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A Conversation with Richard Barlow

Henry W. Block

Abstract. Richard E. Barlow was one of the two founders of modern reliability theory (Frank Proschan was the other). Two of their books [Barlow and Proschan, 1965, 1975] have been influential in shaping this field. Barlow was born in 1931 in Galesburg, Illinois. He received his bachelor's degree in mathematics from Knox College in 1953, his master's in mathematics from the University of Oregon in 1955 and his Ph.D. from Stanford University in 1960. He worked at the Institute of Defense Analysis (1960–1961) and General Telephone (1961–1963) before joining the University of California, Berkeley, in the Department of Industrial Engineering and Operations Research in 1963. He was at Berkeley until his retirement in 1999 and is now Professor Emeritus. He has visited the Boeing Scientific Laboratories (1966) and Florida State University (1975–1976) as well as many other places. He has been an associate editor of most of the major statistics and operations research journals. He is a Fellow of both the American Statistical Association and the Institute of Mathematical Statistics.

In honor of his many accomplishments, Richard E. Barlow was awarded the Von Neumann Prize of the Operations Research Society in 1991 jointly with Frank Proschan.

The following conversation took place in Seattle, Washington, at the Fourth ISSAT International Conference on Reliability and Quality in Design, August 12–14, 1998.

Block: We are both present in Seattle at the Fourth International Conference of the International Society of Science and Applied Technologies. You're serving as the Honorable Chair, presumably for your many contributions in reliability. Is that correct?

Barlow: Well, yes, but also I'm here to advertise my book. That's a very important point [laughter]. In fact I brought up many, many fliers and the idea was to have my granddaughter, who is here with us, hand them out.

Block: Did she?

Barlow: Oh, she has!

EARLY LIFE AND COLLEGE

Block: Speaking about your granddaughter, reminds me…is there anything you remember specifically about your childhood?

Barlow: Well, I was interested in being a professor.

Block: From an early age?

Barlow: From high school.

Block: You were in the Seattle area?

Barlow: No, I grew up in the cornfields of Illinois, 200 miles south of Chicago.

Block: Tell me something about your father and mother.

Barlow: Well, my father died at an early age from cancer. He was a baker…he actually baked. In fact, I worked in a bakery in the summer…I made my college tuition that way.

Block: Where did you go to college?

Barlow: I went to Knox College. It was a small liberal arts college in Galesburg. They had a very good math department, although it was very small. The faculty had Ph.D.'s which at that time was very good.

Block: Yes. I guess a lot of statisticians and engineers begin their careers as mathematicians.

Barlow: I was interested in physics, but I couldn't make the experiments work. I was very good in calculus. I got a master's degree from the University of Oregon in abstract algebra. I was

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FIG. 1. *Richard Barlow and his younger brother Jerrold, 1936.*

there for two years. This was the standard two-year master's program from the Mathematics Department at the University of Oregon. And the first summer there I went to China Lake, California, and worked with an algebraist on Navy research. He encouraged me to get out of algebra and go into statistics because they were using statistical analysis and finding it very useful. I didn't work with very many statisticians; there weren't very many statistics departments then. But the algebraist had a very big influence on me; he was a very bright guy. Also, I couldn't get my master's thesis on completely simple semigroups published, so that turned me off. [Laughter]

Block: Well, I see that didn't disturb you too much because you've published a few things since then. Tell me more about China Lake. Where and what exactly was it?

Barlow: Well, its not far from Death Valley, and it was the Navy; they were testing missiles and this was in the late 1950s; it wasn't that long after World War II. The setting at China Lake was such that the statisticians were the new people who were being very useful and were much needed.

Block: Was reliability a major component of your work at that time?

Barlow: Well, there wasn't any mention of reliability, actually. It was just analyzing statistical data relative to trajectories and missiles.

STANFORD AND SYLVANIA

Block: How did you get from China Lake to Stanford?

Barlow: Well, after I decided that algebra was not the right field, I actually went to the University of Washington (Seattle). I was in the Mathematics Department at the University of Washington and was taking courses with Z. W. Birnbaum.

Block: Aha! That's the connection!

Barlow: He was a statistician, very well known, especially in the research community. And the Ph.D. program was extremely good. I met my wife there; she was also a teaching assistant and had an adjoining office. So I did get my wife there, but I didn't stay to take the Ph.D. oral exam. I was really scared of that Ph.D. oral exam. All my friends were very good topologists, but I switched to Stanford in the Statistics Department partly due to the influence of people I had known at the University of Oregon who were then in Mountain View, California, at the Sylvania Laboratories. In fact, I worked there and went to college at Stanford.

Block: Frank Proschan mentioned Mountain View in his Statistical Science conversation.

Barlow: That's where I met Frank Proschan. Which was very, very fortunate for me.

Block: In his interview in *Statistical Science*, Frank says, among other things, that within a week of meeting each other, you had the idea of writing a book on reliability together. Is that your recollection also?

Barlow: No, I think that's a little premature. What happened was that Rudy Drenick, who was working for SIAM, approached Z. W. Birnbaum relative to a monograph in reliability and for some reason Birnbaum didn't want to do this. Rudy then came to Mountain View, at the Sylvania Laboratories where Frank and I were working, and suggested that we write a monograph on reliability. Frank and I were the only statisticians working in reliability at that time who had any kind of academic credentials. Frank had probably finished his Ph.D. at Stanford and I was still working on my Ph.D. thesis.

Block: I believe he got his in 1960. Was yours a year later than his—1961?

Barlow: No, I think his was in '59 and I think mine was 1960.

Block: Stanford had many renowned faculty at that time. Who are some of the teachers who influenced you?

Barlow: Well, Karlin of course. Karlin had a whole bevy of students following him around. He had more students than any other faculty member in the department. Frank worked with Arrow (who later won the Nobel Prize), Karlin and Scarf. But he was most influenced by Karlin, because total positivity was Karlin's.

Block: That was a hot topic at that time.

Barlow: Well, Karlin made it a hot topic. He was also very much interested in inventory theory at the time. This was the era of Arrow, Karlin and Scarf [see Arrow, Karlin and Scarf, 1958].

Block: Karlin inspired you, I guess.

Barlow: Yes, definitely. That's because of the newness of the topic, mathematically extremely pretty, and because Frank was working summers at Mountain View and, after he got his Ph.D., full-time. I was actually also working full time, but on an academic leave, so to speak, because I could work on my Ph.D. while I was at Sylvania. We got interested in these problems they had, like the spare parts problem Frank worked on and other reliability problems. Anyway, the spare parts problem really made Frank famous, you might say. That was a Sylvania problem and was part of Frank's thesis. The thesis has its own algorithm and it was proved using total positivity and so forth. That's where we got the problems; they were real engineering problems.

Block: Your thesis was on total positivity or was it on. . .

Barlow: Oh, it was on repairman-type problems. It was actually queuing theory, semi-Markov processes that were new at that time, and it was a really nice subject area—getting away from the Markov exponential waiting time assumption, but still using the Markov property. . .

Block: With different lifetimes in between. . .

Barlow: Yes, conditional on the state you're in and perhaps also the state where you're going.

Block: Given your current interest in Bayesian statistics, in retrospect, was there anyone in the Stanford department who stimulated that interest when you were there.

Barlow: [Emphatically] Oh, no! They all had a great admiration for Jimmie Savage. Every statistician I know had a tremendous admiration for Jimmie Savage and his enormous intelligence. But, they were all anti-Bayesian, including Chernoff. He was talking about decision-making under uncertainty, but he was not Bayesian at all.

Block: I remember that elementary book of his that he must have written around that time [Chernoff and Moses, *Elementary Decision Theory*].

Barlow: He was writing it or had written it.

Block: Did Jimmie Savage visit Stanford while you were there?

Barlow: Oh, I think he did, but I didn't get to know him. I was like all the rest of the statisticians who laughed at the Bayesians. . .it was a joke.

Block: After you graduated, you said you went back to Sylvania for a while. Is that correct?

IDA AND GENERAL TELEPHONE

Barlow: Well, yes, but Karlin arranged a year for me at the Institute of Defense Analysis (IDA).

Block: In Princeton?

Barlow: In Princeton, yes, and a lot of academic people had been there, including David Blackwell. When I got there I found that all the very big names in algebra had been there.

Block: And you knew the names in algebra. . .

Barlow: . . .and I knew the names in algebra. I went there sort of on a post-doc for a year. It was supersecret. I had no idea what it was about, except that I could do anything I wanted to do, you know. I had no idea until I got there that it was on code-breaking.

Block: So you worked at code-breaking?

Barlow: No, I didn't work on code-breaking and they were mad. Al Marshall was there and we worked together that year.

Block: Is that when you started some of your work on reliability bounds?

Barlow: One result was one of the first increasing failure rate (IFR) papers [Barlow, Marshall and Proschan, 1963]. It contained Frank's proof of the IFR convolution result, which was known, but his proof was new.

Block: He was quite proud of that proof; I remember him talking about it.

Barlow: Well, the fact that he could use total positivity in the proof was the main thing.

Block: Sure, use your thesis and apply it to everything that you can! What year was that, that you were at the Institute of Defense Analysis?

Barlow: 1960–61.

Block: And after that?

Barlow: Well, I had an offer from Marvin Zelen to go to the University of Maryland. I was all set to join the statistics group in the Department of Mathematics when suddenly at the last minute Marvin left. One of the reasons was that a relative of his died of cancer and he decided he was going into medicine, clinical trials, and later he went to Harvard.

