A Conversation with Leo Goodman

Mark P. Becker

Abstract. Leo A. Goodman was born on August 7, 1928 in New York City. He received his A.B. degree, summa cum laude, in 1948 from Syracuse University, majoring in mathematics and sociology. He went on to pursue graduate studies in mathematics, with an emphasis on mathematical statistics, in the Mathematics Department at Princeton University, and in 1950 he was awarded the M.A. and Ph.D. degrees. His statistics professors at Princeton were the late Sam Wilks and John Tukey. Goodman then began his academic career as a statistician, and also as a statistician bridging sociology and statistics, with an appointment in 1950 as assistant professor in the Statistics Department and the Sociology Department at the University of Chicago, where he remained, except for various leaves, until 1987. He was promoted to associate professor in 1953, and to professor in 1955. Goodman was at Cambridge University in 1953–1954 and 1959–1960 as visiting professor at Clare College and in the Statistical Laboratory. And he spent 1960–1961 as a visiting professor of mathematical statistics and sociology at Columbia University. He was also a research associate in the University of Chicago Population Research Center from 1967 to 1987. In 1970 he was appointed the Charles L. Hutchinson Distinguished Service Professor at the University of Chicago, a title that he held until 1987. He spent 1984–1985 at the Center for Advanced Study in the Behavioral Sciences in Stanford. In 1987 he was appointed the Class of 1938 Professor at the University of California, Berkeley, in the Sociology Department and the Statistics Department. Goodman's numerous honors include honorary D.Sc. degrees from the University of Michigan and Syracuse University, and membership in the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society. He has also received numerous awards: From the American Statistical Association, the Samuel S. Wilks Medal; from the Committee of Presidents of Statistical Societies, the R. A. Fisher Lectureship; and from the Institute of Mathematical Statistics, the Henry L. Reitz Lectureship; also, from the American Sociological Association, the Samuel A. Stouffer Methodology Award and the Career of Distinguished Scholarship Award; and from the American Sociological Association Methodology Section, the inaugural Otis Dudley Duncan Lectureship. Earlier he had received a Special Creativity Award from the National Science Foundation, and fellowships from the Guggenheim Foundation, the Fulbright Commission, the Social Science Research Council and the National Science Foundation. In 2005 the American Sociological Association Methodology Section established the Leo A. Goodman Award to recognize contributions to sociological methodology, and/or innovative uses of sociological methodology, made by a scholar who is no more than fifteen years past the Ph.D.

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The following conversation took place on January 10, 2008, at Leo Goodman's home in Berkeley, California.

Becker: Leo, you frequently refer to me as your academic grandson, as you were the thesis advisor of my thesis advisor, the late Clifford Clogg. And, over the years, as good grandfathers do, you have told me many stories about the people whom you have had the pleasure of learning from and working with. Who got you started in thinking about mathematics, about a career as a statistician and also about a career as a statistician bridging sociology and statistics, and how did all this come about?

Goodman: Mark, yes, I do think of you with pride as a second generation academic descendant of mine, and I would also like to say here that I could think of myself as, in a certain sense, a first generation academic descendant of my first real mathematics teacher, Lipman (Lipa) Bers. He had been a student of Charles Loewner at the Charles University in Prague, Czechoslovakia, and, during my undergraduate years at Syracuse University, both Bers and Loewner were faculty members in the Math Department there, having extricated themselves from Europe just one step ahead of the Holocaust. I took some courses with Bers and with Loewner. (By the way, Bers once told me, many years after I had been one of his students, that he and Loewner were direct academic descendants of Gauss; Bers was a sixth

generation and Loewner a fifth generation descendant of Gauss. Also, it turns out that Bers was a fourth generation and Loewner a third generation descendant of Weierstrass.) Both Bers and Loewner were outstanding teachers, and each of them produced mathematics of top quality.

I should stop here right now to say that I am partly joking when I say that I could think of myself as, in a certain sense, a first generation academic descendant of Bers. [Smile/Laughter] Bers was an important person in my life, but he was not the advisor on my Ph.D. thesis; and, as you know, being the Ph.D. thesis advisor is, strictly speaking, the genealogical criterion that defines this kind of kinship. As you of course also know, Sam Wilks and John Tukey were the ones who approved my Ph.D. thesis.

But now let me return for a moment to Bers and Loewner. Here is a brief description of Loewner written by Bers, in words that could be used to describe Bers as well:

"He ... was a man whom everybody liked, perhaps because he was a man at peace with himself. He conducted a life-long passionate love affair with mathematics.... His kindness and generosity in scientific matters, to students and colleagues, were proverbial. He was also a good storyteller, with a sense of

FIG. 1.   *Leo Goodman and Mark Becker.*

FIG. 2.   *Lipman Bers.*

FIG. 3. *Charles Loewner.*

humor.... But first and foremost he was a mathematician."

Mark, you asked me who got me started in thinking about math. I would say that Bers and Loewner were responsible for that. But, before them, I would say that I got started in math because I actually got started as an undergraduate major in sociology. [Smile/Laughter] During my undergraduate days at Syracuse, sociology majors were required to take the course in statistics given by the Sociology Department, and the sociology faculty member who was assigned to teach the course was Robert E. L. (Bob) Faris. He taught this course for the first time the year that I took it. When he entered the classroom the first day of class, he announced that he was assigned by the Sociology Department to teach this course because he had written a book a long time ago that had some tables in it, and also because he did know a little bit about statistics, but he confessed that he didn't really know very much about statistics, and he hoped that students in the class would be able to help him to get through the course. Well, it happened to turn out that I was able to help out. When the course came to the end, Faris told me that he thought that I had a talent for statistics, and, if I would like to gain still more strength in that subject, he would suggest that I first strengthen my mathematics background by taking some courses in the Mathematics Department. And so, that is what I then did.

Later on, when I was in my undergraduate senior year, it turned out that I had taken just enough courses in math and just enough courses in soc to graduate as a joint math/soc major. But what would I do when my senior year would come to an end? I didn't know.

Bers then suggested to me that I should apply for graduate study in mathematics at Princeton University, and Faris suggested that I should apply for graduate study in sociology at the University of Chicago. Bers, when he made his suggestion, also told me that no mathematics undergraduate from Syracuse University had ever been accepted for graduate study by the Mathematics Department at Princeton; and, with respect to Faris' suggestion, I was aware of the fact that he had been a graduate student in sociology at the University of Chicago, and his Ph.D. thesis, when it was published, turned out to be a kind of sociological classic. So I applied for graduate study in math to Princeton, and for graduate study in soc to the University of Chicago.

Now let me tell you a bit about Faris. He was a strong social psychologist, and he was also very much a genuine sociologist. In both spheres, his many contributions were informed by his strong commitment to sociology as a discipline. In addition, he was an accomplished painter, a pretty good violinist and an enjoyable pianist. We became good friends.

Faris was a member of a four-generation line of sociologists. His father, Ellsworth Faris, had served for fourteen years as the first chairman, after the founding chairman, of the University of Chicago Sociology Department, and he ranks high in the final hierarchy of those who achieved major results in the building of American sociology during the first half of the twentieth century. The father served as President of the American Sociological Association in 1937, and the son in 1961; the father served as Editor of the *American Journal of Sociology*, and the son as Editor of the *American Sociological Review*. Bob Faris' son, Jack, received his Ph.D. in sociology from the University of Chicago; and Jack's son, Robert W., received his Ph.D. in sociology from the University of North Carolina at Chapel Hill. Jack is now the President of the Washington Biotechnology & Biomedical Association (earlier he had been the University of Washington Vice President for University Relations); and Robert W. has just now completed his first year as an Assistant Professor in the Sociology Department at University of California at Davis.

Becker: Well, Leo, Faris gave you rock solid advice for launching a career in sociology, but you ultimately followed Lipman Bers' suggestion and went on to Princeton to study mathematics. How did you decide

to do that, and how did you decide to emphasize mathematical statistics while you were a graduate student in mathematics?

Goodman: Well, after I had mailed out the application to Princeton in math and the application to the University of Chicago in soc, I did wonder what would happen next.

Sometime during my senior year, when I was visiting my parents in New York, I decided to take the train from New York to Princeton Junction, just in order to see what the Princeton campus looked like. After the train arrived in Princeton Junction, I walked from there to the campus, and then walked around the campus, and was impressed by how beautiful it was. Then I just happened to come across Fine Hall, the Mathematics Department building, and it too was beautiful. It was located in the southeastern corner section of the campus, and it harmonized with the other structures in this, the "red brick section" of the campus. Red brick and limestone were used in the Collegiate Gothic architectural style of these structures, and they presented a unified appearance. I was also aware of the fact that, when the Institute for Advanced Study was first founded in Princeton, it had temporary quarters in Fine Hall. All this left its impression on me.

Fine Hall was in the shape of a box (a rectangular parallelepiped). [Smile] The hallway inside the building was rectangular in shape, and it went around the inside of the building, with rooms on each side of the hallway. I walked around the hallway, and then I walked around it a second time, and maybe a third time. (Einstein, I thought, must have worked at an earlier time in one of the offices located right off this hallway.) Then, a secretary, whose office door had been open, noticed that there was this strange young man (me) walking around the hallway, and she came out of her office and asked me if she could be of any help. I told her that I was an undergraduate senior at Syracuse University, and had applied to Princeton for graduate study in math. She then asked me to wait there for a moment, and she went into another office, which was adjacent to her office. Then she came out of the other office with a man who said he was Sam Wilks.

He invited me into his office, which was also beautiful. It was spacious, with carved oak paneling and a splendid fireplace. The office contained a large conference table with chairs all around it, and also a large desk, and wooden bookshelves filled with books from floor to ceiling. Wilks asked me to sit down, and we then talked for maybe an hour or more. He had a very pleasant Texan drawl. After about an hour or more,

FIG. 4.   *Sam Wilks.*

I began to think that I must be taking up too much of his time, and I got up to leave; and he asked me to wait a minute, he wanted to first make a phone call. He then phoned a fellow faculty member, Fred Stephan, who was a distinguished sociologist, statistician and demographer in the Sociology Department there, and he told Stephan over the phone that an undergraduate senior at Syracuse University who had applied to become a graduate student in the Mathematics Department at Princeton was now in his office, and he thought that Stephan would be interested to meet this student. He then gave me directions on how to get to Stephan's office, and off I went. I found Stephan's office, and he and I also had a very nice conversation.

After all this, I felt elated, and I thought that, if it turns out that I am accepted by the Math Department here at Princeton, here is where I will go. I then started walking back to Princeton Junction, walking on cloud nine, and I got on the next train in order to return to New York. But I soon realized, after the train had pulled out of the train-station and was on its way, that it was heading in the wrong direction, heading for Philadelphia. [Smile/Laughter]

So that's how I made the decision to be a graduate student in math if it turns out that I am accepted at Princeton, and not a graduate student in soc at the University of Chicago.

By the way, Mark, I have been telling you here about my feelings and thoughts while on the trip that I made from New York to Princeton just in order to see what the campus looked like. Please keep in mind that I was nineteen years old at that time (and it is now about sixty

years after the events that I have been describing), and I am trying to convey to you, as honestly as I can, what that nineteen-year-old felt and thought at that time.

