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A Conversation with David R. Brillinger

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Abstract. David Ross Brillinger was born on the 27th of October 1937, in Toronto, Canada. In 1955, he entered the University of Toronto, graduating with a B.A. with Honours in Pure Mathematics in 1959, while also serving as a Lieutenant in the Royal Canadian Naval Reserve. He was one of the five winners of the Putnam mathematical competition in 1958. He then went on to obtain his M.A. and Ph.D. in Mathematics at Princeton University, in 1960 and 1961, the latter under the guidance of John W. Tukey. During the period 1962–1964 he held halftime appointments as a Lecturer in Mathematics at Princeton, and a Member of Technical Staff at Bell Telephone Laboratories, Murray Hill, New Jersey. In 1964, he was appointed Lecturer and, two years later, Reader in Statistics at the London School of Economics. After spending a sabbatical year at Berkeley in 1967–1968, he returned to become Professor of Statistics in 1970, and has been there ever since. During his 40 years (and counting) as a faculty member at Berkeley, he has supervised 40 doctoral theses. He has a record of academic and professional service and has received a number of honors and awards.

This conversation took place on September 9th 2009, in the Swiss Alps of Valais, during David's visit to give a doctoral course on "Modeling Random Trajectories" in the Swiss Doctoral School in Statistics and Applied Probability (see Figure 1).

1. GROWING UP IN TORONTO

Victor: I suppose this is an interesting setting to be doing this, as one story would suggest you originally come not from very far from here....

David: Indeed! Now I don't know the specifics, but there were Brillingers in Basel at the end of 1400s. Once we were in Zurich, at Peter Buhlmann's invitation, and we saw a statue that was close: B-U-L-L-I-N-G-E-R. Now, the Brillingers in Basel became protestant at the time of Martin Luther. The next time I find them is in the 1700s when Brillingers went to Pennsylvania as Mennonites. They finally got up to Canada after the American Revolution. They were the original draft dodgers. You see then, in America, men had to be in the militia, but the Brillingers were pacifists. So they went to Ontario where they could practice their religion as they wished. So I'd like to think that there is some Swiss background and presumably it would have been through some great–great uncle who was "Rektor" of the University of Basel.

Victor: I see, I see, so it would then be Brillinger (German pronunciation) rather than Brillinger (French pronunciation)?

David: That's right. And you Victor told me that you've seen a truck on the Swiss highway with Brillinger on it. Also Alessandro (Villa) told me he saw a mailbox with Brillinger on it, or something like that.

Victor: Jumping much further into the future: you grew up in Canada.

David: Yes!

Victor: Could you tell us a bit about your family?

David: My father died—let's just work it out—when I was 7 months old, so this was very harsh on my mother. She woke up in the middle of the night and he seemed to be in some trouble, but then she fell back asleep and I think she felt guilty about that ever after. I doubt there was anything that could have been done back then because he died of a cerebral hemorrhage. I wish I could have gotten to know them together better. You know, they had their house, a cottage, a dog and so on. They had a Harley motorcycle and went off on that on their honeymoon, they had a sailing canoe....

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FIG. 1. *David and Victor with the Swiss Alps in the background. Photo taken during the interview session, September* 2009. *David is proudly wearing the Canadian Soccer team shirt.*

Lakes and Canadian things were very much part of their lives. My mother was actually a very beautiful woman, when you see the pictures, with smiles (Figure 2). But the smiles mostly disappeared after my father's death. Then, it was World War II times and most of the men were gone. It's hard for me to imagine she wouldn't have remarried. But it just never happened.

She really cared a great deal about my education and structured things so that I got a fine education. At the start, there was a bit of money—because my father was going to be an actuary, so she had some insurance money. I went to a private boys' school in Toronto until the money ran out. Then, there was this school for bright kids in Toronto, the University of Toronto Schools (UTS). I took the exam and got into it. UTS was very important for me. I should mention that my maternal grandmother was also very important, and perhaps she raised me. She had had her husband die in the great flu epidemic and found herself with five children to raise. So I had, I think, a beginning that made me appreciate being alive and not really expecting too much to come from it. I really have been pretty content and nonaggressive about things in my life and feel very lucky. You know, all four of my uncles—and I've decided they were my role models—were taxi cab drivers at some point in their lives. The way they could just talk to anybody and the way they engaged people to some extent formulated the way I have become. I had a lot of paying jobs as I was growing up, including caddying, delivering prescriptions, salesperson in a small shop.

I had a lot of cousins that were important to me because I didn't have siblings. And there were a lot of

FIG. 2. *Young David in his mother's arms at the King and Queen's visit to Toronto, as a Cub Scout, and with his ski gear.*

wonderful mother's side family gatherings. So, I don't think I really thought about not having a father when young, but I do wish I could have asked my father certain questions since we did not have much contact with the Brillinger side of the family. That was a shame.

Victor: Did you have any influential teachers at school?

David: Oh, yes! There is one very influential teacher who taught me when I was at Upper Canada College—that was the private boy's school. I had not started the year there and when I transferred, he found out that I was not very good at fractions. So, he spent some time tutoring me. Now he was also an important person in Ontario hockey. And after tutoring me he came in the class one day and said he had 5 hockey rulebooks and he was going to give one of them to whoever answered a mathematical problem first. So first question, my hand went up, one rulebook; second question, second rulebook; third question, third rulebook! So he said, "David that's it, you can't get anymore of those!" I really learned I was good at sports. Or no, actually, I wasn't good at sports, I was good at math, but I was very motivated when it came to sports (laughs)! The teacher's name was H. Earl Elliott.

Victor: And those were the same rulebook?

David: (laughs) Oh yes! I don't know what I was going to do with all of them! He had not specified any rules, so I had three and gave my cousins two! I had realized I was good at math, and I loved working on math problems. A lot of books had problems without the solutions in the back. I had a lot of fun doing them. Perhaps I had more time to do that because the weather was bad in the winter and I did not have siblings. Afterward, I went to UTS. I said that was for bright kids, but part of the definition of "bright kids" then was being male (both laugh).... Luckily things changed, although UTS no longer wins the Toronto high school hockey championship like it used to! I had a very influential mathematics teacher there, Bruce McLean (Figure 3). He was also the hockey coach and is still alive. He would just let me work at the back of the room on my own. Everybody else was up toward the front, but he would just leave me alone at this table and bring these books full of problems (e.g., Loney, 1930). Statistics was one of the topics. And there were these British problems that you've probably seen in the Tripos, Victor, things like that. I don't know about what level I would have been at had I been in England, because students there started working with these concepts very early on. I read a book where I think Dyson said he had

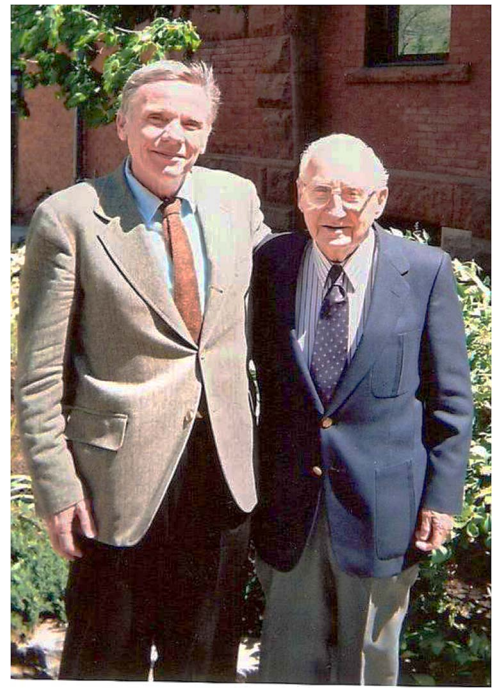

FIG. 3. *David with Bruce "Nails" McLean.*

solved all the problems in Piaggio's differential equation book (Piaggio, 1920), but when he was at public school—I did that when I got to University, so I guess I was lagging behind. But I think I was very independently driven to work on these things. I thought I solved them, but, you know, I didn't quite know; but anyway, I solved them to my satisfaction. Then, Ontario used to have some pretty tough High School exams, for the last year—grade 13—and four of them were on algebra, geometry, trigonometry and problems respectively. I got 100, 99 and 100 on the first three and 96 on the last. I still think about that 96. You see you were to do 10 problems, but there were 12. So I "solved" all 12. Later "Mr." McLean told me that the person who was grading kept getting a total of 116 on my exam, and he could not figure out what was going on for a while. Eventually, he realized that I had attempted all 12. My error was that one of them was finding the maximum or minimum of something, so to show off I used calculus, but I forgot about checking the second derivative! I've never forgotten that since (both laugh)! But anyway, that brought me a scholarship that helped me make my way at University. Back then, prizes were important because there weren't many bursaries. Now, in America, they've switched to means tests. But I won a lot of prizes as an undergraduate which kept my mother and me with food and so on.

Victor: Evidently, mathematics was one thing you enjoyed, but what about sports?

David: I love sports, I always have and I have always been a Toronto Maple Leafs fan. I don't know if I still have it, but there was a wonderful picture of me about 3 years old with hockey stick in hand and skates on feet. I was often the last guy to make the team or the first guy not to make the team—but I was always there! When I was growing up, they would flood the whole neighborhood park so there would be 5 or more hockey games going on. You didn't need all this fancy equipment. I guess I could make the formal teams until I was 13 or so, but then that stopped. It returned for a while when I went to Princeton as a graduate student. There I got to be like an intramural star, because I could raise the puck, knew the rules and played left-handed. Now, I mentioned my high school teacher, Bruce McLean. There's a story I love concerning him: there was my 50th High School reunion a couple of years back and I was in Edmonton the week before the reunion and was going to need to be in Toronto the week after, so it was just too much time to be away from Berkeley. One of my dear friends from High School and University, John Gardner (now Chair of the Board of Directors of the Fields Mathematical Institute), asked if I'd like him to arrange a lunch with "Nails" McLean—his nickname for UTS students was "Nails." I said of course! So, when I went to Toronto the week after, we had lunch. McLean was 96, and had driven in through all the traffic to central Toronto for the lunch. We had a wonderful time. It turned out he had also been in the Navy, so we discussed that. But at the end of the meal he got this incredibly serious look on his face. So I'm thinking, "What's this all about?" And he says "David, when you were at school, there was something I really worried about, I worried about it for a long time." So I'm sitting there with my eyes rolled back and wondering. He continued, "I really wanted you on the hockey team, but there were a lot of good players that year!" (both laugh). I just grin when I remember that. And indeed the team was good. They won the Toronto championship. I just wanted to get the sweater, go to practice, and, if we're winning 7–2, get to skate around a bit. But I had to wait until Princeton to do that.

2. UNIVERSITY OF TORONTO AND THE CANADIAN NAVY

Victor: You mentioned before that you were in the Navy, can you tell us a bit more about that?

David: That was at University. I knew that by joining the Navy I was going to get to go outside of Toronto and perhaps Canada for a bit; because Toronto was really a bit boring back then. Canada did not have a draft—still doesn't—so the way the government thought they could get officers for the regular military was by having army, navy and air force programs at the universities. That was a bit like Boy Scouts, and I'd been a Cub (Figure 2) and a Boy Scout. For me, it was obvious to join the Navy because I loved to canoe and sail, and you got to go to Europe and Mexico. Whereas if you were in the Army, you got to march around in the dust of Ontario; and if you were in the Air force, you were in Saskatchewan, which is flat, and with not so much to do then. So, I was on my way to seeing the world and at the same time got paid very well; the food and the clothing were obviously provided. Plus, it was a lot of fun, I just loved it. I mean guns were only 5% or less of the life. So it was a no-brainer to be in the Navy. Second year I was based on the West Coast (Figure 4). In the program there was a prize for the person who was best in navigation and I think I won probably easily, as I had taken an astronomy course and had learned all this spherical trigonometry previously. The way things worked, I ended up being a communications officer learning about radio and coding. This was great since I had been learning physics as well as mathematics. You know, in my career I've gotten to study mostly the things I was good at and enjoyed. I was principally good at math, and it was obvious what my career was to be.

Victor: You once told me a story about doing some very applied statistics in the Navy.

David: That was my first independent statistical research activity, I would say! So let's think. My fourth summer, I had already gone through a lot of basic training, becoming a communications specialist and a sub lieutenant. I was going to be in the aircraft carrier, the Bonnaventure, and we were supposed to sail into the middle of the Atlantic because the Queen was going to fly over there on her way to visit Canada. And so we were to be stationed out there. I don't know why, maybe in case she leapt out with a parachute or something like that! I mean it was awfully ill-defined (both laugh)!

Victor: . . .after all it is the *Royal* Canadian Navy!

