Statistical Science
2000, Vol. 15, No. 4, 396–408

A Conversation with Johannes H. B. Kemperman

Joseph I. Naus

Abstract. Johannes H. B. Kemperman, born in 1924, received his Bachelor of Science in 1945 and Ph.D. in 1950, each in mathematics and physics, from the University of Amsterdam. From 1948 to 1951 he was a Research Associate at the Mathematical Centre in Amsterdam. During 1951–1953 he was a Visiting Professor on a Fulbright grant at Purdue University, and subsequently he joined the faculty and stayed at Purdue for 10 years, becoming a full Professor in 1959.

In 1961 he went to the University of Rochester, becoming the Fayerweather Professor of Mathematics in 1970. He stayed at Rochester for 25 years. In 1985 he joined the faculty at Rutgers University as a Professor in the Statistics Department and also a voting member of the Mathematics Department. He retired to emeritus status at Rutgers in 1995.

He served in editorial posts at the *Annals of Mathematical Statistics*, *Annals of Probability*, *Annals of Statistics*, *Aequationes Mathematicae* and *Stochastic Processes and Applications*. He is a Fellow of the Institute of Mathematical Statistics and the American Association for the Advancement of Sciences and is a Correspondent of the Royal Dutch Academy of Sciences (Amsterdam).

He has produced 23 Ph.D. students, 3 books and over 100 publications in analysis, number theory, group theory, probability, statistics, functional equations, mathematical biology and other areas.

Naus: Joop, your Bachelor of Science was from the University of Amsterdam in 1945, the year World War II ended in Europe. What were your interests and areas of study as an undergraduate, and how were you able to pursue them?

Kemperman: At the time a study at the University of Amsterdam (my home town) offered only a few directions in each field. Right from the start I went for a joint major in mathematics and physics, with minors in astronomy and crystallography—Roughly half in mathematics and half in physics (and nothing else).

In high school I was pretty good. When just a senior, in the early fall of 1940, the director called me to his office and asked: "What are you going to do after you pass your exams?" He was referring to the written and oral comprehensive final examinations to be held in May 1941 and lasting several weeks. I had no idea. I was barely 16 and hadn't even thought about it. He said: "You better apply for some scholarships right now; otherwise, it is too late." Anyway, I got several scholarships which enabled me to go to the University. My parents were just middle class people. They had five children and could not possibly afford it. Rather my father expected me, the eldest, to help pay the bills.

I started my university studies in September 1941 when Holland was already occupied by Germany. A year and a half later the University essentially closed up, and I had to go into hiding. I could not attend any courses, so instead I studied from books and from notebooks borrowed from older students. By the time the war was over, I had essentially passed the requirements for the B.Sc. degree. I took all exams for my courses in an oral way. For each course, when ready, I made an appointment over the phone with the professor and then went to his home to take the oral exam, usually lasting about an hour. Officially, this was illegal of course.

Naus: So actually they were running the University from their houses rather than from the University?

Kemperman: No, the professors were still teaching at the University, but teaching to very few students. About 10% of the students signed a short declaration to the effect that they promised not to sabotage the system. The Germans required all the

Joseph I. Naus is Professor, Department of Statistics, Rutgers University, Busch Campus, Piscataway, New Jersey 08855.

students to sign that declaration. But this came on top of a lot of things that had happened already, especially the deportation of Jewish people. This was the last drop in the bucket. So most students decided not to sign at all. After that, the University was officially still open, but there were hardly any students left, only those who did sign—their parents might have been in a sensitive position or for other reasons.

I received my Bachelor's in October 1945. After that, it was a normal graduate study. Again there was not all that much choice. The combination I chose this time was a major in mathematics with a minor in physics. I was also in a physics lab (Zeeman Laboratorium) for half a year or so, measuring paramagnetic relaxation at very low temperatures. The results were incorporated in a 1947 physics paper jointly with L. J. F. Broer.

Most mathematics courses lasted a full year, both at the undergraduate and graduate level. These courses tended to be rather theoretical, without a textbook or any exercises. After each course there would be an oral exam, at a mutually convenient time, where the (undergraduate or graduate) student was expected to know most of the proofs in full detail.

THE MATHEMATICAL CENTRE

Naus: I noticed that several of your papers were from the Mathematical Centre in Amsterdam. Is the Mathematical Centre the same as the University? What is their relation?

Kemperman: The Mathematical Centre (Mathematisch Centrum) was established right after the war, February 11, 1946. The Centre is independent of the universities and reports directly to the Ministry of Education. One of its main goals is to give promising young people a chance to do mathematical research without the usual worry about income or being promoted. Up to that time most Dutch mathematics students became high school teachers (without having taken a single course in education). When I started studying mathematics, simply because I loved mathematics, I didn't even know that there was any other possibility. Throughout my student years, I did quite a lot of tutoring as well as part-time teaching at a small private high school, some 20 hours a week, for extra income.

In March 1948 I passed the "Doctoraal Examen" and thereby acquired the title of Doctorandus (Drs.). This degree is about equivalent to passing a Ph.D. qualifying exam in the U.S.A. The same month I started my job at the Mathematical Centre—specifically, a full-time job as a Research Associate (Wetenschappelijke Medewerker) in its Department of Applied Mathematics (Toegepaste Wiskunde). Here, I soon got deeply into research and it quickly became obvious that it wasn't necessary at all for me to become a high school teacher.