Yes, I made the trip to Princeton just in order to see what the Princeton campus looked like, and it just happened that I came across Fine Hall, and when I was walking around and around in awe in the Fine Hall hallway, the secretary whose office door just happened to be open just happened to come out of her office and just happened to ask me if she could be of any help, and she just happened to be Wilks' secretary. Mark, it seems to me that all these happenings might be viewed as examples of what one might call dumb luck. I have always thought of myself as a very lucky person, and I feel deeply grateful for all the lucky events that have occurred to me in my life. Just imagine what might have happened if, when I was walking around and around in the Fine Hall hallway, the secretary whose office door was open was the secretary of, say, Solomon Lefschetz, who was the math department chairman at that time. (I will say more about Lefschetz later.) If that had happened, I suppose it is possible that I might have ended up deciding to be a graduate student in sociology at the University of Chicago. [Smile]

Becker: Leo, let's talk about your student experiences at Princeton. Wilks and Tukey were your major professors at Princeton. What are your remembrances of these two luminaries of statistics, and their respective contributions to your development as a scholar?

Goodman: Let me begin by telling you about two very nice experiences that I had with Tukey during my first year as a graduate student: As I said earlier, when I graduated from Syracuse, it turned out that I had taken just the smallest number of courses in math and the smallest number of courses in soc to graduate with a joint major, and I had the impression, after being a graduate student at Princeton for just a short time, that all of the other first year math graduate students had been studying mathematics intensively full time when they were undergraduates, and also possibly when they were in high school, and maybe even when they were in elementary school. [My cohort of graduate students in the math department included the future Nobel Laureate John Nash (the "Beautiful Mind") and many other brilliant students.] One day, sometime after I had been at Princeton for maybe two or three months, I happened to be walking in the Fine Hall hallway, and Tukey happened to be walking in the hallway too, and our paths happened to cross. He stopped me and asked, "How are you doing?" I then said, "I don't know how I'm doing." He then said, "Follow me." He opened the door of an empty classroom, and he asked me to go up to the blackboard at the front of the room, and he took a seat near the back of the room. He then asked me a math question to explain something or other, and I tried to answer the question writing on the blackboard. I could see that, while I was trying to answer his question at the blackboard, he was doing something else seated there near the back of the room, possibly writing an article. (It was well known that Tukey was able to do two things at the same time.) When I finished trying to answer the first question, he didn't comment on my attempted answer, and he asked me a second math question. I then tried to answer that question writing on the blackboard. Then a third math question; and the questioning and attempted answering continued for maybe an hour or more. Finally the questioning stopped. Tukey then got up from his seat near the back of the room, he didn't say anything, and he walked very slowly toward the front of the room. The expression on his face was that of a man thinking about serious matters. He had his hand on his chin, which was a typical place where he put his hand when he was thinking about serious matters. And this is what he finally said to me, speaking very slowly: "Well, [long pause] what I think you really need, [extra long pause] is some folk dancing."

Mark, this comment by Tukey is a good example of his special kind of sense of humor and his at times elliptical manner of speech. He was telling me, in his own way, that I was doing fine, that my ability to answer math questions was fine, and that I ought to take

FIG. 5. *John Tukey.*

time out for folk dancing or for whatever else might please me. He then told me when and where the folk dancing would take place, and he invited me to come to it, which I then did. (Mark, by the way, when Tukey invited me to come to the folk dancing, he didn't tell me that the folk-dancing group met under his direction, that he was the folk-dance leader, and that he and some of his friends demonstrated folk-dancing steps for beginners, which is what I was.) Later that year, Tukey met his future wife, Elizabeth Rapp, at a folk dancing session.

Here now is the second very nice experience that I had with Tukey during my first year: Attached to the wall in the hallway just outside the Math Department office in Fine Hall were the mailboxes for the math faculty members. One day, sometime after I had been at Princeton for maybe four or five months, I happened to be walking past the math office, and Tukey happened to be standing there reading a postcard that he had just taken out of his mailbox. Written on the postcard was a statistical problem sent to Tukey by someone named Allen Wallis. (I will say more about Wallis later.) Tukey handed me the postcard, and then said something that I didn't understand about the statistical problem written on the postcard. He said just one sentence about the problem. Without him explicitly saying so, I realized that he wanted me to work on the problem that was described on the postcard. (Tukey was a New Englander, and he spoke with a sort of "down-east" accent. He also spoke in an elliptical, enigmatical, oracular fashion, and he liked to coin his own words.)

I then went off with the postcard in order to begin to think about the statistical problem and about what Tukey had meant in his one sentence about the problem. After thinking about this for a while, I found that I still just couldn't figure out what he had meant, and I finally just gave up trying to do so. But I didn't give up working on the problem. I worked on this for maybe three or four weeks, and then I wrote up the results that I had obtained in the form of a paper, but without putting an author's name on it. And I put the paper in Tukey's mailbox. A day or so later, Tukey asked me to come by his office. When I came to his office, he told me that this work was very good, and that it should be published as is in the *Annals of Mathematical Statistics* right away. He started to write my name on the paper as the author, but I said no, that he and I were the authors—he gave me the problem and he gave me his help. He said no, that I was the author, and this was my work.

All this happened sometime near the end of 1948 or the beginning of 1949, and my paper was published in the *Annals of Mathematical Statistics* in December 1949.

Now, let me tell you about Wilks. As I said earlier, it was after I first met Wilks, and had a chance to talk with him in his office, that I thought that, if it turns out that I am accepted as a graduate student by the Math Department at Princeton, that is where I will go. I will give up the notion of becoming a graduate student in soc at the University of Chicago.

Mark, as you of course know, Wilks was the father of mathematical statistics at Princeton and a major leader in the development of this discipline; and many of the people who had taken their Ph.D.'s under Wilks (for example, Fred Mosteller, Ted Anderson, Don Fraser, Ted Harris, Will Dixon, Alex Mood) also had important roles in this development. Wilks was very friendly and very fair. Everyone liked him. He was a quiet, penetrating and influential leader in the work of many organizations, especially in mathematics, statistics and social science. To these organizations, he brought wisdom, commitment and persistence. He had a remarkable sense of what was important and what was not. One of the many important contributions he made to statistics was his work, for a period of about thirteen years, as the first editor of the *Annals of Mathematical Statistics*, when it became a publication of the Institute of Mathematical Statistics (IMS). (He was also one of a small group of statisticians who founded and organized the IMS, and, from its inception, he was a leading member of this organization.) During the period of Wilks' editorship of the *Annals*, he turned it into the foremost, internationally recognized, journal of mathematical statistics; and this had an important influence on the subsequent development of the field of statistics.

I mentioned here Wilks' editorship of the *Annals* in part because, during my first year and a half at Princeton, Wilks would from time to time give me a research manuscript, which had been submitted to the *Annals*, for me to referee; and I think that this experience of refereeing manuscripts really contributed to my education and training as a statistician.

Sometime during the first half of my second year at Princeton, Wilks asked me to select any, more or less, contemporary statistics article, by any statistician, and then study the article and give a talk on it, explaining the results that were in the article. I don't recall how I went about trying to select an author and an article, but it turned out that I selected a recently published article by Charles Stein. (Stein had received his Ph.D. at

Columbia under Abraham Wald about two years earlier, and his articles, which he had written during the period starting from two years before he received his Ph.D. until the time when I was trying to select an article, were all very exceptional.) I studied the selected article and prepared the talk that I would present, but there was a small part of one section in the article that I just couldn't understand, and I decided to leave that section out of my talk. On the day that my talk was scheduled to take place, I entered the lecture room at the appropriate time, the members of the audience were in their seats, and just as I was about to begin my presentation, in walked Wilks and Tukey escorting a third person I didn't know. The three of them sat down in the last row of the lecture room. (The third person turned out to be Willy Feller, a distinguished mathematician specializing in probability theory, who was at that time a professor at Cornell University, and was visiting the Princeton Math Department for a few days. Wilks, Tukey and the Math Department were trying to persuade Feller to leave Cornell and become a professor in the Princeton Math Department.) I presented my talk, and, immediately after it was over, Feller came rushing right up to me. He introduced himself, and, with a really big smile on his face, he told me that he really enjoyed the talk and found it to be very interesting. Well, Mark, I was of course pleased that Feller liked the talk, and I also was pleased when I found out a week or so later that, after his short visit to Princeton, Feller did decide to join the Princeton Math Department.

By the way, many years later, in a conversation that I had with Charles Stein, I told Charles that I just couldn't understand a small part of one section in the article of his that I had studied when I was a graduate student, and he then told me that there was a mistake that he had made in that section.

Now, back to Wilks. What I am about to tell you happened, I think, a few years after I had received my Ph.D. degree, but it is possible that it happened sometime before I received the degree: I received a letter from the University of Texas informing me that Wilks was being considered for an appointment as the President of the University, and asking me to send them a letter of reference about Wilks. Well, I wrote the most affirmative thumbs-up letter that I have ever written about anyone. And I remember that it gave me great pleasure to be able to do this. It then turned out that Wilks was invited to become the President of the University, he did consider the offer, but in the end he turned it down.

Mark, earlier I had mentioned to you that Wilks had a pleasant Texan drawl. But there is more to say about Wilks, who was very much a Texan, turning down the Presidency of the University of Texas: Wilks was born in a small town bordering on a nice lake in North East Texas, and he was raised with his two younger brothers on a 250 acre ranch, which his father farmed, outside of the small town. Wilks and his brothers and his parents had very close family ties, and, after he was settled in Princeton, he would take advantage of whatever opportunities would turn up for him to revisit Texas and visit with his family. Also, Wilks' son, Stanley, would go to Texas whenever possible (for example, during his summer vacations from school) in order to visit with his two uncles and their families, and with his grandparents; and, when Stanley was a senior in high school in Princeton and was considering where to go to college, he decided to go to Texas as an undergraduate at North Texas State college, the same undergraduate college that his father had attended, and not too far away from where his two uncles and their families, and his grandparents, lived. So you can imagine how surprised and disappointed Wilks' family must have been when he told them that he had decided to turn down the job offer and remain in Princeton.

Here now is a story about how Wilks' family viewed all this. I can't remember who told me this story, and I can't vouch for its veracity. I am telling this to you because I think it is amusing: Wilks' two brothers had good jobs in Texas, and his parents thought that, with three grown-up sons, it isn't so unusual that one of the sons might just not be as able as the other two, and just might not be able enough to obtain a good job in Texas. When the parents first heard that Wilks had been offered this job at the University of Texas, they were very pleased to hear the news, and they could then see that this son too was able enough to obtain a good job in Texas. We are then left to speculate about what Wilks' parents thought about his decision to turn down this offer of a good job in Texas.

Becker: Princeton has long had one of the world's finest mathematics departments. You were fortunate to have studied there with statistical greats like John Tukey and Sam Wilks, but what about your interactions with mathematics professors who were not statistically inclined? Do you have any particular remembrances of those?

Goodman: Math graduate students at Princeton at the time when I was there were not required to attend any courses. All you had to do was pass an oral exam, called the general exam, covering four subfields

of math, usually taken when your first year as a graduate student was completed, or sometime after that. You then had to submit a thesis, and have the thesis approved. Also, there was a foreign language requirement, two foreign languages of your choice, and, for each of these languages, you had to demonstrate to a math faculty member of your choice that you had a reasonable ability to read ordinary mathematical texts that were written in the foreign language. (There seemed to be a general understanding among the math graduate students that the math faculty didn't take the language requirement very seriously.) As a math graduate student at Princeton, you had the feeling of having almost complete freedom.

Although it wasn't required, I actually did take some courses during my first year as a graduate student: I remember taking a course in analysis given by Salomon Bochner, and a part of a course in mathematical logic given by Alonzo Church, and I think that I also may have taken a course in point-set topology given by Ralph Fox, but I am not sure about that. I remember that I was very impressed with both Bochner and Church, by their great skills as teachers, by the depth of their understanding of their subjects, and also by their unforgettable personalities. (If I did attend Fox's course, I just don't remember what it was like. I will tell you a little bit about Fox a little later.)