David: Exactly! So we had to toast to the Queen at banquets and such and such. Anyway, they had to find something for me to do during the open period before the mission. So, they decided that, since I was studying statistics, they would like to know how many messages were sent out by the fleet weekly for several years. They took me to this room, and here were these

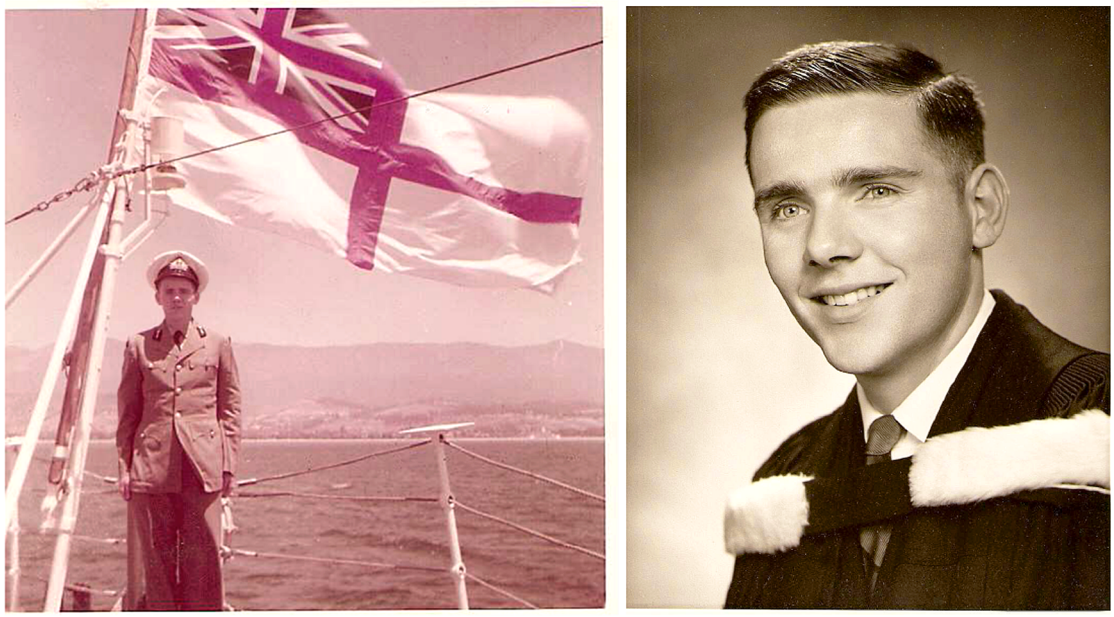

FIG. 4. *David in the Navy off Santa Barbara in* 1957, *and upon graduation from the University of Toronto in* 1959.

huge stacks of signals by week. I would still be counting them if I had done it directly! But instead I thought why don't I just get 100 and weigh them and estimate a weight per signal. And then I asked for a scale, which they found. And I just measured how heavy the piles were, and so I gave them nice graphs. When the fleet was at sea, there were a lot more signals, and things like that. I guess it sounds nutty to be saying the following, I mean I'm totally a pacifist and I think I've been that all my life—but I did enjoy the Navy! I suppose back then Canada was doing peace keeping. Like Brazil's these days, that was the Canadian role then. Our Prime Minister Lester Pearson won the Nobel peace prize for the idea of creating a UN Peace Force. My thought was that the world needs policemen, and since Canada was not in an aggressive posture at that point, I signed up. By the way, in the remaining time before the cruise, I did a lot of dinghy sailing in Halifax harbor.

Victor: Shall we talk a bit about the University of Toronto (U of T)? You did your bachelors honours in pure mathematics. I recall you telling me in Berkeley that you were already reading Bourbaki as a first year undergraduate—in French.

David: Yes, that's true! I was lucky because Canada was trying to be bilingual to support its francophones and I studied French for seven years. So there was a professor at U of T, John Coleman—who is still alive, aged a hundred or so I think; these Canadian mathematicians live a long time. He found out I could learn and read in French. I think he identified me especially because I had won this prize for algebra/geometry/trigonometry and problems. He found what I looked like by watching where my homework handed back ended up in the classroom. He invited me for a coffee or whatever. Actually, he was remembering when I talked to him a couple of years ago that we had butter tarts and tea when we met. He got me reading Bourbaki. And then he said why don't you do some of these problems? So we met then each week: I couldn't do the problems, and perhaps he had trouble too. I don't know if I could do them now, it would be fun to try. The first book was on algebra and I believe that Coleman bought it for me. I still have it (Bourbaki, 1951). The later ones on analysis have probably been the most important to me. Coleman got me reading Bourbaki and I remain very appreciative. Going through them really stood me in good stead when I got to Princeton. I found myself a couple of years ahead of the American students. You see I'd gotten to do mainly maths and physics at Toronto, and I also had this secret weapon: French! I mean the French probabilists were then doing all this wonderful stuff, E. Borel, P. Lévy and M. Fréchet, for example. And most of their things were not being translated. Nowadays the French mathematicians write in English most of the time so that's not an issue. That was first year. That year I also had a course from Ralph Wormleighton, he had been at Princeton—there was a real Toronto–Princeton railroad including Don Fraser, Art Demp-

ster, Ralph Wormleighton; and when I applied to grad school I only applied to Princeton. It never occurred to me to apply anywhere else. I don't think that was a statement of confidence, but I didn't have anyone who had been at university at home, so I just was not getting that kind of advice. The second year was Dempster. Dempster has often taken the geometric approach. When I took a course from Coxeter, I later saw where that approach was coming from. And then in the third year was Don Fraser—he was certainly using a lot of algebra. The fourth year was Dan DeLury. He was this skeptical older guy. He'd been out doing biometrical studies. His attitude was that one might have thought that they had designed an experiment well, but there were many ways that an experiment might have gone wrong. His course was very maturing for me. It's important to have some training in criticism when you're an applied statistician.

Victor: So, that means that you would have had quite a rigorous maths background but also would have been exposed to quite a bit of statistics, which is rather atypical for that time period.

David: Although I was in pure mathematics—that's what my degree was in—I went to all the statistics courses. As a matter of fact, I probably went to *all* the courses, including the actuarial ones. Back then, I could just sit there and absorb things. It's not as though I'm boasting; I used to feel embarrassed about saying things like that, but I think I was just lucky: it was not really anything I did, it's just the way it was. I wish I could have played hockey better, but I didn't get that skill nor the ability to run 100 meters in less than 10 seconds. I guess I'm saying there may be a gene that I was lucky enough to get.

Victor: Do you recall any lectures that you particularly enjoyed? Coxeter had a fine reputation as a lecturer I suppose.

David: Oh yes, Coxeter was wonderful. He had left England after World War II. Also Tutte, who is another geometer, was great. In fact, Tutte had broken one of the important Nazi codes in World War II—and none of us knew that. But some people in the class were mean to him because he was a little shy, and they teased him. I'm sure if they had known about his breaking the code, they would have been more like "wow" instead. Regarding Coxeter, I remember one funny story, where he was talking about a particular geometry for many classes. His course became his book (Coxeter, 1961) or the book was part of his course. So, there was this particular finite geometry he was talking about a lot, with very bare assumptions and he was talking about it during a number of classes. So, finally, I asked,"Why are you spending so much time on this, is it *that* important?" And he said something like: "Well you seemed so interested, Mr. Brillinger!" I mean, I was just asking questions to keep up with where he was going! I was intending to become an actuary for many years, in part because my father worked for Imperial Life. And they were very good to my mother and me. I had realized that if you are poor but good at mathematics, then an actuarial career was a route to the middle class. I'm not sure I was after being middle class, but I needed to help my mother, so I was going to be an actuary. But Don Fraser, who had great influence on me (see Figure 5), said something like: "Well, David, sure that's nice, that you're going to be an actuary, but why don't you go to Princeton first?" So, I did! I went to Princeton, the plan being to become an actuary after I was done with all this childish fun, namely, mathematics.

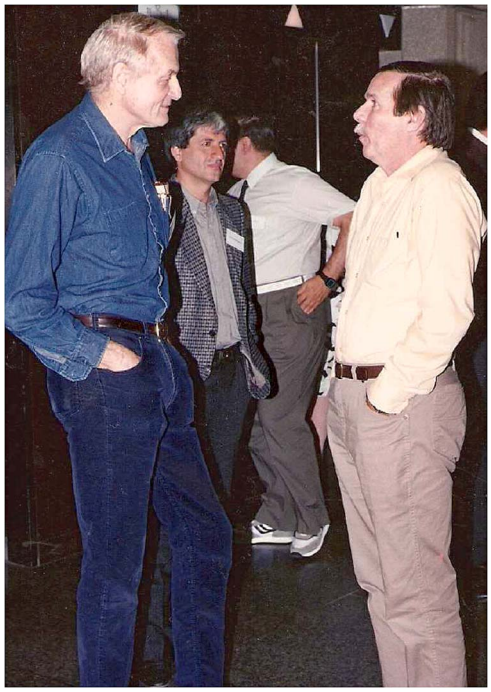

FIG. 5. *David with Don Fraser.*

Victor: Apparently it was *too* much fun. . . !

David: I guess that's right. And I realized at some point that anything I could do as an actuary, I could probably do as a statistician—with the added benefit that I would get to travel and be an academic. I did take enough of the exams to become an Associate of the Society of Actuaries.

Victor: Just before going off to Princeton, you were among the winning five of the Putnam competition of Spring'58.

David: It was again Coleman who got me involved.

Victor: And I recognized a couple of other famous names on the same honours list, Richard Dudley and Larry Shepp.

David: Yes, I got to know them both. You see, both of them went to Princeton for graduate studies. I really had no idea of what was involved. I just went and took the exam! I remember that Erdös visited Toronto for a month and he gave a course. One of the problems he taught us was on the Putnam exam! (laughs) Some number theory thing (continues laughing).... So on the exam day that one was out of the way pretty quickly! He was just a real gem, a real role model. I mean he had these simple direct ways to approach problems, and would advocate that you should take a breath before you start writing down a lot of equations and things like that. U of T was absolutely super. I got a super education in mathematics there and at high school. I mean some people might think of Canada as being a backwater, or as having been one, but there were some very fine researchers and teachers. You know, Coleman had also gone to Princeton just before the War started. I was lucky.

I can't resist adding that, while I was at U of T, I was actually at Victoria University. There, I earned a letter for playing on the soccer and squash teams, each for four years. I can show the letter to you! I would also like to add that Art Dempster and Don Fraser have long been role models for their ex-students. In research they each have taken roads less travelled in their work.

3. PRINCETON

Victor: When did you move to New Jersey?

David: In the summer of '59. That was my last summer in the Navy, and I had become a Lieutenant. I turned up there in the beginning of August having left the Bonnaventure. I had asked if there was some work for me, and it turned out that Sam Wilks had just finished writing his book Mathematical Statistics (Wilks, 1963). My job was to work on the problems. I remember I just lay out under the trees at Graduate School working on them, right by the golf course—which I would golf on most days, illegally. I remember going over to Wilks' office just before term started. One of my Canadian friends, Irwin Guttman, was there. I said, "Well here are the solutions, but I couldn't get one of them." And Wilks went "What???" In the end he took that problem out of the book. It was about proving that the median and the mean were jointly asymptotically normal. It took me a while to figure out a neat way to do that.

Victor: You got right into mathematical statistics upon arriving at Princeton.

David: Oh yes. Already at Toronto, I could see that statistics, perhaps as an actuary, was for me, because you interact with people a lot. Math was a lot of fun too, but you interact with a much narrower group of people. DeLury had impressed me, because he was really working at the frontier of the applications of statistics. I have found myself realizing that statisticians are the keepers of the scientific method. When a scientist comes up with something, what can they reasonably conclude? That appealed to me, to be able to get involved in many fields.

Victor: And when did you meet Tukey?

David: (laughs) Aaaaah, John Tukey.... I watched him like a hawk! Because he was so interesting generally and so much fun to watch. I had been told about Tukey by Coleman. Coleman had been a graduate student when Tukey was at Princeton. And Coleman told me that I was going to meet someone who, at beer parties, was always drinking milk, he just had a big glass of milk. So I knew before meeting him that Tukey was different. Because at a beer party in Canada you drink beer, that's part of your manhood, or something like that. Princeton; at Princeton you didn't have to take any courses. You could sign up for one and would get an A, even if you never turned up. You had to write a thesis and pass an oral exam, so that was pretty good! So let's see; Tukey gave a time series course. And here was this person, unlike any other person I had ever met. He was from New England, very Canadian in a lot of ways. He had pride in his background. He was careful with money, and he had apple pie for breakfast. So I went to his time series course and this involved a lot of Fourier analysis—and I had a strong background in trigonometry and that made the course attractive.

Victor: Did you attend any of these courses along with David Freedman?