In those years the Mathematical Centre was headed by four people, Professors van der Corput, van Dantzig, Koksma and Schouten. They were themselves professors from different places. Van der Corput used to be at the University of Groningen before moving in 1946 to the (Municipal) University of Amsterdam. Van Dantzig used to be at the University of Delft and had also recently joined the University of Amsterdam. Schouten was at the University of Delft while Koksma was at the Free University. Amsterdam always had two universities, the Municipal University of Amsterdam, now simply called the University of Amsterdam, and the so-called Free University, which has its origins in the (Dutch) Reformed Church.

The Mathematical Centre had four departments: Pure Mathematics; Computational Mathematics; Statistics; and Applied Mathematics. Van der Corput was head of Pure Mathematics, van Wijngaarden head of Computational Mathematics, van Dantzig head of Statistics, while van der Waerden was head of the Department of Applied Mathematics, which then consisted of just one other person, me.

Naus: Your Ph.D. thesis was published in the Hague.

Kemperman: That is correct. The final writing was done somewhat in a hurry since the final Ph.D. exam was firmly scheduled for the middle of December 1950. Each time I had finished a chapter, it was sent by overnight mail from Amsterdam to my publisher in the Hague. It came back in one or two days for proofreading, nicely printed, a sort of photo-offset.

I wrote my thesis independently following a suggestion made to me by van der Waerden, my boss at the time. But officially I am a Ph.D. student of David van Dantzig, because my thesis was in his area of probability and statistics.

Naus: At the Mathematical Centre?

Kemperman: Yes. While working at the Mathematical Centre, I started my thesis research early in 1950. At that time I had already written quite a few papers, mainly in number theory, analysis and groundwater flow. My slightly later interest in probability and statistics was very much stimulated by the then very new books by Feller (1949) and Cramér (1946).

The situation with van der Waerden is a little complicated. He was Professor in Leipzig 1931–1945. When he came back to Holland right after the war, some people accused him of silently support-

FIG. 1. *Final ceremony of Doctoral Exam (cum laude); his parents are seated at left front, family friends and colleagues. Behind Joop's parents, standing is a man in a grey suit, statistician, Jan Hemelrijik, then a Ph.D. student, who later became a professor at Amsterdam. Piet Kanters, a mathematician is 3rd from right sitting.—Wednesday, December* 20, 1950

FIG. 2. *Holland America Line (trip from Rotterdam to Hoboken, New Jersey).—early September* 1953.

FIG. 3. *Joop and Wilna on the boat—September* 1953. (*The other man in the picture is Hans Koster*, *a Dutchman*, *who taught engineering at Purdue University*.)

FIG. 4. *Joop in a lounge chair at Gateshead*, *Martha's Vineyard—summer* 1988.

ing the enemy by not quitting at an earlier stage. He was a highly respected mathematician, but he had his family and had his reasons.

A natural thing would have been that van der Waerden be immediately appointed a professor at the Municipal University of Amsterdam. But the Amsterdam city council, being rather leftist and very anti-German, held up his appointment for several years. Already in 1945, van der Waerden did get a position as an Applied Mathematician at Shell Laboratories. Simultaneously, from 1946, he was a member of the Board of Supervisors of the Mathematical Centre as well as head of its Department of Applied Mathematics. In 1949, van der Waerden finally received his much deserved appointment as Professor at the Municipal University of Amsterdam. Two years after that he left Holland permanently for a position at the University of Zürich.

I was the only other member in the Department of Applied Mathematics. Later on there were more. The other departments were somewhat larger. Now

FIG. 5. *Family photo by the shore*; *family came together for Joop and Wilna's* 35*th wedding anniversary—Martha's Vineyard*, *summer* 1988.

and then, as a service to the public, we used to solve some problems that came from the outside. For instance, I solved quite a few groundwater-flow-type problems directed toward understanding and improving the Dutch drinking water supply system. They were posed to me by Ingenieur Huisman, a very good engineer at the Amsterdam waterworks.

Each time, after settling such an outside problem, I immediately went back to my own research. There were no strings attached. You could do any research you wanted to. So I did research in number theory, pure analysis, probability and statistics as well as applied mathematics. An experience like mine would be good for most everybody else. I was at the Mathematical Centre a little over three years, March 1948 to July 1951.

Naus: Was there interaction between the people at the Mathematical Centre, or mainly within each group?

Kemperman: To begin with, in 1948, there were only some 10 people on the scientific staff, but gradually that staff got bigger. There also was a computational staff of some 10 young and intelligent women who skillfully handled all our computational problems, even numerical solutions of partial differential equations, using a Marchant electric calculator.

Each day, someone would bring coffee in the morning and tea in the afternoon. From the begin-

FIG. 6. *Joop and other mathematicians and probabilists at a Conference on Measure in honor of Dorothy Maharam Stone at the University of Rochester—September* 17–19, 1987. *Sixth row*: *S. J. Eigen, W. A. J. Luxemburg, K. Park, N. Jochnowitz, L. Nachbin. Fifth row*: *J. M. Hawkins, M. Akcoglu, A. Bellow, D. Ramachandran, A. Maitra, R. Zaharopol, S. Landry. Fourth row*: *J. King, J. Coffey, T. Bick, T. Armstrong, R. D. Maulding, S. Graf, M. Burke. Third row*: *J. Auslander, K. Berg, D. J. Rudolph, A. Fieldsteel, K. Petersen, V. S. Prasad. Second row*: *J. H. B. Kemperman, D. Kölzow, S. Kakutani, J. R. Choksi, V. Peck. First row*: *A. H. Stone, D. Maharam Stone, C. E. Silva, R. M. Shortt.*

ning, there was a great deal of interaction between departments, and we often visited each other. A popular get-together was during lunch—brown bags with sandwiches—where we played Ping-Pong or chess or simply talked to each other.

