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A Conservation with Frank Proschan

Myles Hollander, with Albert W. Marshall in attendance

Abstract. Frank Proschan was born on April 7, 1921, in New York City. He earned a B.S. in mathematics from City College of New York in 1941, an M.A. in mathematical statistics from George Washington University in 1948 and a Ph.D. in statistics from Stanford University in 1959. He has held positions with the federal government at the National Bureau of Standards (1941–1952), with Sylvania Electric Products (1952–1960) and with Boeing Scientific Laboratories (1960–1970). Since 1970 he has been Professor of Statistics at Florida State University. He has also been a visiting lecturer at U.C. Berkeley (1964–1965), Distinguished Visiting Professor at Texas A & M University (summer 1967) and Stanford University (1960–1970). His many honors include the following: the Von Neumann Prize Award presented by TIMS-ORSA (jointly with R. E. Barlow, 1991); the Townsend Harris Medal (Alumni Association of City College of New York, 1983); the Wilks Medal Award (American Statistical Association, 1982); the Distinguished Alumni Award (George Washington University, 1978); Winner, Best Moderator Award (Annual Reliability and Maintainability Symposium, 1973); and Winner, Ford Foundation Doctoral Dissertation Award (1959). He is a Fellow of the Institute of Mathematical Statistics, a Fellow of the American Statistical Association and an elected member of the International Statistical Institute. Since 1984 he has held the title of Robert O. Lawton Distinguished Professor at Florida State University. He is now Professor Emeritus at Florida State University, after retiring in December 1992.

The following is an amalgamation of two conversations that took place in Tallahassee, Florida. The first, with M. Hollander, took place on Valentine's day, February 14, 1992, in Hollander's office. The second conversation, with M. Hollander and A. W. Marshall, took place in Duane Meeter's office on November 7, 1993, one day after Florida State University had honored Frank by hosting "Frank Proschan Festivities." Edna (Pudge) Proschan accompanied Frank.

LIFE IN NEW YORK CITY

Hollander: You grew up in New York City—what part?

Proschan: I grew up in the slums of Manhattan on 8th Street, between avenues C and D near the East River. "Drop in sometime" was our favorite quote.

Hollander: What was your family like in New York?

Proschan: Six people crowded into a one-bedroom tenement. Half the time watching the older, bigger members fighting each other, the other half the time fighting off bedbugs and cockroaches.

Hollander: Frank, would you like to tell us a little bit about your mom and dad?

Proschan: My mother and father came over in the early 1900's to escape the Russian pogroms and nastiness, I never saw my grandfather. Money was the big difficulty in our family as in most families; after all I grew up during the depression and my father lost his job in about 1930 and never again got another job. So he would walk and read historical novels. He probably knew more than anybody in the family about the United States because of his great amount of reading. It's interesting to go back and see what motivated me later on in life. My father and mother's picture was that everybody in the family, all the children, should get college degrees. That was all he asked for in this life. My

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sister was somewhat bitter at the thought that he wasn't quite so sure about a girl getting a college degree. Possibly my mother fought for that inclusion. So we all did get college degrees.

Hollander: Your father stopped working in 1930; you were nine years old. So where did the family get their resources to put every child through college?

Proschan: The resources were mostly generated by my father's constant worrying and anxiety. Actually, there was a program of home relief that we were on and a very fine program, the WPA (Works Progress Administration). The WPA put people to work in their own professions. If a person had majored in music, he would work in a program in which small orchestras would play for the public in a park or at the waterfront. There was a well-known band leader, Sammy Kaye, whose motto was: "Swing and sway with Sammy Kaye." We changed the slogan to: "Swing and sway with the WPA." Artists were put on programs of producing art and I'd say it was quite good. The government basically did the job of providing jobs for people and getting the country back into economic growth and development.

Hollander: What high school did you go to?

Proschan: I went to Townsend Harris High School that is the preparatory school for City College. Once you got into Townsend Harris and graduated satisfactorily, you didn't have to take any entrance exam to get into City College. Whereas the other 200,000 people who did not get into Townsend Harris would compete in a very difficult exam since there was only a couple of thousand vacant spots.

Hollander: How did you get interested in mathematics?

Proschan: That's a good question. Mathematics entered a practical way. I used to play dice with the kids for small amounts (nickels and dimes), so I got interested in probability. I borrowed some books so that I could determine what bets gave the best odds. In particular I remember Uspensky (Uspensky, 1937). In fact I accumulated as much as 50–60 cents, which I hid. My mother would borrow from me until the next home relief check would come in.

Also I liked mathematics best in school. There was a precision about it that appealed to my rigid mind. I always got "A" in mathematics. It turned out that I wasted a number of years working in industry doing things that were not mathematics, but they expanded my horizons. It demonstrated what I should not do and could not do.

Hollander: Did you follow up on probability at City College?

Proschan: I majored in mathematics, and in particular took an honors course in probability. I studied Uspensky, as I mentioned earlier, as the text. Every so often, I would give a lecture on what I had learned. I remember (with pain) seeing Oscar Wesler, another honor student, cringe when I said "one dice"—I may have known a little probability, but certainly not much of the English language.

Hollander: Do you remember some other noteworthy fellow students at City College?

Proschan: I certain do. Every other student was a Nobel Prize winner practically. Well, not quite. Kenneth Arrow was essentially one class ahead of me. Of course he won the Nobel Prize in economics. Well, Milton Sobel of Epstein and Sobel fame was a classmate of mine, and I remember only one phrase from him during that period. He said, "Frank, you're weird." At that time I thought that was criticism, but I see now that weirdness has served me well. Other City College students who became notable and even famous were Herb Solomon, Herman Chernoff, Ingram Olkin (the last two came along a couple of years later) and Oscar Wesler.

Hollander: In what ways were you weird?

Proschan: I think Milton Sobel was reacting to the fact that I had an imagination that was unconventional and I might exaggerate things in esoteric ways.

WORK FOR THE GOVERNMENT

Hollander: When you graduated in '41, you ended up going to a position in the federal government. How did that come about?

Proschan: At that time, and possibly it still works the same way, the government would send you a telegram saying, "Would you accept, if offered, a job at $1,440 per annum, as cement mixer grade 2?" I always was struck by the weasel-wording which in all these years has not really changed; if anything, it may have gotten worse. I always felt like responding: "Would you send me an offer if I were to accept if offered the position described?" And so it might go on infinitely, converging to the fact that you finally got the job at retirement age. They weren't offering me a position: they were just asking if I would accept it. That is the U.S. government.

Hollander: In those days, did the government and companies come by City College on recruiting trips? How did they make the initial contact? How did they get your name?

Proschan: Well, I knew that a government job was prestigious and yielded a steady income. I can't remember at this point how I happened to apply for this particular job. It certainly did not involve mathematics: what I did was mix cement at the

National Bureau of Standards and clean the sample plates and do other menial things, but I really felt good; my mind was free of this chronic concern with getting ahead. I was able to go home at 4:30 and forget my work, which is something I can't do these days.

Hollander: How did you make the transition from this type of task to working on mathematical problems? What's the chronology of employment?

