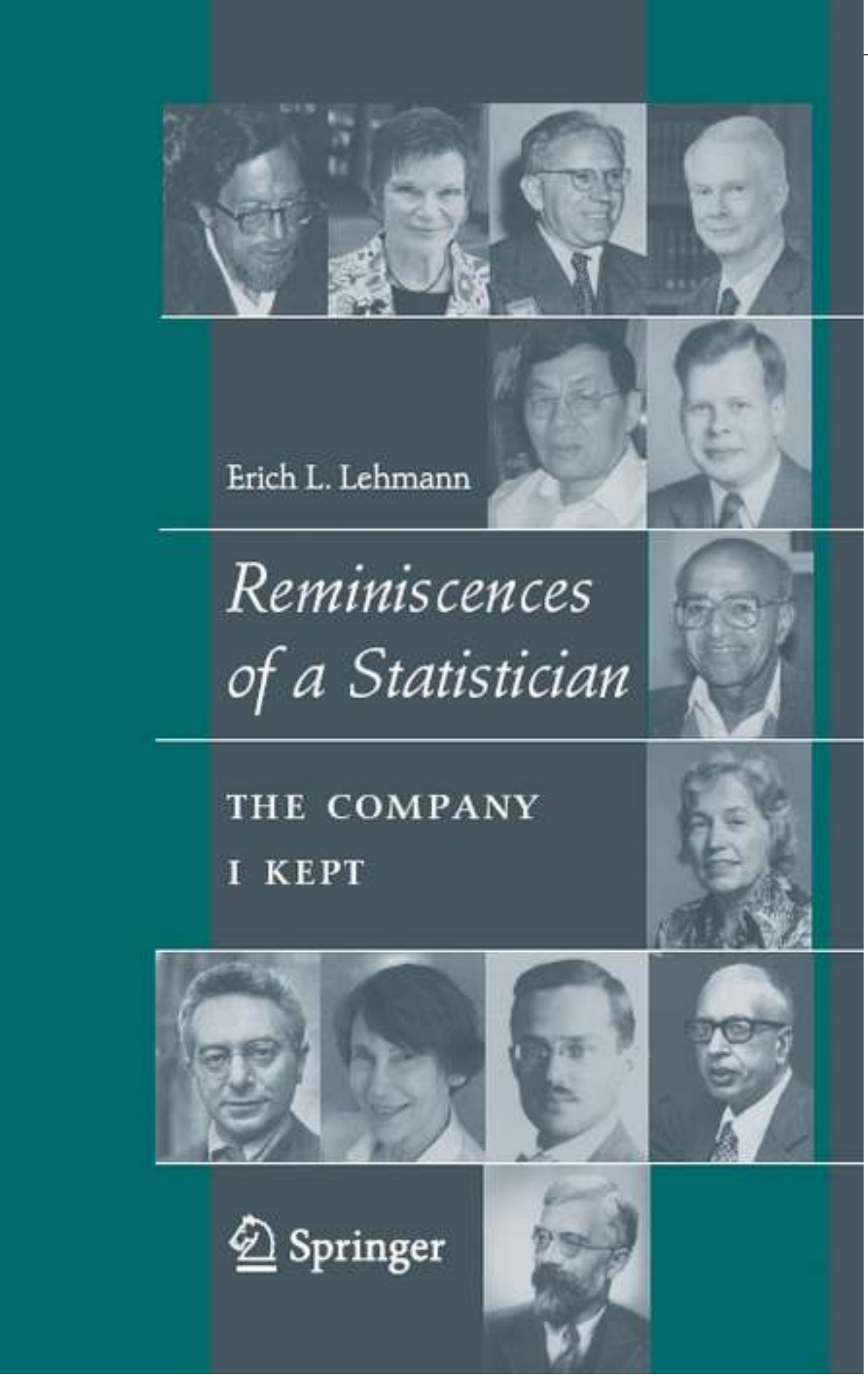
Erich L. Lehmann
Reminiscences of a Statistician
THE COMPANY I KEPT
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To our grandchildren

Joanna, Emily, Paul
Jacob and Celia
Gabe and Tavi
and great-granddaughter Audrey

Preface

It has been my good fortune to meet and get to know many remarkable people, mostly statisticians and mathematicians, and to derive much pleasure and benefit from these contacts. They were teachers, colleagues and students, and the following pages sketch their careers and our interactions. Also included are a few persons with whom I had little or no direct contact but whose ideas had a decisive influence on my work. To provide some coherence, the account is largely chronological and follows the steps of my own career.

