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An interview with Ross Leadbetter

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1 Introduction

Malcolm Ross Leadbetter has made central contributions to stochastic extreme value theory during more than 50 years and has coauthored three influential books. He has also obtained early important results on density and hazard rate estimation. He has provided long-term statistical assistance for USEPA research in support of environmental legislation, and has worked for the US Navy on increasing ship safety. Together with Stamatis Cambanis and Gopinath Kallianpur, Ross ran the Center for Stochastic Processes in the University of North Carolina Statistics Department for more than two decades. The Center had a crucial impact on the careers of very many young researchers, including the second interviewer who did research there for altogether two and a half years, and on the first interviewer who wrote his thesis under

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Ross' guidance during the startup years of the Center. This interview was made during the successful 8th Extreme Value Analysis conference which was held July 2013 in Shanghai.

Ross was born on Christmas Eve 1931. His father was principal at the high school in his native town of Invercargill, and a world-class sprinter, and his mother was a teacher and homemaker. He received an M.Sc. degree (first class hons) in mathematics from the University of Otago, New Zealand in 1954, a BA (Wrangler) from Cambridge University in 1958, (MA 1963) and his Ph. D. from the University of North Carolina in 1963. He worked for the New Zealand Applied Mathematics Laboratory 1955–56, for the New Zealand Naval Research Laboratory 1958–60, and was employed as a mathematician by the Research Triangle Institute in North Carolina 1961–66. Ross was appointed Associate Professor of Statistics at the University of North Carolina, Chapel Hill in 1966, and promoted to Full Professor in 1968, a position that he still holds. He has received honorary doctorates from the University of Lund, and from the University of Lisbon. In 1955 Ross married Winsome Wallace of Oamaru, New Zealand. They have 4 children and 8 grandchildren.

2 New Zealand and Cambridge

Tailen Hsing: How did you become a mathematician and what was it like to become one in New Zealand and then Cambridge?

Ross Leadbetter: In New Zealand, one just goes to the university and does the sort of things one has become good at in school. In my case it was mathematics, physics and Latin, but Latin didn't seem to have a great future so I concentrated on science. In New Zealand you have a general undergraduate education, rather than anything too specialized. I did mathematics, physics and chemistry. It's not like England where you can do purely mathematics and learn everything about nothing, or like the US where you can learn nothing about everything, so to speak. I don't know what it is like in Taiwan and other such places but that's what I did at the university in New Zealand. Then I graduated with master's degrees in math and essentially in physics, and sought a job to use these in the capital city of New Zealand, Wellington. There I located a government science lab titled "Applied Mathematics". So I went there and asked them what openings they might have, and they said we only do statistics here. So I said, well I can try that, though all I knew about statistics was that it seemed to be a form of witchcraft, at least the way the English tradition had been conveyed to me.

T: When was it?

R: That was in 1955. But anyway I was given a probationary job and a nice book on Statistics by Wedderburn to read. Have you ever heard of Wedderburn? A very ancient book, and I learned statistics from it and by practice from the splendid statisticians that were there in this Lab. They quickly taught me how to do covariance analysis, amongst other applied statistics. They said I should take logs before I did anything to data, which seemed a bit dubious but advice that was very thought provoking. They had some outstanding people including Peter Whittle, a pioneer in time series analysis, and he was already in full flight in developing parametric models. The comparison between him and Ted Hannan in Australia was quite interesting because Peter was quite informal but lucid about his mathematics, whereas Ted was thoroughly complicated and abstruse about his. When Ted gave lectures, people would come for a little bit and then give it up because they couldn't follow it too well. But he was very accurate in all respects. I can remember Peter reading a review of one of his papers by JL Doob, who commented, Whittle should pay more attention to rigor. I don't know whether Peter would want to be quoted on this (or even remembered it) but his immediate retort was that he was busy discovering new things and he would leave rigor to mathematical technicians like Doob. I learned a lot about applied statistics, how to do discriminant analysis for earth dams, and that sort of thing. Peter also gave me some guidance on getting into a little bit of research. So I read a paper on soil moisture, and studied some stochastic models on this topic, with his guidance. It was a lesson to me on really how everybody has their role, some of us are theoretical, some of us are applied, but we can all make contributions if we work to the best of our ability, big or small. Also, the comparison say between Peter Whittle and Doob, or Ted Hannan, are obvious examples of mighty contributors in their own distinct ways. After that I joined the New Zealand Navy as a "Scientific Officer", and went to sea on a minesweeper, issued rum to sailors, learned navigation and did research for their lab on sonar signal detection. As part of the bargain I went to Cambridge to study, for two years, doing their so-called Mathematics Tripos, graduating as a "Wrangler" in Part II, and "Honours with Distinction" in Part III.

Holger Rootzén: Did you already have the first two years in New Zealand?

Winsome and Ross in Trafalgar square on the way to Cambridge, in 1956.

R: Yes, I had Master degrees already in math and physics. So in Cambridge they taught me more mathematics and the real truth about statistics, English statistics.

H: Who were “they”?

R: Mainly the Statistical Lab. David Kendall was of course there and certainly was a very rigorous probabilist. Dennis Lindley was the main player and I saw his conversion I was there when it happened.

H: Oh okay, was it a religious conversion?

R: Yes, it was. It was a Damascus Road (or Mill Lane) incident, like the Apostle Paul. He was converted from being a solid $(\Omega, \mathcal{F}, \mathbb{P})$ statistician to being an absolutely Bayesian crusader. He came into class one day, an inference class, and said that he had been reading this book by Jimmy Savage and if you believe the first premise of the book, you had to believe the whole Bayesian gospel.

H: Well, just like the Bible.

R: Yes, that’s where he came from. It was right there, it happened in an instant! After that tour of duty with the Navy [and after Cambridge] I worked some in the [Navy] lab. They were doing things with sonar arrays for signal detection. They would do what they called clipping of all signals which the Americans call limiting, based on zeros and ones, positives or negatives, that was part of the religion, just like taking logs of data before you analyze it. I told them it could be much better if they did something more optimal, e.g. if they added and squared, obtaining a chi square statistic out of the system. So these colleagues, who were mainly engineers, built a processor to do this and we tried it out. It didn’t work at all well! Its problem was that it drifted all over the place as the electronics warmed up or drifted. So anyway they just put it on a shelf and went back to clipping everything, which of course made perfect sense really. When that was over I went to North Carolina.

T: From New Zealand?

3 The Research Triangle Institute

R: Yes, I was offered a job by the Research Triangle Institute, which was just starting with Statistics Division directed by Gertrude Cox, who had really founded statistics in North Carolina. When in 1940 they were looking for somebody to start experimental statistics in Raleigh, at North Carolina State College, [George] Snedecor from Iowa State wrote a famous letter which read: “If you would consider a woman, we have one to suggest.” I went there in 1961 when Gertrude Cox was retiring from her Raleigh department and creating a statistics division in the newly formed Research Triangle Institute. Earlier she had already persuaded the University of North Carolina, in Chapel Hill, to start a Mathematical Statistics department. It was in 1946 she did that, and she hired giants of the field, particularly [Harold] Hotelling, [Wassily] Hoeffding, [R.C.] Bose, [S.N.] Roy and Herbert Robbins, who produced all kinds of methodology and theory and started the department on a fantastic note. Anyway

Ross graduates from Cambridge, in 1958

I took this job at the Research Triangle Institute and became one of what Gertrude called “her boys”, with sort of a hint of reverse chauvinism. Gertrude had this wonderful notice on her desk, “I may not be always right, but I am Boss!” She was a dominating figure and really created statistics in North Carolina and developed it dramatically in the US and internationally through the International Statistics Institute. She had interesting hobbies; an avid grower of roses, and also of exotic orchids. Gertrude used to have these wonderful Christmas parties inviting “her boys” and their families and issuing them with gifts, Lego bricks was one she gave our boys as a gift from her travels in Denmark. She took a kind of family interest in all who worked for her. It was just amazing.

