

Halsey Royden has had a lifelong connection with Stanford University. His maternal grandmother enrolled with Stanford's first class in 1892, and two uncles graduated before 1920. After receiving a B.S. from Stanford in 1948 and an M.S. in 1949, Royden went to Harvard University to study under Lars Ahlfors' direction, obtaining a Ph.D. in 1951. At that point he joined the Stanford faculty. He has since served as dean of the school of humanities and science. He is known for his research in complex analysis and differential geometry, and is the author of a widely-used textbook, Real Analysis.

A History of Mathematics at Stanford

HALSEY ROYDEN

The older history of the Stanford Mathematics Department may be divided into a number of periods: Early Years, the Blichfeldt Era, the Szegő Period, and the later fifties and early sixties. I transferred to Stanford as an undergraduate in 1946, received my B.S. in 1948, my M.S. in 1949, and returned to Stanford as a faculty member in 1951, after a brief period of exile at a small college on the banks of the Charles. Memory in human institutions is transmitted not only by the written archive but also by direct transference of living memory from one generation of colleagues to the next. Those long associated with an institution come to have memories of the events before their time that are only slightly less vivid than those they have actually experienced. Although I arrived at Stanford in the middle of the Szegő era, memories were still current from the earlier time, when H. F. Blichfeldt dominated the Stanford department. My account of the course of Stanford's mathematics department utilizes material gleaned from the archives, together with memories of my own experiences, as well as those recounted by my colleagues, particularly Harold Bacon, Don Spencer, and Jack Herriot. I have taken some of the descriptions of former faculty members from Memorial resolutions of Stanford's Academic Council. I am especially indebted to Harold Bacon for his assistance in preparing this history.

My professional life for the last forty years has been involved with Stanford affairs, and I hope the reader will pardon me if this history sometimes assumes the character of a personal memoir. It is often difficult to view

recent events with a proper historical perspective, and I have therefore not included events of the last twenty years, although this has meant neglecting the rise of differential geometry at Stanford with the appointments of Yau, Simon, Siu, and recently of Schoen and numerous younger people.

1. THE EARLY YEARS

Instruction at Stanford began with the Academic Year 1891–1892. The senior faculty in mathematics, both in the early days and for a long time thereafter, consisted of Professors Robert Allardice and Rufus Green. They were both in their late twenties when they came to Stanford, and though neither had a Ph.D., they were established mathematicians.

Allardice was a professor in Stanford's original faculty of 1891–1892. He had received his A.M. from Edinburgh in 1882 and was a student of Chrystal. His assistance is noted in the preface to the second edition of Chrystal's *Algebra*. At Edinburgh, Allardice was Baxter Scholar in Mathematics '82–83, Drummond Scholar in mathematics for '83–84, and assistant professor of mathematics from 1884 until he came to Stanford.

Rufus Green received his B.S. from Indiana in 1885 and his M.S. in 1890. He was an instructor in mathematics at Indiana in '85–86, a graduate student at Johns Hopkins for '86–87, and a professor of pure mathematics at Indiana from 1887 to 1893, when he came to Stanford as an associate professor. Stanford's first president, David Starr Jordan, had been a professor at Cornell and president at Indiana before coming to Stanford. He recruited most of Stanford's first faculty from among his former colleagues at Cornell, and many of the rest from Indiana.

Hans Frederik Blichfeldt, who was to play such a large role in the history of mathematics at Stanford, began his undergraduate work at Stanford in 1894, and he received one of the three A.B.'s awarded by the Stanford mathematics department in 1896. He was appointed instructor in mathematics, and received his A.M. in 1897. He was reappointed instructor again in 1898, after receiving his Ph.D. from Leipzig. Blichfeldt was born in Denmark in 1873. His father emigrated to the United States in 1888, but before leaving, the young Blichfeldt had taken, and passed with high honors, the general preliminary examination conducted at the University of Copenhagen. In his early school career he had constantly excelled in mathematics, and by the time he took this examination at the age of fifteen, he had discovered by himself the solutions of the general polynomial equations of the third and fourth degrees — a remarkable performance for a schoolboy.

Coming to the United States, he found employment in Nebraska, Wyoming, Oregon, and Washington where he worked at various jobs in the lumber industry. During the four years from 1888 to 1892 he “worked with my hands doing everything, East and West the country across” (quoted by L. E. Dickson). Then for the two years 1892–1894 he worked as a draughtsman for the engineering department of the City and County of Whatcom, Washington, where his unusual mathematical talent began to come to the attention of his employers and fellow workers. Although he had not pursued a formal high school education in this country, he was persuaded in 1894 to apply for admission to Stanford. His application for admission was supported by a letter to Stanford’s President Jordan from the County Superintendent of Schools of Whatcom County, who wrote, “He is about 21, of most exemplary physique and morals, evidently cultured in his native tongue and fairly proficient in English. He is a real genius in mathematics — working intuitively, to all appearance, abstruse integral calculus problems . . . In short all who know him here look upon him as a genius — if he might have advantage of training . . . ” The Stanford Registrar was uncertain how to credit the examination Blichfeldt had taken at Copenhagen in 1888, but, nevertheless, admitted him as a special student in September of 1894. In January of 1895 he was granted “full entrance standing, except for English 1b, on the basis of work done before entering the University,” and a month later he was granted an additional credit of sixty hours toward graduation.

At this time Stanford followed a “free elective” system somewhat similar to Harvard’s, and this was particularly well adapted to a young man of great mathematical ability and originality. He received his A.B. degree in mathematics in 1896 followed by the A.M. in 1897. Apart from courses in English, German, and physics all of Blichfeldt’s courses were in mathematics and included: calculus, quaternions, higher plane curves, differential equations, analysis, solid geometry, and invariant theory as an undergraduate, and projective geometry, curve tracing, vector analysis, theory of functions, and theory of substitutions as a graduate student.

At that time German universities were centers of great mathematical activity, and Blichfeldt was determined to go to Leipzig and study under Sophus Lie. His Stanford friends, and particularly Professor Rufus Green, urged and encouraged him to do this. Although his three years at Stanford (which at that time charged no tuition fees) had been financed by savings painstakingly accumulated during his earlier years of hard work, he held a teaching assistantship in 1896–1897, and Professor Green saw to it that he could borrow what was needed to make the European venture possible. He spent the year working with Lie, mastering the “Lie Theory” of continuous groups, and writing his dissertation “On a certain class of groups of transformations in

space of three dimensions," (*Amer. J. Math.* **22** (1900) pp. 113–120). He was awarded the Ph.D., *summa cum laude*, in 1898.

He returned to Stanford as an instructor in mathematics in 1898 and remained a member of Stanford's faculty until his retirement in 1938. He was assistant professor of mathematics, 1901–1906; associate professor, 1906–1913; professor, 1913–1938; Professor Emeritus, 1938; until his death in 1945. He was executive head of the department from 1927 until 1938. In 1911 he was associate professor of mathematics at the University of Chicago, summer quarter, and professor of mathematics at Columbia for the Summer Sessions of 1924 and 1925.

Blichfeldt made contributions of lasting and fundamental importance to the theory of groups and to the geometry of numbers. In the former he solved the problem of finding all finite collineation groups in four variables, a problem whose solution eluded Klein and Jordan. In the latter he determined the precise limits for minima of definite quadratic forms in six, seven, and eight variables. In addition to some two dozen research papers of importance, he was the author of *Finite groups of linear homogeneous transformations*, which forms part two of the book *Theory and Application of Finite Groups*, by G. A. Miller, H. F. Blichfeldt, and L. E. Dickson, 1916. He was also the author of the book, *Finite Collineation Groups*, 1917.

Blichfeldt was a member of the American Mathematical Society and served as its vice-president in 1912. He was also active in the affairs of the Mathematical Association of America. He represented the United States officially at two International Mathematical Congresses, was elected to the National Academy of Sciences in 1920, and served as a member of the National Research Council in 1924–1927.

When Blichfeldt was promoted to an assistant professorship in 1901, he was joined as assistant professor by George Abram Miller, who had been at Leipzig while Blichfeldt was there. Miller later had a distinguished career at the University of Illinois. Miller's curriculum vitae indicates what the career of a mathematician was like in those days:

George Abram Miller:

A.B., Muhlenberg College, 1887;

Ph.D., Cumberland University, 1892;

Professor of Mathematics, Eureka College, 1888–1893;

Instructor in Mathematics, University of Michigan,
1893–1895;

Student, Universities of Leipzig and Paris, 1895–1897;

Instructor, Cornell University, 1897–1901;
 Instructor in Mathematics, University of Chicago,
 summer 1898;
 Assistant Professor, Stanford, 1901–1902;
 Associate Professor, Stanford, 1902–1905.

The department awarded its first Ph.D. to William Albert Manning in 1904. The department also awarded a B.S. that year to Eric Temple Bell, who, two years earlier, had been admitted and granted sixty hours toward graduation on the basis of previous work at the University of London. Except for several courses in education during his last year, all of his courses were in mathematics. The courses he took, some of which were taught by Blichfeldt and Miller, seemed to be somewhat more substantial than those offered to Blichfeldt eight years earlier. They included: determinants, advanced co-ordinate geometry, calculus, differential equations, solid geometry, theory of equations, theory of functions, theory of numbers, and history of mathematics in his junior year, and advanced calculus, theory of groups, continuous groups, and a second course in the theory of functions his senior year.

A department of applied mathematics had been established in the meantime with Professor Hoskins as its only member. In 1902 he was joined by Halcott Cadwalader Moreno and William Albert Manning as instructors. By 1907 they had become assistant professors and were joined by S. D. Townley, who had received his Sc.D. degree in astronomy from the University of Michigan in 1897. By 1925 all three had risen to the rank of professor. At this time Hoskins retired, and applied mathematics was merged with pure mathematics to form a single department.

The year 1921–1922 also saw the arrival of the first formal summer visiting professor with the appointment of G. D. Birkhoff. The faculty roster for the two departments was the following:

Mathematics:

Allardice, Green, Blichfeldt, *Professors*;
 G. D. Birkhoff, *Visiting Professor (Summer)*;
 H. W. Brinkman, *Instructor (Summer)*;
 Dorothy Crever, W. W. Wallace, *Assistants in Instruction*.