David: Oh yes! David F. was a year ahead of me, and he was influential on me (pauses and reflects for a moment). I guess, oh my, most of these people are dead now, goodness. OK, whatever. I have these two stories about David, one involving Frank Anscombe and the other John Tukey. Now, David was a year ahead of me at Princeton. He was from Montreal, I was from Toronto so we were natural "rivals," right from the beginning! That's just the way it was. Of course I don't mean that in a bad way. Anyway, Frank had asked David F. to be his teaching assistant in a course. And David said, "but I am on a scholarship, I don't have to do that!" "OK, fine," said Frank, and then Frank asked

me (laughs). And I knew what David had said, and got to give the same answer! David analyzed a lot of situations very clearly, and I observed David as I do a lot of people.

David F. never changed in terms of his intellectual calibre and wit, and the character of his questions. David was also in Tukey's time series course. Early in the term Tukey used the word spectrum several times. And David after, I don't know, 20 minutes or some such, asked what the definition of a spectrum was. So, Tukey said something like: "Well, suppose you've got a radar transmitting signals up and it bounces off an airplane and a signal returns ... so you see ... well that's a spectrum." So, David's manner was "Well, ok." Then the next class the same thing happened. Tukey mentioned the spectrum, David wanted a definition, and Tukey said, "Well, suppose you have a sonar system and it bounces a signal off a submarine, or some such"... David never came back (both laugh)!

That was really pure David F., wanting clear explicit definitions. Tukey and David were the opposites of each other. You see, Tukey believed in vague concepts. He believed that if you tried to define something too precisely, then you would have lost important aspects going along with it. But David didn't think that you could talk about things properly unless you were completely clear. Of course, Tukey's and David's great confrontation was over census adjustment. I picture that David took a strict interpretation over what was required, while JWT was after an effective estimate of the counts. It is no surprise that David was debating champion at McGill. He surely could have been a fine lawyer, and then a judge, and then....

Victor: He did get involved with statistics and the law.

David: Yes, he was involved in statistics and economics, too. He worked at the Bank of Canada for a while. I think he might have expected that he would be going down that road. He probably thought that being a statistician you can do anything you want to—that was my own reason for choosing statistics.

David was a very sweet person. I am thinking just now of his taking Lorie and me out to dinner in a nice Princeton restaurant after we got back from our honeymoon.

Victor: Going back to Tukey, what did you learn from him as a researcher, what was his style?

David: I learned that there are novel ways to solve most problems. I think JWT could add two four-digit numbers in ten different ways that no one else in human history would ever have thought of! I mean he was like Richard Feynman. He was of the same ilk. There are people, and there are lots of historical examples, who just think differently than almost everyone else. Also what I have learned from Tukey is that there is a physical interpretation of so many of these concepts when you look at the history of mathematics. That's what I tried to bring up in my talk this morning about how some of these things came out of Kepler and Lagrange and so on (David was lecturing on SDE modeling of random trajectories using potential functions). That you can understand a lot of this contemporary work if you think about how it had been generated in the first place. I think Tukey often found himself explaining things to people who didn't know much mathematics. I paid attention to how he did that. I would like to think that I'm not bad at doing that too. In a sense, you probably lie a bit, I mean you probably use an analogy or a metaphor at some point, which is not quite right, but people get the idea.

Victor: That's the advantage of vagueness.

David: Yes, indeed! Tukey's vagueness meant, for example, that we could start out with standard errors and later find ourselves talking about the interquartile range, just letting the idea of "spread" be vague.

Victor: What was your relationship like when he became your advisor?

David: There were lots of good problems around Fine Hall and the Labs that I worked on. Eventually, JWT suggested a particular one. The deal seemed to be that if I started to have trouble, I should go see him. Maybe his not being around town often was part of the breaks in our meetings. When I would meet him, if I seemed a bit too cocky, he would knock me down; and if I looked discouraged, he would build me up. My thesis concerned formalizing Gauss's delta method by working with truncated random variables asymptotically. Another thing was that during the school year I had the day-a-week job at Bell Labs, so often I drove back and forth to Bell Labs with him, sometimes in his convertible. During those drives, we talked about a lot of things. Sometimes, there were other passengers too. I learned while working with him that, when he used some new word, I shouldn't worry about it. I should just let him talk a while and then try to figure out what it was all about. I think a lot of people had a hard time understanding what he was trying to get at. I would eventually come up with something; now if it's really what he meant, I don't know. I'd say I had a wonderful relationship with him (Figure 6). I would kid him—I mean I didn't know you shouldn't tease professors

FIG. 6. *David with John Tukey at the NBC Election Centre in* 1962.

until much later! Because I was working class Canadian and had my uncles as role models. That's how they'd approach people. Not mean teasing, just seeking a smile. I have also teased David Cox. David was patient with me.

Victor: There was good chemistry between you, then. Because, you know, he was relatively conservative and you've been pretty progressive and open about it all along.

David: There was, yes sure. We could talk about things just like that. No tension. He was on the conservative side, true. But it was more about different cultures. He was American and I am Canadian. Canadians are progressively conservative. In those days, there was a conservative spirit in Canadians when it comes to the way one dresses or the way you talk to other people. So, there was conservatism in me, but it was social conservatism, not political conservatism.

Victor: Well, it would appear that Tukey had a very high opinion of you. It has been rumored that he used a "milli-Brillingers" scale to measure people up?

David: (laughs) Yes, I have heard that from several people, including Mike Godfrey and Bill Williams, but what does one say? Bill told me that once Tukey asked about a prospective student, "How many milli-Brillingers?" Bill's reply was "four or five hundred mB's." John responded with something like, "Well that's very good." I don't know, I guess that I was quick on my feet, I don't mean at running. If I had to do something, I would go and do it.

Victor: What about Sam Wilks whom you just mentioned earlier?

David: Sam was wonderful too. He was just a gem. It's a shame that he died way too soon. One story is that he was taking shingles medicine and drank some alcohol that night and there was a bad synergy. Another is that there was an unpleasant meeting over the admission of a student to the program. Sam was conservative politically, but that was never an issue. He had me work on these problems in the draft of his book as I mentioned. I also sat in on the course that was based on the book he was writing. He was a social animal. I can tell you one story. The Tukeys—God knows for what reason—had decided to have a come-as-your-spouse party. So Lorie was supposed to dress like me and I like Lorie, and so on and so forth, Mrs. Tukey like John Tukey, and John Tukey like Mrs. Tukey. That happened, but Gena and Sam Wilks came along as themselves! Near the end of my studying, I went off for an interview at the University of Michigan, before I knew whether I would receive a postdoc. Jimmy Savage was there then. I told him about the party. And I think he went like this (David holding his chin down) and said, "I know too much Freud to ever do something like that!" I didn't know a lot about Freud and I still don't know what Savage meant, but he did know a great deal about a great deal of things.

Victor: So how did you meet Lorie?

David: Blind date! And we're both proud of that! One has to take risks sometimes. She went to Antioch

College with its work–study program. She was studying sociology and had taken a statistics course using Mood and Graybill—not an easy book. She was in Princeton in the "work" component at the commercial side of the Gallup Poll. The Riehms introduced us. Carl was in mathematics, eventually becoming a professor at McMaster University, and Elaine was also working at Gallup. I think her and Lorie's desks were next to each other. The Riehms were often trying to get Lorie and me together, but Elaine kept complaining because I was always out of town! I went back to Toronto a lot—no course responsibilities, remember? Lorie was attractive and we found lots of things to talk about. Anyway, it was a blind date. And, I don't know, we just hit it off quickly! One thing that I loved about Lorie was that she was very political—my politics weren't well formed at all yet—and she was also very analytical. Her parents even more so! Later, we realized that we each had a parent who had been born in China, the child of Methodist missionaries.

Victor: What a coincidence!

David: Oh yes! They were, in fact, in the same part of China: Sichuan province. And now with the web, you can find surprising things. So, I entered my Brillinger grandfather's name and her Yard grandfather's name, into Google, and then found them in the same book (Bondfield, 1912)! Lorie's grandfather was in an American missionary and my grandfather was a Canadian medical missionary. Her parents were very political and they had a huge wealth of political literature. Probably like the literature you, Victor, grew up with. I was a bit shy with them, and since they had all these magazines and books on the coffee table, I could always check something out while I was listening. So, there was a very political side to it all, too. Anyway, we fell in love and it's been good. Almost 50 years now! People often say about us that we don't need to talk, that we just simply communicate. Lorie changed her career goals quite drastically after meeting me. If she had returned to Antioch College, then I would have gone to Yellow Springs with her, probably to teach statistics. But in the meantime, I completed my Ph.D. and had applied for a post-doctoral fellowship at London, which I was awarded. Lorie decided she preferred to go to London. She was actually studying British Trade Unions at Oxford when I asked her to marry me, so she got back to England quite quickly.

Victor: Indeed, you really dashed through your Ph.D. in less than two years! How did that work? Did the lack of coursework requirements have anything to do with that?

FIG. 7. *David in his Princeton Ph.D. Regalia in* 1961.

David: I don't think so.

Victor: I guess that your "milli-Brillingers" had!

David: (laughs) Aaaah, I don't know, I guess Tukey gave me a problem, and said, "see what you can do with it." So, I graduated that following May (see Figure 7). Why didn't he give me something like Fermat's last theorem, I don't know! But I actually had a try at proving that in high school. I read a lot of the history of mathematics.

Victor: I suppose nowadays in Berkeley, as well as many other US universities, there is quite a bit of structure with a lot of coursework and exams. How do you compare those two different systems?

David: Well Freedman and I talked about that once. And we agreed that we would not have gone to Berkeley, which is pathetic. But that's the system. Plus, Princeton was very selective when I went there, I think, two statisticians admitted each year.

Victor: David Cox once told me that the less structured approach is appropriate for the very brightest of students.

David: Yes, I think so, but I certainly don't claim to be a member of that group.

Victor: What do you think happened with the Princeton group?

David: From hearsay, I think I can make a reasoned guess. Tukey was a dominating figure. I know he had tremendous respect for Sam Wilks, but I'm not sure about some of the other people there. Also, he had the mathematicians to contend with. Yet, he needed people. He asked Don Fraser various times to go to Princeton, he asked Art Dempster various times, he asked me several times. Clearly, I can only speak for myself. I just wanted to do some things that were mine. It sounds selfish, but Tukey was so dominant and so quick. I don't think that he thought any less of me because I refused. A lot of people were afraid of him. For example, if they had a cockeyed idea, he didn't mince words. He told me once that he thought the best way to get a scientific discussion going on something was to start an argument. Now that's just the reverse of my personality. I did see him do a lot of that. It was possible he wanted to get beyond the early pleasantries that go on. He did run over quite a number of people. He liked to argue and expected to win. I think that he wanted to win because he had a goal and wanted to get there quickly. I did love interacting with him during my thesis research. I found I could communicate very easily with him. But still, I felt a need to do my own thing. Princeton did get a viable group at one point, and it became a department. The members included Geoff Watson, Peter Bloomfield and Don McNeil. They each had a definite presence in the statistics world. However, I think that Peter Bloomfield just got fed up with being Department Chair. So he went off to a large department at North Carolina State. And McNeil went back to Australia. Also, I gather that Watson was treated quite terribly by the Mathematics Department. I was very sad when Geoff died for he had spoken truth many times. Eventually, Tukey was the only senior person left and when he retired the department went away. So, it is a sad story, but part of Princeton's strength in statistics was that the people it was producing for many years came through mathematics, so there was no messing with them in terms of mathematical stuff, but yet these people wanted to apply mathematics as opposed to doing research in some mathematical specialty. To deviate from the present topic slightly, I have long found classical applied mathematics a bit boring and old-fashioned, but I do know that Fisher wrote that, "Statistics is essentially a branch of Applied Mathematics" (Fisher, 1925). Nowadays, one might say that statistics is a combination of applied mathematics and applied computing, the two driving the field. A Princeton review committee was set up, and recommended against continuing the Statistics Department, and that was that. But I did have a lot of fun at Princeton.

4. BELL LABS

Victor: Could you please tell us a bit about your summers at Bell Labs?

David: The first summer in grad school, there was a group of us from Princeton that had summer jobs at Bell Labs. I would drive up there with my friend Carl Riehm, an engineer and a logician. I don't know if the Labs had this program to find future employees or if it was just a good deed for science. I had learned some computing at Toronto on their IBM 650. Toronto had these computing services very early on, for example, they had a Feranti from the mid '50s. So, I had started out learning computing in a course in the physics department. This was before Fortran existed, so we were using machine language. Princeton had a 650 also, which I didn't really use that much—I guess I was a lot more interested in group theory then. But when I went to my summer job at Bell Labs, they had an IBM 701. Fortran got created and so they had me programming various things for Tukey. That was pretty much the story during my first summer; it was nice to make the money. Then, the second summer.... Let's think.... I guess the second summer Lorie had appeared on the scene! So, we had a lot of fun. I think that's when Tukey had me writing some programs involved in discriminating earthquakes from underground explosions. He was then involved in the Geneva negotiations for a nuclear test ban treaty with the Russians. Tukey had one of those out of the box ideas, the cepstrum. He thought this might also work for pitch detection. That's what I was doing. Specifically, taking speech signal, digitizing it, doing things to it on the computer, then reconstituting it and listening to it. Really, the spectrum and a lot of these time series things had a real meaning for me at that point. I also golfed a lot. The Labs had a short 3 hole course.