Block: That sounds like a good decision, but I remember he went to Buffalo for a few years.

Barlow: That's true. Yes, he was at Buffalo with Manny Parzen, before he went to Harvard.

Block: But Zelen worked in reliability before that?

Barlow: He worked in reliability at the National Bureau of Standards, when Frank was there, which was ten years before my time. Frank is ten years older than I.

Block: So you didn't go to Maryland.

Barlow: No, because at the last minute Marvin left. That was the only reason. I had another friend at GT&E Laboratories, General Telephone and Electric, (which was actually General Telephone at that time), who was setting up a laboratory at Menlo Park that was going to rival Bell Labs.

Block: This is near Stanford again?

Barlow: Very near Stanford. Up on the hill, they built a beautiful building. Larry Hunter, who I knew at Sylvania, made me a big offer, a lot of money, to come back to this laboratory that they were starting to set up. Of course, California sounded a lot better than Maryland. We had rented our house, actually, in Santa Clara and came back. That's where the research on reliability bounds started. I do claim credit for the IFR bound. Anyway, that's where it was discovered. Larry Hunter made one suggestion that made it all work.

Block: You did a number of papers with him.

Barlow: When we were there, I did papers with Larry Hunter.

Block: Was that on replacement theory?

Barlow: No, I think that topic was probably done more with Frank Proschan. I had a number of papers with Al Marshall, who had gone back to Boeing Labs. Frank was at Boeing, and I continued working with Al. We wrote a number of papers on bounds. Al was an inequalities man, even before Frank became so versed in inequalities.

Block: It seems that Al has always been interested in inequalities.

Barlow: Not always, but more so in the last 20 years, I guess.

THE MOVE TO BERKELEY

Block: So how did Berkeley come into the picture?

Barlow: Well, I really owe that to Frank Proschan. We had the book [Barlow and Proschan, 1965] that was about to be published and Frank was very well known. People at Berkeley, actually the College of Engineering, in 1963, were looking for someone in the area of reliability theory. This was a red-hot topic and nobody at Berkeley in Engineering was working in the area of reliability. You know, most people in statistics wouldn't touch reliability with a ten-foot pole (no, really!), but there were so many nice problems in that area, and they were statistical problems, and if you could just let yourself go and forget about talking to engineers, which I now realize is not a bad idea, you had a lot of opportunities for new research. The Industrial Engineering Department was converting to Operations Research and they had brought in George Dantzig and he was an extremely big name, as you know. Dantzig, of course, wanted a discrete optimization person. The Department had this slot, and Dantzig wanted a person working in integer programming. And the head of the department, Shepard, and some of the faculty, well, they wanted somebody in the reliability area and the dean wanted somebody interested in reliability. They didn't have anybody working in reliability. The rockets were failing and Engineering really was interested in the reliability. So I was given the offer. I think I was probably the only person with a fairly strong academic background in reliability they considered. They were interested in me because of the book. This was 1963 and the book was on the way It was published in '65, but it was probably finished in '64. We had a lot of material. That's what they liked, because it was a path-breaking book, because no respectable mathematical statistician would work in this area. [laughing] So, we had it to ourselves.

Block: So it was because Frank Proschan was well known and he had written the book and papers with you that Berkeley was interested in you.

Barlow: Well, he suggested me. They were looking for an assistant professor. Frank was ten years older than me (you know he went back to get his Ph.D. at Stanford after a career on the East Coast) and he had a high salary, much higher than any of the rest of us. So he couldn't possibly consider an assistant professorship, even at Berkeley.

Block: What was your first salary there? It was probably under $10,000 wasn't it?

Barlow: It must have been less. I was getting $12,000 at GT&E, which was a lot of money, so it must have been $8,000.

Block: Did you have your connection with the Statistics Department at Berkeley from the start or did that come later?

Barlow: No, that came later. It was Peter Bickel, Kjell Doksum and Betty Scott who arranged that.

Block: Did Boeing try to get you to come up as a permanent member?

Barlow: Oh yeah, I had an offer for a permanent position at Boeing and it was absolutely perfect. They had people who would translate, just for you, any Russian paper, any paper in another language; it was just perfect. And that was the reason I didn't take it. [laughter] And I was right, because the Boeing Labs fell apart later [when the Air Force withdrew its support].

Block: Do you want to elaborate on why you didn't take the offer, if it was so perfect? It sounds like it was an ideal opportunity.

Barlow: Well, of course at Berkeley I was the majordomo in reliability.

Block: Big fish in a small pond?

Barlow: Berkeley wasn't too small.

Block: That's true, but as far as reliability goes. . .

Barlow: No, but the students, too. You do a lot more research when you have students that have to get the Ph.D.

Block: I know you've always been at Berkeley except for your sabbaticals.

Barlow: Yeah, I took a lot of sabbaticals and I took an industrial leave for one year (1966) at Boeing Labs.

Block: Frank's recollection was that you did some of that 1965 book at Boeing, but it sounds like you were finished before that. He also said you started on the '75 book [Barlow and Proschan, 1975] there—at Boeing.

Barlow: That's right. This was at a time of increasing failure rate average (IFRA). You know about the convolution of IFRA distributions. That was a big unsolved problem until you solved it. Well, anyway, I was a consultant for Boeing because I was working on the book. I was a consultant going back and forth between Berkeley and Boeing.

BOEING

Block: You mentioned spending a year on sabbatical at Boeing. Could you tell me a little bit about Boeing and about the collaborations and about the atmosphere, and just about the place in general.

Barlow: Well, Boeing Scientific Laboratories was organized on the lines of a university. They had departments. They had a mathematics department. They had a computer science professional within the mathematics department at that time. They had chemical engineering and physics. They had all of the sciences that the employees might be interested in. But it was really being supported by the Air Force, by Air Force overhead, and that was why it eventually died. It was just beautiful because they had a wonderful library and Xeroxing was of course free. At that time, that was something. . .

Block: [laughing] That was a big deal.

Barlow: Yeah, and they had the latest IBM typewriters with the ball for mathematical typing. That was just coming in.

Block: Yeah, so you could switch it and get the mathematical symbols.

Barlow: Yeah. Then you could really do great mathematics papers. But the research was excellent. Birnbaum was a regular consultant. Also, Ronald Pyke was a very good consultant.

Block: Al Marshall was there, Jim Esary. . .

Barlow: Al Marshall was there full-time, Frank Proschan was there full-time. Al and Frank had adjoining offices close to the cafeteria, where we would go at any time and have coffee and continuing talking. George Marsaglia was also there.

Block: He was later at Florida State. He probably was there when you visited.

Barlow: Oh, he was originally at Boeing, and the reason he was at Florida State was because Frank knew him at Boeing. The laboratory at Boeing sat up on a hill and was all glass. It was like the Parthenon of mathematics, and occasionally you would go down and talk to the people across the Duwamish.

Block: The Duwamish?

Barlow: Well there was a river, a sort of a polluted stream called the Duwamish Waterway.

Block: [laughing] Oh, I see, and the applied people were on one side and the theoretical were on the other, on the hill.

Barlow: [laughing] On sthe hill—and they hated us [laughing].

Block: But they did come to talk to you, didn't they?

Barlow: Well, when they would have a problem, they would come and talk, and of course there was a lot of effort by the Mathematics Department head to get us to talk to these people—to make us look good in the Lab.

Block: What were some of the other things you were working on during the Boeing days? Do you remember what other topics were of interest to you when you were writing the first and second book? You mentioned IFRA—you were interested in that and probability bounds for IFRA distributions.

Barlow: As you know, Birnbaum, Esary and Saunders came out with the first IFRA paper on the closure under the composition of coherent structures of IFRA distributions, which was beautiful. Well, Marshall, of course, contributed to that.

Block: That is a very nice result.

Barlow: I worked on a lot of papers on inequalities for IFRA and IFR distributions. Frank and I did several papers on inequalities, published by the Institute of Mathematical Statistics. These papers were very helpful in my getting promoted at Berkeley later.

COLLABORATION WITH PROSCHAN

Block: Could you tell us a little bit about your collaboration with Frank. Was that an easy collaboration?

Barlow: It was very easy. I think it was because we were sort of opposites in some ways, and very similar in others. Because we're sort of loner types.

Block: I wouldn't guess that looking at your vitas.

Barlow: Frank, when he was at Florida State, he would get up at four o'clock. He would always work very early in the morning because...

Block: ... personal loners, rather than research loners, is that what you're talking about?

Barlow: Oh, yeah, that's what I meant. Oh, no, no, Frank wanted to work with people. He collaborated on most of these papers. And it's a good idea because you get a lot of stimulation that way. But the association was usually we got to work on problems that I got interested in, not always, but usually I was interested first, and I was always very, very enthusiastic and pushing and not very careful [laughter]. So a lot of my proofs weren't very good. He was very careful, very careful—total positivity and inequalities. I think I was the excited one. I really liked research.

Block: Well that comes through in his *Statistical Science* conversation. He describes you, I think, in that way.

Barlow: It was always my idea that you always worked with people that were better than you are, or you think they're better than you are [laughter]. They usually are. And you work with them and you associate with them.

Block: And it's satisfying having a relationship like that?

Barlow: Oh, we always got along very well.

Block: When did Frank leave Boeing?

Barlow: Frank went to Stanford after the collapse of the Boeing Labs. Now the collapse was what—'70?

Block: 1970, roughly.

