

# A Conversation with H. A. David

Narayanaswamy Balakrishnan and Haikady N. Nagaraja

*Abstract.* Herbert Aron David was born in Berlin, Germany, on December 19, 1925. He earned a Ph.D. (1953) in statistics from University College London. Since then he has been a Research Officer at CSIRO, Sydney (1953–1955), Senior Lecturer in Statistics at Melbourne University (1955–1957), Professor of Statistics at Virginia Polytechnic Institute (1957–1964), Professor of Biostatistics at the University of North Carolina (1964–1972), Director and Head, Statistical Laboratory and Department of Statistics at Iowa State University (1972–1984) and Distinguished Professor in Liberal Arts and Sciences at Iowa State University (1980–1996). Currently he is Emeritus Distinguished Professor in Liberal Arts and Sciences at Iowa State University. He is a Fellow of the American Statistical Association, the American Association for the Advancement of Science and the Institute of Mathematical Statistics. He is an elected member of the International Statistical Institute. He also has served as Editor of *Biometrics* and as President of the Biometric Society. A list of all his publications until 1996 can be found in *Statistical Theory and Applications. Papers in Honor of Herbert A. David* (1996) 313–326.

This interview was conducted on September 9, 2000 (Saturday) at the Department of Statistics, Iowa State University, Ames.

## CHILDHOOD IN GERMANY

**Nagaraja:** Good morning HA. To begin with, could you please tell us a little bit about your early days?

**David:** Well, I was born in Germany, lived in Düsseldorf in western Germany, on the Rhine River until the age of 13. At that time it was 1939. And I was very lucky indeed to be able to get out with my parents to Australia, not long before war broke out in Europe. My father was a sales representative and he happened to have a half-English boss in Germany who had a sister in Melbourne, Australia, who was married to an attorney and that's how we got our papers to Australia. My schooling began in Germany, which meant I attended elementary school up to age 10 and then started high school (Gymnasium). So I was in the Gymnasium for two and a half years until Kristallnacht, which can

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FIG. 1. A portrait of Herbert A. David.



FIG. 2. H. A. David when he was 13, taken in 1939 in Germany.

be best described as a pogrom foreshadowing worse events that happened later into the Holocaust. But at that stage it became abundantly clear to Jews in Germany that they should try to get out of Germany, if they possibly could. This was in November 1938. The synagogue in Düsseldorf was burnt like most synagogues in Germany, with it the adjacent Jewish school that had been set up in 1935 in response to life becoming increasingly uncomfortable for Jewish children. A makeshift arrangement was made for providing some kind of instruction for Jewish children. So I was part of that for a few months before we left. In the Holocaust I lost my three grandparents who were still living at the beginning of the war, and many uncles and aunts.

**Balakrishnan:** As a student did you face any problem from fellow students when this political turmoil was going on?

**David:** I was relatively lucky. In those days there was so much pressure exerted by the regime that every boy in my Gymnasium class, except for one, was in the Hitler Youth or more precisely in its junior version. Some had enough home background not to be too infected, but many Jewish children found it impossible to continue in regular school. It wasn't just the fellow students, there were also some unpleasant teachers. When I entered the Gymnasium in 1936, a sympathetic principal admitted me, but in 1938 a convinced Nazi was put in his place. On one occasion he came to our classroom to check on a noise going on in the absence of the teacher. He promptly singled me out and smacked me in the face. Such were the times.

## INTO STATISTICS IN AUSTRALIA

**Balakrishnan:** You finally ended up doing mathematics.

**David:** I liked mathematics from the beginning. Perhaps we should talk about the next stage—getting to Australia, by ship in those days. My parents and I came to Sydney, where I had four years in high school. I had had only a few months of English in my final year in the Gymnasium. Like all children and young people who come from other countries, I was anxious to prove myself in a new country. This applies even more when you were suppressed in your home country. I enjoyed mathematics tremendously in my high school years and then subsequently at Sydney University.

**Nagaraja:** Tell us more about your undergraduate experience in Australia. What were the courses you liked? Who were your teachers?

**David:** In 1944, Sydney University (SU) was the only university in Sydney, then a city of two million. In other words, there was absolutely no choice since you didn't go out of your state. In 1946, K. E. Bullen was appointed as Professor of Applied Mathematics. Primarily a geophysicist, he had worked with Harold Jeffreys. So he picked up some probability notions too. But the statistics instruction was given by two people, one of them not in the math department. D. T. Sawkins was highly knowledgeable about statistics and had written some quite nice papers published mainly in obscure journals. So, 1945 was my first exposure to statistics. I found the material interesting and elementary combinatorial problems had already attracted me earlier. There was another man, not well known, who gave a more mathematical course in my fourth year. The books we used are of some interest, perhaps. There was a little book by Aitken, *Statistical Mathematics*, which was very concise, and apart from that there was Yule and Kendall. Then for the more mathematical course we used Uspensky's book, which is mainly probability, with some statistics.

**Balakrishnan:** So was the basis for you to consider going to England for your graduate studies the contacts with the teachers?