Taken together, these sketches provide a very personal picture of the development of statistical theory from the 1930s to the 1970s. It is the period between two revolutions: that of Fisher, Neyman, and Pearson, which laid the foundations for the classical statistical theory of that period; and the second revolution, forty years later, brought about by the advent of the computer, which turned statistics in new directions.

The present account of this history is a highly selective one, which emphasizes the persons, institutions, and statistical topics that were close to my interests. One narrowing effect of this perspective stems from the fact that my career took place in the United States. As a consequence, the book focuses on American statisticians and institutions. Only the last two chapters discuss, briefly and very incompletely, developments in some other countries.

For writing these reminiscences, I did not have to rely entirely on my memory. There is much published material on many of the persons covered here, such as biographical sketches in Festschrifts and collected works, and—unfortunately—obituaries and memorial articles. Of particular value were the "Conversations," which are a regular feature of *Statistical Science,* and which provide firsthand accounts of the subjects being interviewed. An indispensable source for the Berkeley chapters was Constance Reid's book, *Neyman—from Life*.

In addition, I sent copies of their sections to all living subjects, asking them for corrections and criticism, and I am most grateful for their helpful responses. At my request, most of them also sent me pictures of themselves, which form an important part of the book. Other pictures were provided

by Steve Stigler (of Raj Bahadur and Jimmie Savage), and David Brillinger (of John Tukey).

Nearly 20 pictures were put at my disposal by Ingram Olkin from the extensive collection he has assembled at Stanford; another dozen I owe to the courtesy of the archives of the Mathematische Forschungsinstitut Oberwolfach, and still others to the archives of St. Andrews University. An important source for many pictures was the Berkeley Statistics Department, and four pictures came from Reid's book, *Neyman—from Life*. To all of these I extend my thanks. For preparing the pictures for publication, the help of Julie and Tamya Shaffer was invaluable.

I also want to thank Martina Schneider for helpful correspondence concerning the section on van der Waerden; to my editor, John Kimmel, for his encouragement and support; and to Agnes Herzberg for reviewing the book for Springer, and for many corrections and suggestions. To Len Shaffer, I am grateful for his typing of the manuscript from my hard-to-read handwritten version and for correcting many errors.

To conclude these acknowledgments, I want to express my deep gratitude to Persi Diaconis and Julie Shaffer, with both of whom I discussed the project as it went along, and who gave me advice and criticism when I needed it. They also read the manuscript after its completion, corrected many errors of fact, and greatly improved the exposition. To them I owe my greatest debt.

Contents

1
Mathematical Preparation

The crucial event of my early life was the coming to power in 1933 of the Nazis in Germany. This changed my future in two fundamental ways. As an immediate consequence, we left Germany (where my family had lived for many generations), and—after five years in Switzerland and two in England—I moved to America. On January 1, 1941, I arrived in Berkeley, California, where I have lived for the last sixty-six years.

The other basic change concerned not where I was going to live but what profession I was going to follow. My love as a teenager was German literature and, had we remained in Germany, I would have expected to become a professor of literature (or perhaps a writer). Since these were not promising professions outside of Germany, instead I became a mathematician and later a statistician.

Instrumental in both these changes was my father, who had the foresight to make the difficult decision to leave Germany quite early and who persuaded me that mathematics (for which I had shown some affinity) offered much better prospects than German literature because of its international nature.

From the start, the atmosphere in Berkeley was much more encouraging than it had been in Switzerland and England, where I had felt like a foreigner who would never be fully accepted. In Berkeley, on the contrary, I immediately felt at home. In addition, the way the study of mathematics was organized was much more congenial to me than it had been in England. Within a year, it seemed as if an academic career in mathematics was a realistic possibility.

1. Edmund Landau (1877–1938)

My interest in mathematics originated not at school, where the early courses in the subject seemed boring and my performance was mediocre, but rather from my reading a book, *Der Wettlauf mit der Schildkröte* (*The Race with the Tortoise*), by Th. Wolff. It was given to me by my uncle Alfred Schuster when I was thirteen. The title chapter discusses Zeno's paradox about Achilles and the tortoise, which claims to prove that the fast runner can never catch up

with the tortoise, who has been given a start on him. I found this intriguing, but what really captured my interest was the material on prime numbers.