Barlow: That's right. Okay, well we were both together at Stanford. I was on sabbatical for the year and I was visiting Statistics and he was visiting the Operations Research Group, which was fairly new. Frank was visiting Operations Research because they offered him facilities. I don't know if they were paying him a salary. But he was on leave from Boeing. I was occupying Chernoff's office, which was very nice, while Chernoff was on leave.

Block: Jerry Lieberman was there already, wasn't he.

Barlow: He started O.R.

Block: That's probably one of the reasons why Frank went there. What year was that approximately?

Barlow: It was 1969–70.

Block: The year after Frank went to Florida State. If I remember correctly, they tried to woo you also at Florida State, didn't they. They tried to get you to come there permanently.

Barlow: That's right.

Block: And that didn't work out either.

Barlow: Well, my wife wouldn't accept...

Block: She liked the West Coast?

Barlow: Well, we were in Florida long enough (1975–1976) to realize that we didn't want to live in Florida. The elementary school system and the high school system (unless you were at the university high school and elementary school) were not what we wanted. California had a very good system at that time. The elementary school system and the high school system and the whole university system was, at one point, an excellent school system.

Block: Not so today?

Barlow: Well the university system is still good because it has so many parts to it.

Block: Just a little side note. There is a famous or infamous dedication to your 1975 book with Proschan [see Figure 1]. Do you remember anything about that, or how that came about?

Barlow: This was 1975, Frank was at Florida State and I was at Berkeley. I also thought the book dedication was funny. He spent a day at least writing it. He was really proud of it. It was a very long dedication. Most of it had to be expurgated. The dedication you see there is not the full dedication at all.

Block: Does the full dedication still exist somewhere?

Barlow: [Laughter] I hope not. Well, it was not good for me because my wife didn't appreciate it. Pudge [Frank's wife] didn't mind. But my wife just didn't understand it. There was some truth in it, you see, and that's not good, in a dedication [laughter]. It was Frank's work.

Block: What about the publishers; what did they think about it?

Barlow: They weren't sure about the dedication. They cut out a paragraph to tone it down, but they finally let it go through.

Block: They even put it in an unusual place. They stuck it on the back of the title page in the first printing and the back of the preface page in the second printing.

Barlow: I think probably they weren't too happy with it.

Block: I remember laughing about that at the time.

Barlow: I remember that other people told me that I was going to regret this.

Block: Well, do you regret it?

Barlow: Oh, I don't mind; it's my wife who regretted it [laughter].

DEDICATION

To our wives Barbara and Edna, without whose whining nagging for more money we might long ago have abandoned this painful project;

To our numerous children, whose incessant bickering and generally atrocious behavior drove us to spend many long hours at the office, working on the book as the lesser of evils;

To our students, who, with malicious glee, found the many errors in earlier versions and did the dirty work of indexing, checking references, and so on, knowing that a degree and a decent recommendation would have been impossible otherwise;

To our many colleagues who were generous with suggestions—but who asked them? Any failings or errors now present in the book are undoubtedly the result of their unsolicited advice and meddlesome tampering. We accept absolutely no responsibility for errors in this book;

*And finaly, to our tipysts, whose dedic ated efforts, care^{ful} attentiun to de&ail, and skillful w*rk transmuted a rougf ilegible surawl int° a finished boo$.*

FIG. 2. *Dedication to Statistical Theory of Reliability and Life Testing.*

ISOTONIC REGRESSION

Block: Another topic we should touch on is isotonic regression. That culminated in the book with the three other authors whose name begin with B: namely, Bartholomew, Bremner and Brunk. That book [B., B., B. and B., 1972] was referred to as the "four B's". Did your interest in isotonic regression come about because of an application in reliability, or how did it come about?

Barlow: No. It was more theoretical. I contacted Dan Brunk, who was at the University of Missouri and had done a lot of the early work on isotonic regression. For thirty some years I had many contracts, usually simultaneously with the Army, Navy and Air Force. So I had research money and I brought Dan Brunk to visit me at Berkeley at least twice during the summer. And the first summer he came we had a graduate seminar on isotonic regression. People in the Statistics Department, of course, also came. I think the lectures were actually in Etcheverry Hall [where the Department of Industrial Engineering and Operations Research is located]. Brunk was writing this book with Bartholomew and Bremner during this time. I think before this first visit. I'm not quite sure.

Block: Late '60s?

Barlow: No, no, no. We're talking about '70s now. He had visited David Lindley at Aberystwyth, Wales, and he was very much influenced by Lindley. But he still had to finish writing up this book with Bartholomew and Bremner on isotonic regression (which is maximum likelihood estimation under order restrictions) and it was not Bayesian at all. So he had philosophically lost interest, at least by the second time that he visited Berkeley. And I had done work with van Zwet on isotonic regression and IFR and he knew about this work. He wanted to put all this IFR and isotonic regression business in the book. But he really was looking for somebody else who would take up the slack because he'd lost interest, and I was not a Bayesian, this was early '75, and I did not convert until '76. So because I wasn't a Bayesian, it didn't bother me to do isotonic regression, and I gladly accepted the offer to write a couple of chapters incorporating the material that I had worked on. And that's how the four B's came about. It wasn't because Bartholomew and Bremner thought it was a good idea. Brunk had lost interest, but he was under contract to the Air Force and they were pushing him. He had to finish this book.

Block: Didn't you do some work with Doksum? Didn't you apply this work to some reliability problems?

Barlow: Yes, well, Doksum was, of course, very well versed in Le Cam's ideas and theory, and we applied that theory to some of these papers. What was Le Cam's idea? It had a nice name—contiguity. It was a hot topic and was the thing to do. Anyway, it was Doksum's contribution and I was interested in the subject then. Well, I usually have the original ideas and some of the original results, and the paper gets really mathematized in rewrites with the addition of the coworker.

Block: You tend to like the ideas.

Barlow: Yes, I'm more interested in ideas.

Block: Is this about the time that you affiliated with the Statistics Department?

Barlow: Yes.

Block: Was there anybody else in the Statistics Department that you collaborated with at that time besides Doksum? Did you have any papers with Peter Bickel?

Barlow: No, I had no papers with Bickel. Doksum was more interested in applications, at least engineering applications. Most of the statistics people who indicate interest in applications are talking about biometry and medicine and biology; engineering is not a field that most statisticians are interested in.

Block: There's a whole *Technometrics* journal and there's a lot of engineering statistics there.

Barlow: Well, there wasn't always. No, there are other places where there were statisticians interested in engineering statistics, but not at Stanford or Berkeley.

FAULT-TREE ANALYSIS

Block: Well shortly after this, 1974, was the time I first met you. You held a conference at Berkeley on reliability and fault-tree analysis. How did this interest in fault-tree analyses develop? Was it associated at all with the Lawrence Livermore Laboratories?

Barlow: Yes, it certainly was. There was a graduate student named Howard Lambert from the Nuclear Engineering Department (just down the hall from us) who worked on his Ph.D. thesis with me (jointly with some nuclear engineering faculty) and he was taking this course at Livermore with the originators of fault-tree analysis. He was taking notes, rewriting them, and I was learning fault-tree analysis with him and applying it. There were lots of applications of fault-tree analysis, especially in nuclear engineering. One of the applications involved LNG (liquefied natural gas) tankers coming into the Boston Harbor, where a collision with such a tanker resulting in the release of gas and possible resulting fireball could be catastrophic for Harvard and MIT among other places as well as the general Boston area. We actually went to the Boston area, went out on the ferryboat and watched the tankers come in. Essentially I learned fault-tree analysis from Lambert. He was an engineer. I put in the mathematics, algebra, probability and physics. It was upscale, mathematically. This

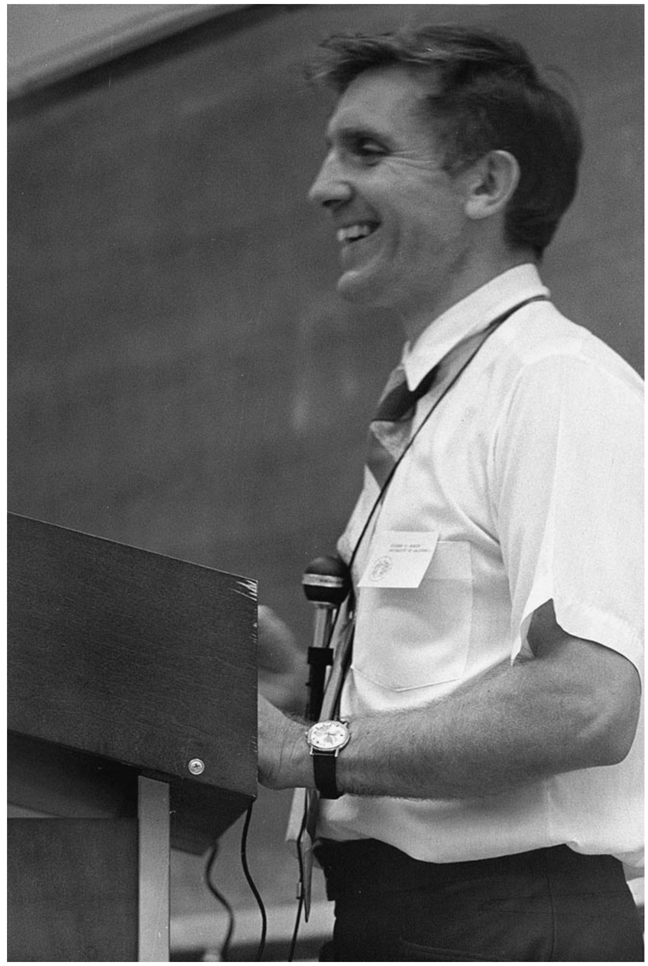

FIG. 3. *Richard Barlow at his conference in Berkeley, 1974.*

was the beginning of the fault-tree era. There was another fellow by the name of Jerry Fussell.

