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A Conversation with Erich L. Lehmann

Morris H. DeGroot

Erich L. Lehmann was born in Strasbourg, France, on November 20, 1917, and came to the United States in 1940. He received an M.A. in mathematics from the University of California, Berkeley, in 1942 and a Ph.D. in mathematics from the same university in 1946. He has been a faculty member there since that time. He was the Editor of *The Annals of Mathematical Statistics* from 1953–1955, and President of the Institute of Mathematical Statistics in 1961. He was awarded a Guggenheim Fellowship in 1955, 1966, and 1980. He was elected to membership in the American Academy of Arts and Sciences in 1975 and to membership in the National Academy of Sciences in 1978. In February 1985 he received an honorary doctorate from the University of Leiden.

The following conversation took place in his home in Berkeley one afternoon in October 1984.

"THE ONE THING IN WHICH I WAS REALLY INTERESTED WAS GERMAN LITERATURE"

DeGroot: Tell me how you got interested in statistics and how you came to the field of statistics.

Lehmann: It's a longish story because I have to start way back. I was raised in Germany and I am of Jewish ancestry. In 1933 when the Nazis came into power, my father decided that we had better leave. That was very early April 1933. After some wandering, because I had a brother who was dying and needed to be cared for first, we settled in Switzerland and I went to high school there. At the beginning of my senior year my father asked me "What do you plan to do after you graduate from high school?" The answer was totally obvious. He knew what I was going to say because the one thing in which I was really interested was German literature. Poetry, novels, short stories—I was reading, I was writing. You know the way it is at 17 or 18. That was what excited me, and that was what I wanted to do with my life. So I said, "Well, of course, study German literature." And he said, "I don't know if that's such a good idea because you can't go to Germany, and studying German literature outside of Germany doesn't make very much sense. You've always had an interest in mathematics. Why don't you study mathematics." This might seem hard for somebody raised in this country to believe, but I said, "Well, if you think, so, o.k." [Laughs] It didn't even cause me terrible pangs. I don't know whether I really believed it or not, but anyway I started supposedly studying mathematics in Zurich.

But you know how European universities are. There are no exams or anything, and nobody controls what you do. So I actually went to lectures in literature and music and history and things like that. I occasionally went to one of the calculus courses, but I really didn't do very much. After a year and a half or two years of that, it looked very much like war. This was 1938, and my father was worried. There was a considerable possibility that the Germans would march through Switzerland, which was not a very comfortable position to be in. So he suggested that I go to England. I got enrolled in Trinity College, Cambridge, and there I really had to start studying more seriously. But there was another very bad obstacle. In those days in Cambridge, when you studied mathematics you also had to study physics and astronomy in at least 50% of your program. That was what the schedule covered.

DeGroot: There was no combination of mathematics with other disciplines?

Lehmann: No, there was a fixed curriculum and that's what you took. Physics has always been my worst subject in school. I *hated* it. I had the same teacher for mathematics and physics and he said, "You know, if I didn't see the results of your exams, I wouldn't believe it." I was always the best student in mathematics and I was among the very worst students in physics. Therefore, at Cambridge they told me that I'd better start concentrating on the physics because the mathematics I could sort of take on the wing. The result was I didn't do terribly well in either. I spent all my time studying something that I did not enjoy at all.

DeGroot: Was it the physics laboratory that you didn't like?

Lehmann: No, it was the mechanics and electricity and that kind of stuff. I guess they didn't call it physics, they called it applied mathematics. I really disliked it intensely. And then somehow at one point in 1940 the idea of coming to the United States came up. By then the war had started in Europe. For some reason I had never thought about coming to the United States as a serious possibility, partly because I hadn't realized that I could come in on the French quota.

DeGroot: How did that happen?

Lehmann: Well, I was born during the first World War in Strasbourg. At the time, that was part of Germany and my father was stationed there as an officer on the German general staff. He was drafted, essentially, like all German males were. So I was born there, and then after 2 months we moved back to Frankfurt where my family had lived before. But because of the fact that Strasbourg is in France today, I was able to come in on the French quota. So in 1940 I came to this country, and I arrived in New York. The only two universities I had ever heard of in America were Harvard and Princeton, but I had a letter of recommendation to Courant.

DeGroot: Had you applied to any of these places in advance?

Lehmann: No, I hadn't applied to anything. I just came. It was very hard to get here because of visa problems and all kinds of problems. But somehow I managed. My mother's closest girlhood friend was the wife of Edmund Landau, the number theorist. She was a close friend of Courant, so when my mother asked her if she had any suggestions, she said she would drop Courant a note. So I got to see Courant and I said, "Where should I study? I want to study mathematics in this country." And he said, "First tell me. Do you want to live in New York or in the United States of America?" [Laughs] There were all these ethnic pockets and people could live as German refugees or whatever, without Americanization, if they stayed in New York. I said I had no particular interest in New York. So he said "Well, why don't you go to Berkeley, the University of California. It's up and coming. It's a young university and it will be a very good place, and I think you'll like it." An *amazing* piece of advice. I said to Courant, "Wonderful, don't tell me anything else." California. I didn't know about Northern California and Southern California. I had visions of a Garden of Eden with palm trees and figs and so on. What could be better. So I came here at the end of 1940 and started studying mathematics.

I didn't have a B.A., which was a little peculiar, and so I was a probationary graduate student. I was allowed to do this because of Evans, who was the chairman of the Math Department—a very nice man of whom I became very fond. He felt that students at European universities were about 2 years ahead of American students at the same level, because the high schools covered so much more. So he thought I could roughly have the equivalent of a B.A. and he was willing to give me this probationary graduate student appointment. After half a year, I got a teaching assistantship and everything was wonderful. And then in late '41 or late '42, he called me in and said, "You know, with this war, pure mathematics isn't very useful right now. I think you should do something that's more useful to the war effort." I said, "Well, how do I do that?" He said, "There are two possibilities. Either you go in the direction of physics—that's very useful—or you go in the direction of statistics." Well, the answer to me was clear. I was not going into physics again. I had just escaped. I didn't know anything about statistics—I had never heard of it—but if I should do one or the other of these two things . . .

This was again a push from the outside, you see; nothing from inside. If that's what I was supposed to do, all right. In German education you have this tremendous respect for authority. I *still* find it very hard to shake it off. So I said, "All right, I'll give statistics a try." He said, "Well, see Professor Neyman." I saw Neyman and he enrolled me in courses. After about a year, I decided this isn't for me. I don't like this one bit. This isn't really mathematics. This is *messy* stuff. Just writing down that normal density they were always talking about was messy; it was practically impossible to remember, it was so complicated. Number theory was what I liked, prime numbers and things like that.

DeGroot: Were these statistics courses particularly applied?