Proschan: I'd better sketch it because I had about four or five different jobs. I worked in the government for roughly 11 years. My first job was with the National Bureau of Standards, cement testing. I forget the length of time but say roughly a year or so. I then applied for a better-paying job that called for a college degree. This was with the U.S. Geological Survey in Arlington, Virginia, making maps by stereoscopic methods. That is, let's say two airplanes take pictures of the same terrain, and when you put these separate pictures in two appropriate viewers, you would see in three dimensions. Not like the ordinary picture, but it would be as if you were in an airplane, viewing the terrain and then you had a little moving pencil on a stand, and you'd follow the contour so you would draw successive contours that would constitute a map, a topographic map of the area that you were interested in. Initially we were making maps of parts of the U.S., but then came the war in the Pacific, and the maps suddenly had no names. It was highly classified of course. We were mapping various islands that our troops were fighting on. I worked there for several years actually. Then I thought I'd better move on because there was no mathematics involved, nothing using what I had learned and was interested in. So I went to see Dr. Deming (W. Edwards Deming) of Quality Control fame. He was a very generous person. He spent at least an hour calling all his friends and telling them about me from my vita sheet.

Hollander: What year was this?

Proschan: This must have been 1945, perhaps, 1946. And I finally did connect with the U.S. Army Security Agency and they were involved with cryptographic, cryptoanalysis work. Very highly classified and there I actually did some mathematics and had a great deal of freedom in what I was supposed to do. We were just starting to introduce big computers. I suppose one of the table-top computers today would be equivalent to a really giant computer that took up maybe 25 or 50 feet in those days and was run by mathematicians and electrical engineers and other such specialists. I worked in the Army Security Agency for about three or four years. It was an interesting job in the government.

Hollander: Was it after World War II ended that you decided to go back and get your master's degree at George Washington?

Proschan: Yes, it was just after World War II ended. I worked during the day and went to George Washington University evenings between 1945–48 and got a master's degree in statistics in 1948. There was very few teachers of statistics. I recall taking courses with Frank Weida and the much better known Solomon Kullback, whom I ultimately worked for in the Army Security Agency. He was a statistician and head of a huge division. Then he took off his soldier clothes and became a professor at George Washington University until he retired.

Hollander: Where did you go from the Army Security Agency?

Proschan: I then worked at National Bureau of Standards. This is the second time, now, at the National Bureau of Standards. This was in 1951–52. I worked in the Statistical Engineering Lab, headed by Churchill Eisenhart. You could spend some indeterminate amount of time on research or you could consult with people on their actual problems. It would be somewhat like our Florida State University consulting lab. And I did both, though I was not well prepared.

Hollander: Could you tell us who else were at the National Bureau when you were there?

Proschan: Richard Savage was my roommate at the National Bureau of Standards. In fact, we not only worked in the same office, we also lived in the same apartment building, a beautiful apartment building that was rather high-rise for those days. We could look out the window and there was a big road running by this building and we could see the traffic cops tagging the speeders. It was a lovely apartment. I wrote to various airlines and said I was opening a restaurant and I would like some of their posters. They didn't check me out: they just sent me some posters. That was my art display. I used to entice beautiful young girls, like Pudge. Well, only Pudge.

Hollander: Who were some other people at NBS?

Proschan: We had a very nice flow of visitors. Kai Lai Chung came down for the summer. I'd be working on some theoretical problem and he'd look at it and then solve it by some clever approach. He was good company in every way.

Hollander: You mentioned your wife, Pudge. I have been meaning to ask you, how did you meet her and at what point did you meet her? She was apparently with you when you went back to study for your Ph.D. at Stanford.

Proschan: I apologize to Pudge for leaving her out of all this and just sort of assuming that she was just somehow part of the scene without mentioning her. I was very much interested in ballroom dancing, especially Latin American dancing, like rumba, mambo, tango, etcetera, and I met her in 1951 at a dance studio. She was a senior student and I was a good dancer on my own. I didn't take lessons there, but rather I would just make a big nuisance of myself by copying what the teachers were doing as they danced with the customers. They tolerated me and I became a good dancer and, in fact, Pudge and I would teach classes. For example, we taught at the Naval Officers Club in Anacostia, Washington, and there would be a break about halfway through the session. I was shocked because all the officers would rush to the bar and start drinking and the second half of the session generally would be buried somewhere under a mountain of Manhattans and martinis. Once they started drinking, they never resumed the dancing. That was just as well for us; we rested quietly.

Hollander: Were you paid to instruct them, or was this volunteer work?

Proschan: We were paid to teach them dancing. It involved a long trip out to southeast Washington, D.C., and it was right after work, but we both were very much in love with dancing. The money was not the main attraction, but the teaching of dancing was much more fun than the teaching of mathematics.

A MOVE TO SYLVANIA

Hollander: After you graduated from George Washington University, did you go directly back to the National Bureau of Standards? What was your path?

Proschan: I decided to get a Ph.D. in mathematics at GWU. Fortunately I was offered a job with Sylvania at a tempting rate of pay and so I abandoned my Ph.D. efforts. Incidentally, in the process I was teaching five courses a semester and then taking some courses in the evening for the Ph.D. Obviously that would not have been a very good program, and I am glad that my desire for money intervened.

Marshall: So in 1952 you took a job with Sylvania.

Proschan: I took a job with Sylvania in Hicksville, Long Island. I was manager of quality control. It was a strange field for me to get into. The field of quality control was mathematically and statistically interesting, but my job was to control the quality of the uranium bars which were then shipped out to the reactor. I really had no practical experience: I knew nothing about uranium, I knew nothing about testing. I did poorly and that was my saving grace because when support money was cut, a certain number of people were cut from the Sylvania payroll and I was one of them. So that was the first time I lost a job. Actually I didn't completely lose it, in that I had the option of looking around for jobs within Sylvania. They had jobs posted. Fortunately there was a job in Waltham, Massachusetts, and this was with a much more theoretical group. Their mission was to develop an antimissile missile system. Our little section was concerned with probability and physics. I remember one fellow in the section who 50 years later I met again at Boeing. He knew one thing and he used it for every problem. The two-person–zero-sum game was his specialty. For example, when thinking about this problem of devising an antimissile system, this man's model would restrict the enemy to shoot a missile at just two possible heights and that was it. Then he could derive what we should do. When I met him 50 years later, he was still solving all problems with that method. The work was more interesting in that there was a lot of theory. We had consultants of various kinds: physicists, chemists, mathematicians etcetera. Anyway, my problem was that I was too smooth and gentle with people. I ended up acting head. I was manager before that, and for a while I was just a statistician at the antimissile system study. Then the boss was let go and they appointed me acting manager and I acted for over a year. Again, I was in the wrong job because I didn't know any physics or engineering and I had these various types working for me, engineers and physicists, and I could not evaluate whether they were doing good work, bad work or nonsense.

BACK TO GRADUATE SCHOOL

Marshall: It sounds as if you were ready to make a change.

Proschan: My thinking up to that point was if you want to make money you go into industry and you rise to the top. That's true, if you have the right interests and talents, which I did not. My work was completely uninteresting and dead end.

I told my wife that I was going to go back to school to get a Ph.D. and teach and that we would never have any money because teachers obviously don't make money. It turned out that a higher principle held: If you do what you like best, you generally make more money than if you go directly for the money and ignore your own interests and talents.