Block: Coauthor of your conference proceedings [Barlow, Fussell and Singpurwalla, 1975].

Barlow: That's right. The reason he was coauthor of the conference proceedings was because he had written a Ph.D. thesis and had come up with some neat algorithms relative to Boolean cuts. They were very simple, actually, but very nice, and exactly what you would need to work into computer programs. Because fault-trees, when they were developed, at least by nuclear engineers, are so large and so complex that you need a computer program and you need a methodology for analyzing the trees. One of the first programs for finding the minimum cut sets is called MOCUS [see page 5 of Barlow et al., 1975, for a discussion]. Fussell and Vesely were the originators of that. I brought Fussell to Berkeley because he had done some interesting work and then we decided to have this fault-tree conference. This initiated more work for him. I don't think Frank Proschan ever liked fault-tree analysis. Nozer Singpurwalla never liked fault-tree analysis. In order to get interested, you probably have to be in a college of engineering and working with engineers.

Block: Isn't Nozer in a college of engineering?

Barlow: Well he's in the College of Engineering, that's true. But he's also close to the Statistics Department.

Block: Weren't you and Nozer together at Florida State during one sabbatical?

Barlow: I was there for a year on sabbatical and Singpurwalla was there for half a year, the second half. While there, I was taking a course throughout the year from D. Basu, and when Singpurwalla arrived, he joined me in taking the course. So he was essentially converted [to Bayesianism], but it took him about another year, because he had other work to finish up, I guess. He was very much influenced by Basu. It was also during this sabbatical that I took my youngest daughter to Calcutta, India.

Block: This was a professional visit also?

Barlow: Yes, there was an institute outside of Calcutta, the Institute of Management Science, and the Indian Statistical Institute within Calcutta, I gave a talk there, but I was being paid by Management Science and that was 1976.

FIG. 4. *Richard Barlow and D. Basu, date unknown.*

CONVERSION TO BAYESIANISM

Block: And 1976 was the year of your conversion?

Barlow: That was the year of the conversion. Now, it was not a conversion in the normal sense because Basu himself never really knew, in my opinion, how to use Bayesian statistics. All he knew was that there was no foundation for classical statistics. So he was teaching this course, a first graduate course in statistics, where Lehmann's book, *Testing Statistical Hypotheses*, was the text, but he never opened it! All he did was to go through this material with counterexamples. This went on for a year, two semesters. A lot of counterexamples! And he is a tremendous mathematician. He got his Ph.D. in logic. But he got interested in statistics because he got a job at ISI. The jobs were in statistics, not in logic. Sir Ronald Fisher, at ISI, gave many lectures there. Basu was a young man in the audience at the time and kept asking him questions because he didn't understand Fisher's exact test and kept asking embarrassing questions. After a while, Fisher said, "It's because I say so." Almost literally. Basu began to question these things. He spent maybe ten years trying to put a foundation under it. Basu had more or less converted by the time he got to Florida State. In fact, he'd written a wonderful paper on sufficiency—a beautiful paper. It appeared in the Indian journal, *Sankhyā*, and it was a very long paper and had a lot of discussion. He would also lecture on it.

Block: It sounds like that was a very good year for you.

Barlow: Oh, it was very exciting. But then, of course, you don't know what to do, once you decide there's something wrong with classical statistics. You don't know how to do research. The first thing I did when I got back to Berkeley was to contact Dennis Lindley, and because I had lots of research money, I was able to bring Lindley to Berkeley on quite a few occasions. He came and gave courses and lectures. Oh, I learned a lot from the horse's mouth, about how you think from the Bayesian point of view. But this was formal Bayesian statistics, not operational Bayesian statistics. The formal Bayesian thinks the likelihood exists that the normal distribution is there, and then he'll operate on it with his subjective prior probability and then integrate it out. That doesn't make any sense.

FIG. 5. *Richard Barlow, Berkeley, 1985.*

Block: It was shortly after this, I think, that you developed an interest in network reliability. Could you tell me a little bit about that?

Barlow: Well, the fault-tree era corresponds to applications in nuclear power reactor safety. That was followed by the era of the computer networks, and the first computer networks were very small, actually. This problem of reliability of computer networks was very, very fashionable in the early 1980s, and why did I become interested in the subject in the first place? Well our department teaches networks, network analysis, and our students are interested in networks. It was partly because our graduate students, whom I was supervising and supporting, had all this background and interest in networks that I became interested in network reliability max-flow algorithms. Of course, I was interested in network reliability. So I had several students working and one of them actually developed a computer program, FTAP, which became very popular among nuclear engineers.

Block: Satyanarayana?

Barlow: No, no, he was always more theoretical. He was in graph theory.

Block: Who was the student you were referring to?

Barlow: Randall Willie.

Block: Randall Willie. I remember him from my visit to Berkeley.

Barlow: An excellent student. Well, he spent a lot of time developing this computer program called FTAP which is a fault-tree analysis program, which became available through the Civil Engineering Department at Berkeley. It was used into the 1980s. All it did was to find minimal cuts and analyze trees to determine whether the dual tree or the primal tree was easiest. There's no probability involved in that particular program. But there was probability in Randall Willie's thesis. Anyway when I was working with these students on network reliability, I found out about Satya [Satyanaryana] through Ralph Evans. Ralph Evans said he had this paper from Satya and according to Satya, this was the best thing since sliced bread, and Ralph wanted to know if this was true. So he sent me the paper.

LAWRENCE LIVERMORE LABORATORIES

Block: You mentioned the Lawrence Livermore Laboratories; I think that you had a long relation with them.

Barlow: Yeah, thirty years.

Block: Still do?

Barlow: No, actually this is the first year I don't have a contract. I just got tired of going out there.

Block: Thirty years is thirty years. [laughter]

Barlow: It was over thirty years.

Block: What type of things did you do at Lawrence Livermore? I guess some of them were classified, weren't they?

Barlow: Very classified, very highly classified at the beginning—atomic weapons. But more recently I was working on problems with Nora Smiriga.

Block: And what were you doing with her?

Barlow: Well, she was the head of the Computer Science Laboratory and Mathematics for the Labs. And we were working on a lot of different problems. One problem involved ranking research proposals. This was a very common problem [laughter]. So the physicists and the mathematicians would submit proposals. These are inhouse research proposals and they were looking for some method for ranking them because they only had so much money. So we spent a lot of time working out ranking procedures. The trouble with the Bayesian approach to ranking is the Arrow impossibility theorem, meaning that you can't do it [laughter]. Well, there are mechanical ways, but you have to get around that result.

Block: So you finally came up with some method?

Barlow: Oh yeah, we had a method, and it's very close to what the physicists had thought of too, so they accepted it. Otherwise, they wouldn't have.

Block: You were working with mostly physicists there?

Barlow: No, Nora got her Ph.D. with Le Cam in mathematical statistics, so I was working with statisticians. I was also working with metallurgists and engineers who were working on kevlar and materials like this. We did a lot of work on accelerated life testing.

Block: Did you work on accelerated degradation at all?

Barlow: Well, it was certainly degradation because they had these nylon strips and they were hanging them and they were waiting for them to fail. When you have a very high stress, then you get almost immediate failure—very fast failure. But if you have very low weights, it takes a long time. During that time there are apparently chemical reactions that occur, so that when they break after several years, it happens all over the room. But if it breaks only after a few minutes under very high stress, it is a clean break. So low stress leads to a completely different failure mechanism.

Block: Did you work with a small number of weights, or were you looking at calibrated weights?

Barlow: Oh they were very calibrated.

Block: You were looking at the individual lifetimes rather than some type of degradation curve.

Barlow: Oh yeah, we were looking at the individual lifetimes, but at different weights. This went on for three or four years at least, and then there was a small earthquake and we stopped the experiment because they all broke [laughter]. That was the end of it, but this was a multimillion dollar experiment that they were doing because they were worried about pressure vessels, pressure vessels that they use on shuttles, kevlar wound around two hemispheres. Kevlar is very strong.

Block: Speaking about multimillion dollar operations, I remember that there was a representative of the Air Force, it might have been a colonel or something, who came to Pittsburgh for a multivariate conference and he talked about the fact that you had saved the Air Force multimillions of dollars through a plan. Do you remember something like that? Could you tell us about that?

Barlow: Well, this was really Bill Jewell with whom I had a joint Air Force Contract. The Air Force had some kind of arrangement in the contracts that they made with industry that if they showed improvement, reliability growth, then there was some kind of bonus arrangement. This was spurious—the improvement wasn't there. That saved the Air Force money because then they didn't have to pay the bonuses.

Block: So this probably saved them more than the money they paid you.

Barlow: That was good for quite a few years of contract renewal [laughter]. Seriously!

Block: You developed over the years a connection with a group of Scandinavian statisticians and engineers. How did this come about? What was the initial Scandinavian connection?

Barlow: Well, they invited me to visit; originally it was Bo Bergmann who invited me to the SAAB Airplane Plant in Linköping, Sweden. There was a friend of his who was working in the U.S. Defense Department and they were both very much interested in fault-tree analysis. And the reason Bergmann brought me over was to give a lecture on fault-tree analysis.

