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An Interview with Bradley Efron

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BN: Hi, Brad. Thanks for taking time for this interview. Of course, we are doing this interview in an unusual time when we cannot meet in person. It was a big honor for me to join you in the department when I came to Stanford and so I'm looking forward to this chat about your career and your experiences etc. Perhaps we can first begin by you telling us a bit about your birthplace, your family, your school, etc. I should say that my knowledge is basically summarised as follows: that you were a transplant from Minnesota, who ended up on the West Coast and never went back.

BE: That's pretty close! Thanks, Naras. You and I have been working together now for the last several years. We've actually worked together very closely and it's been a great thing for me.

So, I was born in 1938, on May 24, which is almost exactly the date they usually say is the end of the Depression. My parents were Jewish. My mother was born in Russia and came over when she was one year old. The family came over. The older brother came over first and then a lot of other brothers, then the older family. Her father was listed in the 1920 census, I saw, as a ragpicker, and by the 1930s he was in fabrics. He was an old-world tyrant, by all things. I'm named after his wife, whose name started with B—Bertha—and so Bradley is from that B. My mother loved her mother. She was, I think, pretty afraid of her father. I met the old-world tyrant once or twice. He was sort of that way. He was a man who ate only one meal a day. He was a formidable man.

My father, Miles, was from a more sophisticated European family but not very sophisticated. He was born in the United States and the family had come over a few years before. My dad was a terrific guy. He was the three-cushion billiard champion of St. Paul some years. He liked to play cards—he was very good at it. He was a lifelong salesman, always selling things, and he had a lot of amateur interest in math. He was a good athlete, which I'm not, and when he got older, he did things like he was the scorekeeper for baseball—that's an elaborate scorekeeping system—and for bowling, so we had a lot of numbers around the house.

BN: Does that explain the interest in sports you have?

BE: It explains more the interest in numbers I have. I like sports a lot, but I'm not a fanatic by any means. Even in Sequoia Hall I'm not very extreme. But he had that. My mom, a very warm woman. She got sick and died pretty young, when I was about 15. She got really sick when I was 13 and died when I was 15. My dad got sick subsequently. He bought a delicatessen, a Jewish delicatessen, and, with bad timing, the Jews all moved to a different part of town just after we bought it.

BN: Was there a big Jewish community at the time you were growing up?

BE: The Twin Cities, St. Paul–Minneapolis, had a population of perhaps 20 000 Jewish families. Northern Minneapolis was sort of a centre of culture and then St. Paul was the old ghetto, the part that they'd lived on the east side. They weren't confined there, but economically they were. I remember going as a little kid, because my parents no longer lived there but they'd take

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us there because you could get food, and you'd see people like scissor grinders. I bet you know what I'm talking about.

BN: Well, actually it's funny you should say that because we had something similar; my father used to take us to the temples, and we' d get all these things to eat.

BE: Yes, right! It was a little scary for a kid, because it seemed like not part of America.

BN: Did you have a lot of uncles?

BE: On my mother's side, there were, I think, eight children that lived to adulthood. One of them, Uncle Herman, had died before I was born, but the others I knew. There was a warm family and I was well taken care of.

BN: I meant to ask you this: with the last name Efron, is it a contraction or is it actually the real name?

BE: Well, now that's an interesting question. My father's father, Sam—I knew him, my grandfather—was sort of a puckish man and he always would never tell us anything about our background. He just thought it was funny that they would want to know something about a place as benighted as Russia. When he came over, the name Efron was assigned at Ellis Island and was assigned to a lot of people but it probably has to do with a village that they came from or a place that they came from. In the 1850s or '60s, the Tsar required all families to take a last name and most of them just took the last name of the village they lived in. Maybe Efron is something like that. It's not an uncommon name. Somebody told me that Poughkeepsie, New York, has a lot of Efrons.

BN: I think, if I'm not mistaken, there's a movie star who's a heart-throb, who's also Efron.

BE: There was Nora Ephron, the writer. Yes, right, Zac Efron. Zac caused me trouble because people started calling. Teenage girls would call and say, 'Are you Zac Efron?' Fortunately, his career hasn't taken off!

BN: How many brothers and sisters did you have?

BE: My father was married before he was married to my mother and his wife died. My older brother Arthur, my dear older brother Arthur, was five when my dad remarried and he's about six and a half years older than me. I just saw him on Zoom. We had a family reunion. I have my brothers, Donald and Ronald. They're twins. They're six years younger. We have sort of birthdays all together, so we Zoom together as all four families. They're all PhDs. My older brother is an English professor, who specialised in Don Quixote and things like that. My two younger brothers both work in family therapy. They both started journals.

My brother Ron had well-known books about things like—one of his best-known ones is called *Angry All the Time*. He gives lectures and goes around and tells people about how people can be angry. He has a stage thing he does, where he starts at one end of the stage and he says, rather calmly, 'I had a desk but my brother took it.' And as he goes across the stage, he gets angrier and angrier, more vociferous, and by the end he's just about foaming and he destroys the desk. Then he asks the audience where in the stage they were at, which level. Somebody always says, 'You didn't go far enough!' Since his brother is Don, his twin, I always wonder . . . Don runs his own journal on family therapy. So there's three brothers.

BN: Accomplished, wow! So you're all boys—no girls.

BE: All boys. My father married subsequently, after our mother died, and I had a stepsister but I was never in his house with her, so I barely know her. I grew up in St. Paul, went to St. Paul Central High School, which wasn't a bad high school, and I went to James J. Hill's Elementary School. James J. Hill was a railroad baron, just like Senator Stanford, so I've spent a lot of my time as the recipient of railroad money, in some sense. Then I got my big break. The year I was going to be 18 was the year they started the Merit Scholarship Program.

BN: What was your experience in high school? Was it competitive? Did you have good teachers—someone who stood out that encouraged you?

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BE: It wasn't a bad high school. By current standards it wouldn't be considered very good. I had a trigonometry teacher, Mr Thorson, who I thought was pretty good and I liked him. I was somewhat rebellious. I got kicked out of school once for doing bad things and throwing firecrackers and stuff like that. School was pretty easy for me. It wasn't like you got a lot of homework in those days. I was working in my dad's delicatessen and starting to date, and when you dated as a Jewish kid in St. Paul, it usually meant driving to Minneapolis. The idea of dating in my high school, which would mean non-Jews, basically, was just beyond my limited comprehension. Anyway, nothing much happened there. I had good friends and some of them died rather early. It wasn't an easy—you know, drugs and stuff like that. My friend Larry Fink I still talk to every once in a while.

BN: Tell us a little bit about Larry Fink.

BE: We moved to 935 Ashland Avenue in St. Paul, which is a street—if you know anything about St. Paul—between Selby and Grand, and there's a long social gradient between Selby and Grand. The first day I was there, I met Larry, who was my age. We were very good friends all through grade school and I still correspond with him every once in a while. His father owned another, more successful, grocery store delicatessen on Dale Avenue. Anybody who has been in St. Paul knows what Dale Avenue was like—it was tough.

BN: Were there any subjects you were drawn to in high school? For example, did you do literature, social studies, or was science a big part?

BE: The only part that I liked was math. I'd go to the library. St. Paul had a big public library. I'd get out math books which I didn't understand very well at all, but it was fascinating to me that there were things like that. I think I early wanted to be a mathematician of some sort, not realising really what the life of a mathematician was like.

BN: You talked about the Merit Scholarship. What is the Merit Scholarship?

BE: Oh, I should say about my high school. It, for example, did not teach calculus and it topped out at trigonometry. This is 1956. That year I'm 18 and I'm thinking I'll probably wind up at the University of Minnesota. Anybody in Minnesota could get into the University and I'd be a good student and all that stuff. But, amazingly, they started the Merit Scholarship Program, which at that point—this was going to be its first year—was going to give full scholarships for four years to your place of choice, enough money to live on, barely, and about a thousand students in the country got it. I went and took the test and I can still remember getting the envelope and it was like magic. It was better paper than I was used to! I opened it up and it said I'd won, wow! They'd asked me where I wanted to go and I said I wanted to go to Caltech.

The reason I wanted to go to Caltech was interesting. You have to realise I knew nothing about the world of science or college or intellectual life. I had my Uncle Joe—I said my first, my oldest, uncle had come over first to St. Paul—but he had fallen into a business dispute with my Uncle Isadore. Everybody liked Isadore. Uncle Joe was a little suspect because he'd made a lot of money and he'd moved to Los Angeles, an exotic place from my point of view. Uncle Joe had sent me a subscription to *Time* magazine, which was way above the level of sophistication around me. One day when I was 17, I think, they had a cover article on Caltech with Lee DuBridge, the president, on the cover. I read the article on Caltech and it followed a typical student, John Andelin, around his day, where he said he worked 80 hours a week on his work. When I got to Caltech, I met John Andelin and he was a typical Caltech guy—he hadn't worked 80 hours in his whole time there! He just had told them that.