The Department of Computational Mathematics was engaged in building a high-speed computer, in fact several in succession. The first one was built with leftover parts from army dumps. But the relays often got stuck because of dust, so a girl would go around and around with a hairblower, blowing the dust off the relays. In fact, that computer never worked very well and was no match for the Marchant calculators.

Naus: We had those Marchants in college for statistics calculations. There was a whole lab with many running at the same time.

I saw that your Ph.D. thesis was on random walks and sequential analysis. And you also had a paper coming out that same year on the distribution of the Mann–Whitney test. How did you get interested in probability and statistics?

Kemperman: Initially, my research was in pure analysis and number theory. In those days probability was never taught as a regular course, and there were only a few available books in probability, such as those by Uspensky, von Mises and Lévy. But then Cramér and Feller came out with some very readable books and these very much stimulated my interest in probability. At the University of Amsterdam, van Dantzig did teach a course in probability and statistics, but I never took that course, being somewhat ahead already. I picked up most things by myself from books and journals.

My interest in sequential analysis relates to a trip van der Waerden made through the United States. Right after the war, it was customary for Dutch professors to visit the United States—they had been isolated for several years—and see what the newest things were. There van der Waerden also learned about the new field of sequential analysis which during the war was a secret project. Its technique was not available to the enemy.

On his return, van der Waerden suggested to me that this new area might be a good subject for my Ph.D. thesis. It turned out to be a productive area

FIG. 7. *Joop and Wilna and their family cross-country skiing, one of the things they loved to do, Old Forge, Christmas* 1980 (*left to right*: *Bruce, Wilna, Ingrid, Eric, Steve, Hubert and Joop*).

to work in and it was also a field I was already interested in, because sequential analysis is obviously closely related to random walks.

Originally, my thesis would have been in number theory. However, my number theory thesis project gradually grew into a joint research project with Professor van der Corput, each contributing many ideas. Thus, it became more natural to write up all our results in several joint papers, appearing in 1949.

Naus: Van der Corput?

Kemperman: Yes. Van der Corput worked in many areas but is most well known for his contributions to number theory. He was also one of the founding fathers of the Mathematical Centre.

Naus: Thus your research followed a sort of random drift. Was there anyone else working on probability or statistics with whom you interacted?

Kemperman: Yes, especially with the members of the Statistics Department, such as Jan Hemelrijk. At least during the first few years, that department was the largest one in the Centre. Professor David van Dantzig was its head. During 1927–1940 he was at the (Technical) University of Delft. Because he was Jewish, he was dismissed in 1940 and immediately moved to Amsterdam. He was never sent to Germany. His research interests had gradually moved from differential geometry and topological algebras to probability and statistics. He started to advocate that at least somewhere in Holland there should be a chair in probability and/or statistics. Right after the war, in early 1946, van Dantzig was appointed to exactly such a chair, namely, at the Municipal University of Amsterdam.

As I mentioned before, van Dantzig was a founding father of the Mathematical Centre, together with van der Corput, Koksma and Schouten. Many of the main ideas about the final shape of the Centre came from van Dantzig. The main positive and constructive influence came from van der Corput, who was more diplomatic and had very good relations with van der Leeuw, who then was the Minister of Education, Arts and Sciences. (Before the war they were colleagues at the University of Groningen.) Van der Corput knew a lot of people, also because he did much underground work during the war.

Right after the war, there was a great feeling of elation that we were free again. Slowly, things became available again, such as clothing, shoes and food. There was a feeling of optimism and that anything was possible. Normally, for a significant new institution such as the proposed Mathematical Centre, one would have to consult with all the Dutch universities and nothing would happen. Instead, the whole thing was done in a somewhat underhanded way, especially between van der Corput and van der Leeuw. The other Dutch universities were thus faced with a “fait accompli.” As a consequence, for several years, there remained a kind of mistrust of the universities toward the Mathematical Centre: “Why is it in Amsterdam and not, for instance, in Utrecht?” And so on.

PURDUE

Naus: In 1951 you came to Purdue on a Fulbright Grant and then you joined the faculty for 10 years. What influenced you to go to Purdue, and what was the department like in that decade?

Kemperman: In the Mathematical Centre, Jaap Korevaar was Research Associate in the Department of Pure Mathematics. He received an invitation to come to Purdue in 1949 as a Visiting Professor. That was for one year, but it became two years. He returned in 1951, having accepted a professorship at the University of Delft. At Purdue they liked Jaap Korevaar and asked him to recommend a successor, and he suggested me, upon which Purdue invited me to be a Visiting Professor during the academic year 1951–52. At about the same time I received an invitation to spend a year at the Istituto Picone in Rome, which was strongly oriented toward applied mathematics, including numerical analysis. Fichera was there. But in those days America was the top of the heap. Thus the choice was not difficult and I went to Purdue. My travel expenses were paid by a Fulbright grant. In 1952 Purdue asked me to stay for another year and in 1953 they offered a tenure track position as an Assistant Professor, starting September 1953—which I accepted.