Applied Mathematics:

Hoskins, Manning, Moreno, Townley, *Professors*;
 L. G. Gianini, *Instructor*;

F. E. Terman, H. E. Wheeler, *Teaching Assistants*.

The 1921–1922 list of teaching assistants in applied mathematics contains the name of Frederick Emmons Terman. Terman was later to become chairman of electrical engineering, dean of engineering, and, ultimately, provost at Stanford. He was always interested in the affairs of the mathematics department, especially the teaching of mathematics to engineering students. When I was an associate dean of humanities and sciences in the early sixties and Fred Terman was provost, I sometimes found it difficult to get on his crowded calendar. If I mentioned anything about our engineering calculus course, however, Terman would hold forth for an hour or so with advice and comment, drawn partly from the time when he taught sections of it as a teaching assistant.

2. THE BLICHFELDT ERA

With the retirement of Hoskins in 1925, the department of applied mathematics was discontinued, and Professors Manning and Townley became members of the department of mathematics. Professor Townley was an astronomer, who had been at Stanford since 1907. Professor Manning, who had been Stanford's first Ph.D. in mathematics, was, like many of his colleagues at Stanford, interested in group theory. Several of his children received Ph.D.s from Stanford: His son Lawrence was for many years a professor of electrical engineering at Stanford. His daughter Rhoda was a professor of mathematics at Oregon State, and I took a course from her on modern algebra during my junior year, when she was a visiting faculty member at Stanford. Another daughter, Dorothy, was a professor of mathematics at Wells College.

A long tradition at Stanford in mathematical statistics and mathematical economics began in 1925, when the roster of the department listed Harold Hotelling as "Junior associate, Food Research Institute." The Food Research Institute had been established at Stanford a few years earlier by Herbert Hoover, and its faculty and staff consisted mostly of economists and statisticians. Hotelling, one of the founders of mathematical statistics in America, received his A.B. from Washington in 1919; his M.S. in 1921; and his Ph.D. from Princeton in 1924. He was an instructor at Princeton, 1922–1924; a junior research associate at Stanford's Food Research Institute, 1924–1926; research associate, 1926–1927; and associate professor of mathematics at Stanford until 1931, when he left to become a faculty member at Columbia. He went to North Carolina in 1946.

In 1928 my long-time colleague Harold Bacon received his bachelor's degree from Stanford. Professor Bacon, who provided the principal direction for Stanford's undergraduate program in mathematics until his retirement in 1972, always seemed to his students and fellow faculty members as the

embodiment of Stanford ways and history. Through his teaching of the calculus over many years, he probably taught more Stanford undergraduates than anyone else in the history of the University. Often in my travels I would meet a Stanford alumnus and mention that I taught mathematics at Stanford. Invariably, the response would be, "Oh, then you must know Professor Bacon."

The following year Bacon received a master's degree, writing his master's thesis under the direction of Harold Hotelling. He spent the year after working for an insurance company in the mistaken belief that he wanted to be an actuary. He returned to Stanford the next year and began work on his dissertation under the guidance of Harald Bohr, who was a visiting professor at Stanford that year. The dissertation was completed a few years later under the supervision of Professor Uspensky. Bacon was an acting instructor during this time, and in 1933 he became one of the three instructors in mathematics at Stanford. He was promoted to assistant professor in 1936.

The year 1929–1930 saw the appointment of James Victor Uspensky as an acting professor of mathematics. He was an acting professor again in 1930–1931 and a professor of mathematics from 1931 until his death in 1946. He graduated from the University of St. Petersburg in 1906, receiving his Ph.D. there in 1910. He was a Privat-Dozent in 1912–1915, a professor in 1915–1923, and a member of the Russian Academy of science from 1921 on. In Leningrad (then Petrograd) he was the teacher of the famous Russian number theorist I. M. Vinogradov. I am told that Uspensky visited America for a year in the early twenties. Upon his return to Russia he was interviewed by an agent from the NKVD (a predecessor of the KGB), who asked him how he liked America. Uspensky disarmed his interviewer by saying, "I loved it. It is a place of great opportunity, and if only I were a young man I would emigrate. But I am a member of the Academy of Science, and my career is established here. I am too old to start over again." The NKVD agent evidently reported that Uspensky was reliable and sound in his views. Thus, when Uspensky did decide to come to America a few years later, he came in style on a Soviet ship with his passage paid for by the government. This was in marked contrast with the case of Besicovitch and Tamarkin, who walked long distances through the woods to cross an uncontrolled stretch of border into Latvia.

Uspensky was conservative in his views. Once during a student's oral examination for the Ph.D. Uspensky asked the student to prove the existence of transcendental numbers. The student responded by showing that the algebraic numbers are countable, while the real numbers are not. Uspensky replied, "Yes, yes, these set-theoretic considerations are all very nice, but can you *prove* the existence of transcendental numbers." Fortunately, the student was familiar with Liouville numbers and could show their transcendence.

After Blichfeldt became chairman in 1927 the Stanford mathematics department had a steady stream of major mathematicians as visiting faculty, mostly for the summer quarter. A charming lecture given by Harold Bacon at a recent meeting of the Northern California Section of the MAA recaptures the flavor of the area's mathematical life during the thirties. I quote verbatim from Harold's remarks:

When I was asked if I would make a little talk at this luncheon, it was suggested that, perhaps, I might tell you something about what mathematics was like at Stanford 'in the olden days', or that I might give you a brief account of the organizing of the Northern California Section. It occurred to me that I might do a little of both, so I shall take the liberty of 'setting the stage' of the early thirties as a background for the organization of the Section in 1939. As a start I checked the Monthly for 1933 and found in the membership list of the Association that there were 31 individual members in Northern California and Nevada distributed as follows: Atascadero 1, Berkeley 12, Chico 1, Davis 1, Fresno 2, Morgan Hill 1, Oakland 1, San Francisco 3, Stanford-Palo Alto 6, Stockton 1, and Reno 2. A similar check in the 1939 monthly shows a total of 30 individual members, similarly distributed. Clearly, this was not a period of spectacular growth! In fact our mathematical community was pretty much scattered with a concentration in the San Francisco Bay Area. There were occasional meetings of the American Mathematical Society in the neighborhood, but even these were not heavily attended. Our situation is illustrated by a remark made by Stanford's Professor Uspensky. He, Professor Blichfeldt, Max Heaslet, and I were riding in Blichfeldt's car to one of the mid-thirties meetings in Berkeley. We speculated on the number of colleges, universities, and other organizations that would have members at the meeting. One of us guessed ten, another a dozen. It was remarked that, if the meeting were in New York, there would be a hundred or so. Whereupon Uspensky said very solemnly, 'Yes, we must recognize that we live in a remote province.'

But to look ahead for a moment, I can tell you that in 1949 there were 91 individual members of the Association in our area, and by 1960 there were over 500.

Stanford in the 1930's was Stanford during the Great Depression, and funds were hard to get to finance such things

as visiting lecturers, faculty, and conferences. All of our educational institutions were on short rations. But some skillful and sympathetic administrators somehow managed to squeeze out some modest sums for such purposes. For instance, at Stanford we were fortunate to have Harald Bohr for the full year 1930–1931, and in the summer quarters we were able to have as visiting faculty Edmund Landau (1931), Gordon Whyburn (1932), Marshal Stone and George Pólya (1933), Dunham Jackson and Dick Lehmer (1934), Gabor Szegő (1935), Rudolph Langer (1936), Lawrence Graves (1937), Gordon Whyburn (1938), Emil Artin and Arnold Dresden (1939), and Artin again (1940). Our regular full-time faculty was 9 in 1930–1931 and 7 in 1938–1939; last year (1984–1985) it was 31. In those pre-war depression years times were tough, but we had good work and good fun just the same. I could tell you dozens of stories, but I shall spare you all but a few.

I mentioned our Professor Uspensky a moment ago. He was a kind and gentle, soft spoken man; quite formal in manner. But he liked to seem quite ‘tough’ — Once I was at his home at a small gathering of graduate students, and he was making a vigorous argument upon some political theme. Suddenly he drew himself up and announced, ‘I have Tartar blood in my veins. That is why I am so fierce!’ And once he gave me reprints of two of his papers that he had written in Spanish and published in an Argentine journal. I thanked him, but had to confess that I could not read Spanish. ‘Well’, said he sternly, ‘learn it!’

Bohr was a very kind man. For instance, I remember my being in professor Blichfeldt’s office shortly after I returned to Stanford in 1930 to continue my graduate work after my master’s degree and a year’s absence working for an insurance company under the mistaken impression that I wanted to become an actuary. Blichfeldt and I were discussing my getting started on work that might lead to a dissertation. Just then Bohr came into the office. Blichfeldt turned to him and, indicating me, said ‘Here’s a man who is looking for a thesis topic. How would you like to suggest one, then be his adviser?’ Bohr bowed, smiled and very courteously replied, ‘I should be honored.’ He generously acted as my supervisor for the remainder of the year he was at Stanford. When he left, I was most fortunate to have Uspensky take over and see me through to the completion of my work on the dissertation. It was indeed a great privilege to have two such inspiring men as my friends and advisers at

the beginning of my career. You may sense that in those days procedures and academic ‘red tape’ were minimal!

Landau was a man of commanding presence with a real sense of humor, an enthusiastic lecturer, meticulously dressed in a somewhat formal fashion. He was particularly annoyed by chalk dust. In those days the blackboards in our department classrooms were of black slate, and we had rather soft chalk — white, yellow, red, green and other colors. Landau would write in unusually large script, quickly filling the front blackboards. He would sometimes dart about the room and write on the sidewall blackboard — once he even climbed over a couple of chairs to get to the board on the back wall. But then, the boards must needs be erased so that the writing could go on. Landau abhorred the usual felt erasers — too much dust. So, on the first day of his 8:00 and 9:00 classes, his assistant brought in a granite-ware kettle in which were a sponge and some water. Since she adamantly refused to use the sponge on the blackboard, Landau himself (shades of Göttingen with assistants who did the erasing!) would grasp the sponge, wring the water out on the floor, make some passes at the board, and then call on one or two students or visitors to his lecture to come up and dry off the slate with paper towels. A very ineffective method of drying! The lecture would continue. But the slate was still slightly wet, so half the chalk marks didn’t show. Eventually the board dried, however, and normal conditions returned. But the paper towels usually got on the floor where they mingled with water and various scraps of white, yellow and red chalk. After two hours of being walked on, these additions to the bare wood floors produced a, shall we say, cluttered appearance. As it happened, the 10:00 class that followed these first two classes in this classroom was a course in Education — something like ‘The administration and care of the School Building and Classrooms.’ I fear that those students had a rather spectacular illustration of the neat and orderly classroom! Incidentally, on the last day of classes, Landau made a graceful and humorous farewell speech in his heavily accented English. His last ‘goodbye’ ended with the request, ‘Please preserve the sponge to remember me by!’