H: What was she like as a person?

R: Wonderful, totally nice, kind of grandmother in a way. She’d run an orphanage in her younger days, so had that concern for children. She had a huge doll collection from all over the world, augmented by gifts from her friends traveling to unusual places. Gertrude was never married but she had a boyfriend whom we all knew about. And this was a boyfriend in a very old fashioned, proper sense I’m certain. She just used to treat us as if we were part of a family. Sometimes she couldn’t remember our names very well and occasionally introduced me: “This is um, this is um … Leadbetter.”

H: She must have been a pretty forceful persuader too to get started all these things. She must have been pretty good at talking to people and convincing them.

R: Oh yes, Gertrude was very imaginative in getting things done. And that of course was evident. She had the saying "We don't say if, we say when", to proclaim this. She also had the maxim that she would never hire anybody that she didn't believe to be much better than she was, which is an unusual quality in a boss really.

H: Yes, I actually remember you told me many, many years ago about this and I remember it all the time. It's a good maxim.

R: And this was evidenced by the way she went to an ISI meeting and heard from [Harald] Cramér that he was retiring from his Swedish Chancellorship and persuaded him on the spot to come to North Carolina to the Research Triangle Institute for a visit, telling him she would have important work to be done to model reliability of manned spacecraft systems in the US for NASA. So she pedaled this to him and I don't know if he believed it totally. But to quote a letter he had written to her: "when I consider the important work that I am going to do, I will need to stop on the way and spend a few days with Feller and find out what's been happening in the field while I've been administrating." Sort of tongue and cheek kind of thing. She came back and asked us "How would you like to work with Cramér?" We haha-ed, "Gertrude, tell us something real!" Two months later everyone was at the window as Harald Cramér arrived in a car and got out and walked up the steps into the building. So there he was and that was sort of typical of her operation. He of course was most interesting, and could always get his way nicely for things he needed. I probably already told you about the typewriter but let me say it for Tai's benefit and [for] the [recording] machines. He asked for a typewriter and they said "Professor Cramér we don't provide typewriters, we have secretaries to do your typing", to which he responded, "And do your secretaries take Swedish?" So he had his typewriter within the hour. He needed a couch for a nap after lunch and we didn't have couches anywhere really, but one was produced and installed in his office.

Harald Cramér and Ross in Research Triangle Park 1963

T: What were your duties at the Research Triangle Institute?

R: It was a so-called non-profit research organization that existed on industrial and government contracts so we had a number of those and if you didn't have a contract then you had to charge your time to what was called overhead and that was not viewed favorably by the administration. At that time Geof Watson became my main research advisor.

H: He was at North Carolina too?

R: He was not, he was employed by the Research Triangle Institute but was waiting in Canada for a proper visa arrangement to move to the US permanently. He did not have his registration card or whatever. He was a good friend of Gertrude's and was employed by them and he had a navy contract and part of my work was to collaborate with him on that contract. So that is when we started with estimation of densities and hazard rates - function estimation of one kind or another.

H: Was that after your work with Cramér, or the same time, or before?

R: No it was really before Cramér came but not long before.

H: So you started working with Watson almost the moment you got there?

R: Yes, and I also registered for study at UNC after being there a few months and arranged to "buy" a doctorate from them, based more on the courses I had done at Cambridge and previously. But I wrote my thesis at RTI.

H: Was Watson coming and visiting or couldn't he come and visit?

R: No, until his visa was cleared. He got a job in Montreal, I think it was, and then he never came permanently to the Research Triangle again. He took a position at Princeton where he spent most of his time.

H: Did he stay there the rest of his career?

R: Yes. We regularly corresponded and he would write me letters. He was very, very innovative in his ideas and he would write saying "Ross, I assert, you prove ...", because he didn't have the patience for rigor. He'd be happy to informally interchange the limits and an integral but from the point of view of writing a paper, he recognized it was good to have it done rigorously so in a way that was my role to take his ideas and see where they would lead mathematically. So that's how I got involved with density estimation.

H: Density estimation was sort of invented by many people at the same time. What is the history about that?

R: Yes, Rosenblatt was the first, in 1956 - the same year he invented strong mixing. I wonder which he now regards as his greater contribution?! Then Peter Whittle was next in 1958 - in a Bayesian framework.

T: What about Parzen?

R: Parzen published 2 papers - one in 1958 on spectral density estimation which I used for its nice classification of characteristic function decay in my thesis and our own 1963 paper (with Watson and Leadbetter (1963)). Parzen's paper on probability density estimation appeared in 1962, just ahead of ours, oddly making less use of his interesting spectral density methods than we had as I recall.

H: You had a more general kernel, maybe depending on the parameters?

R: You have very good memory, Holger, if you remember that far back and you're so young but yes that's right. A lot of the people in density estimation were what Georg Lindgren called "Window carpenters", constructing windows, of course and taking them also from spectral density estimation. I was more taking a line of using general smoothing functions rather than just (convolution) windows and really in a sense rigorizing the use of delta functions which I learned in quantum theory from Dirac, and I had this fixation with making that work like with Stieltjes integrals or sequences of smoothing of functions. Dirac was horrendously non-rigorous. With interesting ideas such as his duality of "< bra] vectors" and "[ket> vectors" and putting them together cutely to write <bra-ket> for their inner product.

H: I never heard of that.

T: Oh yes, it's standard terminology in physics.

R: It's part of Dirac's invention but the whole business of delta functions was fascinating to me, so that was one reason why I liked to do this density estimation in a general sort of way, which I later realized was like a version of Schwartz Distributions. Then of course we looked at hazard rate estimation and that was a little bit more untidy in a way, but it wasn't virgin territory anymore. It may have been more useful, I don't know.

T: Did regression function estimation come later?

R: Yes, that was just a bit later.

T: The Nadaraya-Watson estimator?

R: That's right, I did a little bit on that but Geof got into it more - I don't remember what Nadaraya's role was. My PhD work took about 21 months. UNC wanted to charge me "out of state fees" even though I was paying NC taxes!

H: Was it all work you started when you came to North Carolina?

R: Yes, and Geof introduced me to the problems.

H: It was pretty quick work.

R: Yes, I guess, we probably worked totally for a couple of years from start to finish. But it was exciting to do and as I say I was particularly intrigued with bringing concepts from the "new" quantum theory to function estimation, even if you know you might consider them fairly obvious at this point in time.

T: Perhaps we can move on to your time at the University of North Carolina. When were you hired at North Carolina?

R: During the time I was at RTI I was doing some adjunct teaching (as you might guess of Measure Theory), at the same time as I was doing my degree. In 66 I started permanently, full time and left the Research Triangle.

H: So you started at the Research Triangle in '61?

R: Yes, I was there for 5 years and then in '66 I went full time to the university.

H: Did you write the book with Cramér and Leadbetter (2004) during the last 3 years?

R: Yes, that's right; that was mainly done at the Research Triangle Institute. I can't remember when the writing finished but it must have been about the time that I left.

T: When did you get your PhD?

R: In '63.

T: Did you start to work with Cramér after that?

R: Yes, I'm not sure what year he must have come, '62 probably.

H: What was it like writing a book with him?

R: Oh, it was "awesome" (in the modern vernacular). He was very generous in sharing the writing and I think he quite fairly and honestly recognized he had things he could fairly easily do with the theory from what he had contributed himself to the field, with spectral representations. I wrote more about upcrossings and phase locked loops, the sort of things that I had some engineering experience with. He said "Ross, I just couldn't have done anything with this section of the book." So I think it was realistic but he was just very generous in sharing the writing and his English of course was amazing along with his knowledge of every other language under the sun.

H: Okay so you didn't have to improve his English or anything?