Lehmann: No, no, they were not applied. It was just the kind of mathematics that I didn't like in connection with physics in the first place. I decided that despite Evans and Neyman, I was going to get out of it again and go back and work with Tarski, who did the kind of mathematics that I really enjoyed. But it took me a little time to get up my courage to face these two rather formidable people and tell them I wasn't going to do what they wanted me to do. And before I had a chance to do that, Neyman called me in and said, "I'm in an awkward position. Dorothy Bernstein (whom Neyman had hired half a year before as a probabilist) has just come to me and said she doesn't like the kind of stuff that we do because it's not really nice mathematics. She wants to leave and I'm going to be short-handed. Would you be interested in teaching statistics?" And there were overtones, I don't know how strong, that it might develop into a more permanent position if things worked out. Here I was, a not-very-far-along graduate student—I didn't even have a thesis topic yet—a foreigner with no backer, no connections, no nothing, and somebody offers me what comes close to being a job. I said to Neyman, "That sounds wounderful."

DeGroot: How long had you been at Berkeley at that point?

Lehmann: About 2 years. So three times: First, German literature—my father says do mathematics. Second, I try to do mathematics and Evans says do something applied. The third time, I want to go back to pure mathematics and Neyman says here's a job for you. I gave up. [Laughs] One of the things that I disliked about physics originally, and about statistics when I wanted to get out of it, was the applied flavor, their connection with the real world, instead of their being this ideal abstract stuff. I always had the feeling that whatever abilities I had were more in the abstract direction. But the curious thing is that over the years, I have gotten to like the applied aspect of statistics. It hasn't gotten so far that I actually do applied work. Julie [his wife Juliet Shaffer] calls me an "armchair" applied statistician. [Laughs] But I like to think about statistics in connection with real situations, not totally in the abstract. So my career in statistics has actually worked out better than I had any reason to expect. I think you will find generally that in my generation everybody came to statistics in a peculiar way, because the subject didn't really exist.

"I HAD A TRIPLE OF Ph.D. SUPERVISORS"

DeGroot: Did your study of statistics prove to be helpful in the war effort?

Lehmann: No, not at all. First, Neyman put me on a calculator, a Monroe or Friden, that sort of machine. They worked on very complicated problems and I hadn't the faintest notion of what it was all about. I must have totally repressed that. I just don't remember anything about it. It has always been the kind of thing that I disliked more than practically anything. I was very inaccurate, I would make mistakes, and I can't imagine that I could have turned up anything that was of the slightest use. It didn't last very long because one day again I was called in and was told that they were forming operations research groups and they needed somebody, and they thought maybe I would be interested. This was again one of those times where you really couldn't say no.

I had no idea what was involved. I couldn't imagine that it could involve much statistics because I didn't know any. I had had a year or a year-and-a-half course, or something like that. But I did go. I had no idea where we were going until we got to San Francisco, and there we were told we were going to Guam. I was not sent to the European theater of operations because of my European background. They were worried that I might have divided sympathies. How they could imagine that, I don't know; but these things happen. So I spent a year in Guam sharing a tent with Joe Hodges and George Nicholson.

I actually did mostly photo interpretation. What I was supposed to do, Jack Youden had started with the Eighth Air Force in England: bombing accuracy studies, particularly using a record on the accuracies to pit the various groups against each other, and so on. You know, it's hard to imagine that people who fly and are in danger of their lives daily would worry about which group has achieved the best record. But that was the kind of thing I was supposed to do. Anyway, the first requirement was to keep pretty good records. So that meant that you studied the photographs of each bombing run and plotted the impacts. Sometimes you couldn't see them, but you could extrapolate and then keep records from that. That's really what I mainly did for a year. Not activity at a very high intellectual level.

There were some statistical problems. Joe and I were once asked what was potentially a very important question, but I don't think that anything we said had any impact on it: The B29's had been flying at substantial altitudes and the bombing accuracy was not very good. The question was to determine by extrapolation what would happen in terms of the greater losses and the gain in accuracy if they flew somewhat lower. But then what they actually did was fly at an altitude that was very, very low; these conditions were so totally different, there was no possibility of extrapolating to them.

DeGroot: Did Hodges go with you from Berkeley at that time?

Lehmann: No. He had been in Berkeley, and we knew each other quite well. But he left a little before me and said he couldn't tell me where he was going; it was very secret stuff. We met in Colorado Springs at the training session. [Laughs]

DeGroot: Did you return to Berkeley after the war?

Lehmann: Yes, I returned when the Japanese war ended. I can tell you amusing stories about my military service, but I don't think that's really what you wanted to talk about. So I came back in August 1945. And then I got a thesis topic, but it didn't work out. That is to say, by the time I had the results and was about to write them up, I found out that it was all in Markov and other places. It was some aspect of the moment problem. So I had to get a new problem and Hsu gave me a problem that he had already started a little bit on, and that was a very helpful thing. Then Hsu left for North Carolina, and subsequently went back to China. Then Neyman left and went to Greece to help supervise the Greek elections. Have you read the Neyman book? [Constance Reid, *Neyman—from Life*, Springer-Verlag, New York, 1982].

DeGroot: Yes, I have.

Lehmann: Then you know about these things. And so I was left a little stranded, but he arranged for Polya to supervise me. So I had a triple of Ph.D. supervisors. The problem was given by Hsu; most of the thing was supervised by Polya at Stanford, in the sense that I went up there once a week or once every 2 weeks and told him what I had done; and then Neyman came back just in time for the final exam.

DeGroot: What was the topic of your thesis?

Harald Cramér, Erich Lehmann, and George Polya, 1951

Lehmann: What today we would call uniformly most powerful unbiased tests, although they weren't called that then—Type A regions or Type A_1 regions, that kind of thing. Polya did something for me which I have always remembered. One day he said, "Well, I think that's enough. Write it up into a nice form, and that will be your thesis." And I said, "Really? It doesn't seem to me that we have done all the interesting things." And he said "Well, that's my problem not yours. You can do the good work after you get your degree. It's enough for a degree." Since then, that's been my attitude toward my own students. And then I was in fact kept on at Berkeley, so I have been in Berkeley since 1940.

"NEYMAN GOT RUMORS THAT I WAS NOT FOLLOWING THE SCRIPT CLOSELY ENOUGH"

DeGroot: You've had Guggenheim Fellowships on three different occasions. That must be close to a record. Tell me about the projects you worked on at those times.

Lehmann: I think they were basically all trying to work on the books. [*Testing Statistical Hypotheses*, John Wiley and Sons, New York, 1959; *Theory of Point Estimation*, John Wiley and Sons, New York, 1983] The books have been sort of an interesting thing in my life. They grew out of notes that Colin Blyth took in the late 1940s. It is surprising to me today that a year after my Ph.D. I basically had the pattern that I've worked on for the last 40 years. Although of course the books today look a lot different from those notes, the basic organization and the basic approach has been kept.

DeGroot: Were those some of the first courses that you taught after your Ph.D.?

Lehmann: I think they were, yes. I think they were two summer courses. I don't know whether I also taught them as regular graduate courses. And then I got into this trouble which Constance Reid describes. Neyman got rumors that I was not following the script closely enough. It was pretty painful. I still feel that of all of Neyman's students, I probably am the one who followed his approach most closely. I developed it a little bit further, but I didn't really do anything terribly innovative. And yet he found even these small innovations so bothersome.

DeGroot: For example?

Lehmann: Well, I used invariance, and that he hadn't used. I don't know just what bothered him. We never had a real conversation about it. But he said, "I've heard that you're not doing it the way I did it. I understand you're writing a book. I want to see the manuscript." I said, "Well, I gave you the set of notes that Blyth took, and that's really the best thing I have." I considered it quite bothersome that he would ask me for the manuscript. And then he barred me from teaching the course. I never taught it again until he resigned the chairmanship.

