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A Conversation with Lucien Le Cam

Grace L. Yang

Abstract. Lucien Le Cam is currently Emeritus Professor of Mathematics and Statistics at the University of California, Berkeley. He was born on November 18, 1924, in Croze, Creuse, France. He received a Licence es Sciences from the University of Paris in 1945, and a Ph.D. in Statistics from the University of California at Berkeley in 1952. He has been on the faculty of the Statistics Department at Berkeley since 1952 except for a year in Montreal, Canada, as the Director of the Centre de Recherches Mathématiques (1972–1973). He served as Chairman of the Department of Statistics at Berkeley (1961–1965) and was co-editor with J. Neyman of the Berkeley Symposia.

Professor Le Cam is the principal architect of the modern asymptotic theory of statistics and has also made numerous other contributions. He developed a mathematical system that substantially extended Wald's statistical decision theory to the version being used today. With his introduction of the distance between experiments, we now have a coherent statistical theory that links the asymptotics and the statistical decision theory. Encompassed in the theory are the concepts of contiguity, asymptotic sufficiency, a new method of constructing estimators (the one-step estimator), the theory of local asymptotic normality (LAN), metric dimension and numerous other seminal ideas. The metric dimension, introduced in 1973, has been found to be fundamentally important in studying nonparametric or semiparametric problems. This monumental work culminated in a big book, *Asymptotic Methods in Statistical Decision Theory*, published by Springer in 1986.

Professor Le Cam's scientific contributions are not limited to theoretical statistics. At age 23 he introduced the characteristic functional technique (after Kolmogorov, but independently) to study the spatial and temporal distribution of rainfall and its relation to stream flow. It resulted in a model known as Le Cam's model in hydrology. In the domain of probability theory, he was one of the early contributors to the study of convergence of measures in topological spaces. He refined the approximation theorems and the concentration inequalities of Kolmogorov and made extensions of these results to infinite-dimensional spaces. We also owe to him the introduction of the concepts of τ-smooth, and σ-smooth that are widely used today.

In honor of his 70th birthday in 1994, a week-long workshop and a conference were held at Yale University, organized by Professor David Pollard. In addition, a Festschrift for Le Cam, *Research Papers in Probability and Statistics Papers*, was published by Springer in 1997.

He is married to Louise Romig, the daughter of a founder of statistical quality control, Harry Romig. They have three grown children, Denis, Steven and Linda.

Grace L. Yang is Professor of Statistics, Department of Mathematics, University of Maryland, College Park, Maryland 20742.

The following conversation took place in Professor Le Cam's office at Berkeley in July 1996.

Yang: It is a great privilege and pleasure to talk with you today. Let me begin with your childhood and schooling in France. Tell us about your background. Since you covered this topic in an interview with Don Albers and Constance Reid in the book *More Mathematical People* [Albers, Alexanderson and Reid, 1990], perhaps you just want to give a brief sketch.

GREW UP ON A FARM

Le Cam: My background is nondescript. I was raised on a farm in Felletin, a town of 2,500 people at the time, in Central France. I am a son of farmers. I was the second of three children, three boys. By the time I was 11, my elder brother and I were sent to a Catholic boarding school called Notre Dame in Guéret about 50 kilometers away from our farm and I was there for 7 years until graduation from high school.

My elder brother distinguished himself in school but had to return to the farm to help my mother after my father died in 1938. The priests at Notre Dame decided to keep me on and pay for my room and board.

Yang: How old was your brother at that time?

Le Cam: He must have been about 15. And then my younger brother came to the same school, but he escaped. There was no way you could keep him in school. That is about it for my family. My mother ran the family farm for a long time. Eventually, in the mid sixties or late sixties, she and my elder brother bought the farm. I finished high school and had to decide what to do.

Yang: When did you graduate from high school?

Le Cam: That was 1942. It was during the war. I was at that same Catholic boarding high school. It had been turned into a military hospital. And the

FIG. 1. *Lucien Le Cam, 1977.*

FIG. 2. *Le Cam grew up in Felletin, a little town in central France, current population 5,000.*

students for all the grades, except those of us in the highest two grades, were scattered out in the countryside. The top two classes were in the basement of a church. I passed the state graduation exams given nationwide. Then the director of the school decided that I was a good prospect for a seminary. So I went to the seminary.

Yang: You mean without consulting with your mother?

Le Cam: Look, it was agreed upon by chance. I had decided that I might like to be a doctor. My family investigated what that could mean but decided they could not pay for it. That was out. I was told that in the seminary I would be fed and housed and what-not, and they would take good care of me. So I went. I did not stay there very long—one night.

That was in Limoges, which is about 100 kilometers away from my farm. I had brought some books on chemistry with me to study. I was interested in chemistry at the time. I was told by my student colleagues that you are not allowed to bring any books in without special permission from the priests in the seminary. Oh, come on! Then the head priest sent us to our rooms to study the bible. I told my colleague I did not bring a bible. Ooh! Then we had a special sermon where that preacher said that we were here to see the light. And that the worst thing we could do is refuse to see the light. I thought that was a bit too much. And I went back home the next day.

Yang: What did you do after that?

LEFT SEMINARY FOR LYCÉE

Le Cam: I was interested in chemistry. So, I went to Clermont-Ferrand, the University in the city of Clermont-Ferrand in central France, and asked to study chemistry. But the University had already started. I was two weeks too late. There was no space in the laboratories. Okay, I will study mathematics.

Yang: You switched from chemistry to mathematics by chance?

Le Cam: By chance! [Laughs.] And then somebody there asked me how was I going to support myself. Can I have a loan? Oh no, you do not want to take a loan. If you go to the University, we cannot support you. But if you go to the lycée with the grades you have, they will give you food and lodging. "Lycée" is the name for standard public secondary schools, but some have an appendage with training programs like Mathématiques Supérieures (first year) and Mathématiques Spéciales (the second year) for the examinations for Normale, Polytechnique and other engineering schools.

I went there. But it was also too late; all the beds were taken. I was told I could get my noon meal for free. OK. So I went to the lycée and rented a room in an apartment. That was 1942, '43, '44. It was a two-year program, preparation for competitive exams to enter engineering schools mostly. The stuff we studied was old fashioned, 1820 type. That was particularly visible in the math program. So that's a bit of my background.