**David:** Those courses, yes. I thought that this was a relatively new area that seemed interesting and promising.

## ENGLISH EXPERIENCE

**Nagaraja:** Was England a natural choice at that time?

**David:** Yes. At that time, especially in the Sydney Mathematics Department, one looked very much to England. Mathematicians tended to go to Cambridge. The Professor of Pure Mathematics was a Cambridge man, the Professor of Applied Mathematics was a New Zealander by birth but had studied at Cambridge and most of the faculty in the department had been to Cambridge after graduating from SU. So it was very natural for me to think of England. And for statistics, University College London (UCL) was a natural place. Made famous by Karl Pearson, the department was headed by Egon Pearson. In Australia there were two well-known statisticians by the time I was an undergraduate—E. J. G. Pitman, first of all, and E. A. Cornish of the Cornish–Fisher expansion. So there was some statistical activity, but these two were not in Sydney. There really was not a well-known statistician in the whole of Sydney at that time. So I was grateful for the advice to go abroad. But on the other hand it's clear to me now that actually at that time, 1949, I could have received more intensive training in several departments in this country, including Iowa State University (ISU). It offered a much more elaborate statistics program than UCL, which had a small department with only four faculty members. Apart from Egon Pearson, there were F. N. David, H. O. Hartley and N. L. Johnson. All excellent people, but they had to give all the regular lectures in statistics. They fleshed out the program a bit by having some distinguished people at UCL present 10 lectures on their specialties. So we students were fortunate enough to hear J. B. S. Haldane, C. A. B. Smith, Lionel Penrose and Cyril Burt. The four statisticians gave us a good, down to earth introduction to the subject, with Johnson the outstanding lecturer. And incidentally all but Pearson ended up in the United States eventually.

**Balakrishnan:** When you made the decision to go to England was it something that was readily accepted by your family or were there reservations about the choice you were making?

**David:** My parents had no possible way of judging what was going to be good for me or not, but they certainly supported the idea of going abroad.

**Nagaraja:** Was there scholarship or other financial assistance?

**David:** There were very few scholarships and I didn't go on a scholarship. By that time my parents had been able to establish themselves and could provide some support. Also I was already engaged to be married before leaving Sydney. And then I married Vera in 1950. She found a job working as a biochemist, so that



FIG. 3. H. A. David with Vera in London, taken in 1950 when he was a graduate student at University College.

provided some income. And also the department was quite helpful in finding odd jobs providing some extra money.

**Balakrishnan:** Could you tell us something about fellow students who were there with you in England?

**David:** Well, in my particular year there were seven of us graduate students—one New Zealander, one Ceylonese (Sri Lankan) and the four others, I think, were all Englishmen. D. E. Barton became well known.

**Balakrishnan:** Barton and Dennis?

**David:** Yes, but Barton did a lot in collaboration with F. N. David. He was her student and there is a book, *Combinatorial Chance* (David and Barton, 1962), by the two of them, a very good book. There were several strong students, but some of them finished with Master's degrees. In the year following there was Colin Mallows.

I was very lucky again to be able to work with H. O. Hartley for my Ph.D. dissertation. Another student of Hartley was George Box, who was not around at UCL. He was really already quite well known and just had to get a Ph.D. degree. By then, he had done his

important work on response surfaces. His Ph.D. thesis was not on that subject, but on some multivariate problems. He finished early in 1952 and I late in 1952.

**Balakrishnan:** What was the process of the choice of dissertation topic?

**Nagaraja:** And advisor?

**David:** Of course I had classes from all four instructors. Egon Pearson didn't take any students because as the Department Head and Editor of *Biometrika*, he was fully occupied. But he was interested in my dissertation topic because of his extensive work on the distribution of range. Properties and uses of the range were the main subjects of my Ph.D. thesis. So I was just assigned to Hartley.

**Balakrishnan:** What did you do to choose that particular area?

**David:** Like most Ph.D. students I didn't have a clear idea of a topic. In fact, in my experience in directing Ph.D. students, I don't think any had specific ideas of what they wanted to work on. However, some knew they wanted to work on order statistics. So Hartley gave me one of his papers to look at and cut my teeth on, Hartley (1950a), then in press!

**Nagaraja:** When was your first exposure to computers and computing?

**David:** An interesting question because obviously that's the area in which we have seen the most dramatic changes in our lifetimes. At SU we used slide rules. In London by that time there were some electrical desk calculators available, but they were too expensive for us students to use. The students used the Brunsvigas, which were operated by rotating a handle that turned a central drum. At the end of doing some extensive computing your arm ached. I have a Brunsviga here in my office, as a matter of fact. Actually it was a museum piece already then. So, I was able to take it with me by paying a pound for it when I left England. It is the kind of machine that Karl Pearson used at the beginning of the 20th century (see Figure 4).

The period preceding the advent of the computer is sometimes called the heroic age of computing because it's really quite impressive how elaborate tables were constructed with these primitive methods. There were also some clever ideas about using accounting machines for computing. But I wasn't involved in any of that. Punched cards, however, were coming into use for computing purposes. So that's as far as we got in my graduate student days at UCL. Actually there was quite a bit of computing in my early work. Construction of tables was an important part of work on order statistics, starting with the dissertation. That was all done at

UCL and later on continued in Sydney and Melbourne by various assistants, who, however, had moved up to electrical desk calculators.

**Nagaraja:** At that time, was there any interaction with other statisticians like Fisher and somebody from UCL? We hear about Pearson and Fisher issues.

**David:** Well, of course. By the time I came to UCL in 1949, Fisher had long left and was a professor at Cambridge. But certainly his name got plenty of mentions in our classes. To us students, never a bad word was said about Fisher, but relations certainly were strained. On the other hand, Egon Pearson was a very peaceable man and was quite ready to acknowledge Fisher's outstanding contributions to statistics. Of course, lecturers stressed their own interests. Specifically, Pearson included lectures on the Neyman-Pearson theory and on "my father's system of curves." I had occasion to encounter Fisher at meetings. The Biometric Society had a British regional meeting very close to UCL in 1951. At this particular afternoon meeting, three talks were given. I thought the first speaker talked quite interestingly on capture-recapture methods. It was pretty new to me. But Fisher was in the chair and intoned: "I thought we were going to hear something new from Dr . . . , but everything he said has been said elsewhere and better said elsewhere." So that was my introduction to R. A. Fisher.