R: No, occasionally I would find a word that was a little amusing. One I do remember was the word "conception."

T: Instead of concept?

R: Yes, instead of concept. He used to joke "Americans live in their cars from the moment of conception until the last accident." I never knew whether he fully appreciated what he was saying or not. He just wanted to say that society is built around the car and that they are in them from birth until death, but he stressed the moment of conception when he said it and he may well have understood it fully. I don't know.

H: So it was pleasant to write with him?

R: Oh yes, very exciting. He would look over what I had written and I would look over what he had written. I had made one sort of foolish comment to him, I had written a set of notes about Hilbert spaces that I had thought were very geodesic and did the theory in a very concise sort of way which I thought might be useful somehow in the book and so I showed it to him and he just looked at me, "Ross, I do know about Hilbert spaces." So that was a mistake! But he wasn't mad at me or anything, and of course he wrote his thing about Hilbert spaces which he had done many times before in various ways. Of course it came through with his supreme authority as it was written.

H: And you did most of the rest of the book?

R: I think we did about half each, because we had worked together on much of the material.

H: Did you write papers together too?

R: Yes, we wrote a couple of papers, particularly on moments of upcrossings which I think was the main one we did together. It didn't do me any harm to work with him! He was a gentleman as you know in all respects and a total counterexample to the impressions that I have that great scholars must have character deficiencies (what I call the "Law of Total Maturity"), because he didn't have any. I only once heard him being critical of someone in statistics. He obviously felt quite strongly about it but I was quite surprised this one time I heard him say a negative thing about somebody. He and I went to a meeting in Copenhagen, I probably told you, it was about '67 I think, on extreme values, which would be another predecessor to our EVA series (e.g., "EVA -1"). Gumbel was giving a paper there. It was horrendously heuristic and he was saying things like a_n tends to b_n which I found totally contradictory to everything I had ever been taught and trained. So I said to Harald, "Do you think this is really credible what he has done?" He chided me, gave me a short lecture on tolerance and said we all have our own roles, which really surprised me since he was above all other people in my opinion a stickler for precise, exact mathematics. I don't suppose even his mathematics was 100 % totally precise (perhaps just 99.99 %!) but that was because of the climate of the times. In his book Mathematical Methods he described the Borel sets as obtained from operations on open sets, which doesn't hold logical water, but it was the way it was done at the time. But as far as I know that was the only "blemish" that might be modernized from his entire book. He was just great to work with, very generous and also working with him later in his career was very advantageous, when he was a little bit easier to keep up with. He would want to take a nap now and then! He was very astute and I had a chemical engineer friend who worked there who said "Cramér's big success is that he chooses his problems that he knows have solutions." It was uncanny - he would just work on such things and get answers.

I may have one more anecdote about Cramér (and Fisher) that doesn't involve me personally. Cramér had a lot of stories about famous people like Niels Bohr and Norbert Wiener. Some were apocryphal but one that was certainly factual was how he gave a lecture in Paris at the height of the Neyman-Pearson-Fisher controversies. He gave it in French because he liked to talk in the local language, speaking any local language that was offered to him. So he was going to say some things that were critical of Fisher when Fisher walked into the lecture. However, he decided to just continue anyway and said these things. Then, at the end of the lecture, Fisher went up to him and said, "Wonderful talk, Harald, but of course I don't understand French!" I never knew Fisher but I was curious to see him when I was a student at Cambridge and went to a doorway where he was giving a class. I didn't want to go to the class because I thought I might catch some bad fiducial bug or something. I just took a look at him when a student came late into the class. Fisher always started earlier than was customary for lectures, on the hour instead of five past something like that. When the student entered Fisher bellowed at him, "What's your name?" and the poor guy was so nonplussed he could not remember his name. Anyway, I did see Fisher when he came to North Carolina and gave a talk, wearing his traditional suspenders.

4 Starting at UNC

H: I interrupted Tai. So then you were hired at North Carolina as a tenured associate professor?

R: Yes, I think I was hired for a week as associate professor without tenure. I had been promised the tenure by the chairman, George Nicholson.

T: Was it common at the time to be hired as tenured?

R: No, not at all but it wasn't like it would be today with very, very fixed rules. The departments really decided what they wanted to do and the administration was there to support them, and the deans and provosts and even the chancellor would be faculty members typically, who were doing a tour of duty, but they had a faculty orientation. Not as now where they have these revolving door administrative positions and the corporate model of the university with chancellor as CEO and uncountably many deans. Our chairman George Nicholson - he carried a lot of weight with the chancellor - decided about tenure (I had a competing offer from Yale). It hadn't appeared in the letter of offer that I should have tenure as I had been promised. When I spoke to George Nicholson about it, he said, "I'll call the chancellor." The chancellor was away in the bathroom or somewhere and so he didn't get an answer for an hour or so. George Nicholson just assured me that it would be corrected, and it was.

H: What was it like to start in the department and being there in the beginning?

R: Well in a sense it wasn't much different because I had been teaching there so my entry into the department was really five years previously when I had arrived [in North Carolina]. I had several roles then, one as a student, not a real student, but writing a thesis, and also that I was teaching a bit and so I sort of became involved in the department in those 5 years already. But I was immensely proud to become a full member of this illustrious faculty.

H: Were people like Hotelling and Bose still there?

R: I actually took Hotelling's position because this was at a time when you replaced somebody. I was very proud to be Hotelling's successor. But that of course was the moment the department started to go downhill!

H: I don't think so.

R: But Hotelling was really past his best then. As a matter of fact, Gertrude had to come across and persuade him to give up of chairmanship that he originally had because he wasn't doing that effectively. And he wasn't an administrator really - his primary value was his immense scholarly reputation, and the ability to attract other splendid scholars to the department first faculty. The Hotellings used to have tea parties once a month (surprisingly called "Hotelling's Tea"!) We were sort of all, students, faculty, anybody, expected to go there. His wife, Sue, was a fearsome person and a character in her own right. If you want to make another recording you could have my wife, Winsome, tell you all about Sue. I remember when Winsome and I went there once and Hotelling got Winsome in a corner. He was very expansive physically and he stood in front of her and explained to her about the correlation coefficients at great length.

T: Did she get it?

R: No, not at all of course. But one time we had a visitor, a friend of Winsome's and Winsome was explaining to her the people she would meet at a Hotelling Tea, and said "You will meet someone called Bose she said and he's a bit egotistical." Bose was very, very good, indeed a pioneer in designs and coding theory. But he knew he was important. Winsome said to her friend, "When you are introduced to him you must say "Oh, are you THE Bose?" So when we were at the party, there was a young woman statistician from Wilmington who was visiting Chapel Hill and she was very, very impressed to be with this crowd of people. She was introduced to Bose and that's what she said "Are you THE Bose?" Bose sort of tried to look a bit coy. Of course he wasn't THE Bose anyway. THE Bose presumably worked with Einstein. Funny, Jennifer our oldest daughter, then 3 years old, had heard this conversation at that time and when 6 months or so later Winsome said something at dinner about Bose Jennifer looked at her and said "Do you mean THE Bose, Mummy?". Anyway the Hotelling Tea was a real (pleasant) ritual. Sue Hotelling was hospitable, she was a strange character. You didn't want to tangle with her, as insurance companies found out when her son went missing when he was flying an airplane. It used to be said that she was extremely sensible about important things. In North Carolina we had to declare property taxes every year to the county and had to fill a form saying what furniture we had. Sue would fill out this form saying (truthfully!) that their stuff was worth almost nothing. So then they could send someone around to inspect. So they sent this person around to the Hotelling's house to inspect their furniture. "Come in, Come in" says Sue, "sit there". He sits down there on the couch and the leg falls off the couch! End of interview! I had a tax audit, I was new to the IRS rules and I did something unorthodox like filling out my federal tax before I filled out my state tax Anyway, they audited me and I went to face the audit, and who should be coming out from the auditor but Sue Hotelling, and the auditor was in a very bad frame of mind from this encounter. Apparently she had asked Sue, "Mrs. Hotelling, what about this political contribution? Who did you pay that to?" Sue's response was "You're not allowed to ask me to whom I paid political contributions!" That was the kind of dominant, assertive person that she was. Winsome, we all, got on very well with her. They went to England, Sue and Harold, and left their young children in the house, the oldest being maybe 10, I don't know. They used to let them run naked around the lot. They were sort of an institution in Chapel Hill, the Hotelling family.