It presented Euclid's proof of the infinity of primes and followed it with a section titled, "The Law of Prime Numbers," in which the question is posed of whether they follow some regular pattern. It mentions that the gaps between prime numbers tend to become larger as the numbers increase, but also that nevertheless from time to time prime twins continue to appear, such as (5, 7), (17, 19), and (101, 103). What was particularly fascinating was that at that time (the book was published in 1929), as is still true today, it was not known whether there exists an infinite number of prime twins.

If the impulse for mathematics is the desire to bring order into chaos, the prime numbers provide an ideal prototype because they combine extreme simplicity with behavior that is quite chaotic despite their obviously deterministic character. Today, we know much about their properties statistically—for example, they tend to get rarer and we know at what rate—but their local behavior is still completely unpredictable. To find a pattern in the sequence of primes became a great interest for me over the next few years, and I spent much time looking, calculating, and speculating. Two years later, as a high school sophomore, I was rewarded with what seemed a surprising discovery. It appeared that for any positive integer a and any prime number p, if you raise a to the p^{th} power and subtract a, the difference $a^p - a$ is always divisible by p.

On a vacation a few weeks later, it turned out that we were staying at the same hotel as Matthias Landau, the son of the famous number theorist Edmund Landau, whose wife had been one of my mother's closest girlhood friends. I mentioned my curious result to him, but he did not believe it, and bet me a chocolate bar that he would disprove it by the end of the day. He lost the bet but continued his efforts for another two days. He then decided to write to his father about the matter. The reply came that the result was well known as Fermat's little theorem, and that Landau would send me a proof.

In due time, his letter arrived. One would have expected it to start with an explanation—that he had heard from his son, etc., etc.—but explanations were not Landau's way. *"Sehr geehrter Herr Lehmann,"* the letter began (I was sixteen at the time), "all letters denote integers, p a prime number, x/y means x is a divisor of y," and after more notation came Theorem 1 and its proof and then Theorem 2 (Fermat), which was my result, and its proof. After this, the letter concluded: "With best regards, *unbekannterweise* [i.e., "without our having met"], E. Landau."

I did not understand one step in the proof, and in my thank-you letter I had the temerity to ask whether it did not contain a gap. By return mail came a postcard with his patient reply: "Thank you for your letter! There is no gap in the proof," followed by a slight elaboration on the point in question.

Later that year, my father asked me what I wanted to study after completing high school. The answer was obvious: My passion was German literature, my dream to become a writer, perhaps another Thomas Mann or Gottfried Keller. However, my father pointed out that Germany was barred to me (this was in 1935, two years after the Nazis had taken over Germany, and we were living in Zürich at the time), and that opportunities for German literature were extremely limited in Switzerland. He suggested that mathematics, for which I also seemed to have an affinity, was much more international in character and would provide much better career possibilities. I was used to taking directions from him and, without much inner turmoil, agreed to his suggestion. Thus, the crucial decision regarding the work in which I would spend my life came from the outside, rather than from within me. However, at this point it seemed a good idea to my father to get Landau's opinion regarding my aptitude for the subject. Because of my mother's friendship with his wife, it was not difficult for my parents to ask Landau to do this as a personal favor.

Accordingly, the next time he passed through Zürich, Landau came to our house to have a talk with me. His first words as I opened the door were: *"Machen Sie Ihre Eltern unschädlich!"* (Render your parents harmless; get them out of the way!) Next he asked me for some sheets of paper, as large as possible (the best I could produce were still not considered satisfactory but had to do). Then he withdrew with me to my room and told me about some recent results of a young Hungarian mathematician, Paul Erdös, of whom he thought very highly.

As an aside, let me mention that his assessment of Erdös, who at the time was in his early twenties, turned out to be well founded. Erdös became an outstanding, highly influential, mathematician with a phenomenal number of more than 1,500[1] wide-ranging publications, many of them written jointly with others. His collaborators eventually came to more than 450, and it became a game among mathematicians to establish their "Erdös numbers." This number was equal to 1 for anyone who had written a joint paper with Erdös; it was equal to 2 if one had written a joint paper with someone who had written a joint paper with Erdös, and so on. I have the proud distinction (which, however, I share with more than five thousand others) of my Erdös number being 2, since one of my coauthors is my friend Persi Diaconis, who once wrote a joint paper with Erdös.

But back to my session with Landau. I recall his hammering home the point, after showing how Theorem B followed from Theorem A, which he had not proved (it was too advanced), that of course I had not seen a proof of Theorem B. After he had worked with me in this way for two hours, he closeted himself with my parents and apparently recommended that math be given a try.