I want to tell you one more thing about the UCL years. One day Hartley said, "David, I have got this interesting result about bounds for the expectation of range divided by the standard deviation." And it was a nice bound. But I did a little bit of looking around and the amazing thing was that in the 1947 issue of *Biometrika*, not in some obscure journal, Plackett had obtained this same result. Hartley always published in *Biometrika* at that time, but somehow was not aware of this result. And so Hartley got rather energized by this and in short order produced a much more difficult result by imposing restrictions on the support of the random variable. This then became an *Annals (of Mathematical Statistics)* paper in 1954 by Hartley and David.

## BACK IN AUSTRALIA

**Balakrishnan:** So then you went back to Australia?

**David:** Yes, there was no question of staying in Britain. There were no good jobs really. When I got back to Sydney there were no university vacancies for statisticians in the whole of Australia, but I was able to obtain a job in CSIRO, the Commonwealth Scientific

and Industrial Research Organization, as a consulting statistician. This was in 1953 and I worked under Helen Turner, who was a good consulting statistician for people requiring statistical help in animal experiments, particularly sheep. Sheep, of course, were a big thing in Australia. We were part of the section, later division, of mathematical statistics, directed by E. A. Cornish, who was in Adelaide. There were other CSIRO statisticians around the country including E. J. Williams, Alan James and George McIntyre.

**Balakrishnan:** This is the same McIntyre who wrote the ranked set sampling paper?

**David:** Yes, right.

**Balakrishnan:** In your list of publications you also have, during that period, a publication in the *Australian Journal of Chemistry* and one in *Australian Journal of Biological Sciences*. Did these publications come out of your appointment at the CSIRO?

**David:** That's right. It was useful experience, but I was anxious to get a university position and as soon as I had a chance, I jumped at it.

So I got an invitation in the mail from Maurice Belz, the head of the only university statistics department in Australia at that time. That department at the University of Melbourne (UOM) was created in 1948. The

enclosed newspaper advertisement was accompanied by a letter from Belz suggesting that I might be interested in applying for a position as "Senior Lecturer" in his department. So I applied and got the position—no interview, immediate tenure. Those were the days. G. S. Watson's resignation had opened up the position. It was a tiny department. Belz, another Senior Lecturer, R. T. Leslie, and I gave all the lectures. This was really a good way to learn more statistics, because when you are in a small group, you are required to do all sorts of things that you didn't really learn as a student. So I had to give lectures on multivariate analysis, using C. R. Rao's (1952) book. The number of students was small, but there were some excellent people there. G. A. Watterson was one of the students, and in his fourth year he was the only statistics student. So I gave lectures to one student, but that was quite justified. He did a Masters degree with me on order statistics, then worked with P. A. P. Moran in Canberra for his Ph.D. in genetical applications of probability theory. He did some fine research in this area. The period in Melbourne was good for further education, really. I then, out of the blue, got an invitation in 1957. This was for a full professorship at the Virginia Polytechnic Institute.



FIG. 4. H. A. David with Henry B. Wallace (left) and Iowa State University President Robert Parks, taken in Ames in 1974. This was during the dedication ceremonies of the Henry A. Wallace Room, 319 Snedecor Hall, on October 8, 1974, honoring the occasion of the 50th anniversary of the 1924 seminars conducted by the elder Wallace on rapid machine calculations.

### VIRGINIA DAYS

**Balakrishnan:** Did you apply for this or how did it come about?

**David:** Those were different days. The letter said essentially, “We have this professorship and hope you will be interested.” Well, I was interested, although at that time I thought it was a temporary thing and that I was going to return to Australia after a few years. So we moved to the U.S., to Blacksburg, Virginia. One of the attractions there was the presence of Ralph Bradley. But actually I should say more about my Melbourne years. There were two important visitors during that period to the UOM, both in 1956: Sam Wilks and Maurice Kendall. Sam Wilks, who had worked at UCL for a while with Egon Pearson, was a very nice and friendly man and gave some good talks. Much later I learnt that he had given my name to Boyd Harshbarger, then Head of Statistics at VPI. I should add that at that time there was a shortage in the U.S. of trained statisticians, people who could direct Ph.D. theses and that’s how I came to receive this invitation. The other visitor, Maurice Kendall, was important in another way. He had written a paper in *Biometrics* in 1955 on paired comparisons that I thought was very interesting, and one of his lectures in 1956 was on the method of paired comparisons. At that time I was a keen chess player, so I liked to make the connection between tournaments and paired comparisons. It’s like in taste testing: there are two brands of coffee, say, that you are tasting and that are competing for your preference—just as two chess players facing each other. So that resulted in my interest in the method of paired comparisons and my first paper in this area (David, 1959) was really continuing an essentially combinatorial nonparametric way of handling the subject. This contrasted with the parametric approach of Ralph Bradley at VPI. I had started on this work in Melbourne and continued at VPI. Ralph and I never collaborated, but remained good friends. His work became well known as the Bradley–Terry model based on a 1952 *Biometrika* paper. Then some years later when I submitted more of this work to *Biometrika*, of which Kendall was an Associate Editor, he suggested that I write a Griffin monograph (David, 1963a); he was Editor of the series at that time. So, although I could have received more intensive training in the U.S., coming to London I had the opportunity of meeting some very important people. Moreover, *Biometrika* was published at UCL at that time. So my early work mostly appeared in *Biometrika*.

At VPI, I soon was able to work with some very good students. These included Donald F. Morrison and Jean

Gibbons, both of whom became well known for their textbooks on multivariate analysis and nonparametric inference, respectively. It’s interesting to note that in both these cases, and I think they were the only cases, most of the work on their dissertations was done by correspondence.

**Balakrishnan:** How did that happen?

**David:** They got their Ph.D.s at VPI but were resident at VPI for only short periods. Donald Morrison had been working on multivariate analysis at NIH, so my influence on him wasn’t great because his Ph.D. thesis was on order statistics and reliability. Jean Gibbons was married and so she was with her husband. She did some courses at Columbia. Another successful student was Charles Quesenberry and there were more.