Landau’s MWF 8:00 class was a graduate level course, ‘Selected topics from the Theory of Functions’, while the MTWThF 9:00 class was primarily intended for high school teachers and other interested students on ‘Foundations of Arithmetic’ — essentially Landau’s *Grundlagen der Analysis*. Towards the end of

the quarter Landau and his wife gave an evening party to all his students in the faculty home they had rented on the campus. It began at about 8:00 P.M., and there were excellent and bountiful refreshments, good conversation, amusing 'parlor games', good fun. As the time passed rapidly, and the clock neared midnight, and even after, people decided that they really ought to go home, so they gradually said their thanks and goodbyes. It turned out that Landau feared that people had not had a good time because they did not stay on until 4:00 A.M. or thereabouts. That seems to have been the time the Göttingen parties usually broke up.

Besides being a distinguished mathematician and teacher, Dunham Jackson (University of Minnesota) was an inspired composer of Limericks of the quite respectable sort! In fact, he and I had a fairly extensive correspondence in Limerick form, each Limerick written and mailed on what we used to call a 'penny postcard.' I had purchased Jackson's book, *The Theory of Approximations*, Vol. XI of the American Mathematical Society Colloquium Publications, and I took it into his office and asked him to autograph it. I suggested that, in doing so, he write a Limerick for me. He immediately picked up the book, and without lengthy cogitation wrote on the flyleaf:

There was a young Fellow named Bacon
 Whose judgement of books was mistaken
 In a moment too rash
 He relinquished some cash
 And his faith in the Author was shaken
 (August 17, 1934)

I must add that my faith in the author was by no means shaken; it was greatly reinforced!

In 1936 Emil Artin was offered a professorship at Stanford. Artin wanted to accept but was refused permission to leave Germany. As he later told Dave Gilbarg, who was his student at Indiana, "My work was considered too valuable to the Fatherland for me to be allowed to leave. The next year they kicked me out of the country."

The development of mathematics in northern California was much strengthened in 1934 when G. C. Evans came from Rice to become chairman of the mathematics department at Berkeley. Even before his arrival at Berkeley

he had arranged for the appointment of C. B. Morrey as an instructor beginning in 1933–1934, and Hans Lewy was appointed lecturer beginning in 1935. With the arrival of Gabor Szegő as head of the Stanford department, Stanford and Berkeley began their development of a major faculty group in analysis.

The roster of the department in 1938 when Blichfeldt retired was the following:

Professors: H. F. Blichfeldt, W. A. Manning, J. V. Uspensky.

Assistant Professor: Harold Maile Bacon.

Instructors: Helen Glover Brown, William H. Myers.

Acting Instructors: Charles R. Bubb, Carl Douglas Olds.

3. THE SZEGŐ PERIOD

Gabor Szegő was appointed professor of mathematics and executive head of the department of mathematics at Stanford upon the retirement of Blichfeldt in 1938. Szegő was a distinguished Hungarian mathematician. He studied in Berlin, Göttingen, and Budapest, and received his Ph.D. from Vienna in 1918 while in military service. He was an assistant in Budapest in 1919 and 1920 before becoming a Privat-Dozent at Berlin in 1921. The dissertation for his habilitation was a fundamental paper on the development of an arbitrary function in orthogonal polynomials. He was an “extraordinary” professor at Berlin from 1924 until he succeeded Knopp as professor in Königsberg in 1926. In 1934 he left Germany for the United States as a result of the rise of Hitler. He was a professor of mathematics at Washington University in St. Louis from 1934 until he came to Stanford in 1938. He and George Pólya were the authors of the famous problem book: *Aufgaben und Lehrsätze aus der Analysis*. While at Washington University he wrote a volume, *Orthogonal Polynomials*, for the Colloquium series of the American Mathematical Society.

Before coming to the United States, Szegő established a number of fundamental theorems in complex analysis, potential theory, Toeplitz forms, and special functions. His thesis contained a limit theorem which has been fundamental for the study of Toeplitz forms. He showed that the ‘transfinite diameter’ of a compact subset of the plane is just its logarithmic capacity and that the Hausdorff and capacitary dimensions of a set are the same. Szegő was the first to introduce the study of orthogonal polynomials on the unit circle, and for twenty years he was the only person studying them. Later his work on recursion methods for them was fundamental for work in the electronic synthesis of speech (*cf.* the note by Kailath in [Sz]). Related to this work is his development and use of the Szegő kernel, which was later studied

by Garabedian and which plays an important role in contemporary work in several complex variables. One of his theorems on best approximation by the boundary values of holomorphic functions on the unit circle became a cornerstone of Beurling's theory of invariant subspaces of H^2 . Szegő had extreme technical facility: Hans Lewy once sought to prove the positivity of the coefficients of a certain multinomial series. He wrote to Szegő, who responded a week later with a beautiful three page proof involving triple integrals of Bessel functions. Carl Loewner, who was a contemporary of Szegő's at Berlin, called Szegő a virtuoso and related that I. Schur referred to him as "der begabte Szegő."

During Blichfeldt's time much of the emphasis of the mathematics department had been on algebra, group theory, and number theory. It is reported that Szegő was met on his arrival by Fred Terman, head of the electrical engineering department, and Felix Bloch, a professor of physics who later received the Nobel Prize for his work on nuclear magnetic resonance. They wanted to make sure that Szegő would arrange for the type of mathematics courses that were important for students in engineering and physics. Szegő was just the man for their purposes, and when I arrived as an undergraduate transfer student in 1946, there were many beautiful courses in analysis, useful for engineering and physics. One was an undergraduate course with the nondescript title "Advanced Calculus." Although this course was then taught by other faculty, it was clearly designed by Szegő, and the contents reflected his elegant style. Besides a thorough treatment of ordinary differential equations, it provided a magnificent treatment of the classical partial differential equations of mathematical physics. Topics included Bessel and Hankel functions, Legendre and associated Legendre Polynomials, spherical harmonics, orthogonal expansions in eigen-functions, boundary and initial value problems.

I later took courses from Szegő on the calculus of variations, mathematical methods in physics, and transform theory. Gabor Szegő was the best classroom teacher I have ever had the pleasure of taking courses from. His lectures were elegant, and covered the important material in what looked easy. He always used a direct approach to his topics that seemed natural, however, rather than some clever shortcut which might be a quicker way to prove things. My course in functions of a complex variable was taught by George Pólya for the first term and by Gabor Szegő for the second. Virginia Voegeli (later Virginia Royden), Lincoln Moses (who was beginning his graduate work in statistics, and who was later professor of statistics and graduate dean at Stanford), and I used to sit in the back row of this class. Sitting next to us were Fred Terman, then dean of engineering, and Hugh Skilling, then head of electrical engineering.

Szegő believed that one built a strong department by first building strength in a specific area, and he concentrated the majority of his appointments in classical analysis, particularly in complex function theory. His first appointment made at Stanford was that of Albert Charles Schaeffer as instructor in mathematics for the year 1939–1940. Al Schaeffer had received his B.S. from Wisconsin in 1930, his Ph.D. from MIT in 1936, and had been an instructor at Purdue before coming to Stanford. The following year saw the appointment of T. C. Doyle as instructor. By the time I arrived at Stanford, he had left to begin his career at the Naval Research Laboratory, but “D-” Doyle was already a legend among students and faculty.

George Forsythe, who had just received his Ph.D. from Brown, was appointed instructor in 1941. He was a conscientious objector during the war and left the following year to perform his alternative service. He was replaced as instructor by John G. Herriot, who had been a fellow graduate student with Forsythe at Brown. Forsythe returned to Stanford as a professor of mathematics in 1958. When a formal division of computer science was formed as a subunit of the department of mathematics in 1962, he and Herriot constituted its original faculty, and Forsythe was the architect and first chairman of the department of computer science when it was established in 1964.

The Register for the year 1942–1943 lists the appointments of Donald Clayton Spencer and George Pólya as associate professors, and the promotion of A. C. Schaeffer to associate professor.

George Pólya holds a special place in twentieth century mathematics, not only for his original and lasting contributions to pure and applied mathematics, but also as a great teacher of mathematics and for his contributions to the teaching of mathematics through his seminal work in heuristics and the methods of problem solving. He studied at Budapest and Vienna, receiving his doctorate from the former in 1912. He was at Göttingen from 1912 to 1914. Then, at the invitation of Adolph Hurwitz, he took his first teaching position at the ETH in Zurich, where he was to stay until 1940 and to which he returned for frequent visits thereafter.

At the suggestion of G. H. Hardy, Pólya was awarded the first international Rockefeller Fellowship in 1924. This was used to spend the year at Oxford and Cambridge with Hardy and Littlewood. Thus began the long friendship and collaboration with these mathematicians, one outcome of which was the famous book *Inequalities* by Hardy, Littlewood, and Pólya. While Pólya was at Cambridge, Hardy was in the midst of his campaign to reform the mathematics Tripos and asked Pólya to take the exam unofficially. Hardy expected Pólya’s poor showing would demonstrate that most of the questions on the Tripos were irrelevant to “modern continental mathematics.” Unfortunately

for Hardy's plan, Pólya's performance was the best on the examination, and he would have been named Senior Wrangler if he had been a student.

In 1940 Pólya left Switzerland, and, after two years at Brown and Smith, came to Stanford, where he remained for the rest of his academic career. Pólya was one of the most popular teachers at Stanford. When Pólya became emeritus in 1953, Terman, who became provost shortly thereafter, and who had attended some of Pólya's courses, used the excellence of Pólya's teaching as an argument to modify strict rule that emeritus faculty no longer taught. Thus Pólya became the first Professor Emeritus at Stanford recalled to active duty. He taught nearly full-time for a decade, and part-time for many years thereafter. The last course he taught was combinatorial analysis for the computer science department in the Autumn Quarter of 1977. He also celebrated his ninetieth birthday that quarter.