So at that point, 1956, I went to Stanford University, and this led to an interesting episode. Years earlier I had taken a test given by the Atomic Energy Commission for fellowships to universities. I did very well and so I was sitting there talking to

FIG. 1. *Frank and Edna Proschan in Waltham, Massachusetts, 1954.*

Dr. Lapp, who was a nuclear physicist, and he said, "Well it's a shame you have the knowledge, the ability, but you're too old." Let's say I was 25 at that point. Ten years later I got a fellowship from NSF. I was, naturally, 10 years older. Anyway, Lincoln Moses commented to me, "Well, 10 years does make a difference." It's funny, just before going to Stanford, I was riding home from some statistical meeting with two other guys, Ph.D.'s in statistics. I was sitting in the back of the car reading Mood and they said, "What are you reading, Frank?" I said, "Mood." And they said, "What for?" I said, "Well, I'm going back to Stanford to get a Ph.D. in statistics." Then they started laughing and said, "You're reading Mood? You know that is trivial stuff." But I'd been away from statistics really for over 10 years, and I never did learn it too well at GW. But I did go to Stanford and that's where all the action took place.

Hollander: You enrolled in Stanford in 1956. What was Stanford like during the mid and late 50's?

Proschan: It was a very pleasant place to study. The countryside was beautiful and not as crowded as now. We had excellent teachers. Many or all of them were involved in important research in areas like quality control. At that time, that was a very big application of statistics and there would be conferences at Stanford involving the government and the professors doing research. You got to meet many of the top people in academia and the government. For example, Deming, who was one of the great quality controllers and now a thousand years later, still is. [Dr. Deming died at the age of 93 on December 20, 1993.]

My first year at Stanford was a year of great anxiety. (In fact, the next 40 years were even worse with respect to anxiety.) I recall taking walks in the evening and expressing my anxiety to my wife, I'd ask, "What is the worst thing that could happen?" She would say, "You could die," and I'd say, "No, that's not so bad. The worst thing is that I might fail, I might not make it," but somehow, I made it.

Hollander: Who were some of your fellow students in those years at Stanford?

Proschan: Rupert Miller and Don Guthrie were one year ahead of me. Joe Kullback, son of Solomon Kullback, was in my class. Arthur Albert, Bill Pruitt and Don Ylvisaker and people like that. I often wonder what has happened to them, although I do see their names in the journals.

I was very lucky to come across Dick Barlow working at Sylvania Electronic Defense Laboratories. He too was a doctoral student in the Statistics Department at Stanford. We followed parallel paths: we studied full time at Stanford during the academic year and then worked full time at the Sylvania Labs during the summer. Most significant of all, we both were interested in mathematical and statistical reliability theory and practice. My interest in reliability, well, some of it came from sources at work, other than Dick Barlow, but Dick was not the kind of guy who would say "Go away. Don't tell me about your problems." He was very intense, hard working, very aggressive in his mathematics. He had a sharp mind, a sharp tongue and a competitive approach, softened by a respect for his elders (me), and he had his own weird, vicious sense of humor. Dick was a great guy to work with. We apparently hit it off because within a week after meeting, we had this idea of writing a book together (Barlow and Proschan, 1965). Dick was not shy in appraising his work. He writes: "A remarkable lower bound on $1 - F(x)$ is true when F has an increasing failure rate (IFR)...." My first reaction was to remove "remarkable" as being immodest, and besides I had even more remarkable results that I described in more modest terms. But being older and wider, I left Dick's self-praise as it stood since my maturity was remarkable for my age. We had a mutual friend, Igor Bazovsky, who was an engineer we knew who wrote the first book on reliability. Igor didn't know much statistics. He'd come by when Dick Barlow and I worked together at Sylvania. And Dick had a buddy, Larry Hunter, who had a Ph.D. in mathematics and was also at Sylvania.

Hollander: So you were actually working at Sylvania while you were a graduate student. That was a little unusual for the time wasn't it?

Proschan: Yes, it was, in the sense that actually I did not work during the academic year for Sylvania, but rather only during the summer. I made much more per month at Sylvania for the three months of the summer than I made for the academic year. I was on an NSF fellowship and got a modest stipend. It was more than the stipend of a full-time fellowship student. So that first week we got to Stanford, within a week, I'd bought a house in Sunnyvale, which was a half-hour drive from Stanford. I was a conventional homeowner, had a lawn, and it was fun.

I was so ignorant of academic ways that when Professor Lincoln Moses in his usual charming way said, "Frank, I think you'd make a good teacher; how would you like to teach statistics?" I thought, here I am, I'm getting an NSF stipend. I am very busy taking these courses; why on earth would I want to teach a course? No extra money, I'm not obligated to the University in any way, I paid my tuition. I said, "No, I don't think I would like to teach." Since then, I realize what a horrendous response that must have constituted, but Lincoln Moses in his usual smooth friendly way smiled, somewhat hurt and rejected, but he never sought retribution, which is more than I can say for some of the other faculty.

Marshall: I'd like to know a little bit more about the kind of work that you did at Sylvania while you were a student.

Proschan: That was really good. I was a student most of the time and so they hardly kept track of me. During that time, an economist named Guy Black posed the problem of an optimal spare parts kit; that is, you know the cost of the part, and you know the reliability of the part and you want to make a kit of spare parts that will serve to see the system through for one year. You want to make sure you have enough spare parts of each type so that you don't run out of any crucial parts. I worked it out on a crude level.

Hollander: Both Herb Scarf and Sam Karlin played a role in your choice of dissertation topic. How did you become interested in total positivity, and how did you end up having two major professors?

Proschan: Somehow I was describing the spare parts problem to Herb Scarf. He got interested in it as an application of renewal theory, and God saw to it that it could be approached from the total positivity point of view.

Marshall: How did God tell you that?

Proschan: Well, Scarf was a buddy of Sam Karlin, and Scarf mentioned the problem and the theoretical aspects of it to Karlin. Then I developed a theorem and I remember Scarf saying: "Karlin and I think it's not true. We're working on a counterexample."

Marshall: That involved total positivity?

Proschan: Yes, and five days later Scarf said: "We think it's true. We're working out a better proof." And it was true and it was a beautiful theorem (Karlin and Proschan, 1960, Theorem 1). It seems like at every stage Scarf tried to discourage me, because here's another thing he said: "It's too bad you got started in total positivity. Karlin has been working in that field for 10 years. If there was really anything worthwhile left, he would have gotten it by now." And since that time there have been hundreds of beautiful new theorems and I'm very pleased that I ignored that warning. Karlin and I did write a joint paper on some aspects of total positivity, and it was published in the *Annals of Mathematical Statistics* (Karlin and Proschan, 1960).

As I indicated earlier, the replacement tool kit problem arose at Sylvania Labs in Mountain View. It turned out that, to solve that problem, one could use total positivity. It wasn't the other way around, that I learned total positivity and then looked for applications, which is the way an academic person generally proceeds in writing a paper. I had a real problem and I looked for tools to solve it. The basic theorem of the dissertation was one of the tools that enabled me to solve the practical problem. This reflects a deep belief of mine that the best mathematics arises out of trying to solve real problems.