Many years later, I found among my mother's papers a letter from Landau's wife, Marianne. She said that she was glad that the interview went well, and that if Eddy (Landau's nickname) had given his blessings, my mother could be assured that it was okay, since she knew how many beginners he had discouraged.

The aftermath of this visit must be seen against the background of Landau's situation at the time. In the previous year, a few months after Hitler's ascent to power, he one day found the door to his lecture room in Göttingen blocked by protesting students (reinforced by some Nazi storm troopers). He asked the leader of the group, Oswald Teichmüller, to come with him to his office and explain the objections to his lecturing. At the end of the conversation, Landau requested a summarizing letter that he could use for official purposes; the next day he resigned his professorship, twenty-four years after he had first been appointed to it.

Surprisingly, a copy of Teichmüller's letter, which had long been believed to be lost, turned up a few years ago. Details are given in Schappacher and Scholtz (1992). The following is the central paragraph in the translation of Segal (2003):

> It was for me, not about making difficulties for you as a Jew, but solely about protecting German students in their second semester from being instructed by a teacher of a completely foreign race precisely in differential and integral calculus. . . . I dare as little as any other person to doubt your capability for pure international-mathematical-scientific teaching of suitable students of whatever heritage. However,

[1] These and the following figures are given in Schechter's book about Erdös, *My Brain Is Open*.

I also know that many academic lectures, especially also differential and integral calculus, at the same time have educational value and lead the student not only into a new conceptual world, but also to a different mental viewpoint *(geistige einstellung)*. Again since the mental viewpoint of an individual depends on his mentality *(geist)* which thus should be transformed; this mentality, again, according to fundamental rules, not only contemporary ones, but already long recognized, depends completely substantially on the racial composition of an individual; allowing Aryan students to be educated by a Jewish teacher, for example, ought not in general be recommended.

G.H. Hardy, in his obituary of Landau, wrote: "This enforced retirement must have been a terrible blow to him: it was quite pathetic to see his delight when he found himself again in front of a blackboard in Cambridge, and his sorrow when the opportunity came to an end" (Hardy and Heilbronn, 1938).

It is undoubtedly due to his enforced idleness that I had the privilege of his coming to our house to give me a lesson (perhaps three or four in all) whenever he passed through Zurich. On the second of these visits, he asked me what I was doing mathematically. When I told him that I was trying to learn calculus by reading Courant's book on the subject, he became very concerned. "Courant is a good friend of mine and an excellent mathematician," he said, "but he does not understand pedagogy. His book is poison for you; it will teach you sloppy thinking. If you want to learn calculus, you should do so from my book." So I dutifully bought his book, but it was written in the famous uncompromising Landau style: definition, theorem, proof; theorem, proof; . . .; without giving the reader much help by providing motivation, intuition, or geometric illustrations. Concerning the latter, Landau once explained to me that he knew geometry and had even taught it at Göttingen, but that it had no place in a calculus book since the necessary axiomatic foundation could not be given there. (It is not surprising that Landau's dry and formal way of teaching calculus had been unpopular with the students in Göttingen. Only, of course, it had nothing to do with his Jewishness. Courant was just as Jewish and his approach to calculus was the opposite of Landau's: intuitive and with much help from geometry.)

On his later visits, Landau increased the level of his mathematical discourse, and the level of my understanding decreased correspondingly. After a particularly hesitant "yes" of mine, he broke off. "Mr. Lehmann," he said, "I notice that you use three types of yes: a comprehending yes, a somewhat doubtful yes, and a noncomprehending yes. Of which kind was this last one?"

Though admired not only for his mathematical achievements but also for his honesty and integrity, Landau was considered "difficult" and was feared for his biting wit. I cannot resist giving at least one example of the latter. He had proved an important inequality that involved a universal constant—in German, *Welt Konstante* (world constant). A few years later, another mathematician was able to substantially improve Landau's result by showing that it remained valid when the constant was halved. Landau fired off a terse telegram: *"Gratuliere zur Halbweltkonstante!"* (Congratulations on the

half world constant.) The joke lies in the two ways of reading this triple word. As *halb Welt-Konstante,* it means half the universal constant, but *Halbwelt* (in French *demi-monde*) indicates shadiness, ill repute. Thus: "Congratulations on your disreputable constant!"

