing, and there was the Biometry department in the School of Public Health. But the biometry faculty didn't really seem to want any part in this, and so they resisted, and ultimately the school had two departments plus the Statistical Center—the consulting center that was associated with our department on the St. Paul part of the Twin Cities campus. Seymour and I interacted

FIG. 8. *Judy Tanur, John Bailar, Steve, Henry Block and Jim Press at a conference in Bejing*, 1987.

throughout my eight years at Minnesota, but we never wrote a paper together.

JT: I want to take you back a little more. You talked about these two giant figures who were colleagues and mentors—Fred Mosteller and Bill Kruskal. How do you see how they shaped your career, your interests—not only technical, but practical?

SF: One of the things I didn't know as a graduate student was how easy it would be to work on and contribute to new problems and new areas of application. The worst fear of a graduate student—well, the worst fear is that they won't finish their thesis—the second fear is they won't have a new idea, and, in fact, 80% of students never publish anything other than their thesis. But Fred was going from area to area: when I arrived at Harvard he had just published *The Federalist Papers* with David Wallace; while I was there he was leading the effort on the Halothane report; I worked with him evaluating television rating surveys from Nielsen and other companies for a national network (that was a consulting problem). He just seemed to work around the clock on all sorts of different topics, and so I figured that's just what a statistician did. It's funny because, in some senses, clearly, everyone didn't behave like Fred, as we all know. But that was my model! So when I got to Chicago and Bill acted in the same way, and Paul Meier in addition, that seemed like a natural way for me to do work as a statistician. They seemed to work around the clock on statistics, so I did too.

Now Fred liked art; in later years he actually took up reproducing art and it showed up in his office. When I was a graduate student I went into his office one day and there was a picture by Escher, the Dutch artist, called "The Waterfall" and I was very surprised because I had been introduced to Escher as an undergraduate. Escher's work showed up on the cover of a book called, *Introduction to Geometry*, written by Donald Coxeter—the great geometer at the University of Toronto. I had three courses on different aspects of geometry from Coxeter. This influenced some of my thesis research—and I still do some geometry—but I also

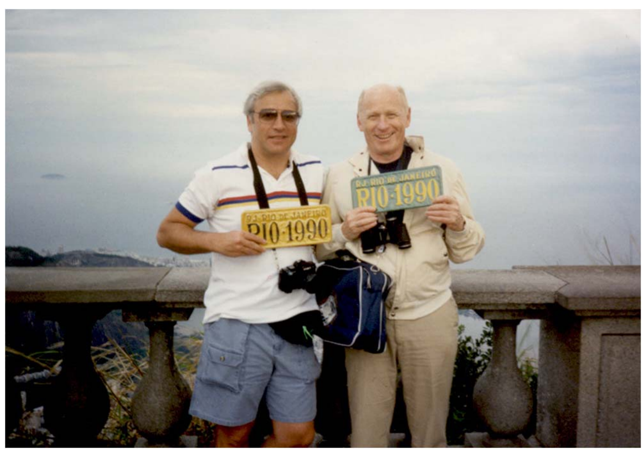

FIG. 9. *Steve and Seymour Geisser, attending a Bayesian Workshop in Rio de Janeiro, Brazil, summer* 1990.

learned about Escher from Coxeter! And there was this Escher print in Fred's office which I recognized immediately. Fred told me where he had purchased it, and shortly afterwards I went off to the store. I still own two Escher prints as a consequence, ones that I couldn't afford to buy today, all because of Fred. Fred and I would occasionally go off to museums, and while we looked at the art we would talk about statistics, art and other topics.

Both Fred and Bill were Renaissance men and I didn't know how I would do things in the same way they did, but it became very clear to me that just doing papers in the *Annals* and in *JASA* wasn't enough. While I had colleagues whose careers looked like that, I thought I should be doing something different with my career. I was easily seduced into all these other activities—and everything was so much fun. For example, Dudley Duncan, the sociologist, called me one day and asked me if I would join an advisory committee set up by the Social Science Research Council on social indicators in Washington. I hadn't been to Washington since I was 7 years old and I went off to this meeting and then spent eight years interacting with giants in the field of sociology and survey methods! That experience just reinforced the way I was using my statistical knowledge in diverse applications.