WORK AT THE BOEING LABS

Hollander: So you had experience with government, you weren't about to start teaching as exemplified by your answer to Lincoln Moses and, after you got your degree, you went to back to Sylvania. How did you finally end up at the Boeing Scientific Research Laboratories in Seattle?

Proschan: Well, when I went back to Sylvania, I expected I would get a raise, having just gotten a Ph.D. That was not their picture of what you got paid for. I decided I'd look for a job elsewhere. I interviewed with only two companies: IBM in Yorktown, New York, and Boeing in Seattle, Washington. My decision was very simple. They were both quite attractive places to work and quite appealing.

FIG. 2. *Frank Proschan at Stanford upon receipt of Ford Foundation Doctoral Dissertation Award*, 1959.

I had answered one of the questions that was, "How much money do you think you should be getting per annum?" I had put down $18,000, which was a small fortune at that time, and IBM offered me $17,500. Boeing took the gallant point of view and offered me $18,500, more than I had asked for. In my world that constituted love. They didn't have to do it, but they did: there was a certain flair and gallantry about that offer that really overcame me. It was a beautiful place. Shiny aluminum and glass, large glass windows, landscaped and, best of all, a beautiful cafeteria. It looked like an ideal place to work. In fact they allowed a great deal of freedom concerning the research topics you worked on.

Hollander: That was what I was going to ask you next. I've often heard you say that the environment for research at the Boeing Labs was excellent. Would you describe it for us?

Proschan: When Dr. Burton Colvin, head of the main lab, hired me, he said, "I'm not going to tell you what to do. We need people in reliability. Now presumably you know a lot more about reliability than I do, so why should I tell you what to do? You do whatever you think is useful, important, pretty, whatever...," and that to me was unheard of. I always had the picture that when people gave you money they wanted you to do something specific for it. There was a tremendous amount of freedom present. There were some people who liked applications a great deal (I presumed they had somewhat less training and interest in research), and they would spend 95% of their time consulting with applied people from other parts of Boeing. In my own case, I liked research basically, but the beauty of the Boeing Labs was that you could spend as much of your time on research and as much of your time on consulting as you felt appropriate. So if an interesting problem that might lead to useful, new and interesting research came along, I might spent months on it with the person having the problem. On the other hand, if it was a routine textbook problem, I would get rid of the consultee in a day or two. A striking phenomenon observed was that some of the thing we worked on (Dick Barlow, Al Marshall, Jim Esary, Sam Saunders, Bill Birnbaum, Ron Pyke and so on) turned out to appear purely theoretical at the time, but then along came a problem for which we actually used the results we had discovered, perhaps six months earlier. A striking example appears in my most often quoted set of data, my Boeing 727 airplane data (Proschan, 1963). That data has been quoted in maybe 50 different papers. No one ever mentions the brilliance of my analysis, nobody ever mentions that I did anything at all with the data, all they do is quote the data. I have never felt so rejected, because I liked what I did in that paper. Aside from writing down 212 numbers, I did what I thought was nice statistical analysis, but since that time that data has been quoted and manhandled. Any student who had some kind of method for working with data would ask me if they could use this data and then they would really misuse it. I guess that's the freedom of the researcher.

Hollander: Frank, I think we are going to see that data not used so often. It now is called the "oft-used Boeing air conditioning data" and some of the referees and editors want to see new data.

Marshall: Your analysis of the air conditioner data depends upon your theoretical result that the class of decreasing failure rate distributions (DFR) is closed under mixtures. How did you discover that result?

Proschan: There was a theorem in a book by Artin (Artin, 1931; see also the translation by Butler, 1964) and he proved that a mixture of exponentials is DFR although he certainly didn't use the term decreasing failure rate, but the mathematical version of it. His proof is completely different but, to put things straight historically, he had the result.

Marshall: Is this something you stumbled on? It must have been something you suspected is true and then you found it through looking at the book?

Proschan: I don't remember, but I should tell you my style of research. It is not to read what's been done up to this point and then extend the theorem. I proceed by first principles and whatever I know or whatever seems intuitively reasonable. I made a conjecture and then I got to work on it. And in those days I had the persistence and capacity to work on one problem maybe three weeks, day and night. Like the preservation of IFR under convolution. I finally got that sitting at home with two kids running around and howling, with additional noise in the background. Did we have a piano, Pudge? Maybe it was a statistics theorem piano, but I remember sitting there and the right step popped into my mind. I'd lived with the problem for three weeks straight, day and night, and I had tried every possibility and the only one left popped into my head and I then did it in detail. But the key idea struck me at that point.

Hollander: Could you say more about Boeing?

Proschan: One indication of the scholastic nature of Boeing Labs is the fact that Dick Barlow and I wrote one book and most of a second book during my 10-year "sabbatical" at Boeing. Speaking of sabbaticals, I did spend a year at the University of California at Berkeley and a year at Stanford University, both years with full salary from Boeing. Dick and I worked it out so that we were both at the same university at the same time. In addition, Dick spent a sabbatical year at Boeing. This, of course, permitted more rapid progress on the two books and the completion of a number of research papers.

Hollander: You've been called the father of modern reliability theory and you mentioned Al Marshall, Dick Barlow, Bill Birnbaum, Sam Saunders and Jim Esary. I suppose then that they are your relatives?

Proschan: Well, if I'm the father of reliability theory, then I suppose that would make Dick Barlow the mother and that doesn't sound right. Actually, all of these people that you have mentioned had brilliant ideas and it would be a terrible mistake to attribute all these ideas to me. I must have been a friendly guy, because I got in on all these new ideas in reliability theory and was immensely interested in them. I worked with people like Dick Barlow, Al Marshall, Bill Birnbaum, Sam Saunders and Jim Esary who had very good ideas. I recognized it and tagged along. I think I'm sort of a little brother instead of a father.

Marshall: In looking over your publications, I find the idea of association of random variables. I think this concept has turned out to be extremely important. Of course it was at Boeing where that came about. Can you tell us how that originated?

Proschan: Well, I would say that Jim Esary thought up the idea (Esary, Proschan and Walkup, 1967). It's not well known at all, but Jim thought

FIG. 3. *Frank Proschan explaining air conditioning systems data to a colleague at Boeing*, 1971.

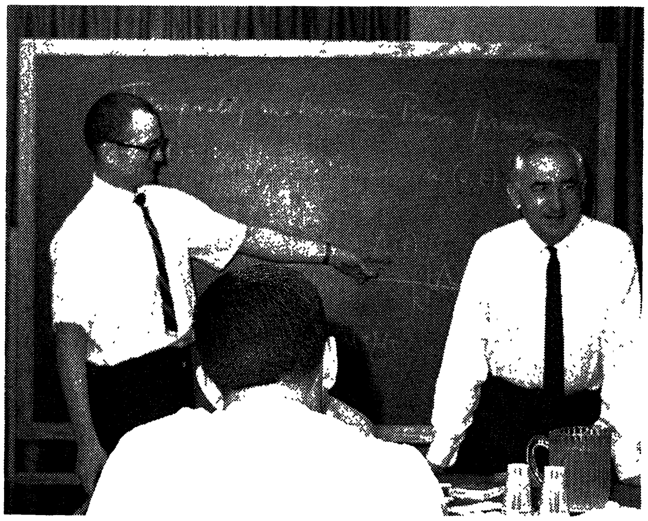

FIG. 4. *Frank Proschan teaching short courses with Benjamin Epstein, New York*, 1972.

up a number of ideas, and association is one of them. I'm almost certain that he first thought of that. I believe he had the idea behind binary systems.