Bochner was one of the foremost twentieth-century mathematicians whose research profoundly influenced the development of many different areas of analysis. He was born into a Jewish family in what was then a part of Austria–Hungary, and is now a part of Poland. Fearful of a Russian invasion at the beginning of World War I, his family moved to Germany, seeking greater security. Bochner was educated at the University of Berlin, where he wrote his dissertation, and he then lectured and made very important contributions on a surprising variety of topics in analysis in Germany, at the University of Munich, for about ten years. His academic career in Germany came to an abrupt end a few months after the Nazis came to power, when laws were established providing for the removal of Jewish teachers (and those of Jewish descent—those having at least one grandparent of Jewish descent) from the universities, and he then received a timely offer of a position at Princeton, which he accepted. He was a very important faculty member at Princeton for the next 36 years.

Mark, now you might find it interesting to compare what I have just now told you about Bochner with what I will now tell you about Alonzo Church and his pedigree: Church was a mathematician who was an early
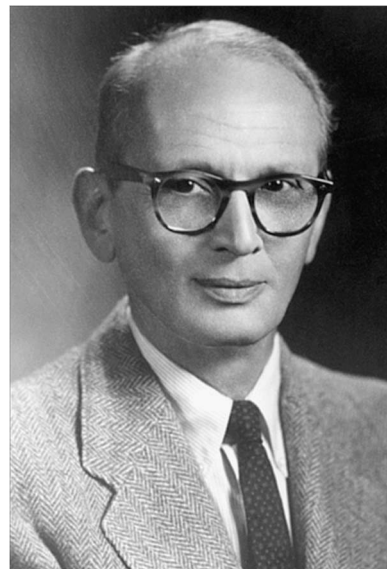

FIG. 6.   *Salomon Bochner.*

pioneer in and major contributor to the field of mathematical logic, and he was also responsible for some of the foundations of theoretical computer science. His great-grandfather, Alonzo S. Church, was a professor of mathematics and astronomy, and then became the president of the University of Georgia for a period of thirty years. Alonzo Church's grandfather, Alonzo W. Church, was at one time Librarian of the U.S. Senate. His father, Samuel R. Church, was a Justice of the Municipal Court of the District of Columbia, but he resigned from that post because of failing vision and hearing. The family then moved to rural Virginia, where Alonzo Church and his younger brother grew up. He was an undergraduate at Princeton, graduating with an A.B. in mathematics, and then he continued as a graduate student, completing his Ph.D. there. Following a two-year post-doctoral National Research Fellowship, which he spent at Harvard University the first year, and the University of Gottingen and the University of Amsterdam the second year, he was invited to return to the Princeton Math Department, to begin his academic career there. He was a very important faculty member at Princeton for the next 39 years.

Here now is a nice story that I would like to tell you about Bochner: Sometime during my first year as a graduate student, during the period of time when I was taking his course, he once told me that, if he had his life to live all over again, he definitely would not choose to be a professor. So I asked him what would he choose to be instead. He then told me that he would choose to be a laundry-truck driver. I then asked him why would he make that choice. "Well," he said, "when you're
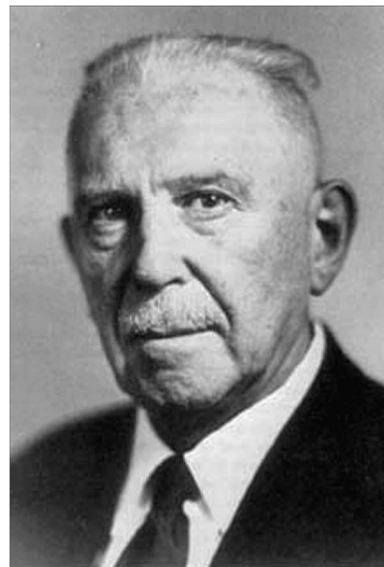
FIG. 7. *Alonzo Church.*

FIG. 8. *Solomon Lefschetz.*

a laundry-truck driver, you drive the truck to the first house on the delivery schedule, then you deliver packages of clean diapers to the housewife, and you pick up packages of dirty diapers from her, then you drive the truck to the second house on the delivery schedule, and you repeat the same procedure there, then you drive to the third house . . . and you continue to do this all day long, every working day. *And*, while you are doing this during all that time, [extra long pause] *you can also simultaneously spend all that time proving interesting theorems!!!"* [Smile/Laughter] Bochner was a civilized and erudite mathematician, also a lover of music and art.

Mark, earlier in our conversation, I told you that there was a language requirement, two foreign languages of your choice, and, for each of these languages, you could ask a math faculty member of your choice to test you on that language. I selected French and German, and Bochner passed me in French, and Solomon Lefschetz passed me in German. I have just now been telling you here about Bochner, and I would like now to tell you about Lefschetz. (Mark, before we move on, I should say here right now that, with respect to the language requirement, if the pass/fail decision had been in the hands of the corresponding Princeton language departments, instead of the math department faculty, I would have had a much harder time trying to pass the language requirement.)

Now about Lefschetz: When he was 23 years old, he was working as an electrical engineer in the U.S., and, in a terrible industrial accident—a transformer explosion—he lost both his hands and a part of each

forearm. For the rest of his life, he used a pair of artificial hands (wooden hands, gloved)—his prosthetic hands—that fit over the remaining parts of the forearms. Three years after the accident, he enrolled as a doctoral student in mathematics at Clark University in Massachusetts, and he received his Ph.D. a year later with a thesis on algebraic geometry. In the next thirteen years, he produced many research articles of striking originality and importance in algebraic geometry and algebraic topology. He then was invited to be a visiting professor at Princeton, and, at the end of his first year there, he was offered a permanent post, which he accepted.

At Princeton, he profoundly affected the development of mathematics in the U.S. as the editor of the *Annals of Mathematics* during a thirty-year period. During his editorship, the journal became one of the principal journals of research mathematics in the world. Also, during Lefschetz's chairmanship of the department, eight new faculty members were appointed, all of first rank. A mathematics faculty of first rank became an even larger faculty of first rank, one of the world's great centers of mathematical research and teaching.

Lefschetz was born in Moscow into a Jewish family (his parents were Turkish citizens), and soon after his birth they moved to Paris. He was educated there in engineering, and he emigrated to the U.S. when he was 21. Two years later, there was that terrible industrial accident. When he first came to Princeton in the 1920's, he was one of the first Jewish faculty members on the Princeton campus, and it is reported that he felt that people avoided him in the hallways and on

campus on that account. He said that he was an "invisible man" there at the time. It is also the case that he could be rude, imperious, idiosyncratic and obstreperous, with a commanding (bossy) personality. He also was a man who had a really great amount of energy, a supercharged human locomotive.

Lefschetz was sometimes accused of caving in to anti-Semitism at Princeton for refusing to admit many Jewish math students—his rationale being that nobody would hire them when they completed their degrees. (It is my impression that Lipman Bers was implicitly alluding, in part, to this kind of problem at Princeton when he told me that no mathematics undergraduate students at Syracuse University had ever been accepted for graduate study by the Mathematics Department at Princeton and he suggested to me that I should apply for graduate study there.) Times have really changed at Princeton since the bad old days.

Becker: Leo, many academics have rich or humorous stories relating to the exams they experienced en route to the Ph.D. When did you take your oral general examination, and what was that like?

Goodman: I took my oral general exam soon after the very beginning of my second year as a graduate student. This exam covers two required subjects, algebra and real and complex variables, and two special advanced topics of the examinees choice; mathematical statistics and point-set topology were what I chose.

The examinee is not told beforehand who his examiners will be. He finds out when he enters the examination room and sees the examiners sitting there. When I opened the door to the room, sitting there were Salomon Bochner, Emil Artin, Ralph Fox and Sam Wilks.

I haven't yet told you anything about Artin and Fox. First, about Artin:

He was one of the leading algebraists of the twentieth century. He was brought up in a town that was mainly German speaking, in what was then a part of the Austrian Empire, and is now in the northern part of the Czech Republic. He received his doctorate in mathematics in Germany, from the University of Leipzig, and he began his academic career at the University of Hamburg. He lectured and made many very important contributions to a wide range of topics in algebra there over a period of eleven years before Hitler came to power. His position at the university was not affected by the laws established a few months after Hitler came to power providing for the removal of Jewish teachers from the universities, since he wasn't Jewish. However, since Artin's wife was Jewish, his position at the university was affected four years later by a new law that
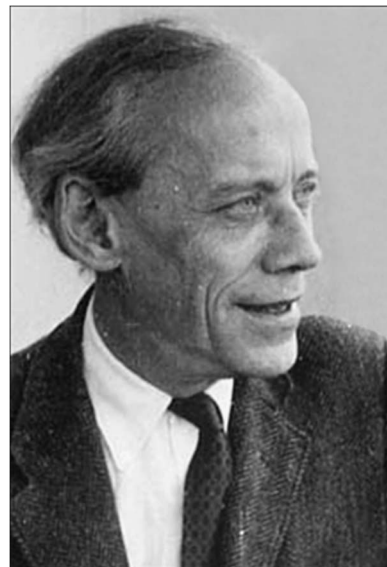

FIG. 9. *Emil Artin.*

provided for the removal from the universities of those teachers related to Jews by marriage. He then came to the United States, taught at Notre Dame for one year, at Indiana University for eight years, and then at Princeton.

Now about Fox: He was an American mathematician who devoted most of his career to the field of topology, and, in particular, to knot theory. He taught and advised many of the later contributors to topology, and he played an important role in the modernization and development of knot theory. After receiving his Ph.D. from Princeton, he spent the following year at the Institute for Advanced Study in Princeton, and he then taught at the University of Illinois and Syracuse University before returning to Princeton, six years after having received his Ph.D. there, to join the math faculty.

One of Fox's strong interests was the ancient Japanese board game of Go. He represented the United States in the first international Go tournament, held in Tokyo, and he popularized the playing of Go at the Princeton Math Department. Go was one of two favorite board games that were played in the Fine Hall common room just about every day by some of the math graduate students at tea time and at other times too.

Sometime before the time when I took the general exam, I was sitting around in the Fine Hall common room, chatting with another graduate student who was then in his third year at Princeton and who had taken his general exam the year before, and he asked me how was I preparing for the part of the exam on complex

variables. So I told him what I had been studying in complex variables, and he asked me what did I know about complex manifolds. I said that I didn't know anything about complex manifolds. He then offered to tell me about it. So the two of us went into one of the empty classrooms, I sat down, and he went to the blackboard and started to tell me about complex manifolds writing on the blackboard. After about an hour or so of his exposition, I thought that that was enough, and we stopped.

Well, sometime later, on the day when I went into that exam room, and was facing the four examiners, the first one to ask me a question was Bochner. He said: "What do you know about complex manifolds?" I then said, "Not very much." And he said, "Tell me what you do know about complex manifolds." So I went up to the blackboard and proceeded to write on the blackboard (facing the blackboard), trying to repeat what I had been told earlier about complex manifolds by the more advanced graduate student. After a while, as I was proceeding with the exposition, I suddenly heard someone (one of the examiners), with an authoritative-sounding voice, say, "*That is incorrect!*" I then turned from facing the blackboard to facing the examiners, and I could tell that the examiner with the authoritative voice was Artin. Then Bochner said, "No, that is correct." And the two of them, Artin and Bochner, then started to argue with each other as to whether what I had said was incorrect or correct. Meanwhile, I backed up, and leaned against the blackboard until the argument came to an end. After that, Artin, Fox and Wilks, each of them in turn, asked me his own questions, and I did my best to try to answer them correctly.