**Balakrishnan:** What was the attractive feature for so many graduate students to come to VPI at that time?

**David:** Well, there weren’t all that many well established statistics departments at that time, the late 1950s, and VPI was an early department, established in 1948. There were eight faculty members, all young except for Harshbarger, so for that number of faculty it was a busy time for us.

**Nagaraja:** And you had several students working with you at the same time?

**David:** Yes, I directed 11 students in my seven years at VPI. I have subsequently had much less intensive supervision of Ph.D. students.

**Balakrishnan:** One of my colleagues, Charlie Dunnett, was telling me that when you were still in Melbourne, there was some sort of a committee on statistical computing, with Don Owen as chair, of which you were a member.

**David:** Well, yes. I had almost forgotten about that committee. As I mentioned, in those days before high speed computing I was quite involved in computations. Several of my early papers had tables. It was still while I was in Melbourne that I was approached, actually by Julius Lieblein, about a multiauthored book on order statistics. Would I contribute to this? Later he had to drop this project, which was taken over by Sarhan and Greenberg and eventually resulted in a 1962 book. I contributed several chapters. That was really the first general book on order statistics.

### CHAPEL HILL YEARS

**Nagaraja:** Is that what led you to Chapel Hill?

**David:** Blacksburg was a good place to work, but then I had an invitation from B. G. Greenberg. Chapel Hill seemed a very attractive place to be. It had some outstanding statisticians. However, Greenberg was the Head of the Biostatistics Department and it was the

Statistics Department that had the really eminent people in it, like R. C. Bose and Wassily Hoeffding. Walter Smith and N. L. Johnson were also there. My working conditions were very good. I soon got involved as Editor of *Biometrics*. The position allowed the time to take on this activity. My teaching was not heavy and my role was to provide theoretical support in the Biostatistics Department. Some of the time I had a joint appointment with the Statistics Department and did some teaching in both departments. The number of students I directed in my eight years there was quite small. The Consolidated University of North Carolina had an interesting arrangement, which allowed faculty to direct students in any part of the university system. So that way I came to direct P. C. Joshi, who was in the Statistics Department. I directed only one student who was actually in the Biostatistics Department and two others who were in the North Carolina State Statistics Department. One of them was Melvin Moeschberger.

I had the pleasure of attending some courses in the Statistics Department, one on nonparametric statistics by Hoeffding, and two from Bose on the design of experiments. I had become interested in this at VPI through the method of paired comparisons. There was a dearth of designs for paired comparisons and I developed some theory for cyclic designs. Actually Oscar Kempthorne had provided a way of analyzing certain cyclic designs. What was lacking were methods of construction and an enumeration of cyclic designs. My first paper on the subject appeared in *The Journal of the Australian Mathematical Society* (David, 1963b).

My main effort, however, was the preparation of the first edition of *Order Statistics* (David, 1970). Also as Editor of *Biometrics* I got interested in the theory of competing risks as a result of a submitted paper.

Then, suddenly I got an invitation to come and visit ISU for the job of Department Head. The invitation was very welcome, because my period as Editor of *Biometrics* was coming to a close and I felt ready for a department head position.

#### HEADSHIP AT IOWA STATE

**Nagaraja:** Do you know who was behind the invitation to come here, like what happened at VPI?

**David:** No, I'm not really sure about that and I don't think that I should speculate except that it's very clear that George Zyskind was much involved.

**Nagaraja:** At that time, had Bancroft retired?

**David:** In those days at ISU you had to retire from administrative duties at age 65. This was obviously an attractive department to come to, with Oscar

Kempthorne and H. T. David probably the best known at that time. Wayne Fuller soon became prominent, and generally it was a high level faculty.

It's a somewhat complex setup at ISU in that there are three bosses that the Statistics Department Head has to answer to. The biggest component is what is now the College of Liberal Arts and Sciences. That's the teaching component. Then there are the College of Agriculture and the Statistical Laboratory. So there were three budgets. It required quite a bit of administrative attention and it was a large department. So that meant restricting my academic activities considerably. No more design of experiments.

**Balakrishnan:** What was the size of faculty, support staff and graduate students?

**David:** The number of faculty was in the low 30's, although that included several joint appointments. The 22-year tenure of my predecessor, T. A. Bancroft, was a good time for expansion, and he had been very active in that direction. A special feature of the department was many joint appointments, with Psychology, Sociology and Political Science. Also, while there were no joint appointments with Agriculture, several faculty members had heavy duties teaching and consulting with people in agriculture, since agriculture is very strong at ISU. So we had several faculty members who did some research, but whose primary focus was on helping others. There were joint appointments with Mathematics also. We averaged over 20 M.S. and about 9 Ph.D. graduates annually. There was also a large survey section, a continuing specialty of the department.

While I was Department Head, I still did some work on competing risks with Mel Moeschberger. We wrote a monograph in the Griffin series (David and Moeschberger, 1978), but that subject moved so fast that this, I think, became dated rather quickly and I did not try to keep up. However, Mel has continued to be active in life testing. I concentrated, therefore, mainly on order statistics, and tended to have only one Ph.D. student at a time.

I appreciated the fact that this was an old department. It's pure chance that I went to a very old department for my Ph.D., really the first Department of Statistics, started by Karl Pearson in 1911. As here, that department was preceded by a statistical laboratory. I soon realized that 1974 marked 50 years since Henry A. Wallace, the New Deal Secretary of Agriculture and Vice President in Roosevelt's third term, had given a series of 10 lectures, very significant in the development of statistics at ISU. Wallace found statistics very important and felt that people at ISU needed to



FIG. 5. H. A. David with Sir David Cox during the 50th anniversary conference of the Statistical Laboratory at Iowa State University, taken in Ames in 1983.

know more about such things as regression, correlation and the use of machines. His lectures were attended by about 20 agricultural and biological scientists and by George Snedecor.