And of course Bill and Fred would just sort of nudge me once in a while to get things done that they cared about deeply. In particular, Fred wanted to see the log-linear model work that his students had done for the Halothane study appear in a book. Fred was big on books. And as I left Harvard, he gathered together all the different students who had worked on different aspects of contingency table analysis—Yvonne Bishop, Dick Light, myself and Paul Holland, who was a junior faculty member, for a meeting at his house. There were also a couple of other faculty members who sort of disappeared by the wayside in this enterprise, there were a few more graduate students—Gudmund Iversen who ended up at Swarthmore, for example—and Fred said, "We need to have a book on this."

But we didn't have Fred's grand picture in mind and the book didn't begin to take shape until long after I had joined the faculty at the University of Chicago. I taught a contingency table course in my first year there and it included the first three Ph.D. students I worked with—Tar (Tim) Chen, Shelby Haberman and Kinley Larntz. Shelby extended Yvonne's code for multi-way tables and this inspired his thesis. I began to use iterative proportional fitting on new problems and this triggered a paper on multi-way incomplete tables and a draft of the first book chapter. But then everything progressed rather slowly, and the book took a full six years to produce. Fred kept pushing the book behind the scenes.

One of the things I learned is the time to produce a book goes up as the power of the number of authors. It would have taken less time if I had written the book myself instead of with Yvonne and Paul. But while we worked at the core of the enterprise, the three of us had different conceptions of some materials, and this slowed us down. Fred was a full partner, pushing us to "get the job done." He edited draft chapters over and over again, and Dick Light contributed big chunks to the chapter on measures of association, which Paul and I redid and integrated with the asymptotics chapter. If everyone who had come to Fred's house back in 1968 had become involved, we might still be working on the book today! Fred didn't want his name on the cover of the book. So we had this back-and-forth. The book ended up with five names on the title page; it's Yvonne Bishop, Stephen Fienberg, Paul Holland, with the collaboration of Frederick Mosteller and Dick Light; Dick had contributed to a chapter in the book and Fred had contributed to the whole enterprise.

JT: The book, which many have called the "Jolly Green Giant" because of its cover, really put you on the map. In fact, that's how we met, when I took the short course the three of you gave based on the book in 1976 at the Joint Statistical Meetings.

SF: We actually met earlier, when Fred organized a meeting in Cambridge to discuss the ASA-NCTM book projects that ultimately produced *Statistics by Example* and *Statistics*: *A Guide to the Unknown*, your first *magnum opus*. I was a bit intimidated since you seemed to be the organizer for *Statistics*: *A Guide to the Unknown*, and so we just didn't talk much.

MS: Steve and I met around the same time as well. I remember his coming to Chicago to interview and talking about the geometry of 2×2 tables. I asked him a question which he didn't really answer and then he wrote a paper about that problem several years later!

SF: But when I got to Chicago you were one of the few good students who didn't take my contingency table course. You were too busy campaigning for Hubert Humphrey and worrying about weak convergence!

MS: Well, one of the things that you have advanced in that book and elsewhere derives from the geometric structure that gave you so much insight into what's going on in these tables. Now, you mentioned taking geometry at Toronto, and we know R. A. Fisher was influenced by this, so how did that play out in the later research?

SF: It's come into play in an amazing sort of way. If you look at the cover of *Discrete Multivariate Analysis*, there is an artist's depiction of the surface of independence for a 2×2 table. You'd hardly know it was a hyperbolic paraboloid sitting inside a tetrahedron by the time the artist got done with it, and you see one dimension of rulings—a hyperbolic paraboloid has two dimensions of essentially orthogonal rulings—and those are things I actually learned from Coxeter in that course on the Introduction to Geometry. And so my first work actually drew upon that; I wrote a paper with John Gilbert on the geometry of 2×2 tables that appeared in *JASA* and published a generalization in the *Annals*, and I always thought about contingency tables and other statistical objects geometrically. Don Fraser thought geometrically, and so you're always up here "waving arms" in some abstract space, and he would always wave with his arms. And I think in high-dimensional space in some sense, although obviously we don't see in high-dimensional space. But a lot of statistics is projecting down into lower-dimensional spaces. I had left the geometry stuff behind, except for motivation, until I got into confidentiality research in the 1990s.