Then I was asked to leave the exam room and wait outside the room. After a little while, Wilks came out with a nice smile on his face, and he congratulated me, and said that in his experience he'd never seen an examinee answer questions so calmly, and that I did a nice job. Well, Mark, I was of course very pleased to hear this, especially since, when I left the exam room, I wasn't sure that I had passed the exam! [Smile]

Thinking some more about Wilks' comment about examinees answering questions calmly, I am reminded of something amusing that Wilks' wife, Gena, had told me about a former Wilks student and his general exam. This conversation with Mrs. Wilks took place sometime near the end of my first year as a graduate student at a time when I was supposed to be studying in preparation for my general exam. My studying was interrupted by a sudden appendicitis, with surgical removal of the inflamed appendix carried out just in the nick of time, and with a required stay in the hospital for a few days to recover from it all. Mrs. Wilks came by to visit with me there, and she asked me how I was feeling. I told her that I was recovering just fine, but I wasn't able to focus on my studies for the general exam. She then told me this story: There was this well-known, very good statistician, a former very good Wilks student (whom I will call student X), who, a few days before he was scheduled to take the general exam, came into Wilks' office, and said the following to Wilks, "Sam, I don't know a thing! I'm not going to take the exam." So Wilks told student X that the exam would be put off for two months. Then, two months later, a few days before student X was supposed to take the rescheduled exam, he again came into Wilks' office, and this time he said the following: "Sam I still don't know a thing! I'm not going to take the exam." So Wilks told student X that the exam would be put off this time for one month. Then, one month later, a few days before student X was supposed to take this rescheduled exam, he again came into Wilks' office and again said the same thing that he had said twice before. Wilks then told student X that he should take it easy, go out to a movie or do something else that would be relaxing and fun, and that he should drop by Wilks' office the next day at 10 AM. Student X then came by Wilks' office the next day at the appointed time, and he found the four-member examination committee ready to proceed with the exam. Student X passed the exam, and then went on to become a well-known, very good statistician.

Becker: Once you had your examinations behind you, how did you obtain a dissertation topic, and how did you proceed to write your Ph.D. thesis?

Goodman: After my general exam was over, I started to try to think about possible thesis topics. One day, while I was thinking about statistical problems that had been discussed in manuscripts that I had refereed earlier for Wilks, an idea just happened to come to me by free association, and with this idea there came a statistical problem that I thought I would try to work on. I then worked on this problem for a few weeks, but I didn't seem to be getting anywhere with the problem. Then, it crossed my mind that, during the time period when I slept each night, I may have been dreaming about the problem that I had been working on during the daytime; but, if I had been dreaming, when I awoke each morning the dream was gone. So I put a pad of paper and a pencil on the night-table next to my bed, and that night, I awoke in the middle of the night and proceeded to write down on the pad of paper what I had

been dreaming—a very long dream. I then fell back to sleep, and I slept for a long time. When I awoke, I noticed the pad of paper lying next to me on the bed with a lot of writing on it, and I wondered what the writing was about.

After looking over the dream document, I could make out that it was about the problem that I had been working on during the daytime. I could make out, in some of the paragraphs, what were the ideas or results contained in them; and in some of the paragraphs, I couldn't. Well, I then worked on the dream document for a few months, developing and extending it; and, when I was finished doing that, I wrote it all up in the form of a Ph.D. thesis, and I put a copy in Wilks' math department mailbox, and a copy in Tukey's. After a week or so, Wilks told me that it was nice work and that he approved it, and Tukey told me that, first, I should include in the thesis a numerical example that illustrates the statistical method introduced and developed in the thesis; and, second, I should give a talk on the thesis in August at the annual Summer Seminar in Statistics, which would take place in Connecticut (it turned out that Tukey was one of the organizers and leaders of the Summer Seminar); and, third, he approved the thesis.

Sometimes, Mark, when someone happens to ask me about the writing of my Ph.D. thesis, I like to say simply that the thesis was given to me in a dream. [Smile] It turns out that there is some truth in my saying that; and, to the person asking me about the writing of the thesis, I am willing to clarify what I mean, if clarification is what is called for. [Smile]

Becker: You opted to take your first faculty position at the University of Chicago, and yet at that time there was not a formal statistics department at Chicago. What or who motivated you to accept the offer at Chicago?

Goodman: During the Winter/Spring of 1950, while I was completing my work on the Ph.D. thesis, I began to think about what I might do when the thesis was done. A short time before that in 1949, Allen Wallis (about whom I will say more later), who was at the University of Chicago at that time, had persuaded the Chancellor of the university to permit him to establish a "Committee on Statistics," which was to be essentially a nascent department, but the Chancellor at that time was unwilling to approve a "Department of Statistics." When I visited the University of Chicago to look it over, I was intrigued by the idea of being a part of a small group of statisticians that could grow into a pride of statisticians, and I was also aware of the fact that

the Sociology Department at the university had been for many years, and still was, a very distinguished department. I also had the impression that the University of Chicago was a good place to be an assistant professor, and I liked the spirit of the place. So that is why I opted to go there, rather than to one of the other good universities where I might have gone.

Becker: Your research collaborations at the University of Chicago with Bill Kruskal on measures of association for cross-classifications spawned a citation classic. How is it that you and Kruskal came to work on measures of association for a cross-classification of counts?

Goodman: Bill Kruskal and I arrived at the University of Chicago at about the same time, in time for the beginning of the 1950–1951 academic year. We became colleagues and good friends, and we worked together very harmoniously and productively as colleagues, and also as co-authors, over a very long period of time, even after we completed our series of four joint articles and after Bill became the Dean of the Social Science Division at the university, and even after I left the university in 1987 to begin working at the University of California, Berkeley.

Bill and I started to work together in the early 1950s on the introduction and development of various measures of association for the analysis of cross-classified categorical data, and we published our first joint article on this subject in 1954, followed by a series of three other joint articles on the subject in 1959, 1963 and 1972; and the four articles were brought together
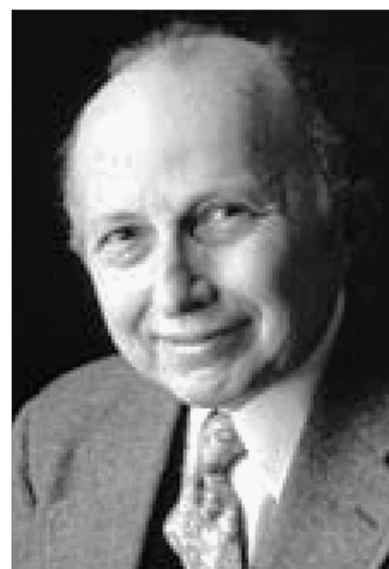

FIG. 10. *Bill Kruskal.*

in a single volume in 1979. Bill and I worked on the first article—the core article—on and off for about two years before we submitted it for publication, and the series of four articles evolved over a 20-year period. The 1979 volume appeared in print 25 years after the publication of the first article.

The joint work that Bill and I did grew out of a conversation that we had at a New Year's Eve party that each of us happened to attend at The Quadrangle (Faculty) Club at the university. Our conversation at the party was about our earlier experiences serving as statistical consultants after we arrived at the university. As beginning faculty members, Bill had been asked to serve as a statistical consultant to Bernard Berelson in the Graduate Library School, and I had been asked to serve as a statistical consultant to Louis Thurstone in the Psychology Department.

Berelson was the Dean of the Graduate Library School at that time and later became the President of the Population Council. He also was an important figure in the social and behavioral sciences at that time, and later he became an even more important figure. Thurstone was a distinguished professor in the Psychology Department where he was the founder and director of the Psychometric Laboratory. He had been instrumental in the development of the field of psychometrics, and was at that time the major figure in the development of factor analysis.

Well, the conversation that Bill and I had at that party took place after Bill had met with Berelson and after I had met with Thurstone and some other members of his Psychometric Laboratory. Bill and I were describing to each other what happened when he met with Berelson and I met with Thurstone and his group. And we observed in this conversation that the kinds of statistical problems that Berelson was concerned with and the kinds of problems that Thurstone and his group were concerned with could be viewed as problems concerning the measurement of association for cross classifications. We discovered that each of us had been independently thinking about similar kinds of questions. So, right then and there, at that party, Bill and I joined forces, and we were off and running.

As I said earlier, Bill's and my first joint article— the core article—was published in 1954. In 1979, the Institute for Scientific Information (ISI) informed us that this article had been selected as a Citation Classic, and we were invited to write a commentary on that article, which the ISI published in *Current Contents, Social and Behavioral Sciences*. It turns out that, according to the ISI, our 1954 joint article has been cited

about 1125 times so far. This article still continues each year to be cited in a wide range of different articles in journals that cover a very wide range of different fields of study. In each of the past three years, the number of citations of this article was greater than the corresponding number in each of the twenty years prior to those three years.

Mark, let's go back for a moment to when Bill and I submitted the manuscript for our 1954 joint article for possible publication in the *Journal of the American Statistical (JASA)*: Each of the referees of the manuscript said that the manuscript should be shortened, and the main referee said that it should be shortened by 50%! If we had followed the referees' instructions, our joint article definitely would not have reached the large and wide audience that it has actually reached and continues to reach. We decided not to follow the referees' instructions. Instead, Bill wrote a very long, detailed letter to the editor explaining why the manuscript should not be shortened at all—why it should be published as is. The editor, after reading Bill's letter, accepted the manuscript for publication as is. (By the way Mark, the editor of *JASA* at that time was Allen Wallis, who was also at that time the first chairman of our nascent Department of Statistics.) [Smile]

Becker: There is another joint article that you wrote, this one with Ted Anderson, that also continues to be highly cited. What is that article about?

Goodman: Ted and I are the coauthors of an article on "Statistical Inference about Markov Chains" (*Ann. Math. Stat.* 1957). This joint article is Ted's most cited article and my second most cited article. Our joint article developed and extended the theory and methods presented earlier by Ted in a 1954 article, and it also presented some newer methods, which were first presented by me in a preliminary report at the 1955 meeting of the Institute of Mathematical Statistics. According to the ISI, our joint article has been cited around 600 times so far. As was the case with the 1954 joint article that Bill and I wrote, Ted's and my 1957 joint article still continues each year to be cited by a wide range of different articles in journals that cover a very wide range of different fields of study. In each of the past two years, the number of citations of our 1957 joint article was greater than the corresponding number in each of the fifteen years prior to those two years, except for one of those fifteen years.

Becker: You have written many other articles on many other topics over these many years, in addition to the joint article that you wrote with Bill Kruskal and the joint article that you wrote with Ted Anderson. And

you have received some special recognition of this by the Institute for Scientific Information (ISI). What was that special recognition from the ISI?

Goodman: A few years ago, the ISI informed me that I have been identified as an "ISI Highly Cited Researcher." The institute stated that it has identified the "250 most cited researchers in the last two decades, for their published articles in the Mathematics category." For the Mathematics category, citations of the researcher's articles published in mathematics journals are considered, with statistics journals included in that category.

Well, Mark, I have been around for quite a long time, and I have written quite a few articles—over 150 articles so far—and I have had published four different books (each book a gathering of my articles on a particular topic, with one of the books a gathering of Bill's and my joint articles).

Becker: Leo, the eminent quantitative sociologist, Otis Dudley Duncan, has published comments on your statistical contributions in four different research areas that are of interest to sociologists and to some other social and behavioral scientists. How did this come about?