Victor: You got experience with getting your hands dirty with data.

David: Oh yes, right away. I really loved that. But, more importantly, I got exposed to a whole cast of characters creating exploratory data analysis! John Tukey was the leader, obviously. But there were others right up there with him, Martin Wilk, in particular—he wrote some important papers with John. There were also Roger Pinkam, Bill Williams my buddy, Dick

Hamming, Ram Gnanadesikan, Colin Mallows who had a strong influence on me. I was in an office with Colin so that was enjoyable and educational. And lunch was where I became a statistician, really. The whole group of us would go down to the cafeteria and sit around a big circular table. So, lunch was about this communal group trying to help each other with their scientific and statistical problems. Then, people would go back to their offices and do their own things. I mean the old Bell Labs worked wonderfully and it's just pathetic that it went away. There was an open door policy and everybody shared the problem they were working on. We had a lot of fun playing pranks up there, too. You know, it was all a gentler world back then in the early 60s. It had an incredible influence on my becoming a statistician because really they were creating a lot of applied statistics. I was very lucky. I mean I got onto a pretty good escalator going up. You don't realize at that time how special it all is scientifically and socially. When I've talked to some of the other Bell Labs people, we've all said, "Those were magic years," and that we were so lucky to be right in the middle of them. Bell Labs was clearly years ahead of people in digital signal processing. Tukey coming up with the Fast Fourier Transform was just part of it. He was working on EDA methods too....

Victor: Did you "witness" the FFT being developed?

David: Tukey's form, yes. In his time series course, John had some way of doing it by complex demodulation. Filtering this and filtering that and then putting things together. But one day in '63, he turned up at a class with an iterative algebraic approach to computing the discrete Fourier transform for the case when one could factor the number of observations into a product of two integers (Tukey, 1963). It turned out that F. Yates and I. J. Good had a related way for getting the effects in factorial experiments. The FFT idea switched a lot of Bell Labs effort from analogue to digital signal processing. It was wonderful to be there. It gave me things to do in statistics. The people involved got to be five years, maybe even more, ahead of the rest of the world.

5. LONDON SCHOOL OF ECONOMICS

Victor: How did England come about?

David: Well, part of the Canadian educational perspective—and maybe you felt this too even though you are from Greece—was that your education wasn't complete until you spent some time in England. It was that simple. So, I finished my doctorate, applied for a postdoc and got one! And then Lorie and I were off to England and to the London School of Economics. Actually, come to think of it, I've applied for only one job in my life that I wasn't offered. See I've been in the Navy, and then Lorie and I met up. She had strong political beliefs and I had strong social ones. Both of us were concerned with doing things about poverty and helping the developing world. So, I applied for a job at the United Nations—they were advertising for a statistician. Didn't even get interviewed! Didn't get it! Sometimes I think of how different our lives would have been. It is impossible to know, but things have certainly worked out.

Victor: ...for statistics definitely, but maybe not so for the United Nations!

David: (laughs) Sample surveys, I think that's what they were looking for.

Victor: But you've been involved in the International Statistical Institute, which has this attitude of solidarity too.

David: Oh, yes, definitely! That's been traditional and I'm glad I've had the chance to get involved in that. Anyway, England was about completing my education and I guess something led me to the London School of Economics. I am not sure just what it was, but that was wonderful. Because Kendall had just retired but was still around, Jim Durbin had just become a Professor, Alan Stuart was about to become one too, Maurice Quenouille was a Reader, Claus Moser was a Professor, as was R. G. D. Allen. I was surrounded by these senior people who were right in the middle of analyzing fundamental economic and political structures. It was pretty good, exciting even. They used to call these grants "post-doctoral drinking fellowships" (both laugh). Lorie and I bought a Renault Dauphine and we went all over Europe. It was pretty cheap and safe then. Fred Mosteller wanted to offer me a job at Harvard when I came back, but he could never track me down. We were traveling to Austria for skiing!

Victor: Was there any difficulty in adjusting to the British view on statistics, having been raised to the American attitude?

David: No, not really. I mean in Toronto then there was a very British background culture there. Dan DeLury was a common sense person who said once that he reread Fisher's Design of Experiments every year. I think I was different from the other British statisticians at the time, however, as I knew a fair amount of mathematics. Nowadays there are a lot of British statisticians who know a lot of mathematics. I'm afraid

it sounds like I'm boasting too much just now. I saw Jim Durbin one time and he had some paper. He said he had tried to figure out something in it a few times but failed. He asked me, "David can you explain this?" I could tell at a glance that it was incorrect and said so. Jim said, "I wish I had your confidence." What he didn't have was my training, that's what the difference was.

Victor: Did you enjoy the RSS meetings?

David: Very much. I had never seen anything like them before in my life. There were people like Jack Good. He would stand up and be coming from a totally outside-the-box angle. I respected that because I had seen Tukey doing that all the time. At this point in my life, I believe that I have read most of Good's papers. I was honored to be asked to speak at his 65th birthday. I paid a lot of attention to what David Cox, Maurice Bartlett and George Barnard had to say, in particular. The way the meetings worked back then was that people could get the galleys of a meeting's paper before it was presented. So, you could compete with all these famous guys. You could read the papers and see if you had something to add to the discussion. That was a lot of fun. I'm not sure whether they do that now. I mean there certainly are discussions that go on. Back then, it seemed mostly in a spirit of friendliness, but now there seems to be real antagonism in the discussions as well as in referees' reports. They would make some strong remarks, but I wouldn't say they were mean then. Being a postdoc in England in the early sixties was great. We had a wonderful time. During the summer we went to the International Congress of Mathematicians in Stockholm. I found that I was reasonably well prepared for the level of the talks, having been to the various Princeton and Institute for Advanced Study seminars. It was exciting to see faces attached to many of the names that I had only read before. Hadamard is one I can mention. I went to one lecture in Stockholm—I think it was Linnik's. I got there early and talked with him. After I sat down, in comes Cramer, who sits right next to me! Then, in comes Kolmogorov and he sits on the other side of me! (both laugh) I was speechless! As you well know, I am usually quite talkative. I guess that I could have asked for autographs. That would have surprised them I am sure. Sadly I don't have a photograph to preserve the moment. It was pretty special and perhaps justified my having gotten a doctorate.

Then, we went back to Princeton. Lorie was pregnant so our life was going to change a lot. I went back to a job that was half time at Bell Labs, as Member of Technical Staff, and half time as a Lecturer in Mathematics at Princeton, teaching. The two positions were complementary in important ways. Tukey had created such a structure for himself; however, he was probably half-time in Princeton, half-time at Bell Labs and half-time in Washington. I guess that I then set out to have my own research career. I had done some writing of papers before, but now I settled into a more adult research program.

Victor: You seemed to be quite spread out at the time, I can see stuff in asymptotics (Brillinger, 1962a), Lie group invariance (Brillinger, 1963a), fiducial probability (Brillinger, 1962b), resampling (Brillinger, 1963b).... Really going off into many directions.

David: Well that was based on material I had learned. I would pick up a journal and see somebody had done something and if I thought there would be a way to contribute, I would try. The Lie group material was motivated by Don Fraser. He was creating this area he called structural probability. I was trying to see if fiducial probability could be more formalized. R. A. Fisher kept pushing the idea of fiducial probability. It seemed as if in all his examples the fiducial probability was a Haar measure. So that was a natural thing to do. The Lie group paper arose also because people had wondered whether or not working with the correlation coefficient would lead to a fiducial distribution. I showed there was no prior—at least no Lie group measure that lead to one. But I was still solving problems, minor ones I suppose.

Victor: You mentioned reading papers and thinking about problems. I remember reading Tukey's *Statistical Science* interview (Fernholz and Morgenthaler, 2000) where he said that he would pick up journals and read papers, but not really study them. Which did you do?

David: I think I read them over. Because I had a reasonable memory and I could read quite quickly. So, a lot of my life has been working on something and then suddenly thinking, "Oh, yes, I've seen something like that before...." That's a problem with changing universities: because in the Princeton library, I might have picked up some journal, but then having moved on to, say, LSE, I had to search seriously. Anyway, I would pick up some journal, and read a paper that I sought in it, then, just as I was taught to read the dictionary, I'd look at the paper just before and the paper just after. That way you build up your knowledge. Also, when I have a journal issue in my hand, I don't think I read it to study it; rather, I read it to enjoy it.

Victor: And then came the baby and a decision to make: moving back to England.

David: Yes, that's right. Returning was an easy decision. Because Lorie and I both had loved living in London. Her being from New York city, and me from Toronto, we were used to, "Which movie do we want to see? Then, where is it showing? OK, let's go!" Princeton was a small town and Lorie felt pretty restricted. Now we had the baby at home, but her parents lived up near New York City. I think it was pretty hard for her. Now women do keep working, albeit part time or volunteering. But back then, they were right in the middle of the world, interacting with many people and ideas. Then, all of a sudden, they were at home for many hours with a baby. Well, Jim Durbin wrote me about there being a lectureship at the LSE, and was I interested. I think Lorie and I just had to look at each other for a moment to know we were interested. I stayed at Bell Labs through that summer to finish some projects and to build up some savings to go to England with. We had a VW van, so we were ahead of the hippies, and we shipped it over with us. We were driving around London for six years with this left-hand drive big red VW van.

I have remarked many times that Bell Labs was the best job I had had in my life. Stimulating facilities, stimulating colleagues, stimulating problems and minimal restrictions on what one worked on. It is just that Murray Hill was in the middle of New Jersey. We were very fortunate to have the opportunity to decide how important was the choice of job as compared with the choice of where to live. My salary went down considerably of course.

Victor: What was life as a lecturer at the LSE like, and what was the contrast with Princeton?

David: Well, there were students of both sexes in the classroom at the LSE! They were left, not rightwing. In both cases, the students were very bright. Bill Cleveland was in a class that I took over when Sam Wilks died. Princeton and LSE were very different in many ways. I did prefer the English system in important ones. The thing I remember most about LSE is that there were five, perhaps six of us, who were lecturers at the same time. We were of about the same age, having kids at the same time, watching the same TV programs. When Monty Python came along, we would all be talking about it the following Monday morning. They were teaching me about football/soccer and were learning about hockey and frisbee from Alastair Scott and me. We pretty much have all had successful careers. Fred Smith became the President of the Royal Statistical Society, Alastair Scott went back to New Zealand and was elected to the Royal Society of New Zealand, Graham Karlton moved to the Survey Research Center at the University of Michigan and became prominent in the US survey community, Wynn Lewis died young, Ken Wallace, the econometrician among us, was elected a Fellow of the British Academy (Most of the LSE statistics group in Fall 1969 are pictured and listed in Figure 8). We were all together, all the time. We would go to the morning coffee, then have lunch and then afternoon tea again together. We drove across and around London to visit each other. At Princeton I was pretty much alone as a young person doing statistics.

Victor: But did your decidedly mathematical outlook tie in well with what was expected to be published in the British stats journals at the time?

David: I think that I know what you have in mind with that question. Just before we moved to England, I had submitted a paper to the *Series B* of the *Journal of the Royal Statistical Society*. It wasn't all that complicated, it was doing factor analysis with time series, getting latent values of spectral density matrices. I had in mind the problems Tukey had had me thinking about, concerning a signal from an earthquake or an explosion coming across an array of sensors. In an appendix, there was a derivation of approximate distributions of spectral estimates using prolate spheroidal functions, which Pollack and Slepian had come up with (Slepian and Pollack, 1961). The referee said he didn't understand it and the paper was rejected! And I mean back then I didn't know about protesting an Editor's or Referee's decision. I probably should have rewritten it and sent it back to JRSSB, but what does it matter? I did give a talk at an RSS meeting. Eventually, I put it on my website, and it's still there now. I developed the dimension reduction aspect further and have a paper on that in one of the multivariate analysis symposia and a chapter in my book. I don't think this occurrence affected me too much, but some of my students have been very disappointed by similar things in their career. Best I can tell them is that parts of life are arbitrary, resubmit.

Victor: By that time, you had been doing quite a lot of work on spectral analysis and then in '65 came the influential paper on polyspectra. That sounds like a Tukey term.