In the 1990s there was a paper, unpublished for five years by Persi Diaconis and Bernd Sturmfels. Persi was at Cornell and Bernd had been at Cornell but moved to Berkeley. In the paper, they talked about the algebraic geometry structure associated with contingency tables. This turned out to be right at the heart of what I needed for my problem, and so I learned algebraic geometry, which I had not really studied carefully before. I learned at least enough to bring my problems to Bernd for help. And one of the things I realized is that figure on the cover of Bishop, Fienberg and Holland was being used by algebraic geometers in a different context; it's called a Segre Variety, named after Corrado Segre who was one of the fathers of algebraic geometry. That work is now reflected in the theses of a couple of my former Ph.D. students and lies at the heart of a lot of what I've been doing over the last several years, including recent work on algebraic statistics and network models.

JT: I think I derailed you sometime back where you were talking about the trajectory of your career. And we've left you at Minnesota. Can you tell us why you left?

SF: Minnesota was a giant bureaucracy. It was a big, big university, and one of the moments that convinced me of this was after I had presented a report, prepared with colleagues from around the university, to the president and the vice-presidents on the teachings of statistics at the university, where I had pointed out that 40 different departments or units where teaching statistics or courses in which statistics represented a serious part of the activity. Virtually all of this was going on with little or no coordination with the School of Statistics. And then I met him [the president of the university] about a month later at a reception. Joyce and I were going through the reception line, and I shook his hand, and he asked what department I was from. I said applied statistics, and he said, "*Do we have a statistics department at the University of Minnesota?*" At that point I said to myself, "Oh my goodness!" and I understood where the School of Statistics and my department stood in the big picture of the university.

FIG. 10. *Steve*, *Judy Tanur and Morrie DeGroot*, *Joint Statistics Meetings*, 1978.

A year or two later, I was wooed by friends at another Big Ten university, but the right offer didn't quite come to pass. In the mid-70s I was working as an associate editor for the *Journal of the American Statistical Association*, initially with Brad Efron as theory and methods editor, and then with Morrie DeGroot. Later I became Applications and Coordinating Editor of *JASA*, and so Morrie and I worked together. We had become friends a number of years earlier, drinking in a bar together at an IMS regional meeting. Morrie and Jay Kadane, who had joined the Department of Statistics at Carnegie Mellon in the early 70s, and I would interact at the Bayesian meetings that Arnold Zellner organized twice a year. They both knew that I had flirted with the possibility of leaving the University of Minnesota, and they said, "You should just come to Carnegie Mellon; you could bring the rest of *JASA* over and we'd have the whole journal. Besides, it's a great place." So they worked on the possibility of an appointment for me. When I came to interview, it wasn't just to meet with the Dean, and with Jay and Morrie and the people in the department that I knew. They took me to see the president of Carnegie Mellon (CMU), who at the time was Richard (Dick) Cyert. Dick was an economist but also a statistician! He took courses from Hotelling and Cochran at Columbia as a graduate student, and although his degree was in economics, he always thought that he was a statistician as well. In particular, he was a member and Fellow of ASA. Dick helped found the CMU Department of Statistics in the mid-1960s when he was the dean of the Graduate School of Industrial Administration. He was actually the acting chair at the outset until Morrie took over. So the staff ushered me into his office. I had never met Dick before, but that afternoon I spent two hours with the president of Carnegie Mellon. And I told you about my interaction with the president of the University of Minnesota! Here I am sitting with the president of Carnegie Mellon, this great university, and he's telling me how important it is for me to come to Carnegie Mellon and what I'm going to do for the field of statistics. He said, "If you come here, everything you do will be called statistics. You will get to change the field." So I came. And I hope that I've changed parts of the field.