Marshall: I know I certainly appreciate him, because I benefited by his collaboration too.

Proschan: It's a shame he never got the real credit.

Marshall: Not what he deserved.

Hollander: Could you tell us about the closing of Boeing?

Proschan: The closing of the Boeing Scientific Research Labs (BSRL) came about in a very simple way: BSRL had been supported (indirectly) by the U.S. Air Force. Then one day the Air Force decided to save money. How? By simply withdrawing the financial support of BSRL and other similar labs. For a while the scientists wrote proposals for specific research tasks, but the amount of money generated in this way was pitifully inadequate. The BSRL folded and the building and grounds were used for other purposes. Most of the scientists obtained academic positions. In my own case, I obtained a position in the Department of Statistics at Florida State University. The Air Force supported my research at FSU for 22 years, from 1971 until I retired. For that I'm very grateful.

ACADEMIC LIFE IN TALLAHASSEE

Hollander: Was it a big adjustment going from research and consulting at Boeing and that almost ideal environment to teaching classes, directing students and so forth?

Proschan: It was a great shock to a delicate nervous system. I think the worst aspect of academic life is the so-called democratic self-rule. I wish it were as at Boeing, where all of the decisions, which really didn't interest me much at all, were made by the head of the lab. Let me give you a small example that illustrates my conception of academic self-rule. The faculty in our statistics department was having one of its regular meetings. The topic at hand, a burning issue, was: Should the price of coffee, which at that time was 5 cents per cup, go up to 10 cents per cup? We spent one hour discussing this deep, important and vital issue. Finally, I said: "Look, the difference at the end of a year might come out to be, say, $100 or whatever. If you calculate one hour times the salaries being spent here, it comes out to much more. In fact, let me pay the difference." I don't think I made friends in those meetings. I either said something destructive or I kept quiet. The smartest of all were those who brought their pads and pencils and ostensibly took notes while actually working on their research problems.

Hollander: I just might insert here that you brought a real spirit of cooperation to the department, and you got our department doing more joint research than had been the case before you arrived.

Frank, your seminars and class lectures are always sprinkled with wit and humor, and much of it is spontaneous. How did you ever get to be so funny?

Proschan: When I was in City College, I majored in mathematics and education; that meant I was learning mathematics and how to teach mathematics. When I got up to teach and follow all the rules that they had given me, I was as tense as I could be, as boring as possible and nervous. It was a horrible hour whenever I taught, most importantly for me, but also for the students. I couldn't stand it. As I mentioned earlier, I had to teach five courses at George Washington while I was working on a Ph.D. I thought there was no way I could stand for five hours and teach, *and* do all the correct things I had learned in education courses at City College. So I threw all that to the wind and instead just sort of spontaneously decided to entertain myself and possibly entertain the students in the process. I had a wonderful time just saying things off the top of my head. I would teach the mathematics okay, but I would sprinkle it with whatever popped into my mind that would seem to me to be funny.

I remember only one time that I was beaten by my students. We sort of had a little intellectual game going, and one of my students said, "Dr. Proschan," well, I wasn't a doctor then, "Mr. Proschan, would you please step to one side. I can't quite see what you've written." In my rather childish response, I said, "Why? Can't you see through me?" And this retired naval commander said, "No, your ears aren't lined up right." I was forced to join the class laughter. There was no way I could top him.

Hollander: Richard Savage once said Frank Proschan is one person who really teaches by doing research. Care to elaborate?

Proschan: When I do the research, I really understand the essence of whatever subject I am researching and so I can explain much more clearly. It is not far from the truth that I can't understand anything of what other people do; but if I can do it myself, I generally can understand it. In fact, my tastes and evaluation of good papers has changed considerably over the years. I remember many years ago at Boeing proving a result (it is in the 1975 book). Whenever I tried to explain the proof, there was no way I could do it except by following line by line what it said. There absolutely was no intuition in the proof. On the other hand, most things that I have proved, I see a beauty in them. It *has* to work that way. So it becomes much easier to explain if you see the beauty of a new result, rather than a sequence of difficult steps for which intuition is lacking.

Hollander: I think also part of what Richard meant when he said that was that you were constantly creating new material, and you were literally teaching it to your students hot off the press, or right out of your head.

When you first came to Florida State you had a big safe in your office that you kept locked.

Proschan: Five hundred pounds.

Hollander: What was in it?

Proschan: I consulted for Jeff Krukjian, who was a civilian in the Army Materiel Command in charge of the quality of papers sent to journals. He wanted somebody who could read these papers and see if they made sense, and if they could be improved in any way. A number of them were classified. Not very high, confidential, certainly, but possibly secret. The keepers of security didn't particularly like having an isolated safe in the middle of the university, so they had it moved, proving that any enemy agent or crook could come in and move it. I doubt that there was anything worth moving, just papers I would read and make comments on. My interest often lies in clarity and clear writing and I would certainly check that. I should make the comment that most technical people do not write well at all.

Hollander: Over the many years at Florida State you would start some of those orientation colloquia emphasizing the importance of clear writing and even in your reliability course, taught out of Barlow–Proschan, you would make that point. And in your technical reports you always insisted on strong, clear motivation for the problem.

Proschan: Yes, I think it's clear cut when you get some foreign person who practically knows no English; you can see their writing is obviously very stilted. I'd say, however, that the standard of technical writing is poor in general.

Hollander: Do you want to mention some of your papers that are your favorites? I know that is a risky undertaking.

Proschan: Well, my first paper of course was based on my dissertation. Actually it was a short dissertation, compared to some of the dissertations that I have directed. It contained only about 44 pages, and I bet it wouldn't have been accepted just because it didn't have enough pages, but actually it won the Ford Foundation Doctoral Dissertation Competition in 1959. When I got the call telling me about this great award, I asked him: "What is the prize, a Ford?" "No, we do, however, publish your dissertation (Proschan, 1960) and you get the royalties." So I earned a big \$14 that way, but it was the honor that was more important. What was your question anyway? There was a research paper that I wrote with Professor Karlin (Karlin and Proschan, 1960) based on ideas of the dissertation and that paper really did have a very good idea in it because subsequently Professor Karlin went on to publish a 75-page paper in the *Transactions of the American Math Society* showing all kinds of applications in probability and related subjects based on that key theorem of the dissertation. Favorite papers—of course the Boeing data paper I think is a favorite, although it is just one. I have always liked probability and inequalities much more than statistical applications. I really wasn't very good at that. It takes a little more thought.

Hollander: Proving inequalities has always been a special pleasure of yours; why are you so good at it and how come you like it so much?