Goodman: The American Sociological Association (ASA) had selected Dudley and me to share their main methodology award, the Samuel A. Stouffer Methodology Award. When I heard this news, I felt pleased and honored, especially since I thought very highly of Dudley's research work. However, Dudley had a quite different reaction to this news. He wrote a statement, which was published in the *ASA Footnotes*, that commented on my research work in four different areas of interest, and he said that it would be a great honor to share the award with me, but he felt strongly that I should be the sole recipient of the award, and that the honor should not be diluted. He turned down the award.

Becker: What were Duncan's comments about your work in those four areas of interest?

Goodman: The four areas were the following: (1) social stratification and mobility, (2) survey data analysis, (3) panel studies data analysis, (4) latent structure analysis.

With respect to social stratification and mobility, Dudley said that my work on methods for analyzing social mobility tables had solved a problem that had plagued research workers in this field for at least two decades; and, in solving this problem, my work had rendered obsolete a substantial corpus of previous work.

With respect to survey data analysis, he said that my collection of models for survey analysis had provided for the first time a set of statistical methods that were adequate to the tasks posed by the "language of social research" hitherto associated with the Columbia school and kindred approaches to survey analysis; and that the practiced user of my methods could accomplish with ease everything that this school attempted, and a great deal more. He also said that almost any complex body of data previously analyzed by even skilled practitioners of survey analysis, of the kind associated with the Columbia School and kindred approaches, yields different conclusions from those obtained with my methods, and that it is easy to see after the fact how the practitioner fell into error.

With respect to panel studies data analysis, he said that I had put panel analysis on a sound footing for the first time, in a similar way to what I had done with survey analysis, and, as a consequence, a substantial body of previous misguided literature that provided erroneous, misleading or merely useless procedures for manipulating panel data and survey data could now be ignored.

And, with respect to latent structure analysis, Dudley said that I had provided a substantial statistical foundation for latent structure models, and that the methods of analysis that had been suggested earlier, and that had been applied over the preceding 25 or 30 years by various research workers, had not been satisfactory; but now, using the methods introduced in the statistical foundation that I had presented, it was possible to begin to understand correctly what is at stake.

Dudley's statement referred to work done by me in those four different but related research areas, published in various statistics journals and sociology journals. I have continued to work in one or more of these four research areas from time to time, and also in other research areas as well, and I recently returned to all four of those research areas when I was invited by the Editorial Committee of the *Annual Review of Sociology* to write the lead article for their 2007 volume. I then wrote an article for the *Annual Review* on "Statistical Magic and/or Statistical Serendipity: An Age of Progress in the Analysis of Categorical Data." This article describes in simple terms some of the major concepts of categorical data analysis (CDA) that have been useful in the analysis of sociological data, examples of which include data in the area of social stratification and mobility, and in many other areas that make use of survey data and/or panel studies data, and in the empirical study of latent types, latent variables and

latent structures. The exposition in that article does not make use of any mathematical formulas. Simple numerical examples, constructed for expository purposes, are used as an aid in describing CDA concepts, including quasi-independence, quasi-symmetry, symmetric association and uniform association, which are concepts useful in the analysis of social mobility tables, log-linear models useful in the analysis of survey data, recursive models useful in the analysis of panel studies data and latent class models useful in the analysis of latent structures.

Becker: You told us earlier about your two most cited articles, the 1954 joint article with Bill Kruskal and the 1957 joint article with Anderson. What about other works, which ones stand out in your mind as having had a particularly significant impact?

Goodman: The third most cited has been my main article introducing log-linear models, "Multivariate Analysis of Qualitative Data: Interactions Among Multiple Classifications" (*JASA*, 1970). It has been cited around 450 times so far.

Mark, while I was writing my 1970 *JASA* article, I also developed a computer program in order to apply the log-linear models, which were introduced in the article, to analyze in the article the set of data in the multidimensional contingency table presented there. (This computer program was also then used in some of my other articles on log-linear models in order to analyze the different sets of data in the multidimensional contingency tables presented in those articles; e.g., in *Technometrics*, 1971; *American Sociological Review*, 1972; *American Journal of Sociology*, 1972, 1973; *Biometrika*, 1973; *JASA*, 1973.) When I was developing the computer program, I wanted to make it easy to use by anyone—even someone who had no familiarity with statistics or hardly any familiarity with the subject. One of our graduate students at that time, Robert (Bob) Fay, helped me make the computer program as user-friendly as was possible at that time. The computer program was called *ECTA*: *Everyman's Contingency Table Analyzer.* (Mark, perhaps I should have called it "Everyperson's" or "Everyman's and Everywoman's" rather than just "Everyman's"; but at that time, I viewed "Everyman" to mean also "Everyperson" and/or "Everyman and Everywoman.") [Smile]

Copies of *ECTA* were then sold at cost by our statistics department at the University of Chicago. It is my impression that, over the years, as many as five hundred people from many countries all over the world purchased copies. Now most statistical computer packages include a log-linear program.

Five years after my 1970 *JASA* article appeared in print, Shelby Haberman's monograph, *The Analysis of Frequency Data*, was published, and it made substantial theoretical contributions to log-linear modeling, This monograph was based on the earlier Ph.D. thesis that Shelby wrote when he was a graduate student in our statistics department at the University of Chicago. (I was the advisor on his Ph.D. thesis.) And starting six years after my 1970 article appeared in print, and continuing until now, many good textbooks, covering log-linear analysis and other related topics in the analysis of categorical data (frequency data, qualitative data), were published; and many statistics departments began to introduce courses covering these subjects in their curriculum.

Mark, returning now to your question about which of my other works (besides the Goodman/Kruskal article and the Anderson/Goodman article) had a significant impact, in addition to my 1970 *JASA* article on log-linear models, there are also eleven articles of mine, each of which has been cited between 200 and 400 times so far. I think it is interesting, and sometimes surprising, to see which articles appear in this group, and which do not.

Becker: Leo, I will include the articles in this highly cited group, and also some of the articles that are not in this group, as an Appendix to this interview, which will be located at the end of the interview. Here now is a question about another important set of your articles:

You have written many articles developing modeling frameworks, and the corresponding statistical methods, for the analysis of multidimensional cross-classifications of counts. I have in mind here your evolutionary path from log-linear models to the recursive-models modeling framework, the latent-class modeling framework, the association-models (log-bilinear) modeling framework and the correspondence-analysis modeling framework. A hallmark of these works is that they are expertly crafted and exquisitely illustrated with insightful applications. What has been your approach to developing these modeling frameworks?

Goodman: In my 2007 *Annual Review of Sociology* article on "Statistical Magic and/or Statistical Serendipity: …," which I mentioned briefly earlier, I discuss two different methods that I have used to obtain the results presented in those many articles on modeling formulations, namely, statistical magic and statistical serendipity. [Smile/Laughter]

First, let me comment on magic: By a magical result, I don't mean here a result obtained by magic or

by some other supernatural means, but rather a result obtained as if by magic.

When the great Michelangelo was sculpting his colossal figure of *David*, he worked under the premise that the image of *David* was already in the block of marble that he had selected, and his task was to release the image from the block. Now, faced with a set of categorical data of interest, say, a multidimensional cross-classification of counts, data analysts can work under the premise that there is an image, or more than one image, embedded in the set of data, and their task is to release that image, or those images, using suitable tools.

The tools might include the kinds of modeling frameworks developed in some of my articles and also other kinds of tools of categorical data analysis. The results obtained by using these tools sometimes seem magical—the sudden release of form formerly hidden, embedded in a block of dense data.

Now, let me comment on serendipity: By a serendipitous result, I don't mean here a result obtained simply by accident or chance, but rather a result obtained by an accidental exposure to information and an application of the prepared mind. Perhaps serendipity, rather than magic, better describes the way in which the modeling frameworks were developed. [By the way, Mark, I discuss the possible meanings of *serendipity* in "Notes on the Etymology of *Serendipity* and Some Related Philological Observations" (*Modern Language Notes*, 1961).]

When I first developed the general concept of quasi-independence and the corresponding iterative procedure needed to apply this modeling formulation in the analysis of data in a cross-classification of interest, and, in particular, in the analysis of social mobility tables (see, e.g., *JASA*, 1968), the information to which I was exposed in my work on this one statistical problem, and on the corresponding set of substantive areas of interest, then led me to look at a second set of substantive areas of interest and to develop the general concept of the log-linear model and the corresponding iterative procedure needed to apply this modeling formulation in the analysis of data in a multidimensional cross-classification of interest, and, in particular, in the analysis of survey data (see, e.g., *JASA*, 1970). And the information to which I was exposed in my work on this statistical problem, and on the corresponding second set of substantive areas of interest, then led me to look at a third set of substantive areas of interest pertaining to recursive models, and to develop the corresponding iterative procedure needed to apply

this modeling formulation in the analysis of data in a multidimensional cross-classification table in which some variables (dimensions) are posterior to other variables (dimensions), and, in particular, in the analysis of panel studies data (see, e.g., *Biometrika*, 1973). And the information to which I was exposed in my work on this statistical problem and on the preceding statistical problem, and on the corresponding sets of substantive areas of interest, then led me to look at a fourth set of substantive areas of interest pertaining to latent types, latent variables and latent structures, and to develop the corresponding iterative procedure needed to apply this modeling formulation in the analysis of data in a multidimensional cross-classification table in which some variables (dimensions) are observable, and some of the variables (dimensions) are unobservable (latent) (see, e.g., *Biometrika*, 1974). Then the log-linear models formulation applied to the two-way cross-classification of interest and the corresponding iterative procedure led me to develop the log-bilinear models (viz., the association models) formulation and the corresponding iterative procedure needed to apply this modeling formulation (see, e.g., *JASA*, 1979). And the association models formulation applied to the two-way cross-classification of interest then led me to develop the correspondence-analysis model formulation applied to the two-way and the *m*-way ($m > 2$) cross-classification of interest, and to develop further the association models formulation applied to the *m*-way ($m > 2$) cross-classification of interest, and the corresponding iterative procedure needed to apply these modeling formulations (see, e.g., *Ann. Stat.*, 1985; *International Stat. Rev.*, 1986; *JASA*, 1991).

This step-by-step movement from one statistical problem to the next statistical problem, and from the corresponding iterative procedure appropriate for the one statistical problem to the corresponding iterative procedure appropriate for the next statistical problem, might be described as step-by-step evolutionary elaboration.

Becker: What are some of the interesting experiences you have had in connection with the writing of some of your articles?

Goodman: Mark, the following experience took place having, as the background setting, the Cold War between the Soviet Union and the United States: [Smile] In the late 1950s, I wrote a number of statistical articles pertaining to Markov chains (*Ann. Math. Stat.*, 1958a, 1958b, 1959; *Biometrika*, 1958), and after those articles were published, I happened to come across a 1957 note by V. E. Stepanov on Markov chains, written

in Russian and published in the Soviet journal, *Teoriya Veryatnostei i ee Primeneniya* (*The Theory of Probability and Its Applications).* (Stepanov was one of the Russian mathematician/probablists who worked in the famous Russian school of probability, founded by Kolmogorov, Markov, Kinchin and Lyapunov.) I wasn't able to read the Russian in the Stepanov note, but I could read the formulas, and I could see from the formulas that the topic covered in the Russian note was very similar to the topic that I had covered in some of my articles. So I had the Russian note translated, and I then studied the translated version. And I found that there was a serious error in the note, and that the main result was incorrect. I then wrote "A Note on Stepanov's Test for Markov Chains," showing what was incorrect in Stepanov's note and also showing how what was incorrect could be replaced by a corresponding correct result. I sent it to the Editor of the Soviet journal for submission, and received a very quick response saying that it would be published "in the nearest possible future." When the galley proof arrived, I noticed that my Abstract had been deleted and replaced by a Russian Abstract in which any reference to the error in Stepanov's note and the correction in my note had been deleted. The Russian Abstract was misleading. So I had my Abstract translated into Russian, and I sent it to the Editor informing him that the Russian Abstract that was in the galley proof needed to be replaced by my Russian Abstract. Now, Mark, what do you think the Editor did about this?