Yang: Is that how you acquired an "engineering" taste? You designed and built a country house on your own with Louise, later in California.

Le Cam: Not really! I did not acquire a taste for engineering. I could have gone to a university directly. But then I had to feed myself.

Yang: With no future goals in mind?

Le Cam: No, of course not. [Laughs.]

SAW BOURBAKI IN A BOOK STORE

Le Cam: I went to the University (Clermont-Ferrand) from time to time just out of curiosity. I went to the University once for a reason that was funny. Walking around the town, I had seen a little book from the collection *Eléments de Mathématique* by Bourbaki. It had in there symbols that I had never seen before.

Yang: That was your first exposure to Bourbaki. Many of us are curious about how you got such an abstract way of thinking and writing statistics.

Le Cam: Yeah. It has intersection signs and union signs and things. I had never seen that before. And it was a book of results without proofs, just the statements. One of them was Zorn's lemma. So you have the axiom of choice and you can prove Zorn's lemma out of the axiom of choice. And somehow I was unable to do that.

I sent a postcard to Hermann, Bourbaki's publisher. They sent me the first volume of Bourbaki's *Topologie Générale*.

Yang: You have been interested in Bourbaki ever since. Perhaps you could explain the history of Bourbaki. Although it is well known to mathematicians, it probably is not familiar to most statisticians.

Le Cam: Nicolas Bourbaki (1935–) is the pen name of a loosely delimited, self-renewing aggregate of French-speaking mathematicians. The group apparently started in Strasbourg for mutual assistance in teaching from the obsolete French texts such as Goursat (known here as Goursat–Hedrick). By 1935, H. Cartan, C. Chevalley, J. Delsarte, J. Dieudonné, A. Weil, R. de Possel and S. Mandelbrojt had decided to write a treatise called "Eléments de Mathématique." de Possel and Mandelbrojt dropped out after the first meeting. The treatise starts from scratch, gives precise axioms and definitions. Presently, it has many volumes for a total of over 12,000 pages.

One of the basic ideas of the founders was to organize the existent mathematics according to "structures," that is, combinations of axioms and their consequences. An example of "structure" could be topological vector spaces. The resulting Treatise is concise, clear and eminently readable. It does not yet encompass all of known mathematics and never will, since new developments occur constantly. However, it is a good basis for undergraduate and first-year graduate study.

The Bourbaki group itself continues under few regulations, except that retirement at age 50 is mandatory. Many of the famous French-speaking mathematicians are, or have been, members of the group.

[For more, see "Twenty-five years with Nicolas Bourbaki" by Armand Borel (Borel, 1998).]

So, yes, I became interested in Bourbaki. Then, even though I had not taken courses at the University, I decided to take one examination there, mathématiques générales. I passed that fairly well. In France, you could register for an exam at a university without attending there.

Yang: What kind of exam?

Le Cam: Calculus and analytic geometry. I remember one question that almost downed me. It was Lichnérowicz, I think, who asked me that question. You take a function $f(x) = x^{x^{x}} \cdots$, to the power x, to the power x indefinitely. I was asked what is the derivative of that function? I proceeded to get the derivative of it and the guy who was asking me the questions said: "Well, you have not proved that the function exists; how can you take the derivative of it?" Too bad, I got a bad mark.

Yang: If you passed the exam, what would that have given you?

Le Cam: Not much. But it is stamped on your card that you have passed. If you did not pass, nobody cared. They did not keep a record of it, at least at that time they did not. Eventually, you get a piece of paper that says you have passed.

Yang: That got you started on rigorous formulation of mathematics.

Le Cam: [Laughs.] Not really. I am entirely unrigorous.

Yang: We think you are very rigorous.

WENT TO PARIS

Le Cam: [Laughs.] Then after that, well, there were some complications because of the war. I decided to go to Paris. They had the examinations for the engineering schools, and Ecole Polytechnique, and so on.

Yang: Was that 1944?

Le Cam: This was in December 1944. I decided that I would try my chance at the Ecole Normale. That was partly a matter of choice, but mostly a matter of possibility. I tried to register for the Ecole Polytechnique. But at that time it was still in occupied France. To be able to register for the examination at the Ecole Polytechnique you had to prove that you were obviously and utterly French. That meant that you had to produce a certificate of birth of your grandfather on your father's side. My father died when I was about 13 and *his* father died when *my* father was about 13. I wrote to Brittany to get the birth certificate for my father's father. They sent a birth certificate for me, not for my father's father. So, I could not register for the examination. That system was intended to prevent Jews from applying. For the engineering schools, the problem was drafting; my drafting was not too good. So I did not even try. I tried Ecole Normale Supérieure, passed the written part and flunked the oral part beautifully. The questions were fair and I knew how to answer as soon as I left the room. Too bad!

That meant if I wanted to try, I could ask to repeat the exam. But repeating meant going back to the lycée.

Yang: Were these exams given once a year?

Le Cam: Yes, once a year. Having another year of repeating the same material? I said "forget it." I registered at the University of Paris. It was a good thing to register at the University. It cost about one dollar to register and one dollar to register for each of the exams you want to take. Nobody would bother you, nobody would ask anything, and by registering you got to use the student cafeteria, which was cheap and the food was reasonably good.

Yang: That was during the war.

Le Cam: That was at the end of the war.

Yang: How did the Germans treat the French during the occupation? It seems that your education was not interrupted by it.

Le Cam: The Germans were most obnoxious. Think of the way the Japanese treated the Chinese. But for my education it did not matter that much. In May 1944, I was asked to report for the medical exam to be drafted. That was under the Germans. I tried my best to look sick, pale and thin.

Yang: You mean you could have been drafted and made to serve in the German army?

Le Cam: Probably not, but I would have had to serve in the so-called youth camps where people were asked to make charcoal for the Germans. After that experience, I decided I had better hide in the woods and I disappeared. Then De Gaulle's government returned to Paris in August 1944. I went to Paris. But then I had to take a medical exam for the French army. That was either in the end of 1944 or the beginning of 1945. I passed the medical exam twice, because the first time they messed up the papers. Then somebody decided that it was time to call the next group of people one year younger and that there would not be any space in the military barracks for the two groups. I was therefore dispensed from the military service by decree of the Minister.