T: At the Tea Parties, did you discuss statistics?

R: No, only to the extent that Harold might give a lecture.

H: He gave a lecture?!

R: Well, he gave Winsome the lecture on correlation as I mentioned. They once came to our house and visited together with Gertrude. Harold stood up and started to give a lecture – sad because obviously it was a dementia situation but he was among friends so we all just sat quietly. Sue tried to stop him. But it was an understanding group of friends and not embarrassing.

T: So let's talk about the rest of the people.

H: Was it pleasant to be there when you were a young, new faculty?

R: Yes - I was really in familiar territory in which research was the dominant occupation for all faculty

H: What kind of problems, what kind of research did you do when you started at the department at UNC?

R: Well, that was when I was getting into crossing problems and extremes and similar things.

T: You had a paper with [Walter] Smith.

R: That was sort of incidental, because that's one thing that I learned early, to be one's self. Going into that kind of environment it was sort of expected of me that I would take up renewals and queues and Markov chains. It was very interesting that Cramér had allergies about Markov properties, he told me. He never had any dealings with them and I always felt the same way. So I got into stationarity then, and that's where I wanted to be.

H: Was this from the projects you were doing or was it just taste?

R: It was largely from the problems I had been involved with already but also mathematical taste. I did work a little bit on renewals with Wally [Smith]. We wrote about the renewal function for the Weibull distribution together (Smith and Leadbetter 1963) and then I wrote another couple of papers myself on linear approximation of the renewal function (Leadbetter 1964) and on series expansions for monents of the renewal function (Leadbetter 1963). That was just a one shot sort of thing. Wally was just absolutely brilliant with all those kinds of things. He guided endless students and he would always add a new twist to a queuing problem for them to do.

T: I was one of them. Do you remember that?

R: Yes, I do when you mention it. I'd forgotten totally.

H: So you worked from that time with stationary processes and with extremes and upcrossings of stationary processes. Was the situation then that S. O. Rice and Mark Kac had the main results? Did you read those papers?

R: Kac was more of a background sort of thing, on zeros of random polynomials.

H: So it was Rice.

R: I went to visit Rice at Bell Labs and he was very, very nice to me, which impressed me, a young person coming in. He was dubbed the world's humblest man and had this real humility. When I went to visit with Rice it was a big moment for me. I remember he had this 3-dimensional piling system for all the stuff on his desk as some people do, but he was just totally helpful. David Slepian, who just died recently, incidentally, was one of Rice's contemporaries, and was also very friendly and helpful and good. It reminded me, I had also written to Doob, sort of brashly as a young man, when Per Martin-Löf said to me there would be no need for Doob's "Separability", if what was called the Kakutani Extension could be shown jointly measurable with T and a man named [Mark Edward] Mahowald produced what was supposed to be a proof of that. So I wrote to Doob asking, what's the future of separability now that this has been proved, which was a bit brash at the time. But I am sure I put it less impolitely than that. He wrote back straight away saying, "Yes I believe what Mahowald said but we still should use separability." I was impressed that Doob would write back to somebody totally unknown, just young. I never knew him very well. Oddly Edward Nelson of Princeton found an error in Mahowald's proof so separability stayed secure. My other main encounter with Doob was at that Gothenburg meeting when Cramér, Doob and I gave talks on stochastic processes. Cramér's last meeting really, I think it was, and I was put in that same session. Peter Jagers I guess did that. I'm rarely nervous giving papers, but this did make me nervous! Justifiably so, I really couldn't compete with the polish and grandeur of these two world renowned pioneers. Cramér speaking at close to 90 years of age without notes!

H: Longuet-Higgins had done his work before you started. Did you know of it?

R: I knew of it when I was with the New Zealand Navy. I didn't study it very closely then. It was certainly very much relevant to what I was doing later with Igor [Rychlik] for the US Navy.

5 UNC continued

T: Let me show you something. I want to show you I paid attention to what you told me. In our last conversation before I left North Carolina when I graduated in 1984, you told me that "On three basic results on the theory of stationary point processes" (Leadbetter 1968) was your favorite piece of work.

R: I certainly was happy doing that because people, like Khintchine for one, had used function equations to describe the fundamental properties of point process intensities. I had found work that Belayev had done, which was approximating by counting, and which seemed much more natural. I was very pleased at the time to have a simple connective proof for 3 basic results at once, not dependent on anything other than the simplest counting procedures. But I did it and sent it to the *Proceedings of the American Mathematical Society*. They sent it back and the referee said all difficulties have been swept under the rug, so we can't accept this. The reason was it was so simple! So I wrote back and explained this. I got a nice response from the referee saying, I wasn't really thinking very clearly when I wrote that report. So they published it, in '68. That was a nice experience. When it was done right, it was just so simple. You just couldn't believe it was right, or the referee couldn't.

H: So you did point processes at the same time as you did crossings? They tie together. I suppose your use of Olav Kallenberg's work was later?

R: Yes, I thought of upcrossings as representing point processes. In the early 70s perhaps I remember Peter Jagers telling me that he was happily working away digging in his little garden, of point processes, when all of a sudden this mighty Kallenberg machine came by and dug it up all in one swoop. The way Peter told it, but I'm sure he had a bit left. Anyway, that was when Olav started his mighty work.

H: Okay so this was point processes and upcrossings from '68 and it was continuous time, mainly which was interesting?

R: Oh yes, it was continuous time for those, leading to the Poisson properties. I didn't worry much about Poisson properties for discrete time, first of all, and the reason I say that is I didn't even think about the need for time scaling and to talk about Poisson properties and discrete time. I was just thinking about continuous time.

H: Is some of this in the book with Cramér?

R: I'd have to check. But certainly we were interested in instances in which upcrossings were important. Supposedly we were solving the problem of a guidance system going out of control when navigation errors reached levels causing failure.

H: And it was typically a differentiable stationary normal process?

R: Yes, exactly. Then, around '69 Simeon Berman and Jim Pickands got their results on non-differentiable Gaussian processes.

T: I couldn't find many papers with you and Cramér. This one I found was in '65, "Moments of normal crossings" (Cramér and Leadbetter 1965).

R: There was one other with Serfling also, in 1971 (Cramér et al. 1971) but our main publication was the book.

H: So Simeon started work around that time, or he had been working with extremes earlier too?

R: I don't know when Simeon started, perhaps earlier. He was Gumbel's student, I believe.

H: Did you have contact with Simeon at the time when you wrote about upcrossings?

R: I think the first time I had contact with Simeon was at an IMS meeting at Amherst, University of Massachusetts. I don't remember the date. He had all his 6 children there. A bit like somebody we all know, Holger, who takes children and adults too to meetings.

H: Simeon didn't really do much joint work did he? Did you ever have any idea of working with him?

R: No, not really. Simeon would write by himself. He didn't even write with Jim Pickands, and Jim was his student. We made some (seminar) visits back and forth but never really got into each other's turf. He was a real scholar (as you know) and the ultimate authority on Gaussian Processes I think. A good friend too.

H: I somehow got the feeling that Simeon regretted that it was Jim who proved the results about the maxima of non-differentiable Gaussian processes and not Simeon. Simeon did it in the Simeon way a couple of years after Jim Pickands' work.