Anyway, Caltech wasn't enthusiastic about admitting me. At that point, Caltech would send a professor out to interview all prospective students and one came to my high school. He warned me with this dour face that I would be a B-minus student or something like that, because nobody from my high school had gone to Caltech and I hadn't had really a good training. When I got

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to Caltech, sure, the kids who'd gone to better schools had an easier time with calculus but that only was a couple weeks. Calculus isn't really very hard.

BN: The National Merit Scholarship must have meant a great deal for you because I could see you recounting the story with some emotion. It sort of brings back memories to me too, when I got admission to a school in the US. It changed my life.

BE: Yes, I was very emotional when I was thinking about it. You know, I have won lots of things but that was the thing that actually made a difference. The letter was from the head of the Merit foundation, John Stalnaker, and much to my surprise, 50 years later, I got another letter from him in the same fancy envelope sort of wondering how we had done. I was amazed he was still alive and I got that same emotional reaction when I saw the letter.

BN: Many, many people will relate to this story, I think.

BE: I should tell you one more thing. Both of my younger brothers, the twins, also won Merit Scholarships.

BN: Wow, okay, that is an accomplishment! Funny that you mentioned somebody warned you that you would be a B-minus student because I remember reading an interview with Don Knuth. When he was going to college, somebody told him, you know, college is very hard and so on and so forth, and so they frightened the hell out of him, which only made him work even harder. Was that the same case with you?

BE: The truth is, I never believed exactly in the idea of smart or dumb. I just thought some people have it and some people don't, ‘it’ being something like ‘have ideas’. Anyway, it didn't impress me much and I did work really hard when I got to Caltech but everybody worked really hard, except for John Andelin! Getting there was the deciding point in my life. I went from being a Minnesota kid to a California person on his way up in the scientific world, because going to Caltech meant you had a badge on you (Figure 1).

BN: How was Caltech and how was the math department? Who were the professors around at the time that you ran into?

BE: When I got to Caltech, my freshman class was 180 people and incidentally all of them had gotten perfect scores on their SATs or whatever it was. It was a tough school to get into and every day at Caltech was sort of like an IQ test. What they taught at Caltech, I decided, was cleverness. The subjects themselves—it was a small faculty and if you were in the right subject, you had a state-of-the-art kind of situation. Math was not in that category. The math department was a little old-fashioned. There was no computer science, just barely any, no statistics at all,

Figure 1. *Left: Brad as a student at Caltech, 1960. He planned to go to the University of California, Berkeley, but ended up going to Stanford University. Right: At the whiteboard in his old Sequoia Hall office at Stanford, 1996. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

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and it wasn't very good for a person who is going to be eventually, but didn't realise it, an applied mathematician, not a theoretical one—a statistician.

I was an A-plus student but I wasn't really an A-plus scientist in math. I wasn't a natural. There was a guy there, a wonderful co-student of mine, Al Hales. I bet you know Al Hales. He's had a distinguished career working for the NAS. He was a natural mathematician and I just couldn't believe how good he was at it. It made me realise that I'd better find a better job to work with, because I wasn't going to do that one. I'm just not very good at abstract stuff. I lose interest in it.

BN: By the way, I should say that I googled Caltech in the '60s and out came an interview with neuroscientist Michael Gazzaniga on the NPR program Science Friday. The title of the article was ‘From Animal House to Prufrock House: Memories of Caltech in the ’60s'. He seems to describe quite a party-like atmosphere, as well as smarts.

BE: I'll tell you more about it. What it was was an Animal House kind of situation. There was a lot of pressure and no girls since it was all men. There were some wonderful people there, including my friend Carl Morris, and then Peter Bickel.

BN: I was going to ask you more about that later.

BE: People behaved weirdly. They would be up all day and all night, some of them. There'd be weird contests like could you eat 24 doughnuts in an hour, that kind of thing. You could but it wasn't a good idea! It was a very extraordinary time. I didn't really feel I got a great education, but I got something great from them and it was the feeling that, if you were clever enough, you could make real progress on intellectual things. It wasn't a matter of just grinding away. Cleverness was very highly taught at Caltech. It's a peculiar institution. If I'd really known what was ahead in my life, I might have very well done better to come to Stanford but I hadn't even heard of Stanford. It was not a school I'd heard of. At Stanford at that time, computer science was taking off, statistics was very strong, the department was just getting going, and I would have had a different career.

BN: By the way, Michael Gazzaniga also says the mystique of Caltech undergraduate life remains today. It featured in the TV series *The Big Bang Theory*. Now both you and I are big fans of that series.

BE: Right, *The Big Bang Theory* does a wonderful job of getting, just every once in a while, little bits of the Caltech culture, how childish they could be. The childishness was mixed in with some really clever ideas and things like that, the pranks and stunts. You know when you look out the window on *The Big Bang Theory*, you're looking at the actual Caltech campus, or at least some. They are very careful. The names of the streets they drive on are the right streets, and that always gives me a thrill, there on Colorado Avenue or something like that. The campus would have fit inside the main quad pretty much. It was just a couple blocks square. It was a very compact school with some very excellent people in it. The undergraduates were arrogant and thought they knew a lot more than their teachers, who were mainly the graduate students.

BN: As a young student, I was into physics. Of course, Caltech was known for physics and so I was wondering whether you actually ran into people like Feynman, Gell-Mann or anybody like that. Did you take classes?

BE: Oh yes, so who are the famous people?

BN: Feynman, for example.

BE: Feynman and the chemist Linus Pauling. Yes, Pauling was my teacher in chemistry. He taught the first-year chemistry course, which all the students took. He was a wonderful gentleman and he seemed to know everybody's name. I remember once I was late running into the class and there he was at the door. He opened the door for me and says, ‘Here you are, Mr Efron.’ Later I had some dealings with Pauling because he got very interested in his own version of the bootstrap.

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Richard Feynman taught his course on physics for freshmen but it was a very eccentric course and I was determined to get good grades. I didn't take his course and that's a regret. I should have taken it. He was around. He'd come to the dorms and play his bongo drums and stuff like that. He sort of epitomised the Caltech undergraduate spirit of cleverness—that's what he was. He was always a clever, unusual guy and so I regret I didn't do that. There was also Beadle, the biologist who was a Nobel Prize winner, but what there wasn't was a really great computer scientist or anything like that. Computer science was just getting going.

BN: That's correct, if I remember. Also, I wanted to ask you: at that time, you mentioned Pauling had his own version of bootstrap. What was that?

BE: This is 50 years later, 60 years later maybe. He just had his own way . . . I never did get the full story from him. He gave a talk to the biostat seminar. He was the speaker when he was in his 90s. He came to give a talk to the biostat seminar in which he'd had some ideas for data analysis that seemed like that. He said that he'd remembered that there was somebody from Caltech who'd done something that he'd read about and he sort of remembered my name vaguely. He was such a nice gentleman.

BN: So, you graduated in mathematics in 1960.

BE: Yes, I was third in my class, incidentally, and the first was the guy I mentioned, Al Hales, and the second was a brilliant physicist who subsequently killed himself. There was an awful lot of pressure at Caltech (Figure 2).

BN: You already have told us that you were more into applied versus abstract mathematics, so how did you choose statistics and how did you choose Stanford? Was there a senior thesis or something you had to write at Caltech?

BE: No, that would have been far too humanities-ish. Caltech had one little building that was called 'humanities'! Anyway, in my senior year I got interested in doing something more applied. I went to one of my professors and he said he'd give me a reading course, even though he didn't follow the stuff very much, and he gave me the book by Cramér on statistics. I read that book and I thought it was a wonderful book. I still have it on my shelf, if I could ever get into Sequoia Hall again. I said I wanted to go into statistics as a graduate student.

The people who were writing my recommendations wrote to Stanford. I was either going to go to Stanford or Berkeley, and somehow Stanford wrote, sent me a nice letter instead of a postcard, so I went to Stanford. They somehow wrote that I really shouldn't go into statistics, I

Figure 2. *With the Fisher family, daughter, far left, son, second from right, on the occasion of the Fisher lecture, England, 2000. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

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was a good mathematician and I should go into math, so when I got to Stanford I found that I was in the math department, even though I wanted to be in the statistics department.