David: Yes, that is a Tukey term. One of the first things Alan Stuart said to me in London—you know how picky the English can be—was, "David, poly is a Greek prefix and spectrum is a Latin word. You are committing linguistic miscegenation!" He was just teasing me. But in Volume 1 of Kendall and Stuart

FIG. 8. *The Statistics Department at the London School of Economics in Fall* 1969.

(Kendall and Stuart, 1963) they say this against Tukey regarding "*k*-statistics."

Victor: Surely, there are many such examples—I can think of the word *bureaucracy* off the top of my head....

David: ...there's another thing that's wrong with bureaucracy! (both laugh) But anyway, I mean I was into all this nonlinear stuff. Tukey, in an early memorandum, had done something on the bispectrum. So that motivated me to do some research. You know, when you have a math background you seek to generalize things, to abstract them. It turned out I was unknowingly at first competing with the Russians—like Sinai and Kolmogorov—when I was doing that work. I heard that Kolmogorov had said some nice things about my work from Igor Zurbenko. That was really nice. Later on, the Russians translated my book into Russian. I learned to read Russian mathematics in a fashion, in particular, the works of Leonov and Shiryaev. That's what got me into the ergodicity results. For example, what I talked about today was the Chandler wobble. Arato, Kolmogorov and Sinai had a paper using stochastic differential equations to explain that motion (Arato, Kolmogorov and Sinai, 1962). I was strongly influenced by French mathematics and a lot by Russian probability. I read the journals of both regularly. The work on cumulant functions and polyspectra let me get away from the restrictive assumption of Gaussianity in much of my later research.

Victor: Then, into the picture must have come Murray Rosenblatt, judging from your three joint papers on higher order spectra (Brillinger and Rosenblatt, 1967a, 1967b, 1967c). I suppose he was in touch with the Russian school.

David: Oh yes, for sure. I had met Murray in New Jersey when he consulted at Bell Labs in 1963. I remember they had him working on the cepstrum, which is the inverse Fourier transform of the log of the spectrum. That work was part of estimating how deep earthquakes and explosions were, and so on. Then, Murray came to London. And again, I didn't know I shouldn't do something like this, being a young jerk, but I just went up to Murray and said something like, "How about we write a paper and do some work together?" And he said, "Fine." Murray has been my statistical role model, in many senses. Tukey was a creative role model. But at one point he said, "Well, David, now that you are finishing, what do you think you want to do?" He might have thought that I still wanted to become an actuary. What just came out of my mouth was, "I really don't want a life like what you have and I am concerned about whether I want to be an academic." And then Tukey put his hands on his chin as he would often do and said, "What about Willy Feller? He has a

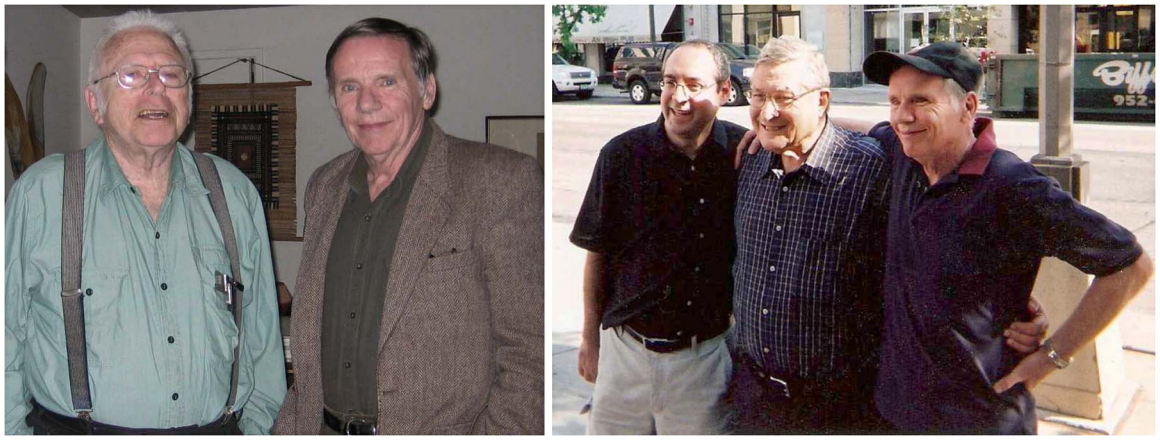

FIG. 9. *David with Murray Rosenblatt, and with Emanuel Parzen and his son, Michael.*

pretty good life." So, he found a role model more to my liking. But then, I found Murray Rosenblatt. He just seemed to love his wife and his kids and had a lot going on in his life outside academia as well as a fine academic career. So he was a good role model. I don't think I really managed to express that to him until Richard Davis and I interviewed him for that article in *Statistical Science* (Brillinger and Davis, 2009; see Figure 9). He was a lot more of a mathematician than me, but in terms of his life, and interacting with people, I respected him.

Victor: Am I right that you also met Emanuel Parzen in England?

David: Oh yes, and we've been continually in touch since (see Figure 9)! We also met the Chernoffs then. This year, 2010, Manny and Carol are moving back to Palo Alto to a retirement home. So we expect to see a lot of them even though Palo Alto and Stanford have gotten steadily farther apart during our Berkeley years, in part because of the growth in traffic. But, with the Parzens moving there, I expect Palo Alto to come much closer. Manny and Carol are role models for us in different ways. One is being a loving couple that were equal, with each member of the couple helping the other. And the other is Manny certainly helped me a lot by getting invitations to conferences, and by describing research that someone else was doing, so I was being kept up. And I think also by describing my research to other people. He was really the troubadour who was carrying the information of what was going on in other places around.

Victor: While maintaining a very strong concentration on cumulants and polymeasures, you also did some things on economics on the side.

David: Bell Labs had a lot of signal processing, so I was going into spectral analysis in detail. I think Kolmogorov and Sinai defined cumulant spectra in some sense, or cumulant functions. These functions turned out to provide a natural way to describe ergodicity and asymptotic independence. That's what I grabbed on to. That was the '65 paper. I think I might have been the first one to show that spectral estimates were asymptotically Gaussian without assuming that the time series itself was Gaussian. The economic work started in Princeton. Clive Granger—the Nobel prize winner—was at Princeton before I went to London. He and Michio Hatanaka were working on a book on spectrum analysis of economic series with John Tukey providing advice. When I moved to England, Clive was also there, at Nottingham, and would come down to the LSE every so often, so we had some contact over important periods. Hatanaka and I began working together and wrote a paper (Brillinger and Hatanaka, 1970). I presented the work as an invited talk at the First World Econometric Meeting in Rome in 1965. Milton Friedman made the invitation. The work was concerned with the permanent income hypothesis and we had developed a time series spectral analysis formulation. After the talk, Friedman came up and said something like: "I didn't understand any of that but I am sure it was good!" (laughing) There is another paper with Michio (Brillinger and Hatanaka, 1969). Data analyses were involved. My period at the LSE was by far the most theoretical in my career. I think because the time series data just weren't there. I was working as a consultant with the seismology group at Blackness. It was an offshoot of the Aldermaston Atomic Weapons Research Establishment outside that base. At one point, I provided an effective scheme for them to use with array

data, but I guess that I wasn't able to explain it well enough. That's often been the story of my ideas. I don't know, Manny Parzen once quoted someone as saying, "First you have an idea and then you go out and sell it." But that was never me. I do try to ask myself, "Why am I writing this paper?" In the end, I think that I am writing for John Tukey.

Victor: You've often mentioned the influence of scientific heroes.

David: Feynman would be one. I have read a lot by him and about him. I know that he enjoyed going to Brazil, as I have.

Victor: You didn't have a chance to meet him at Princeton, though.

David: No, he was long gone. He was there in the early war years, and left during them for Los Alamos. He ended up at Caltech. When I was asked to give a talk in Caltech once, he had died before. I might have been too intimidated to go talk to him anyway. Although I did talk to.... Goodness, probably you know the name better than me. Who's the MIT linguist, who is in the news all the time?

Victor: Chomsky?

David: Yes, Chomsky! I took Chomsky out for coffee once. It turned out that he and Tukey had organized a seminar on linguistics at the Institute for Advanced Study. This was when I was doing all these memorial articles about Tukey (Brillinger, 2002a, 2002b). I had noticed that Chomsky came to Berkeley regularly. So, I called a mutual friend and asked if they could arrange for a meeting next time Chomsky was in Berkeley. They did. Eventually, I met Chomsky at the linguistics department and took him over to this coffee place run by Palestinians. Victor, you have been there. While we were there, all these people were looking at Chomsky. One woman couldn't resist expressing her admiration for his work. He was such a humble, sweet person. I asked him whether Tukey had any impact on the seminar. Chomsky said he sat there and grinned. I guess one takes that for what it is! So, being a Tukey student has given me entrée to countless situations. I'll tell you a story concerning that: just as I was finishing my studies at Princeton, I was invited to speak at the University of Michigan—I am sure due to Tukey interacting with Jimmy Savage. Jimmy Savage did a bit of political analysis of Lorie and me, and decided that our politics were on the left. He quickly organized for us to meet with Leslie Kish, sociologist in the Survey Research Center. That's when our close friendship started.

Victor: Leslie Kish had fought as a volunteer in the Spanish civil war.

David: That's right, and he was a leader of the Campaign for a Sane Nuclear Policy. So, Leslie had come to London and was giving a talk somewhere there. He later told me that he saw that I was in the last row doing something else. He said he got annoyed, but then immediately thought, "Oh no, he is a Tukey student, so that's all right!" (laughs) Now actually I was listening! Tukey could do three things at a time, I could maybe do two, sometimes.

Victor: Another name you often mentioned is David Cox.

David: Oh, yes, he is another hero of mine. He too visited Bell Labs when I was working there. He was not a professor yet. He clearly had special things to say. Others might have done some of the things he did in a more mathematical way and subsequently gotten their names attached to them. I don't think he had a problem with that. I am thinking of things like getting approximate distributions of maximum likelihood estimators when the model is incorrect. He did that early on in a Berkeley Symposium paper (Cox, 1961). Then, in another Berkeley Symposium, Huber came along and did it in a more formal way. Cox's paper has a wonderful statement, "Discussion of regularity conditions will not be attempted." There were very few, if any, of David's talks or papers that didn't have something clever in them. It's as if when he did something, if there wasn't anything clever in it (David thrusts his hand as if throwing away a piece of paper), then, no! Out of the window. He does it all in a very humble way. I have been on several committees with him and he would say few things for a while, but he would accumulate information and then he would come up with a proposition: "Well you could say ... maybe we could do...." And everybody would agree. He could merge a lot of different opinions and information. He is one of my statistical heroes. He did reject a couple of papers that I submitted to *Biometrika*. I took that as saying, you can do better.

6. GOING TO CALIFORNIA

Victor: I understand that you would have been very happy to stay in London, but then things changed.

David: Yes, well my mother retired. She had had a hard life. She was a very bright woman, but because my maternal grandfather died in the great flu epidemic leaving my grandmother with five children, my mother had to go to typing school to help the family survive. Many years later, she went to adult school and got to be a country schoolteacher. We were sending her some

FIG. 10. *David with Lorie along with David Blackwell and Maria Eulalia Vares.*

money, but when she retired her pension was tiny. Even though I had become a Reader at LSE, there was just no way I made enough to make up what she needed. We had Jef and Matthew at that point, we were living quite happily, had a nice house a block away from Wimbledon Common. We were going to the theatre and concerts regularly. But there just was no way to be able to also support my mother. So I had to look for a higher income. Berkeley had already invited me several times. Actually, David Blackwell had called me just before I finished at Princeton (see also Figure 10). Now in the late sixties Berkeley was the place to be, with the free speech movement, rock concerts, experimentation in the arts and all that. We had learned that when we were there on sabbatical in '67–'68. There were a growing number of protests against the Vietnam war, and Lorie was quite involved. So we knew Berkeley, and they knew me. And when Henry Scheffé asked me about moving there, we agreed. A person high in the academic totem pole told me once that a senior department member had said that I was the most influential appointment in the '70s. There were lots of mathematical things going on and I enjoyed that, but I was strongly interested in applications of mathematics. I immediately fell into place with Lucien Le Cam and Jerzy Neyman and all their visitors—they had a lot of important ones. So, we left London because we needed a higher income, but we landed in a very special place. Our older son, Jef, loved England. He was very sad about the move and that made Lorie and me sad. I think we expected that eventually he would move there.

Victor: So tell us a bit about your early Berkeley years.