MS: Cyert was a visionary, and really led the Graduate School of Industrial Administration to a high place among business schools and understood that he needed quantitative strength, and so he influenced you and supported you. I wanted to ask about one of your greatest honors, and that is your election into the National Academy of Sciences. Where were you and how did you get the word?

SF: Most people don't know what goes on at the National Academy—it's like a secret society—and it's selection process is Byzantine, running over the course of one or more years. At the end, the NAS members meet in Washington at the annual meeting in a business meeting and they elect the new members. That happens between 8:30 and 9 in the morning; then they take a break in the meeting and everybody rushes out to find a telephone and they call their friends and the newly elected members to the section to congratulate them. This was in the spring of 1999, and I was teaching—actually that year I was teaching an introductory statistics class, so I had to be there relatively early—it was just at 9 o'clock, I was opening the door to my office, and the phone rang. I answered and it was several friends, mainly demographers—Jane Menken, Doug Massey, a couple of others—and there was a chorus on the phone saying "Congratulations, you've been elected to the National Academy!" I was floored, because I'm not quite sure whether they knew, a year or so earlier I wouldn't have been eligible, because I was born and raised in Canada, and I hadn't become an American citizen until January 1998. Thus being elected the next year was a special honor.

FIG. 11. *Richard Cyert, Dennis Gillings and Steve, at a National Institute of Statistical Sciences Board of Trustees Meeting*, 1993.

JT: You have received many other awards and honors; that must be very exciting.

SF: Well I would be lying if I said that receiving honors and awards is not fun, and each is always very special. But I am reminded about something that Fred taught me. He said that awards and honors are really not for the people who get them, but they are for the field. Of course the person getting the honor benefits, but the field benefits more, for example, when statisticians get elected to the National Academy of Sciences. In that sense we don't have enough big awards.

MS: There are some of our colleagues who are happy that there isn't a Nobel Prize in Statistics, and as a consequence statisticians cooperate more with one another than scientists in other fields. Do you agree?

SF: Well, I think if we follow Fred's reasoning we would all be better off with a Nobel Prize in Statistics because once a year all of the newspapers and media in the world would focus on our field and the accomplishments in it. What most statisticians don't know is that there almost was a Nobel Prize!

The story goes back several decades when Petter Jacob Bjerve, who was the director of Statistics Norway, began to raise funds for a Nobel Prize in Statistics. He was off to a good start when he ran into a political obstacle. Those in charge of the prize in Economic Sciences objected because, they argued, their prize encompassed a large amount of what was important in statistics. In the end Bjerve abandoned his quest, and the money he raised was left in a special account in Statistics Norway. Finally, the government auditors forced Statistics Norway to close this account and our colleagues there decided, among other things, to use the funds to host a special international seminar, to which they invited statisticians such as Fred Smith from the UK, Jon Rao from Canada, Wayne Fuller, me and a few others. They paid for our spouses to come as well and we got the royal (small R) treatment, with relatively fancy hotel rooms and outstanding dinners. So in this sense you could say that I ate the Nobel Prize in Statistics, although there is no public record and it doesn't show up on my CV.

JT: You've been active in several committees and panels and so forth, including at the National Academies before and after your election as a member—what stands out particularly from those?

SF: Well, of course this is Bill Kruskal at work—most statisticians who are going to read this interview don't know the history—Bill Kruskal founded the Committee on National Statistics (CNSTAT) at the NAS. It was an outgrowth of the 1971 Report of the President's Commission, chaired by Allen Wallis and co-chaired by Fred Mosteller; and Bill talked the people at the National Academies, and the National Research Council (NRC, its operating wing), into creating a committee although there was no external funding, and the NAS really had to put up resources. Bill ultimately got some money from the Russell Sage Foundation to tide the committee over with a part-time staffer—Margaret Martin, who was and is absolutely fabulous and with whom the three of us have worked—and the committee slowly got going. Bill was succeeded by Con Taeuber. At that time I actually was on another committee, on the rehabilitation of criminal offenders, but Miron was working for CNSTAT and I would run into him on occasion. I got to join CNSTAT a year or so later while I was still doing the work on criminal justice. Getting involved in CSTAT was like all these other activities I have been describing—I was exposed to lots of new ideas and problems to work on. I was like a kid in a candy shop! The committee didn't have a lot of projects then, but I just got to look around the Academy and the Federal government, and there were possibilities everywhere. I could only do so much, but I pushed the staff to do other things and got my friends on the committee to lead panels. By the mid-80s the committee was humming and there were all these neat activities on census methodology, on cognitive aspects of survey methodology, statistical assessments as evidence in the courts, sharing research data—there was just no end.