**Nagaraja:** There were other major events while you were Department Head.

**David:** There was Kempthorne's important conference in quantitative genetics in 1976, and the 50th anniversary conference of the Statistical Laboratory in 1983 was a fine occasion. There was widespread recognition that at least in the U.S., this statistical laboratory was an important first of its kind—a first for mainline statisticians to receive formal recognition as a group (David, 1984). So there was an excellent response to having such a conference. We obtained some money from various agencies. D. R. Cox was the keynote speaker. Dennis Lindley and Andrew Ehrenberg also came from England.

**Nagaraja:** John Tukey was at the 50th anniversary conference.

**David:** Oh yes, from the U.S. we had him, we had C. R. Rao, George Box, Erich Lehmann and many other distinguished statisticians. The proceedings appeared as *Statistics: An Appraisal* (David and David, 1984). The conference tried to review what progress had been made in the field of statistics in the preceding 50 years, with some emphasis on those areas of particular interest at ISU.

### ORDER STATISTICS

**Nagaraja:** We would like to hear a little bit more about order statistics. Your book is considered the early

masterpiece. Of course earlier contributions were there in the Sarhan and Greenberg book. Could you tell us more about them?

**David:** The Sarhan and Greenberg book was an interesting undertaking involving quite a few people and I felt fortunate to be asked to participate, sitting rather remotely in Melbourne. In the 1960s, Greenberg had some idea of revising that book. However, I felt that I could write a more unified book on my own (David, 1970). And so this was a major part of my early years at North Carolina. I had actually given a first course of just 10 lectures as soon as I was appointed to the UOM in 1955, and I gave more courses on order statistics at VPI and again at UNC. So I felt ready to write a book on the subject. It seemed necessary to revise it after about 10 years, but by that time I was at ISU. And so it is rather gratifying that I now have Nagaraja working with me on the third edition, for which I definitely need help.

**Balakrishnan:** Any interesting thing that you can reflect on while you were working on this book at North Carolina; particularly somebody who either participated in reading and making some suggestions.

**David:** Well, P. C. Joshi was a strong student working in this area with me. And so he was the one who looked at a draft and made some good comments. Other than that, I think the first 1970 edition was a fairly solitary effort. I had been collecting references on everything I could find relating to order statistics and used that as the basis in writing that book.

**Balakrishnan:** During this time there was research on robust estimation that people like Tukey, Dixon, etc. were working on. I am aware that you had some interest in this direction and one of the chapters in the book was dedicated to this particular topic—quick tests—and one section was on robust estimation. Did you continue your interest in this area?

**David:** Early on I was very interested in dealing with outliers and a number of tables for outlier tests resulted. I have continued to be concerned with the effect of outliers in introducing bias in estimation (David, Kennedy and Knight, 1977).

### HISTORY OF STATISTICS

**Balakrishnan:** Of late you have developed a keen interest in the history of probability and statistics. Could you just tell us how this all started, how it developed and where it is now?

**David:** Well, it's a fun area to work in. I had done occasional little pieces of a historical nature earlier, as



in my 1963 monograph on paired comparisons (David, 1963a) that has a little bit of a history of the method. Then I had a historical article, just a note, in *Biometrika* on the so-called Gini mean difference, which really precedes Gini. So I have had some interest in the history of statistics for some time, but couldn't really indulge the interest until recently. In the last few years I have written a few historical articles. And now I am co-authoring a book with A. W. F. Edwards of Cambridge University, which is in press with Springer at the moment as *Annotated Readings in the History of Statistics* (David and Edwards, 2000).

**Balakrishnan:** How did this collaboration with Anthony Edwards come about?

**David:** I had seen Edwards at the occasional meeting, but it was really through my interest in statistical terms that we started to correspond.

**Nagaraja:** Your recent talk related to this at Ohio State was very well received. And also you informed us of a book on mathematical statistics by Carl West, a faculty member at Ohio State, published in 1918. This was a real surprise for everybody in the audience, as you know. You also told us that it was apparently the first use in English of the term "mathematical statistics."

**David:** Yes, I was a bit surprised that *everybody* was surprised. Studying history has been a very enjoyable activity.

**Balakrishnan:** You also taught a one-unit course in the department here. What was your experience with that? How was this course received by the students and how is it structured?

**David:** After some preliminaries dealing with the more distant past, I started with two principal contributors to our field—Laplace and Gauss. With Laplace there was a lot to select from as he contributed so prolifically to statistics. I ended up with R. A. Fisher. Some of the material had been written about by Steve Stigler or Anders Hald. Of course, we didn't want to repeat too much of that in our book.

I think it is fascinating to see how statistics developed, not necessarily in a logical progression but with contributions from various corners. It is remarkable how early certain ideas came up, sometimes not fully appreciated until a lot later. So the history is really older than many people think. Laplace and Gauss and even earlier contributors already did some very sophisticated things, one of the facts we want to bring out in our *Annotated Readings in the History of Statistics*. And, of course, Anders Hald has done this in a splendid way in his two volumes.

## PROFESSIONAL SOCIETIES

**Nagaraja:** We would like to ask a few questions about your professional impact—in terms of your involvement in professional associations such as the Biometric Society, ASA and IMS. Could you please tell us about these?