I was not married and did some dating during 1951–53. But in my thoughts there always was Wilna Ypma, whom I had met in February 1949 at a Mardi Gras party with friends at a home in the south of Holland. Wilna actually came with another boy. We immediately fell for each other. During 1949 we dated regularly but then things cooled off, also because Wilna was kind of young then and still in high school. But we kept in contact and during 1952–53 we started to correspond again on a regular basis. Soon after my return to Holland, in early June 1953, I proposed to her and she said yes. That same summer we got married in Alkmaar, her hometown. After a short honeymoon in Belgium, we embarked for the States. The people at Purdue were quite surprised that I came back with this lovely girl Wilna.

I have very fond memories of my time at Purdue. It certainly was a very lively place with much interaction. Many of the students were GI-students, somewhat older, hard working and well motivated. Teaching them was quite a pleasure. I shared an office (in the Recitation Building, which is still there) with Arthur Rosenthal, a full Professor, and Merritt Webster, an Associate Professor. (Later on we were promoted to having only two in each office.) Purdue had 13,000 students. The Mathematics Department had about 45 faculty, small enough to resemble a large family.

Naus: This was the Mathematics Department?

Kemperman: Yes. The Statistics Department did not exist at the time. Instead, within the Purdue Mathematics Department there was the so-called Statistical Laboratory, headed by Carl Kossack. The Laboratory had a budget of its own and about five or six full-time appointments. Morris Skibinsky was there as well as Irving Burr, Louis Cote and Virgil Anderson. Other faculty in probability and statistics, such as Henry Teicher and myself, had only a partial appointment (and office space) in the Statistical Laboratory. Initially, most of my research at Purdue was in number theory and in analysis, such as asymptotic expansions. Because of the joint appointment, I was often assigned a class in probability or statistics. This greatly stimulated my interest, and ever since about half of my papers have been in the area of probability and statistics. For instance, teaching a class on nonparametric statistics led to my 1956 *Annals of Mathematical Statistics* paper on generalized tolerance limits.

Naus: In 1961 there came out a book—I remember it, as I was a graduate student then—*The First Passage Problem for a Stationary Markov Chain*. It was a classic then. And even recently my Ph.D. student used a result from it. How did you get interested in writing the book?

Kemperman: Again, it was not planned. I just wrote a long article to be submitted to the *Annals of Mathematical Statistics*. Somehow, the paper became longer and longer as I found more things. I was a little hesitant, but I did send it off to the *Annals*. It seems that Jack Kiefer first came up with the suggestion that a somewhat extended version of the paper would be very suitable to be the first volume in the already planned Monograph Series of the Institute of Mathematical Statistics. And I agreed. The final version of the monograph was written during 1958–59 when I was on a full-year leave in the Netherlands, supported by the National Science Foundation and a sabbatical. That year I got an awful lot done—the book and several papers. Some background material had to be added here and there, but the larger part of the book is new. Quite a few of these new results were inspired by the then recent work of Frank Spitzer. Specifically, his beautiful Wiener–Hopf type approach to random walks. Hindsight tells me that the book was somewhat too compact, thereby obscuring many new results. Perhaps I should have made more noise about them, for instance, by formulating them as theorems or by discussing interesting special cases and/or applications. The next academic year, 1959–60, I was back at Purdue. And during 1960–61, I was on leave at the University of Wisconsin, doing full-time research at the Army Research Center.

Naus: There are many ideas in your book. For instance, useful explicit formulae for a random walk with integer jumps. In 1961 you went to the University of Rochester . . . .

Kemperman: By the way, before going to Rochester, I was also invited to Cornell University for the summer of 1960 to work on a project in information theory headed by Jack Wolfowitz. At the time, Wolfowitz was writing a book on the subject and he had all sorts of interesting problems. I got very much interested in his problems and found a lot of new things. As a result, I still have a strong interest in information theory. It was a great time. In just three months, I got to know a very active research group at Cornell, learned a new field and got a lot done. The weather being perfect, we went on lots of hikes and picnics, often with friends and visitors, while enjoying Ithaca's beautiful environment.

ROCHESTER

Naus: In 1961 you went to the University of Rochester. You were there for 25 years if I calculated correctly. What led you to go there and could you tell us about the department in those years?

Kemperman: At the time, the University of Rochester had quite a small mathematics department. It always had some very good people. Bill Eberlein was already a member as well as John Randolph, Ralph Raimi and Norman Johnson. Walter Rudin had been there (before he went to Wisconsin) and I believe also Paul Cohen. In 1960, Leonard Gillman was selected as mathematics Chairman, with his main task being to expand and upgrade the department. The University Administration granted him much freedom in hiring new people. Before that Gillman was a colleague of mine at Purdue. He got a Master's degree in piano from the Juilliard School of Music and a Ph.D. degree in mathematics under Alfred Tarski. He is also a coauthor with Meyer Jerison of a classic monograph on rings of continuous functions.