Yang: You were very lucky. Not in taking university exams, but lucky in not being drafted by the German or the French government. So you stayed at the University of Paris?

Le Cam: Yes. I stayed at the university and passed a few examinations. I took the exams in calculus and in rational mechanics, and then I needed another examination and decided that probability would be it.

I went to see Fréchet to ask whether I could take the exam in probability. Fréchet said simply: "You might be able to take the written exam and pass. I would be the one to examine you at the oral exam.

Since you have not taken my course, I will flunk you."

FIRST EXPOSURE TO STATISTICS

Le Cam: But I could have looked at the notes. Fréchet said no way. But he said if Darmois accepts you in statistics, that's fine with me. Fréchet was the boss of the three sections. So I went to see Darmois. We talked a bit. He said: "Well if you can find my notes you can try it." Where do I find your notes? "There was someone who was taking notes during the entire course, so ask him." I got the notes, and I had a weekend to work at it. Then I had the exam.

Yang: You never had statistics before either?

Le Cam: No. Darmois was the one who asked me questions. I remember one of them. You know the Cramér–Rao inequality that was proved by Fréchet a few years before? Darmois had extended it to the multidimensional case. It was not published. Darmois asked me to prove it, the multidimensional version, during the oral examination. I did it. It was in 1945.

FISHER'S SHOES

Le Cam: Then something really funny happened. It was about Fisher's shoes. I heard the story from Line Loève and from F. N. David. It happened when Fisher was invited to give some lectures in Paris in the spring of 1946. Darmois and Fréchet had invited Cramér, who talked about the Cramér–Rao inequality, to the great amusement of Darmois, who knew it well. Then Cramér announced that in his second lecture he would give his ideas about fiducial and confidence intervals. Fisher was then sitting in the front row, so Cramér diplomatically changed the subject. I attended Cramér's first lecture, but not the second. I did not go to Fisher's lectures, except the last one. In the meantime Fisher had called my friends Edith Mourier and Colette Rothschild "Idiots, morons, you will never understand anything about statistics." Fisher's lecture was given in a sort of French that was impossible to understand.

Michel Loève had been told to take care of Fisher, but his wife Line was much better at that kind of thing. On a Saturday morning, Line got an excited telephone call from Fisher: "I have no shoes. Do something." Line ran to Fisher's hotel and found Fisher in his stocking feet. It was a period when many things were restricted. There were regulations specifying that certain days were "no meat" days, other days were "no hardware" and some were "no shoes" meaning that stores selling such were forbidden to open. Saturdays, in particular, were "no shoes."

So here is Fisher without shoes and no stores to buy shoes. Line conspired with the manager of the hotel, representing to him that one could not let such an eminent scholar go back to England without shoes. Finally the manager found a friend of his who had a shoe store and was willing to flout the law if Line and Fisher entered by a back door. It was on the other side of Paris but they made it there somehow.

Unfortunately Fisher was very fussy about shoes. They had to have leather soles. Those had not been seen in France for years. My soles were of some wood slats tied with strings. That was most common. It took Line a lot of time and persuasion to get Fisher to accept a "substandard" pair of shoes, with composition soles, but she finally succeeded and shipped him off.

Now, why was Fisher without shoes? All he had done was to follow a long tradition of putting his shoes outside his hotel room door, so that some valet would shine them during the night. Someone had noticed the quality and helped himself. That would have been par for the course, but Fisher had done that *three* nights in a row and not noticed on the first two that something was not quite right!

So that was the guy who called my colleagues "Idiots, morons."

WORKED AT THE ELECTRICITE DE FRANCE

Yang: That's very funny. Let us go back a little bit. You went to the University of Paris in 1944 and by October of 1945 you passed all three exams and received a university diploma. That did not take very long.

Le Cam: Yeah, I had what was officially called the Licence es Sciences, the university diploma. By that time I had become addicted to reading Bourbaki for pleasure. I also took another course and another exam later on something called "advanced analysis." That was a course given by Julia.

Yang: The mathematician of "Julia set" fame?

Le Cam: Yes, the famous Gaston Julia.

Yang: After you received the diploma, you almost immediately went to work for Eléctricité de France.

Le Cam: Well, I looked around for a job. At first I thought I would try something that had to do with fluid mechanics or things like that. I was told by some friends that there was a place just outside Paris that was hiring people to work on helicopters. I went there for a job interview. First, the ride there and back was obnoxious. You are there in the back of a bus. They had those open-back buses and you have those chimneys, those—what do you call them?—towers that send fumes and cinders at you. When you get out your hair is full of cinders. Then, you enter a room where you have maybe 50 or more people working, each in a cubicle. I did not like the environment. So I said "no." It was a good decision, because they were working for the company that was supposed to build helicopters for the French and mostly what I would have had to do is to work on vibrations of helicopters. But then the United States in the name of the Marshall Plan decided that the French should not build helicopters. That was out. It closed up.

Then I decided that maybe I should be an actuary. So I took a few courses in the evenings from Dubourdieu, who was a renowned actuary. Then, just as it happens, I decided to ask Darmois. I went to talk to Darmois at home, which was not done. He told me he had a friend who needed statistical help. He gave me the name and phone number. "Call him; see whether he can help." That was Etienne Halphen. That is how I ended up working for Halphen at the Electricité de France. Actually, it did not exist by that name. Massé and a number of other people then were working very hard trying to create Electricité de France as it was eventually called.

I was hired as a statistician. There was not enough equipment to produce power in France at the time following the war. We were trying to figure out what was the probability that we would lack so much power from the hydraulic system. It meant looking at the stream flow trying to figure out what kind of distribution it might have, this way or that way. That was one part of what we did. The other part was: Suppose that you have a river and you have a big dam on it. How should you operate the dam to be able to produce power when you need it and still not waste water because it might overflow? So that's mostly what we were doing. There were occasional questions otherwise, about what size spillway should be built on a dam to evacuate floods that would occur once in a thousand years and various things like that.