**David:** Well, I found this country to be very open and receptive. When I got to VPI, Ralph Bradley happened to be the young Editor of *Biometrics*, so I very soon became an Associate Editor of *Biometrics*. That was really my first involvement in that kind of professional activity. And very much to my surprise, I was elected four years after my arrival in the U.S. to become President of the Biometric Society, ENAR. This was a very friendly welcome. Later on at UNC, I became editor of *Biometrics*. I had excellent conditions for doing this, Bernie Greenberg being a very enlightened administrator. Then what started by chance at VPI, ended at ISU with a term as President of the International Biometric Society. Of course, I also did various lesser jobs in ASA and IMS. Nearly all these assignments were enjoyable and it was a pleasure to get to know numerous fine colleagues as a result.

## INFLUENTIAL PERSONALITIES

**Nagaraja:** We would like to ask you about people who have influenced you as a person, or as a statistician. One I can think of is your wife Vera, for example. May be there are some statisticians you looked up to.

**David:** Vera certainly broadened my horizon. She was fully supportive of my career, entailing some major moves. Vera took an interest in all my students and when I was Department Head at ISU, was particularly active meeting with female graduate students. Appreciation for this activity continues to be expressed by contributions to the Vera David Graduate Fellowship.

Professionally, I was definitely influenced most by H. O. Hartley, my Ph.D. supervisor. Before elaborating I would like to mention N. L. Johnson, who was a splendid lecturer. He was the youngest at that time in the UCL department, in his early thirties when I got there. He did bring some notes along but never looked at them and could even give a course on multivariate analysis without consulting his notes. He had it all beautifully organized in his mind. Hartley was much more diffident in his lectures and had to look at his notes all the time. He became much freer when he came to the U.S. I think he blossomed in this country because there had been no satisfactory job for him in England. Just to give you some idea, he traveled

to work in the morning by train on what was called a workman's ticket. You had to arrive by 7 a.m. He did this just to save money. Pay was very low and he had a family, a wife and two children. Once he came to this country, he became a very self-confident person which, in an intellectual sense, he had been already. He was a delightful person to have as thesis supervisor, which was the term we used for major professor. He had a number of interests, having come to England in 1934, at age 22, with a fresh Berlin Ph.D. in mathematics. Two years later he published four statistical papers in British journals. Then, while holding down a job, he wrote a thesis under John Wishart, for a Ph.D. in statistics. During World War II he worked in a consulting firm, mainly on war-related computing. But with E. S. Pearson he also published some statistical tables during that time, in *Biometrika*, on range and studentized range. So when Hartley had me working on properties and applications of the range, Pearson took quite an interest. And I became more friendly with Pearson later. He visited UNC and gave some talks there. And somehow in the end I was asked to write an obituary notice for him in *The American Statistician*. Actually, while he was still alive, I had written a piece on his work, in a biographical supplement to the *Encyclopedia of Social Sciences*.

Undoubtedly E. S. Pearson, mainly through his connection with Hartley, has also influenced me. Of course, he was very much involved with papers submitted to *Biometrika* and, with only two Associate Editors, he handled a number of my papers himself. So I kept in correspondence with him. Actually there is one paper, that is joint with both of them (David, Hartley and Pearson, 1954). It was immediately postthesis.

**Nagaraja:** At ISU, when you came and even after, (the late) Oscar Kempthorne was considered a very senior member and I was kind of curious about your interactions with him.

**David:** Well, Oscar Kempthorne had developed a deserved reputation of being a bit of a bear for making controversial statements at meetings and the like.

**Nagaraja:** At least in the ISU seminar series the graduate students would have to present a talk on their Ph.D. thesis and they always had nightmares about being challenged by him.

**David:** I will tell you a true story. As I was supported by the Army Research Office, I went to quite a few of their conferences. On this occasion I just gave one contributed paper, on cyclic designs. This was before I came here. Kempthorne was present in the audience

and afterwards he said, "Oh very nice, very nice, as long as you don't think you are making a contribution."

**Balakrishnan:** I wonder whether you remembered this when you came and joined here.

**David:** Oh, I think so. So I was also a little bit worried about Oscar Kempthorne. But I found him extremely supportive here. After all, he put his life's work essentially into this department. He joined the faculty in January 1947, so had already been here for 25 years when I came. He even allowed his arm to be twisted a bit to arrange a conference, a very successful conference on quantitative genetics. And he was very helpful as a key person for the 50th anniversary of the statistical laboratory conference in 1983. It was when away from Ames that he sometimes had this bearish behavior for which he was well known, but he was very deeply concerned with the field of statistics. He was an extremely valuable person to have at ISU, with his several interests—linear models, design of experiments, statistical genetics and statistical inference. We had an Oscar Kempthorne day at the time he retired. I was glad to prepare a personal piece for that.

## RESEARCH REMINISCENCES

**Balakrishnan:** A common curiosity that we both have, Raj and I, is about your 1954 *Annals* paper with Hartley. This deals with bounds on the expected value of the range and extreme order statistics. In the same issue, there was a paper on the same topic by Gumbel (1954). Were you aware of this or did you come to know later about this particular work?

**David:** We were not aware of Gumbel's work, although he used similar calculus of variations methods. This was part of my Ph.D. thesis, which was completed in 1952. Actually, we went well beyond obtaining the two bounds you mentioned and, as I have already said, Hartley contributed the lion's share. Gumbel had a long history of writing on extreme values, of course, and on the range.

**Balakrishnan:** Another interesting note with regard to this paper is that it was mainly considered as a contribution toward a bound on moments of order statistics, but inherent in the work is also a related characterization of the extremal distributions. Some of these results were discovered and rediscovered later on. At that time when the paper was written were you also concerned, you and Hartley, about the characterization aspect of distributions?