DeGroot: This was the course in statistical theory?

Lehmann: It still is, and I still teach it quite often. I'm teaching it this semester. But it seemed to me out of character for Neyman to do that because he was a very generous man in many ways and a very liberal person. But we had lots of troubles.

DeGroot: It certainly was ironic for him to object on those grounds, given his own history with Fisher.

Lehmann: It's very strange. It went from Karl Pearson to Fisher, from Fisher to Neyman, and in a different way, from Neyman to me. I must say though that despite the fact that we had these conflicts, he was always tremendously good in furthering my career. He always got me promotions, he got me better salary increases, he got me invited to conferences, and all of that kind of thing. That's very different from Karl Pearson who tried to squash Fisher, tried to prevent him from getting a job, tried to prevent him from publishing. I don't know how generous Fisher was in these respects. So Neyman and I had our differences, but he was always very good about these things. I was very appreciative of that.

DeGroot: I gather you feel that he was a major influence on your career.

Lehmann: Well, certainly in the sense that I've worked on Neyman-Pearson theory in one aspect or another all my life.

DeGroot: Are there other people that you regard as major influences on your career?

Lehmann: Yes. When I first started teaching on the faculty, there were four of us sharing a little office with four desks. Two desks on one side and two desks on the other side forming a block with just enough space that one could walk around. That led to a lot of joint work. Joe Hodges, Evelyn Fix, Charles Stein, and I. So I got to learn a lot from Charles about invariance among other things. We wrote quite a number of papers together. He was a very important influence on me. You know, all the people with whom you collaborate are important influences. Joe Hodges, with whom I collaborated for a long, long time, was important. We wrote many papers together and a book; I also collaborated with Henry Scheffé, and learned a lot from both of them. But Neyman really set me on the course, as I said earlier. In a way I rebelled against it, but in a way I also accepted it because I really stayed pretty much on the course.

Here's another example of how difficult Neyman found it to accept something that was a little different. The first thing I wrote was as a graduate student, and there was a little note which is in the *Annals* on what is today called minimal complete families. ["On families of admissible tests," *Ann. Math. Statist.* **18** (1947), 97–105] I set out the concept and applied it to a very simple hypothesis-testing situation. I wrote it up—it was just a few pages—and said to Neyman that I would like to publish it. He essentially said, "It's junk. Don't bother." But I sent it in to Wilks anyway. [Laughs] And this essentially gave me the friendship of Wald and Wolfowitz, because Wald seized on this and it became one of the two fundamental concepts of his decision theory. Minimax is one, and complete families is the other one.

DeGroot: So your note really introduced the concept of minimal complete families?

Lehmann: Yes, quite explicitly, without the terminology and without having any realization that this could be much broadened, that there was such a thing as decision theory, that there was a connection with Bayes solutions, or any of that. This has always interested me in connection with priority assignments. I think very many things come about in two stages. First, somebody has a germ of an idea in a rather specific context and perhaps uses it to solve a specific problem, but without developing any of its consequences or seeing any of its logic or contextual possibilities. Then somebody else comes along and develops it into a general theory. Now I've been on both sides of this. In general, I'm more on the system building side than on the initiating side. But on this particular topic I was on the initiating, and that's rare.

With the concept of the completeness of a sufficient statistic I was on the other side, because that idea is basically apparent in some of Rao's papers, you can find it in one of the Wolfowitz papers, and there are one or two other places where you can find it. In the Rao-Blackwell theorem, if the estimator is unique then you're finished. So it had already been applied. I think the main contribution of these big *Sankhyā* papers by Henry (Scheffé) and me is that we isolated this concept and then explored its general theory. ["Completeness, similar regions, and unbiased estimation, Parts I and II" *Sankhyā*, **10** (1950), 305–340, and **15** (1955), 219–236]. In this case other people had the idea and we expanded it and made a big thing of it, while with the notion of complete families it was the other way around. It seems to me that typically it's the second people who get the credit, I think a little unfairly.

FAVORITE PAPERS

DeGroot: You have published about 90 papers in statistics, as well as your famous books. Let's get to the books in a moment. Are there some favorites of yours among the papers that you liked doing or you think were particularly influential?

Lehmann: I enjoyed doing them all. You know, it's fun. But yes, there are probably some that I like better than others. The .864 paper with Joe (Hodges) I like very much. ["The efficiency of some nonparametric competitors of the t test," *Ann. Math. Statist.* **27** (1956), 324–335]. I think that was influential in the sense that it dispelled the belief that while nonparametric techniques are very convenient because you don't have many assumptions, they have so little power that they are no good.

DeGroot: Was that really the first paper that did an explicit efficiency calculation?

Lehmann: No, Pitman was the first to do explicit calculations, and those are in his mimeographed nonparametric notes which students at Columbia published. There he calculated it for the normal, $3/\pi$. That was already very surprising that you would get such high efficiency in the normal case. But that for *all* possible distributions you would get at least .864, that I think is really a great surprise. I also had a paper on the power of nonparametric rank tests which has played a rather peculiar role in the literature. ["The power of rank tests," *Ann. Math. Statist.* **24** (1953), 23–43] What I noted there was that the difficulty of calculating power for rank tests against the usual types of alternatives is that the power depends on the following: Suppose you have a two-sample problem. One distribution is F and the other is G. Then the power depends on the way G is a function of F. So if $G = g(F)$, then it depends on this function g and doesn't depend on F at all. This function g is very complicated for the usual types of problems. So I said well, one way of getting approximate power is to approximate the true g by, say, a polynomial for which it is very easy to calculate the exact power. And the very simplest is of course if g is just a power.

Erich Lehmann, 1968

DeGroot: The Lehmann alternatives . . .

Lehmann: Yes, that's gotten the term the Lehmann alternatives. Because it is so very convenient to calculate, it has been used quite a bit. That is a paper which Neyman for some reason appreciated. I remember that I was at Columbia when I did that, and I wanted to visit Berkeley. He invited me to give a seminar talk and he was very pleased with that. It's the only paper that I wrote that I remember he was pleased with.

DeGroot: When were you at Columbia?

Lehmann: In 1950. You see there was a policy really in Berkeley that you didn't keep your own students. In statistics that policy was practically impossible because there were so few people being trained. Hotelling was training, Wilks was training, Neyman was training, and for theoretical statistics I think that was about it in the 1940's. So unless you were willing to keep your own students, you couldn't build up. So Neyman kept a lot of his students—Hodges, myself, Le Cam—But there was sort of an understanding that we should leave at one point or another for a year or two to see another place and get more experience. I found that the ideal time to do that was when that horrible loyalty oath was introduced here, which caused Charles (Stein) to leave. In the beginning I was here and the atmosphere was just so poisoned that you couldn't work. You just went to faculty meetings of one kind or another practically every day. Some people were not speaking to other people. It was just awful. So I thought it was a good time to take the opportunity of leaving. I arranged for one year away, 1950–1951, and I spent one semester at Columbia and one semester at Princeton. And the second year it was still going on and I didn't want to go back, so I went to Stanford and spent a year there. Then after that I came back here and settled down.