Another example of Landau's quirky sense of humor occurs in the preface to his 1930 book on the foundations of analysis. He mentions there that the book was written partly for family use, "since—as is well known—my daughters have been at the university for several semesters, . . ." Well known? In explanation, Landau gives a reference to volume one (p. v) of his three-volume work on number theory. The dedication to this book, published in 1927, reads: "To my daughters on the day of their high school graduation."

What Landau meant to those who knew him best is indicated in a letter of condolence written to his widow by Hardy[2]:

> I suppose that his reputation stood higher in England than anywhere else in the world. It was high enough everywhere. Even his enemies could not deny that he was a great mathematician; but we owed more to him than the rest. And we admired him as a person almost as much as we did as a mathematician. We loved his directness, and his "100 percent" honesty. All his little eccentricities, his peculiar humour and his individual likes and dislikes, were entirely sympathetic to us, and made him a sort of tradition: the Landau way of writing and the Landau jokes were familiar to all sorts of people who had never met him. But for those like Littlewood and myself, who really knew him and owed so much to him, he stood, naturally, for a great deal more, and we feel that we have lost one of the best friends we ever had.

To me, in the short time I knew him, Landau was unfailingly patient, considerate, and kind, showing a gentle side which in other, more competitive, circumstances he may not always have wanted seen. The last communication I received from him, a little more than a year before his death, was a postcard:

> Dear Mr. Lehmann, since around now the conclusions of your graduation examinations are due, I should like to ask you to let me know the results. Although I have no doubt as to the success, I am curious to hear how well you did and how the report on mathematics was formulated.

Some time in 1937, my mother was in Berlin and on the occasion paid Landau a visit. He was separated from his wife (although they were on friendly terms), without a position, and depressed. He told my mother that he was having heart trouble but asked her not to tell his wife. Soon after, he suffered a fatal heart attack. At the age of sixty-one, this dynamic man, who had always been so full of vitality, died—figuratively and literally—of a broken heart.

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[2] A portrait of this great mathematician is provided in C.P. Snow's *Variety of Men* (1967).

2. Rolf Noskwith (b. 1919)

After five years in Zurich, my parents and I came to the conclusion that Switzerland did not offer good prospects to a young German refugee and that perhaps I should continue my study of mathematics elsewhere. Thus, in the spring of 1938—as it turned out shortly before his death—we once more asked Landau for advice. He unhesitatingly recommended Cambridge, with the stars Hardy and Littlewood and an outstanding group of younger mathematicians such as Burkill, Heilbronn, and Ingham, as the best place to get a first-rate mathematical education. So in the fall of that year, I became a student of mathematics at Trinity College, Cambridge.

It was a difficult time for me: my English was rudimentary, and the organization of the mathematical curriculum at Cambridge, with its emphasis on physics and astronomy (considered applied mathematics), was not well suited to my interests and abilities. I felt lonely, but was rescued from my isolation in this new environment by a fellow student, Rolf Noskwith. A son of Polish immigrants, with a knowledge of German and similar interests to my own, Rolf soon became a friend. I saw him nearly daily in his room at Trinity, we worked together, and he helped me better to adjust to English ways.

Two years after entering Trinity, it became clear to me that I could not be successful there. In the meantime, the war had started in Europe, and the

threat of a German invasion of England seemed very real. I was not so very much worried about being hit by a bomb, but under no circumstances did I want to fall into German hands. So in the summer of 1940, I decided to leave England and continue my studies in the United States.

For a while, Rolf and I kept in touch through correspondence, but eventually this petered out. Only much later did I discover what was probably the main reason for this break. In June 1941, Rolf joined British Intelligence at Bletchley Park, where in Hut 8 he became a cryptanalyst and was part of the team that broke Naval Enigma, the code of the German Navy. This supersecret work was both very demanding and could not be written about, so correspondence became difficult.

When he left Cambridge, Rolf had already decided not to become a mathematician. "When the war ended," he writes in an account of his work at Bletchley Park,[3] "I could not tear myself away from decoding and spent a further year working on other ciphers. When I finally left to join the family business created by my father, I made sure that I could come back if a six months' trial did not work out. The option was unnecessary: I am still involved in the business."

What Rolf does not say is that over the years he greatly expanded the textile business that his father had started and made a spectacular success of it.