Proschan: I like inequalities because I was brought up to be unequal. My brother dominated me and inequality seems to be a major subject in the country and in my own family. Actually, I learned from Al Marshall that it was the subject of his dissertation. It is interesting that, during the first five years of our stay at Boeing, he was in the office next to mine and we never did any joint work, although we talked about many things. Then, because of more crowded conditions, he and I occupied one office. From that point on, we did a number of papers together and it was great fun and much more interesting. So, from that point on, and even somewhat earlier, while working with Dick Barlow, I found that working with fellow researchers was much more interesting, lively, stimulating and likely to give results. The whole is greater than the sum of its parts.

JOINT RESEARCH

Hollander: You seem to have a small army of friends, colleagues, and former students coming back to Tallahassee each summer to do research with you. They include Phil Boland, Emad El-Neweihi, Kumar Joag-Dev, Subhash Kochar and Yung Tong. How did this get started and how do you manage to juggle so many projects?

Proschan: This is related to the fact that I like to work with people and talk to people. I can't read papers. They are written to impress and to compress ideas. They must be very terse, and the key idea is often not clear. How exciting can a paper be if it starts off with "Let F be"? What is the key idea? That is often not stated. On the other hand, if a person has spent a year writing a paper and I sit down with him and I ask him "What is the idea here, what are you trying to do?" and then I ask him "Give me a small example; suppose we have $n = 2$." Incidentally, I found that general theorems are true even in the case where $n = 2$. This is often overlooked by researchers. They try to prove things in general and it is always very hard work. If you can prove it for $n = 2$, you may be able to prove it in general. Let's get back to the small army you mentioned. It evolved in the following way. I used to run what we affectionately called the Saturday Reliability Club. There was no credit given and I got no credit for teaching. I *didn't* teach it, but someone would give a lecture or synthesis of some paper that was of interest, either to me or to anyone in the group. The spirit of the presentation was such that if some idea struck any of us, we were free to speak about that idea and how far you can push that idea and what its ramifications might be and so on. The interest was in stimulating and getting new ideas rather than learning in rote fashion what someone else wrote. And as you can imagine, it was much more fun. We had quite a group of attendants, if you take into account there was no credit for this club meeting and it was a Saturday morning when students tend to sleep late. We still got about 15 to 20 people each week.

Hollander: Yesterday, at your festivities, I pointed out that the person with whom you wrote the most number of papers was Jayaram Sethuraman, our colleague Sethu.

Proschan: I wouldn't have believed that somehow. Is it true?

Hollander: I counted them on your résumé —don't hold me to the number right now but Sethu was the leader with about 17. I was going to ask you a general question about how you started working together. You obviously hit it off very well. One idea, in which Al Marshall also had interest, is the decreasing in transportation (DT) concept, Marshall and Olkin changed the terminology to arrangements increasing (AI). You and Sethu had early papers on that.

FIG. 5. *Frank Proschan receiving Distinguished Professor Award, Florida State University*, 1984.

Proschan: Yes. Somewhere along the way I got interested in partial orderings, and the DT or AI seemed like a beautiful, obvious partial ordering to study (Hollander, Proschan and Sethuraman, 1977), and in certain ways it's more general. It subsumes majorization; that is not well known at all. It generalizes majorization.

Hollander: Did you have statistical reasons for studying that? Or was your main interest still in the beautiful inequalities you could get?

Proschan: Yes, it was the latter. I do not recall it being statistically motivated. I think after having worked with partial orderings this one seemed so nice, so pretty, that it should be studied, and it turns out that it contains majorization.

Hollander: Well, Sethu was usually able to take some of your ideas and extend them to a much more abstract basis and more general classes, was he not?

Proschan: Yes, He's a very excellent mathematician. I speak in a general way. He definitely does not start with the spare parts problem gener-

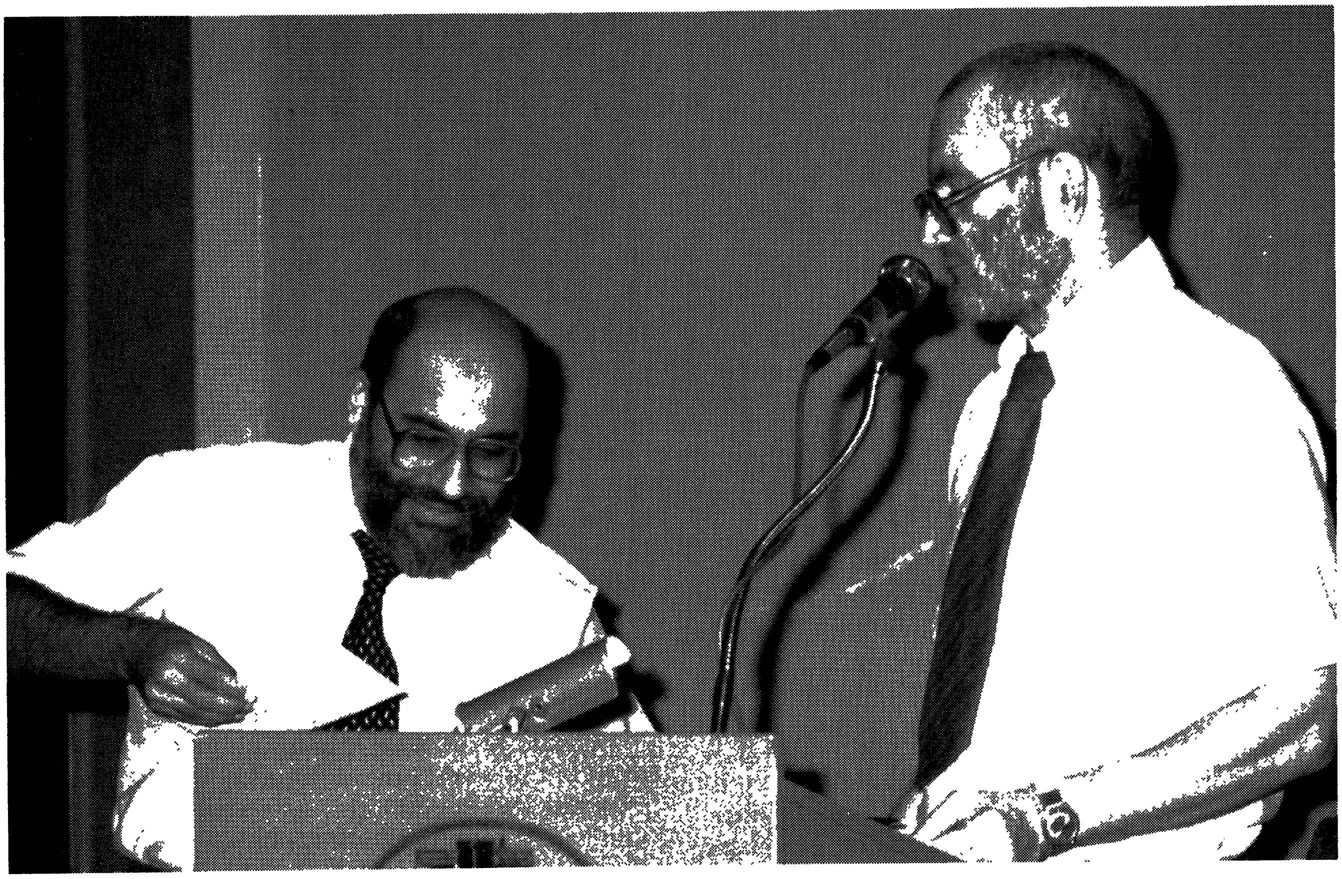

FIG. 6. *Frank Proschan and Nozer Singpurwalla at U.S. Army Research Conference Banquet, Las Cruces, New Mexico, 1984.*

ally. He starts with a theorem and shows how to generalize it. When we worked together, I would have an idea and then he'd see it in greater generality and see all the mathematical angles to it. If I wanted the best paper to emerge, I'd show him something of mine I knew could be improved and if he got interested, he'd jump in and extend it to its full generality.