David: The earliest years were '67–'68 when I was a visitor on leave from LSE and we have already talked about them. We moved to Berkeley permanently, arriving by ship, in January 1970 to be met by Erich Lehmann on one of the piers. At that time, there were a number of individuals who were then Assistant Professors but who did not get promoted to tenure, that is, had to pack their bags and leave town. They were able academics so their nonretention was quite a shock for me. Actually, it seemed inhumane. Some of these people had children already at school. I was used to the English system where, if you were a Lecturer, and you had passed across the bar after three years, then you had tenure. You would hit the top salary of the lecturer scale but you might stay in your department the rest of your career—you had tenure. Some people did take advantage of that. We lost Berkeley friends that we had made and that was a great shock. Apart from that, we were really enjoying the department, Berkeley and the Bay Area. The department seminars and the quality of the discussions in the lunch room were top notch. In these early years Kjell Doksum and his family became close friends.

Victor: Did you thus quickly forget about London?

David: No, not really. In fact, when in 1971 David Cox wrote that a professorial chair was available at Imperial College, and asked if I was interested, I was very interested! But going through the sums, with Alan Stuart's help, we just could not afford to return. Our old

house was now worth more than twice as much as we had sold it for, within that short period. We couldn't afford to buy a comparable house.

I have sometimes wondered how things would have worked out with Jef's brain tumor had we returned. Cormack had just developed the first CT scanner at Atkinson Morley Hospital just down the hill from our Wimbledon house. That technology wasn't yet available in the US, and might have helped.

Victor: But you found data at Berkeley.

David: Yes, I found data and fine applied scientists to work with at Berkeley. On reflection, I had reached the career that Tukey and Bell Labs had been training me for. Soon after arrival, I just wandered over to the seismographic station where I met this Australian fellow, Bruce Bolt. He and his family became dear friends. He was a sailor also, so we spent time on the Bay in his boat. Our families mingled. Bruce was religious, and I was no longer. However, we didn't seem to have the slightest difficulty talking about religion and other serious topics. He got me working on time series and other problems in seismology. We wrote several joint papers, but affected each other's research quite generally.

Victor: Was that around the time you wrote your invited paper on point process identification (Brillinger, 1975)?

David: There is a history to my work on point processes both in London and Berkeley. David Vere-Jones, another dear friend, another influence, presented an Invited Paper at a meeting of the Royal Statistical Society (Vere-Jones, 1970). I was asked to second the vote of thanks. When you are the seconder you are supposed to criticize the paper's content. Victor, you've probably been to these things. So I read David's very seriously. I don't think I had much in the way of criticizing, but it got me very interested in temporal point processes.

At Berkeley, Neyman and Scott had done path breaking work on spatial point processes, particularly in astronomy. Six months after my arrival in Berkeley in January, the Sixth Berkeley Symposium took place. I presented a paper showing a way forward for making inferences based on data for processes with stationary increments (Brillinger, 1972). This included stationary point processes. Around that time I also had a student, Tore Schweder, who was looking into that point process material when modeling whale tracks. To continue the story, while Betty Scott was still department chair she asked me if there was anyone it would be good to invite to Berkeley for a term. I suggested David Vere-Jones. He and Daryl Daly came, and a whole world of point process work got started. In particular, David and Daryl organized a seminar series. Peter Lewis and "Pepe" José Segundo were important speakers. Peter's energy and enthusiasm and broad knowledge captivated the audience. Pepe came with specific problems and data concerning the firing of nerve cells. Pepe was a Professor in the Brain Research Institute at UCLA. And he had all these wonderful data on nerve cells firing. And I just said, well this model that I have been fitting for earthquakes might be good. So then he sent me these massive piles of boxes of computer cards! They took up perhaps 10% of my office for many years! The thing that was interesting was that second-order spectral analysis seemed to be quite effective. So, I was working on point process data from seismology and point process data from neurophysiology at the same time. My students Rice and Akisik worked on these models/data also. The advantage of the neurophysiology case was that it was a designed experiment situation and, thus, you could repeat the experiment. So, that collaboration resulted because I was working on point processes from seismology. To my mind, one of the major successes was that the concept of partial coherency analysis could be extended quite directly to the point process case (Brillinger, 1975), and it let one infer the causal structure of networks of neurons (Brillinger, Bryant and Segundo, 1976).

Pepe had a daughter who died in a plane crash at Puerto Vallarta. At that time, I had a son with a brain tumour that could not be removed. These tragedies brought us very close together. Having a child die is pretty hard. Pepe and I had our scientific conversations to keep us focused on one good side of life.

Victor: Would you like to talk about Jef?

David: (David pauses and speaks with a broken voice.) Well, yes. I mean it really affected Lorie, Matthew and me, as well as Jef's and our friends. We have cared a lot about other people always. I don't believe that it is an accident that Lorie became a nurse midwife or that I started working with nerve cell spike trains. One works to fight for political ideals and to improve the system, but it is totally humbling to care so much about a child and not be able to help them in their time of greatest need.

Jef's illness went on many years. The first hint was in 1968 and he eventually died in 1988. It was not diagnosed as a brain tumor until 1973. He had three bouts of brain surgery and radiation between 1973 and 1988. In 1973 he was supposed to die within 6 months, but

he just kept coming back. The night he died, I didn't think he was going to die. He graduated from UC Santa Cruz in 1988, just two years behind his class. Everyone did everything imaginable. The doctors, his brother Matthew, Lorie and her nursing friends, our friends. The doctors made home visits. Nobody wants to see a child die. Many, many people attended the memorial.

Jef had a motorcycle, just as my mother and father had. I sometimes think about his motorcycle. I knew that I wasn't going to get on it, but I knew about it. Jef rode it back and forth to Santa Cruz, in part over a mountain. Once, there was a heavy rain storm and he thought that he might die. Another time, someone in the back of a pickup truck threw a bottle at him. He could have died on that motorcycle so easily. Then it would have been: if only, if only, if only.... That's what our memories would have been. But our memory is that everybody did the best they could. Including Jef. Lorie has been really hard hit with death. She's had to nurse her dying parents, her son and her sister now.

Victor: Practically everybody who's met you will attest to what an uplifting person you are, how it seems that you are always smiling.

David: Not always but most of the time. Probably my life was all fun until 1973 when Jef was diagnosed with the brain tumor. Science and researching kept me going through those times. Nowadays, I just have to think about my grandchildren and a smile surely appears on my face. Having gone through all this, I do go to a lot of effort to communicate with the Berkeley students about the importance of enjoying every day and realizing how lucky they are. In one of my classes in Berkeley, I realized that I was assigning a great number of problems. What I did at the spur of the moment was to say, "OK, your problem assignment for this week is to go to a movie and then write on a piece of paper the name of the movie you've been to!" I think they just thought I was kidding. I wasn't. I have a hard time convincing today's students to put things into perspective. They seem quite terrified and not having all the fun that I had as a student. They are overly worried about getting registered in a class, about finding a thesis topic, about getting a post-doc, about getting a job, then about getting tenure, about getting a grant, getting to be a professor, getting to be invited to conferences. They have the problems of old people on their shoulders already! I am just sad for them. Things do work out. I hope you're trying to get your students to enjoy life, follow sports, things like that!

Victor: Well, I've had good advice, and try to pass on what I learned. Did research and sport help you at all during that difficult period?

David: When I was recently preparing an encyclopedia article on "soccer/world football"—that was the title I was given—and I was pulling out a lot of books, I found that there was a book by a couple of Russians on applications of mathematics to sports (Sadovskiĭ and Sadovskiĭ, 1993), because it has some material on soccer. When I read the introduction, I found them saying that to do mathematics well, you want to be healthy and fit. I have known this for many years, but it was reassuring to see it in print. I think that participating in sports is important. You know, running around and interacting with others. I think of Shiryaev, since we're talking about the Russian point of view. He is a very good skier. He received a medal for it. There is something specific I'd like to feed into our conversation just now. I played a lot of intramural and informal soccer over the years. One year, two teams the Statistics Department was involved with met in the final. However, I stopped playing after Jef died. I wanted to be alone. Friends would come by my office to try to get me to play, but I just wanted to be alone. But my office looks over the Bay and much of the time I could see people sailing and windsurfing. I thought, "Why don't I try windsurfing again?" I had tried once before and it hadn't really stuck. But when I tried again, I got the basics. Windsurfing is one of those things where if you don't know what to try to do, then you are in big trouble. What I found personally was that if I thought of anything else when I was windsurfing, I would fall into the water. After I windsurfed for 2 hours I was just high. One day when I went back to Evans Hall, I saw Andrew Gelman and said something like, "I windsurfed all the way to Emeryville today!" Andrew said, "Well, I climbed up the outside of Evans Hall today!" (laughs) It was that male thing, if someone is boasting too much, they get brought down. I do recommend to anyone who has some tragic situation to deal with, and they do like outdoor activity, that they take up windsurfing.

Victor: What was it like to arrive at Berkeley in the late 60's–early 70's?

David: Super. Rock concerts, progressive politics, long hair, hippies, tear gas. I was teaching once in a room in Wheeler Hall and all of a sudden there was some strange unfamiliar smell. I didn't know what was going on until someone in the class said, "That's tear gas!" It was really something. There had been "troubles" at LSE, but none with tear gas. I remember one friend I have, especially. When there was something radical going on I was out of there, headed away from the trouble. But I would invariably see him heading the

opposite way, that is, in the direction of the trouble. I did see some bad things. Through my then office window on the third floor in the Physics building, I saw a sheriff's deputy club a young man who was just sitting under a tree reading a book. I think officers were totally frustrated because the demonstrators where leading them in a chase across campus. I do have to say that some were throwing rocks—and that's not cool. The deputies chased but they could not catch these guys. So, they just got more and more frustrated. Here's another story from that time period. Al Bowker had become Chancellor and joined our department. He had to deal with various ticklish situations during his tenure. Somehow, he always found a way. Evans was a new building and its inside walls were stark. One weekend some of the mathematicians came in and painted some murals. There was one of the death of Galois. The custodians cleaned them off. But the mathematicians repainted the murals. A battle of wills was developing. Bowker said just leave them. Long after the murals were painted over when the building was refurbished and I don't know that there was any fuss.

Victor: Al (Bowker) told me a story about some students who were demonstrating. They came into his office wearing dark sunglasses—I suppose it was some sort of statement. But then Al caught them off guard: to their surprise, he was already wearing dark sunglasses himself (both laugh)!

David: I had some fun like that too. When I was department chair, Lorie's brother was working for a video company that had produced a movie titled "Take This Job and Shove It." He mentioned that they were giving away hats with the movie title embossed. I asked if he could get me one of those. He did. One crisis that developed in my chairmanship occurred when the campus wished half of our space back—I confess that Betty Scott had been too effective in getting us space in the new Evans Hall. Anyway, when I went to see the Vice Chancellor I wore the hat and then passed it on to him! (both laugh) We ended up losing a quarter of our space.

Victor: What about departmental life? For example, Jerzy Neyman?

David: As far as I was concerned, being around him was a treat. One of Neyman's goals was "to find a model describing the data." In contrast, Tukey's goal was to "discover surprises in the data." Neyman was more for formalization, whereas Tukey was more for intuition. Surely, both are needed. I saw the two masters of these things at work (Figure 11). I attended the Neyman Seminar regularly and went for drinks afterward. Neyman had a host of really wonderful visitors coming to Berkeley. I had total respect for that man.

FIG. 11. *David with John Tukey* (*left*) *and Jerzy Neyman* (*center*).

Victor: And Neyman was one of the people you had gotten closer with, along with Le Cam and Scott?

David: Yes. For one thing, they were always in the coffee room at lunch time, often with famous visitors eating Neyman's hard boiled eggs. The talk was lively, what with Neyman knowing so much about European history, all his languages and poems, and Betty being so full of heart and caring for people; Lucien being very French in such positive ways. The three cared so much about the students. Surely, the best part of Berkeley has always been the students. Once when I was in the coffee room, with Neyman and Le Cam, a student came in whose father was having a medical problem. Lucien and I were chipping in suggestions. After listening a while, Neyman remarked, "Isn't it wonderful that the professors are helping out the students with their personal problems?" All three would jump to help with student's personal difficulties. They were wonderful. I have been a bit unsatisfied with the Neyman biographies. They don't seem to bring out the essence of the man. I said this to Betty and Lucien once and they agreed. Biographies of scientists, by their nature, seem to focus on the science side. Setting down the human side is surely much harder.

I'll tell you one of the funny things that came to my head just now: somebody asked me once if I thought that Betty Scott and Jerzy Neyman were lovers. My immediate response was, "I hope so!"

Victor: You had been exposed to two of three main schools of thought in statistics: Tukey-esque, British and then came the third: Berkeley. What was that encounter like?

