Editors' introduction

The papers in this volume were contributed by the friends of Lucien Le Cam on the occasion of his 70th birthday, in November 1994. We wish him a belated happy birthday.

In addition to all the usual excuses for our tardiness in the preparation of the volume, we must point to the miracles of modern computing. As the old proverb almost put it: there's many a slip 'twixt \cup and \baselineskip. We beg forgiveness of any of our infinitely patient contributors who finds that the final product does not quite match with the galley proofs.

Our task was also made harder by the sad death of our friend and fellow editor, Erik Torgersen.

We greatly appreciate the editorial help of David Donoho with one of the more troublesome contributions.

In addition to the 29 contributed articles, we have included a short vita, a list of publications, and a list of Lucien's Ph D. students. We are also pleased that Lucien allowed us to include a private letter, written to Grace Yang, in response to a query about the extent of his formal mathematical training. The letter gives some insights into what made Lucien one of the leading mathematical statisticians of the century.

Grace-Lo Yang and David Pollard

Letter from Lucien Le Cam to Grace Yang

August 31st 1992

Dear Grace,

You made references to the fact that I don't have a formal mathematical education. This is true, and yet it is not. Writing after dinner and a glass of wine, I will try to fill in some of the details.

My elementary Math education was, I believe, standard for the country French of that time. We learned multiplication tables, up to 12×12, and learned to perform long division. We also learned a few things about geometrical shapes and their names, trapezoid, parallelogram, square, circle, etc. In the meantime the teacher had gotten a heart (a cow's heart) from a local butcher, and was teaching us about the functions of the four chambers. He also created some hydrogen from zinc and sulfuric acid and had an explosion.

By high school time, it was different. By the time I was 11 or 12 years old, I learned lots about cells, but cells in those days had no organelles, just a nucleus whose function was unknown. I learned also about Linnaeus classification of animals and that a lion and a cat are in the same family. Two years later, or so, we started on geometry, Euclidean type, similarity of triangles and such. The next year brought about Pythagoras' theorem. All of that was in the plane. We hit three dimensions only in my last year, 1941–42.

There we were subjected to an abundance of "conic sections". In the meantime, in the physics class, I had learned to solve linear differential equations. You need them to understand the simplest electric circuits and the form of a "catenary".

Some of my friends were stunning teachers by proving Bezout's theorem, but I was more interested in other things. We had just learned how to solve quadratic equations; I presented my teacher with what amounts to $\int 1/\sqrt{a+bx+x^2}\,dx$, from geometric considerations.

Also I found, by chance, that the roots of algebraic quadratic equations were given by periodic continued fractions. This and the quadratic integral earned me a trip to the house of l'Abbé Mirguet, a mathematician priest, kept out of the regular priesthood, who ruled that I had better read some books, which, of course, were not to be found around.

I did not have any bent for math, but was very interested, fascinated, by Chemistry and Physics. I remember reading avidly a chemistry textbook written around 1830. It was most fascinating. The author had HO as [the] formula for water, [and] did not believe much in Dalton's atomic theory. Besides, he had a few mistakes, like about ammonia gas dissolving in water and a wrong relation between the resulting volumes. I was able to disprove that in the chemistry lab, for which I had made a pass key. The Chemistry-Physics teacher found me there at odd times, but never said a word. Instead, he enlisted me to repair radio sets and burned out electric motors. He even asked me to construct a torsion balance, where to judge the tension of the wire, I had to use the pitch of a diapason. I broke some by tightening

too much. This was interspersed with requests that I should saw up to reasonable lengths his supply of firewood. Old apple trees can be hard to saw by hand.

On the final high school exam, I was asked whether electrons exist. My teacher did not believe in them. I said "yes" and was asked "but, pray, tell me in what direction they orbit the atom?"

By that time, I had bought books in a store that took refuge from the Germans. One was Thompson (??) about "valence". He computed forces between atoms by some exotic quantum mechanical technique. Another was a french translation of Max Planck lectures on electromagnetism. I still have that one. It was easy to read in 1940–41. I cannot read it now. The third book was lectures of Gauss on "Theorie der quadratischen Körper" which was impossible and boring to read. There was a fourth book, about what is now called "radar". It gave very detailed instructions about how to go about it. Since it had been written in 1937, I wonder what that means for the "inventors" of radar. Anyway, in 1942, the Thompson book and the "radar" book left me for undisclosed reasons.

The Max Planck lectures were full of "Curls" and "Divergences" and the like. I must have known or inferred what they meant. It certainly was not taught in my high school.

Going to Clermont-Ferrand in October 1942, I found I could not be accepted in the Chemistry program of the University. I was too late. They suggested I study math at the Lycée. Those kind people would even offer me board and room. It was too late, but they offered me my noon meal for two years. There we had 16 hours of math lectures and 7 of physics, 6 of chemistry every week. The math was mostly 1800 style for engineers. Lots of drawing curves. The physics had a lot to do with the hydrogen thermometer and the chemistry was to puzzle out what kind of chemical the teacher may have put in a solution that looked like copper sulfate, but was not. It turned out to be some sort of methylene blue.