Pólya's doctoral dissertation was on probability. Since there was no one at Budapest in this subject, he wrote without an advisor. He continued his study of probability, and early papers explored aspects of geometrical probabilities. He may have been the first person to use in print the term "Central Limit Theorem" to describe the normal limit law in probability. Pólya also worked on characteristic functions in probability theory, for which there is a "Pólya criterion." One example of his work is the "Pólya urn scheme," which is often used as a model to describe the spread of contagious diseases. An offshoot of this model is the "Pólya distribution." He was also the first person to investigate "random walk," a phrase he originated. In 1921 he showed that a random walk in a plane almost surely returns to its starting point, but in three dimensions it almost never returns.

Pólya's most profound and difficult work is in the theory of functions of a complex variable. He was one of the pioneers, along with Picard, Hadamard, and Julia, of the modern theory of entire functions. It is an indication of the level of Pólya's contribution that the language of the subject contains such phrases as "Pólya peaks," "the Pólya representation", "the Pólya gap theorem", "the Pólya-Carlson theorem," "Pólya's 2^Z theorem," etc. Some of Pólya's most interesting work in this area concerns the zeros of entire functions. One paper of Pólya's in 1926 came close to proving the Riemann hypothesis. Although it failed to do so, it led to further developments, including some in statistical mechanics.

Pólya was much interested in geometry and geometrical methods, especially those involving symmetry. In 1924 he described the 17 types of symmetry in the plane. The Dutch artist M. C. Escher studied this paper, and soon after, some of the additional symmetries found by Pólya began to appear in Escher's etchings and prints. Pólya and Escher corresponded with each other prior to the second world war. Pólya's interest in symmetry emerged again in

1935 in a series of papers on isomers in chemistry, culminating in his monumental paper in 1937 on groups, graphs, and molecular structures. One of the high points in the history of combinatorics, this paper showed how to count essentially different patterns, patterns that could not be changed into each other by geometrical transformation such as rotation in space. Pólya's work was accessible and comprehensive, and the principal theorem is now called the "Pólya Enumeration Theorem." Found in any combinatorics text, it provides a powerful and subtle technique for counting graphs, geometrical patterns, and, not surprisingly, chemical compounds.

In his later years Pólya became much concerned with problems of the teaching of mathematics. Even before coming to America he had started a manuscript for his book *How to Solve It*, which was published by the Princeton University Press in 1945. It proved to be very popular and has now sold more than a million copies. It has been translated into fifteen languages. After this came the two-volume set, *mathematics and Plausible Reasoning* (1954), again illustrating some of the heuristic principles set out earlier in *How to Solve It*, and in some of his articles. That was followed by a more elementary set of books, *mathematical Discovery*, in 1962 and 1965. These works established him as the foremost advocate of problem solving and heuristics in his generation. Though he had distinguished antecedents from Descartes to Hadamard, the latter having also written about heuristics and the psychology of problem solving, Pólya nevertheless is the father of the current trend toward the emphasis on problem solving in mathematics teaching.

Donald Spencer had been a premedical student as an undergraduate at Colorado, and began simultaneous studies in medicine at Harvard and in aeronautics at MIT. He soon found that he preferred engineering and mathematics to medicine. After receiving his master's degree in aeronautical engineering from MIT in 1936, he studied at Cambridge University, receiving his Ph.D. in 1939 under the direction of J. E. Littlewood. He was an instructor at MIT from 1939 until he came to Stanford in 1942. In 1944–1945 Spencer worked with Max Shiffman in the Applied Mathematics Group at NYU, a research group established by the applied mathematics Panel, a subsidiary of the Office of Scientific Research and Development. They were concerned with some problems on impact and splash, developing a mathematical description of the behavior of the shape of the surface of a liquid into which a solid sphere is dropped.

His dissertation with Littlewood had been on mean p -valent functions, and at Stanford he began his fruitful collaboration with A. C. Schaeffer on variational methods in conformal mapping. This led to the determination of a number of coefficient and other regions of variation for the class of schlicht

functions. He and Schaeffer received an ONR contract at Stanford to support their work on conformal mapping. They explicitly calculated the region of variation of the second and third coefficients of a normalized schlicht function. They were able to have models of three dimensional cross-sections of this region cast and machined in aluminum by a local firm specializing in the precision casting and machining of blades for jet engines. This work gave a new proof of Loewner's result that $|a_3| \leq 3$. Their goal, of course, was to prove $|a_4| \leq 4$. They knew this was true for points in the coefficient body sufficiently close to those corresponding to the Koebe function, and they also had an estimate for the continuity of points on the boundary of the region. As Spencer told me, this would enable them to do the inequality for a_4 if "only we could integrate 10^6 differential equations numerically with sufficient accuracy." That was far beyond the possibility of computation at that time, but some years later Garabedian and Schiffer greatly improved the estimates Schaeffer and Spencer. They sufficiently reduced the number of equations that needed to be integrated so that Garabedian could integrate them by hand on a Marchant Calculator.

In 1943 Mary Virginia Sunseri was appointed to an instructorship in mathematics. She became an assistant professor in 1948 and later an associate and full professor, becoming Professor Emerita in 1986. For forty-three years she was to be one of the mainstays of our freshman-sophomore teaching. In her teaching career at Stanford she has taught more students than anyone else in mathematics except for Harold Bacon, and has been the much respected advisor of many generations of Stanford undergraduates. She has received many awards at Stanford for her teaching and university service.

Paul Rosenbloom earned a Ph.D. at Stanford in the early forties working under the supervision of Gabor Szegő. He was later to become a professor at the University of Minnesota, and sometime later at Teachers College, Columbia. He has written of his study at Stanford [Ros], and I include a brief excerpt from his account:

In September 1941, I started my graduate work at Stanford after graduating from Pennsylvania. Szegő met me once a week to discuss my progress. My assignment was to do problems in Pólya-Szegő and to read Titchmarsh's *Theory of Functions*. This weekly meeting gave me a feeling of responsibility to have something to report so as not to waste Szegő's time. . . . Often Szegő would discuss the ramifications of the problems and related results in the recent literature. He would point out natural questions for further investigations. Szegő had a broad and deep knowledge of general theories, but he preferred to work on

concrete problems which test the power of these theories. He had an amazing technical facility.

We had a weekly seminar. Since I was the only serious graduate student in mathematics, the members were Szegő, Schaeffer, Hille (on sabbatical), Forsythe (then an instructor), Doyle (an assistant professor), and myself. Hille returned to Yale the following year, but Pólya and Spencer joined the department then. Hille lectured on the Nevanlinna theory and on the Gelfond–Schneider solution of Hilbert’s problem (later written up for the *Monthly*). I was assigned to present Brun’s twin prime theorem and Ahlfors’s thesis. The second year Spencer lectured on multivalent functions, and Schaeffer on Schiffer’s variational method in conformal mapping. That was when their collaboration on univalent functions began.

In January 1943, I received an offer of an instructorship at Brown, providing that I could start in February. Szegő arranged for me to take my final orals immediately, even though my thesis wasn’t written yet. I was asked to outline my main results thus far, and then the committee probed me with general questions. Pólya began asking me to give the definition of Gaussian curvature in terms of the area of a spherical map by the normals. I protested that I had never studied it, but he insisted that I try to work it out at the blackboard. He said, ‘It is not forbidden to learn something from an examination!’

A few years after Rosenbloom had left for Brown, I transferred to Stanford from a junior college. The mathematical student life in 1946 was much more active than in Rosenbloom’s time. Veterans were returning en masse from the war, classes were well populated, and there were numerous graduate students in mathematics. Although Al Schaeffer had just left Stanford for Purdue and Uspensky died during my first quarter at Stanford, there were still many inspiring teachers of mathematics. I found the mathematical atmosphere quite stimulating.

The graduate students in mathematics included Albert B. J. Novikoff, Mike Aissen, Ken Cooke, Arthur Grad, Joseph and Betty Ullman, Burnett Meyer, and David Haley. Although I was an undergraduate, I took many graduate courses and came to know the graduate students even better than many of my fellow undergraduates. The distinction between undergraduate and graduate students was not so great then as usual, since most of the undergraduate men were returning veterans of the same age as the graduate students.

There was an active summer term in those years, and I attended the summer term of 1947. I took a course in non-Euclidean geometry from Harold Bacon which was particularly memorable for meeting at 7:00 AM. I also had a course of Farey Series and Continued Fractions from Hans Rademacher, who was a visiting faculty member that summer. I also got to know Peter Lax, who was often visiting at Stanford, and for the summer of 1949 he was accompanied by his new wife Anneli. I remember attending a seminar by Kurt Reidemeister together with Anneli and my wife Virginia, who was finishing her M.S. in physics.

It was my good fortune that Harold Davenport was a visiting professor throughout my senior year. I took undergraduate courses in group theory and number theory from him, much of which I have forgotten, but I remember vividly his graduate courses on continued fractions, geometry of numbers, and analytic number theory. Davenport was extremely friendly and encouraging to students, and he would lunch several days a week with me and Nesmith Ankeny, one of my undergraduate classmates, who later got his Ph.D. from Princeton and is now a professor at MIT. Our discussions ranged over mathematical topics, anecdotes about mathematicians, the differences between American and British ways of doing things, the philosophy of mathematics, politics, history, etc. I have often supposed that discussions at the High Table of an English college were like those we had then. Harold Davenport was the first established mathematician that I felt I knew on a personal basis, and I remember him warmly as a teacher and friend. The department wanted Davenport to stay as the successor to Uspensky. Davenport told me that it was an extremely attractive possibility, but he ultimately decided that he should remain department head at University College, London, because of his responsibility to the young mathematicians he had recruited there. He was a good friend and frequent visitor to Stanford thereafter.

As an undergraduate I had been interested in Hilbert's program for the foundations of mathematics, and remember Davenport telling me about a theorem (possibly Littlewood's theorem about the alternation of the number of primes of the form $4k + 1$ versus the number of those of the form $4k + 3$) that had been proved under the assumption of the Riemann Hypothesis and had later received a very different proof on the supposition that the Riemann Hypothesis was false. The question then arose whether the theorem had really been established. Fortunately for the number theorists' peace of mind, a constructive proof was soon found that was independent of the Riemann Hypothesis (using the Skewes number). Not long after I returned to Stanford as a faculty member, Davenport was again visiting for a quarter, and our discussions continued. He had looked into intuitionism as a result of a talk he had given for undergraduates at London. He found it appealing

philosophically, but thought it unsuitable as a basis for mathematics because of the extreme limitations it put on the mathematics one could do.