R: My impression was that Simeon, as Jim's advisor would have suggested the topic. So why they didn't publish it jointly, I don't know. It's a bit like Robbins and Monroe, do you remember that famous paper [A stochastic approximation method]? Apparently Monroe was a student of [Herbert] Robbins at North Carolina. Robbins had Monroe working on this problem, and if I understand it correctly, Monroe hadn't made enough progress and Herb went ahead and solved it himself and sent it in for publication. Everybody in the department got up in arms when they heard about this and persuaded Herb that this was a bad idea. So he relented and made it the Robbins - Monroe paper. I don't think the Simeon and Jim thing was anything like that but it may have had a little similarity. Simeon would have helped Jim in getting the result and publishing but possibly wanted to see it done in what he thought was completely the best way. A guess.

T: Maybe we can still talk about UNC, perhaps mention a few memorable events or other memorable people besides Hotelling, during your career?

R: Of all the faculty members at the time, Hoeffding was the one I related to most in Chapel Hill. I wouldn't say he was characterized by extreme humility but he had no trace of any arrogance at all. He knew that what he had done with U-statistics was important. Even after he retired he would read the journals mainly to see if there were any references to U-statistics. He was a very interesting character with his background of having a Russian mother and a Danish father. George Nicholson, the UNC Statistics chairman for many years, used to call him a very saintly person because he looked after his mother, who had Alzheimer's, for very many years until her death. His parents were estranged, but the father turned up for the funeral. I met him at that time and it was kind of a strange thing. Hoeffding was a real pioneer, as others in the department were too. Bose certainly was. Bose recognized that fact quite clearly. He used to invite us around once a year to have dinner and he would sit there through dinner and then he would go take a nap while his wife entertained us. She would cook these little round balls, about the size of golf balls. They were sort of perfumes, an Indian delicacy. I could not stomach them at all. I used to smuggle them into my pocket and dispose of them later! She was very protective of Bose. He was obviously very, very good. And we got to know him of course, and Roy, whom I think was amazing. He was a multivariate analyst - perhaps a bit in Bose's shadow but I think he was great. He had two courses in multivariate analysis The first one was MANOVA generalizing the methods of analysis of variance to multivariate cases, using what he called the union intersection principle, which was to take univariate tests and make them work simultaneously over all variables. He used to describe this in a very, very lucid and intuitive way. It was a wonderful introduction to multivariate analysis. But then if one went to his second course, it was all the mathematics and that was horrendous. I never really did get to know Herb Robbins well. I didn't intersect with him at North Carolina at all but I met him a few times later. I remember going to Columbia to give a talk and when I got there I found out it was an interview trip, and Herb Robbins and Ted Anderson were there, so they started to talk jobs to me. Herb went to sleep during my seminar. He was regarded as being a very fierce criticizer of talks. At the end he asked me a question, and I said, "Well Herb, I covered that while you were sleeping." After that we had this very cordial relationship because he respected somebody who wasn't afraid. I'm sort of reminded of a student we once had who was having his preliminary thesis defense and the examiner said to him, "Is that obvious?" Tai is laughing, he recognizes he was the student. "Yes, I think it is" he responded after thought, which dissolved [Gopinath] Kallianpur who asked the question.

H: I was there too, I remember, you and I and Gopi all thought he was wrong. In fact what Tai had written seemed obviously wrong, and then you saw a small clause hidden below which made it all correct.

T: I don't remember a thing.

H: So, then you switched interest to discrete time, I guess. How come?

R: Yes, that was much the same time I think. That must have been simultaneous with when Cramér was doing continuous normal cases. He really proved a theorem about a maximum of normal processes and I think I was looking into discrete time, trying to find what happened to the maxima of stationary sequences, at the same time. That was really based on what Bob Loynes had done in a more sketchy way.

H: Was this before the Berman "$r_n \log n$" theorem? Did you work with normal sequences specifically, or just general sequences?

R: I think I only looked at normal sequences as a special case, because my interest was to try to do things whatever the distribution may be, as cleanly as possible without fussing about particular conditions, which was easier.

H: It's not clear to me it's easier. Your '74 D and D' paper (Leadbetter 1974) a bit resembles the mixing conditions from central limit theory? Was that an inspiration, or how come you developed the D and D' conditions?

R: I don't recall looking too much at central limit theory. Somehow I became aware of the big block, small block thing that Bernstein had used there. I don't think it was from looking at central limit theory, I believe it was motivated by Bob Loynes' 1965 paper (and perhaps Geof Watson's use of M-dependence in the 50s suggesting that weaker dependence conditions might work).

T: Yes, Loynes had a strong mixing condition.

R: I was trying to figure out the minimum one needed to assume for this. So scary the amount you guys know about me! What you can hold against me!

H: Important things to know. The extremal index paper (Leadbetter 1983) was about the same time or little bit earlier?

R: The extremal index was certainly foreshadowed by Bob Loynes.

T: I think it was '83.

R: My paper? Yes you're right Tai - significantly after Loynes' 65 paper.

6 The center for stochastic processes

H: Okay, we are now up to about 1975. The Center for Stochastic Processes, when did you start that?

R: It could have been in the beginning of the 80s. What happened, Kallianpur was certainly there, but a former student from our department was the director of probability and statistics in the Air Force and Stamatis [Cambanis] had a contract with them. Stamatis was extremely careful in administrating contracts. He was careful about everything he did. The Air Force was impressed with Stamatis, his production and the way he kept reporting beautifully, all i's were dotted and t's crossed. Of course, Stamatis had more than 24 hours in his Greek day! So his Air Force program officer suggested to him that since he and Kallianpur and I were all working on stochastic processes of one kind or another, we might think about joining forces and just seek one grant from DOD, Department of Defense. He was planning to fund part himself, so we proposed that - I had Navy funding and Gopi had funding from the Army. So all these were combined and that's what started the Center. This enabled a much wider effort with many collaborating visitors. That must have been the early eighties

Stochastic center visitors and a host, around 1985. From left to right: Laurens de Haan, Stamatis Cambanis, Mohsem Pourahmadi, Daryl Daley, Richard Davis, Jim Bucklew

T: Early '80s because my thesis research started with reading a Tech Report Ross wrote with yellow cover. I recall it said '82 on the cover. So the Center was in existence in '82. I also heard complaints from other professors, whom I won't name, that you guys because of this grant were not teaching enough courses. We had Kallianpur, Cambanis, and you and we hardly had any advanced probability courses. We had probability and measure theory but nothing beyond that. So people complained about it.

R: Oh, that's right, though you may have heard any complaints more overtly than I did. Seems like any successful project must attract some envy, which we tried to avoid - it was accepted practice that all faculty teach an undergraduate course and we felt responsibility to do that. So to help with our Center administration needs we paid a lot of money to the university to buy out of a graduate course, which, if it had an effect, was on the probability program. I had only the one course a year reduction in teaching load which I paid for. It was a bit strange since if I correctly identify the interests of the complainers, they were not at all in promoting probability theory. But I think most colleagues were fully supportive and indeed proud of the addition to the department's effort, and the visibility it engendered in the international statistical community.

H: But the Center for Stochastic Processes, you bought out a little of your teaching and got part of your summer salary money, and then the rest was for visitors? Did you typically have 5 to 10 visitors?

R: Not 10, but quite a few because they occupied quite a lot of offices at University Square.

H: That was a fabulous operation I thought. A lot of people came by.

R: Oh yes, it was fantastic and it had all kinds of repercussions. Winsome went to yard sales, bought a lot of stuff for the apartment that we rented for visitors. It was a great time. We never intended it to be a permanent institution, I remember Stamatis saying so specifically. But of course he died prematurely, and then Gopi retired, and I kept it going for a few years with what's called NAVSEA, special money for special projects from the Navy. We had just a few more visitors, Igor [Rychlik] for one, until eventually it petered out.