DeGroot: Was that period at Berkeley at all comparable, in terms of the political turmoil, to the 1960's and the period of the Vietnam War?

Lehmann: Well, the students, of course, weren't involved and so there were no mass movements.

DeGroot: Do you recall the faculty, for example, being divided during the Vietnam period?

Lehmann: Oh, the faculty certainly was divided. I had a very funny experience. I had always refused to chair the department for a variety of reasons. And then in 1970, Betty (Scott) was department chair and she said she was going to take off in the spring, and she had run through all the people whom she could reasonably ask to do this kind of thing, and would I be acting chair for this one semester. She said it wouldn't involve any work because she had prepared everything, and all that would happen was that from time to time the administrative assistant would ask me to sign something. Well, that was the Cambodia spring of 1970, when the university exploded. When they stopped having graduation ceremonies, so I suddenly was saddled with organizing a departmental graduation ceremony. When we had department meetings around the clock, with the faculty varying from the people who said they would not teach on campus to the people who said they wanted it in their records that they taught each class at the assigned time at the assigned place. [Laughs] It was a pretty wild period.

DeGroot: So you paid your dues for having avoided it earlier.

Lehmann: I paid my dues, but I found that I could do it and I was willing to take it on not too long after that on a regular basis for 3 years. But you asked me about papers and books. I want to mention a third category because it's one of the things that I enjoy the most and also it seems to me one of the most important ones, and that is Ph.D. students. I've had over 50 Ph.D. students who have spread all over the world, many of whom continue to be close friends. I occasionally collaborate with one or the other now. I have found that of all the teaching activities, the one that I enjoy the most is working one on one and getting to know people as people. Also, because we have this very international student body, I've had students from Norway, Switzerland, Germany, Thailand, Nigeria, Singapore, Israel, you name it. I have found that to be really one of the nicest and most appealing aspects of university teaching. But both the book writing and the supervision of students are, in my opinion, vastly underrated by university administrators and by organizations like the National Science Foundation. We don't get teaching credit for supervising Ph.D. students. On one occasion, I simultaneously had 10 Ph.D. students; it just happened to build up like that. Now I have usually one or two. And I didn't get any credit for that. It's an enormously time- and energy-consuming activity, and it's an extension of your research activities.

On the other hand, book writing is not considered research in our profession, which I think is totally crazy. I will admit that writing an elementary text like Joe (Hodges) and I did [*Basic Concepts in Probability and Statistics*, 2nd ed., Holden-Day, San Francisco, 1970] is not research, and yet there could be considerable intellectual activity even in that, as you very well know. But an enormous amount of research goes into writing advanced books. You know, in the humanities that *is* research and there book writing is the thing that you have to do if you want to be recognized or advanced. But I was told at one point by an NSF program director in statistics that if I mentioned that I was working on the *Estimation* book in my research proposal, I wouldn't get a grant. It's totally crazy because I think a book like that can have a much greater impact than just writing another paper.

DeGroot: There's no question that your books have had much impact on the field and serve a vital research purpose.

Lehmann: I have never understood why the mathematical sciences take this dim view of book writing. To try to synthesize things in some way seems to me a very important activity.

"I WANTED TO BECOME A WRITER"

DeGroot: You tell me that you are now involved in preparing a new edition of the *Testing* book. Is that occupying much of your time?

Lehmann: Yes, most of my time, whatever free time I have. Actually, I didn't realize it until fairly recently, but in a way the book writing ties in with what I originally wanted to do. I wanted to become a writer. It's true, I thought of a different kind of writing from *this* ... And, of course, in many ways they are very different, but they also have many similarities.

DeGroot: There is certainly good writing and bad writing in technical fields just as in other fields. And your books are beautifully written.

Lehmann: Well, trying to write a book like that has many points of contact with any other kind of writing. And so in this very back door way, I have finally realized that I am doing something that was my life's ambition when I started out and that I thought I had given up. So I do it with much happier feelings today than when I originally did it.

DeGroot: What kind of changes can we look for in the new edition?

Lehmann: First of all there is of course an enormous amount of updating. The book is 25 years old, so a lot has happened since then. I have practically no robustness considerations in the book, and in those days I didn't really understand that optimality wasn't everything. [Laughs] Today I see that optimality is only one side of a rather complicated picture and so I have a lot of robustness in it now. I struggled with whether to put in Bayesian things. I think I will not, partially because Bayesians don't believe in hypothesis testing anyway. [Laughs] Why worry about it. But there is another criticism which I think in some sense is the most legitimate if you take the Neyman-Pearson theory more or less on its own terms. There is a very legitimate attack on testing that has been mounted by several people. John Pratt has been very effective that way. That is, you sometimes get totally unreasonable things, like optimal confidence sets which turn out to be intervals for σ^2 which go from -100 to -7. It's a little embarrassing to say the least. [Laughs] There are other related things which really raise the question, in the Neyman-Pearson theory, "What is the relevant frame of reference?" That brings up the question of determining the proper conditioning, so I will have a last chapter in the book which is a very tentative, speculative chapter on conditional inference.

DeGroot: Will this be in the spirit of Kiefer's papers? [J. Kiefer, "Admissibility of conditional confidence procedures," *Ann. Statist.* **4** (1976), 836–865; and "Conditional confidence statements and confidence estimators" (with discussion), *J. Amer. Statist. Assoc.* **72** (1977), 789–827].

Lehmann: Well, Kiefer's ideas will be mentioned at the very end. He died too early to push them all the way through. With regard to conditioning on ancillaries, Cox, I think, is the person who saw this all so clearly. Fisher saw everything, too, but not very clearly. You can find a lot of it there. And then there's relevant subsets. I don't know whether you follow that literature. It actually has some Bayesian connections. That's a different type of conditioning. When conditioning on ancillaries, the conditioning event has a known probability. With relevant subsets it has an unknown probability. So the unknown parameter is involved not only in the conditional distribution, but also in the probability of the conditioning event. Which makes it more suspect, and some people don't believe in it. There is an Australian, G. K. Robinson, who has done some very interesting work on this. And Kiefer brought some new ideas. So I will give an introduction to that area, with a paragraph or two summarizing Kiefer's approach. That will be the most novel feature, I think.

DeGroot: So some of your revisions have been carried out with an eye on John Pratt's review of the first edition? [*J. Amer. Statist. Assoc.* **56** (1961), 163–167]

Lehmann: Right. [Laughs]

DeGroot: How is the *Estimation* book doing?

Lehmann: I think that it is being used. It's been out about a year and a half, and it's just going into the third printing. The first two printings together were 3000 copies. So it has sold about 3000 copies in a year and a half. Those are not huge numbers, and as you know you will never get rich on it. But on the other hand, for a book at that level I don't think that's too bad. At the moment the book suffers from the fact that the *Estimation* and the *Testing* books don't fit together. The *Testing* book is just obsolete. I hope that when the two books are together they will really form a more or less cohesive, natural one-year course at the graduate level.

DeGroot: That will be wonderful. You are a victim of having scooped yourself in a sense, since the original notes were used by so many other writers before your own books came out. So you have to compete with yourself now.