David: I would like to start by replacing "Tukey-esque" with Tukey-Bell-Labs-esque. That's the school that I learned EDA in. OK the encounter. I start by

quoting Le Cam at this point. Once, at lunch, I told him about some research that I had just seen suggesting that cigarette smoking wasn't bad for one's health and at about the same time another report that suggested it was bad. What did he think about that? He replied, "They're both right!" The three schools are all right. We need each. I think it is important for people to travel and experience all three. The RSS meetings, for example, are a way to learn the British school. One meets these people and compares their discussions of the same paper. A lot of things exist in the scientific air, but are not written down, particularly heuristics. And it's very important to have heuristics along the way to nailing a problem down. Often, when you go to another center and are in a discussion, they quickly draw a little diagram and then you have picked that representation up. The thing is that you could go a whole career and never know that something could be simplified that much. As the years have passed, the British statistics school has become a lot more American. For example, consider measure theory and theorems. There have always been a lot of wonderful probabilists in England, but they did not appear to have much influence on the statisticians until recently. One thing that I particularly respect about the English system, including people who aren't famous, is how well they can ask questions. There would be someone at a seminar, and then there would often be someone with a British accent who would put their finger on a crucial point that's going on in the science. Not so much the mathematics, but the science of the situation. I have a lot of respect for that. What was the encounter like? I flitted among each of these schools. I am a scavenger. I have the luxury of trying a Tukey approach, trying a Cox approach and trying a Neyman approach to problems. The Bell Labs group was influenced strongly by Cox, by Kempthorne and by Tukey. They weren't much influenced by Berkeley or Box.

Victor: 1975, *Time Series: Data Analysis and Theory* (Brillinger, 1975).

David: Well, that book has got blood on every page! I wrote it when I was in England during the late sixties. It took too long to be published. I did enjoy working on it. I was going to LSE two days a week. We had a three-story townhouse. I would sit down on the top floor listening to the BBC's wonderful radio programs, working away on the book, while Lorie would be two floors down with Jef and Matthew. In the afternoon, I would be all involved with the kids. It was so enjoyable. The book started from my research, which got simplified for my lectures at LSE. Before reaching Berkeley in my 67–68 sabbatical, we spent the summer in Princeton. Tukey and I were supposed to be writing something up. But Tukey decided to go off somewhere, and there I was at Bell Labs. Ram Gnanadesikan asked me to give a course on time series. Luckily for me, somebody at the Labs was available to type up the notes. This provided a fine start to the book. There were all these wonderful computing facilities. The fast Fourier transform, a fast computer and graphics all came together there. Then I got back to England in the summer of 1968 and I guess that's when the serious filling in of material was done. The manuscript went to the publisher in '72 after I had made a serious attempt to have the references complete. It was printed in '74, but they put a date of '75 on it. It has now been with 4 publishers! That sounds amazing, but Holt–Reinhart gave up their statistics list, Holden-Day went broke, and then it went to McGraw-Hill who put their binding on it but didn't do much else. It is now with SIAM and called a classic. How about that? There were some surprising benefits, like not having to do much preparation for lectures for many years. The thing that I enjoyed the very most was making up the problems at the ends of the chapters. Because I'd be thinking, "Maybe there is a problem sort of like this," or "Maybe reasonable assumptions are something like these," and last "Maybe a solution could go as follows." The thing is one is negotiating with these three different vague items. It turned out that solving a problem was a lot easier than creating one! Victor, I did a vain thing the other day. I typed "Time Series: Data Analysis and Theory" into Google. It claimed to have located 136,000 results!

Victor: You must have taught the time series graduate course "Stat 248" at Berkeley for many years.

David: I think every single year, except when I was on sabbatical. I believe Bob Shumway came then.

Victor: So did you change it quite a bit? I remember sitting in on three different versions.

David: Oh yes. I design it totally differently every year—and no one seems to notice! To allow variable content, I call it "Random processes: data analysis and theory." A couple of students, not you of course, have said they should have come back. I try to tie it in to something I'm excited about at the time. Perhaps trajectories, perhaps point processes, perhaps spatial-temporal data and so on. I think if you are not excited about something, or if it is something you have done a long time ago, it's boring. Nowadays, there are all these wonderful data sets and graphical devices to employ. It can take some time to prepare a display, but it would be a great shame not to.

Victor: You spent some time as a Visiting Professor of Mathematics in New Zealand. I know you are in love with New Zealand, is that when it started?

David: Yes. Alastair and Margaret Scott became dear friends in London. Alastair and I were Lecturers together. We had met at Bell Labs, and when I arrived in London he wrote me wondering if there were any jobs. So, I asked Jim Durbin, and there was a Lecturer position. Alastair stayed a couple of years longer than me. When Jef had the first surgery, he was really set back a long way. We wanted to go somewhere gentle, and that was New Zealand. There, his energy came back and he could do things like play basketball at a boys club Friday evenings and come home alone on the bus. He was about 12–13 years old then. It was the way things had been for me when I was that age. The Scott's friends became our friends right from the start. Alastair and I tried to collaborate on a paper once, but we never seemed to talk statistics. It wasn't that we didn't want to or couldn't, we just seemed to get talking about other things. But I do believe that we have influenced each other statistically a lot. So, New Zealand became our home away from home. NZ is where Lorie and I retreated to in 1988. That year was horrible. Lorie's father died, Jef died and my mother died. It has been important to Matthew, too. When Matthew decided he wanted to do a doctoral thesis in literature on Nabokov, it turned out that the world's expert on Nabokov was in Auckland! To tie the knot even tighter, we have three Kiwi grandchildren.

Another place I have a strong connection with is Brazil. It began in the context of graduate students. I had three Brazilian graduate students pretty early in my career. For many years, they were inviting me to come visit. I would tell them I was not going to any dictatorship. But, eventually, the generals went away and luckily I was asked again. I went that time and had a wonderful visit. Brazilians and Canadians are very similar in many ways it turned out. In particular, they both have very high levels of teaching and research in statistics and, of course, sports are very important in both countries. Then, I got invited to another meeting and Pedro Morettin proposed that we apply for a joint NSF-CNPq (stet) grant. When the grant was funded for 3–4 years I decided it would be rude to have that grant and not make some attempt to learn Portuguese and took two courses. I have given talks in Portuguese there and they have been very patient with me. One of the days that I was most proud of professionally was when I got elected to the Brazilian Academy of Sciences. That was quite a surprise!

Victor: You also chaired the department at Berkeley for a couple of years. How was that?

David: I liked some parts of it, a lot. I got to know the staff very well, which I hadn't before. I got to know all the grad students very well, and many undergrads. I had many pleasant interactions with my colleagues also. But I couldn't do any research. Because whenever I tried to do research, all of a sudden the day became too short or I was interrupted too often. I had agreed to do it for one year. The "candidates" had come down to David Freedman and me. David Blackwell said, "Well, it's you two. Time to choose." David and I each agreed to take it on for one year. I thought it was unfair that I was being expected to take it on then, because I had so many projects in process. David Freedman probably felt the same concerning himself. In the end, I did it for two years. David F. did it for five. As I just said, I did enjoy the job, but only after accepting not doing much research. The person whose model I followed in the job was Erich Lehmann. He had been chairman perhaps for four years and I just liked the way he did it. He would be in the coffee room at 10 a.m. in case any of the students or faculty wanted to see him. One needs role models for how to do these different things, and Erich was my model for the chair position.

I just remembered a story. Actually, during Erich's term I was (Acting) Chair for half a day. Erich had felt compelled to resign over some matter. I was Vice Chair which I guess made me Chair in a sense. However, Erich didn't tell me that he had resigned until my "term" was virtually up.

Victor: So what is your opinion on leadership in academic departments? There's a sort of patriarchal paradigm with a dominant personality at the top and a democratic paradigm—for example, Neyman years vs. post-Neyman years. What's your take on that?

David: There is also an anarchist model. In fact, when I first came to the Department there was something of an anarchist attitude—everything was being challenged, like language requirements. Barankin gave a stirring speech, which got rid of them. I believe that Neyman created some things that might never have existed without him. That was very special and what the right great leaders do. I don't feel that the faculty resented it too much, but I don't know. I liked being at the LSE rather than some other English university, because then there were something like 5 professors in the department (Figure 8). Also, mathematics was growing out of statistics there, not the other way around. The professors rotated the position around being chair for

FIG. 12. *David with a group of his Ph.D. students in Banff*, 2003. *From left to right, starting at the top*: *Bruce Smith*, *Peter Guttorp*, *Tony Thrall*, *Knut Aase*, *Mark Rizardi*, *Rick Schoenberg*, *Ed Ionides*, *Isuo Miyaoka*, *Haiganoush Preisler*, *Jostein Lillestol*, *Tore Schweder*, *John Rice*, *Andrey Feuerverger*, *Alan Izenman*, *Raju Bhansali*, *David*.

three years. What I tend to say when people tell me that they have been asked to be chair is: well, if you can do it, you have to. The thing is if the people who could do it manage to get out of doing so, then the system of good governance collapses. Anyone who could do it has to take their turn. An advantage is that different things are emphasized depending on who is the chair. In my term, I put a lot of department resources into computing. It seemed the time for that and I could handle the decisions. Incidentally, one of my students said that as soon as he learned I was going to be chair, he worked very hard to get his thesis finished. So my taking the job on was good for him.

There are different attitudes concerning how to behave as chair. When I was doing it, the budgeting was actually very loose, but I didn't know that. A friend who was chair of another department heard me muttering about restrictions on money. And he said, "Oh just spend it! Let the dean find the money!" I guess there was no mechanism at the time to pick up on overspending. When I told the financial dean that I was spending money like it was my own he said, "Good!" Many university things were much more casual back then.

Victor: By next year, you will have had 40 students, some very notable people among them.

David: Students have been one of my great joys at Berkeley. If for no other reason, they are a motivation for seeking a position here. There is a nice picture of me with many of "my" doctoral ones in Banff (Figure 12). I sometimes wonder whether I could have supervised a student and not become friends with them. They certainly do become friends. As you point out, my rate is about one student a year, and that's probably a reasonable one because they take 2–3 years to complete the thesis. Nowadays, there are research groups or labs. I tried that in the mid-seventies, but it didn't seem to work well for me, or, more importantly, for the students. My goal is to have the students learn how to do independent research. This was Tukey's way. I sometimes see my ex-students treating their students the same way. I interact with a student to find a topic that they are really interested in. Nowadays, statistics is everywhere, so that hasn't been too hard. I think when you are interested in something, you just find yourself progressing and the time flying by. I used to play a lot of intramural soccer (see Figure 13). That's actually a good way to get to know students and visitors. When you kick them, accidentally of course, you see how they respond and when they kick you, they see how you respond. You learn a lot about each other!

By the way, I will not sign off on a student's thesis until they have started arguing with me and are calling me David. For some students that can be hard, but they need to be toughened for the outside world.

FIG. 13. *David and the Berkeley Statistics Soccer Crew. From left to right, starting at the top*: *Tom Permutt*, *Jan Bjornstad*, *Jim Veetch*, *?*, *Annibal Parracho*, *David*, *Peter Guttorp*, *Kai(-squared)*, *Eldar Straum*, *Albrecht Erle*, *Ken Suttrick*.

7. "$2\pi \neq 1$"

Victor: I was wondering if we could go back to research a bit. The title you used for your 2005 Neyman Lecture (Brillinger, 2008) was "Dynamic Indeterminism in Science." Would you say this describes your scientific vita?

David: I like your question. In a word, the answer is maybe. That expression is to be found in a 1960 paper of Neyman's (Neyman, 1960). He was encouraging people to learn about stochastic processes. I don't think many statisticians did back then. And then I was invited to give a talk (Brillinger, 1984) at the International Congress of Mathematicians in Poland in 1983. I talked about statistical inference for stochastic processes in a general way. There weren't many people doing that then. Murray Rosenblatt and Ulf Grenander were involved with it, but the list of people working with a general process framework was short. One conceives a datum that is a realization of a process. That's what Neyman was encouraging people to work with. Le Cam's approach was totally abstract, so everything was a particular case—but in a sensible way.

Victor: I recall you were mentioning in the doctoral course on applied statistics at Berkeley that, "Any mathematical object that can be mathematically expressed is potentially data.

David: For sure. You just put a collection of the objects in a hat. Then you find a sensible way to pick one of them at random and then you've got a realization of a random object. Think about the article I showed at my talk this morning about statisticians being the sexy thing to be for the next 10 years (Lohr, 2009). The rest of the world has clued into that, finally! There are these wonderful data sets with people who care about them. And statistics has an immense amount to contribute to their study. Plus, it's going to be a lot of fun to be doing it. You have music in your computer, videos in your computer, you may even have a Bible in your computer—all this stuff is nowadays in a computer, just waiting for you to discover surprises in it! That's a Tukey attitude. I never saw Tukey doing any computer programming, but he could surely visualize it. And he was very much involved in the first Von Neumann computer (Brillinger and Tukey, 1985). So, he knew about it in that sense. I did see him with coding sheets, but he was preparing things for cards to be punched for his citation indices (Brillinger and Tukey, 1985).