During 1960–61 Gillman called me repeatedly to see if I had an interest in joining the University of

Rochester. From Wisconsin, I visited Rochester; Wilna came along, and we both liked what we saw and soon agreed to come. Frankly, the salary was part of it. My salary more than doubled in just two years's time. Other newcomers to the department, in the fall of 1961, were Arthur Stone, Dorothy Maharam (Stone) and Charles Watts. Others soon followed, such as Norman Alling, Leopoldo Nachbin, Sandy Segal, Govind Mudholkar, Norman Stein, Rick Lavine and Gerard Emch. After just a few years there were some 20 people there. Some came and went, such as Ken Ross, Wis Comfort, Stan Tenenbaum and Richard Mosak. There was a lot of time for research. The graduate students in mathematics were small in number, but they were of high quality, as Rochester had a very good name. One other nice thing about the University of Rochester was the close contacts between departments. This was in no small measure due to its excellent faculty club, where during daily lunch or coffee (after lunch) there were many animated conversations between members of different departments.

Naus: So you were in the Mathematics Department at Rochester. What was the relation between Mathematics and Statistics?

Kemperman: In 1961, Rochester did not have a separate Statistics Department. In fact, initially, the statistics on campus amounted to a very few appointments in total, spread over psychology, business, economics and medicine. The first statistician in the Mathematics Department was Govind Mudholkar, joining in 1963. Also, early on, Charles Odoroff came to the medical school, while Poduri (Sam) Rao and Julian Keilson joined the business school, as statistician and applied probabilist, respectively.

In 1963, Allen Wallis became Chancellor of the University of Rochester. He himself is a well-known applied statistician who also was instrumental in the formation of statistics departments at Columbia University and the University of Chicago. Wallis strongly promoted the idea of having a separate Statistics Department also at the University of Rochester. The natural way was to start out with a good chairman and we considered several good candidates. In 1968 Govind Mudholkar, while on leave from Rochester at Stanford University, met Jack Hall, who was then also at Stanford on a two-year leave from Chapel Hill. Jack happened to be interested in the chairmanship and, clearly, would be an excellent choice. During Jack's subsequent visit to Rochester in early November, things seemed to click and we did offer him the position. We were somewhat concerned because, exactly at the time of Jack's visit, the city of Rochester was (rather unusually) under a foot of snow. Fortunately, he accepted.

Jack Hall was a very good chairman. He stayed on as chairman for many years and attracted excellent people to the Department of Statistics, such as Ruben Gabriel, David Oakes and also (for several years) Al Marshall and Jon Wellner. The Statistics Department at Rochester was never very big, perhaps five or six full-time equivalents. Part-time members were Sam Rao, Julian Keilson, Charlie Odoroff, myself and others. My own tenure was always in mathematics.

Naus: We were fortunate to have you come to Rutgers University some 12 years ago. At Rutgers, you were on the faculty of the Statistics Department and a full voting member of the Mathematics Department. Could you tell us about some of your experiences here?

RUTGERS

Kemperman: I came to Rutgers in the fall of 1985. Rutgers University is a lot bigger place than the University of Rochester, with many different weekly seminars across statistics, pure and applied mathematics, computer science and operations research. The University of Rochester also had many invited speakers, but we usually had to pay them an honorarium plus travel expenses. Because of its central location, Rutgers enjoys having a large supply of speakers who can be attracted with less money.

In Rochester, we interacted a lot on a social level and to a lesser degree on a professional level. Naturally, we did attend each other's lectures and often commented on each other's papers. I myself had only a few joint papers with other Rochester faculty: one with Dorothy Maharam and several with Chris Waterhouse in the medical school on compartment models. In addition, I wrote several papers jointly with Ph.D. students of mine and a number of joint papers on mathematical photography with Eugene Trabka of Eastman Kodak.

Here, at the Rutgers Statistics Department, I did joint work with Arthur Cohen and Harold Sackrowitz, some of which has not yet fully developed. Several other joint papers had a New Jersey coauthor. Moving to Rutgers, with its vibrant mathematical atmosphere, was like starting a second or third life. Though I always liked Rochester, after 25 years the challenge was gone somehow and Rutgers meant a sort of rejuvenation. When coming all the way from Europe, you never feel completely rooted anyway.

Naus: I recall that Harold Sackrowitz explained at a seminar an approach that he and Art Cohen have had great success with in handling problems. They had a particularly effective technique, and that was to ask Joop.

Could you tell us about some of your interactions with other researchers? I see that, in addition to the faculty at Rutgers, you have done research with Persi Diaconis, Morris Skibinsky, David Cox, ...

Kemperman: David Cox was a Ph.D. student of mine. As often happens, I made major contributions to some of his problems and David strongly felt that such results should be written up as joint papers. As to your question, it is difficult for me to give a fair picture of the joint research I have done with more than 30 different co-authors spread over a period of more than 50 years and comprising about one-third of my published work. Each joint paper has its own history.

Naus: You have worked on a wide variety of problems in number theory, analysis, probability and statistics. Tell us about some of the papers you particularly liked.

RESEARCH

Kemperman: Some of the areas I worked in are analytic functions, the theory of moments (such as measures with given marginals or sharp inequalities for martingales), random walk (such as Wiener–Hopf theory or oscillating random walks), information theory (such as additive noise or channels with feedback), functional equations, distributions modulo 1, birth and death processes, mathematical biology, mathematical photography, tomography and others.