Harold Bacon has given a vivid description of the mess in Landau's lectures caused by sponging off blackboards full of colored chalk. I was first exposed to this phenomenon during my senior year: It was announced that Marcel Riesz would give a series of four lectures on the wave equation, Clifford numbers, retarded potentials, and the Riemann–Liouville Integrals. The day of the first lecture was warm, and the good-sized lecture room was full of faculty and students. Gabor Szegő introduced Riesz, who promptly took off his jacket and proceeded to lecture in his shirtsleeves and suspenders. A bowl of water and sponge had been provided. After filling up the blackboard, Riesz motioned imperiously to Szegő, who jumped up and washed off the blackboard while Riesz stood by and watched! Now Szegő was very distinguished and autocratic, wore elegant tailor-made suits, and was always regarded with awe by the students and most of the faculty. To see him in the role of a young European assistant to Riesz was startling! After several repetitions of this performance, needless to say, blackboard and floor soon became quite a mess. Sitting directly behind me was George Pólya, who had brought Felix Bloch to hear a distinguished fellow Hungarian. Pólya was somewhat embarrassed by the performance and muttered apologies *sotto voce*. The lectures were brilliant, however, full of new insights into novel mathematics I had never seen before.

Marcel Riesz was a frequent visitor to Stanford in subsequent years, and I always found him friendly and helpful. One of my graduate students invited Marcel to his house for dinner. Besides Riesz there were only graduate students and their wives. The host was quite nervous about how the evening would go, but all went well. After dinner a full bottle of whiskey was brought out and put on the coffee table in front of Riesz. Riesz stayed and talked amiably with the students until the whiskey bottle was empty, whereupon he got up, graciously said good night, and walked home.

Richard Bellman became an associate professor at Stanford, beginning with my senior year in 1948. His degree was from Princeton a few years earlier. He had spent some time there as a junior faculty member and brought the aura of Princeton with him to Stanford. At that time he was interested in the qualitative theory of ordinary differential equations, *a la* Lefschetz, what would now be called dynamical systems. He gave a beautiful course on the subject from which I learned much. At the beginning this course was overflowing with students both from mathematics and from engineering and physics. After one look at the crowded classroom, Bellman talked about prerequisites and assigned a long, highly theoretical problem set involving existence theorems and the Arzela selection theorem. By the second meeting of the class all of the engineering and physics students had dropped the

course, but there were still a good many students left. Bellman promptly assigned another lengthy problem set, this one involving very applied numerical problems requiring numerical calculations to a high degree of accuracy. By the third meeting most of the mathematics students were gone, and Bellman proceeded to lecture for the rest of the term to a few hardy souls, mostly auditors. No mention was ever again made of the assigned problem sets, for which no due dates had been specified, and no more problems were assigned. He did, however, require a paper analyzing a particular differential equation as a final exam. I confess that I have sometimes emulated Bellman's method when a course that ought to be a small informal advanced class starts out to be overcrowded.

Max Shiffman became an associate professor beginning in 1948. He had received his degree from NYU in 1938 as a student of Courant. He had been a faculty member there since then, and during the war he was with the Applied Mathematics Group at NYU. Shiffman was largely interested in the calculus of variations and hydrodynamics, but was well versed in a wide range of modern analysis. I learned some of the rudiments of differential geometry from him as well as the use of topological methods in analysis and variational theory.

Max Schiffer first came to Stanford for a short visit in 1947 as a guest of Spencer and Schaeffer. (Schaeffer was then at Purdue, but was frequently on the Stanford campus because of his ongoing research work with Spencer.) They had not previously met Schiffer, and there was speculation among the graduate students about the possibility of friction, since Spencer and Schaeffer considered Schiffer the "competition" in the development of variational methods in conformal mapping. Arthur Grad, who was writing his dissertation with Spencer, kept us apprised of the arrangements for the coming meeting with the great man. As soon as the historic meeting took place, we eagerly sought out Arthur for a blow-by-blow account. Arthur reported that Schiffer had turned out to have charmed everyone and that there were no fireworks. Arthur sounded disappointed!

Schiffer returned as visiting faculty member in 1948 and lectured on potential theory, a course I was fortunate enough to attend. Schiffer originally began his mathematical studies in Berlin with I. Schur and emigrated to Jerusalem in the thirties, where he received his M.A. and Ph.D. from the Hebrew University. In addition to his work on group theory with Schur, he was active in developing variational methods in mathematical physics and in conformal mapping. He was a senior assistant and lecturer there from 1938 to 1946. From 1946 until 1949 he was a research lecturer at Harvard, where he worked with Stefan Bergman along with Nehari, Garabedian, and Springer. He was a frequent visitor to Stanford, where he collaborated with

Szegő on problems in mathematical physics and hydrodynamics and with Spencer on variational problems in function theory. Following a year visiting at Princeton and a year as professor at the Hebrew University, he accepted an appointment as a professor of mathematics at Stanford beginning in 1952. He has been at Stanford ever since.

I spent the academic year 1948–1949 as a graduate student at Stanford writing a master's thesis under Spencer's supervision. The title of this thesis was "Loewner's Kappa Function when the Slit is Analytic." The kappa function appears in the parametrization of schlicht functions that Loewner obtained by growing a slit into the interior of the unit disk, and I was concerned with getting the first few terms of the expansion for it in terms of those for the slit being grown into the disk.

Spencer, as many later generations of students will attest, was an excellent and stimulating man to study with, always friendly, helpful, and quite interested in the work done by his students. His stories of mathematicians fascinated me, especially those of his student days at Cambridge with Hardy and Littlewood. I came to feel almost more like a very junior colleague than a student. This feeling was reinforced by the presence of Paul Garabedian, who had just gotten his degree with Ahlfors, and was spending the year 1948–1949 at Stanford working with Spencer as a National Research Council Postdoctoral Fellow. Paul was an assistant professor at Berkeley the following year, returning to Stanford in 1950 as an assistant professor, becoming an associate professor in 1952.

Shiffman and Garabedian conducted the departmental seminar for the year 1948–1949. Various topics in conformal mapping were treated, including variational theory and the conformal mapping of multiply-connected regions. There were no books on variational methods in conformal mapping at that time, and the only book on multiply-connected regions was that of Julia.

The departmental seminar was an institution of some standing at Stanford. In Paul Rosenbloom's time it was largely a faculty seminar, but by the time I arrived, it had become very much a student seminar. It met for two hours every Thursday afternoon with an intermission in between. All graduate students were obliged to attend and to present assigned talks developing aspects of the theme for the year. Most of the faculty were in regular attendance. For the previous year the seminar had been conducted by Davenport and Pólya on topics in irrational number theory and Diophantine approximation.

Fellow students told me that in earlier years it had sometimes been a harrowing experience for them. According to Albert Novikoff, one of our more flamboyant graduate students and now a professor at NYU, the first hour

consisted of the student's attempt to present the assigned topic, while Pólya and Uspensky argued with each other about the adequacy of the student's statements. For the second hour Pólya or Uspensky would demonstrate how the lecture should have been given. The only mitigation for the unfortunate student was that, whichever of Pólya or Uspensky was critical, the other would be supportive.

During the intermission Pólya and Uspensky would bait Schaeffer and Spencer about the latter's ignorance of classical mathematics. Schaeffer was oblivious to this, but Spencer would sometimes rise to the bait and respond by asking Uspensky what the Betti numbers of the sphere were. Uspensky would indicate that this modern stuff was nonsense beneath his notice. Sometimes the argument descended to the personal level, with Uspensky maintaining that the younger generation (i.e., Schaeffer and Spencer) lacked the strength of character and fortitude exhibited by their elders. Spencer recalls only once that he or Schaeffer got the better of the exchange: Uspensky had been holding forth about the degeneration of the younger mathematicians and recounted the story of an ancient Roman who, becoming tired of the world, ordered his servants to construct a huge funeral pyre which he proceeded to walk into. Turning to Schaeffer, Uspensky said, "Would you do that, Schaeffer?" Schaeffer bowed and replied, "After you, Uspensky."

It had always been my intention to go East for doctoral work after finishing my master's degree at Stanford. Don Spencer and Paul Garabedian both told me to go to Harvard and work with Ahlfors. Although that may not have been the best advice I have ever been given, it was certainly the best advice I ever accepted. Paul also instructed me to introduce myself to Stefan Bergman and to ask him for a research assistantship. This I did upon my arrival at Harvard, and my acquaintance with Garabedian, Schiffer, and Spencer were sufficient to obtain an appointment as one of Stefan's assistants.

Stefan Bergman, who joined the Stanford faculty as a professor in 1951, studied engineering at Wroclaw and Vienna. Finding himself strongly attracted to the theoretical aspects of engineering and to problems in pure and applied mathematics, he entered (in 1921) the Institute for Applied Mathematics which had just been established by Richard von Mises at the University of Berlin. In 1930 Bergman was appointed Privat-Dozent at the University of Berlin. His scientific career at the university, however, was cut short in 1933 by the Nazi takeover of Germany. He left Germany and found refuge for some years in Russia. In 1934 he became professor at the University of Tomsk in Siberia. In 1936 he moved to Tbilisi, Georgia, where he stayed until 1937. The success of his stay in the Soviet Union is best appreciated if one observes that some of his students became leading mathematicians in their own right, such as Vekua, Fuks, Kufarev, etc.

In 1937 his position became precarious, because of the increase of Stalinism. An agent from the NKVD, with whom he was acquainted, told him that things would become extremely difficult for foreigners, and advised him to leave the country while he still could. This he did, escaping the fate of Fritz Noether (Emma's brother). He had been with Bergman at that time, remained in the USSR, and was never heard of again.

Bergman left for Paris, where he worked under most difficult conditions. He spent most of his time at the Institut Henri Poincaré, where he wrote a two volume monograph on the kernel function and its applications to complex analysis, which appeared in the series "Mémorial des sciences Mathématiques." Through the help of Hadamard he was able to immigrate to the United States, leaving France just before the outbreak of the second World War in 1939. He was at MIT and at Yeshiva College from 1939 to 1941, and in 1941 he went to Brown, which was at that time a real haven for refugee scientists from Europe. His assistants there included L. Bers and A. Gelbart, who worked out his lecture notes.

In 1945 he joined his old teacher and friend von Mises at the Harvard Graduate School of Engineering. There he directed various research projects until 1951. During his years at Harvard his research projects included an impressive array of associates and assistants. Before my time these included Max Schiffer, Zeev Nehari, Paul Garabedian, and George Springer, and while I was there, my fellow assistants included Philip Davis, Henry Pollak, and Bob Osserman.