**David:** No, it was really only the bound that Hartley was interested in.

**Balakrishnan:** Can you tell us about your association with J. A. John?

**David:** It started through my interest in paired comparison designs. Alan Stuart at the London School of Economics (LSE) had put his student, J. A. John, on to this subject and suggested to him that he should write to me. I had met Alan Stuart casually in my London days. He came to listen to F. N. David's lectures at UCL, and I and a group of others at UCL went to the LSE to hear M. G. Kendall give his lectures. M. G. Kendall became a Professor of Statistics in 1950. Until then he had been working in the Chamber of Shipping, where he had written those two important volumes, *The Advanced Theory of Statistics*. Alan Stuart and James Durbin sat in on those lectures. Kendall actually went through his *Advanced Theory*, a chapter at a time, expecting the students to have read the chapter. So we got to correspond, J. A. John and I. Eventually he visited the U.S. I gave a talk on paired comparison designs at the National Bureau of Standards. James Cameron there was interested in such designs and suggested publishing a catalogue of cyclic designs in their *Applied Mathematics Series*. This resulted in our publication, jointly with my student, Fred Wolock.

#### VERA AND RUTH

**Nagaraja:** I have very fond memories of attending Thanksgiving dinners at your place, which I thought was an extremely thoughtful action as far as international and even domestic students were concerned.

**David:** It was the occasional U.S. student that couldn't go home for Thanksgiving that came, but it

was mainly of course for international students. Well, this was Vera's doing.

**Nagaraja:** Can you tell us a little bit more about Vera?

**David:** We met in Sydney, in 1947 already. We were both students at SU. She was doing biochemistry and also we were in the same Jewish youth organization. So we got engaged in 1949, I went to England, but she had to finish off her final year of biochemistry. Then we were married in London in 1950 and I think there is a photo here of that period. We returned to Australia after completion of my Ph.D. Our son, Alex, was born in Sydney while I was in CSIRO. Then came three years at University of Melbourne and she was willing to make all these moves including going to the U.S. in 1957. In London, as I have mentioned, she worked as a biochemist, but in the U.S. she got interested in educational television and eventually got a degree in Chapel Hill, an M.A. in radio, television and motion pictures. Soon after we arrived in Ames, Vera was roped in to be in charge of a statewide television program called "Iowa 2000." This look ahead from 1973-74 was quite an assignment for someone from out of state. However, her main job in Ames was as a science writer for the Ames Lab, a major organization supporting the research of physical scientists at ISU. So she was finally able to combine her talent in writing with her scientific training. But then Vera came down with cancer and died in January 1991.

**Nagaraja:** What happened then?

**David:** In 1989 Vera had attended a 50th anniversary function of a group of women who as girls in Germany,



FIG. 6. H. A. David and Vera celebrating Thanksgiving at home with international students, taken in Ames in 1979.

Austria or Czechoslovakia had been sent to England without their parents. Living in a refugee girls' hostel, they were a small subset of nearly 10,000 children who were rescued from Nazi Germany by this imaginative Kindertransport program. Vera spent only a short time at the hostel before being picked up by her parents and traveling by ship to Sydney at the outbreak of World War II. At the reunion she renewed her acquaintance with Ruth who had spent seven years in the hostel. Like nearly all the girls, Ruth had lost her parents in the Holocaust. Vera and Ruth corresponded and Ruth and I continued the correspondence after Vera's death. We married two years later.

### RENEWING GERMAN TIES

**Balakrishnan:** During this period you also, after a long lapse of time, went back to Germany, and developed some relations there. Could you tell us something about that aspect of your life?

**David:** Actually I had been back in Germany a few times. My first visit was in 1949 to see a half-Jewish



FIG. 7. H. A. David with Ruth during the banquet at the conference in honor of H. A. David, taken in 1995 at Ames.

cousin. He survived a camp but his Jewish mother died of malnutrition after successfully hiding. Returning was a traumatic experience, with rubble still all over Düsseldorf. In 1984, I went to a conference in Oberwolfach, where I met Ursula Gather, now at Dortmund University, who has become a good friend.

But in 1991 I paid a formal visit to Düsseldorf, such as fostered by the German authorities for Jews and others driven out of Nazi Germany. My visits became more regular after that as Ruth had established links with some very fine Germans who were fighting hatred of foreigners and who were determined to keep the lessons of the Holocaust alive. I met up with some of my high school classmates after about 60 years and am now in regular touch. In the last several years our visits have become annual as Ruth has been repeatedly invited to make presentations from the German translation of her book, *A Child of Our Time*, dealing with her childhood in Nazi Germany and in the refugee girls' hostel in England. She has been well received by her mainly 16–18-year-old high school audiences.

**Nagaraja:** You also attended the Berlin Mathematical Congress in 1998.

**David:** Yes, that was very interesting. The Mathematical Congresses take place every four years, but there had not been one in Germany since 1904, when Hilbert was around. There would surely have been another one in between but for the Nazi period. So the German Mathematical Society took the opportunity to remember the mathematicians, and incidentally the statisticians, that were expelled or worse. They decided to concentrate on those who had worked in Berlin and found as many as 53, including Richard von Mises and H. O. Hartley. Both were Jewish only on Nazi criteria. von Mises was quite remarkable, a flight instructor in the Austrian Army in World War I. In 1921, in Berlin, he founded a journal, now called the *Journal of Applied Mathematics and Mechanics*. Although his war service entitled him to stay on a while, he resigned in 1933 in protest of the firing of other scientists, and left for Ankara and later Harvard.

Hartley left in 1934, straight after obtaining his Berlin Ph.D. His name was then Hirschfeld and he wrote his first five papers under that name. "Hartley" is a brilliant translation: hart = deer = hirsch and ley (Old English) = lea = field = feld. He may not have been responsible for the translation.

**Nagaraja:** I know you are an avid reader. What other things do you do in your spare time—in the past or in recent times?