The very idea of "vectors" and linear algebra had not hit those teachers, even though they used "vectors" to represent forces in mechanics.

Then, after some difficulties, I went to Paris where I enrolled in the "Calculus" class. This was taught by two people, Valiron and Garnier. Garnier had surfaces that rolled onto each other. Valiron had a combination real variables, complex variables, differential equations, partial differential equations, integral equation and calculus of variations. Unfortunately he mostly repeated his book word for word and I hardly ever went to the lectures.

The Lebesgue integral was barely mentioned. Valiron said: "It is very simple. Let us work instead on the improper Riemann integrals".

I passed, just by chance, the "Calculus" exam and the "Rational mechanics" exam. However I needed another "certificate". I had attempted to follow some lectures of Jean-Louis Destouches on quantum mechanics, but it was no hope. So, after some problems, I took the Statistics exam. Darmois was my examiner. He asked me to prove the multi-dimensional (matrix) version of Cramér-Rao. That was 1945. Cramér and Rao's papers were available in 1946. I did all right. I had never heard about matrices. It is true that Marcel Paul Schutzenberger had been holding seminars on van der Waerden's book on algebra, but he was a student, the crowd attending was uppity and I did not go.

I had more success with analysis. I had proved a few odd theorems, such as "If a set can be well ordered in two opposite directions, it is finite". I mentioned it to Colette Rothschild who said I better talk to "le Choc". That was Choquet. He was a student at the time and lived in a basement, working on industrial drawings. Choquet told me I better read de la Vallée Poussin's book on Baire classes.

Then I got employed by what was to be Electricité de France. That was a very nice employer, under the guidance of Pierre Massé for scientific ideas. He had anticipated much of Bellman's dynamic programming.

In the Spring 1947 Electricité de France told me I could take courses at the Université if I wanted. I took a course from Julia on Hilbert Space. He was a phenomenal lecturer (and a Nazi) but covered only "elementary Hilbert Space theory". I learned mostly that Hilbert norms have special properties and that Hilbert spaces have orthonormal bases.

That in a way, is the extent of my formal mathematical formation. But there is more. I had taken a "subscription" to the Bourbaki books and liked to read them. For my needs at Electricité de France I borrowed, and then bought, Watson's on Bessel functions. I did read with interest parts of Paul Lévy's book of 1937, "Addition of random variables". In a fit of despair about the Navier-Stokes equations, I

attended some of Leray's lectures on "fixed point theorems" at the Collège de France. We were worried about turbulence.

By 1947 Halphen persuaded me to publish a note about "characteristic functionals" in the Comptes Rendus. That was a bit miserable. After I came to Berkeley and met Bochner, he described it as "the work of an old man". I am sorry, I did not know any better. Once upon an occasion, the appointed speaker in Darmois weekly seminar did not show. So, being in charge of speakers, I spoke about Bochner's work on "Stochastic processes", because it was close to mine. Unfortunately, I could not answer several questions. Bochner had mentioned Kolmogorov's consistency theorem. I did not know what that was. Later on I was to present a paper of Barankin, under similar circumstances. It referred to the Hahn-Banach theorem. I did not know what that was. Somebody suggested I should look at Banach's book, but it was missing from the library. This may have been in 1949.

At the same time Edith Mourier who was writing her thesis on probability on Banach spaces was pressing me for instances of practical applications. Not even knowing what a Banach space was induced some difficulties for applications.

All of that changed when I came to Berkeley. I think Neyman hired me as an applied statistician. However Loève was lecturing on "measure theory", "Stochastic processes" and the like. Neyman was lecturing on uniformly most powerful unbiased tests. I had no idea what measure theory was about. My "practical" bent, from Electricité de France, made Neyman's lectures seem a bit spurious. Then I was assigned a topic for qualifying examinations: "Fixed point theorems". I went at it with a vengeance, reading most of Fundamenta Mathematicae, the Annals of Mathematics and such like, plus Saks "Integral", Kuratowski's topology and a few other things.

Then, in April 1951, I flunked my qualifying examination.

I will tell you more some other time like my brush with Kantorovitch, Vulich and Pinsker, but those were in Russian. I don't read Russian very well, if at all.

In those days, I could read, fast, very fast and I could remember. It is not sure how well I remembered. For instance I could remember in *English* stuff I read in Kuratowski in *French*, complete with the number of the page. The reverse occurred too.

. . . (the letter ends with some personal remarks)

Lucien

Biography of Lucien Le Cam
Personal Data

Date of Birth:	November 18, 1924
Place of Birth:	Croze Creuse, France
Citizenship	French

Education

1947	License es Sciences, University of Paris, France
1947-48	Graduate Studies, Sorbonne, France
1952	Ph.D., University of California, Berkeley, California

Positions Held

1945-50	Statistician, Electricité de France, Paris
1950-52	Lecturer in Mathematics and Research Assistant, University of California, Berkeley
1952-53	Instructor in Mathematics and Junior Research Statistician, University of California, Berkeley
1953-55	Assistant Professor of Mathematics, Statistical Laboratory, University of California, Berkeley
1955-58	Assistant Professor, Department of Statistics,