FIG. 6. *Nozer Singpurwalla, Henry Block, Richard Barlow at de Finetti Conference, Rome, 1981.*

Block: So this was the middle 70s, roughly?

Barlow: Right.

Block: And you maintained the connection with them?

Barlow: Oh, sure. I've given several lectures at meetings that were held in Linköping and Stockholm. Now what's happening is that the Scandinavians have gone more towards quality control and management-oriented problems. They're not so reliability oriented.

DE FINETTI AND ITALY

Block: Speaking of Europe, I remember that in 1981 we met at a conference in honor of de Finetti in Rome. At that time you demonstrated great interest in de Finetti and his teachings and this interest seems to remain to the present day. Could you talk about that a little bit? About de Finetti, his influence on you.

Barlow: His two volumes on the theory of probability are almost unreadable, but they were really written for frequentist statisticians, to convince them that they were wrong. And my interest in de Finetti came about because of Dev Basu and my interest in trying to figure out what the criticisms were about Bayesian statistics. So it was sort of a natural thing. When you get interested in Bayesian statistics in any depth, then you pick up de Finetti and start reading his papers and so forth. Well, I went to the conference and I was actually giving talks at ENA, which is like the Atomic Energy Commission, and which is located outside Rome. The de Finetti Conference was in Rome, and I did get his autograph on his book at that time. In subsequent years I went back and visited Fabio [Spizzichino] in Rome and we went to de Finetti's office. De Finetti had died in ‘85, so this was in ‘87.

Now de Finetti was a very interesting person. A lot of people didn't like him. His friends were mainly economists and people in government, but he was in the Mathematics Department and he was a professor of the calculus of probability. But he had this sort of an operations research center off campus that we visited, at least a year after he died, and it was exactly as he had left it. The Italians are very slow in changing anything, and so we picked up a lot of reprints there. And his library is now in a branch of the University of Rome, "Tor Vergata," which is outside Rome. (Now there are three branches and La Sapienza is the main one). So, anyway, I spent some time looking at papers in his library in the University of Rome. As a result I wrote an article about de Finetti for the Johnson and Kotz encyclopedia. I had a Ph.D. student by the name of Sergio Wexler who was in the Berkeley Department of Statistics. He did his thesis with me, and it was essentially on de Finetti and his philosophy. So, I don't know what else to say about de Finetti. You know, he was like a god in the universe of Bayesians.

Block: You mentioned visiting Fabio Sprizzichino.

Barlow: Fabio was a student of de Finetti.

Block: And you've since done some joint research with Fabio, I believe.

Barlow: Oh yes. Well, Fabio was very interested in Schur concavity and when he saw an application that I had in Schur concavity, with Max Mendel, he went on with that idea and he wrote a lot of papers. But before that, Fabio was always sort of interested in applications, reliability applications actually, even though he was always in the Department of Mathematics, which is a sort of a pure mathematics department. I don't think there is anyone else in that department who has the same interest that he has in reliability. And so, I visited him several times in Rome.

Block: Was the de Finetti Conference the first time that you visited?

Barlow: That was the first time, actually, that I met him. But, as I was saying, Fabio was a very well-trained mathematician, but he had this interest in applications and he liked our book [Barlow and Proschan, 1975]. You mentioned that you had seen a paper of ours. Which one were you thinking of?

Block: I saw a paper that you two did, I believe, on software reliability and burn-in for software. I think you gave that to me at the conference (Lifetime Data Analysis) in Harvard in 1994. I don't know if you finished that paper. It was in a draft version.

Barlow: Oh, I see. I think it is still in a draft version.

Block: Also, a colleague and collaborator of Fabio is Carlo Clarotti who held a series of conferences in Italy. I think Dennis Lindley was connected at least to some of them. Could you tell us something about those conferences?

Barlow: Well, Carlo wanted to be "the reliability expert" in Europe, and these conferences that he arranged with Fabio's help were to further those aims, I think. But the first one was in a former convent in northern Italy (Varenna).

Block: That sounds like the conference in Siena which I attended.

Barlow: No, Siena was the last of the three conferences.

Block: Was it the one in Liguria near Genoa?

Barlow: Well, that was the second on the Italian Riveria. The first was on the theory of reliability. The second conference was on the coast (Genoa) and this was about accelerated life testing. It was going to be Bayesian. So Lindley was invited; in fact, Lindley was a coeditor of the proceedings of this conference. I think Carlo and Fabio probably did most of the work, but anyway, he's listed as a coeditor. Very elementary lectures on Bayesian statistics were given. DeGroot was there too. So, Carlo was able to get all of the best-known names in Bayesian statistics. In Siena, he got Bruce Hill—that was the third conference and you were there.

Block: Yes, that was also held in a former convent.

Barlow: That was very nice. The first one had been held in a convent, too.

Block: The Certosa di Pontagnano, near Siena.

Barlow: Yes, that was beautiful.

Block: Concerning this "Italian School of Reliability," Carlo and Fabio had some other collaborators. There was at least one fellow from Padua. I can't remember his name right now; he had a German name, but he was, in fact, Italian. [It was Wolfgang Rungaldier.]

Barlow: Right. I know who you are talking about. They were all interested in the Bayesian approach. Now, I think that Fabio wasn't that interested in the Bayesian approach before the de Finetti conference. They arranged this conference for de Finetti, because he was very, very close to retirement. But exchangeability itself, which was the

FIG. 7. *Nozer Singpurwalla, Richard Barlow, Henry Block, planning for Siena meeting, Rome, 1989.*

FIG. 8. *Richard Barlow and Max Mendel, Netherlands, 1992.*

topic of conference and also Fabio's major interest, was just mathematics. So that, I think, I may have had some influence on Sprizzichino at that time.

Block: I remember some of Fabio's early papers were on exchangeability.

Barlow: Yes, but it wasn't statistics; it wasn't inference; and it wasn't data analysis. It wasn't that sort of thing. He was a student of de Finetti and de Finetti was not easy to work for; he was a very hard man.

Block: From what I understand, de Finetti did not have a lot of students, did he? There were just a handful, basically.

Barlow: Yes. You see he had a great deal of interest in actuarial problems. He actually worked for an insurance company in Trieste when he was younger.

Block: I didn't know that.

Barlow: So, frankly, according to Fabio, the colleagues that de Finetti really talked to a lot were in economics and in insurance. He didn't get along too well with mathematicians. Anyway, Fabio and I had several papers, and the last paper continued with this Schur concavity idea. It was pretty mathematical.

ENGINEERING RELIABILITY

Block: Have you been influenced by anyone recently?

Barlow: Well, I was influenced greatly by Max Mendel who came to Berkeley in 1989, which was exactly the right time for me because I'd pretty much lost interest in reliability. I'd been working in the field for a long time. Mendel had worked with Peter Kempthorne at MIT in the Sloan School. Mendel's thesis, which was extremely good, was on Bayesian statistics. He had done it all on his own. It was excellent. He had rethought everything. He knew about de Finetti, but he really hadn't read much of his work. In fact, he didn't even reference de Finetti, but it was a beautiful thesis. Anyway, Kempthorne called me up, probably in 1988, that he has this wonderful thesis student who's done this great thesis and wanted to know if there was an opening in the department. Well, we had just had a human factors faculty member retire, so there was this opening in human factors. I mentioned this to Kempthorne and he said that Mendel had had courses in human factors in Holland, but not at MIT. I got his thesis and upon reading it found that it contained completely new ideas and approaches. So we arranged for him to come out and give a talk. All the other candidates were people with the human factors background, but technically, of course, they were washouts.

Block: Your department is pretty mathematical.

Barlow: For an industrial engineering department.

Block: In recent years you seemed to have become more interested in engineering, although you've always had an interest in engineering.

Barlow: Well, at the College of Engineering you're constantly talking to engineers and you're under some pressure to have some contact with industry and other people outside. So it's a different environment.

Block: But you've always had contact with engineers and people in industry, haven't you?

Barlow: Well, that starts with the Sylvania years. But it's been more so in the last seven or eight years.

Block: Recently you've written a book called *Engineering Reliability* [Barlow, 1998]. Could you tell us how this came about?

Barlow: Well, the inception was in 1989 when Mendel came to Berkeley. His Ph.D. thesis had this idea of starting with finite populations and then deriving the conditional probability model that you're going to use as a likelihood model. The finite population exponential distribution was actually derived a long time ago, not just in this thesis by Mendel. But I was very much taken by that idea. We started with the finite population, and if you're a physicist you believe that there are only a finite number of protons in the heavens and earth, anyway. All populations are finite, but it's a very convenient assumption to go to a very large number. But anyway, what I did in this engineering reliability book was to start off in Chapter 1 with a finite exponential model. In other words, we start with a finite population and we make an invariance assumption relative to the average. And we derive the finite population exponential model and then we derive the total time on test. This is essentially the maximum likelihood estimate, but is off by a factor of (1-1/n), where n is the population size. As the population size goes to infinity you get all the usual results for the exponential. But you can introduce all of the fundamental ideas on Bayesian statistics starting with this model. So what I did was I derived a lot of the standard total time of test statistics and methodology for this distribution, the finite population exponential model. Now Max Mendel was not that interested in data analysis; he was never really interested in analyzing data. So that part was my contribution. Then in Chapter 2 I went into the infinite population exponential model. The idea is that the book starts out with failure data analysis, and then there's a chapter on static stress, which is an engineering concept. I had to learn quite a bit about engineering, but there's a beautiful theory about elasticity in mechanical engineering, and there's the von Mises–Hencky criterion relative to the energy—you have elastic energy in any material. When you try to pull material apart you can measure this apparent elastic energy. Now the elastic energy can be divided into two parts. One is distortion energy, and the distortion energy is essentially the energy that is used to distort right angles. If you had a rectangular piece of material, if you just push it like this [demonstrates], you distort the right angles. That's distortion energy. So the theory is that it's distortion energy that is causing failure—the failure of materials that is mainly due to this distortion energy exceeding some value of the applied load.