There was a lot of collaboration, because at that point the department, which was in old Sequoia Hall, had a wing of people who were both math and stat and included Chung and Karlin, who were having a terrible feud with Herb Solomon, who was the head. They subsequently left the department completely, which was very good for the department—it cured the fighting going on—but it was bad for me. I started working for Karlin, I switched over to the stat department, and then Karlin left and said I had to go back to the math department, and I said no, I wouldn't. So I really didn't get into the stat department until my second year at Stanford (Figure 3).

BN: Just to back up a little bit, you and Carl Morris were also students at Caltech, so did you know each other? Of course, he was in aeronautics or something like that.

BE: Carl was in my house at Caltech. There were four houses at Caltech. You might think of them sort of as fraternities. Everybody had to go in one of them. We were in Ricketts House, which had a sort of grungy atmosphere to it, but it was fun. Carl was in that and we were friends. One year, I was president of Ricketts House and I'd appoint people to have jobs. Carl was the entertainment guy, which meant that he had to try and go get girls to come to our parties, because Carl was better-looking than all the other people. Yes, so I knew Carl quite well at Caltech.

BN: But he went elsewhere. He didn't go to Stanford.

BE: Yes, he went to Illinois and then suddenly, in my second year at Stanford, there he was again. He'd transferred back, he'd decided not to be an engineer, and there was wonderful, fun Carl. He came back one day. It was in the summer, I remember. I was really pleased to see him. He had a little MG sports car with the top down and he said, 'Let's take a ride'. He sped off and there was one of these bar kinds of things that bars the way—they keep you from going into a

Figure 3. *A reunion of Caltech distinguished alumni in 2010. Carl Morris is behind Brad, to the right. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

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certain street. He went at it at full speed and it just was high enough so that we could go under. He had timed it before! That was very Carl-like, I thought.

BN: But you both ended up at Stanford by a different route.

BE: Yes, so then we were colleagues in the same PhD class at Stanford.

BN: You said you didn't want to do abstract mathematics as you you didn't want to be an abstract mathematician. Were you consciously thinking about theoretical versus applied math at that time?

BE: Well, as I said, somehow when I got to Stanford, my Caltech masters had said that I shouldn't go into statistics, that I should really go into math. I don't know why they said that and I wasn't comfortable. I took the math qualifying exam and I did okay, and I wanted to go into something more applied. Gilbarg was the chair of the math department then and I went in and talked to him. I said I would like to do more applied, and he gave me a book. His idea of applied statistics was special functions. He gave me a huge book on that and I think I lasted about 30 seconds on that one. I did go where the stat department taught more, started taking courses, and by the second year I was fully engaged in the stat department. But that caused difficulties because Karlin, who I was wanting to work for, was leaving the stat department under bad vibes and so I was caught in the crosswalk there. I started getting closer to Rupert Miller and Lincoln Moses at that point.

BN: I've always heard you say what a big influence Rupert Miller upon you. Can you tell us how you met him and how you decided to work with him?

BE: Rupert was the youngest—I think he was associate by that time. He'd been Karlin's PhD student, I think.

BN: What year was this roughly?

BE: 1960, 1961, that's all, and Rupert's only been at Stanford as a teacher for a few years. His main focus is the biostat regime which—Lincoln Moses, as a pioneer, had gone up to San Francisco where the medical school was and been their biostatistician. They didn't have one, of course. The big thing that happened in '58 was that the medical school was brought down to Stanford and the Stone building was put up. It was an enormous change to have the medical school down here and, of course, a big advantage. Rupert was brought in as Lincoln's companion over there. It was like a separate little department.

It was completely different than the stat department in its feeling, in that here we're doing Fisherian statistics and they had t-tests. They could do almost anything with t-tests because they were so clever at phrasing problems. Most typical thing, the data set would be a hundred points, maybe less, a couple parameters and the tricky question, and we'd sit around and discuss it. They'd have the biostat seminar, which was then about ten people sitting around. It was really fun and exciting for me. It seemed much more like getting somewhere than in the stat department itself. Of course, Rupert and Lincoln were part of the stat department, but in that department itself everything math ruled. A typical talk started out with a collection of Borel sets and measurable functions and stuff like that, maybe five curly symbols, and then it might get somewhere but there was an awful lot of feeling that you had to be exact mathematically. I got pretty excited about being over the other side there.

I eventually worked with Rupert but Herb Solomon took up my support and was very good about that. He helped too, so there were three people involved in, somehow, my graduate training, which was Rupert and Herb and Karlin. I wrote a paper for Karlin, which actually, when I look back at it, wasn't a bad paper, but he wasn't going to pay much attention to me if I stayed in the stat department.

BN: This paper—is this the one on the increasing properties of the Pólya frequency function? (Efron, 1965)

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BE: Yes, right, and the paper was written one night. I got sick and spent a night sitting up in a chair. As a matter of fact, Tom Cover and I were roommates at that point, living in a little house down near where Shoreline Theatre is now. It was an area where there were still chickens running around. I thought through that paper sitting in that chair and so it might have a feverish quality to it.

BN: I'm still trying to get a sense of how the biostat unit was. It was a division, but the teaching courses—you said you spent time in the biostat, so did you . . .

BE: It was very much like what Data Studio is now. People coming in with problems and getting advice. Less of this long-term collaboration with individual projects, more the way we've been working with, say, Wes Brown and things like that. People coming in with projects. Of course, there wasn't nearly as much data as there is now, but there wasn't also as much computation as there is now. Jerry Halpern was there computing. I'd have to tell him to do something and then a couple of days later I'd get it back. He was very good.

Figure 4. *With son Miles, at the National Medal ceremony, Washington, DC, 2007. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

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BN: Oh yes, Jerry was an amazing guy. Of course, I worked with him too, in the bone marrow transplant programme and so on. Yes, Jerry was a pioneer in some ways (Figure 4).

BE: Yes. Meanwhile, he had monkish tendencies. He would go to Esalen and things like that. Bill Brown joined at some point.

BN: I think that Bill Brown, of course, was an important personality doing biostat. Did you work a lot with him too?

BE: Yes, some. I was a disappointment to them in some ways, in that Bill and Rupert and Lincoln were a very close group who were totally centred in their thinking over there at the medical school and I wasn't. We're drifting now into the time after I got my degree and I'm over there still, but I'm not as much over there as they would like me to be over there.

BN: Let's go back a bit. What did you work on for your thesis?

BE: Oh yes, my thesis. The thesis was called something like 'Problems in Probability of a Geometric Nature' (Efron, 1963). I was very interested in geometric probability, a lovely old subject that goes back to . . . In about 1900, a guy named Crofton was asked to write the entry on probability in the *Encyclopedia Britannica*. What he wrote was his life interests in a subject he'd pretty well invented, geometric probability, full of clever ideas. The thing about throwing a needle down on a flag is the first step along that very long . . . Kinematic motion stuff—I just love that stuff.

I started writing papers. I wrote three or four rather disconnected papers, and then Tom Cover and I both worked one summer at SRI for Nils Nilsson and they were getting interested in robots and the perceptron. The perceptron's a lot like a modern algorithm; it has definite connections to modern prediction methods and stuff like that. It was the self-learning kind of thing. There was some kind of question about whether the perceptron always would converge under it and I wrote a really horrible paper, went on and on, but I did prove it (Efron, 1964).

Rupert, who was usually always very pleasant with me, was exasperated and said it was really hard for him to read it. But the paper was solid. Later, I was criticised by Minsky and Papert in a popular book (Minsky & Papert, S, 1969). They criticised my very long proof of the convergence of the perceptron algorithm and gave a short proof, which then turned out to be wrong! As far as I know, my proof is the only one that stood up but nobody cares, of course, any more (Figure 5).

BN: In your research, you've been able to bring a lot of mathematics to bear upon problems. I'm reminded of this famous paper by—remember in the 60s there was a paper by Eugene Wigner about the unreasonable effectiveness of mathematics in the natural sciences or something like that? I was wondering whether your mathematical training had a similar effect on you and helped you in your statistical research.

BE: I have had a certain love–hate relationship with mathematics. The part I love of mathematics is when somehow a complicated problem is suddenly stripped down to its bare minimum and its structure is revealed and you can move faster and it really helps you think. A lot of mathematics and statistics seems to me to go the other way: start out with something fairly simple and, by the time you've mathematised it, you've got something that's hard to think about.

I think I've lost confidence in my mathematics over the years. I should say this: I just wrote a paper about prediction (Efron, 2020) that got discussions and Professor Cox discussed the paper. Near the end of his discussion, he goes on and he says something like, and I can't quote this, it's clear, he says, that most of the interest in mathematical completeness adds very little to the statistical content. I thought, you know, I really agree with that. He added that Fisher and Bartlett both showed a complete lack of concern for mathematical niceties.