Yang: This explains the paper you wrote on precipitation in the 4th Berkeley Symposium. In there you used characteristic functionals. That is a widely cited paper in hydrology. Actually, you introduced characteristic functionals in your very first publication [Le Cam, 1947]. I discovered that by accident. My student Enzo Capasso was visiting Maryland from Italy. We were working on propagation of epidemics and wanted to use that technique. We found your article in the library and were impressed (with a sense of history) that your paper was communicated by Emile Borel to the French Academy. Had you ever attended Borel's lectures?

Le Cam: No. He had retired by that time. I saw him once at a seminar given by Sierpinski.

Yang: While working for Electricité de France, you also attended seminars at the University of Paris?

Le Cam: Somehow, right at the start in 1945, we formed a group of people about my age: Edith Mourier, Colette Rothschild and a few more people. We had decided that we were certified statisticians, but we did not know anything about statistics. Why not instruct each other and meet once a week at the University and debate what we had read during the week? We were just starting to get some of the periodicals that had been published elsewhere, during the war, like the *the Annals of Mathematical Statistics*. There was a lot to discuss, but mostly we discussed politics. But then, Fréchet, and after he retired, Darmois, had one seminar a week on probability and statistics. I used to attend that.

The situation at Electricité de France was quite liberal. My boss, who by that time was Pierre Massé, said: "If you want to go to the Bois de Boulogne [a big park next to Paris] to smoke a cigarette while you are thinking, that is fine with me. All that counts is what you produce. Who cares where you do it?" We were free to go listen to things at the University. Eventually, Darmois asked me to produce speakers for his seminar. So I got more involved. But it was in addition to my work at Electricité de France.

INVITED TO BERKELEY BY NEYMAN

Yang: How did you decide to come to this country?

Le Cam: Oh, just accidentally, I guess. That was around Easter 1950. Neyman was visiting. Edith Mourier had been in Berkeley the year before and decided that Neyman would like to have high tea. High tea is tea and petit-fours and some cognac. We had a high tea after Neyman's lecture. That is how I met Neyman. Neyman sent me, through Fréchet, an invitation to visit Berkeley for a year as a lecturer.

Neyman went away in not quite a week. I vaguely remember going with Neyman and Charles Stein for an aperitif at a café. All Neyman and Charles talked about was the "Loyalty Oath" in California. Neyman decided to sign it. Charles refused.

Yang: Was that the reason that Stein resigned from Berkeley and moved to Stanford?

Le Cam: No, he was fired! He went to Chicago and then Stanford. I did not understand what it was about. If I had known what it was about I probably would have refused to go to Berkeley.

Yang: Did you sign it too?

FIG. 3. *Cramér chatting with Lucien Le Cam and Betty Scott after the ceremony dedicating the Neyman room, Evans Hall, Berkeley. Cramér, age 92, flew to Berkeley from Stockholm to open the ceremony. "I cannot hear but I can still talk," said Cramér during his speech.*

Le Cam: No. Being a foreigner, I couldn't sign. By law, I could not sign it, which made a mess in many ways. One of the people at the U.S. embassy said that I should not sign before coming to Berkeley, but another insisted I had to sign. Finally one person at the embassy decided I could not sign. The oath was imposed by the Board of Regents of the University of California. It was declared unconstitutional by the California Supreme Court. The Regents had no right to impose such an oath.

FLUNKED THE QUALIFYING EXAMINATION

Yang: Neyman invited you to Berkeley in 1950 as a lecturer and you stayed on, except for a stint at the University of Montréal?

Le Cam: Yes. Then in 1951, Neyman said I could stay on if I would get a Ph.D. So I became a student.

Yang: Looking at your vita at that time, you seemed to have repeated some of your experience of 1944–45 in Paris. During 1950–51, you flunked the first Ph.D. qualifying exam and barely passed the second time, wrote up your thesis and got married. All of that in one year. Tell us about your "resounding defeat" at your first Ph.D. qualifying examination at Berkeley. Who was on your committee?

Le Cam: The first time, it was Alfred Tarski, Joe Hodges, Steve Diliberto and perhaps Raphael Robinson and someone else.

In the second time it was Tarski, Charles Morrey, Mike Fell, Raphael Robinson and John Kelley (not sure).

The first time I tried to present the most recent version of the fixed point theorems I knew, a theorem by E. Begle. It took a lot of topology. One committee member had just taught a course on algebraic topology, but he was using the Čech homology theory and I was using Vietoris. Čech and Vietoris do not quite match. Any time I gave a definition, that member would call me to the carpet, "That is not right." So I never got to present the fixed point theorem.

The worst part was the second time, really. Griffith Evans gave Julius Blum and I the same topic: "fixed point theorems." We were instructed not to talk to each other. But we did. I knew very well that Julius did not know beans about it, but he passed easily. Julius advised me to stay at an elementary level and use no algebraic topology. So, I did not use algebraic topology, as such. I used an approximation argument of Leray and Schauder and deduced the Brouwer fixed point theorem from a determinant formula of Picard.

Each member of the committee gave me a hard time. Some complained about my notation, but the worst was about the proof of Brouwer's theorem. One member exploded: "Any damned fool knows that this cannot be done!" A similar proof, but more complicated, appeared later in the first volume of

FIG. 4. *Le Cam (left) with Rafail Hasminskii (second from left), Boris Levit (third from left) and Lucien Birgé (right) during a break, at the conference at Berkeley to celebrate his 65th birthday, 1989.*

FIG. 5. *Conference at Berkeley for Le Cam's 65th birthday: Second row, Larry Brown (center) and Iain Johnstone (right).*

FIG. 6. *Reception at the Berkeley Faculty Club for Le Cam's 65th birthday: Rick Kreisler (son-in-law, far left); Vera Byers (immunologist who treated Linda's cancer, center).*

Dunford and Schwartz. There was also a theorem in locally convex vector spaces. The committee expressed doubts about its validity. It was published a couple of years later by Irving Glicksberg.

Yang: How frustrating to see one's own work or similar work published elsewhere later!

Le Cam: It was not that bad. I did not have the feeling I had done anything special, but I had studied pretty hard.

Yang: Then you wrote your thesis under Neyman. But you were Julius Blum's thesis advisor. How did the Berkeley system work? Were you a student and a thesis advisor at the same time?

Le Cam: No. After passing his exam, Julius dropped out for a year. When he came back I had my degree. A dean objected to my supervising Julius and, after the thesis was approved, appointed Loève head of the committee.