How do I know this, if our connection ended during the war? About fifty years later, I received a phone call asking me whether I was the E.L. Lehmann who had been a student in Cambridge in the late 1930s. It turned out to be Rolf, who was in San Francisco on business. My wife, Julie, and I had reservations at a restaurant for dinner that evening and tickets for the opera. He was an opera fan; we were able to get another ticket and he joined us.

Unlike I, who had become bald and grown a beard, Rolf had changed very little, and in the slightly stooped man of seventy I instantly recognized the student from half a century before. As a result of this renewed encounter, two years later when Julie and I were in England, we stayed for two days with him and his wife, Annette, and had a wonderful time hiking and catching up. Since then we have kept in contact.

3. Richard Courant (1888–1971)

After having lived in Germany for fifteen years, followed by five years in Switzerland and two in England, finally—in November 1940—I reached what was to be my ultimate destination, the United States. It was my intention to continue studying mathematics (which I had begun in Cambridge), but at what university? When I left Zürich, Landau had recommended Trinity

[3] For details of this work, see Noskwith (2001).

College, Cambridge, where I had studied for two years. Landau was no longer alive, but his widow had given me a letter of introduction to his Göttingen colleague, Richard Courant, who was now at New York University, where he was building up what was eventually to become the Courant Institute.[4]

Courant's name, of course, was very familiar to me, since my exposure to calculus had been through his book, against which Landau had warned me so sternly. When I told Courant my problem, he began by asking me whether I wanted to go to New York or the United States of America. I had no idea what he meant (only later did I realize that many refugees preferred New York, where they had relatives and friends and the emotional support of a large refugee community), but I said that I had no particular interest in New York. He then suggested Berkeley in California, a place and university I had never heard of. "It is an up-and-coming university," he added, "and I think you will like it."

Courant knew Berkeley. He had spent the summer of 1939 there as visiting lecturer, and he was right on both counts. Berkeley was an up-and-coming

[4] The life of Courant is recounted in Constance Reid's book, *Courant—in Göttingen and New York*"

university, in particular its mathematics department would soon become one of the leading departments in the country, and yes, I did like it—in fact, it was love at first sight.

Courant, undoubtedly as a result of Marianne Landau's letter, was very kind to me. For one of the few days remaining before I started for California, he invited me to dinner at his house in New Rochelle. The only thing I remember of that evening was that another guest was a young mathematician, Herbert Robbins, not much older than myself. At the time, he was working with Courant on their highly successful book, *What Is Mathematics?* Later he would become a statistician, and I had frequent contact with him.

I never saw Courant again after that evening, but his unorthodox recommendation of Berkeley had a determining influence on my life. Berkeley became home for the rest of my life, and was responsible for a change of profession, from mathematics to statistics. Courant over the years helped many others, but I for one owe him a great debt of gratitude.

4. Griffith C. Evans (1887–1973)

Upon arriving in Berkeley on January 2, 1941, my first task was to see about enrolling at the university as a student of mathematics. Accordingly, I went to the office of the mathematics department (then on the fourth floor of Wheeler Hall), where the staff at that time consisted of one half-time secretary, Sarah Hallam, a graduate student who was the receptionist and typed the chairman's letters. She told me that the chairman, Professor Evans, was in his office and could probably see me. Evans was welcoming and friendly when I introduced myself. The question of my status was quickly settled: although I did not have a degree, he thought that my two years at Cambridge were equivalent to an American B.A. degree, and to my amazement suggested that I start as a graduate student on probation.

What a breath of fresh air! In Cambridge I had been accepted through connections, as a favor to a high government official who was a friend of my uncle. Here I brought no letter of introduction, or even a transcript attesting to my Cambridge courses. Evans took my statements on trust. He did not ask who my parents were or whom my uncle knew. America was reported to be the land of opportunity, and now I was experiencing it firsthand. What a wonderful beginning to my American life.

I am still enormously grateful for the warmth and generosity with which Evans received me. However, I now see that there was also another reason for his unexpected behavior. Undoubtedly, at that time it was not easy to attract good graduate students. Here came this fellow Lehmann with a Swiss high school education and two years of study at Cambridge—I must have looked like a good bet.

Evans continued to keep me in mind. After I did well during the first semester, he lifted my probationary status and I became a regular graduate

student. A few weeks later, to my great surprise, he offered me a teaching assistantship. I also had a good chance of a fellowship, which would have provided the same stipend without any duties. However, without hesitation I accepted Evans' offer, since it would make me a member of the staff. It was important for me not to be an outsider but to become part of the departmental community.