I wrote back—I said I completely agreed with Professor Cox but both Fisher and Bartlett combined their lack of concern with the ability to produce profound results. That's a small club, I said, which Professor Cox belongs to also, which is to say that not everybody can get away

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with no math. I've always wanted to write a stat paper that had as little math as possible but was very big on stat, and the one I did was the bootstrap paper which had, originally, almost no mathematics.

BN: In one of your interviews, you said that the reason you came to Stanford was because of its humour magazine. While at Stanford, of course, you were suspended, so maybe you can tell us a little bit about it.

BE: Oh yes, the humour magazine. When I was at Caltech, I wrote a humour column in the Caltech weekly called ‘Fifth column’ and it would appear in the front page's fifth column. I sort of thought of myself as a decent humourist. Both Berkeley and Stanford had good humour magazines. In Berkeley, it's the *Pelican*. At Stanford, it was the *Chaparral*. *Chaparral* goes back well over 100 years. They weren't very good. Times for college humour come and go. But when I got to Stanford, I ran right over to this *Chaparral* to introduce myself. I was good friends with them. I still have a very good friend from that time, Judith Skinner, who lives up in San Francisco and writes novels and things like that.

That's what got me in trouble, because they were planning a parody of *Playboy* magazine and the editor literally went crazy and was confined somewhere, and they made me the editor. I was pretty much the fall guy. I was into it. I wrote things. That was the time I got kicked out of school. I was, I remember, in a rowboat on one of the lakes. They used to have more lakes up there. I got a call saying Dean Winbigler wanted to talk to me—Dean Winbigler was the Dean of Men—and I knew that one couldn't be good. I was banished for six months. I would have been banished permanently except that Halsey Royden, who was the Dean at that point, and Herb Solomon combined to get my banishment limited to six months.

That was a little worrisome because it was the time of the Vietnam war. I could have been drafted. But Dean Winbigler wasn't bad about it. I don't think his heart was in it, to tell you the truth, and I subsequently over the years would see him and say hello. I'd get Christmas cards from them and I'd write back and say hi. Eventually he died and the son asked me if I would say something at the funeral. I said no, I didn't think I knew him well enough to take up time. But I felt nice toward him.

Figure 5. *With Charles Stein, at the departmental National Medal celebration, 2007. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

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BN: What did the six-month suspension mean, in that case?

BE: Well, what it meant was that I got in the car and drove. I got a job at Mitre Corporation in Massachusetts, the first time I had lived in the east, and I spent several months working for the Mitre Corporation with a friend of mine who was back there also, Louis Padulo. He was a friend with Tom Cover and me. We'd all been living in Crothers Memorial Hall. That was fun and then I came back. It really wasn't full five, six months. I left in March or something. I came back for September.

BN: I wanted to ask you about the atmosphere in the department because I know the Stanford–Berkeley seminars there were going on right at that time, probably.

BE: They were every month. It was easier to get back and forth, and the two departments had both been basically seeded at the same time, with the dominant figure being Neyman, who was incredibly good at spotting talent and collecting it. People like Charles Stein and Ted Anderson, those kinds of people. Neyman was always arranging things for people to have things. He was very good at getting honours for other people, getting positions for other people, and so the departments shared a common birth. The way to think of it, the Berkeley department was absolutely dominant in American, world statistics and we were a powerful satellite. We had Chernoff and Lincoln Moses and let's see, who else was there?

BN: Emanuel Parzen was there, wasn't he?

BE: Yes, Emanuel Parzen was there. Ingram Olkin. Of course, Charles Stein.

BN: Jerry Lieberman too, right?

BE: Jerry Lieberman was there, but he was busy setting up the OR department (Figure 6).

BN: You also had a big lot of visitors, right? My own connection here is that I'm from Florida State and Sethuraman was my teacher.

BE: Yes, Sethuraman came up. Barndorff-Nielsen was here for a couple years. Herman Rubin was always around. That's because Herb was a very good money raiser and in with the US Navy, with the group that provided the money basically, so there was a lot of extra money. The

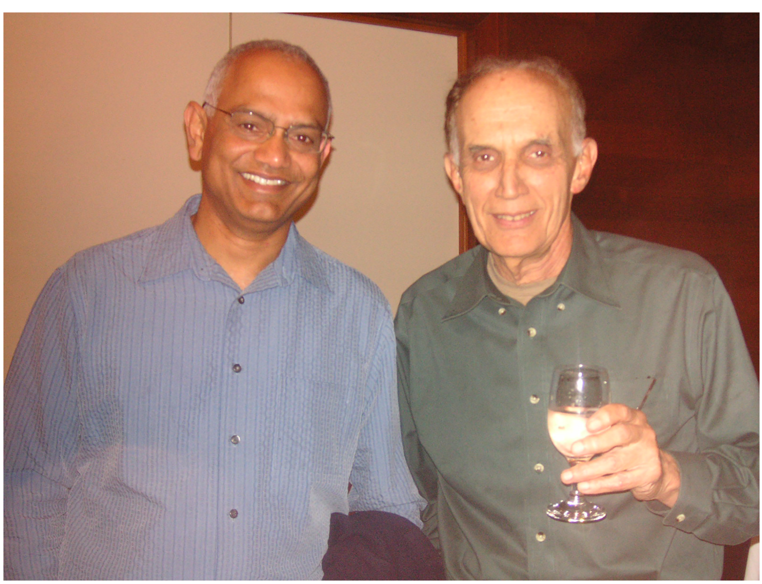

Figure 6. *With Balasubramanian ('Naras') Narasimhan, at a department gathering, 2010. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

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department was heavily on soft money. Remember that you could have defence money at that point for research.

The department was reeling from the fight between the rest of the department and Karlin and Chung, who left to go full-time in the math department. When I first got there, one wing of old Sequoia Hall, one part of the wing, was occupied by people who were in some form of applied mathematics—that is, it was somewhere intermediate between statistics and pure math—and that cleared out. Actually, I believe that was when the department really got going, after they got the contention out. Somehow, in the next ten years or so the department really got very, very lively. They had good students.

BN: So once you've graduated, you've got your thesis out, how did you then get a position at Stanford?

BE: So, it's 1964. I've really finished everything. I don't want to declare that I'm through because I'm still in the draftable age and so I sort of stall for a year on—it wasn't called the postdoc—it was some kind of fill-in thing. Then it's time to apply for a job, and my things that I've worked on, on geometric probability, aren't likely to be extremely popular. I interviewed in a few places. I had a job offer from Minnesota, which I would have taken. I did apply to Stanford, and they had, and continue to have, a bias against their own students. I was the third choice and the first two, one of which was Larry Brown and, I forgot, another very good English statistician, both turned them down, so I got the job. I started as an assistant professor, I think in '65 but I can't quite remember.

BN: As a young assistant professor, how did your life change?

BE: It changed partly—my salary went down! I remember it being $7000 a year. But it was exciting to be at Stanford. It wasn't very long after that that I took a visiting year at Harvard, '67–'68, where I was the visiting assistant professor. When I got to Harvard, there was a nicely typed thing on the door that said 'Bradley Efron, Assistant professor of statistics at Stanford University, Instructor in statistics at Harvard'. They were very keen on those kinds of things.

I liked being at Harvard. It was fun. They had a good little department. Paul Holland was there and I hung around with Paul a lot. Fred Mosteller and William Cochran. Cochran was a wonderful gentleman. It was fun. It was interesting. It was different. It was less backstabby than Stanford. I almost would have stayed and I got a job offer from them not that long after, but when I talked to them, it seemed clear that the Dean had very limited prospects for the stat department and I wouldn't go. Maybe I wouldn't have gone anyway.

BN: In your earliest papers, I think I see only one (Cover & Efron, 1967) on geometrical probability. But soon you go into mathematical statistics. I think that your fascination with exponential families starts right after (Efron & Truax, D., 1968) and continues to this day.

BE: Yes, and the notes I have for the book I'm writing go back to that period of time. The start was in multinomial models and the exponential family representation of multinomial models. The year I was at Harvard, '67–'68, I taught a course on exponential families. But at that time, exponential families was more like multinomials and multinomial analysis and stuff like that. In the course were Holland and all the authors of that big green book on multinomial families. They were all in my course and also was Don Rubin, and so I had a very good course to teach. I don't know if I have any notes from then. I should go back and look. This having to move around has made me wonder where all my notes have gone.

Harvard was an interesting place and it still is. The difference in attitude toward statistics is still strong between the West Coast and the East Coast. What the difference is is somewhat different now. Now there's a lot more big data stuff out here and back there there's a lot more interest in social statistics and economics and census and government statistics.