H: Was that sort in the late 90s?

7 Ship design

R: Yes, it was kept going until then. In all my time I always had a grant in probability and statistics with the Office of Naval Research (following Geof Watson's early grant that I worked on). But they went out of the probability and statistics business for a while. They instituted something they called Command and Control. It was sort of computer programs for intelligence gathering. That is when I moved to NAVSEA,

Ross after receiving honorary doctorate in Lund 1991, under painting of Harald Cramér.

that had these more applied problems about damage to ships that were smashing into the waves, what they called slamming. Igor [Rychlik] worked a bit on that.

H: You had lots of fun with that I suppose? I guess that the combination of physics and stochastics was something you enjoyed a lot?

R: Oh yes, simple physical models and sort of empirical. There was a guru called Ochi from Florida who's into ship stresses. He had this simple physical law about stress when a ship would hit the water. So we just assumed that physical law and built a stochastic framework around it. Igor worked some on it and other people too. I had a student Gregory Spaniolo, who did his thesis on it. We worked with a wonderful engineer from the Navy who really was first rate, but he got cancer and died, unfortunately. We would send him our work and wait with baited breath for his comments because they were always very searching. Because of that the Navy took particularly Spaniolo's thesis to heart a bit in their Naval Architecture division, in design of new hulls. At that time I got involved with stability and capsize with the Navy people, and then they sort of went out of that for a while, but the Coast Guard got interested in it instead, so that was when Igor and I started to work more together. We didn't work through the University, they would just feed us these 20K grants every year or so. Absolutely no money really, but we had a good time with them.

H: Do you continue to do that still? Do you still work with the Coast Guard?

R: Well, you know Igor has a lot of commitments. And I am very dependent on him well we are dependent on each other. We are very much delinquent. They want us to do now a study of the weather, what are the effects of weather on sea states and on the stability of ships. We sort of paused. Igor tried to get into this in conjunction with some people at Chalmers who work with their tank. But he didn't get much encouragement from them, his friends there, and so that sort of lapsed. The situation now is that the coastguard folk are waiting for us to show some signs that we're awake.

T: It's been 50 years since you started doing research, early 60s to now. What keeps you going?

R: What keeps me going? Well you know, I'd better be careful Tai. What would I do? What's the alternative? Winsome would say "we could go travelling", but we do go travelling. When I didn't have a parking permit one time because of construction on the campus, I used to do some consulting for the EPA a couple of mornings a week. I would go home and work in the afternoon and she very quickly said to me "I sure wish you would go to the office." I don't know, I guess it's just inertia, keeping doing just what I am used to doing. I don't plan ahead of time and if I couldn't function then I would certainly retire quickly, but I've been able to function. I used to jog every day, a lot, but I got this condition called Atrial Fibrillation that is kind of a short circuit in the heart. It is controlled with medication but it doesn't let you be quite as active as you used to be. So I have to reduce the amount of exercise.

T: It's hard to tell.

R: Well, thank you. It's hard to tell when I'm sitting here, but if I ran around the block with you, you would notice it.

T: Oh, you don't know, you run faster than me, I'm sure. I don't run anymore.

8 EPA

H: Tell us about EPA. How did you get involved? What has happened? What did you contribute?

R: Again, I'm not sure when I started, perhaps it was about 35 years ago. A young colleague, Sandy Smith had been out there and worked for a year as a statistician and grants officer. So I followed him. I went for a year on a loan and they paid my salary.

H: Why did you want to do that?

R: Well he persuaded me and it seemed like a good contact to make. They were new in the area and also I had my choice of research problems that I could do, and no teaching for the year.

T: But then over time, you genuinely believed that one should do something like that, not just all academic research. I think this was one advice you gave me, which I didn't take.

R: I hope I didn't advise you too strongly on that because I really do believe people should do what suits them best and you've shown that. I think you've lost some credibility by taking on administration – I'm kidding you! You've been very successful. It's like our daughter Karen, who does a lot of administration at Clemson and I tell her she's lost her credibility. But if people are good at that then they should do it. It's not people who think they are good at it, want the power, who should be doing it.

T: You have been working at EPA, up until now throughout the years, correct?

R: Yes though not as a major time investment, through a small contract to provide statistical consulting style help for them from myself and other colleagues. First I worked for that year mainly doing projects. Mostly it's been with air pollution and various control systems that they been developing, for power generation and this sort of thing. But I also had a peripheral hand in some of the regulation problems, particularly ozone, advising on the setting of criteria there, doing something a little more theoretical. The only problem with that was that even though they were very receptive to it they had too many political constraints and legal fights to push their ideas through as one would want to do, in a clean way. Their first ozone criterion was purely one based on counting exceedances and that was fine. But then when they revised that, industry got into it, the Ford Motor Company and other places. They started to say, you can't do this, you can't do that and there were these big arguments. But I enjoyed figuring out what the math should be. After that one year there, I took a contract with them, a very small personal contract for something like 30K a year, to provide special statistical services. But I always had other faculty (or the occasional student) working with me. The first collaborator was Norman Johnson. I remember him doing projects on wood stoves, telling them fractional factorials they should use. And my role was to translate into language they could understand because he would just write it as a professional experimental designer. More recently Len Stefanski and I have been working on this and he now has actually been doing most of that work. I think it comes up for renewal each 5 years and next time I'll most likely turn it over to him if he's interested because they have become more and more restrictive, I can't any more choose my research topics which I could before.

H: Okay so you could choose somewhat freely among the things they were interested in?

R: I could choose anything. If I thought some work could be environmentally useful, I could essentially do it. But they got more and more restrictive and now we have to have written orders to give statistical help to an engineer for specific purposes. The engineer has to write a justification why he wants statistical help, but of course they still come and want free advice. We can tell them, look we can't be paid for this unless you get the approval. But Len Stefanski is the world's best applied statistician and I think partly because he was UNC trained in theoretical and applied statistics! They just absolutely worship him, the engineers at EPA. So he is doing most of it because it is mostly statistical modeling. I do a little bit still when they have simple things they want to ask like comparisons, or counting data or something they want to ask about.

H: To what extent has your work been helpful for the EPA, has it changed things?

R: Certainly the present model is extremely helpful to them. They have to get quality requirements approved and it's not so obvious that doing that is a great thing, but they have data they need to have analyzed properly and they are liable to do any old wild thing with it, but particularly Len can look at it and tell them what to do. Often do it for them, and do it the way it should be done. Particularly things like detection limit problems and things that require more sophistication.

H: Detection of what?

R: If they want to detect very small quantities of pollutants, possibly carcinogens that might be present in very small quantities or something that might still be present after a furnace destruction. But they can't measure to the limits, some of these below these limits, what do you do statistically about that? It's still hazardous to health and Len is just very good at that.

H: Your earlier projects with the ozone, to what extent did you survive political hassles with the industry?

R: It was very much a compromise. I was not involved directly but advised some who were.

H: But did it have an impact?

R: Yes, semi-scientifically but the way the criterion is worded they do all kinds of convoluted things like taking the average of the top ten observations in 6 months, and combine with some other counting procedure giving something one can't directly analyze, but of course they can see how it works in practice.

H: Did you propose any of that or they invented it themselves and then you tell them to modify and justify and change the limits?

R: I advised them on purely scientific procedures, like modifications of the way they counted. Things that I could analyze, primarily, they would say can we have this limit or that limit or what. But then they had to compromise with guys from industry who could threaten lawsuits. So it became a hybrid of stuff. It really makes a lot of sense to me now, but I'm not working on it any longer. That's part of the regulatory branch. To me it was a project which was fun to be involved in even though quite tangentially.

H: Did you ever use extreme value theory in that kind of work?

R: A bit - mainly things like testing for exceedance rates of high (violation) levels, under simple assumptions. But no fitting of extreme value distributions.