Block: A type of threshold model?

Barlow: Yes, a threshold. Well, the elastic energy is a sum of the distortion energy, which is what you're interested in, and the volumetric energy. Now the volumetric energy is the energy that would be required to reduce the volume. So if you had a cube and you put it in the ocean, the sides would all contract, and the energy involved there is called volumetric energy. Thus there are two different kinds of energy, and what we did was to concentrate on the distortion energy as the parameter of interest. This has always been the parameter of interest. Engineers are also interested in maximum stress. You condition on this distortion energy as your parameter of interest, and then that defines the finite population. You can think of a manifold. On this manifold you're invariant relative to this distortion energy. It's a way of deriving the strength of materials based on a criteria that mechanical engineers are using. See, the idea was to start with mechanical engineering and physics and the laws that are underlying the phenomenon that you're interested in. And then to use this phenomenon and these laws to derive your conditional probability models.

So the role of the mathematical statistician in engineering is that you start with a class of problems, like strength of materials, and then you use the underlying principles that mechanical engineers and other people use when they analyze materials. You start there, and then you fix on a parameter of interest, like this distortion energy that I was talking about, and then you find a manifold and condition on the distortion energy, but you don't really know it (the distortion energy is your parameter of interest), and this procedure produces a manifold, not necessarily a simplex. And the idea is that you're looking for the invariant measure on that manifold. So it's like a uniform distribution but it's much more complicated. And deriving that distribution then allows you to analyze the strength data. This was discussed in Chapter 4 and the Weibull-with-shape parameter two is derived and it's what you get when you start with Hooke's Law. Hooke's law is a very simple linear relationship between stress and strain. Strain is this way; stress is this way [demonstrates]. And I was extremely excited when I found this relationship—Weibull's distribution coming out of Hooke's law. This led to some other papers and actually some very good Ph.D. theses relative to this approach. The best possible thesis coming out of this was by John Shortle and it's about rotors. This hard disc is rotating at a very high speed. The point is that when you manufacture such a cylinder, there is this axis that has to be drilled. It's never precisely what you want; it's always a little bit on an angle. What Shortle did in his thesis was to start with this problem, with this angle problem, and then actually work out the distribution of stresses. A very, very complicated piece of work. Now, the interesting thing is that when you take this approach and work on these engineering problems, you almost never come out with normal distributions. So all of the usual distributions that statisticians love to use, they don't come out of these derivations.

Block: You said the Weibull did come out.

Barlow: Well, the Weibull, that's true. The Weibull came out when I used Hooke's law. But if you use a more complex criterion such as the von Mises criterion, you come out with a much more complicated distribution. That was also in the thesis of one of my students.

Block: And this is for the distribution of stresses?

Barlow: The distribution of stresses, but in three dimensions, you see. In three dimensions you have stresses, and usually the stresses are not principal stresses. Principal stresses are when the stress matrix is diagonal. That's not usually the case. So, even in two dimensions when you consider a certain problem in static stress, you don't get the Weibull distribution—you don't get any distribution you've ever seen before, and if you take some kind of a limit, you don't get the normal distribution. The normal distribution is extremely special; it comes out of making certain assumptions about energy. The normal is not usually what you get. Well, anyway, it's all in the book.

Block: Much of your current research seems to be on engineering applications.

Barlow: Yes, I spent a lot of time on Chapter 4. Well, first of all I had to learn the subject of static stress. It's a beautiful subject, actually. It's very mathematical. It's all linear.

Block: And how about the rest of the book? Do you want to talk a little bit more about the other chapters of the book?

Barlow: Well, there's a chapter on fault-tree analysis and there's a chapter giving all of the asymptotic formulas that we derived for availability theory. Then there are two chapters on influence diagrams. An influence diagram is related to a fault-tree diagram. In fact, a fault-tree is an influence diagram without any decision nodes. A fault tree has logic nodes and probabilistic nodes. We're just interested in the probability of system failure, that's usually the top [most important] event. The influence diagram itself was invented by Ron Howard and his students at Stanford in the sixties, I think. And they sort of kept it a secret for a long time. Well, they were using it in consulting. This was one of their main consulting tools, and they developed it as an alternative to a decision-tree. Now the problem with the decision-tree, as you know, is that you just have too many branches and the decision-tree explodes. So they were trying to figure out a way to explain things to generals and so forth about what was going on relative to failure in systems. And they invented this influence diagram. In the influence diagram, the circle stands for a random quantity. In the decision-tree, it's a random node, but the arcs correspond to the outcomes. In the decision-tree, the arcs contain the information, the possible values of a random variable. In the influence diagram, the circle is a random quantity, and it has all the values of a random quantity; the arc only indicates possible dependence between circles which are random quantities. So it's quite different, and the value of the influence diagram is that it explains conditional probability. Using the influence diagram many problems can be explained, and you can understand what's going on when you draw this influence diagram. You can understand conditional probability. So I think it's a great teaching tool, as well as a consulting tool. So anyway, Chapters 9 and 10 are on influence diagrams and Chapter 9 is about probabilistic influence diagrams and it is based on papers that I coauthored, and Chapter 10 is basically on decision influence diagrams. Chapter 9 is based on work with Carlos Pereira and then there's some work with Carlos also in Chapter 10. Carlos visited me at Berkeley for about three years, and I converted him to doing influence diagramming. He wasn't interested. In fact, it took a year, at least a year, before he became interested in influence diagrams. But Carlos likes to consult for biologists and for university people in psychology, and he was able to use these influence diagrams in his consulting.

Block: He's a former student?

Barlow: No, actually he was a student of Basu.

Block: At Florida State?

Barlow: When I was visiting Frank and taking his courses with Basu, that's when I met Pereira.

Block: Anything else you want to say about the book?

Barlow: Well, the appendices are of a special interest. Appendix A, its title is "Classical Statistics is Logically Untenable." Now this is a phrase that Basu used. In this Appendix, I quote from Basu and the first question is "What is wrong with classical statistics?" Basu's answer was that he had tried for something like two decades to develop a foundation to understand classical statistics and essentially he failed. He had come to the realization that statistics was really antimathematical because it's inductive and you have to make judgments—you have to guess. But it took him twenty years to figure this out. So I quote Basu,

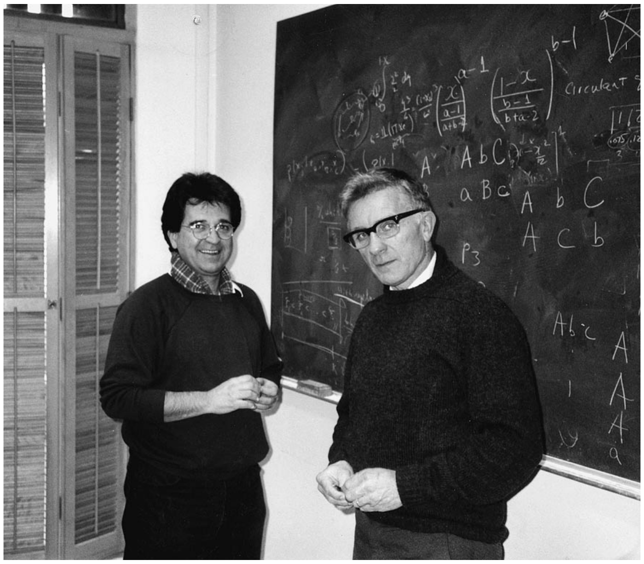

FIG. 9. *Richard Barlow and Carlos Pereira, Berkeley, circa 1986.*

using almost those words, and then there are some examples. There's an example showing why unbiasedness doesn't make sense. The counterexample given there is relative to estimating the mean of an exponential distribution. The minimum variance unbiased estimator for the mean is not the minimum squared error estimate. So that should pose a problem for frequentists. And the second example is confidence intervals, and this was a problem that I got from Lindley when he was visiting me and Ben Epstein was also present. So Epstein and Lindley were sharing the same office at Etcheverry Hall. And Epstein, of course, is considered the father of the exponential distribution in reliability. The engineers weren't really using exponential distributions in engineering to the extent that they were after he and Sobel wrote their papers.

Block: All their theory of life testing assumed exponential distributions.

Barlow: That's right. There are arguments for that. Well, anyway, Lindley wanted to give an example showing Epstein that we should, of course, be Bayesian and the counterexample consists of the idea of using confidence intervals. I won't go through all the details, but it's essentially a betting situation and after all is said and done, you find that you're always losing if you use the frequentist approach with this confidence interval. Lindley sets it up in terms of confidence intervals and bets and of course eventually the parameter's revealed, and the way it's described, the frequentist with his approach to confidence intervals will lose. These are very concrete examples, very, very concrete.

Block: So, was Epstein convinced?