Some of the papers I am most proud of are in number theory and related group theory. For example, the following is an easy-to-state special result from my 1964 *Fundamenta Mathematicae* paper. Let G be a unimodular and connected locally compact group with two-sided Haar measure μ. Further let A, B be nonempty measurable subsets of G and let AB denote the set of all products ab with $a \in A$, $b \in B$. Then $\mu(AB) \geq \min(\mu(A) + \mu(B), \mu(G))$. The proofs were inspired by some of my earlier work in number theory.

It all started in the 1930s with the so-called $(\alpha + \beta)$-conjecture. You take a set A of nonnegative integers, containing zero and having density α—meaning that, for all $n \geq 1$, at least a proportion α of $\{1, 2, \dots, n\}$ belongs to A. Let B be an analogous set of density β. The $(\alpha + \beta)$-conjecture states that then the sumset $A + B$ has density $\min(\alpha + \beta, 1)$. This conjecture was finally proved in 1942 by Henry B. Mann. (Mann had also a broad interest in statistics, such as design theory. Particularly famous is his 1947 *Annals of Mathematical Statistics* paper jointly with D. R. Whitney.)

About 1946, while still a graduate student, I attended a series of lectures by Professor van der Corput where he discussed Mann's 1942 proof as well as some new and more general results by himself and by E. Artin, P. Scherk and Freeman Dyson (who at that time was still a mathematician). Thinking about such proofs, I came up with some new ideas about how to do things more simply even in a more general setting. When my ideas were streamlined enough. I showed them to van der Corput and he found them quite promising. This became a joint research project and we wrote three joint papers on it called "The second pearl of the theory of numbers." This because of a little booklet by Khintchine titled "Three pearls of number theory." Mann's proof was the second pearl.

Ever since I have been interested in related sumset problems. For instance, in my 1960 *Acta Mathematica* paper I determined the precise structure of a pair of finite subsets A, B of an (additively written) Abelian group such that $|A + B| < |A| + |B|$, where $|C|$ is the number of elements in the set C. Roughly speaking, such pairs can be built up from arithmetic progressions in associated factor groups. Analogous results hold for any Abelian locally compact group. Often, my structure theorem implies that a theorem about sumsets $A + B$ needs to be verified in only very special cases.

Another area I have worked in is functional equations. My 1957 paper in the *Transactions of the American Mathematical Society* had a lot of influence. One of my students wrote a Ph.D. thesis on a related subject and so did a student of his (a sort of grandson). Another student and I studied functional equations over Abelian groups of the mean value type, but that paper wound up in the drawer. In fact, I have many beautiful things waiting for a final write-up. Most everybody has that.

At the University of Waterloo, there is Professor Janos Aczél, who throughout many years has been a strong promotor of functional equations—not only through a long list of publications but also in general. Every year he organized a get-together in some nice place of the world, such as Elba, Lago di Garda or Waterloo, and I attended quite a few of those conferences. I noticed that Ingram Olkin did the same thing. No doubt, much of the considerable progress in the field can be attributed to these yearly international conferences.

Naus: You have done work in mathematical biology.

Kemperman: My interest here was stimulated by a junior–senior level course in mathematical biology that I taught at the University of Rochester, now and then. At the time there was not a good textbook on the subject, so I had to hand out notes, sometimes on new results of my own. For example, I wrote several papers on systems of mating. Consider the following imprinting model among pigeons, which was proposed by M. B. Seiger. You have white pigeons and black pigeons, where black is dominant. We will assume (as taboos) that a pigeon will always refuse to mate with another pigeon whose color is different from both of its own parents. For instance, a white male with two black parents can only a marry a black female. The trouble is that this female will not like him unless she has a parent of each color, in which case she would accept any male. This assumption forces us to distinguish between six types of pigeons (depending on its genotype and on the color of its parents). For this and many other taboo models, and assuming large populations, I studied the possible equilibrium situations. More precisely, using linear programming methods, I determined what type distributions can be maintained, from generation to generation, by at least one suitably chosen admissible way of pairing up the available males and females. Some individuals may remain unmated. But if the type pair (i, j) is not taboo, then there must be either no unmated males of type i or else no unmated females of type j (in order that the pairing on hand be admissible). For the above pigeon model, we find for instance that there exists an equilibrium situation with precisely a fraction u of unmated individuals if and only if $0 \le u \le (2 - \sqrt{2})/4 = 14.6\%$.

There are other biology type papers in the drawer. For instance, two papers on the computation of identity by descent probabilities for sets of genes relative to a given pedigree (tree of known ancestors). It turned out that these papers were too mathematical to be published in a biology journal and too biological to be published in a mathematics journal.

Naus: What about the *Journal of Mathematical Biology*?

Kemperman: I sent it there. They found it too mathematical. I could publish them as lecture notes. Somehow you lose interest after a while.

Naus: In terms of your current research, I see that you are doing a tremendous number of things. Could you tell us a little bit about it? You mentioned a new book that just came out.

Kemperman: I have it here [reaching over the coffee table]. You can have this copy.

Naus: *Comparisons of Stochastic Matrices with Applications in Information Theory, Statistics, Economics, and Population Sciences*, jointly with Joel Cohen and Gheorghe Zbaganu. Birkhäuser is the publisher, 1998. This is hot off the presses. Could you tell us a little about it?