BN: I did want to ask you about two topics in particular. First on biased-coin randomisation which I actually wrote code for, and also non-transitive dice.

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BE: I'll tell you about both of those things. First of all, where the biased coin came from . . . Early in my career as a biostatistician, I was asked to work with Henry Kaplan and Rosenberg. These are the people who were . . .

BN: Saul Rosenberg, yes.

BE: They were revolutionising the treatment of—

BN: Cancer.

BE: Yes, cancer or leukaemia.

BN: I think it was bone marrow transplant.

BE: Yes, something like that and they were running small studies and there was a question of sequentially assigning subjects to treatment or control. I came up with the biased coin design and the paper I wrote was one of my favourite papers (Efron, 1971) I've ever written because it had very little math and a lot of—

BN: Beautiful Markov chains.

BE: Yes, so that was really very satisfying for me. The dice are a completely different story. My wife at the time, and mother of my son, Gael and I were taking an auto trip through Victoria Island and there was a long trip. My mind somehow drifted to the old Steinhaus thing about non-transitive probability distributions. I somehow got thinking it would be fun to put them on dice and I was doing the computations in my head. They aren't very hard computations. I realised four dice were the right number and I worked it all out in my head. When I got back, I told Persi Diaconis about it and he was friends with Martin Gardner, and so I got published in 'Mathematical Games' (Gardner, 1970).

BN: The *Scientific American* column.

BE: Yes. I think somehow that gave me penetration into a market that I should have taken advantage of but I never did.

BN: But you have written a few pieces even on the bootstrap for *Scientific American* and so on, right?

BE: Yes, that was the second thing, again with Persi's help getting that in. That was a serious article (Diaconis & Efron,1983). There was also an article about Stein's paradox (Efron & Morris,1977). That's where we coined the term 'paradox' for Stein's rule. That was Carl and I writing together.

BN: So, back to research. At that time, who was your first student?

BE: My first student was Norman Breslow.

BN: Yes, and I think that mathematical genealogy shows that you had something like 23 students many of whom are famous. Breslow and so on, but of course we also have a colleague of ours, Rob Tibshirani, who was your student too.

BE: Yes, right. I have a real flaw as a professor and the flaw is, without quite realising it, I'm very private about the way I think and my door is closed. I'm sitting there working on things. I love my colleagues and I get a lot out of them but I'm just not the kind of guy who can sit around in front of a blackboard for hours talking things through and getting new ideas and doing it. I get worse rather than better (Figure 7).

BN: How do students find you and how do you find students?

BE: The students that I've had have always come to me and sometimes they've been foolish to do so, because I didn't help much. I had very good students by a form of selection bias because if they weren't very good students, they weren't going to do very well with me. At the beginning, of course, I was better at helping, but I would say that in Sequoia Hall I'm one of the poorer advisors compared with the people like Trevor Hastie and Rob Tibshirani and Emmanuel Candès and people who are really good advisors. David Siegmund's a terrific advisor. And Tze Lai.

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Figure 7. *With several of his students, celebrating his 80th birthday, 2018. Standing from left: Stefan Wager, Alan Gous, Arthur Peterson, Jr, Terry Therneau, Stanley Shapiro, Ronald Thisted, Robert Tibshirani, Bradley Efron, Karen Kafadar, Timothy Hesterberg, Leonid Pekelis and Abhinanda Sarkar. Kneeling: Gary Simon and Armin Schwartzman. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

I've always been a little embarrassed that I'm not. It's part of my personality and there's nothing I can do about it. People are good and bad at certain things. I love my students, I have always been very fond of the ones that I have had, and sometimes we've even done good work together—mainly sometimes after! But the only two people I've had extensive past collaborations with were Carl Morris with the James–Stein stuff and Rob with the bootstrap stuff. But lately you and I have collaborated, so you're my third most common!

BN: It's been fun.

BE: I enjoy it very much. Maybe I'm getting a little less private. But you see how I work. I type, type, type and send you something, hoping that you'll look at it and think more about it.

BN: To me, the main thing is that I've always admired the depth, maybe—how you grab on to an idea and take it all the way through and drag me along with you.

BE: Yes, I know. I tend to be assiduous at going after something. It'll bother me for months. I'll wake up at night trying to think about: what is it that we're really trying to learn? If I have one criticism of a lot of our literature, it's that people don't really pay as much attention as to what the question is. They want an answer and the answers are often quite formal and technical and, as Professor Cox said, don't add much to the final product. One of the good things about Sequoia Hall is people really do think about good ideas, not just technical proficiency.

BN: Okay, I would like to talk a bit more about the role of computing. Lately, of course, computing has begun to play such a big role in statistics. How would you characterise the role of computing in statistics?

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BE: Well, that's really a big question. The history of statistics is the history of frustration with bad computing. It took a person like Gauss who was an incredible numerical computer to actually do least squares and things like that—a simple one—and it was incredibly hard to do. The whole history of 20^{th}-century statistics from 1900 to about 1950 is people trying to get things that they could do—something that the theory was good enough to cover but it wasn't too hard to compute on the desk calculator.

And so the desk calculators started coming in around 1910 or '20, I think. A big step forward was when they got electrified. I think that was right after the war but maybe it was before. I grew up using them. A Monroe was a good one. It didn't make quite so much noise and it had mysterious black and red numbers that you could use. I remember at Harvard, '68, I watched Bill Cochran do a four-way ANOVA on the thing, using the full . . . Sometimes the dials would spin backwards and red numbers would come up but he knew how to do that. I couldn't do a correlation coefficient for 15 pairs of numbers correctly without screwing up entirely.

The transition to electronic computing was about removing an enormous bottleneck from the theory. A theory that doesn't have any applications sort of devolves into specialists playing games. Statistics really isn't a wonderful mathematical subject. It's an okay mathematical subject. Probability is a wonderful mathematical subject, but statistics comes into its own when it's useful and the computer just allowed us to do things, to do much more useful theories. That's what the book with Trevor Hastie is all about.

BN: I'm going to come to that a little later, but I wanted to know—you said you initially started using these calculators, despite the fact that you were not that good at it—

BE: But nobody was except Bill Cochran.

BN: But my question is: before we had software like S, which I'm going to come to, S+, how did you compute? Did you write Fortran programs?

BE: I did do Fortran programs for a while and I was about as bad as bad. It was incredibly hard work.

BN: Were there good facilities at Stanford for those kinds of programs?

BE: I never was one of the people to go over to the central batch place with a bunch of cards or anything like that. I relied on other people like Jerry Halpern, but basically did very simple things.

BN: You had a master, right, not far away, in Jerry Friedman.

BE: I didn't realise and of course I was thinking about things that didn't require . . . I was still thinking the kinds of thoughts you have when you're constrained very much by computation. For example, working on James–Stein stuff you don't really need very much computation to do simple cases. I do remember, when I finally got to the bootstrap, by then I was doing R and things like that. S, I guess.

I remember the reason I ever got into S. I was using Fortran and Rob Tibshirani, bless his heart, always said that I should try this new thing, S. I didn't trust anything. You know me—anything new is frightening to me about computing but I said I'd try it and it seemed pretty easy. I said, okay, I guess what I'll do is I'll use S for simple things and Fortran for complicated things, and there never has been a complicated thing!

BN: That's an easy transition, in some sense!

BE: And of course S got better. At first you couldn't write your own functions. You were probably in on the beginning of that.

BN: Actually, I came a little later. Do you have a rough idea as to what year this was?

BE: It was in the 70s. The bootstrap comes in at the end of the 70s, later anyway, and by then I was using S.

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BN: Okay. So I asked about the role of computing in statistics. What about: has statistics changed computer science? For example, at least currently, we do have several of our department faculty who have joint appointments in computer science. What are your thoughts on that?

BE: Statistics and computer science have both been called applied mathematics, but they have a different attitude towards science. Statistics, given its history, is a very science-tied discipline. Everybody in our department, even the most mathematical people, are tied to science and it's a wonderful thing. Computer science—of course they have connections but the computer part of that is much more important than the science part in the title. They live in that world of computing, and computing is everything they're interested in.

Our world, at least my world and I suspect most of the people, we have a set of problems. People come to us with data analysis problems and big classes of those problems emerge over time. We have in our mind working on methods that will get both the calculation and the inferences from the calculations in a way that's really useful.

BN: But isn't it the case that these days we do have a lot of computer scientists working with large amounts of data that has been gathered? Maybe not by designed experiments but still they do deal with data and people ask questions about the data or some questions that drive that.

BE: So are we and computer science getting closer together?

BN: That's exactly what I'm asking.