Victor: Some consider you as a theoretical statistician, others consider you as an applied statistician. Which one is it? Always learn new theory?

David: Oh yes? Where did you get that?!? (both laugh) That's my motto: always learn theory, for the theory becomes the practice. I can provide a lot of

evidence about that and I think it is what places the Berkeley students in a good position when they finish. Because other places will create students who are really up to date the moment they finish, but not ready for new things that come along. It's harder for them to keep on top of things. They may well feel intimidated and struggling to keep up. I think the students coming to Berkeley get a lot of gifts from the people here. One can mention Le Cam with his abstract approach to things and depth of thought. I had great respect for him for a lot of reasons. One of them is he could sit in his office and he could dream of these incredible mathematical problems, and dream up solutions. Whereas my thing to do is to find a parallel scientific situation where that problem exists. This can give important clues about how to approach the problem. Lucien always seemed able to generalize these things in such a way that he would encompass so many things. I would take some of his work and particularize it to a specific situation.

Victor: Is that your research strategy? How do you attack problems? How do you find or choose them?

David: I find them by people interacting with me, or by my asking them. As I mentioned earlier, when I arrived in Berkeley, I went over to the Seismographic Station. They didn't come to me. I think that with a consulting service you don't really get the special people coming. You have to go over to them, to the scientists. You have to present yourself to them. Terry Speed and I agreed on this once. Terry was chasing across campus some time after he arrived, interacting with people, particularly in biology. When I think about my recent work: risk analysis was motivated by interactions with Bruce Bolt of the Seismographic Stations, the trajectory modeling was based on data collected by Brent Stewart of Hubbs Sea World, while both topics involved Alan Ager and Haiganoush Preisler of the US Forest Service. The work on sports statistics is based on data that I collected on my own. At a certain point you've got all the problems you can handle. It seems in any case that if you want to work with good people, then you have to go after them. So I've just come to know a lot of people. Various of my papers may be found in (Guttorp, 2010).

Now, I am a member of the scientific of advisory panel this new center of excellence for evolutionary biology at the University of Oslo, and there is a flood of new problems coming into my head from that. It is just wonderful. But I was wondering: why me on this panel? And then I thought, "Oh, evolution, that is time-series, isn't it?" It is just a totally different group of scientists from any I have been involved with before. Now I own a great thick book on evolutionary biology.

Victor: In a recent article (Dyson, 2009), Freeman Dyson classifies mathematicians as frogs and birds; or as Erich Lehmann put it (Lehman, 2008): problem solvers and system builders. Where do you stand?

David: I like to be a bit of both. I like solving problems, but yet from my math background I like to abstract things. I like to transfer information between fields. So, I have worked at the same time with a seismologist, Bruce Bolt, and with a neuroscientist, Walter Freeman. Walter works with EEG (electroencephalogram) analysis. I would be telling Walter some of the clever things the seismologists were doing and I would be telling Bruce some of the clever things that the neuroscientists were doing. They each could then be thinking of applying these things to their own data. Abstraction was the route between the two fields. Transfer of knowledge is a topical goal and the politicians like it a lot. It probably makes sense because you can "start sooner" in a different field. Dyson by the way is another hero. I think I read various of his books and papers. I used to look a lot at the physics literature.

Victor: Do you have a favorite paper?

David: I believe that my favorite papers are the ones that I had to work the hardest to get the result. I believe I told you I had solved all the problems, except one, in Sam Wilks' book. The one which was about getting an asymptotic joint distribution of the median and the mean. I did not know how to get that and when I told Sam I don't think he knew how either. He said he had found the result in a paper by some Hungarians. I never found that paper either. Eventually, I ran into the notions of strong approximations, later called coupling, and read a report by Ron Pyke—another role model of mine—and one of his students, on getting a strong approximation for the empirical CDF using tied down Brownian motion. But for the problem I was concerned with, I needed an error term. I think I was the first to set down that approximation with an error term. The Hungarians then referred to my work and generalized it to get a lot of wonderful results.

Victor: You're referring to your early *Bulletin of the AMS* paper on the representation of an empirical distribution function (Brillinger, 1969)?

David: That's right. That's one of my favorites. It just opened up a whole host of things. Then, of course, when you get such a result you can improve it a great deal. But this strong approximation just lets you write down results using standard calculus. That was an important one to me.

Victor: And what about a "favorite rejected paper," or, to put it differently, is there an instance when you might have felt angry at a referee?

David: No, never anger at an academic referee, sometimes anger at a soccer referee (Victor laughs). I had a paper once, that I thought was quite interesting, on a representation for polymeasures. So polymeasures do relate to polyspectra, but really it was more useful for nonlinear operators. I mean there's this huge world of linear operators, but polymeasures provide you with representations for an important class of polynomial operators. And then, since I was just about to move to England, I thought it would make sense to send it to the *Journal of the London Mathematical Society*. To this day, I think that if I had actually been at LSE and sent it from there, they would have accepted it. But I just got a referee's report back saying that they were just not interested in that type of paper. I was young, I was learning. I still had the attitude that I'd rather be playing hockey than doing this stuff, and that stood me a good stead. Really, that's not made up. Plus, I had Tukey telling me that he had many papers rejected. I think I read somewhere that Rob Tibshirani said that his first ten papers were rejected. Tukey's thing was resubmit somewhere else. I sent it to the *Proceedings of the American Mathematical Society* and they accepted it directly (Brillinger, 1967).

Tukey and I had a paper rejected by two journals (Brillinger and Tukey, 1985). He told me not to worry, it could appear in his *Collected Works*, and it did.

Victor: Going in the other direction, was there a paper that you found had much more impact than what you would have expected?

David: I just love to do math problems. All through High School and University, there were problems from the *American Mathematical Monthly* that I would try to solve. So, I was doing it for my amusement. You know, you could send a solution and sometimes they would publish it. So, I think in many cases that's why I was doing things: there was a problem, and I was there. So, the polyspectra paper (Brillinger, 1965) just started out from having fun. I found that cumulants were a way to go. They had this property that, if there was a multivariate variable, and if some set of its variables was independent of the rest, then the joint cumulant was zero. This takes one directly to a definition of mixing for general stationary processes. Perhaps the Russians knew that result, but anyway. But I was working on this for fun. At one point, Tukey mentioned the word, polyspectra, and I made the connection—and wrote that paper. That paper might have helped me get some invitations to speak and job offers and promotions. It surely led to my collaborating with Murray Rosenblatt.

Victor: Well, it's been cited over 200 times, I think!

David: I remember I gave a talk on that research at Cambridge. David Kendall, whose work you know well, had invited me. When I was done with the talk, I think he was as baffled as most other people were by what I was up to. Maybe I was just not good at explaining it. Hopefully, I eventually learned how to do so. Anyway, Kendall said something like, "Now let's go have some poly-tea in our poly-cups." So that broke the ice (laughs). Most of these great people have a sense of humor. They can seem pretty serious because one has to think hard to do the research. But you realize that basically they're people who have families, and have fun with their children at the playground. There is a human side to all of them. So, in the beginning, very few people would refer to that paper at all. I think Kolmogorov knew about it, and I had a bit of an interaction with Zurbenko about it. But that was pretty much it. But then, in the early '80s all of a sudden I get this flood of reprint requests! This was when people still used reprints, they didn't have things on the web. And so, all of a sudden I'm being invited to these conferences, some of them in exotic places, on "Higher Order Spectra"—that's what they called it. My preference is cumulant spectra. I remember saying things at some of these conferences, like, "Nothing matters unless you show it used on a real data set." And I remember seeing some of the engineers looking at each other. Because in so many cases they would tend to use proof by simulation. That gave them the feeling they had done their duty in terms of a proof. I don't put them down, I have a huge amount of respect for engineers. My favorite committees are engineering committees because they have something better to do than being on the committees! And they have this attitude, that Allin Cornell, an earthquake engineer expressed to me once, the attitude that every engineering problem has a solution. And I think Tukey was showing me that many times over in the form that every statistics problem has a solution. And that it's the statistician's responsibility to find it. You can't just abandon a scientist and their data.

Victor: On your office door in Evans Hall there is a sticker: $2\pi \neq 1$. Would you care to elaborate on this for the uninitiated?

David: Oh well, yes, that's my logo! I usually like to make people figure it out. It goes back a long way. Here's one story: this student, Raffa (Irizarry) whom I have mentioned already, was just a joy. I would hear

loud footsteps of someone running down the corridor toward my office. And then Raffa would appear, slide me off my chair, and open a window on my computer saying, "You have got to see this!" One day he ran into my office saying, "I found it! 2π is not 1!" He had discovered what was going wrong in his computations by simulating the basic procedure countless times for a known case. His answer was out by a multiple of 2π. Raffa was already a modern statistician using Mathematica and simulation to deal with analytic problems. By the way, he just received COPSS' Young Statisticians Award. That made me very proud. Peter Guttorp just got an honorary degree from his home University of Lund. The grad students have been my great joy at Berkeley. Ross Ihaka received the Pickering Medal in New Zealand for his work in developing the statistical package R. Others too. I mean my students make me proud for their research and professional contributions. John Rice has excelled in those two areas and just completed a second successful term as our Department Chair. They are grandchildren of Tukey's, and a lot of what they are getting from me is what I learned from Tukey. For example, you've seen me filing papers with these plastic ziplock bags? Well this is a Tukey idea from many years ago! Victor, does Stephan (Morgenthaler) ever do that?

Victor: I don't recall, I'll make sure to check!

David: Well, you can tease him about it. If he says no, tell him that Brillinger says he would have a better career using these bags! He will have an answer to that, I'm sure (both laugh)!

Victor: Churchill (Churchill, 1930, p. 17) wrote something like, "All students should learn English, and then the clever ones should take Latin as an honour and Greek as a treat." Translated into mathematical or statistical topics, what would be your pick?

David: You could probably ask me that five times and get five totally different answers! Because right now I think it's puzzles. As a youngster, I was always doing problems in the newspaper, you know "three men are in a room and they can't see what's on their own head..." and things like that. I had a lot of fun in doing that and a lot of good intellectual exercise. Perhaps the exercises in my book was the part I enjoyed most. It was the hardest part too. The things I had to work hardest on are the ones I respect the most. I developed an estimation method and a paper once, on my bike ride home. I had the idea, went to the typewriter upstairs, sat down, and typed it up. I sent it to *Biological Cybernetics* directly (Brillinger, 1978). All done in a couple of hours! That didn't impress me. Then, there are some other things like how to handle the "integrate and fire" model in neuroscience (Brillinger and Segundo, 1979), which took quite a while to come along.

Victor: As we already mentioned, you will have supervised 40 Ph.D. dissertations by next January. What would be your advice to the next generation?

David: It seems to me that learning mathematics is nowadays being replaced by learning computer science. I think it would be good for students to learn near equal amounts of each of these. Computer science lets one check out proposed methods, learn about data structures—after all the data are typically in a computer—and get approximate answers. But I am not sure it really takes you to the essence of a lot of situations. Think of the neural net models. They can be justified by the science, as in the threshold case mentioned above. However, I am uneasy about throwing everything in there and getting an answer without a scientific interpretation. I would rather use something that has scientifically interpretable parameters. Let me add, though, that I am certainly not averse to using some tool to see what it can do for me. I would like to see students come back to studying more serious mathematics. I'm astonished that some students in the computer science community don't know elementary trigonometric identities. For them, the Fourier transform is just the FFT: you put this in and you get this out. People learn a lot by just doing something and seeing what you get. That's a system identification approach where one inputs a signal and sees what comes out. I think it is a lot more rewarding to really get some understanding of *why* it is happening. Although in science it doesn't always work that way. I remember Fred Mosteller saying many years ago that nobody knew then why aspirin worked, but that of course we are going to use it because it appeared to work. But still I think learning what the thing was doing is fundamental, because then you can improve on it.

My bottom line is: have fun! That sounds trite but I'm serious. If you are worried about something, consider what you can do about it. If there is something, do it. If not, what's the point of worrying? When you have a child die after a very long battle with cancer, as Lorie and I did, you simplify a lot of things. You take things to their essence. Don't be afraid to cry. It is another thing you learn going through a tragedy. Many say crying is hard sometimes. For me, it just happens.

Victor: David, thank you very much for sharing these memories of your remarkable life and career. But I have to ask one last question: would you still rather have been a hockey player?

David: Oh yes!!! (laughs out loud) There is noooooo doubt in that! I gave the after-dinner talk at one of the Canadian Statistical Society meetings and the title was: "Why I became a Statistician." You can guess what the punch line was!

Victor: Thanks again, David.

David: Thank you, Victor. You had some good questions. I mentioned only some of my students. I probably have an anecdote about each, but I'll save those for another time.

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