What courses do you like to teach? You tell me that you still enjoy teaching the statistical inference course.

Lehmann: Yes, I think that's the one I enjoy most. Then I typically teach a nonparametric course at some level or other. I have a book on that topic.

Erich Lehmann, Elizabeth Scott, and Jerzy Neyman

[*Nonparametrics: Statistical Methods Based on Ranks*, Holden-Day, San Francisco, 1975] I try to teach only courses where I can use my own books. Then I don't get into that horrible dilemma: Do you fight the text or do you give in? Nobody else's text ever satisfies anybody who teaches. I teach sometimes from *Basic Concepts*; last year I taught a freshman course which, if I don't do it too often, I quite enjoy. And then I'm developing a new course. I call it "Asymptotics for Applied Statistics," or something like that. It's for people who want to understand the applicational side of statistics; for example, robustness. Why does the t test work reasonably well while the F test for variances is absolutely disastrous? You cannot understand that unless you understand a little bit about asymptotics. You don't need very much. You need the central limit theorem, the theorem that people sometimes call Slutsky's theorem, and one or two other little things like that which one can explain pretty well. And there are all kinds of other asymptotic things. I've given the course twice now and I hope to give it again next year. I'll give a course which requires a year of calculus, preferably a year and a half, and some exposure to probability and statistics.

DeGroot: But it's not mainly for statistics majors?

Lehmann: Well, the people at the M.A. level who cannot take the serious probability course would get something out of it. I think they all need to understand a little of this. I start out with $O(\ )$ and $o(\ )$ and Taylor's theorem, and I get applications of all of these things to statistics. I find one can explain this to people who don't have much background so that at least they understand what the intuitive ideas are with practically no proofs.

DeGroot: Is the main purpose of teaching the background that you just described to get to these robustness results? Or are there also other kinds of applied results?

Lehmann: There are all kinds of things. For example, to understand what heavy tails means, you have to understand $O(\ )$ and $o(\ )$ and rates of convergence and things like that. That's a different type of asymptotics. That's going out to infinity on the real line, rather than the sample size going to infinity. And to understand consistency. Or asymptotic relative efficiency; that's a tremendously useful concept. I do some of that so that I can discuss the efficiency of nonparametric tests relative to t tests. So there's quite a body of applications.

DeGroot: That's an interesting idea—to take a topic like asymptotic theory and make it tie in with the Master's program.

Lehmann: I don't think there's anything like that available at the moment.

"THE PENDULUM WILL SWING BACK AGAIN"

DeGroot: I would be interested in your views about the current state of statistics as a field and where you think it's going. I've heard some bleak predictions. Computer science seems to be sweeping up students and cornering the market on interesting topics, and I've heard people say they don't even see a future for statistics.

Lehmann: No I don't see it that way, but you know I'm not at the forefront of what goes on today. I'm not really a good person to look into the future. I find it interesting to see the swing of the pendulum, and at the moment we are swinging away from the kind of stuff that I was interested in. You know, Student and Fisher, and then following them, Neyman-Pearson, really brought about a revolution and created this theory of mathematical statistics which is very strongly model-oriented. I would agree with all the critics who say it's *too* strongly model-oriented; we don't know these models. So they are now interested in adaptive and nonparametric methods. They are all trying to get away from narrow models but they are still perhaps too bound by assumptions.

Well, now one of the big swings is in the direction of data analysis which, if you take it seriously, is just as silly as the model business, if you take *it* seriously. They say ideally we don't make any assumptions whatever; we just treat data on their own terms. Well, if you make believe that you have absolutely no knowledge of how the data were generated and where they come from, then you are throwing away a lot of very useful things. The big thing today is data analysis and nonparametric procedures—regression analysis where you assume nothing about the regression function and nothing about the distribution of the errors, and so on. But it seems to me clear that after a while the pendulum will partially swing back again, because this direction is chaos. If you have no way of evaluating how well you're doing and of comparing different techniques, I don't think you're going to be satisfied.

DeGroot: It used to be that the role of statistics was *precisely* to help you evaluate how you were doing.

Lehmann: Consider optimality theory, which is not very alive at the moment. I never took those optimality results very seriously. Well, when I was young I probably did take them seriously, but not for terribly long. But I think they provide what Tukey calls "bench marks" and as such they are quite useful. I was recently at a talk given by Brad Efron where he said that for one of the problems he was talking about, he was in real trouble because there was no optimal procedure with which to compare the various results. So I think this kind of approach does have some use. It's not as overriding as people maybe thought when they first worked on it. And I'm not saying that all of us will necessarily go back to optimality theory, although people even now talk about optimal convergence rates. They talk about that in computer science, too. The smallest number of steps, exponential versus polynomial times, and so on. So maybe in this slightly looser sense, people will be interested. But certainly they will be interested in looking at some kind of performance characteristics. Of course, model assumptions get very hard when you have really large data sets because every model assumption is going to be proved false if you have enough data. There is the interesting approach due to Huber where you use neighborhoods of parametric models, but it hasn't really caught on.

DeGroot: Those are nonparametric neighborhoods of parametric models?

Lehmann: Yes. I thought that was an awfully interesting idea, and I hoped that people would do a little more with it. Another area in which there has been surprisingly little progress is inference in stochastic processes. People are just beginning to come to grips with it now. Practically all of the standard theory of estimation and testing is for Euclidean spaces, where the observations are random variables, preferably i.i.d. I remember telling Wald that I thought an awfully interesting problem in sequential analysis would be to decide at which time points you are going to observe a stochastic process. This would be an extension of sequential analysis to design-type problems. He encouraged me to work on that problem but I said I didn't think I knew enough mathematics. The whole area is just wide open.

DeGroot: Was Wald at Columbia when you were there?

Lehmann: Wald died while I was at Columbia. It was a horrible thing. They wouldn't release the information. We were first approached in the department by newspapers, and everybody was terribly worried that the family would first hear it that way. We made telephone calls and sent telegrams to India trying to find out what happened, but we couldn't get contact. The government didn't want to release any information.

DeGroot: The Indian government?

Lehmann: The Indian airline and the Indian government. And I actually took over three of Wald's students. So in addition to the students that I supervised here, I had three students from Columbia that I worked with. Allan Birnbaum was one of them. They finished there while I was back at Berkeley.

DeGroot: Had you gotten to know Wald fairly well?

Lehmann: Yes. Wald had spent a year here prior to that. He finished his decision theory book here. He and Charles and Joe and I went on a hiking trip to Yosemite with all kinds of interesting events. [Laughs] So I got to know him fairly well. Wald was fun. I remember one thing he loved to do. He looked up at a mountain that we were going to climb and he estimated—he loved to estimate—that it would be a climb of 1800 feet. And then a couple of hours later, when we were on top, he would look down and say, "It was 1900 feet. A very good estimate."

DeGroot: Do you still hike?

Lehmann: Yes, Julie and I love to hike. We go up and do some parts of that loop trail in Yosemite occasionally. Two years ago we went to Europe and hiked in the Alps. It's very nice hiking out of Berkeley, except that we have both been so busy that we haven't been doing much.