Barlow: Oh no, later in Stockholm we were together at a conference. These were essentially, well, they were mechanical engineers interested in discussing analysis of failure, and I gave a paper using a Bayesian approach to these problems. After I finished my talk, Epstein rose from the audience and he said "Must we wash our dirty linen in public?" [laughter] Meaning that we shouldn't talk about Bayesian statistics. He didn't want anything to do with Bayesian statistics. So I'll always remember that one. He's a good guy. So, anyway, you should remember that all of frequentist statistics is ad hoc. It always starts anew with each problem, and you can do anything and it's usually based on deductive reasoning, not inductive reasoning. So the deductive reasoning will have some probability, but it's not all probability. If you use inductive reasoning, and use the Bayesian approach it's all probability. It's nothing but the laws of probability. You have to be clever, but from a frequentist point of view you need to be even more clever because you have to start all over again each time. These two examples, I think, are two of the best ways to explain to people that maybe there's something wrong with the frequentist point of view—unbiasedness doesn't make any sense and confidence intervals don't make any sense if you think in terms of bets. That's Appendix A. Now Appendix B was based pretty much on a lot of study that I did on a paper by Herman Rubin. Now Herman Rubin wrote a paper in which he was arguing for the Bayesian approach from an axiomatic point of view. So he starts out with axioms that he sets up, which he calls axioms of self-consistency. If you are going to be self-consistent in your analysis, then you should start with axioms. He starts with these axioms and then derives the existence of a utility function. A rational self-consistent person who follows these axioms has a utility function. Rubin's big thing is that you can't really separate the prior from the of utility. This is a short paper, but extremely mathematical. I learned about it from Herman who told me that I must read this paper. It was work that he'd done at Stanford probably in the 50s and he finally published it in the 1980s.

Block: Pulled it out of his file drawer so to speak.

Barlow: Yes. So I spent a lot of the time reading and trying to understand this paper, and then rewriting it using my own words literally with the same axioms—how it implied the existence of the utility function. So that's Appendix B; it's my interpretation of what Herman was saying in this paper.

Block: Do you think the engineers will find this useful?

Barlow: No [laughing], that's why it's Appendix B. No, there's no mention of this in the Introduction or Chapter 1.

Block: While we're on the topic of Bayesian Statistics, I want to just briefly ask you whether the modern computational Bayesian techniques have influenced you at all. Are the Markov Chain Monte Carlo methods something that has had impact on your research.

Barlow: No, I really haven't done very much with this. I was really more interested in the problem of deriving the conditional probability of a distribution with various assumptions and working with Mendel. Shortly after he arrived, we started working on a paper where we make the argument that if you're going to consider, mathematically, the concept of aging, it really doesn't make any sense to consider a single unit. You must think in terms of a population of units similar in the sense of exchangeability. You start there and then, if you introduce Schur concavity, you can make an argument for the aging concept, relative to a population, by using the Schur concavity

Block: This was a JASA paper?

Barlow: Yes [Barlow and Mendel, 1992]. We spent at least two years working on it in the coffee shop. Now that was for us a big paper. You see, it was the ideas that were of interest. The ideas are the main thing. There was some mathematics too, but it was the ideas, which have not been adopted yet.

Block: I have a general question about reliability which is viewed by some as a statistical topic, and others think of it as a mathematical topic, and certainly it is considered operations research to others and many think of it as engineering. What's your current thinking about reliability?

Barlow: Well, reliability problems come out of engineering, and Frank and I, at Sylvania, encountered these engineering problems, such as the spare parts problem. But they always led to a mathematical problem and they came from a real engineering problem. But there isn't really much engineering involved in it. So much of the work that we did in reliability was with applications to engineering problems, but they were really mathematical problems, and we were solving them using mathematical techniques. No engineering was involved.

Block: Some of the engineers did find some of the things you developed useful at some time or another, didn't they?

Barlow: Oh, yes, they're talking about it at this conference. Glen Benz was talking about this spare parts problem yesterday morning. He was using algorithms from my '65 book, and solving problems that he had. He's a good engineer and he also reads, but he's one of the few engineers who actually uses the mathematical methods that were in the '65 book.

Block: Your feeling is now that it is probably more important to go back to engineering first principles?

Barlow: It's more fun. You start with a physics foundation for engineering. One of the best references is the Feynman lectures on physics. Now those are considered to be fairly hard, but they're taught to freshmen, the beginning of a three-year course.

Block: In your college?

Barlow: Yes, they had to use some kind of physics. But a mathematician would enjoy the way Feynman explains things. A lot of engineering is based on physics. All these problems that engineers are working on have some kind of a physical basis and you can find out the underlying principles in this Feynman book, and then you can go from there. A mathematician is not going to read a typical engineering textbook because it is just too empirical. You start with physics and then consider engineering problems, and then you try to use the principles behind these problems and then develop your statistics and your probability models. But I don't know how to make the connection between the engineering physics principles and the statistical models from the frequentist point of view. I really don't know how to do that, but from the Bayesian point of view I can do it, because I can think in terms of indifference and invariance and so forth. These are judgments that you make. You start out making certain judgments, and on the basis of these judgments you derive probabilities. And it's fun and can be extremely mathematical. In fact, this Shortle thesis that I was telling you about uses differential geometry to solve a very practical problem. Starting out with the physics and the principles of the problems, you derive probability distributions. But then the distributions aren't standard statistics distributions.

Block: Anything else you want to say about your current research interests?

Barlow: Well, I'm still interested in this idea that we were playing around with before Mendel left. The idea is that the quantities that we're interested in, in engineering statistics especially, are not really quantities that live in Euclidean space. Euclidean space is a very special case.

If you use the fact that when you are out in space, it doesn't matter what coordinate system you use. You have rotational invariance with the Euclidean metric. And this rotational invariance in finite dimensions gives you a finite dimensional version of the normal. Thus the normal distribution, the gamma distribution, all of the usual distributions can be derived from a finite population and a certain invariance assumption. Now you have items and they have lifetimes. The lifetimes are really fibers in a differentiable space. That's really what they are. Now the question is: how do you use differential geometry to derive life distributions using the fact that you're really not in Euclidean space? Well, anyway, this is something that I'm quite interested in.

Block: So it's fiber bundles in the mathematical sense rather than the engineering sense?

Barlow: Well, it's in the mathematical sense, but you see physicists use differential geometry because that's the way the world works. If you have infinite differentiability, so that things are very, very smooth, you're not talking about quantum mechanics. Lots of things in physics can only be explained mathematically using differential geometry. At Berkeley, there have been three Ph.D. theses that use differential geometry to derive conditional probability models in reliability applications.

Block: Your students?

Barlow: One was my student.

Block: Was that Shortle?

Barlow: Well I was on the committee with Shortle, but he was originally Max's (Mendel's) student. My student was Peisung Tsai who went to work at the University of Maryland in a reliability institute. It's involved with electronic packaging. Well, anyway, he wrote the first Ph.D. thesis using differential geometry in a reliability context deriving strength distributions. It's very good, but it's probably not as realistic as Shortle's. Shortle's is probably the best application to date. Well, that's where we are.

STUDENTS

Block: I want to change the subject a little bit here and talk about your career. Most of it has been spent in California, although you had opportunities to move. This preference was mostly personal as opposed to professional, I guess, or was it?

Barlow: Oh yes, but until 1989 I never coauthored or worked with any faculty at Berkeley. I always worked with people outside Berkeley. The thing about Berkeley is that it has excellent students. Oh, I brought visitors; I worked with visitors. Satya [Satyanarayana] said it best: he said that there's something in the air here. Research is in the air.

Block: I know what he means.

Barlow: Yes, he really liked that. He was here for three years on my research contract.

FIG. 10. *Richard Barlow and his Ph. D. student Sung Chul Kim, 1988.*

Block: But he was not your student; he graduated from someplace else.

Barlow: Bangalore in India. It was Ralph Evans that brought his paper (which was joint with his thesis advisor) to my attention. Almost immediately after this I realized this guy was so good; I sent a telegram asking him if he could come. He was quite a guy. I had enough money so I could support him for three years.

I also supported many students over the years.

Block: How many Ph.D. students do you have, have you kept track?

Barlow: No, I haven't kept count. I'll have to count them all, but it's around 40. They're all over. Some of the Ph.D. theses were written with Satya as codirector. Then Mendel and I had some students. Well, unlike Frank, I don't have all these theses on my bookshelf. Well, you see Frank edited all of his Ph.D. theses. Frank is the only person who can write clearly. He's told me that [laughing].

Block: Yes, when his students finished, the papers usually were ready to go right out for publication, but, from what I understand, this was not necessarily the case with you.

Barlow: That's true. I thought the theses should be in the students' words pretty much. You correct gross errors, but you don't rewrite in most cases.

Block: At least you don't.

Barlow: I didn't. But there is a temptation because the students are mostly foreign students, and usually their English language isn't very good and there is a temptation to start all over and rewrite the whole thesis. But my research has mainly been motivated by research contracts and students.

Block: So the agencies funding you were one of the reasons for your research?

Barlow: Yes, I was always writing papers and doing research for these agencies, and then the university was promoting me because I was writing these papers, but I wasn't doing it for promotion, I was doing it for the agencies.

Block: So they directed your interest in some sense.

Barlow: No, not really. I mean it had to be called reliability and I recall that ONR in the sixties would have been extremely happy had I been working on stress analysis and strength of materials. That's what they wanted me to work on in the sixties, and I had no interest whatsoever in the subject. I never wrote a paper with Frank or anyone else in that period on that subject or in the seventies either. Then later, of course, they were very much interested in software reliability. Nozer and I have a paper on that subject, but I never really did very much work with software. My feeling was you really had to be a person who knew a lot about software, being able to write software, to really do a good job, to really model the problem correctly.