**David:** As already mentioned in connection with the method of paired comparisons, chess was an early hobby of mine, but I didn't continue that in the U.S. and now it's mainly reading. Also, even before Ruth came, I gave occasional talks on my experiences as a boy in Nazi Germany to schools in Ames. Together and separately we have done more of that in Ames and vicinity, to school and university student audiences. With much still being written about it, the Nazi period takes up a good bit of our time.

### CURRENT PURSUITS AND PLANS

**Balakrishnan:** Could you elaborate on projects that you are currently involved in and what you think the future holds for you personally?

**David:** Well, it's unreasonable to count on too many more years, but I was really quite surprised to be asked by Wiley, "Was I interested in revising the second edition of the OS book" and I'm sure you, Bala, had quite a bit to do with that. I felt I couldn't cope with all that had been published in the past 20 years, especially the asymptotic theory which I had never done a great deal with. Fortunately Nagaraja has agreed to co-author the book. So, we are at the beginning stage of this. Although I have fewer distractions now, I am also slower in getting things written, so it's going to take time.

**Balakrishnan:** I think it will be a good project to come out because I distinctly remember when I was working as a Ph.D. student, the book served not just as

a textbook, but more as research guidance for people who were interested in the subject and it certainly is going to do the same for the next generation, I am sure.

**David:** Well I'm hopeful.

**Balakrishnan:** We are on the final part of this interesting conversation. What is your opinion on the current status of the areas that you have been involved in, not only OS, but also other areas? And in your opinion, what have been the interesting and noteworthy developments in these areas?

**David:** Well of course, a very great deal has been written on OS to which both of you have made many contributions and there are these two fat volumes edited by Balakrishnan and Rao (1998a, b). There are now a great many areas of application of OS. One that came fairly late for me was OS filters. Electrical engineers are involved with these in quite a deep way. I imagine that there is still plenty of scope for dealing with OS in nonstandard situations, not just the i.i.d. case on which, of course, an enormous amount of work has been done. There is a need to delineate other situations and wrestle with them when there are various kinds of dependencies. The computers are going to be involved in some of that, especially in finite samples.

The method of paired comparisons is a fairly narrow area, although it includes design aspects. I'm not sure that there is a great deal more to be done with it, especially as there are more general methods in which you can embed the method of paired comparisons. The theory of competing risks has many facets, although it is



FIG. 8. At the surprise 75th birthday party in Mysore, India, on December 19, 2000, during a conference on order statistics and extreme values. At right is Ruth David and at left is Professor C. R. Rao, who turned 80 on September 10, 2000.

best viewed as part of life testing and I think there are still problems that people will want to investigate.

### PARTING THOUGHTS

**Nagaraja:** You always keep coming up with new ideas and research questions. Is there a recipe for this creative thinking process?

**David:** Well, I don't know how transferable these ideas are. In a way I have been lucky. Hartley certainly got me going on order statistics, even though the term doesn't appear in my thesis. We used "*m*th value." So that was the obvious area I was going to keep working in, and then I mentioned how paired comparisons came up. I was lucky that there was this 1955 paper of Kendall's followed by Kendall's visit which got me interested in the method of paired comparisons. As for the theory of competing risks, well, as editor of *Biometrics* I came across some work in that field and decided that perhaps it could be done in a different way.

Then, as a result of my CSIRO experience, I had an exercise in the first, 1970, edition of *Order Statistics*, on the expected gain in fleece weight of daughter sheep when the best few father sheep had been selected for mating. Soon after, a paper was submitted to *Biometrics*, on ranked set sampling with judgment error. I realized that the two situations could be modeled in terms of the same statistic, which seemed worth studying as a class. That initiated work on concomitants of OS (David, 1973). As it happens, Bhattacharya (1974) independently came up with a different motivation altogether, using the term "induced order statistic." I then worked on the subject with Janos Galambos and my students, M. J. O'Connell and S. S. Yang, as well as Raj.

So there were specific stimulants for much of my research. However, I feel that the field of statistics is still young enough that further thought on many issues will raise new problems to investigate.

**Balakrishnan:** You were very successful as a writer, communicator and academic advisor. Many of your students like Raj and nonstudents like me have very high regard for what you have accomplished both academically and administratively. If you were to give some advice for the next generation, what will that be?

**David:** Well, I have to recognize that the field has changed a great deal with the advent and impact of the computer and there I'm not really at home. Ph.D. students of mine and M.S. students to some extent continued to use the computer to prepare various tables, and that was important, but now the impact of the computer is ever greater even on the theory side, for example, in bootstrapping and the theory of

the bootstrap. But it's interesting to see how the field of applications of statistical methods has grown. Of course, now with a computer one is prepared to tackle situations which had to be simplified greatly in the past in order to obtain results and which can now be handled much more thoroughly. So I think that's where the main drift and impact of the field is clearly going. The field was still on a fairly modest scale when I started. Take Australia. When I was a student, there was no chair of statistics in the country. There were these two well-known statisticians—Pitman and Cornish. Pitman was professor of mathematics and Cornish was in charge of CSIRO statistics. Finally Melbourne got a chair in 1948 and now there are many chairs in Australia. That's just a microcosm of what has happened in this country, Canada and other countries. There are plenty of opportunities for young people to become involved and make contributions.

**Balakrishnan:** I remember Ted Anderson telling me a few months ago that he considered retirement seriously when he realized most of his students were retiring. He said, "That was an indication that maybe I'm staying too long."

**David:** I was the first in this department to have the option not to retire at 70. But I thought that the time had come and I had better set a good example. I retired but haven't set such a good example because I am still around. And I am very appreciative of the fact that I still have a room to work in and I still have secretarial help.

**Balakrishnan:** Well, that is only appropriate. You are still active.

**Nagaraja:** Thank you for meeting with us.

**David:** I greatly appreciate the time you've taken to come here and do this interview.

### ACKNOWLEDGMENTS

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