"I DON'T THINK EXACT NONPARAMETRICS WILL PROVE TO BE VERY VIABLE IN THE LONG RUN"

DeGroot: What do you think have been some of the major breakthroughs in statistics and some of the major influences on the field?

Lehmann: Well, let's say since Fisher. I'm amazed how often, when I try to write something up and then look back, I find that Fisher really understood it very well. One of the most amazing examples to me is that in my estimation book I took as my two basic types of models, the exponential families and group families. And I found that there's a 1934 paper by Fisher in which he basically defines these two types of families, and for exactly the right reasons. Namely, that in one way or another, they reduce the dimensionality of the problem. In one case by having a low-dimensional sufficient statistic, and in the other case by having a high-dimensional ancillary which has a low-dimensional conditional distribution. So certainly Fisher, with the analysis of variance and the design of experiments ...

DeGroot: Maximum likelihood ...

Lehmann: Well, actually Edgeworth had quite a bit on maximum likelihood. That's a very nebulous thing. I don't know whether you read Jimmie Savage and John Pratt on that. [L. J. Savage, "On rereading R. A. Fisher," *Ann. Statist.* **4** (1976), 441–483 (edited for publication by J. W. Pratt)] The priority situation as to what Fisher did or did not know about this is nebulous. Edgeworth is impossible to read, so you can't blame anybody for not really having understood him. One doesn't really know just how much Edgeworth understood ... The 1922 paper by Fisher on sufficient statistics ... Then the Neyman-Pearson theory, which was quite precise and modeled optimality theory. Fisher had no small-sample optimality, although he was interested in large-sample optimality ... Certainly the Bayesian movement was a major

Harald Cramér, Kim Cramér, Erich Lehmann, Charles Stein, Joseph L. Hodges, and Juliet P. Shaffer, 1983

development. I guess, as far as we are concerned, Jimmie Savage is the hero of this, although he credited de Finetti. I have never gone back to read de Finetti, but I've read Jimmie quite a bit.

I think that the whole nonparametric development is interesting, but personally I don't think the exact nonparametric rank-based approach will prove to be very viable in the long run. It's too restricted. For example, you can't treat linear models in a really exact nonparametric fashion with rank type procedures. It only works in rather simple situations. The robust approach seems to work much better and it is much more general. I don't know whether you've looked at Hettmansperger's book that recently came out [T. P. Hettmansperger, *Statistical Inference Based on Ranks*, John Wiley and Sons, New York, 1984], but it does some of that. And Julie and I jointly supervised a student, David Draper, who tried to develop robust nonparametric analysis of variance and linear models types of procedures which are pretty close to being operational now. The tests of Wilcoxon type will probably continue to be around just because they are so neat. However, on the whole, there are better procedures. Recently, adaptive procedures are being developed. That goes back to Charles in 1956 or thereabouts . . . Certainly Tukey was very important in the robust direction, and I guess everybody agrees that he was also the one who revived data analysis.

DeGroot: You mentioned Charles Stein in connection with adaptive procedures. What was that connection?

Lehmann: He conjectured that, for example, in the two-sample problem you can get tests which are asymptotically fully efficient. He also gave an intuitive, heuristic argument which isn't quite rigorous but is fairly close to rigorous. Suppose you have $F(x - \xi)$ and $F(y - \eta)$, and you want to test $\xi = \eta$. You can get fully efficient tests without making any assumptions about F. Today it can be done in a more sophisticated way, but the way he did it was this: First you estimate F from part of the sample, say $n^{1/2}$ observations, and then you use the asymptotically best test for that F. And that works; asymptotically you do as well as if you knew F all along.

DeGroot: When you think about asymptotic theory, do you distinguish between asymptotic asymptotic theory and reasonable-sample-size asymptotic theory?

Lehmann: Well, the asymptotic theory that most people do consists of limit theorems. You can embed the true situation in lots of different sequences, and try to find one which gives you a good approximation. You use the limit result as an approximation. But when I write or talk about these things, I always say that you have to supplement this method with spot checks of how well it works for a number of different distributions and a number of different sample sizes. And then you have this blind faith, for which there is no analytic argument that I know of and which doesn't always hold precisely, that as you increase the sample size the approximation will improve. I mean, it could dip or go wildly, but in practice it typically doesn't. So, if you know that it works fairly well for $n = 30$ and a distribution which looks this way or that way, then you say that for $n = 50$ or 80 or 100 it will work even better.

DeGroot: That's the statistician's religion.

Lehmann: Right. You take that on faith. But I don't know whether that answers your question or not.

DeGroot: Well, I was just thinking that there are some asymptotic results that are truly very large sample results . . .

Lehmann: Yes, I think that one wants to warn against those. In fact, at first Charles didn't pursue his adaptive stuff further, and people thought it would take hundreds of thousands of observations. It turns out that (Charles) Stone has a procedure for a problem of that type where you can do quite well with 40 observations.

Well, to continue . . . Certainly Wald's decision theory was a very major step, but I think it has been disappointing in its influence. If you look at theorems about decision theory, there haven't been very many since Wald. However, it has freed people from necessarily sticking to hypothesis testing, point estimation, or confidence intervals. One of Wald's other contributions, sequential analysis, is important but it hasn't really blossomed the way I would have expected either.

DeGroot: Well, it may have been that the wartime applications were really the right ones for sequential analysis.

Lehmann: That's true. Quality control and that type of application. Now let me ask *you* something. What do you think of Jim Berger's book? [*Statistical Decision Theory: Foundations, Concepts, and Methods,* Springer-Verlag, New York, 1980].

DeGroot: I like it a lot. Especially the Bayesian parts. I think Jim would be the first to admit, especially now that he's in the process of revising the book, that despite his statement in the Preface that he is a rabid Bayesian, there is some non-Bayesian material in there.

Lehmann: Yes, that's of course what I liked. I feel that it's a very peace-making book. As a method for generating procedures, of course, anybody with my kind of persuasion likes Bayesian procedures.

DeGroot: As a technical device.

Lehmann: Well, even as a way of thinking about a problem. This would be the right procedure if you thought that this was approximately how θ behaves. But I do like the fact that he talks about risk functions.

DeGroot: Sure. He doesn't immediately integrate out over θ.

Lehmann: And that of course in a certain sense makes him a nonrabid Bayesian.

DeGroot: I've been kidding him ever since his book came out that it's not enough simply to say you're Bayesian. If you want to get into the Bayesian group, you have to prove it in your writing. And he says that he is revising the book and it is becoming more Bayesian. So you better hold on to that first edition. [Laughs]

"WHY DO PEOPLE USE .05?"

Lehmann: But Morrie, where does the movement away from narrow models leave the Bayesian? You are even worse off than I am.

DeGroot: In many ways that is true. Just as we were saying about Neyman-Pearson theory, taking the Bayesian approach strictly on its merits is the extreme case of optimality theory. In many problems it's impossible to find an optimal procedure, and where does that leave the Bayesian? I think the answer is that the Bayesian view is useful in helping to *think* about the problem.

Lehmann: No question about it. Stein estimation is a wonderful example of that. Lindley came up with a Bayesian interpretation which led essentially to the Stein-type procedure. I think that's great.