	University of California, Berkeley
1958-60	Associate Professor, Department of Statistics University of California, Berkeley.
1960–	Professor, Department of Statistics, University of California, Berkeley
1961-65	Chairman, Department of Statistics, University of California, Berkeley
1957-58	Fellow of the Alfred P. Sloan Foundation
Summers of 1958, 1959	
1971-72	Miller Professor, Department of Statistics, University of California, Berkeley
1972-73	Director, Centre de Recherches Mathématiques, Université de Montréal
1973–	Professor of Mathematics and Statistics, University of California, Berkeley

Professional Activities

1965	Editor (with J. Neyman) of Bernoulli-Bayes-Laplace Anniversary Volume, Springer, 1965
1967	Editor (with J. Neyman) of *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability, University of California Press, Berkeley,* Vols. 1-V, (1967), 3365 pages
1968–	Associate Editor, *Zeitschrift für Wahrscheinlichkeits-theorie u.v. Gebiete*
1968-70	Member of Council, Institute of Mathematical Statistics
1970-72	Editor (with J. Neyman and E.L. Scott) of *Proceedings of the Sixth Berkeley Symposium on Mathematical Statistics and Probability*, University of California Press, Berkeley
1973	President, Institute of Mathematical Statistics
1973	Member of Council, I.A.S.P.S.
1974–	Member comité aviseur du Centre de Recherches Mathématiques, Université de Montreal, Montreal, Canada
1974	Founder, Publications de la Chaire Aisenstadt, Les Presses de l'Universit de Montreal, 1974
1976	Elected to American Academy of Arts and Sciences
1977	Elected Fellow of the American Association for the Advancement of Science
1979–	Associate Editor, Polish Journal of Probability and Mathematical Statistics
1981	Associate Director, Statistical Laboratory, University of California, Berkeley
1982	Member, New York Academy of Science

Member of Professional Societies

International Statistical Institute

Institute of Mathematical Statistics
American Mathematical Society
American Statistical Association
International Chinese Statistical Association

Publications of Lucien Le Cam

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[3] "Les lois des débits des riviéres francaises", *Houille Blanche*, Numéro Special B (1949), pp. 733-740, (with G. Morlat).

[4] "On some asymptotic properties of maximum likelihood estimates and related Bayes' estimates", *Univ. Calif. Publ. in Stat.*,**1**, No.11 (1953), pp. 277-329.

[5] "Note on a theorem of Lionel Weiss", *Annals of Mathematical Statistics*, **25** (1954), pp. 791-794.

[6] "An extension of Wald's theory of statistical decision functions", *Annals of Mathematical Statistics*, **26**, No. 1 (1955), pp. 69-81.

[7] "On the asymptotic theory of estimation and testing hypotheses", *Proc. Third Berkeley Symposium on Mathematical Statistics and Probability*, University of California Press, **I** (1956), pp. 129-156.

[8] "A remark on the roots of the maximum likelihood equation", *Annals of Mathematical Statistics*, **27**, No. 4 (1956), pp. 1174-1177, (with C. Kraft).

[9] "Convergence in distribution of stochastic processes", *Univ.Calif. Publ. in Stat.*, **2**, No. 11 (1957), pp. 207-236.

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[81] Technical Report #170, Statistics, U.C. Berkeley.

[82] Technical Report #211, Statistics, U.C. Berkeley.

[83] C. Berkeley, August 1990. 39 pages.

[84] C. Berkeley.

In Preparation

[85] To be submitted (with Grace Lo Yang).

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Students of Lucien Le Cam

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Charles Hall Kraft,
On the problem of consistent and uniformly consistent statistical procedures, 1954.
Bayard Rankin,
The concept of sets enchained by a stochastic process and its use in cascade shower theory, 1955.
Georges Powell Steck,
Limit theorems for conditional distributions, 1955.
Thomas Shelburne Ferguson,
I. On the existence of linear regression in linear structural relations. II. A method of generating best asymptotically normal estimates with application to the estimation of bacterial densities, 1956.
Israel Jacob Abrams,
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Esary, James D.,
A stochastic theory of accident survival and fatality, 1957.
Charlotte T. Striebel,
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Lorraine Schwartz,
Consistency of Bayes' procedures, 1960.
Helen Wittenberg,
Limiting distributions of random sums of independent random variables, 1963.
Gian Domenico Majone,
Asymptotic behavior of Bayes' estimates in Borel sets, 1966.
Nora Snoeck Smiriga,
Stochastic processes with independent pieces, 1966.
Grace Lo Yang,
Contagion in stochastic models for epidemics, 1966.
David Gomberg,
Estimation of asymptotes, 1967.
Stephen Mack Stigler,
Linear functions of order statistics, 1967.
Min-te Chao,
Nonsequential optimal solutions of sequential decision problems, 1967.
Erik Nikolai Torgersen,
Comparison of experiments when the parameter space is finite, 1968.
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Pedro Jesus Fernandez,
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Arnoldo Guevara De Hoyos,
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Mohd Nawaz Goria,
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On the central limit theorem in $C(0,1)$, 1974.
Robert Henry Traxler,
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