Yang: Your dean did not want a student to be the Ph.D. thesis advisor of another student.

I looked at your c.v. and saw what you did as Assistant Professor. I am astonished by your extraordinary accomplishments. In five years you produced seven Ph.D's: Julius Blum, C. Kraft, B. Rankin, George Steck, Tom Ferguson, Jim Esary and I. Abrams. During the same period, you wrote many fundamental papers and introduced the theory of contiguity, theory of local asymptotic normality (LAN), an asymptotic optimum estimation procedure [obtaining estimates without Newton–Raphson–like iterations, some call it one-step estimator], asymptotic sufficiency, tightness in weak convergence and on and on. This would scare aspiring young assistant professors. Could you comment on this?

Le Cam: I had more energy at that time than now. Besides, I had to teach and I made an effort to understand what I was teaching. I should add that, with one possible exception, these students were great!

CLOSE ASSOCIATION WITH NEYMAN

Yang: You had a close association with Neyman both professionally and socially. You were his coeditor of the *Berkeley Symposium*. By the time of his death, you were Associate Director of his Statistics Laboratory. You were at the hospital when he passed away. He treated you like a son. He also introduced you to your wife Louise. (The reader may find this in *More Mathematical People*.)

What was it like working with Neyman? How was Neyman as a boss?

Le Cam: It has been my privilege to have some very good bosses. At the Electricité de France, I had

FIG. 7. *David Pollard (far left) discussing statistics with an attendee at the Yale workshop, 1994.*

Pierre Massé, André Nizery. At the University of California, Neyman.

Actually, my relations with Neyman had two distinct periods. In 1950–52, I was a lowly lecturer–student. Neyman was a big shot and the *pater familias*. He rarely ordered you to do anything, but you felt compelled to do it. Thus Terry Jeeves and I used to come after dinner, sometime past midnight, to do various computations that had to be done for reports at Institute of Mathematical Statistics (IMS) meetings, especially on cloud seeding.

We were expected to go to such meetings, just as we were expected to show up at social functions, but I don't remember Neyman actually ordering us.

After that first period, Neyman let me do whatever I wanted. There were always reports due on grants. He may have hinted that it would be good to write a paper. He did not push. He assigned us to give courses, but did not interfere with our choice of material. I remember being assigned to teach "asymptotics" and asking what should be covered. Neyman just answered: "You are the doctor."

I know that he had a reputation for being bossy. That is how I became Chairman after Blackwell's term. Nobody else wanted to take the job as long as Neyman was around.

We got along just fine. Around 1962, he was late in drafting requests for grants. I wrote them. He signed without any complaints.

He could get angry, or upset, at times. Once he was reflecting aloud about his $C(\alpha)$ tests, saying that it would be nice to prove their asymptotic optimality among all asymptotically similar tests, not just tests based on sums. When I answered "That is obvious," he raised his voice and ordered "If it is so obvious, prove it!"

Generally, I found it very easy to get along with him. It hurts me that, after Neyman died, his colleagues at Berkeley essentially revolted against the authority of the *pater familias*.

Yang: You mentioned the $C(\alpha)$ tests. I remember Neyman told me that C stands for Cramér, is that right? He wrote the paper for Cramér's sixtieth birthday.

Le Cam: That is right. The C was to honor Cramér and the α was for "asymptotically similar with level α." Actually, it was the question about the asymptotic optimality of those $C(\alpha)$ tests that prodded me to write my paper for the 1955 Berkeley Symposium and, later, my "Locally asymptotically normal families" paper (1960).

PREFERRED PAPERS

Yang: You have written so many important papers. Are there papers of your own that you like better than others?

Le Cam: My own papers, not Wald's?

Yang: No, not Wald's. For that we need to have another entire conversation with you.

FIG. 8. *Dinner at a restaurant in New Haven, Connecticut, after the Yale workshop: Louise Le Cam (center), Jon Wellner (right).*

Le Cam: Well, I like my paper on the extension of Wald's theory of decision functions [Le Cam, 1955]. I like my LAN paper.

Yang: The LAN is the 1960 paper on "Locally asymptotically normal families of distributions" in *Univ. Calif. Publ. in Stat.*?

Le Cam: Yes. Also, I like the paper on sufficiency and approximate sufficiency [Le Cam, 1964]. That one took a long time to get published. It was written in late 1959 and finally got out in 1964. The editors objected to its abstract nature. I also had editor trouble with the LAN paper.

Even though it is not very good, I like my paper on the relation between dimension and the bounds on the risk of estimates in *Annals of Mathematical Statistics*, 1973. [Pause.] What else? I am not too sure. Well, there is one paper I like because it is neat. That's my paper with Grace Yang, on the preservation of asymptotic normality, in *Annals of Mathematical Statistics,* 1988.

Yang: Oh, thank you! Why do you say that our paper "is neat"?

Le Cam: Suppose that you have independent observations, say $X_{n,j}$, $j = 1, 2, \dots,$ forming an asymptotically Gaussian experiment as described there, but all you can observe are functions $Y_{n,j} = f_{n,j}(X_{n,j})$ or maybe $Y_{n,j} + \epsilon_{n,j}$, where the $\epsilon_{n,j}$ are corrupting noises. The asymptotic Gaussian character assumed for the X's carries over to the Y's. Just like that. No other conditions are needed! Of course one has to be careful that, when you lose information, the estimates might not be consistent at the right rate.

Yang: Actually, the paper covers many special cases in applications including censoring. But not many read the paper.

Le Cam: I think that Aad van der Vaart gave reference to it. He writes good papers. Peter Bickel and Yacov Ritov used it.

Yang: How did you come up with the concept of metric dimension for the parameter space? It is such an important concept, particularly for determining rates of convergence and approximations.

Le Cam: To tell you the truth I don't remember how that happened. I had been mulling over the paper that Rainie Schwartz wrote about Bayes estimates in the *Zeitschrift für Wahrscheinlichkeitstheorie und Verwandte Gebiete*, 1965, and things like that. I was wondering what would be a good way to handle such problems without having the special parametric representation by Euclidean space and so forth. The parametric representation hides what is going on, complicates it. If you are just after estimating the distribution, the probability measure itself, you should not have to worry about the parameter. Then how do you define in some sense that, if the class of measures is not too complicated, there would be an estimate?