My colleague Max Schiffer, who knew Stefan from the time when they were both in Berlin, Stefan as Dozent and Max as student, has written [Sch] of those days in Berlin and of Bergman and his work:

Von Mises was one of the leading theoreticians in aerodynamics and probability theory, who believed that applied mathematics should be as precise as pure mathematics but that its methods should be feasible and practical. His ideas had an enormous impact on Bergman's scientific outlook. At the Institute for Applied Mathematics Bergman worked on such down to earth problems as the magnetic field in an electric transformer and the distribution of temperature in the stator of a generator. He studied boundary value problems of elasticity and various other problems of potential theory. To obtain a large number of harmonic functions he applied the Whittaker method for creating harmonic functions by means of integrals over analytic functions.

Another mathematician at Berlin who had great influence on Bergman's scientific development was Erhard Schmidt. Shortly after his arrival at Berlin, Bergman participated in Schmidt's seminar and was charged with giving a lecture on development of square-integrable functions in terms of an orthogonal set. As he told me, he misunderstood the task, and instead of dealing with real functions over a real interval, he attacked the problem for analytic functions over a complex domain. He found the task hard but carried it through. This was the genesis of his famous theory of orthogonal functions and the kernel function, which formed his doctoral thesis in 1922. Interestingly enough another student from Schmidt's seminar, Salomon Bochner, was also attracted to the problem of orthogonal systems but developed into a different direction of analysis.

Bergman applied his results on orthogonal analytic functions in fluid dynamics, conformal mapping and potential theory, but also developed the central concept of his theory which is now called the *Bergman kernel*. He soon realized that he could define his kernel function in this case of functions of several complex variables. The subject was still quite undeveloped in the Twenties, and he was one of the founders of this important branch of research. An impressive achievement in this field is his construction of a metric on domains in the space of two complex variables which is invariant under mappings by means of a pair of analytic functions.

Another fruitful idea was his discovery that for a large class of domains an analytic function of several complex variables is already completely determined by its value on a relatively small part of the boundary. He called this part the *distinguished boundary*, but it is now known as the *Bergman-Shilov boundary* of the domain. He then connected the theory of the boundaries of a domain with that of the kernel function by classifying boundary points in terms of the asymptotic behavior of the kernel under an approach to that boundary point.

In 1930 Bergman became a 'Privat-Dozent' at the University of Berlin. His thesis, which he had to submit for his official 'Habilitation,' dealt with the theory of boundary behavior of the kernel function. He was appointed simultaneously to the Institute of mathematics and the Institute for Applied Mathematics at the Berlin University. This was at that time a rare distinction. I was then a very young student present at his inaugural lecture

and very impressed by his sponsors von Mises and Schmidt who presided in full academic dress on this occasion. The topic of the lecture was about the theory of a wing of an airplane.

I again became a colleague of Bergman's at Harvard in 1946. After I had shown the relation between the harmonic Green's function of a plane domain and its kernel function it was natural to extend the method of orthogonal solutions to problems of partial differential equations and obtain representations for their fundamental solutions. We worked in close cooperation at Harvard from 1946 to 1950, and I remember those days with nostalgia. In 1952 Bergman became again my colleague when we joined the mathematics department at Stanford University.

Gabor Szegő arranged for me to be a teaching fellow at Stanford for the summer term of 1950, and I earned my keep by teaching the three calculus classes that were offered that term. Visiting mathematicians for the summer included M. Fekete, W. Rogosinski, W. Fenchel, and Walter Hayman. Hayman and Fenchel were accompanied by their families. Szegő solved the housing problem for them by renting a fraternity house for the summer and putting them all there. One morning when Rogo met Fekete as they were both shaving, he asked Fekete if he had spent a good night. Fekete replied, "Not bad. I proved the following theorem . . . "

Paul Garabedian had just returned to Stanford from Berkeley, and discussions about function theory abounded. Hayman was working on p -valent functions and successfully applied Pólya symmetrization to them. It was from Hayman that I learned about p -valent functions and the details of the work of Pólya and Szegő on symmetrization in function theory. Spencer was also at Stanford for most of the summer, and I remember a lunch at Rickey's Restaurant with Spencer, Hayman, and Rogosinski. We were talking about the beauties of Pólya Symmetrization, and during the conversation several of the people at lunch, principally Rogo and Walter, used it to give a two line proof of the Koebe "One-quarter Theorem." We all agreed that this was the simplest possible proof. I also learned from Walter about mean p -valent functions and the problem of establishing the "One-quarter Theorem" for them. I worked on this problem quite a bit that summer and kept finding "solutions," but when I would show them to Walter, they would turn out to be founded on a certain amount of wishful thinking. Nevertheless, some of these attempts indicated the proper direction, and a year or so later Paul Garabedian and I succeeded in finally proving the theorem.

In 1951 Carl Loewner, Max Schiffer, and Stefan Bergman came to Stanford as professors, and I arrived as an assistant professor.

Carl Loewner received his Ph.D. from the Charles University in Prague in 1917 and was an assistant there until 1922, when he became a Privat-Dozent in Berlin. In 1928 he left to be an assistant professor at Cologne for 1928–1930 and Prague for 1930–1933. He was professor at Prague from 1933 until the Germans came in 1939. Upon coming to America, he taught at the University of Louisville from 1939 to 1944 and was a research associate at Brown in 1944–1945. He was professor of mathematics at Syracuse until he came to Stanford in 1951. Loewner is probably best known for the work on univalent functions where he gave a method for generating a dense set of conformal maps of the unit disk by means of semi-groups. This gives a representation for the coefficients of a univalent function, enabling him to prove $|a_3| \leq 3$, and this method forms the basis of the recent proofs of the Bieberbach Conjecture. Carl was interested in and knowledgeable about a wide range of mathematical topics: differential geometry, lie groups and semi-groups, matrix theory, geometric topology, complex analysis, and differential and integral equations. He was a popular teacher of graduate students and an excellent dissertation supervisor. While at Stanford he probably directed more Ph.D. students than the rest of the department's faculty combined. Lipman Bers, who wrote his dissertation for Loewner at Prague, once remarked that any mathematics department containing Loewner was fully qualified to give the Ph.D. degree, even if he were the sole member! Carl treated his students as colleagues, inspiring the best to superior work, while exhibiting much patience and help for the slower student. Because of the generosity of his help when needed, it has been said of his students "the weaker the student, the stronger the dissertation."

During my early years on the Stanford faculty, I was closer mathematically to Loewner than to anyone else in the department. I learned a great deal of differential geometry in those days by working or trying to work out problems and theorems for myself. Whenever I got stuck in the process, I would ask Carl how to do it. As I remember, he always knew how. I also learned an enormous miscellany of mathematics by collaborating with Carl on a proseminar for graduate students. A decade later I suddenly realized that, although I was still close to Carl personally, I seemed to have less mathematical contact than previously. I wondered if his outlook was finally aging, but observed that he was still in active contact with students and our youngest faculty — it was I who had grown older in my ways, not Carl!

Shortly after I joined the faculty at Stanford, there was a movement to reform and modernize the mathematics curriculum led by the "Young Turks" consisting of me, Paul Garabedian, Max Shiffman, and Carl Loewner. Although Carl was the oldest in years, he was probably the youngest in viewpoint. Gabor Szegő demonstrated his skill and polish as an administrator by his handling of this reform effort: He promptly constituted a committee

consisting of Paul Garabedian and me to rewrite the curriculum and degree requirements for the University Bulletin, although I am sure he thought our ideas were newfangled nonsense.

Szegő was extremely productive mathematically at Stanford and provided mathematical and intellectual as well as administrative leadership for the department. He worked on inequalities for geometrical and other quantities from mathematical physics and, together with Pólya, developed and applied the theory of symmetrization to them. This work appeared in a book written with Pólya, *Isoperimetric Inequalities in mathematical Physics*, Princeton University Press, Princeton, 1951. He collaborated with Max Schiffer on finding estimates and extreme values for the virtual mass of a body in hydrodynamics. In 1952 he published a fundamental paper “On Certain Hermitian Forms associated with the Fourier Series of a Positive Function,” which has had major applications to mathematical physics. Not long afterwards he wrote a fundamental paper with Helson at Berkeley on prediction theory for weighted L^2 spaces, and a paper with Mark Kac and W. L. Murdock on “Eigenvalues of Hermitian Forms” which has been influential in problems of numerical analysis and partial differential equations. He also wrote a book with Ulf Grenander on *Toeplitz Forms and their Applications*, which appeared in 1958.

The evolution of the Stanford department of mathematics owes much to the skill and effort of Gabor Szegő. Upon his arrival at Stanford he found a department largely oriented towards number theory and group theory and whose strength, although typical of American departments of that time, was hardly of the caliber of the major European centers. In his tenure as executive head Szegő reoriented the department towards classical analysis and took advantage of the influx of distinguished European mathematicians to build a department of world stature. In the fifties Stanford, along with NYU, Berkeley, and MIT, had become one of the leading departments in classical analysis. With a faculty containing Pólya, Szegő, Loewner, Bergman, Schiffer, Garabedian, me, and later Osserman, and with such visiting faculty as Ahlfors, Bers, and Spencer, Stanford had become the leading center for complex function theory. Szegő presided over this development with old world courtesy and tact, but with an autocratic, almost aristocratic, firmness and certainty of purpose. His knowledge and evaluation of the mathematical activities of the department’s faculty was unusual, and I am told that he would read all of the papers published by members of the mathematics department. He ran the department with a grace that seemed effortless and still found ample time to give beautiful, well-organized courses and to maintain an active high-level research program.

At the conclusion of Szegő’s period as executive head of the department in 1953 the roster was as follows:

Professors: Gabor Szegő, Harold M. Bacon, Stefan Bergman, Charles Loewner, George Pólya, Menahem M. Schiffer, Max Shiffman.

Associate Professors: Paul Garabedian, John G. Herriot.

Assistant Professors: Gordon E. Latta, Halsey Royden, Mary Virginia Sunseri, Robert Weinstock.

Several of the Stanford mathematicians of this period, Loewner, Pólya, Spencer, and Szegő have had volumes of collected works published. These are noted in the bibliography. In addition, the Pólya Picture Album [P 5] contains a number of photographs of Stanford mathematicians of that period.