Kemperman: I am quite excited about this monograph. It all started in January 1992 when Joel Cohen gave a lecture at the Rutgers Statistics Seminar on a yet unpublished six-author paper. In a few days, I constructed a simpler and very different proof of one of the main results. Soon similar ideas led me to all sorts of interesting generalizations and by-products. Subsequently, that research quickly grew into a joint research project between Joel Cohen, Gheorghe Zbaganu and myself. Since the resulting joint paper became much too long, we extended it into a monograph. The second half is largely due to Gheorghe Zbaganu and generalizes many results from the first half to a more general setting.

The book looks at many different criteria to compare information channels. (A statistical experiment where the sets of possible outcomes and states of nature are finite is one example of an information channel.) The book centers on the following question: If one information channel (statistical experiment) is better than another by one of the criteria, does this imply that it is also better for some of the other criteria?

Consider a k-tuple $\mu = (\mu_1, \dots, \mu_k)$ of probability measures on the same measurable space F. Such a k-tuple might represent a statistical experiment, or a noisy channel (with input alphabet $E = \{1, \dots, k\}$ and output alphabet F), or a Markov kernel, or a stochastic matrix (if F is finite), or the distribution of k economic goods over a population or else the geographical distribution of k different species. Let ϕ: $R_+^k \to [0, \infty]$ be sublinear—more precisely, $\phi \in S_k$ meaning that ϕ is convex and lower semicontinuous such that $\phi(1, \dots, 1) = 0$ and $\phi(\lambda x) = \lambda\phi(x)$, for all $\lambda \in R_+$. We next define a quantity $H_\phi(\mu)$ which roughly measures how far apart the measures $\mu_1, \dots, \mu_k$ are. If F is a discrete space, then $H_\phi(\mu)$ is defined as the sum of $\phi(\mu_1(x), \dots, \mu_k(x))$ over all $x \in F$. Analogously for the general case. If μ is identified with the Markov kernel A, then we also write $H_\phi(A) = H_\phi(\mu)$.

Always $H_\phi(\mu) \ge 0$, with equality if all the μ_r are equal. If ϕ is "essentially" strictly convex at $(1, \dots, 1)$, then $H_\phi(\mu) > 0$ in all other cases. For example, if $k = 2$, then $H_\phi(\mu)$ reduces to the total variation distance or the entropy (Kullback–Leibler) distance or the Hellinger distance, according as $\phi(s, t) = |s - t|$ or $\phi(s, t) = s \log s/t$ or $\phi(s, t) = (\sqrt{s} - \sqrt{t})^2$, respectively.

Consider a Markov kernel $A = A(x, U)$, $x \in E$,

$U \subset F$, between measurable spaces E and F. It transforms the probability measure ν on E into the probability measure νA on F, $r = 1, \dots, k$. It is easily seen that $H_\phi(\mu A) \le H_\phi(\mu)$. Among other things, we studied the best constants $\eta_\phi(A)$ $\phi \in S_k$, and $c_k(A)$ such that $H_\phi(\mu A) \le \eta_\phi(A) H_\phi(\mu)$ for all μ and $H_\phi(\mu A) \le c_k(A) H_\phi(\mu)$, for all μ and all $\phi \in S_k$. Thus $0 \le \eta_\phi(A) \le c_k(A) \le 1$ for all $\phi \in S_k$. For instance, $c_2(A)$ is precisely the Dobrushin ergodic coefficient.

The book next introduces some 10 different partial orderings $A < B$ among Markov kernels A and B assumed to have the same finite input alphabet $E = \{1, \dots, k\}$. For most corresponding pairs $<_1$ and $<_2$, we were able to determine whether or not $A <_1 B$ always implies $A <_2 B$. The kernels A and B can be identified with k-tuples μ' and μ'' of probability measures (on F_A and F_B, respectively). For instance, $A < B$ might mean that $H_\phi(A) = H_\phi(\mu') \le H_\phi(\mu'') = H_\phi(B)$, where $\phi \in S_k$ is fixed. Each of the above partial orderings $A < B$ says that, in some sense, channel B is more noisy than channel A. That is, the k members of μ'' are, in a certain sense, mutually farther apart than the k members of μ'. Here, μ', μ'' might represent a pair of statistical experiments having the same parameter space $E = \{1, \dots, k\}$.

Naus: You have written many papers on moment problems. Could you tell about some of your recent research in this area?

Kemperman: The theory of moments is a somewhat ill-defined field that lies across many other fields. Here, one is usually confronted by a set of random variables (often in the form of a stochastic process) that satisfies prescribed conditions. The first question is then whether such a set exists at all. If so, then a related problem is typically to determine the precise range of possible values of an associated probability or expected value.

A good illustration might be a paper that I presented at a 1996 Prague conference on distributions with given marginals. Consider dependent random variables $X_1, \dots, X_n$ all having the same known d.f. F. Let $X(1{:}n) \le \cdots \le X(n{:}n)$ denote the corresponding order statistics. In the very special case that the X_j are independent, it is an easy matter to calculate any moment $E_\phi(X(s{:}n))$. On the other hand, if there is no further restriction at all, then the range of possible values $E_\phi(X(s{:}n))$ tends to be large, even when n is large. The precise range of possible values $E_\phi(X(s{:}n))$ was determined by Rychlik (1992) and independently by Garaux and Cascuel (1992). Here, and from now on, we assume that ϕ is increasing.