I decided that metric dimension is the thing to use. It is only after 1975 that I recognized that I was using something very closely related to Kolmogorov's metric entropy.

FIG. 9. *Lunch with Steve Stigler and Grace Yang in Montréal, attending the annual meeting of the Institute of Mathematical Statistics, August 1995.*

Yang: I thought that the two are not quite the same thing. Conceptually there may be some similarity. Could you elaborate?

Le Cam: Yes, they are not the same but there is a close connection, especially in totally bounded sets of high dimension.

Yang: Donoho and Liu used the Kolmogorov metric entropy very effectively in their study of the optimal rates of convergence.

Le Cam: Yes. But as Donoho has shown, there are other related concepts that are also very important.

Yang: For instance?

Le Cam: One can find many in the treatises on approximation theory.

Yang: What's happening on that front? The role of dimension in nonparametric and semiparametric estimation?

Le Cam: I am not too sure. I think right now we are going through a phase where the standard assumptions are about something on the real line, or close to that, with spaces of functions that satisfy certain conditions by being Sobolev balls or Besov balls, or something, and that has taken over. I think there is more written about that kind of thing, Sobolev balls, than there is about the Kolmogorov entropy in general. It is a pity because if you specialize you might not see what is actually going on and there are problems there that I don't quite know how to solve. I don't have the right isomorphism theorems.

I tried to estimate the measure itself, using Hellinger distance. But, suppose you are not interested in that and you are just interested in a function of the measures. You get into the so-called semiparametric system. I am not too sure I understand or I can handle what can happen there. The dimension (in my sense) for the nonparametric part, and the dimension of the part that you are trying to estimate can get mixed up or they can be totally inseparable. I am not too sure what is going on there, especially when you are not in the situation where you can use asymptotic normality.

Yang: So people use Stein's approach, looking at the most difficult subproblem?

Le Cam: Yes, with a definite modification by Donoho. He shows that the most difficult one-dimensional problem technique works for parameter sets satisfying certain convexity conditions. That is for asymptotically Gaussian situations for subsets of Sobolev-type balls.

Nussbaum wrote a difficult paper on approximating the problems of estimation of density or regression functions by problems of estimating trends in Gaussian noise in the *Annals of Statistics*, 1996. Sara van de Geer at the Joint Statistical Meetings, Chicago, 1996, spoke about approximating i.i.d. experiments by their Poissonized counterparts.

FIG. 10. *In the countryside, California, 1987.*

All of that was under Sobolev-type conditions. By contrast, I tried to study the statistical implications of the metric structure defined on the parameter set by Hellinger distances. I do know that this particular metric structure does not tell you everything, by far, but it tells quite a bit, as can be seen from the inequalities on pages 475–520 of my 1986 book. I am not sure whether Sobolev-type assumptions bring in something additional, and, if so, what this may be.

Yang: Your distance is hard to calculate?

Le Cam: Oh, my distances are very impossible to calculate; you know that. But bounds are feasible. And for the Bayes risk, I know that just the metric structure does not catch everything, but I don't know what else to look at, except, as you said, calculations.

CONTIGUITY, CONVOLUTION THEOREM AND HÁJEK

Yang: We have not discussed Hájek. Parts of your work and Hájek's are closely connected. Your powerful lemmas on contiguity were first applied by Hájek in his 1962 *Annals of Mathematical Statistics,* paper, and by Hájek and Šidák in their book, *Theory of Rank Tests* (1967).

Le Cam: [Laughs.] It is a bit complicated to describe my relations with Hájek because I don't remember which year he was here. But I think it was late '61 and early '62 the first time. I was Chairman and I was busy. Hájek being a polite gentleman did not dare bother me. If he had barged in and said, "Lets talk about this or that," it would have been just fine. But he did not dare. He thought he would bother me.

At that time we had very little contact. But he told me some of the things he was trying to do. I gave him a set of notes, handwritten. So he did several things. He already had a paper accepted for the *Annals of Mathematical Statistics* in 1962. He rewrote that paper. Contiguity—he had not thought about the contiguity in that bare form. He rewrote that part. He rewrote several pieces of it. He said so in the paper. Then he and Šidák wrote their book. When they were writing that book, he used my notes quite a bit. He was isolated in Prague. Then he came back. I think it was the beginning of 1966.

Yang: I think so. I remember he was at Berkeley when I was working on my thesis. He would ask me questions about my thesis and give me comments. Then one day, he said "Too many cooks spoil the soup." He decided not to get involved.

Le Cam: [Laughs.] I know he was here in early 1966. Maybe he was here before in 1965. I am not sure of the dates. Hájek was a marvelous person. He was full of life and full of ideas. One of the papers that he wrote [Hájek, 1968] should have been put in his book but was too late for the book. It concerns bounds for the variances of estimates that are functions of ranks that can be given without putting in the assumption of identical distribution of the observations. It is a hard problem. He did it. Then he came here for the Sixth Symposium in 1970. At that time he was ill.

Yang: What did he have?

Le Cam: He had kidney trouble.

We tried to convince him that he should stay in this country longer or forever. But he would not hear of it, because he knew that he would have medical treatment in Czechoslovakia. Here it would have been difficult.

Yang: In Hájek's paper on "Local asymptotic minimax and admissibility in estimation" [Hájek, 1971] he pointed out that the local asymptotic minimax and admissibility results were first proved by you [Le Cam, 1953] but have been overlooked by us for many years.

Le Cam: [Laughs.] I should have made more propaganda. But I am not good at propaganda. The convolution theorem, I had not thought about that at all. That was before the Sixth Berkeley Symposium.

FIG. 11. *A view of the country house hand-built by the Le Cams.*

Peter Bickel had read the article in *Z. W.* [*Zeitschrift für Wahrscheinlichkeitstheorie und Verwandte Gebiete,* 1972] and told me what the general result was. We were at the counter in the Statistics office in Campbell Hall. My reaction was: "Oh, I know how to prove that." So I devised another proof, then Peter devised still another proof.

Yang: I thought Hájek's proof was very complicated.

Le Cam: Yes. It is a bit complicated. I must say I never read his proof. Some papers are very hard to read.