DeGroot: Nowadays we often think about large data sets and multiparameter problems. Even though Bayesians don't have nice distributions to handle them, since nobody knows how to put down prior distributions on a high-dimensional space . . .

Lehmann: But before you can even have the prior distributions, you have to have distributions with parameters on which you can put those priors.

DeGroot: Yes, you have to be able to think about parameters. What is needed and has to be developed, in the same spirit as all of statistics, is some reasonable approximation theory. Nobody is going to get a distribution that precisely represents the experimenter's or the statistician's beliefs about some complicated setup. But just like asymptotic theory in the rest of statistics, if you can't find the Bayes decision or the optimal decision with some model, you try to get a model that works reasonably well and you try to find procedures that work reasonably well.

Lehmann: Doesn't that go against Bayesian principles? I would certainly agree with what you have just said, and I would push it slightly further and say that statistical problems don't have unique answers. There are lots of different ways of formulating problems and analyzing them, and there are different aspects that one can emphasize. Personally, I very much like an eclectic point of view where you say let's try two or three different things. For example, let's try two or three different prior distributions and see how they compare and how I like what I get with them. But I thought that most Bayesians would not buy that.

DeGroot: Well, I can't speak for other Bayesians but certainly I would accept it. The idea is that I could spend much time specifying a prior that really represents my information and my beliefs, but instead I might simply try a few standard distributions. I agree with you. Given a problem I am working on, I will try various techniques. And if the answers are more or less the same then I don't have to worry about it.

Lehmann: And if they are not, then that also guides you as to how to think about the problem. You can see *why* you get different answers.

DeGroot: Especially if you are doing this work for some client who is going to be using your analysis. You can point out that different approaches, different prior distributions, or even different methods of analysis would yield different answers. So even within the Bayesian framework, it never hurts to try a few different prior distributions, a few different likelihood functions, a few different loss functions.

Lehmann: Then as far as what we would do in practice, I think that there's practically no difference between you and me and most other statisticians. The philosophy behind it may be different, but the funny thing is that it doesn't really matter when you actually do it. The same kind of question comes up in a different context. I read over and over again that hypothesis testing is dead as a door nail, that nobody does hypothesis testing. I talk to Julie and she says that in the behaviorial sciences, hypothesis testing is what they do the most. All my statistical life, I have been interested in three different types of things: testing, point estimation, and confidence-interval estimation. There is not a year that somebody doesn't tell me that two of them are total nonsense and only the third one makes sense. But which one they pick changes from year to year. [Laughs] I think these views result from the fact that different people work primarily in different areas of application. In some areas of application, one type of inference is particularly appropriate and in other areas another one is particularly appropriate.

DeGroot: That's right. There's no doubt that testing is widely used in applications. The question is, *why* is it, and should it be? I think that, in effect, we statisticians have been very successful. They have now learned this methodology, it is appealing to them, and it's being widely used.

Lehmann: It's really surprising that testing has been so successful, because it's a very involuted way of thinking.

DeGroot: It is, but it's also easy to carry out the procedures. Take the simple example of a χ^2 test. Why is it universally used? Well, it's a simple procedure. You calculate it, and you don't have to think about it.

Lehmann: That leads to an interesting topic. This whole question of standardization, with its tremendous drawbacks. There are people who have to do statistics who can't possibly understand it at a research level or at an innovative level. It is necessary for them to have more or less cut-and-dried procedures to apply. And this is not going to get any better with the computer. We now have these packages, and people will understand even less of what they are doing than they did before.

DeGroot: And they will be dealing with much more complex situations.

Lehmann: They will just push buttons and they won't have the faintest notion of what's going on. But there is another aspect to it. Why do people use .05? I always try to talk to my class about that, because it's an interesting question. It's obviously a silly thing to do. You should take into account what power you can get. And yet there are very interesting studies that show that people use testing in situations where they have so little power that they might as well forget about carrying out the experiment, because there is almost no chance of discovering the kind of effect that they are interested in. They really needed a larger sample size. But short of that, they would do better to carry out the test at a somewhat higher significance level where they would have a better balance between the two kinds of error. But besides the fact that people like to be told to do things in a fixed way, and editors like to apply a fixed rule such as not to accept a paper unless the result is significant at the .05 level, there is also the advantage that if you use procedures in a standardized way, you get used to them and you understand them at that level. You talk about the same thing and it creates a universal language in a way. People can communicate much better than they would if you didn't have that standardization. So although I am basically strongly opposed to it, I can see that there are also some positive values to it.

DeGroot: Yes, that's a very good point. To come back to your other point, that everybody criticizes two out of your three topics: It has always amazed me about statistics that we argue among ourselves about which of our basic techniques are of practical value. It seems to me that in other areas one can argue about whether a methodology is going to prove to be useful, but people would agree whether a technique is useful in practice. But in statistics, as you say, some people believe that confidence intervals are the only procedures that make any sense on practical grounds, and others think they have no practical value whatsoever. I find it kind of spooky to be in such a field.

Lehmann: After a while you get used to it. If somebody attacks one of these, I just know that next year I'm going to get one who will be on the other side.

DeGroot: Nonparametrics, your other area, suffers from the same criticism. Some people think that's the only way to go because parametric models are never correct, and as a Bayesian I would say that, strictly speaking, there's no such thing as a nonparametric problem. Maybe that's what keeps the field interesting.

Lehmann: I think it's interesting both on a technical level and on an intellectual level. There is a lot of turmoil, and the computer has changed the landscape quite considerably. As we were saying a minute ago, the problems that these packages are going to pose for the practice of statistics are enormous. I think they will make robust techniques—techniques that are not very sensitive to errors in the data—particularly important, because people may not even notice that one of the numbers is wrong by an order of magnitude. If you do a calculation by hand or on a calculator and you have 1,000,000 where you should have 100, you are likely to notice it. But if the data are just fed into the computer and all you see is a final summary number, it may be totally off without anybody noticing it if you don't worry about this. We teach statistics courses to about 5,000 students each year at Berkeley. We will have to worry about what kinds of things we should teach them so that they can make the best use of these techniques. I think it continues to be an absolutely fascinating field.

"STATISTICS TOTALLY DOMINATES MY SOCIAL LIFE"

DeGroot: What relationship do you see between your professional work and your social life? Does one influence the other?

Lehmann: My social life, and this is partly the result of being at Berkeley, is practically entirely confined to statisticans. We have so many visitors and so many interesting people coming through whom Julie and I both enjoy seeing. In fact, we find that there is more of this than we can handle. My closest friends are people in the department that I have known for a long time. I find one of the great pleasures of academic life, particularly of statistics, is that the statistical community is a worldwide community of people that one enjoys being with. So statistics totally dominates my social life.

But the most important impact of statistics on my social life was of course that it brought Julie into my life. She was at the time on the psychology faculty at the University of Kansas and had obtained a fellowship to strengthen her statistics. Having seen some of my papers, she thought she would like to work with

Erich Lehmann in Leiden, 1985, at a party celebrating his honorary degree

me and asked me to sponsor her year in Berkeley, assuring me that she would not take up much of my time—a prediction that turned out to be rather inaccurate. The fact that we are now in the same discipline and department, that we do joint work, that each of us reads in draft whatever the other writes, and that I rely very heavily on her judgment and advice, means of course that professional concerns spill over into our private lives much more than would normally be the case.