Well, the Editor didn't respond to my request, and he didn't pay any attention to my Russian Abstract. When my note was published in the Soviet journal, I then saw that the note included the misleading Russian Abstract that was in the galley proof, and it also included something else that really surprised me. In addition to the misleading Russian Abstract, it also included a second Abstract, my original Abstract (which had been written in English) printed in English in my note!

Here is my conjecture as to why the Editor included the two Abstracts in the published note: I think that he included the misleading Russian Abstract because he didn't want any nontechnical Russian readers to know that an American statistician was critical of work done by a Russian mathematician/probabilist; and he included my Abstract in English because he wanted technical readers, who might be interested in the topic, to know as much as possible about the topic.

Here now is another interesting experience that I had: This experience takes place having, as the background setting, the early 19th century French philosophers. [Smile] In 1819, the Marquis de Laplace, in

A Philosophical Essay on Probabilities (title translated from French), discussed the attempts that had been made still earlier to explain the excess of the birth of boys over those of girls by the general desire of fathers to have a son. Laplace's results suggested that the sex ratio at birth of boys to girls will be unaffected by this general desire. In the early 1950s, the statistician Herb Robbins arrived at a similar conclusion. However, in the early 1960s, I found, in the sociological literature, first, an article by a sociologist who suggested that, for the particular group of families he had studied, the prevalence of the desire for male offspring on the part of parents, together with their knowledge of methods of birth-control, appeared to be significant in relation to the high sex-ratio at birth of boys to girls; and, second, an article by another sociologist who proved that the sex-ratio at birth of boys to girls will be either decreased or unaffected by the preference for male offspring, if certain assumptions can be made concerning the way in which this preference affects the parents' decisions as to whether or not to have another child.

Taking all this into account, I was able to reconcile the different conclusions obtained by these different authors in an article that I wrote on "Some Possible Effects of Birth Control on the Human Sex Ratio," in which I presented a general framework for the study of these possible effects that included as special cases each of the possible assumptions that might be made in this context, and I introduced formulas that show under which possible assumptions the sex ratio would be unaffected, under which assumptions it would be decreased, and under which assumptions it would be increased. The article was published in the *Ann. Human Genetics (London)* in 1961, and it was reprinted in 1966 in a volume on *Readings in Mathematical Social Science.*

Next is another interesting experience that I had: It took place having, as the background setting, James Bond, Agent 007. [Smile] In the early 1950s, I came across an article that described how the Allies in World War II analyzed serial numbers obtained from captured German equipment in order to obtain estimates of German war production and capacity. Within the limits of its capabilities, this method of analysis turned out to be superior to more abstract methods of intelligence. After reading that article, I thought that I would try, for the fun of it, to see if I could improve on the method of analysis of the serial numbers that was described in that article. Well, it happened to turn out that I could improve on it. With my method of analysis of the serial numbers, I was able to introduce a simple estimator

that was the most efficient unbiased estimator of the corresponding total number of pieces of equipment in the population of pieces of equipment from which the observed serial numbers came. I was also able to show that the method of estimation described in the article on the World War II method of analysis provided an estimator that was unbiased, and that the efficiency of that estimator was relatively high for large or moderate sized samples of serial numbers. I wrote up these results, together with some other statistical results on this subject, in "Serial Number Analysis" (*JASA*, 1952). About five or so years after the publication of that article, although he should not have told me about this, someone whom I knew told me that there was a group of people in the government in Washington, D.C., who were using what they called the "Goodman method," making use of the results in that article. I was surprised by this news, although perhaps I should not have been.

Now let me tell you about just one more interesting experience, which I had just last year: [Smile] The U.S. Court of Appeals for the Ninth Circuit is the largest of the thirteen U.S. circuit courts of appeals. Over the past thirty years or so, many different legislative proposals to split the Ninth Circuit have been introduced in the U.S. Congress (in both the Senate and in the House of Representatives), and each of these proposals has failed to become law. There has been no consensus within Congress. The debate on this subject was revisited a year ago in The Los Angeles Times (Opinion, July 11, 2007), this time with a statistical argument purporting to conclude that the Ninth Circuit Court should be split. Then the Circuit Executive of the Ninth Circuit Court contacted me to inquire whether I thought that there could be a rejoinder to the statistical argument in the LA Times. I replied that there could be, and I then wrote a rejoinder in the form of an OpEd statement that I submitted for publication in the LA Times. It was rejected. I then submitted it in turn to two law newspapers, and it was also rejected by each of them. (The newspapers' rejections were explained this way: The Republicans are no longer the majority party in Congress, and it was primarily Republicans in Congress who, over the past thirty years or so, had tried to split the Ninth Circuit. So there is less interest in this subject now that the Democrats are the majority party.) I then wrote an article, "To Split or Not To Split the U.S. Ninth Circuit Court of Appeals: A Simple Statistical Argument, Counterargument, and Critique," which has now been published in the *Journal of Statistical Planning and Inference* (*JSPI*, 2008).

Becker: Your 1974 article in *Biometrika*, "Exploratory latent structure analysis using both identifiable and unidentifiable models," is one of my personal favorites. It brought clarity to an area of data analysis and modeling that was cluttered at the time, and in a very straightforward way you used what we know today as the EM-algorithm to both provide a computational devise and theoretical insights. You must be pleased to have both "anticipated" the EM-algorithm and to have exploited it for theoretical gain.

Goodman: Mark, as you know, the first article on the "EM-algorithm" was written by Art Dempster, Nan Laird and Don Rubin, and was published in 1977 in the *J. Roy. Stat. Soc.* And, as you said just now, my article was published in 1974 in *Biometrika*. When the data in a two-way or in an m-way ($m > 2$) cross-classification are analyzed using a latent-class model, the "EM-algorithm" described in the 1977 article is essentially the same as the iterative procedure introduced earlier in my 1974 article. And so, with the 1974 article iterative procedure available, the 1997 article "EM-algorithm" isn't needed to analyze a two-way or an m-way ($m > 2$) cross-classification using a latent-class model. The 1997 article "EM-algorithm" is useful in other contexts.

I am pleased that, with my 1974 *Biometrika* article and the 1997 *J. Roy. Stat. Soc.* article available, the appropriate computational device is now widely used, and the theoretical insights are now widely understood, in the study of latent structures. (See, for example, my 2002 article, "Latent Class Analysis: The Empirical Study of Latent Types, Latent Variables, and Latent Structures," which is the lead article in *Applied Latent Class Analysis*, edited by Jacques Hagenaars and Allan McCutcheon.)

Becker: You were a member of the Population Research Center at the University of Chicago, and you've published in demography. I know that you wrote one article with (the eminent demographer) Nathan Keyfitz who was also at Chicago at some time. Was he there when you and he wrote that joint article? And was the work that you did in demography inspired by him in any ways?

Goodman: Nathan Keyfitz was at the University of Chicago from 1963 to 1968. The Goodman/Keyfitz/Pullum article on "Family Formation and the Frequency of Various Kinship Relationships" was published in *Theoretical Population Biology* in 1974. Nathan and I started working on that article during the time period when he and I were colleagues at the University of Chicago. The third coauthor of that article,

Tom Pullum, had been a graduate student in the sociology department at the University of Chicago during a part of the time when Keyfitz was there, and he received his Ph.D. degree in 1971. (I was the advisor on his Ph.D. thesis.) In 1974, when our joint article was published, Nathan was a professor at Harvard, and Tom was an assistant professor there.

Nathan has had a very interesting career, and I would like now to tell you about it. He graduated with a degree in mathematics from McGill University in Montreal in 1934, and in 1936 he began working for the Dominion Bureau of Statistics, the precursor to Statistics Canada, in Ottawa, as a research statistician and later as a mathematical and senior statistical advisor. He analyzed Canadian census schedules and census results, and he prepared statistical surveys that examined various characteristics of the Canadian population. He remained with the bureau for the next 23 years. In 1952 he received a fellowship to attend the University of Chicago, and he graduated with a Ph.D. in Sociology. (I was one of the examiners on his oral exam, and I can attest to the fact that he did very well indeed.) In 1963, at the age of 50, he was invited to join the Sociology Department faculty at the University of Chicago, and he accepted the invitation. At that point, as far as I know, he had not expressed any special interest in the field of mathematical demography nor in the application of mathematical tools and computer technology to the analysis of demographic data.

After Nathan's arrival in Chicago, in one of our first conversations, I happened to mention to him that I had written an article in mathematical demography ten years earlier on the "Population Growth of the Sexes" (*Biometrics*, 1953), and that I thought that much more work could be done on this topic and in other areas of mathematical demography as well. He and I then began to stimulate each other to do research in this field. He then wrote many very good articles and some very good books in this field, and I also wrote some articles in this field, and in some other related fields too. He and I became good friends. He became a very important leader in the field of mathematical demography and a pioneer in the application of mathematical tools and computer technology to the analysis of demographic data.

After Nathan taught at the University of Chicago, he then taught at the University of California at Berkeley for four years (1968–1971), and then he taught at Harvard for twelve years (1972–1983). He then spent ten years (1984–1993) as the Project Leader of the Population Project at the International Institute of Applied Systems Analysis (IIASA) in Austria, before returning to Cambridge. Between 1973 and 1993, seven different universities awarded him honorary doctorates, and in 1997 he was named the International Union for the Scientific Study of Population (IUSSP) Laureate. Isn't it interesting, Mark, that all this began after Nathan's arrival at the University of Chicago as a faculty member at the age of 50? He is now over 95 years of age, and in good shape considering his age.

I wrote twelve articles in demography and related fields, one of which was the 1974 joint article with Nathan and Tom. The twelve articles appear in seven different journals, and they cover a wide range of topics. Two of the articles were published before Nathan's arrival on the faculty at the University of Chicago, and seven of the articles were published during the six year period when he and I were colleagues there.

Becker: Leo, I will include a reference to your articles in demography and related fields in the Appendix. Now let us move from demography and related subjects to economics.

W. Allen Wallis, the first chairman of the Statistics Department at the University of Chicago, was both a statistician and an economist. You wrote an article related to a joint article that Wallis wrote with Geoffrey Moore, an expert in economic statistics and business cycle research, and you also wrote some other articles of special interest to economists. How did this come about?

Goodman: One day, sometime in the second half of the 1950s, Allen happened to be telling me about this joint article that he had written with Geoffrey Moore about fifteen years earlier, which introduced a time-series significance test concerning the correlation between the movements in two different time-series, based on the signs of the first differences in each of the time-series. He told me that, in the joint article, he and Moore described the conditions under which their test would be valid, but they were aware of the fact that those conditions were actually not realistic conditions—those conditions wouldn't be satisfied by real economic time-series. He and Moore noted in their article that more research was needed in order to find out how their test would need to be modified so that the modified test would be valid under more realistic conditions. Allen then told me that, even though about fifteen years had now gone by since the publication of the joint article, no one yet had successfully solved this problem.

And so I then worked on this problem for a while, and I happened to solve the problem. I then invited a

good friend of mine, an economist, Yehuda Grunfeld, to join me in writing a joint article that would introduce an appropriately modified time-series significance test concerning the correlation between the movements between two different time-series (a new test valid under realistic conditions), and that would apply this test to some economic time-series of interest. We then wrote our joint article on "Some Nonparametric Tests for Co-movements Between Time-Series" (*JASA*, 1961).