Yang: That is how you wrote your papers. When you find that other papers are too hard to read, you just re-prove the results yourself.

Le Cam: [Laughs.] If you know how to get the result in a different way, you are tempted to do it yourself. So for the convolution theorem, a bit of thinking and I had a proof without writing anything.

Yang: So you proved it on the spot?

Le Cam: Yeah, on the counter in the department office. The idea was clear. I am surprised that, when Peter wrote a book with three other authors on semiparametrics, he still used essentially the proof he devised in 1970. I think that hides some of the essentials, because it relies on analyticity properties. No analyticity property is needed. It is a fact that is simple. It does not need any deep functions of a real variable or complex variable. I think that hides it.

Yang: Then came van der Vaart's version.

Le Cam: Van der Vaart has several versions. I think the subject is not quite right yet, not quite finished yet.

Yang: David Pollard has generalizations and you have another one. What is the status now?

Le Cam: Of the convolution theorem? That was given at the Fifth Purdue Symposium.

Yang: Has the convolution problem been solved completely?

Le Cam: No. My paper is published [Le Cam, 1994]. I read the referee's report. The referees were nice, pointed out a number of papers I had missed. They did not find mistakes, but I am not happy. From time to time I try it again. From time to time, I try to find counterexamples. So I am not happy.

Yang: What do you try to generalize it to?

Le Cam: The finite-dimensional formulation can be done in the locally compact groups situation. As soon as you go to infinite-dimensional Hilbert

FIG. 12. *Daughter Linda and husband Rick Kreisler in the country house, Casadero, California.*

spaces, or Banach spaces, you lose that locally compact property; you don't have Lebesgue measure or anything that resembles it. So you have to proceed differently or proceed by approximation. Now if you are in the Gaussian case, the approximation is visible: approximate the infinite-dimensional situation by a finite-dimensional one, project orthogonally and everything works, as shown by Moussatat in the early seventies. But if you are in a different situation where you don't have Gaussian processes or you don't have a decomposition into independent pieces that you can build on, it is not clear what could happen. I tried to write it out sort of brutally. Somehow the first time I wrote it out, I made a mistake; nobody noticed it. I have written it out several times since and published it. I will not be surprised if there are mistakes. As I say from time to time, I tried to find counterexamples. Right now I think I have one.

Yang: Okay, what is your counterexample?

Le Cam: Oh, well, it's too complicated. I will try to write it out.

There are things that happened in my proof that rely on topology when they should rely on measurability, and I am not happy about it.

Yang: Well, we look forward to your complete solution of the convolution theorem.

Le Cam: It probably is going to be hard to write. It might take a long time, if it is possible. To tell you the truth, very few people care. So why should we bother?

Yang: I would not put it that way. Your papers are very difficult. Chinese statisticians refer to your papers as "book from heaven." That is a Chinese expression for hard books or papers. Many of us worked long hours trying to understand your papers.

Le Cam: Those concepts that I use are not really that hard. It is true that in my book I started with vector lattices. But just plain vector lattices are lot simpler to describe than what can happen in measure spaces.

COMPUTER AND STATISTICS

Yang: What do you think is going to happen to statistics in the cyberspace environment?

Le Cam: I think I will quote Niels Bohr: "It is very difficult to predict, especially the future." What is happening now is that people are really playing with packages on the computer. It is very useful but it is just playing. Sometime somebody will have to look at the basic ideas and find out whether the so-called neural nets are really regression, or modify one to get an answer you would not expect them to be able to get, and so forth. Somebody would have to think about what the situation is. Besides that

FIG. 13. *Honorary Degree of Science, Université Libre de Bruxelles, Brussels, Belgium, February 1997.*

I don't know, I really don't know what is going to happen.

Yang: What about the present? What should be the right curriculum for a graduate statistics program?

Le Cam: Statistical education worries me. It is true I am an old-fashioned dinosaur. But I think there is a place for some sort of instruction in some sort of theory that allows people to think, not only to compute things, but to think about the problem. I am worried that teaching is getting more and more on how to use packages, how to program some packages and not so much on what it is that we want to do. I have been reading a lot of astronomy and cosmology lately. The amount of thinking going on there is enormous compared to what is going on in statistics. We had some thinkers in statistics. I could say Laplace. Gauss was not so much involved. He derides Laplace's use of medians because, says Gauss, "it makes use only of one observation." He pushed least squares, a technique (and name) he may have "borrowed" from Legendre. Fisher did plenty, but not so much in theory. Neyman, of course, tried to set himself a goal and then tried to find out how to achieve that. Wald was magnificent. And Hájek. All of these people would try to set themselves a goal and try to achieve it. Now, we still have people who can do that—I would say David Donoho, Iain Johnstone—but there are very few and statistical theory is not taught. Even in this 'good' department here, emphasis is either on finding packages or doing something with packages. Do something or other without enough thinking about what it is you want to do. I don't mean that for people like, let's say, Terry Speed, who tries to do something about genetics, but in the general teaching of students how to think.

Maybe 50 years from now, there will be another person who will decide: "Well, we have to know what we are doing." And then, it will start over again.

A STATISTICAL FAMILY

Yang: Shall we change the subject? Tell me about your family. Begin with your father-in-law, Harry Romig, who was a prominent statistician in quality control.

Le Cam: My father-in-law was a pioneer in the subject of quality control, extensively developed at Bell Laboratories in the early twenties. Other well-known authors in the group included W. A. Shewhart and H. F. Dodge. Romig introduced the idea of "double sampling" and published in 1926–27 the first tables for the application of the method. More extensive tables were later published as "Dodge and Romig Sampling Inspection Tables" (1941, 1944). In 1950 he left the Bell Laboratories and subsequently worked in many corporations and was involved in the Apollo missions.

In 1951, Romig asked Neyman to help his daughter who was coming to Berkeley. Neyman, thinking that Louise was 13, asked me to take care of her. That was how I met my wife, and I met H. G. Romig for the first time the day of our wedding. (For more accounts, see *More Mathematical People*.)

My father-in-law's interests and mine were too far removed to allow much scientific interaction. We almost never discussed statistics.