BE: There is some sort of difficulty in the way we correspond with computer science. We're envious of their tremendous reach and abilities and success but we're also a little scornful—that's too strong of a word. We think, or at least I think, that we go deeper into data analysis problems, that we look below the surface and so that's got to be good for both groups, right? We've certainly gotten a tremendous amount out of it.

Statisticians, when I started, were always hanging on to mathematics and mathematicians. You could get higher marks if you were more mathematical and the mathematicians liked you, so Charles Stein was everybody's ideal because he was such a great mathematician besides being a great scientist. Well, nowadays the envy goes to the computer scientists. Oh man, they can do all these enormous things. The more you're in with computer scientists, the higher your prestige.

BN: In our department we do have many joint appointments. I always like the statement—I think it was Rob Tibshirani who said that—we're very promiscuous collaborators or something like that, or maybe you said it.

BE: Our department was built in a genius fashion on joint appointments by Girshick and Herb Solomon, the naval research money. Originally the joint appointments were funded at least half by outside money. It made it possible for the new department to start to get quite a few people because we had support, so for the Dean of H&S—humanities and sciences—it was a cheap deal. You could hire a good guy at around a half price. That was a wonderful strategy both politically and intellectually because it's put the department always in a phase where most of the people aren't lost in some kind of abstract world. We have very useful citizens of the scientific world, by and large.

BE: Okay, so that's helpful. You mentioned that in the biostat workshop you were struck by the fact that there was a lot of frequentist analysis being done. Could you elaborate your views on, say, frequentist versus the Bayesian approaches? In other words, if someone asked you why you were into Bayesian, what answer would you give? In fact, I think you have a paper by that title, or something like that.

BE: Yes, 'Why isn't everyone a Bayesian?' (Efron, 1986)

BN: Yes, so can you tell us that?

BE: Why I'm not a Bayesian?

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BN: Yes, okay.

BE: Well, first of all I am sort of a Bayesian in that I've always liked Bayesian thinking. I just don't think it applies very well, by and large, to the kind of work we get from the medical school like the projects we've been doing recently. Bayesianism applies better to repeated situations where you keep having the same sort of problems coming up, but the problems we get out of the medical school almost by nature are all over the place and typically we don't have much past experience that we can bring to bear on the inference part. So the only trouble with Bayes' theory is that you have to know so much background to apply it and I've never quite trusted that. But whenever I have been able to use Bayesian thinking, it's wonderful.

With false discovery rates, I'd say my contribution to that was to put it on an empirical Bayes footing—so false discovery rates, the wonderful theorem by Benjamini and Hochberg, frequentist theorem, that you got exact guaranteed criteria. That exactness, that's a very frequentist kind of thing. It captured the attention of everyone and getting more exact false discovery rate control became very important. But I always thought that was not the important thing about the false discovery rate theory but rather the fact that it gave a way to get at what you'd get if you were a Bayesian without having to be a Bayesian. I thought that was a tremendous step forward, so that little book I wrote on empirical Bayes inference takes that point of view.

BN: You have several papers on empirical Bayes with Carl Morris, Rob Tibshirani and even, you know, we worked on the empirical Bayes deconvolution stuff (Narasimhan & Efron, B., 2020). I looked at the time period—it's almost 50 years you've been focused on this topic.

BE: There were three main Bayesian-influenced paper series. The first with Carl on putting the James–Stein phenomena in an empirical Bayes framework, and incidentally the story there is very simple, similar to false discovery rates (Efron & Morris, 1971, 1972). Charles comes up with this wonderful method that you can prove an exact result. The exactness, which is a frequentist kind of exactness, captures everyone's attention but Carl and I didn't think that that was the main thing about it. We thought that the main thing about it was that it made it possible again to do Bayesian thinking without having to put in all the Bayesian background. So that was the first two, and Carl and I wrote a dozen papers together. I've never done that before or since.

the missing species problem with Ron Thisted, 'How many words did Shakespeare know?' (Efron & Thisted, 1976). That's a whole other empirical Bayes story and I always thought it was a wonderful story, the missing species problem. Then the third go at it was the false discovery rate, where I spent a long time trying to put particularly the local false discovery rate in terms of Bayesian thinking. It has the disadvantage of not giving exact frequentist results and yet I think it's more important to say that, given the Z value you see is 3, the probability of still having an illness is 0.27 rather than to give an exact bulletproof algorithm.

BN: So the advances in computing make Bayes method more applicable?

BE: Well, the actual thing that seems to have happened is that Bayesian statistics is sort of disappearing under the big data. The big prediction algorithms and stuff like that couldn't be less Bayesian. If anything, they're frequentist but they're sort of beyond that pale even. They're almost atheistic. In order to use Bayesian theory you have to have enough probabilistic structure to state the problem and that's just what is missing. The big data people don't want to put on that kind of structure. They want to have things that are basically what you might call nonparametric but I think are just statistically unprincipled.

BN: I want to switch now to the other paper, a very important paper: your least angle regression paper (Efron *et al.*, 2004). I had been in the department for just a few years. I recall that I would see you, Trevor, Rob and Iain Johnstone talk before and after seminars in the corridor a lot and there was quite a bit of physical activity accompanying the intellectual effort. Can you tell us a little bit about the paper, how it came about? I think it has an interesting history too.

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BE: First of all, my office is next to Jerry's and when we moved into new Sequoia Hall I'd hear them working on the book and hearing them wasn't what I wanted to do. My hearing was better then.

BN: So, this was the *Elements of Statistical Learning* (Hastie *et al.*, 2001) book that Trevor, Rob and Jerry were working on.

BE: Yes. Basically, they were working on writing the book and the book came out and there was this amazing graph that showed the lasso and forward stagewise giving sort of the same answers. I looked at the book—it's a wonderful book—and I was reading it. It's hard for me to read almost anything new but I was interested enough and I had an idea that there'd be a different way to generate the lasso than stepwise. That's where the LARS thing came from—the least angle regression—and then I showed that you got LARS, lasso and the forward stagewise. I started talking with Trevor and then with Rob about it and they showed more things and then Ian showed the degrees of freedom result, which is pretty important. So that was my only real contribution to that whole world of lassoing.

BN: For me it was striking because, as I said, I was relatively new and I got to see how collaborative research worked, in a way.

BE: That was quite untypical for me. I'm not much of a collaborator. I'm a sit around in my office with the door closed guy at this time. Nowadays I sit around my house with the door closed. I don't have to worry about collaborations any more! I freeze up usually when people are around wanting to go back and forth about some statistical subject. I have to have time to think for myself. Anyway, it was very enjoyable. Some moments in my life when I've broken out of that cocoon have been with Carl and with Rob, with you sometimes and with that one particular paper. However, other times that's happened is with data analysis from the medical school. I don't think of the data analysis from the medical school as different than other things, writing papers and stuff like that. I think it's all of the same cloth.

BN: Journals. I know that you've been the editor of several journals. What has changed over the years? What is your view of the landscape at the moment?

BE: When I was quite young I was made an associate editor of *JASA* and then suddenly the editor of *JASA*. Let's see, it was in '76, '77, or something like that, and suddenly I was editor of *JASA*, which is a big job and papers would pour in. In those days it was all mail, regular mail, and I'd look nervously at my mailbox, hoping that more things hadn't come in but they always had! I would try and get rid of papers that looked very bad at first and then try and get associate editors to do them.

Things would happen, like somehow there was some trouble with statistical design. It was a subject that had sort of gone out of favour and there were only one or two people of the many *JASA* editors and one of them simply went black. No amount of effort would get him to respond to me and this went on for a year. Authors were crying and stuff like that.

Every Saturday I'd pull all the week's papers on my desk and start going through. I had the final say, of course, as editor. I'd get the associate editors' reports and I'd rate the papers from one to ten, ten being a really wonderful paper, one being garbage. There weren't very many tens. One thing I did realise after a while is there were a few papers that really made the reviewers angry and got nasty reviews. Most of the time that was justified but some of the ones that made them angry made them angry because they had a good new idea.

BN: Did they tend to have high scores?

BE: One of the papers I got I remember was from—we asked Jimmie Savage to write a paper. He'd given the Fisher lecture and he wrote a paper about rereading Fisher. It was a wonderful paper. It's still noted. Trying to get it into the journal—there was also a sort of bureaucracy that published the journal and trying to get his paper past the copy editors so he didn't have to be

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harassed was a hassle for me. Many years later I was asked to found the new journal on applied statistics, *The Annals of Applied Statistics*.

BN: Yes, I was going to ask about that. Great, yes.