About eight months before our joint article appeared in print, a terrible tragedy occurred: Yehuda died in a drowning accident at the age of 30. At the time of his death, he was Lecturer in Economics and Statistics at the Hebrew University of Jerusalem, and earlier he had been a graduate student and then an Assistant Professor in the Economics Department at the University of Chicago. In memory of Yehuda, a volume was published, *Measurement in Economics: Studies in Mathematical Economics and Econometrics*, in 1963. Authors contributing to this volume included the Nobel Laureate Milton Friedman and other top economists and econometricians. A section on Econometric Methodology was included in the volume, and I wrote on "Tests Based on the Movements in and the Comovements between m-Dependent Time-Series," which was included as the first article in that section.

The results presented in my article on movements in and comovements between time-series were a further development and extension of results presented in the earlier joint article that Yehuda and I wrote on comovements, which could be viewed as a further development and extension of results presented in the Wallis/Moore joint article.

Mark, I would like now to tell you about Allen Wallis. During World War II, Allen, in his early 30s, served as director of research in a Statistical Research Group of the U.S. Office of Scientific Research, and he recruited a stellar group of young statisticians, mathematicians and economists who contributed significantly to the war effort in many ways. Before the war, Allen taught very briefly at Yale, Columbia and Stanford universities; and after the war, he went back very briefly to Stanford, and then he became a faculty member at the University of Chicago. As I said earlier, Allen was responsible for establishing our statistics department at the University of Chicago, and he was the first chairman of the department. He then became the dean of the University of Chicago Graduate School of Business, and then he moved to the University of Rochester as president and then chancellor, and after he retired

there, he served as Under Secretary of State for Economic Affairs in the Reagan administration. He also served as an economic advisor to four U.S. presidents, Eisenhower, Nixon, Ford and Reagan.

Well, Mark, let's return now to your question about the work that I did of interest to economists. In addition to my article on movements in and comovements between time series, and my joint article with Yehuda, I also wrote a joint article with another economist, Harry Markowitz, who later was awarded a Nobel Prize in Economics. (Strictly speaking, the prize in Economics is actually the "Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel.") The prize was for Harry's pioneering work in financial economics, in modern portfolio theory, and in studying the effects of asset risk, correlation and diversification on expected investment portfolio returns.

In the early 1950s, Harry was a graduate student in the Economics Department at the University of Chicago, and he was also a Research Fellow at the Cowles Commission for Research in Economics, which was affiliated with the university and with the Economics Department. At the Cowles Commission at that time, there was great interest in recent work by the economist Ken Arrow, who had earlier been a Research Associate at the Cowles Commission and an Assistant Professor in the Economics Department at the university. (Arrow was also awarded a Nobel Prize in Economics for his pioneering contributions to general economic equilibrium theory and welfare theory.)

The work by Arrow that was of great interest at the Cowles Commission in the early 1950s was his research on *Social Choice and Individual Values*, in which he described five apparently reasonable properties that any voting system or other "social welfare function" should have, and he demonstrated mathematically that no voting system (or other social welfare function) could possibly have all of these properties. Harry and I, in our joint article, "Social Welfare Functions Based on Individual Rankings" (*Am. J. Soc.*, 1952), demonstrate that one of Arrow's required properties is questionable, and, if this property is modified, then many voting systems become acceptable. The joint article also considers which of the many voting systems, considered acceptable by us, seem most reasonable.

Becker: You mentioned the Cowles Commission for Research in Economics. What was your relationship with the Commission, and with some of the other members of the Commission?

Goodman: The Cowles Commission was founded to advance the scientific study and development of economic theory in its relation to mathematics and statistics, and it played a major role in promoting the use of mathematics and statistics in economics. Innovative and seminal work in mathematical economics and econometrics took place at the Commission in the years 1943–1955, years in which the research directors were first Jacob Marschak and then Tjalling Koopmans. The research output at the Commission over that period of time was extraordinary. Nine economists who were at the Cowles Commission at some time during the 1943–1955 period were later awarded Nobel Prizes, and I am quite sure that there would have been ten such Nobel Laureates if Marschak had lived a little bit longer. The nine were Ken Arrow, Tjalling Koopmans, Herbert Simon, Lawrence Klein, Gerard Debreu, Franco Modigliani, Trygve Haavelmo, Harry Markowitz and Leonid Hurwicz.

Jacob (Yascha) Marschak and I were good friends, and he would from time to time ask me statistical questions that he needed to deal with in his economics research, and from time to time I knew the answers to his questions or I could figure out the answers. Some other economists at the Cowles Commission would also ask me statistical questions from time to time. In 1955, the Commission moved from the University of Chicago to Yale where it was renamed the Cowles Foundation for Research in Economics. Yale at that time did not have a statistics department, and there weren't any statisticians at the university who could help Cowles Foundation members with statistical questions that came up in their economics research.

In 1960, Kingman Brewster was selected by the Yale President, A. Whitney Griswold, to be the Provost at Yale, and the Cowles Foundation then asked Brewster to establish a statistics department there. The Foundation also invited me to give a talk at their Economics Colloquium. When I came to Yale to give the talk, I was introduced to Brewster, and he came to the talk. In the question and answer period following the talk, Brewster asked some excellent questions. I was very impressed. He then invited me to meet with him for lunch at the Harvard Club in New York. (I was at that time a visiting professor at Columbia University in New York, on leave from the University of Chicago.)

At our first lunch meeting, we considered the possibility of establishing a statistics department at Yale, and it seemed pretty clear to me that Brewster was very uncertain about moving ahead with this. He was the Provost, and he was being groomed to become possibly the next President of Yale. If he, as the Provost at Yale, were to establish a new department in some field, it would, of course, need to be the best department, or at least among the very best departments, in that field in the country. At another lunch meeting, he was telling me at one point in our conversation that, when he was an undergraduate at Yale, in order to be considered a Yale man—a Yale educated man—there was a special course in philosophy that one would need to complete, taught by a very special professor (whose name I have now forgotten). And I responded as follows: "Well, that was in your day at Yale. But now, in the second half of the twentieth century, in order to become an educated person, a Yale man could use a good course in statistics, in part, in order to help him to avoid being misled by statements read in newspapers or heard on radio or television, or more generally communicated by any medium." After that, Brewster seemed no longer to be uncertain about moving ahead with the possibility of establishing a statistics department, and we began to discuss in detail what was needed in order for this to happen. At our next lunch meeting, Brewster offered me the job of building the department, and I thanked him for the offer. I thought about the offer for a few days, and then told Brewster that I was turning the offer down. He then asked me whom would I recommend for the job, and I recommended Frank Anscombe who was at Princeton at that time. Brewster then offered Anscombe the job, and Anscombe accepted the offer.

Becker: In this conversation, we have talked to some extent about your working relationships with some statisticians and social scientists, sociologists, demographers and economists, and about some joint work that you have done with some of these people. You had mentioned to me in some earlier conversation that there was a way of showing pictorially who were some of the people in these fields whose works were related to your works and/or who were some of the people in these fields to whose works your works were related. I would be interested to see this pictorial representation.

Goodman: Here it is—a map of authors who are co-cited with me in at least 100 articles. The information that was used to create this map was obtained from the Institute for Scientific Information (ISI) Web of Science, based on 5678 articles that cite articles written by me. The creator of this map was Olle Perrson, Professor of Sociology, at Umea University in Umea, Sweden.

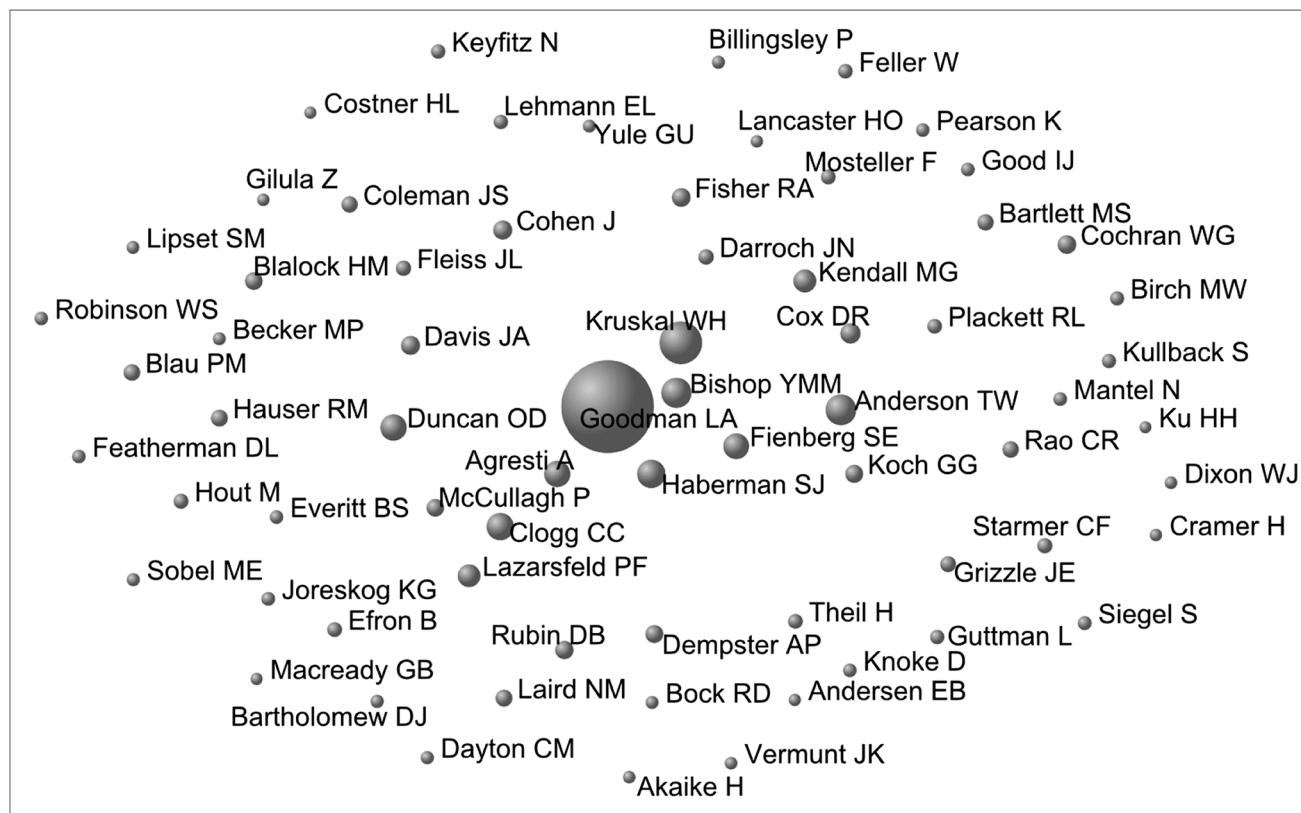The size of the ball associated with each author on the map is proportional to the number of citations of

FIG. 11.   *Map of authors co-cited with Leo Goodman in at least* 100 *articles.*

that author's articles in the 5678 articles; so we see here that the largest balls were for W. H. Kruskal, T. W. Anderson, Y. M. M. Bishop, S. J. Haberman, C. C. Clogg, O. D. Duncan, A. Agresti, S. E. Fienberg, M. G. Kendall; and the next largest balls were for P. F. Lazarsfeld, D. R. Cox, J. Cohen, J. A. Davis, R. A. Fisher, W. G. Cochran, D. B. Rubin, G. G. Koch, A. P. Dempster; and the next largest balls were for H. M. Blalock, P. McCullagh, N. M. Laird, R. M. Hauser, P. M. Blau, C. R. Rao, J. S. Coleman and M. S. Bartlett. The place where an author appears on the map is based on the number of co-citations of the author with me and also on the number of co-citations of the author with each of the other authors on the map. It turns out that the authors on the right side of the map are mainly statisticians, and there also are some mathematician/probabilists and some others there; and the authors on the left side of the map are mainly sociologists, and there also are some statisticians and some others there.