Hollander: I noticed on your résumé, Frank, that you have a Bayesian paper with Nozer Singpurwalla (Proschan and Singpurwalla, 1979), and then later, in 1986, with Dick Barlow (Barlow and Proschan, 1986). The title of that latter paper doesn't give its leaning away, but it is also Bayesian. We know that Dick and Nozer are confirmed Bayesians. Have they tried to convert you?

Proschan: Well, I think that to a modest extent they have tried to but not really seriously. I think they're wise enough to know that if a person is dragged into something, he's not going to be much good. Dick and Nozer both had contracts with the governments, they had private consulting, they were in on a lot of real problems, especially Dick Barlow; and Dick saw firsthand that the engineers just did not have much data in most problems and so he was much more strongly motivated. We have to do something better than the kind of statistics that people conventionally use. There is not enough data.

Hollander: For example, in the accelerated life-testing problem where data are scarce, the Bayesian route is extremely helpful.

Proschan: Yes. In many problems the engineer knows things intuitively but can't quantify them, and the Bayes paradigm is a method of quantifying information that has its weaknesses but at least provides an answer that makes use of information that otherwise is completely ignored. So they were motivated by reality, the problems they'd run into, and it's a good way I think to develop any area of mathematics.

Marshall: How did this man, Myles Hollander, fit into the picture with so many joint papers with you. What was his role?

Proschan: Myles was a very hard working guy to begin with; very fast and very alert mind. He also knew of applications. If, say, Sethu and I did something, we might not know how to apply that lovely theorem. Myles would come back with four applications, very useful. I always told my students, "Find applications." Somebody mentioned yesterday that many of the titles of my papers contain the phrase "with applications." I think a problem of mathematical theory should start with a real problem; then develop the theory and it can then be applied to a number of real additional problems. Myles is very strong in nonparametrics that played a big role in some of the areas we treated; and he was very good on finding applications that I never could do.

Hollander: The way I always explain my interaction with Frank and why we hit it off so well is that when he first came here he was interested in reliability and I was interested in nonparametrics. He came into my office in 1971 and brought up that "new better than used" class which you, Al, had also worked on. We started doing nonparametric reliability for that class and then sort of developed a subject area that was at the intersection of our interests. We worked the boundary, and sometimes our papers were more reliability than nonparametrics and sometimes they were more nonparametrics than reliability but we always found a good common ground. Frank was tremendously easy and open to work with.

Marshall: Oh, I know all about that.

Hollander: Many of your papers dealt with shock models. For example, you had a joint paper with Jim Esary and Al Marshall (Esary, Marshall and Proschan, 1973). How did the idea of shock models originate?

Proschan: I think probably Al and Jim started that topic. I remember Al asking me a question: "Is $\{F^{n^*}(x)\}^{1/n}$ decreasing in n?" This is x on the positive line and F^{n^*} is the n-fold convolution of F with itself; and this is any distribution on the positive line. It has a fundamental interpretation in renewal theory. We were on the way from the pool in the afternoon, and I wondered, "Where in the hell did he get that question from?" I subsequently worked on it. I used the induction method. I seemed to have a three-week gestation period and then the proof was born.

We used induction on n but you have to do it in a certain way, otherwise, it's not going to work. I used to think that that was a beautiful proof; but I don't think that now because I can never remember why at this point you introduce this and later on you do that. There is no logic to it. You just have to throw a dart to figure when you expand this. I think the way I worked in those days I couldn't do it now. There is no way I could persist hour after hour trying every possibility, and this is a pretty important theorem.

Hollander: Al, in his reminiscences yesterday, pointed out an interesting aspect of the problem-solving approaches you, Al and Ingram took when working together. You had a joint paper in 1967.

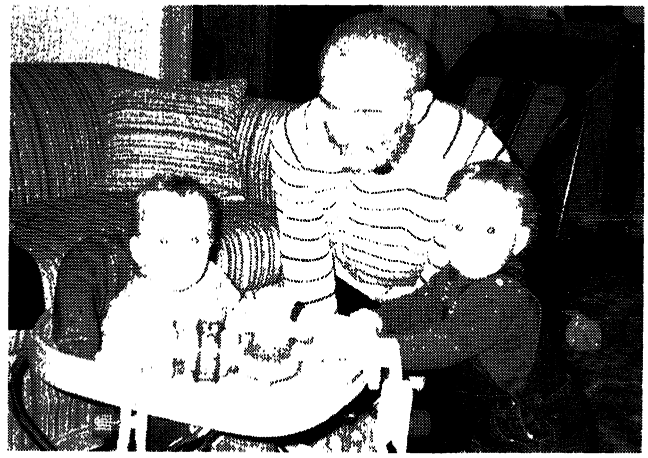

FIG. 7. *Frank Proschan between twin grandchildren, Laurel and Rachel Smoliar, Tallahassee,* 1987.

(Marshall, Olkin and Proschan, 1967). Al said that, whenever you got stuck on a problem, your inclination was to integrate, whereas Ingram's inclination was to differentiate and this led to some serious roadblocks. Do you want to comment on that?

Proschan: In that paper, Al and Ingram did most of the work, practically all of it. All I did was take one theorem, which was stated a certain way, and looked at it and I always have my total positivity glasses on. I looked at it and whenever I see a ratio, I think of total positivity. I took their theorem and generalized it so that it became a total positivity theorem that yielded the theorem that they were needing. That's about all I did.

Marshall: My memory of it is you were at least a one-third participant as we went along. And I do remember sitting there watching you and Ingram. What do you do next? Ingram said "differentiate" and you said, "Well I think we should integrate by parts."

Proschan: That I don't remember.

Marshall: Well, I do because I was so amused that I couldn't forget.

Proschan: I thought it was just made up.

Marshall: No, that wasn't made up.

BOOK WRITING

Hollander: Along with all these former students and colleagues with whom you've written papers, you've also written or coauthored five books. How does book writing contribute to your overall approach to statistics and to keeping sane?