Yang: You are a theoretician. But you did get involved in applied work in clinical trials and immunology because of your daughter's bout with osteosarcoma. Linda's recovery was extraordinary. She lost a leg and a lung. How is she doing? Are you

FIG. 14. *At the reception after receiving the honorary degree: Louise Le Cam* (*left*); *Le Cam* (*second from left*); *Claude Lefevre* (*third from left*); *Marc Hallin* (*right*).

still working with the doctors at the San Francisco Medical School?

Le Cam: At one time I had the opportunity to help Vera [Doctor Vera Byers, who treated Linda in the 1970s] in planning clinical trials, even some sequential ones. Then, little by little we lost contact. We now rarely see each other except socially.

ESTABLISHED THE LOEVE PRIZE

Yang: You are officially retired but as busy as ever. On weekends, you and Louise often go to the country house that you two built. On weekdays, you come to the office and still supervise students. Interestingly, your latest Ph.D. student, Jim Schmidt, is in biophysics not statistics. You also organized a special year in statistics at MSRI (The Mathematical Sciences Research Institute) and established the Loève Prize. How did the Loève Prize come about?

Le Cam: Something had happened before that. One day in February 1979, Loève came to my office and said, “Here are the keys; if necessary give them to Line.” Then he disappeared and died a few days later without telling anybody that he was going to a hospital. Then at 3 AM on Saturday, I got a phone call from the Kaiser hospital, “Are you Professor Le Cam? Loève is dead.” Loève had a small tumor in the lung and he thought it would be better to remove it. He was recovering and chatting with a nurse, then all of a sudden he died of an embolism. I asked the hospital, “Can you keep the body there? We should inform Mrs. Loève.” She was living in France and was on vacation. I called Laurent Schwartz. Between Laurent Schwartz and Mrs. Schwartz, they located her. She said “I am here; I will be in Berkeley in two weeks.” I asked her “What about the body?” She said “Take care of it.”

Michel Loève was my colleague at Berkeley for a long time. I had met him and his wife, Line, in Paris before coming to Berkeley. In May 1992, Line phoned me from Paris saying she had metastatic breast cancer and that her end was near. She had some money that she wanted to donate to a good cause. She asked that I set up fellowships for graduate students in probability at Berkeley. The University is slow and it took most of the month of June to set up the Fellowships in a way that was satisfactory to Line and the University administration.

By that time Line was feeling much worse, barely getting out of bed. She still had some money and asked me to create an International Prize in Probability. That was for young researchers, past the Ph.D. but less than 45 years of age.

It was a major enterprise to get the University to agree, but we succeeded, just a few days before Line died, on July 28, 1992.

Line was a refined lady, very bright and totally indomitable. Why she selected me, who is just the reverse, to take care of her donations is a mystery. Anyway, she put me (or my designated successor) squarely in charge of both the Fellowships and Prize. Fortunately, she specified that I must appoint committees to help. That is particularly important for the Loève Prize. I appoint a committee of 5 or 6 people. Together we select a larger committee of about 30 people. Then we vote.

That happens every other year. We have had good luck so far, electing Aldous (1993), Talagrand (1995) and Le Gall (1997).

LEISURE READINGS

Yang: What do you like to read in your leisure?

Le Cam: I like to read French poetry: Marot, Ronsard, Baudelaire, but not the new stuff that has no rhythm and makes no sense. Some parts of Voltaire or Rabelais are pretty good. Some of Anatole France is great; I am thinking of *Penguin Island*, dear to Neyman, of the *Revolt of the Angels* and some more. I think I have three copies of *Penguin Island*, including one that Neyman willed to me. All three are in French. One has footnotes in Russian. We used to have one in English but Louise tossed it away. In English literature, I think that Shakespeare is a deadly bore, Dickens not much better, but a bit better. *Alice in Wonderland* is nice. I read pieces of it from time to time.

Yang: I remember *Penguin Island*. Neyman wanted me to read it and loaned his treasured book to me. But it was in French. So I bought an English translation and gave back his book. He was not too happy about that and said, "Your education will not be complete if you cannot read French."

Le Cam: At one time I liked to read plays in ancient Greek. I like the Chinese classics *Outlaws of the Marsh*, *Dream of the Red Chamber*, *Jin Ping Mei* and such. It is powerful writing that even survives translation. Unfortunately, I cannot read the original.

At one time I also read Bourbaki for pleasure, but they have not published in 20 years. I like to know how things work. So I read a lot about immunology. For example, I recently read a popular account of what people did at the National Cancer Institute. Some were very sleazy!

We subscribe to an infinity of magazines and journals. Occasionally one has a decent article.

On a different level, I read with pleasure *Un Mathématicien aux Prises avec le Siècle*. That is Laurent Schwartz' autobiography. It is not short, about 530 pages, but after you start reading, it is hard to stop.

Yang: I would like to ask you one very last question. This is a burning question from some of your former Ph.D. students. Many of us wonder that your office door was always open; you held office hours eight hours a day except when you were teaching or doing something. None of us ever saw you do research. So, when did you write your papers? What is your thought process when writing a paper?

Le Cam: You think about it for a while. Only when it is clear in your mind do you start writing, and that is it.

Yang: So by the time you start writing, you already have the paper in your head.

Le Cam: That is right.

Yang: That can speed up things!

Le Cam: Well, so you start writing. Too bad it does not go the way it was supposed to go. So you start over, maybe two or three times. But it's much more effective to think of the paper in advance, organize it in advance without writing. Sometimes you have to carry out some algebraic computations on paper before you can see what is going on. Typically, you don't have to. I should make an exception: my big book was rewritten perhaps 20 times!

Yang: That was a tremendous amount of work. Your big book is over 700 pages long. Not to mention the fact it is mathematically very difficult and condensed. Your book sets the course for mathematical statistics in the 21st century.

What are you working on lately other than our project of revising the little book [Le Cam and Yang, 1990]?

Le Cam: I would not want to set the course for the next century. Younger people will have to do that. For me, I am still trying to figure out what one can extract from the tangent spaces of Pfanzagl, or modifications of them. I am also trying to understand the functioning of sodium channels in nerves, as you know. However, just rewriting our little book takes a lot of time and energy, commodities that are increasingly in short supply, unfortunately.

Yang: Then, that is all the more reason to thank you for taking the time to have this conversation with me.

Le Cam: I enjoyed it very much.

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