9 Recent years

T: Changing topics? My colleague Susan Murphy, our friend Susan Murphy, mentioned she wanted to see your measure theory notes published. I believe she talked to some people, certainly. Has there been progress?

R: Yes, it's finished. I developed the notes over this entire 50 year period and Stamatis worked a lot on them too. Then the IMS, largely due to Susan's suggestions, planned to publish them as one of their lecture note series books. Publishers over the years had said they would be interested in doing it and I had always said no. Meanwhile everybody else published books on measure and probability. So everyone has their own nowadays! But the IMS framework seemed more appealing to me. So I said ok, we'll do it. A major problem was the patchwork sort of manuscript from endless revisions over the years using varied typing technologies (some TeX) put together to get a "cut and paste" pdf file. The IMS solved this by magic it seemed by having their amazing (Lithuanian) "VTeX" group, which produces their journals, somehow work with us to resolve all the differences and get a coherent LaTex manuscript. It was then that the IMS outsourced their series to Cambridge [University] Press. So it's going to come out as a CUP volume. David Cox is the person in charge of that now. So Cambridge Press has it and we just got page proofs of it. But actually to get it in final shape proved to be a big task, even as late as that, so Vladas Pipiras, you know Vladas, got interested in it, and had used the notes as a reference. He then did so much work that I persuaded him to also be a coauthor. He did a big lot of work, he is a perfectionist he even worried about the lengths of hyphens! Things I wouldn't even have any patience with. But more importantly, he went over everything thoroughly and had suggestions on things that might be done. He was upset with something I had done oh I know what it was, I had said that singular distributions are of no real interest in statistics. He of course works in chaos and areas where he finds singularity and so that had to be changed.

H: Did you change it?

R: Oh yes, of course. I acted as if I knew about it all the time. Not to him, but we just changed it. I mean these things are pretty obscure to me even now and what he's doing, clearly was important and a situation in which one did have singularity. So he went through it with a very fine tooth comb, and now we expect publication in October [2013 – the book has now appeared, see Cambanis et al. (2014)].

H: Is it measure theory and probability theory? What kind of probability then?

R: Very standard beginning probability, not standard as Olav has in his book, classical independence and characteristic functions and convergence theorems.

T: Conditional expectations?

R: Yes, conditionals, and martingales and now a chapter that I added to it on random measures, sort of a simple "journey-of-Olav-land" chapter.

H: Without the metric spaces?

R: Without the topology, yes. Because the whole tenor of the volume is virtually non topological.

H: That's what I thought would set it apart from other measure theory books.

R: Yes a little bit, certainly from Billingsley, and even now the students that we get don't know any topology.

H: Okay, so it saves effort in the teaching.

R: Yes, we don't teach topology, students can do it in Math if they want to, or later if they need it in their research. It's always been an ethic to me, starting with reading Halmos' original book (the New Testament of integration theory at that time. About the back third of his book is topological but in the main measure and probability treatment, he makes a big point that is clear of topology. This was also the ethic of the renowned mathematician BJ Pettis some of whose classes I was privileged to attend, and who always wanted to make clear whether arguments in proofs were topological, measure theoretic, or algebraic.

H: That's always been your viewpoint too, on measure theory and for point processes too?

R: Yes but that's where I got it, from Halmos.

T: What you need is straightforward reasoning in developing measure theory. You build from scratch.

R: Yes, exactly! So anyway, Susan will get a copy in October.

H: What are the drawbacks and advantages of your way of doing it in a non-topological way?

R: Well the advantage for us is just what I said, that students don't know any topology, and we don't have to teach it to them. It's clean and it doesn't require any other stuff. Of course we do apply it to Lebesgue measure and so on. It's a little bit like compactness of closed sets, of closed intervals, kind of a Heine-Borel theorem, that sort of thing. There is that amount of topology in particular examples. The interplay between measure and topology (and algebra) is very beautiful but we find it most conveniently taught in "advanced" courses taken by students specializing in the probability area after a basic probability course for all (Statistics) students.

H: You do it like that also because you like it, I suppose?

R: It's a matter of taste, yes. I say in the preface that people have their own preferences and in my view there's no uniformly best way of doing it. For me (as for BJ Pettis whom I mentioned) it clarifies the machinery used in the different disciplines. But a Danish colleague regards measure theory as "an unfortunate historical accident", feeling that integrals are more naturally regarded as linear functionals, a not too uncommon view, and thus a part of functional analysis inextricably involving topology. I believe probability (rather than expectations) is the lifeblood of at least my statistical world!

T: I'm certainly very comfortable with it, growing up with it. When people tell me measure theory is hard, I don't know what they are talking about.

R: It certainly makes teaching it easier because if I had to get into topological theorems as I do when I teach weak convergence at the more advanced level then I have to teach them the topological concepts. That's nicely set out in Billingsley's classic weak convergence book and good to teach in that more advanced context.

T: Another advice you gave me was that when you write a thesis you have to make sure every detail is there, you have to really be careful about details because next time, you worry so much about details, is when you write a book.

R: I'm not sure. I can't tell if you are misquoting me or not Tai!

T: I'm just making this up!!

R: No, you're right - I do encourage students to be totally detailed in their theses to demonstrate complete command of their subject but more concisely in writing later papers. If books are expository they can need more detail I think. I perhaps err in the direction of more detail.

H: I have more questions here. Which of your papers has been the most fun to write?

R: Tai just told me that!

H: Well we would like to hear your answer too.

R: Most papers I've enjoyed writing, but not all. Some are a bit tedious. I enjoyed the early papers on function estimation quite a lot, the research was new and fresh. I enjoyed particularly the short paper in Statistics and Probability Letters on peaks over threshold (Leadbetter 1991), because it seemed to me what was written about peaks over threshold was very ad hoc and not well justified. What appealed to me

From left to right: Ross, Georg Lindgren and Holger, after conferral of honorary doctorate to Ross in Lisbon, 2013

was the ability to take Olav's framework, and put it there and deal with stationarity as well as iid cases. That seemed to me to be quite attractive. I also enjoyed our expository paper (Leadbetter and Rootzén 1988) from the Annals which I think was well received by quite a number of experts.

H: Mainly Ross' doing as usual.

T: It's got a lot of citations.

H: But you like review work too, and you like writing books too?

R: Yes, I think as long as you got some Swedish co-authors!

H: You missed out on that on the last one.

R: Yes but Greeks and Lithuanians can make splendid substitutes I find! As long as you've got some good co-authors, better than oneself, writing books is fun. It also helps ensure authority in the writing and completeness of relevant coverage. I know I would never have got a book written like ours on Extremes (Leadbetter et al. 1983) doing it by myself and of course, the care that the Swedes have built into them somehow gives a lot of confidence to the authors.

H: It doesn't match the care that New Zealanders have built into them at all, I don't think. You were very careful about getting rid of extraneous detail and getting to the heart of the problem. That's extremely useful for readers.

R: Not necessarily but I will accept that as a compliment. That is one of the things that I can try to do. I'm not so imaginative about new things. But I was taught you should talk about fields rather than the real line in algebra because you don't have many assumptions to worry about and have the Lebesgue set and all kinds of complications. I was taught in basic mathematics that group theory was great because if you just take the assumptions that you need and it always pleased me when I could do that. I think one capitalizes on the abilities one has and the thing I can do fairly well, I believe, is to write decent English. You know Olav Kallenberg, he invited me to write with him when he wrote his point process book. He said "I know the mathematics but you write so well!" I just said absolutely not, it's your work. I think at least in the 2nd or 3rd edition he has come out of that very well. The 1st edition was on the cryptic side, though a bible on the subject. And his probability book is exceptionally well written and organized. Even in high school, the English teacher would say to me, Leadbetter, you're a factual kind of fellow. I took a lot of Latin which helped with grammar. I'm not ashamed in believing that I can write satisfactorily but I also respect things that I can't do so well. I couldn't think like Ted Hannan or Olav in terms of abstract mathematics. Again, I think it's important to have good perspective of what you can do and what you're not so good at. I think we complemented each other nicely in the writing of our Extremes book.