MS: I wanted to ask about one of them in particular, which Judy chaired and which you were instrumental in creating, and that is Cognitive Aspects of Survey Methodology. When you were inducted into the American Academy of Political and Social Sciences, you referred to that in your speech as one of the most important activities that you had participated in. Why was this and how did it affect your work?

SF: Well, sample surveys is a very strange part of statistics. In my department, nobody else really does it, in the research sense. People think the theory is settled. But *doing* surveys is *really* hard. The measurement problems are enormous. Designing questionnaires is a big, big problem. In the 1970s I got interested in the National Crime Survey on Victimization through the SSRC committee on social indicators in Washington on which I served. I learned about the difficulties in counting victimization events. In 1980 Al Biderman, who was involved in the re-design effort for the victimization survey, brought together a few people from the re-design project with cognitive psychologists to ask if we could learn something from cognitive science. I thought this was just terrific because I could see ways that I could take methodological statistical ideas and really intertwine them with the theoretical ideas that came out of cognitive psychology. As a consequence, I pushed for that CNSTAT activity even though others thought it made no sense. I was part of the CNSTAT workshop that you and Judy organized—Judy and Beth Loftus and I wrote a series of 4 papers on cognitive aspects of surveys afterward. I was also on the SSRC council, and we created a committee that followed up on those activities. It brought in new people to the enterprise, and it helped get these ideas embedded in the statistical agencies. Janet Norwood ran with the idea at BLS. It was part of the culture at NCHS at that time because Monroe Sirkin was at the CNSTAT workshop and a moving spirit in establishing a cognitive laboratory at NCHS. The Bureau of the Census was actually the last of the big three agencies to create a separate laboratory facility—but they did—and the influence spread because the associated ideas changed research at the boundaries of survey methods and psychology in a variety of different ways. The reason I am especially proud of this activity is because you'd hardly know that there was any statistical theory or methodology lurking behind it, but there really was.

FIG. 12. *Participants at* 1983 *CNSTAT Workshop on Cognitive Aspects of Survey Methodology watching a survey interview video, from left to right*: *Kent Marquis*, *Judy Tanur*, *Phil Converse*, *Lee Ross*, *Steve* (*in upholstered chair*), *Miron Straf*.

MS: It's really had a profound effect on the survey field, and now in many places it's commonplace—concepts of cognitive interviewing and all that.

You've been especially close to your students, fostering them personally as well as professionally. Pictures of you attending weddings of your students appear frequently on websites in your honor. So could you tell us a little about your personal interactions with your students.

SF: Well, in the early years the students were my contemporaries. In fact, I had a couple of students who were older than I was. Kinley Larntz was not only my Ph.D. student and collaborator, but we were good friends, and remain so. Over the years I got a little older than my students, and when I moved to Carnegie Mellon I really had the opportunity to have a different kind of student, and with them different kinds of interactions. We were a small department in those days and I interacted with lots of students, not just those whose research I supervised. Each of the students I worked with then was interested in a somewhat different topic; they went in different directions, and we remained close in most instances.

But then, something happened—first, I became a dean, and then four years later I left Carnegie Mellon, as you know. I had a second administrative career going on the side—actually, I had three careers, or four. There was also the committee work at the National Academy, which was a full-time job for awhile, there was the methodology I worked on in part with students in the Department of Statistics, and I was also an administrator—I was Department Head for three years and then I was Dean of the College of Humanities and Social Sciences. I was on an administrative track in the late 1980s and early 1990s, and my contact with graduate students actually tailed off toward the end of my time as Dean. I was also teaching, but there are only so many hours in the day and days in the week. In 1991, I left and went to the York University in Toronto as Academic Vice-President (that's like a provost—they don't have that title at York) and so my regular ties with graduate students were severed. I resigned from Carnegie Mellon to go to York, although we didn't sell our Pittsburgh house, and I returned to Carnegie Mellon a few years later and re-joined the department.