A very specific impact Julie has had on my work is connected with the estimation book. After the publication of *Hypothesis Testing*, it would have been natural also to turn the estimation notes into a book. However, I never felt as comfortable with the theory of estimation, partly because of the dependence on the choice of loss function, partly because the subject was growing so much that the job seemed daunting. It was Julie's influence that eventually caused me to tackle this project.

In general, I find university life just absolutely wonderful. I don't know whether it's going to continue to be that wonderful in the future. I worry, for example, about what unionization may do to academic life. But the thing that I have appreciated so much about it is that you get the best of both worlds: You get the security of a salaried job with tenure, and at the same time you are to a very large extent your own agent. You can shape your activities in your own way. You can vary and emphasize differently the teaching, the research, the writing, and the administrative aspects. I chaired the department, I edited the *Annals*—things like that. Usually if you want to shape your own activities like that, you have to take more risks.

But coming back to statistics: The second way in which it influences you very strongly, and I don't see how anybody can be a statistician and not be influenced by it, is that it gets you used to stochastic thinking. Almost every newpaper article you read makes you say, "My God, they claim this or that, but don't they see that the real reason may not be that at all—that there is no causal relationship there." Probability and stochastic things just dominate our lives. I *wish* I understood what probability is. You Bayesians, of course, think you know. [Laughs]

DeGroot: Do you think about probability at that foundational level?

Lehmann: Not really; just occasionally. The fact is that after you do all of the technical and professional things you have to do, there isn't that much time left. And I like to read recreational things. I just finished reading the wonderful autobiography by Iris Origo, *Images and Shadows*. I am now reading this book [*Refugee Scholars in America: Their Impact and Their Experiences* by Lewis A. Coser] on a topic that interests me very much, namely the effect that the immigration from Europe during the Nazi period had on the development of American art and science. This book is particularly aimed at the social sciences, but I have other books that deal more with the arts and physical sciences. These books are not about my generation. They're about people who had already established themselves in Europe, and then came over because they were kicked out or because they had to flee from the Nazis in the 1930's and '40's, or because of some other difficulties.

I think that's how this university came to be so great. They brought over people like Tarski and Neyman. It was just an incredible achievement that Evans, an American traditional mathematician and a fairly conservative man in some ways, would have the foresight to bring *two* people like that, each of whom essentially created a new field and had a tremendous impact on its development. Neyman and Tarski were politically as far apart as people can be. Tarski was quite conservative and Neyman was a radical in a way. Neither of them were traditional mathematicians.

DeGroot: Did Tarski come at about the same time as Neyman?

Lehmann: No, Tarski was later. Neyman opposed Tarski's coming because he wanted Zygmund to come. There is a bit about that in Constance Reid's book. But to come back to the question you raised—I don't really think very much about foundational questions. After I've done my stint at teaching and so-called paper writing and research, and talked with students, then I really enjoy reading other types of books. I always read three or four books simultaneously. And I listen to a lot of music and go to concerts. The day only has 24 hours and my energy is not unlimited.

"COMING TO AMERICA WAS A VERY GOOD THING FOR ME"

DeGroot: Did you ever get around to writing, other than your technical writing?

Lehmann: Well, about 10 years ago I really thought I'd get out of statistics. I thought I would sort of retire. And I started translating German literature. There's a nineteenth-century German writer whom I was particularly fond of and who had not yet been translated. I translated one of his stories and then two by other writers. So, I have a set of three stories which I thought I would like to publish. Three love stories, by three very different writers of the same period who took three very different approaches, and that needed very different translation techniques because the language was so different in the three. But then I never polished it and never got it into publishable shape. I also started translating a very baroque nineteenth-century novel. Julie is still mad at me because it's a mystery story in a way, and about halfway through I stopped and she never found out how it ends. [Laughs] It's by an author who was essentially untranslated and very hard to translate. So then I got quite interested in translation and read a lot of things about it.

I've also done a little autobiographical writing for my children and grandchildren. Since I am the generation that came over from Europe, all of this European stuff will be lost if I don't put some of it down. And so I wrote a little bit about my family. I can trace my mother's family directly back to about 1500. These German Jews kept very good records. There were these ghettos with about 200 families, and they recorded whenever a house was sold, the new family that took it over, what professions these people had, and so on. I have a book. [Getting the book, *Stammbuch der Frankfurter Juden*, published in 1907] It's like a book of family trees, and it traces the Jews of Frankfurt back to the 1500's.

DeGroot: Did your mother's family remain in Frankfurt all through those years?

Lehmann: Yes. My father's family came to Frankfurt in the early ninetheenth century, around 1830, from Hamburg. I've always wondered whether there exists a similar book about Hamburg where I can trace his family back. But I haven't made a real effort to try and find it.

DeGroot: Are you still writing your memoirs?

Lehmann: Not at the moment because I want to finish the revision of the *Testing* book. After that I might get back to it. Anyway, I'm approaching retirement. I'll be 67 next month. So at worst or at best, I'm not quite sure, I'll continue working at the university for another 3 years. After that, I'll have more time.

DeGroot: What do you see yourself doing over the next 3 years?

Lehmann: Well, the book hopefully will be finished by January or February (1985). And I mentioned to you that I have an idea for this asymptotics project; that's one possibility. And Peter Bickel, Bill van Zwet, and I have been talking about an advanced asymptotics book. I don't know whether I can really play a part, but that's another possibility. Julie and I are talking about various projects. And occasionally I think that maybe I should see if I can write a book that will *sell*; a popular book that represents a point of view that I like and that might be useable. But I think it's probably a little late for that.

DeGroot: But the Hodges and Lehmann book must have sold quite well.

Lehmann: Well, it sold something like 50,000 copies over the years, which is respectable. It would have done much better if we had been willing to revise it every few years, because you know what happens: After 5 years there are stacks of second-hand copies. So even if the book is still used in classes, few new copies are sold.

DeGroot: And after retirement?

Lehmann: Spend more time with music. I play the piano a little bit and I will be able to spend more time with that. Go to more concerts, read more, try to write more about the family, all those things. Coming to America was a very good thing for me. My life would have been totally different if I had stayed in Europe. I can't tell you how glad I am that I came. I had a chance to go back to Zurich in the early 1950's. They wanted to start a chair in mathematical statistics at ETH. But I have no real roots in Europe. In Germany obviously there are no roots of any kind left. Everyone in my family there either emigrated or died. I have some kind of attachment to Switzerland because I spent some of my formative years there, although they were pretty unhappy years. But I don't want to live in Europe. Things are so much narrower and so much more restricted. There's much less freedom and everything is much more prescribed.

America is really great. Despite the fact that people are very much worried about the various ways in which the government interferes with our lives, this is still an amazingly free country. Berkeley is, of course, particularly free. Whenever our children accuse Julie and me of being squares, I tell them that Berkeley is a place where even squares can feel comfortable. [Laughs] There are many things that I would enjoy doing if I had enough energy, but as you get older the main drawback is that you become tired more easily. Otherwise I haven't found it too bad.

DeGroot: Thank you, Erich.