I became interested in the natural intermediary case where one assumes a little more, namely, that each k-tuple among $X_1, \dots, X_n$ is i.i.d. Here, k is fixed, such as $k = 2$ or 3. In the Prague paper, I showed how to calculate the smallest possible and largest possible values of $P(X(s_1{:}n) \le x_1, \dots, X(s_r{:}n) \le x_r)$. Here, $1 \le s_1 < \cdots < s_r \le n$ and $x_1, \dots, x_r$ are given. By integrating such a sharp bound on $P(X(s{:}n) \le x)$, one easily obtains (not necessarily sharp) lower and upper bounds on $E_\phi(X(s{:}n))$. Already in the case $k = 2$, it turns out that, for large n, the latter upper and lower bounds tend to be very close to the value of $E_\phi(X(s{:}n))$ in the independent case. One moral is that, in a certain sense, the pairwise independent case is not all that far away from the fully independent case.

TRAINING AND TEACHING

Naus: At Rutgers you taught both mathematics and probability courses. What courses do you like to teach?

Kemperman: I prefer to teach different courses at different times, because from each course you learn a lot. At Purdue, Rochester and Rutgers, at least half of the teaching load is at the undergraduate level. At the junior–senior level I taught not just probability, stochastic processes and statistical analysis, but also number theory, complex variables, advanced calculus, mathematical biology, discrete mathematics and linear programming. At the graduate level, measure theory, functional analysis and advanced courses in probability, stochastic processes and complex variables. I also taught an occasional topics course, such as asymptotic expansions, moment theory, calculus of variations and optimal control theory.

Naus: What are your views on the training of statisticians and probabilists? You have seen the European and American systems.

Kemperman: As a rough approximation, most European statisticians and probabilists started out with a pure mathematics–physics curriculum. Until about the Master's degree, that is, during about the first four years after high school, they typically take only mathematics or physics courses, and nothing else. Courses such as advanced calculus, algebra, topology, measure theory and functional analysis tend to be compulsory. In addition, there is room for a few topics courses, which generally include introductory courses in probability and statistics. The specialization itself starts in the fifth year and often leads to a Ph.D. degree. At least in probability and theoretical statistics, such a training through mathematics seems ideal. You need a lot of equipment to do good stuff.

On the other hand, if you require too much mathematics, the end product will be a research-type person. Also, by going very deep in selected areas, broadness may suffer. Thus the mathematical training described above may be less ideal for applied statisticians.

The same problem holds true in the States, where the training is already more practical. This reminds me of a young physicist from Holland who many years ago enrolled in a Ph.D. program in physics somewhere in the States and then twice failed to pass the Ph.D. qualifying exam. It turned out that most of his physics courses in Holland had been rather theoretical. There were many formulae but he hardly ever put any numbers in them, while, at his qualifying exam, there were many questions such as: you have a gun with such and such dimensions, so much powder, etc. But I understand that the situation in Holland has since changed considerably.

THE FUTURE

Naus: Do you have any travel plans?

Kemperman: A year ago, I made a trip with my daughter to Egypt and Israel. Unfortunately, as you know, Wilna, my dearest and wonderful wife, passed away in 1995. I also attended recent meetings in Antwerp, Vienna, Prague, Szeged and Berlin. There are several other countries I would like to visit, such as China and Indonesia. But most of the time I enjoy doing mathematics, at home or in my office. I have five children, three daughters-in-law and six grandchildren and enjoy seeing them. Once or twice a year I am in Holland to see my family out there.

Naus: These children and grandchildren are the best statistic of all.

Kemperman: Absolutely.

REFERENCES

COHEN, J. E., KEMPERMAN, J. H. B. and ZBAGANU, GH. (1998). *Comparisons of Stochastic Matrices with Applications in Information Theory, Statistics, Economics, and Population Sciences.* Birkhäuser, Boston.

KEMPERMAN, J. H. B. (1956). Generalized tolerance limits. *Ann. Math. Statist.* **27** 180–186.

KEMPERMAN, J. H. B. (1957). A general functional equation. *Trans. Amer. Math. Soc.* **86** 28–56.

KEMPERMAN, J. H. B. (1960). On small sumsets in an abelian group. *Acta Math.* **103** 63–88.

KEMPERMAN, J. H. B. (1961). *The Passage Problem for a Stationary Markov Chain.* Univ. Chicago Press.

KEMPERMAN, J. H. B. (1964). On products of sets in a locally compact group. *Fund. Math.* **56** 51–68.

KEMPERMAN, J. H. B. (1996). Bounding the moments of an order statistic when each k-tuple is independent. In *Proceedings of Conference on Distributions with Given Marginals and Moment Problems* (V. Benes and J. Stepan, eds.) 291–304. Kluwer Dordrecht.

GARAUX, G. and CASCUEL, O. (1992). Bounds on distribution functions of order statistics for dependent variates. *Statist. Probab. Lett.* **14** 103–105.

MANN, H. B. and WHITNEY, D. R. W. (1947). On a test of whether one of two random variables is stochastically larger than the other. *Ann. Math. Statist.* **18** 50–60.

RYCHLIK, T. (1992). Stochastically extremal distributions of order statistics for dependent samples, *Statist. Probab. Lett.* **13** 337–341.

VAN DER CORPUT, J. G. and KEMPERMAN, J. H. B. (1949). The second pearl of the theory of numbers I, II, III. *Indag. Math.* **11** 226–234, 277–285, 325–335.