I like to describe the move back to Carnegie Mellon as a promotion to the best position in the university—as a tenured professor with no administrative obligations. I slowly began to work with graduate students again. Somewhere along the way I think I had learned something, which is you can't necessarily get graduate students to do what you want, and thus what you have to do is get them to do what they want to do in the

FIG. 13. *Steve with friends at the Objective Bayesian Analysis meeting in Rome, June, 2007. From left to right: Steve, Larry Wasserman, Jim Berger, Susie Bayarri, Robert Wolpert, Isa Verdinelli.*

best possible way. You have to get them to complete a thesis, but you have to be able to get them through and have them gain confidence in what they're doing so that they think they can make a difference. And I was lucky—I just had fabulous students; they were terrific people and all the rest of the stuff just sort of happened. I had the opportunity to give away in marriage one of my students, Stella Salvatierra, who was working in Spain, at a ceremony in the mayor's office in Bilbao, because her father had a heart attack and couldn't come to the wedding. And there have been several other weddings since! Because my students have been so great, the best thing I can do in some sense is to get them to do the things that they do best. That's in many ways a serious part of my legacy.

JT: I was going to ask you what advice you would have for graduate students in statistics, or undergraduates for that matter. Clearly, the best advice I could give would be for them to come to be your students, but since you can't spread yourself totally thin, failing that, what alternative advice would you offer?

SF: Well, I really can't work with them all! It's really bad because now we've got this undergraduate program with upwards of 150 majors. I can deal with one or two graduate students at a time. But my advice to budding statisticians is simple: statistics is an exciting field. There are all these neat problems. There are neat theories, neat methods, neat applications; we're in a new world. Big, big data sets. My joint appointments are now in the Machine Learning Department and in the Heinz College (of Public Policy and Management). I'm working with data sets that people couldn't conceive of dealing with a few years ago. And the students I'm working with have the ability to go and do things with those data sets that were unimaginable a decade ago. So my advice is simple. Work with data, take problems seriously, but you have to learn the mathematics and statistical theory if you want to do things right. And then you need to take seriously teaching people what you've done, not just doing the research. You need to get the descriptions of your work into a form that other people can understand—that's a really important part of what we do. That's what National Academy reports are all about. Academy reports don't have impact if they're badly written. Enormous effort goes into the executive summaries of reports, into the review process, and everything up the line. Learning how to do that as a student is time well spent. It's too late when you're a full professor and you still haven't learned how to write articles so that other people can understand what you've done.

FIG. 14. *Steve with his wife Joyce and many of his former graduate students at a 65th birthday celebration at Carnegie Mellon, October, 2007. From left to right: Ellie Kaizer, Edo Airoldi, Elena Erosheva, Jason Connor, Sesa Slavković, Mike Meyer, Joyce, Steve, Alessandro Rinaldo, Justin Gross, Russ Steele, Adrian Dobra, Amelia Haviland, Elizabeth Stasny.*

MS: So, of your vast experiences, what are you the most proud of?

SF: I'm actually proud of a number of things. By the way, I didn't tell you what my fourth career was. I play ice hockey—I still play, that's number one, although the one for which I have the fewest skills or accomplishments.

MS: All right, let me interrupt you...

SF: Ha ha, no-no, as I left the locker room last Saturday night, one of the guys across the dressing room said to me, "So how many years have you been playing?" And I said, "62." He then said, "*62*?" and silence ensued. But maybe hockey is really number two; number one is my children and my grandchildren. They're really amazing. They're another part of my life. Joyce and I were really fortunate; I have two very smart sons, Anthony and Howard. They have independent careers, they have lovely wives...

MS: Where are they now?