BE: And now why was I asked? I guess I was considered an applied statistician, which is fine and I sort of think it was that. Anyway, I'm not sure there's much of a difference between statisticians—theoretical and applied. I hope not. Anyway, I made one brilliant decision, which was I was going to appoint three editors. I'd be the editor-in-chief but I'd appoint three editors who would have actually full responsibility in both sending out the papers and making decisions. I wouldn't over-ride them. I chose—let's see who did I choose?—Steve Fienberg for social science and Michael Newton for biostat, and Michael Stein for physical science, and who else did I choose? Suddenly my mind went blank there. Choosing three good people was all I needed to do and the journal was off and running.

Now what I hoped for the journal—they asked me to say what kind of papers would be applicable, useful, for this journal, and what I felt was that I didn't want to make it a strong line between applied and theoretical. Oh, I had to make one decision first that was very important: what colour should the cover be? A lot of them were taken and finally I chose pink. I thought maybe it would appeal more to women. It didn't. Anyway, I wrote a little thing about what kind of papers we'd take and it's still up there. I said that statistics covers a wide range of ideas from very applied to completely abstract and that we were interested in the left half of that. No more—I didn't want to be . . . But unfortunately I think they've gotten more fussy about: it has to be applied, really applied. I never liked that. I said to people, but if David Cox submitted the paper on proportional hazards, would we turn it down because it wasn't really about a specific application?

The other thing I tried to do, unsuccessfully, is to have less refereeing. I think we have way too much refereeing in our journals. It takes forever and it tends to focus on fussy little points rather than actually choosing the papers that have something freshest to say. So I wanted to have less refereeing but that was impossible. The culture is such that every associate editor has to choose two referees and every referee has to say something. One of the things in starting a journal—at first you're really afraid that you won't get any papers. I went and talked to lots of people about papers and I invited papers. I invited Don Rubin to give a paper on EM and stuff like that, and I got a note from Ray Carroll saying, 'Why are you giving Don Rubin stuff?' It was a good-natured letter and I wrote back that, well, Don Rubin was a very important person and he gets to do this, but you're a very important person—would you submit a paper too? It was a good paper too!

BN: What are your thoughts on reproducibility in terms of applied work and so on? You know, these days when people publish results, it would be nice to be able to replicate their results. We have all these new tools like GitHub and you can publish code packages. You can also make the data available where it's not encumbered by privacy concerns and things like that. Any thoughts on that?

BE: I'm sort of a contrarian on reproducibility. I don't think it's all that important. I think that obviously it's a good idea to have people be able to get the data and things like that but the system we have right now, the effective system on the ground, is that it allows people to submit papers who don't have the best facilities to work with or are eccentric. You can write a paper yourself sitting at home in India in a small place, not supported by beautiful equipment and people like we are, and if it's a good paper it can get published. You know, science grinds pretty fine. If things are wrong, people find out about them and I'd rather take the chance of publishing some wrong results rather than having very strict rules about how you can publish things—you know, you have to have the data in this form. Usually it's the form that the people who make the rules like to use.

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BN: I think I really do sympathise with your point about the people in other countries and other places who may not have the resources. But would you not say that now if you have access to a computer and you have access to free software like R or Python or whatever, I think that it would help for others to build upon your research if they could sort of redo what you did?

BE: Yes, I think, and all that's to the good, and I believe that. I just don't believe in putting strict uniform rules—every paper has to have the data this way.

BN: I get it.

BE: I think those rules tend to filter out occasional eccentric good papers and I really don't believe that we have a terrible reproducibility crisis in our scientific world. I think we have what you'd expect, a lot of regressions of the mean after something is published, when it turns out it's not as good as the authors thought. But if you start trying to rule that out, you'll rule out a lot of the fresh impulse from scientific publishing.

BN: Let's now turn to your books. You've written several books, including a biostatistics case book with Rupert Miller, Bill Brown and Lincoln Moses (Miller *et al.*, 1980), and then a bootstrap book with Rob (Efron & Tibshirani, 1993), and then the *Large-Scale Inference* monograph (Efron, 2010), and most recently the *Computer Age Statistical Inference* book (Efron & Hastie, 2016) with Trevor. How was the experience working with co-authors, especially since you have said you work a little alone?

BE: Actually, one of the book-like things that I did that was most successful was the little monograph, when the bootstrap first came out, *The Jackknife, the Bootstrap, and other resampling plans* (Efron, 1982).

BN: The SIAM monograph, you're talking about?

BE: That's been surprisingly popular. It's sold and sold and it's only about a hundred pages. I was asked to give a ten-lecture symposium. I was right in the middle of just getting ready, really rolling on bootstrap stuff. I was really thinking about things like the delta method, things that people use but don't pin down very carefully, and I really enjoyed working on that. It's a lot more technical than anything else I've done because, at that point I was on, technical seemed right to me. The biostat casebooks—I say with pride it was my idea that we should do that because I wanted to get this wonderful stuff I saw every week with Rupert and Bill and Lincoln into print. That was a lot of fun working on. I just have a couple of chapters in there. And then there was a long time I didn't write in any books. Then the book with Rob on bootstrap.

It's great to have something like the bootstrap that everybody's interested in. I've never had that again. I remember them coming out at one of the JSMs. The book was suddenly there and people were grabbing it off the pile. That book incidentally has absorbed two-thirds of all the references on the bootstrap. Look on my Google page of my references. By far the biggest thing in my reference list is that book. People like referencing it rather than the original paper, which is a good idea because it has a lot more stuff in it. It was written rather quickly and I've sometimes wished that I'd given a second try at it but Rob and I didn't. Neither of us really wanted to do a second try, and other people have written books. So that was a commercial success, I think.

Then a long time went by and working on the large-scale inference book was my attempt to put together something on empirical Bayes ideas, focused around the false discovery rates. It's a sort of peculiar book but it has its virtues. It's peculiar in that chapters 1, 10 and 11 are on one subject and the other thing in the middle is for something else on false discovery rates, but it was written when people were very interested in the false discovery rate idea.

BN: Yes, especially combined with all the new microarrays and all the data that was coming out of all the biology experiments.

BE: Yes, I guess you'd say it's . . .

BN: Bioinformatics?

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Figure 8. *With Trevor Hastie (left) and Robert Tibshirani (right), 2018. [Colour figure can be viewed at* wileyonlinelibrary.com*]*

BE: Yes, it definitely was connected with the surge of interest in AIDS. For example, survival analysis was very much related to AIDS. That was when it really got going. There was a methodology that was very nice for that, and then microarrays and false discovery rates go together awfully well. Every once in a while there's something like that. Now there's the prediction algorithms, and I was just reading some stuff about conformal prediction, which is a clever idea. Is it a good idea? I suspect there'll be some real advances in that soon but I'm not going to write another book! (Figure 8)

BN: What about the book with Trevor?

BE: *Computer Age Statistical Inference* is a love letter written to 20th-century statistics.

BN: That's a nice way to put it!

BE: I started on the book and got going—I was doing great on 20th-century. Then I got to the 21st and I realised that I wasn't the right person. I begged Trevor to please join me and he not only did, he did a spectacular job. Really, choosing good people to work on something is a wonderful administrative tool! That was so much fun to work together with him and I learned so much from Trevor. He has a different kind of point of view on statistics—not completely different, but he is just such a natural at multivariate analysis, things like that, and fitting it all together and using it. He doesn't break a sweat and it's inspiring. And so that book was pure joy to work on. It's been very popular.

BN: Yes. I think, just like you mentioned about the conference selling out for the bootstrap book, this book also sold out a lot.

BE: Yes, it went over 10 000, which made me happy. There's going to be a paperback version that has problem sets in it, if you want to use it to teach.

BN: Not to put you on the spot, but any more new books you're working on?

BE: Right now I'm finishing, and Cindy Kirby is composing, the exponential families book, which is centred around generalised linear models. This, as we discussed last time, goes back 50 years of notes. It really isn't that old because a lot has happened. It's more of a textbook than the other books. It really is intended to be taught from. Also, I have to say, in my dreams

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perhaps, I've been writing a ‘popular book’. If you go to the bookstore—when you could go to the bookstore—there is a shelf of books on teaching quantum physics to your dog and stuff like that.

BN: There used to be *Physics for Poets* or something like that.

BE: All of those books have about two ideas in them. There is a picture of Einstein and a train going by and lightning strikes on each side. I found when I tried to write some of this—and I have tried to write some of it—statistics is a much harder subject to bring to the unwary. Most scientific subjects are about some natural phenomena, so physics is about astronomy, stars, geology, rocks, medicine, people's health, but statistics is a subject whose natural media is what scientists do. We start one step further back from immediate reality and that makes it very hard to explain to people what it is that we're really doing. So every time I've tried to write my popular book, I realise how hard it is and how difficult it would be to be truly popular.