The department of which I so unexpectedly had become a member was still fairly new.[5] In the early 1930s, it had fallen into disrepair. Its faculty was no longer doing research or keeping up with the rapid development of mathematics. The situation had become so damaging that other science departments were complaining to the administration, and a search committee was appointed to find a new chairman who would revitalize the department.

The committee selected Griffith C. Evans, then one of America's leading mathematicians. He had broad mathematical interests and had made fundamental contributions to potential theory and the plateau problem, as well as to the quite-unrelated area of mathematical economics.[6] In 1916, he was the colloquium lecturer of the American Mathematical Society, and he had just

[5] For a history of the Berkeley mathematics department, see Moore (2007)
[6] For a discussion of Evans' work in this area, see Weintraub (2002).

been elected to the National Academy of Sciences. Since coming to Berkeley in 1934, Evans had made many new appointments. He had added three outstanding mathematicians at the professorial level, and four promising younger men who between them covered an array of different specialties.

As a result, it was a very attractive and vigorous department that I was now joining. The atmosphere was bracing, confidence was in the air, and self-doubt not encouraged. It was assumed that you could do whatever was required; if you needed help, you could ask for it. A good example of this attitude was my first introduction to teaching, that fall.

At the time, the mathematics department employed seven teaching assistants, who—despite this title—did not assist faculty members with their courses but instead taught their own, quite independently. This included both the examinations and the assignments of grades. We shared a large office in the basement of Wheeler Hall and also a great luxury: a reader—an elderly, gruff, and taciturn man who corrected all homework papers.

When Professor Evans informed me of my appointment, he added that my assignment would be a section of analytic geometry and that the professor in charge, Professor Sciobereti, would provide me with further details. When I looked up Professor Sciobereti, he told me the title of the textbook—all sections were to use the same text—but that was the total amount of coordination. He also told me that the course would meet Monday, Wednesday, and Friday at 9:00 a.m. in Room 210 of Wheeler Hall, and then he wished me good luck. This was all the instruction I received, although I had no experience with the system. Nor was there any kind of supervision during the semester.

Toward the end of that first semester of teaching came the Japanese attack on Pearl Harbor and America's entry into the war. Soon the army asked the university to run a training program for meteorology recruits, for which I became an instructor. But the summer of 1942 brought a bigger change for me, of which Evans once more was the agent. He expressed to me his belief that I could be more useful to the war effort if I switched from pure mathematics to either physics or statistics. I greatly respected his judgment and felt that if possible, I should follow his advice. Physics had been my undoing at Cambridge and seemed out of the question, but I knew nothing about statistics, and did not even realize that there was such a subject. I agreed to give it a try.

For this purpose, Evans told me to see Professor Neyman, a member of the mathematics department whom I did not know. Neyman seemed pleased to get a new student, and our meeting resulted in the focus of my studies shifting to statistics. From then on, I had much less contact with Evans, and saw him mainly on occasions involving the whole department. One event of this kind has remained in my memory. It showed how modest Evans was despite his eminence.

When a new building was authorized to house the mathematics and statistics departments, it was decided to name it Evans Hall, and the artist Erle

Loran was commissioned to paint an official portrait of Evans. (It now hangs in the mathematics and statistics library on the ground floor of Evans Hall.) At the dinner celebrating the unveiling of the portrait, after a talk by Charles Morrey, Evans rose to reply.

His opening sentence was rather startling: "Who was Bacon?" he asked. He then proceeded to list several Bacons. "Was it Francis Bacon, who wrote the Novum Organum? No! Or the thirteenth-century philosopher and scientist Roger Bacon? No! Or perhaps the Irish painter Francis Bacon? No! But who was the Bacon after whom Bacon Hall [a campus building] is named?" And with this he sat down.

During the two years of Evans's tutelage, he became a father figure to me. His unvarying kindness and concern during a short, but for me crucial, period profoundly influenced my life, and he has served as an inspiring role model.

5. Raphael Robinson (1911–1995) and Julia Bowman Robinson (1919–1985)

In 1941, when I became a student in the Berkeley mathematics department, some of the older members from the pre-Evans era were still teaching, but I had relatively little contact with most of them. An exception was John McDonald, a sweet, rather shy man with a lovely sense of humor. When lecturing, he would arrange things so that at the end of the hour he stood close to the door. This enabled him to slip out after his last sentence without having to answer any questions. But once, in the last lecture before the final examination, he did not escape in time. "Will there by any choice questions