SF: Anthony lives in Paris, and I have five grandchildren in Paris, four granddaughters and a grandson. And Howard lives in the DC area and I have a lovely granddaughter in Vienna, Virginia. Howard actually has come very close to statistics, as government liaison for a consortium dealing with surveys and marketing. The grandchildren are terrific. I love being with them. We get to look after them every once in a while.

Then there are my students. They're really the people who are going to do the things that I can only imagine. As I look back over what I've done, I see a changed field of statistics. Fred Mosteller and Bill Kruskal were fabulous—and we've talked about how they shaped all three of *our* careers, not just my career. And they launched the Statistics Departments at their respective universities. I was part of both departments and their programs in retrospect look "traditional." They emphasized mathematical statistics and probability. I like to think that when I left Chicago and went to Minnesota, I started to change what statistics did and how we thought about it. And applications today sit at the core of much of statistical theory and methods, and in my department at Carnegie Mellon our students come out having worked on multiple applied projects, and they're in demand, because that's the fu-

FIG. 15. *The longtime members of the Carnegie Mellon Department of Statistics in the DeGroot Library*, 2011. *Back row*: *Rob Kass*, *Mark Schervish*, *Steve*, *Joel Greenhouse*; *middle*: *Margie Smykla*; *bottom row*: *Jay Kadane*, *Bill Eddy*, *John Lehoczky*.

FIG. 16. *Steve and Bill Eddy celebrating the* 20*th anniversary of Chance*, *a magazine they co-founded in* 1988, *wearing their original Chance t-shirts*.

ture of our field. People recognize that advances in statistical methods—and theory—are intertwined with real problems, major applications. I like to think that I contributed to the change that we've seen over the past 40 years.

MS: Very nice, Steve. What you talk about is a legacy, not the individual research that may wane in importance over the years...

SF: And it's not just my work, it's a collective...

MS: But it's the influence of your students, as well as your children. I wanted to interrupt, because I never thought you had four careers, I thought you had dozens of careers. You talked about these professors that, you know, worked 24/7, so that was your model. As long as I've known you, you're always multi-tasking, and you were doing that before the word was even in vogue. You're fielding questions at a seminar or flying a hockey puck across the ice. Did any of that rub off on your sons, on your students?

SF: I don't think that either Anthony or Howard is quite as obsessed as I am with doing so many things simultaneously.

MS: How fortunate...

SF: That's right! But Anthony did play hockey in Paris for many years, and both Anthony and Howard have these terrific kids—since Anthony has five they

FIG. 17. *Steve* (*on the right*) *playing for the Division C national championship as a member of the Leiden Beaver Beer Team*, *in Eindhoven*, *March*, 1997.

take up more of his time than mine did. Actually, Anthony has inherited some of this multi-tasking, at least at some level. He's created his own business in France—a subsidiary of a Dutch insurance company. His job went from finding the location to organizing the offices, to hiring the staff, to inventing the insurance policies and making sure that they were consistent with the ones of the parent company.

My students also develop multiple facets of their careers and lives. I tell them when they come in and ask if they can work with me that there are a couple of things that are going to happen if the arrangement is going to succeed. One is they're going to live and breathe statistics. I see it everywhere. One of my favorite examples in my little contingency table book came out of the program from the symphony at the Minneapolis Orchestra one night when we were there in the 1970s. It didn't *quite* look like a contingency table, but I made it into one, as Table 2.4. Then in my book, I described why you shouldn't analyze it the way you would have otherwise because the units of observation are not independent. At any rate, I tell the students that I expect them to live and breathe statistics. They'll get their ideas in the shower... they'll play hard too, but when all is said and done, if they're not into what they're doing, they should find another advisor, because other people have different attitudes about work and how to get your inspiration! Students of course have their own lives, and as I've said, you don't tell students what to do, they tell you what they want to do.

FIG. 18. *Steve with twin granddaughters*, *Tiffany and Selena*, *trying out their new bikes*, *Paris*, 2006.

JT: What's next? For you?