BN: Let's talk a little bit about your awards. Throughout your career you've won lots of awards: the National Medal of Science, the BBVA award with Sir David Cox. You both also won the International Prize in Statistics. What are your thoughts on that?

BE: They make you feel good for a couple of days but doing the work's a lot more important than getting the award. As we talked about last time, the one that actually made a difference was the Merit Scholarship. That made an actual difference. I like being honoured—I'm not above that kind of thing—but there's something ephemeral about all such things. What's left after all the honours are gone is what actual effect one has had on the development of your field. You'd like to think that the honour has actually honoured something that was important.

Amongst the honours I've gotten that I somehow really appreciated were ones that had a more personal feel to me, one of them being on the 50th anniversary of my graduation from Caltech I was chosen as the outstanding alumni or something like that. That made me feel awfully good because I remember how smart my fellow Caltechians were. Incidentally, I didn't really feel I deserved that one because a couple of the people made much bigger splashes but I took it anyway! I actually went to the reunion—I never would have gone otherwise.

Then I really liked getting the Guy Medal in Gold because England always seemed to me the home of statistics and, oh boy, they're recognising me so that was awfully nice of them. The International Prize in Statistics—they make no bones about it, the idea is to publicise useful ideas in statistics, so the reward is for the bootstrap, not for me in general, and for Professor Cox it was for proportional hazards. I guess there's a couple—I guess there's going to be more in our department.

BN: I think that you also served the community. In our own department, I think you were the chairman more than once, if I remember.

BE: Three times. Two and a half times, actually.

BN: And you also served as the president of American Statistical Association and IMS. Any thoughts?

BE: I'm an indifferent administrator. I am good at appointing good people.

BN: That's a skill.

BE: I used to care a lot more about these things. I used to care a lot about Stanford's administration and trying to get Stanford . . . I was elected to the first Faculty Senate and I have a little clipping in a scrapbook somewhere that says ‘53 to 1 vote in Faculty Senate supports administration’. I was the 1! And it was about—I felt they should be giving bigger raises. I was a quite annoying person in my youthful days.

When I was first an assistant professor here and then associate professor, they had a special programme for faculty leaders of the future in which I participated, so I got to know more about how the university is run and it's pretty interesting. Stanford's a well-run university, by and large, and ‘well-run’ means not overly run. The departments have a lot of flexibility and

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when you think of how well they've supported statistics over the years, it's not an accident that Stanford has the best department. They've really supported the best department. And so I have a lot of fondness for Stanford and its administration but I don't want to participate any more.

Besides being the chair of the Faculty Senate, I was also the chair of the faculty advisory board that is the last barrier before you're given tenure. I eventually resigned because I was getting too mean. I started thinking that nobody deserved to get tenure. I was somewhat out of sympathy with what seemed like political appointments, which I now see were mostly correct. Anyway, so I haven't done much in faculty administration for a long time and the world has gone on anyway.

BN: I cannot but bring up the subject of Sequoia Hall. The name Sequoia Hall is dear to you because on your webpage there is a picture of you in old Sequoia Hall. As a graduate student, I used to hear about Sequoia Hall from Hani Doss and Fred Huffer who were professors at Florida State. Can you tell us about old Sequoia Hall and the role you played in designing the new Sequoia Hall?

BE: The very term ‘Sequoia Hall’ warms my heart every time I hear it and I think it stands for a proud statistics tradition that goes back to 1900. So you know that the history of Sequoia Hall—it was the women's dorm originally. It was a counterpoint building to Encina Hall, which is at the other end of the front quad and it was a three-storey big old building. It housed the 500 women that would be admitted to Stanford every year. And so there was a fire and the building was partially demolished. Only the first floor was left. It was the second home, I think, of the statistics group. The first one was up on the hill in some place.

It was a dump—there was no way around it—and the rooms were old and funny but they were pretty big. When I was first here, my first appointment as something like a lecturer for one year, I had an office at the far back, pointing toward the hills. The building stuck out that way. And there was something funny stuck to the old curtain, sort of a badge or something. I looked at it. It turned out to be a radiation badge, because one of the very first linear accelerators was under the ground going in that direction.

Then I got my first faculty office. It was at the corner of the building nearest math. I was in that office for a long time. It had a radiator that clanked a lot during the winter and it wasn't a very big office, but it had an L shape to it. When we went to design new Sequoia Hall—they were awfully generous just to let us help design it—I insisted that they make the offices have an L shape so we didn't have to spend the rest of our lives in a box, and I think it's given some character. It was Iain who prevailed upon the administration to give this the name Sequoia Hall for the second going. We didn't have to name it after somebody.

BN: I think that even though many new buildings were coming up in the university, our building is unique because it has offices that have transoms and windows and things like that, which you asked to be put in.

BE: Yes, I pushed hard to have the geometry and in particular wide halls, which we said were for where people could have conversations but that wasn't really . . . When I thought about it afterwards, the wide halls are very much more inviting to the building. They make it seem like you're not claustrophobic in the building and I think it has to do with the popularity of the building. People who have joint appointments always seem to want to be in Sequoia Hall. That's a wonderful thing. Of course, it's crowded the building.

BN: We're bursting at the seams. We're full, I guess.

BE: Yes, and the transoms were so you could see who was in or not, and we have windows that open. We asked the faculty whether they wanted air conditioning or windows that open. They said windows that open. Then afterwards, the buildings that were built after that, they did both, but we never could get in on that. The building of our department is something that's very close to my heart. I mean the physical building and the people that are in it. If I have something

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I'm proud of, it's that I helped bring good people, the very best people, to Stanford. I won't name names. You know who they are.

It's been a wonderful continuity to have two nice buildings called Sequoia Hall. At one point we were crowded and when I was still a graduate student Herb Solomon had the basement sort of dug out. There was a basement but it was almost abandoned. He had it, quotes, fixed up and I had an office down there for a while. It was really like being in Hitler's bunker or something. I do wish we'd put a basement in the current building.

The current building was almost an afterthought. There were previous plans to have a new quad over on the side where we are now and they were bad plans. They were crowded. The great thing that happened was the Loma Prieta earthquake scotched those plans. Then nothing happened for a few years and then the Gerhard Casper administration got together big fundraising and we were going to have these new buildings. A lot of the money was Packard money. They were very generous. They sort of threw in Sequoia Hall. We didn't have to raise money for it. They sort of threw it in as a little bonus to us.

BN: I think that we are coming to the end a little bit here so I just want to ask you for some final thoughts. For example, the world is now awash in data and there's a lot of attention being paid to machine learning, AI. I know that you've thought deeply about the place of statistics in modern science and in your talk at the ISI World Congress last year you touched upon some of these issues. Can you summarise that for us?

BE: Well, of course I always worry—statisticians are always worried that their little field is going to get lost beneath the heels of the big guys. It was math or biology or computer science but we seem to do okay. Our current crisis, the Covid crisis, is the most statistical crisis I've ever seen. It's the biggest statistical story ever, from my point of view. The papers every day have beautiful statistical analyses. People are asked to understand difficult things. If this doesn't do it for us, nothing ever will! The term 'epidemiologist' has become famous and statisticians in there too. What do statisticians tell us about this cure or that cure? There's going to be a lot more of this because people are desperately interested in vaccines and cures. Remember there was this one drug that they treated eight people and they said it was going well. Nobody even knew it was going well and people were jumped all over so there'd be a lot of that.

The *Computer Age Statistical Inference* book makes the distinction between the two levels of statistics, the algorithmic level and the inferential level, which is somewhat an artificial distinction but a pretty good one. It says that the first level is doing something and the second level is understanding what you did in the first level. The algorithmic level always gets more action, in particular in these days of these big prediction algorithms like deep learning. You'd think that's the only thing going on. It isn't the only thing going on. The deeper understanding of the kind of thing that Fisher and these people—Neyman, Hotelling—did for early 20th-century statistics, putting it on a solid intellectual ground so you can understand what's at stake, is terribly important.

That's going on again now—people struggling to do that for this new environment of enormous data sets, predictions, things like that. That was what that International Statistics Institute paper was about, that struggle. Are these prediction algorithms really doing the job they're supposed to do? It's an awfully interesting subject. Of course their successes are a great argument for them but it's not the only argument. You can see from the disarray in these prediction algorithms they're different from each other. All of them are. There can't be four different prediction algorithms. There must be some central idea in there that's going to be grasped. I think it's part of the deal that statisticians are supposed to tell you what's at the essence of these things and that's our big job that we haven't done yet. Some genius is out there going to do it.

BN: Well, Brad, thank you so much for all your time. It's been fun and a nice ride with you through history.

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BE: Thank you, Naras. That was really fun for me. Talk to you soon.

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