H: Yes maybe, but I think your parts are the most useful ones because they are the basic ones and that's what the book lives on. Again, of your papers, which one was the most fun to write?

R: I would say papers are basically fun to get into print, although they can all be a bit tedious to finish much of the time. But I just liked doing that Letters one (Peaks over thresholds), and I did like the early papers. I enjoyed doing those quite a bit. I wasn't so fond of the one I did with Patrik [Albin and Leadbetter (1999)] because I didn't do it. It was great but it was Patrik's paper!

H: Yes, I had forgotten about that! The other question is which of your papers you think are the most useful?

R: I would think without almost any doubt, the ones on clustering.

H: You mean the extremal index?

R: Yes, the extremal index, mainly the one that introduced clustering (Leadbetter et al. 1989). It was about climate, it was very applied in which I was counting cluster sizes. It's one of those things that gets quoted a little bit.

H: It's been extremely influential I think, and of course it would have been much highly cited if the book hadn't been there, because the book gets all the citations. I suppose the D and D' conditions have also been very much used?

R: Yes, I guess so, D' more than it deserves, probably. Olav Kallenberg said to me you can't be considered a real mathematician unless you have a theorem named for you. I said I only have a condition so I am "conditionally" accepted perhaps. That reminds me to hop right back to Cramér who has this famous condition C, for characteristic functions. I once asked him if that stood for Cramér and he was indignant, absolutely not, there used to be A, B and C, and A and B disappeared. I thought to myself, that's a likely story.

T: That's all the questions we gave you, but would you care to comment on the field today, the field of extreme values?

R: I'm just not a good one in prophecy. It has just expanded so much with all the activity on heavy tails and all kinds of things. Even my knowledge of history is mainly limited to my own personal contacts. I am impressed by the number of young faces I see at this meeting, younger faces. Clearly we have a community here. And it's not just the meetings, it's the journal as well.

H: It's good to see that lot of new people come in. Of course my optimistic interpretation is that it's because we work with important and useful problems.

R: And even these "big-data dwellers". They are likely to quote type-one extreme value distribution, without knowing too much. After all isn't big data somehow in our domain?

T: I recently heard a talk by Rob Tibshirani, where he discussed hypothesis testing in LASSO variable selection in high-dimensional problems, and extreme value theory was a part of it. It's hard to explain in a few words but somehow the test statistics are the extreme order statistics in a large sample.

H: Somehow, extreme value statistics was not so popular in the US after David Aldous' attacks on extreme value theory. One has a feeling they had a far less of an effect outside the US, but I think the effect on the US is changing fairly much now, which is interesting.

T: I asked Jeff Teugels that question, why is it that there is more interest in extremes in Europe than the US. I think he mentioned something about the insurance industry in Europe, that's a part of it.

H: My belief is the US is faddish, so when David Aldous said it was not good to do extreme values, that it was all too simple, he wrote a whole book to prove that it wasn't worth working with, then people were scared away from it. It has changed, now there's much more interest, and much more activity in extremes in the US.

R: I thought that David Aldous' book was interesting though a bit odd from such an accomplished rigorous pure mathematician. Simeon really took him to task sensibly. But he's a splendid mathematician, there's no question about that.

T: Maybe the US has changed, but not in a major way. Of course, nowadays the main interest is high-dimensional analysis and if extremes could become a part of it I think there would be a lot of interest.

R: Like I say, the candidates for our jobs we've had in the last year or two talk about high-dimensional data of one form or another, but they do quote the extremal types theorem, not necessarily explicitly, but it seems to be hard to talk about high-dimensional data without thinking about extremes.

T: To me the most compelling example would be Tibshirani's test statistics, which is really based on extreme values.

H: So how has the field changed, it's a long period you have been inside, not just extreme values but stochastics, how are things different now from 20 years ago and 40 years ago?

R: I don't quite have an answer to that. I see a big lot of interest these days in applications of stochastic calculus to biological problems, much more of an advent of those sorts of applications. I guess even early, stochastic differential equations were worked on, but it seems to me this is even more a part of life science disciplines these days, and of course the papers in the Annals get more and more obscure and less understandable titles, unless you know more of the history of how things have been changing than I keep track of. I just never got into prophecy very much at all. Some people like to do it and are good at it, but I just like to work in my small corner, to solve the puzzles that I am involved with.

H: Is there anything we should have asked you but we haven't? Something that would be interesting to talk about? Not the future, but what's already happened.

R: I've been fortunate meeting a lot of well-known people, although we each have our own selection. I never did meet Kolmogorov, which would have been nice to do, I think, and say "Privet" as I shook his hand. I did meet a student of his once whom Kolmogorov had taken on a cruise in the South Pacific. This guy worked in time series, and he told me about this cruise. "All we did was cruise around measuring temperatures, what a useless occupation, just going around measuring temperatures, and then I would have to analyze these, model and all. Haha, how stupid." I didn't explain to him that this was common practice so that the refraction properties of the ocean would be known and submarines would know how deep they would have to go to escape detection by sonars. It seems that this guy had been cruising around and had not told what the purpose of this was. In a sense he was letting the cat out of the bag. That is my only indirect contact with Kolmogorov. I never did meet [AC] Aitken, but we're very proud of him as a New Zealander. He was before my time. I went to his 100th anniversary celebration that they had in New Zealand. He was an interesting character in that he could never forget anything that he once learned. And one thing he learned was the wrong value of pi to 100s of decimal places. So he had to relearn it. But he used to give performances. He remembered the serial numbers of everybody in his World War I battalion, this sort of thing. But after a while, being unable to forget became a psychiatric burden to him. I very much prized his wonderful little books on statistics and algebra, which were within my range of affordability!

H: It's very strange that these possibilities even exist in the human brain.

R: Yes, isn't it? Of course, there's lots of others. Henry Daniels was an interesting character doing important work in statistics. I remember going to a meeting birthday party for Geof Watson at Princeton and Henry was there. Geof asked me to take care of him on my return plane trip to North Carolina and Henry was coming to visit us. I was standing behind him at the airport. He kept on setting off the security system alarms and they would take away more of his clothes and he was practically naked before he could go through the security without setting these bells off. He was known for his accordion playing. Peter Whittle had a party at his house one time in Cambridge, and Henry was to play his accordion. Peter had arranged for a young girl to accompany him on the piano and they were going to play together. But the piano had been tuned one tone off. So this just didn't work. But Henry was ready to play and not to be denied he got out some music, a record, that had all the parts except the accordion, an "(n-1) one record" he called it. He went ahead and played all evening for the party. I knew David Kendall a little bit, and we had a very cordial relationship. He nominated me for ISI membership I remember. He was certainly an impressive character, who did so many fundamental things. I was quite excited when he came to me to discuss his use of our work on extremes of normal processes to determining measurement units used by the builders of ancient monuments like Stonehenge!

From left to right: Ross, Sylvia Hsing, Winsome Leadbetter and Tailen, at the 2015 EVA in Ann Arbor

R: If I may make a final comment. I have immensely enjoyed these reminiscences with you of contributors to our field whom I've been fortunate to know. Especially great mentors and colleagues who have shared my professional path, been my friends, and helped me make best use of my abilities over the years. High on my list of course are the two of you! I am thoroughly embarrassed but very touched that you would go to this immense amount of trouble to make something of me. Not only in this actual interview but in the huge effort which remains to decipher the recordings, put them in written form, and reduce them to a reasonable size! It's an effort I don't at all deserve, but my sincere thanks to both of you.

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