Proschan: Well, this depends somewhat on the type of book. We had a meeting a number of years ago in which we brought together reliabilists and biostatisticians who were concerned with survival of living things. We had the brilliant idea that the two subjects couldn't be that far apart in their basic

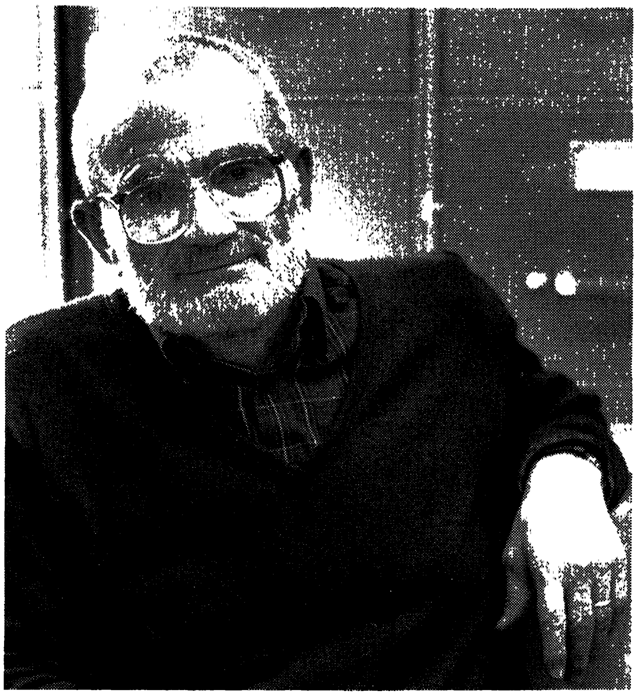

FIG. 8. *Frank Proschan in his office at Florida State University*, 1989.

ideas since they are both concerned with survival. In one case objects, engineering objects, and the other case living objects. So, a number of people came from both camps and naturally we put out a book of papers presented. That was one type of book (Proschan and Serfling, 1974). That's a rather tedious, dull task when you have to look at other people's work and see if it is right or wrong and have to get somebody to referee it.

Another type of book is the book that Dick Barlow and I wrote. Actually there were two such books in reliability but only the later one has been quoted quite a bit. All I can remember of the title is Barlow–Proschan (Barlow and Proschan, 1975). I bet it has been referenced a thousand times easily. The appeal there was to set down what was then relatively new ideas, new approaches, which explained in an organized, actually somewhat terse but clear way the basic ideas of reliability, or rather, mathematical reliability theory. There was something wrong in our relationship, Dick Barlow's and mine. Here is an example. We finished a book (Barlow and Proschan, 1965), actually the one preceding the one I just referenced, 1975. The earlier book took us 10 years to write. Ten years went by from start to finish. We had a few other things we were doing. The day we sent the book off to the publisher, Dick turns to me and says: "Well, what book do we write next?" I nearly had that point of view. Let's not stop and celebrate by having a drunken dinner tonight, with our wives, or have cocktails and celebrate these 10 years of hard work. No, we'll just sit down and dash out another one. I probably have 10 years left of life. Sounds weird but that was my compulsive approach.

Now the book I like best is the one that is completely unknown; nobody buys it, nobody uses or reads it. It shows, I'll be damned if I know what it shows! I had so much fun. Myles Hollander and I were coauthors (Hollander and Proschan, 1984). I think the title is too esoteric. I remember one editor saying, "Before you know it, they might classify this book in magic and wizardry." The title is *The Statistical Exorcist: Dispelling Statistics Anxiety*. We really had a good time writing it. It had cartoons, it had phony quotations that read just like what might be published in an ordinary novel. Of course at the end of the book, we tell which are the phony ones, because we have many real quotations too. It's, in a sense, a book of statistical examples without any of the mathematics, equations or Greek letters. It is just statistics by examples.

Hollander: It was fun writing that book, Frank, and it is not completely true that nobody liked it. There is a person at the Educational Testing Service, Howard Wainer, who wrote that we should be anointed for writing it.

Marshall: I wanted to ask about the conference on reliability and biometry and the conference volume described above. You indicate that one purpose of that conference was to try to bring those two fields together better. Why haven't they come together better than they have? Do you have any feeling for that?

Proschan: Well, clearly they have not come together very well because, this is characteristic of half of my books, very few people bought the book even. It was a rather formidable book. Yes, I think some of the basic problems are different, like reliability. But my hope is that there are enough things in common.

Marshall: No spare parts kits in biometry.

Proschan: No. Biometry does not have that particular type of motivating problem but of course they have others. My son, Michael, is in biometry. He's always visiting doctors and setting up experiments. You know there's bias and he has to straighten them out. But as far as I know he doesn't use any reliability ideas; it's strictly a class of problems that did not arise in reliability. Even though there are differences between biometry and reliability, I maintain there is enough in common we just haven't... we need a Dick Barlow to jump into it and energize people.

Hollander: Continuing my eclectic range of subjects, your son, Michael, wrote a Ph.D. under Fred Leysieffer on arrangements increasing—the chip off the old block? Did you try especially hard to get Michael and your daughter, Virginia, interested in mathematics when they were young?

Proschan: I followed a policy of hands off completely. If they needed help, I was there. I would help them, but I never told them what they should study or not study. I might tell them what I was doing in some simple way, but I never tried to push them in any direction. In fact, it worked out surprisingly well. Take Michael, for example. He started off chipping at the old block. He kept saying during his Ph.D. studies here at FSU, "I hate research, I like teaching." He was very good at teaching both individuals (while tutoring) or teaching a class. He had no opportunity to do research, but for some reason he thought he hated it. There must be something emotional relating to me in that attitude. I could not explain to him how beautiful research is. You know, you either feel it or you don't. You have to try it. So he got as far as his dissertation and then he just couldn't do anything. He suffered from depression. It is literally true that he inherited my blue genes. We sent him to our depression doctor in North Carolina. Got the right medicine for Michael. Then he went to work on his dissertation, finished it a year later and he discovered that he liked research. Now he works at the National Institutes of Health, and he just works day and night on research problems and he loves it. It is a really dramatic example of how if you let people find their own way (with help, if requested), they will choose what comes naturally and it will work out best. In the case of my daughter, Virginia (Ginnie), she liked music early in her career and is now a professional musician. I had very little influence in her choice of career. She just did what she liked and it has worked out well.

Hollander: Let's change the subject quickly. For many years you've done short courses at various places, such as George Washington University, with many people, including Dick Barlow and Nozer Singpurwalla. What are those courses like?

Proschan: It's interesting that Nozer was one of our early students in a short course. At that time he was going for a doctorate, I believe, and he was working for somebody. He took a short course and apparently he was sufficiently interested and interesting so that we had him join us, Dick Barlow and me. It was hard work for me anyway. I don't have much energy although I keep a poker face. I suffer a lot of tension and three hours would wear me down. So we could use a third member and especially since he was at George Washington he arranged with the managers in the short courses to introduce our course. That is how the three of us worked together at GW.

Hollander: Where do you think reliability theory stands today and what are its prospects?

Proschan: That is a good question. You are asking the wrong person. I have just moved away from reliability, at least in my own interests; I am interested in areas like inequalities and partial orderings. Of course, they have uses in reliability, and whenever possible I draw applications from reliability theory. But I must make a confession that I do not spend my time actively solving reliability problems. It is a bit embarrassing. People write to me and come to me, and I don't know how to tell them that I am retired from reliability and that I have begun another career in partial orderings. But really, I can get interested in anything that strikes me as pretty, especially if it starts with an intriguing actual problem.

Hollander: Frank, it's been a lot of fun for Al and me to interview you. We wish you and Pudge the best in your retirement.

Proschan: Thank you very much. I enjoyed it.

ACKNOWLEDGMENTS

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