Block: So what you're saying is that the agencies supported you and let you do whatever you wanted.

Barlow: Yes, pretty much. The same was true for Frank. The beautiful inequality work that he did was done on reliability grants. Not all of it had reliability applications.

The agencies are supporting universities and they're supporting students and today a lot of our research institutions wouldn't be any good if we didn't have researchers who were originally foreign. They get their degrees here and stay. We are very lucky.

Block: Do you keep in touch with a lot of your old students?

Barlow: Well, some of them, the more recent students. The older students come by. My first Ph.D. student, Ben Fox, wrote a book on simulation. It's very good. He's in Montreal.

Block: He's a simulation person generally?

Barlow: Yes, he's in simulation now, but he wrote his Ph.D. thesis on reliability. But he's a Bayesian and I wasn't a Bayesian then.

Block: This was back in the sixties, I guess.

Barlow: This was '64. He was one of my best students. There are a lot of students that I have not kept in touch with. Now what has happened is that the students in the department, in order to get their degree, had to pass the oral examination. They had to learn a lot of optimization. They had to learn a lot of things not connected with reliability at all. The department is not a department of reliability in any sense of the word. So their interests were often more geared toward optimization rather than probability.

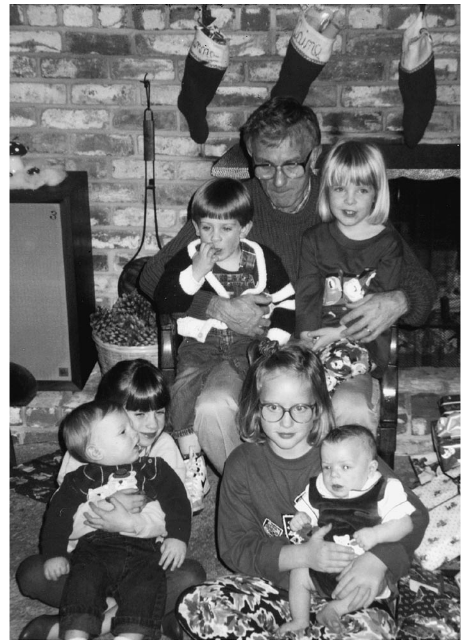

FIG. 11. *Richard Barlow with his six grandchildren, 1991.*

Block: The operations research aspect of the department was probably pretty important.

Barlow: Yes. Well, I found interesting problems and they became my students for one reason or another, and they of course worked on reliability problems because they were being paid, usually, from a reliability contract. But subsequently, many of them went to universities, many went to business schools and they didn't usually continue with reliability work. Network reliability may be the exception.

Block: You had a few students who went to the East Coast, I think. North Carolina, Maryland?

Barlow: North Carolina, Maryland, a couple. Well, I haven't been a good advisor. I should have kept track. You see what happened. I converted in 1976 to the Bayesian approach, the Bayesian way of thinking and of course no previous student had been exposed to that.

FAMILY AND FUTURE PLANS

Block: Did you want to talk about your family a little bit? I know you're traveling with your wife, Barbara, and your granddaughter now. You've got four children, is that right?

Barlow: Well, they're adults now. The youngest is an astronomer. His Ph.D. is in physics from U.C., San Diego. He is currently at Pasadena and he's working on several projects that NASA has relative to satellites. They're putting up satellites in order to look at the infrared rays, gamma rays and so forth. Well, he's interested in quasars. His Ph.D. thesis was on quasars and experimental data relative to quasars. Since the Ph.D., he's been doing research. My other children are not researchers. They don't have Ph.D.s.

Block: I think they probably can be forgiven.

Barlow: They all have bachelor's degrees.

Block: You have a daughter that lives near you?

Barlow: She's an artist. She got her degree at San Diego State in graphic arts and worked for *Runners' World Magazine*.

Block: And she's in Walnut Creek [where Barlow has lived for over 30 years]?

Barlow: Walnut Creek and she's hoping to have a little exhibit. We have "Art on the Main" where people bring their paintings. She's starting to do well. She's been taking water color classes again. Her instructor tells her she should quit her day job. Her day job is taking care of her kids. Anyway, she's pretty good.

Block: And your other daughter ?

Barlow: Well, Jeanne is in Elburn, Illinois which is a very small community. It's about 200 miles north of where I was born. Her husband works for the Trans-American Corporation. She has been an elementary school teacher, so she's very much interested in elementary education.

Block: And your other son?

Barlow: He's an accountant. He's in Nashville, Tennessee, working as an accountant.

Block: And your wife, Barbara, is interested in taxes, is that correct?

Barlow: She has some degrees in investment counseling and she does taxes and helps people with their income taxes. But she also teaches people to help other people prepare their income taxes and she does this for the AARP and the IRS. She's very good on the subject, in which I have no interest whatsoever. It's a pretty gloomy subject.

Block: I know that you've always been a swimmer and you also have an interest in classical music. I understand that you attend the Bach Festival every year in Carmel.

Barlow: Yes. We go to the Bach Festival and we go to the Shakespeare Festival in Ashland, Oregon, in September. We've been doing that for probably about five years.

Block: Are there any other avocations that you would like to pursue when you retire at the end of next year?

FIG. 12. *Al Marshall, Richard Barlow and Ingram Olkin at Frank Proschan retirement celebration, Tallahassee, 1993.*

Barlow: Well, I'll probably take some courses in horticulture.

Block: Are you a gardener now?

Barlow: I'm a gardener now. But things keep dying on me [laughter]. I need to take a course in horticulture. I've been developing an herb garden for Barbara. An herb garden is very specialized. Now the difference between an herb garden and regular landscaping is that the plants are aromatic. They have all kinds of good smells. I have some flowers too.

Block: What kind of herbs?

Barlow: Well, there are five different kinds of thyme, four or five different kinds of mint.

Block: How about basil?

Barlow: We have sweet basil and ruffled basil (which is a purple basil) and Italian flat parsley, arugula (that's very strong), onions from onion sets... There are five parts to this herb garden, five sections, and I just recently installed a drip system. That's a very big project because there are pathways between these five sections, so you have to dig under them. And I use shrublers. Have you ever heard of shrublers?

Block: No.

Barlow: There's been a lot of activity and innovation in drip systems. The shrublers took me three days to put in.

Block: Do you think you'll continue to do research after you retire?

Barlow: Well, you see I'm interested in keeping up the web page [www.ieor.berkeley.edu/~ieor265] for the book, and I'm interested in communicating with people relative to the book and research may or may not be motivated by this book. That's sort of the connection.

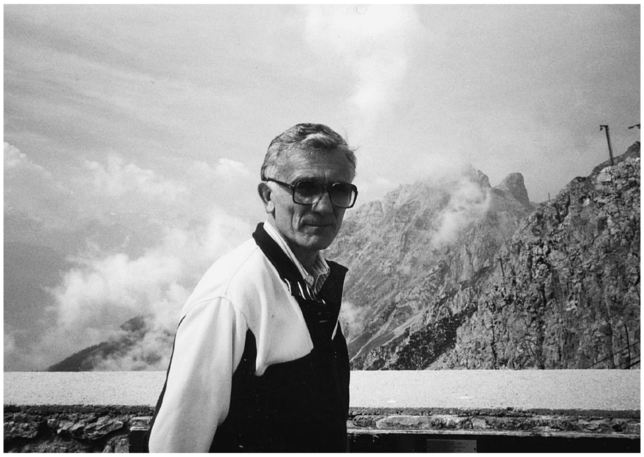

FIG. 13. *Richard Barlow, Innsbruck, 1993.*

Block: Will you continue to have Ph.D. students too?

Barlow: Probably not. I get letters from people wanting to come, but they usually need more support than I can give them. I'll teach this graduate reliability course next spring and it will attract some good students. I really haven't been trying to get thesis students recently.

Block: Do you think you might continue to teach the reliability course after you retire?

Barlow: If the course is taught, I very likely would have to teach it [laughing], because the people that have the academic credentials at the university are not interested in reliability. Well, there are people who do, but they're more interested in simulation than reliability. Quality assurance, quality control, that sort of thing. Reliability is not as hot a topic as it was once. But you know the problems never cease, because every new technology introduces new reliability problems. Well, they're essentially new if you look at the principles behind them. In the sixties we had aerospace and rockets, then we had the nuclear power reactors in the seventies, and in the eighties we had the computer networks.

Block: What about the nineties?

Barlow: The nineties, what do we have in the nineties? Well, the computer networks don't really follow the pattern of the eighties because of the satellites. There are no transmission lines, no ground lines. That raises some interesting questions. There are problems involving materials, smart materials. I think material science is a good area. The new technology, probably material science, is interesting. The people in these fields have their own reliability meetings and so forth. I think that if you start with their engineering ideas and then incorporate them into your mathematical probability models, that would be interesting.

Block: Is there anything else that you want to talk about?

Barlow: Well, I think that a lot of my research results are archived. And my memory isn't that sharp about things that I did a long time ago.

Block: Well, I think we've covered a lot of that material. You've had a remarkable career and I'm happy to have had this opportunity to talk to you about it.

Barlow: And read the book!

Block: Read the book. Okay, that's good advice. Thanks very much, Dick.

ACKNOWLEDGMENT

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