4. FORMATION OF THE DEPARTMENT OF STATISTICS

Statistics and probability have a long history at Stanford. Harold Hotelling was a member of the mathematics department in the late twenties. Uspensky, one of whose fields was probability, was professor of mathematics from 1929 until his death in 1946, and the appointment of Pólya in 1942 added more strength to the field of probability. The statistical tradition had continued in economics and the Food Research Institute from the time of Hotelling, and its importance was becoming recognized in engineering. There was also a long history of research and teaching in statistics carried out in the department of psychology. In the middle forties there was a Committee on Probability and Statistics consisting of Allen Wallis from economics, Holbrook Working of the Food Research Institute, Eugene Grant of industrial engineering, and George Pólya from mathematics.

In 1946 Albert Hosmer Bowker accepted Szegő's offer to be an assistant professor of mathematical statistics in the mathematics department. Bowker had studied under Hotelling at Columbia and North Carolina. During the Second World War Allen Wallis had been the Scientific Director of the Statistical Research Group at Columbia, and Bowker had served as one of his assistant directors.

It was expected that Wallis would play a leading role in the establishment of statistics at Stanford. Before the department was established, however, he left Stanford to become chairman of the department of statistics at Chicago and later dean of the business school there. He suggested that Al Bowker would give leadership for the development of the new program in statistics. His suggestion was followed, and when the department of statistics was established in 1948, Al Bowker became acting head, and, subsequently, its first executive head.

M. A. Girshick, then at the Rand Corporation, was recruited to be professor of statistics. Abe Girshick had studied under Abraham Wald at Columbia,

and had a distinguished career with the federal government as a statistician before coming to Stanford. Herman Rubin was appointed assistant professor in 1949. Herman Chernoff came from Illinois as an associate professor in 1951, and the following year Charles Stein came from Chicago.

Herbert Solomon, who had done his graduate course work at Columbia, received the first Ph.D. from the new department in April of 1950. Lincoln Moses and K. D. C. Haley had been doing graduate work in the mathematics department before the establishment of the statistics department, and they received Ph.D.s in statistics not long after, Moses in August of 1950 and Haley in August of 1952. Solomon, after receiving his degree, headed the statistics section of the Office of Naval Research and was later on the faculty at Teachers College, Columbia. He returned to Stanford in 1959 as professor and head of the department. Moses, after teaching for two years at Teachers College, returned to Stanford in 1952 to a joint appointment in statistics and the school of medicine. His long career at Stanford includes a term as dean of graduate studies. Haley has been a perennial visiting professor teaching in the department's summer program. The early group of students admitted for graduate work in the new department included Gerald J. Lieberman, who received his Ph.D. in 1952 and was appointed to an assistant professorship jointly in statistics and industrial engineering. He originally taught quality control and sampling inspection, but soon began to establish a group in the newly developing field of operations research.

In its early years the department of statistics began the practice of making joint appointments with other departments that made major use of statistics: Quinn McNemar, a professor of psychology at Stanford with a distinguished career in psychological statistics, was made professor of psychology and statistics when the department was established. Kenneth J. Arrow, who had joined the economics department in 1949, was appointed associate professor of economics and statistics in 1950. In the fall of 1952 Lincoln Moses, who had received his Ph.D. from the new department of statistics in 1950 and had spent two years on the faculty of Teachers College, Columbia, was appointed to a joint assistant professorship in statistics and in the department of Community Medicine in the Stanford Medical School, where he began to build up a group in biostatistics.

Not long after this Sam Karlin was appointed professor in mathematics and statistics. Karlin received his Ph.D. at Princeton with Bochner, and spent his early years at Cal Tech. He was a frequent consultant for Rand, and became one of the leaders in the new field of operations research. By the time of his arrival at Stanford he had become interested in "birth and death" processes. This led him into his research on population genetics and to his collaboration with Stanford's department of genetics when it was established some years later.

The department's roster for the year 1968–1969 was the following:

Emeritus: Quinn McNemar

Professors: Theodore W. Anderson, Jr., Herman Chernoff, M. Vernon Johns, Gerald J. Lieberman, Rupert G. Miller, Lincoln E. Moses, Ingram Olkin, Emanuel Parzen, Herbert Solomon, Charles Stein, Patrick Suppes. *Consulting:* William G. Madow

Associate Professor: Bradley Efron

Assistant Professors: Richard Olshen, David O. Siegmund, Paul Switzer, George G. Woodworth

5. THE APPLIED MATHEMATICS AND STATISTICS LABORATORY

On the recommendation of Al Bowker and with the support of Fred Terman, then dean of engineering, a laboratory for applied mathematics and statistics was established in 1950. The laboratory, under the directorship of Al Bowker, was a significant factor in the development of the mathematical sciences at Stanford in the next decade. Not only did mathematics and statistics prosper through involvement with the laboratory, but also computer science and operations research grew out of activities of the laboratory. In addition the laboratory's work influenced mathematical and statistical development in economics, psychology, and other social sciences. The Applied Mathematical and Statistics Laboratory provided direction and coordination for Stanford's activities in the mathematical sciences, was a channel for university support, and was instrumental in obtaining funding from the federal government. The availability of external funding in those days allowed the mathematics and statistics departments to grow more rapidly than they would have otherwise, since appointments could often be made well in advance of the time when university support was to be expected.

Much of the early government money came from the Office of Naval Research, where the mathematics program was directed by Mina Rees. She had been involved during the war in the administration of mathematical and statistical work with the applied mathematics Panel of the Office of Scientific Research and Development. Schaeffer and Spencer's work on the coefficient region for schlicht functions had received ONR support, and Pólya and Szegő had an ONR project on potential theory and capacity out of which came their beautiful work on symmetrization.

The Office of Naval Research supported basic research which could be useful to the Navy in the long run. This support was focused largely in hydrodynamics, partial differential equations, complex function theory, and other areas of classical analysis, although some support was distributed more generally over pure mathematics, since Rees and her deputy Joachim Weyl

believed that the health of mathematics as a whole was important for the development of those parts with more direct application to engineering and physics. I was told by Mina Rees that in the early postwar years the major share of ONR's support of mathematics was concentrated in five centers: NYU, MIT, Stanford, and Berkeley in classical analysis, and Tulane in modern analysis and topology.

There was an amusing anecdote about ONR's support of mathematics in those days: The admiral in command of the Office of Naval Research was rehearsing the staff at ONR for an impending inspection by the Chief of Naval Operations. He asked Jo Weyl, who had succeeded Mina Rees as the director of the mathematics branch, "What do we tell the CNO when he asks why we spend all this money for research in mathematics?" Joachim responded with a typical Weyl metaphor: "The tree of science has many branches, but the trunk is mathematics," to which the Commandant said, "No, no, much too high flown! We must have practical examples of the usefulness of mathematics." Consequently, when the Commandant brought the CNO around to the mathematics branch, Weyl responded to the CNO's inquiries by talking about a number of research projects and pointing out their applications and usefulness to topics in physics and engineering. When Weyl had finished, the Commandant turned to the CNO and said, "Perhaps I can explain it this way, Admiral: The tree of science has many branches, but the trunk is mathematics!"

When Loewner, Bergman, Schiffer, and I arrived at Stanford in 1951, work was being completed on a remodelling of Sequoia Hall to house the Applied Mathematics and Statistics Lab. This provided offices the department of statistics and the new arrivals in mathematics. Paul Garabedian, who had returned to Stanford from Berkeley the year before had his office there, as did Patrick Suppes, then an instructor in philosophy with interests in logic and the philosophy of science. There was space for research associates, visitors, and graduate students. We were soon joined by new faculty, Gordon Latta, John McCarthy, Paul Berg, Bob Osserman, Harold Levine, and James McGregor in mathematics, and Herman Chernoff, Charles Stein, Lincoln Moses, and Gerald Lieberman in statistics, and Sam Karlin jointly in mathematics and statistics. Kenneth Arrow and a group of mathematical economists associated with him were affiliated with the laboratory and located in Serra House next door to Sequoia Hall.

Garabedian and Schiffer had a substantial contract from the ONR for work in hydrodynamics. Research associates on this project included Hans Lewy, who was in exile from Berkeley because of his refusal to sign the Loyalty Oath, and Julia Robinson, who was, in those days, precluded from an appointment at Berkeley by Berkeley's anti-nepotism rules.

One of the major projects from the earliest days of the Applied Mathematics and Statistics Laboratory was in quality control and sampling inspection. Originally headed by Al Bowker, it soon came under the joint directorship of Bowker and Gerald Lieberman, who received his degree from the Stanford statistics department in 1953, and became an assistant professor jointly in statistics and industrial engineering. I understand that procedures and tables devised by this project still form the basis for the Department of Defense handbook on procedures for testing and acceptance.

Those were radiant days, and life in the Applied Mathematics and Statistics Laboratory was permeated with excitement and expectation. Garabedian and Schiffer were doing $|a_4| \leq 4$. A number of people including Schiffer, Garabedian, and Szegő were engaged in hydrodynamics. Game theory and decision theory were in the air and applied to many fields. David Blackwell was a regular visitor in those days and was in the midst of his great collaboration with Abe Girshick in their development of statistical decision theory using game-theoretic ideas. Chernoff and Stein were each involved in basic work in mathematical statistics. Ken Arrow and Herb Scarf were applying the new ideas to mathematical economics. Logic and foundational investigations were vigorously pursued with Chen McKinsey, Pat Suppes, and Jean and Herman Rubin. Suppes also began his study of the methodology of the social sciences using some of the concepts from game theory. With Dick Atkinson, who was later director of the National Science Foundation, and others, Suppes succeeded in applying some of these concepts to psychology. Excitement from work in one field was infectious and affected those working in a very different area. In this atmosphere it was easy to believe in the "Unity of Science" and to expect mathematics to be the ideal tool for its understanding and unification.

6. THE MATHEMATICS DEPARTMENT IN THE FIFTIES AND SIXTIES

Gabor Szegő was succeeded as department head by Max Schiffer in 1954. Schiffer was assisted in that role by Al Bowker who served as co-head of the mathematics department, as well as head of statistics and director of the Applied Mathematics and Statistics Laboratory. It was at this time that I began my long involvement in Stanford administration by serving first as assistant head and later as associate head of the mathematics department. Work with Schiffer and Bowker in planning the department's programs and charting its future growth was an exciting educational opportunity for me.