SF: Wow. I'm too busy to stop at the moment to find out! I still have more than one job. I'm editing, with some others, the *Annals of Applied Statistics*, I have launched the *Journal of Privacy and Confidentiality*, I'm co-chair of the Report Review Committee at the Academy.[2] I have a whole bunch of new Ph.D. students and post-docs. We've got some absolutely fantastic projects going on: research on confidentiality problems and on network modeling, which by the way, links

[2]Steve took over as editor-in-chief of the *Annals of Applied Statistics* on January 1, 2013, and is simultaneously serving as the founding editor of yet another publication, *The Annual Review of Statistics and its Application*, scheduled to launch within the year.

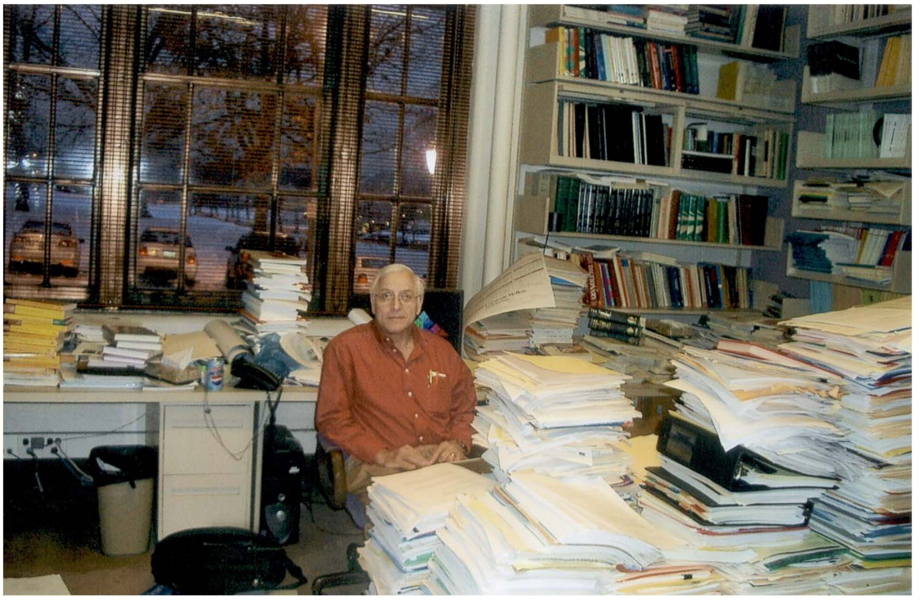

FIG. 19. *Steve, buried amidst files, in his CMU office*, 2005.

to confidentiality. Judy and I also have a book on surveys and experiments to polish up for publication, as Fred Mosteller would say. I have six chapters that were, I had thought, pretty polished at one stage, but they are still in a drawer in my office. At least I know where the drawer is.

JT: And I know where my copies are…

SF: And so, I've got more books to write too—with good collaborators.

MS: Well, we're almost out of time, but I have one final question. How would you like to be remembered, Steve?

SF: Unfortunately not as a great hockey player. As long as my teammates just let me on the ice, I'm happy to be able to skate around and get off safely.

I guess I'd like to be remembered as somebody who produced really good students and who helped change the image of statistics in the sense that lots of people now work on serious applied problems and help solve them. And that's not just about statistics, that's real interdisciplinary scientific work, and that's the legacy I inherited from Fred and Bill Kruskal and Paul Meier, and all those other great people that I had a chance to work with, like Bill Cochran. I would just like for people to think of me in their kind of company, in some way or another. I suspect that a couple of decades from now, if anybody ever looks at the video we're making or reads this interview, they may not remember log-linear models for contingency tables and other forms of counted data because there will be new methodology, like the mixed membership and related models I now work with. What I know from students today is that, if it wasn't in the journals in the last three years, they're not sure it's worth their attention. So, if I am to have a legacy it needs to be something larger. I have no theorems, well, I do have theorems, but none of them are named *Fienberg's Theorem*. And even if there were a Fienberg's Theorem, it probably wouldn't be important—what's important is the attitude, for what statistics is and how it's recognized by other people outside of our field.

MS: Well, you've changed statistics, and you've made it fun along the way. Thank you very much.