Becker: Leo, I can see how this kind of map could be useful in many different contexts. Thanks for bringing this to my attention. Now there is just one more topic that I feel I need to bring up with you before our con-

versation comes to an end. I know that you are a cancer survivor, now more than 30 years. How did your battle with and victory over cancer influence your work?

Goodman: After the course of treatment for my cancer was completed, I then wrote my main article introducing and developing association models: "Simple models for the Analysis of Association in Cross Classifications Having Ordered Categories" (*JASA*, 1979). I view the contents of this article as a big step forward in categorical data analysis (CDA). Somehow, having my mind completely focused, during the course of the cancer treatment, on doing whatever I could to increase the chances that I might become a cancer survivor, this focusing of my mind then helped me later to clear my mind, after the course of treatment was completed, and to take a big step forward when I was able to return to statistical work. [In Alan Agresti's first and second edition of his book on *Categorical Data Analysis* (Agresti, 1990, p. 505, and 2002, pp. 631), my 1979 *JASA* article is included in his list of 25 articles that convey a sense of how CDA methodology had evolved during the twentieth century. (By the way Mark, Agresti also lists, in the second edition of his book on *Categorical Data Analysis*, Karl Pearson, G. Udney Yule, Ronald

A. Fisher and Leo Goodman, as "Four leading figures in the development of categorical data analysis.")] My 1979 article was included as the core article in my 1984 book on *The Analysis of Cross-Classified Data Having Ordered Categories*, and I also extended the work presented in the 1979 article in some of my later work— *JASA*, 1981a, 1981b, 1991, 1996; *Ann. Stat.*, 1985; *International Stat. Rev.*, 1986; *Amer. J. Soc.*, 1987.

Mark, with respect to the cancer, here's an interesting experience that I had. This experience leads me to say sometimes that *it was statistics that saved my life*. [Smile] Here's what happened: After the surgical removal of the cancer in New York, there was a disagreement between my New York oncologist and a group of oncologists in Chicago as to what should be done next. The New York oncologist said that, for the particular kind of cancer that I have, a course of chemotherapy and immunotherapy should be administered at once; and the Chicago group of oncologists said that, for the particular kind of cancer that I have, a course of radiation should be administered at once, and that chemotherapy and immunotherapy should not be administered. The Chicago group of oncologists gave me a number of articles to read on this subject. These articles had been published in various British medical journals, and the abstract in each of the articles stated that, with this kind of cancer, radiation was recommended. I then studied carefully the text of each of these articles, and it seemed to me that the detailed medical and statistical evidence presented in the articles themselves did not warrant the recommendation presented in the abstracts. So I returned to the Chicago group to ask them some questions about the articles, and their responses to the questions left me with the impression that they had read the abstracts but they had not studied carefully the articles themselves. I then decided to follow the New York oncologist's regimen.

It turned out that the New York oncologist's regimen was somewhat similar to what was done at that time in France for this kind of cancer, and the Chicago group's

Karl Pearson   G. Udny Yule

Ronald A. Fisher   Leo Goodman

FIG. 12. *Four leading figures in the development of categorical data analysis. Source*: Alan Agresti, *"Categorical Data Analysis"* 2*nd edition*, 2002; *and "An Introduction to Categorical Data Analysis"* 1*st and* 2*nd edition*, 1996 *and* 2007.

regimen was similar to what was done at that time in Britain. A few years after I had completed the New York oncologist's regimen, it turned out that an international medical conference was held on "Cancers of the Mid-East," and comparisons were made there, for those who had the kind of cancer that I had, between mortality statistics for those receiving the British regimen in Britain and those receiving the French regimen in France. For the British patients, the death rate was really terrible, whereas the death rate for the French patients was not as bad. Mark, imagine what might have happened if I had just read the abstracts in the various British medical journals, and had not bothered to study carefully the detailed medical and statistical evidence presented in the articles themselves? [Smile]

Becker: Leo, thank you for being so generous with your time and reflections on your almost six decades statistical career. And what a career it has been—the experiences and relationships that you have had have been nothing short of amazing.

APPENDIX A

As was noted earlier in this conversation, there are eleven articles each cited between 200 and 400 times so far. These articles will be described here (but not necessarily in the order pertaining to the number of citations of each article): (1) the R. A. Fisher Memorial Lecture, "The Analysis of Cross-Classified Data: Independence, Quasi-Independence, and Interactions in Contingency Tables With or Without Missing Entries" (*JASA*, 1968); (2–3) two articles introducing new methods for the analysis of latent structures, "Exploratory Latent Structure Analysis Using Both Identifiable and Unidentifiable Models" (*Biometrika*, 1974), and "The Analysis of Systems of Qualitative Variables When Some of the Variables Are Unobservable: A Modified Latent Structure Approach," *Amer. J. Soc.* (*AJS*, 1974); (4) an article introducing association models, "Simple Models for the Analysis of Association in Cross-Classifications Having Ordered Categories" (*JASA*, 1979); (5) an article introducing various procedures for using log-linear models to fit contingency-table data, "Analysis of Multidimensional Contingency Tables: Stepwise Procedures and Direct Estimation Methods for Building Models for Multiple Classifications" (*Technometrics*, 1971); (6–7) two articles introducing multiplicative models to analyze categorical data, "A General Model for the Analysis of Surveys" (*AJS*, 1972), and "A Modified Multiple Regression Approach to the Analysis of Dichotomous Variables," *Amer. Soc. Rev.*

(*ASR*, 1972); (8–9) the second and third joint articles with Bill Kruskal, "Measures of Association for Cross Classifications II: Further Discussion and References" (*JASA*, 1959), and "Measures of Association for Cross Classifications III: Approximate Sampling Theory" (*JASA*, 1963); (10) an article on some methods for dealing with the ecological-correlation problem, "Some Alternatives to Ecological Correlation" (*AJS*, 1959); (11) an article introducing exact formulas for the variance of products, and formulas for estimating this variance, "On the Exact Variance of Products" (*JASA*, 1960).

There are twelve articles each of which has been cited between 100 and 200 times so far. These articles will be described next (but not necessarily in the order pertaining to the number of citations of each article): (1) the Henry L. Rietz Memorial Lecture, "The Analysis of Cross Classified Data Having Ordered and/or Unordered Categories: Association Models. Correlation Models, and Asymmetry Models for Contingency Tables With or Without Missing Entries" (*Ann. Stat.*, 1985); (2) an article introducing correspondence analysis models, "Some Useful Extensions of the Usual Correspondence Analysis Approach and the Usual Log-Linear Models Approach in the Analysis of Contingency Tables" (*International Stat. Rev.*, 1986); (3–4) two articles introducing recursive models for panel analysis, "The Analysis of Multidimensional Contingency Tables When Some Variables Are Posterior to Others: A Modified Path Analysis Approach" (*Biometrika*, 1973), and "Causal Analysis of Data from Panel Studies and Other Kinds of Surveys," *Amer. J. Soc.* (*AJS*, 1973); (5) an article introducing methods for the analysis of data obtained by snowball sampling, "Snowball Sampling" (*Ann. Math. Stat.*, 1961); (6) a joint article with Clifford Clogg, introducing latent structure models for analyzing simultaneously more than one multidimensional contingency table, "Latent Structure Analysis of a Set of Multidimensional Contingency Tables" (*JASA*, 1984); (7) an article introducing additional methods for analyzing mobility tables, "How to Ransack Social Mobility Tables and Other Kinds of Cross-Classification Tables" (*AJS*, 1973); (8) an article introducing new methods for analyzing scales, "A New Model for Scaling Response Patterns: An Application of the Quasi-Independence Concept" (*JASA*, 1975); (9) the fourth joint article with Bill Kruskal, "Measures of Association for Cross Classification IV: Simplification of Asymptotic Variances" (*JASA*, 1972); (10) an article describing the relationship between RC association models and canonical correlation, "Association

Models and Canonical Correlation in the Analysis of Cross Classifications Having Ordered Categories" (*JASA*, 1981); (11–12) two articles on simultaneous confidence intervals, "Simultaneous Confidence Intervals for Contrasts Among Multinomial Populations" (*Ann. Math. Stat.*, 1964), and "On Simultaneous Confidence Intervals for Multinomial Proportions" (*Technometrics*, 1965).

There are fifteen articles each of which has been cited between 50 and 100 times so far. Ten of these articles will be described next (but not necessarily in the order pertaining to the number of citations of each article): (1) an article, based on an invited lecture, presented at the invitation of the Amer. Stat. Assoc. Social Statistics Section, on correspondence analysis models and related topics, with comments by J. P. Benzecri, the founder of and major figure in the "French school of data analysis" (the school of correspondence analysis), and also comments by D. R. Cox, C. R. Rao, S. J. Haberman. H. Caussinus, C. C. Clogg, and by three others, and a rejoinder by me, "Measures, Models, and Graphical Displays in the Analysis of Cross-Classified Data" (*JASA*, 1991); (2) a joint article with Nathan Keyfitz and Tom Pullum on kinship relationships, "Family Formation and the Frequency of Various Kinship Relationships" (*Theoretical Population Biology*, 1974); (3) an article in mathematical demography, "Population Growth of the Sexes" (*Biometrics*, 1953); (4) an article introducing multiplicative models for mobility table analysis, "Multiplicative Models for the Analysis of Occupational Mobility Tables and Other Kinds of Cross-Classification Tables" (*AJS*,1979); (5) an article on the relationship between the RC association models and the bivariate normal distribution, "Association Models and the Bivariate Normal for Contingency Tables with Ordered Categories" (*Biometrika*, 1981); (6) an article on additional methods for analyzing multidimensional contingency tables, "Partitioning of Chi-Square, Analysis of Marginal Contingency Tables, and Estimation of Expected Frequencies in Multidimensional Contingency Tables" (*JASA*, 1971); (7) an article on the analysis of certain kinds of panel data using the mover-stayer model,

a generalization of the Markov chain model, "Statistical Methods for the Mover-Stayer Model" (*JASA*, 1961); (8) an article presenting explicit formulas for analyzing three-factor interaction in contingency tables, "Simple Methods for Analyzing Three-Factor Interaction in Contingency Tables" (*JASA*, 1964); (9) an article on simultaneous confidence-limits, "Simultaneous Confidence-Limits for Cross-Product Ratios in Contingency Tables" (*J. Roy. Stat. Soc.*, *Ser. B*, 1964); (10) an article developing further some statistical methods presented in an earlier article by Ted Anderson and in the earlier joint Anderson/Goodman article, "Statistical Methods for Analyzing Processes of Change" (*AJS*, 1962).

APPENDIX B

Twelve articles in demography and related fields: *Biometrics*, 1953, 1969; *Ann. Human Genetics (London)*, 1961, 1963; *Demography*, 1967, 1968; *J. Roy. Stat. Soc., Ser. A*, 1967; *Biometrika*, 1967, 1968; *Ann. Math. Stat.*, 1968; *Theoretical Population Biology*, 1971, 1974 (the joint Goodman/Keyfitz/Pullum article).

APPENDIX C

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