During Szegő's time as head there was an emphasis on complex analysis, but the following years saw growth in other areas of analysis. The first new area in analysis to blossom was that of partial differential equations and its applications to problems in fluid dynamics and mathematical physics. I

think Paul Garabedian was probably the strongest supporter for moving in this direction. His interests were shifting from pure function theory towards applied mathematics even then, and he had collaborated with Hans Lewy while Hans was at Stanford, and he began work with some of the aeronautical engineers coming to Stanford. Of course he was still involved in complex analysis: He collaborated with Schiffer to give the first proof of $|a_4| \leq 4$, with me to prove the 'One-quarter Theorem' for mean univalent functions, and with Spencer to discover an early form of the $\bar{\partial}$ -boundary condition. Schiffer and Szegő were also interested in the partial differential equations of mathematical physics and had collaborated on some problems in fluid dynamics and given bounds for various quantities, including the virtual mass of a body in a fluid.

Thus it was natural to add appointments in this area, and senior appointments were soon made during the latter part of the fifties. These included David Gilbarg in partial differential equations and hydrodynamics, Harold Levine in differential equations and mathematical physics, Robert Finn in nonlinear partial differential equations, particularly fluid flow and the Navier–Stokes equation, and Erhard Heinz in theoretical aspects of differential equations. When Heinz left to return to Germany in 1962, he was succeeded by Lars Hörmander, who spent half the year at Stanford and half at the University of Stockholm.

The Stanford mathematics department also began to acquire strength in broader areas of analysis with the beginning of the sixties: The appointment of Ralph Phillips as a professor, and the appointments of Paul Cohen, Karel deLeeuw, and Don Ornstein as junior faculty members, gave representation in functional analysis, measure and ergodic theory, and harmonic analysis. Yitzhak Katznelson became a frequent faculty visitor, adding additional strength in these areas. Paul Cohen was also interested in partial differential equations and had given an example to show the failure of uniqueness for the Cauchy Problem. The appointment of Kai-Lai Chung, jointly with statistics, continued the tradition of strength in probability, complementing the activities of Karlin and McGregor in stochastic processes. The appointment of Hans Samelson in 1961 continued the tradition of work in Lie groups, as exemplified by Blichfeldt and Loewner. He also gave us representation in topology.

Complex analysis, although no longer in a preëminent position, continued to flourish. Robert Osserman and Newton Hawley were promoted to permanent positions, and there was a distinguished group of younger complex function theorists on short-term appointments, including Jim Hummel, Peter Duren, and Simon Hellerstein. Visitors and research associates in the earlier part of this period included Lars Ahlfors, Lipman Bers, Hans Bremmermann, Fred Gehring, and Albert Pfluger. In 1962 Don Spencer returned

to Stanford from Princeton, and a few years later he was joined by his colleague Kunihiko Kodaira.

In addition to the activity in analysis and topology, this decade also saw the growth of activity in logic and foundations. This was an area that had its origins in the philosophy department with Suppes and McKinsey. Solomon Feferman began his career at Stanford with a junior faculty appointment in mathematics and philosophy in 1956, and the philosophy department appointed John Myhill and Georg Kreisel to professorships. Although these appointments were primarily in the department of philosophy, they also held rank in the mathematics department. Myhill was later replaced by Dana Scott, who, because of his background, had the larger share of his appointment in the mathematics department. At about this time Paul Cohen, as a result of a dinner conversation with Sol Feferman after a Joint Stanford–Berkeley Colloquium, became interested in consistency proofs for arithmetic and succeeded in giving one after an intense period of work. Of course it turned out to use a nonelementary argument at one point (induction up to ε_0) and had similarities with Gentzen’s consistency proof. This led Paul to think about the axiom of choice and the continuum hypothesis and eventually led to his famous proof of their independence.

The Joint Stanford–Berkeley Colloquium was a regular institution in those years. It was started in the early fifties and met on alternate months at Stanford and Berkeley. The speaker was a faculty member (or occasionally a visiting faculty member) from the department at which the talk was *not* held, and a large percentage of the speaker’s colleagues would make the fifty mile trip to the other institution. The dinners were eagerly anticipated as an opportunity to meet our Berkeley colleagues and to share mathematical news and gossip. I first met John Kelley at one of these dinners and discovered our common interest in function algebras. It was at one of these Joint Colloquia held at Stanford that Charles Morrey first announced and described his proof that every compact real analytic manifold can be real-analytically embedded in Euclidean space. My interest in the foundations of geometry first became serious as a result of a Joint Colloquium talk by Tarski.

The two departments were close in those days, with numerous collaborations between their members. Loewner and Pólya often gave courses at Berkeley, and for many years Kelley and I conducted a joint seminar on function algebras for many years, sometimes meeting at Stanford, but far more often at Berkeley. As the two departments grew in the later sixties, attendance dropped off. The newer faculty members never achieved the familiar contact of the older ones, and the Joint Colloquia were discontinued. I am pleased to observe that in very recent years there has been a tendency to have joint Stanford–Berkeley seminars in a number of disciplines, although not with the frequency and universality of the old days.

One term Pólya was giving a course at Berkeley and would go up to one morning each week, spend a night or two there, and return to Palo Alto for the rest of the week. Since I had one of my seminars with Kelley on the day he went up, we arranged to go together by taking a bus that left at 7:30 in the morning and arrived about 9:00. Sitting next to Pólya on a bus for an hour and a half each week was a priceless education. I learned much mathematics and even more about well-known mathematicians and their idiosyncrasies. Many of Pólya's mathematical anecdotes have later been published elsewhere. Our conversations were by no means limited to mathematical topics. Since Pólya had lived in Zürich for many years and I had recently spent a sabbatical there, I asked Pólya about the etiquette of using the familiar 'du' forms in German — a topic that native speakers of English find it difficult to comprehend. He told me, among other things, that Hungarian also has familiar forms and their usage is as subtle as the German. He said that since coming to America he and Szegő always spoke English because they could never decide about the use of the familiar when they spoke Hungarian or German. I particularly enjoyed Pólya's accounts of Switzerland and of his times at Cambridge with Hardy and Littlewood. Pólya told me that he felt that he never really knew Hardy personally and that most of his information about Hardy's personality came from Littlewood, with whom Pólya was quite close.

David Gilbarg succeeded Max Schiffer as executive head of the mathematics department in 1959. Although the leadership of the department had become more collective by this time with many of the senior faculty actively involved in departmental decision and planning, Gilbarg led the department with deftness during a period of major growth. He was in the forefront of our recruiting efforts and took the lead in setting high standards for appointment and promotion. The department prospered under his guidance and direction.

I conclude this section by giving the department's roster for the year 1967–1968:

Emeriti: Stefan Bergman, Charles Loewner, William A. Manning, George Pólya, Gabor Szegő.

Professors: Harold M. Bacon, Paul W. Berg, Kai-Lai Chung, Paul J. Cohen, Karel deLeeuw, Robert Finn, David Gilbarg, Samuel Karlin, Kunihiko Kodaira, Georg Kreisel, Harold Levine, James L. McGregor, Robert Osserman, Ralph Phillips, Halsey Royden, Hans Samelson, Menahem M. Schiffer, Dana S. Scott, Donald C. Spencer.

Associate Professors: Solomon Feferman, Newton Hawley, Donald Ornstein.

Assistant Professors: Michael G. Crandall, Paul Rabinowitz, Mary V. Sunseri, Alan Howard, Amnon Pazy, Ngo Van Que.

Instructors: Robert O. Burdick, Mark A. Pinsky, John B. Walsh.

7. THE DEPARTMENT OF COMPUTER SCIENCE

During the Second World War the Statistical Research Group at Columbia had been calculating mathematical and statistical tables, and this activity was continued at Stanford in the Applied Mathematics and Statistics Laboratory. Gladys Rappaport (later Gladys Garabedian), who had worked for Bowker at the SRG, was in charge of this activity. In those days computers were young women with Marchant calculators, who calculated by following the steps of a program written by Gladys, who supervised and checked their work.

In 1952 Bowker and Fred Terman, then dean of engineering, decided that Stanford should have a computer jointly funded by the Applied Mathematics and Statistics Laboratory and the School of Engineering. Stanford's first machine was an IBM Card Programmed Calculator. The steps of a computation were controlled by a sequence of punched cards and a plug board. It had an electromechanical memory which would store all of 16 words. Although very primitive by of present day standards, it was a great improvement over manual computing. With the advent of this machine a Computation Center was formally established in 1953 with Jack Herriot from mathematics and Alan Peterson from electrical engineering as co-directors. The entire staff of the center consisted of the co-directors and a secretary. Stanford got its first real real computer in 1956 with the installation of an IBM 650. It had electronic operation and 2000 words of drum memory. In a few years this was replaced by a Burroughs 220.

The Computation Center had expanded considerably by 1963, and a new building was built to house it. At Bowker's instigation this building was called "Polya Hall" in honor of George Pólya. When Pólya was asked if the building could be for him, he replied that it was all right as long as everyone understood that he had not contributed any money for it. At that time the Computation Center acquired two new machines, an IBM 7094 and a Burroughs 5000. The program for the Burroughs (and I presume also for the IBM) was still entered on punched cards, but the program was written in the Burroughs' version of ALGOL, and then compiled by the machine. I don't know the memory capacity of this machine, but John McCarthy was considered a dreamer for talking about having a machine with a million words of memory one day.

With his long connection with computing projects Al Bowker believed it was desirable to have faculty members whose area of expertise was in

computing. He was strongly supported in this view by Paul Garabedian, who had used a significant amount of hand calculating in showing $|a_4| \leq 4$ and already had some notion of the possibilities for computing in fluid dynamics. Accordingly, it was arranged in 1956 to create a new position in the mathematics department for someone in the field of computing.

The natural choice to fill this position was George Forsythe, and I believe he was the only one ever seriously considered for it. He had spent a year as an instructor at Stanford and was well known to everyone here and had been a classmate of Jack Herriot, who was then the director of the computation center. He worked in numerical analysis applied to partial differential equations and conformal mapping and had been involved in joint work with Schiffer on error estimates in conformal mapping. He came to Stanford from UCLA, where he was part of the group working on the Bureau of Standards machine SWAC (Standards West Automatic Computer).

Forsythe became the apostle at Stanford for the newly emerging discipline of "computer science." At that time most of us thought of computer science as dealing with mathematical or statistical computations and numerical analysis, but George was well aware of the new developments in such fields as programming languages, artificial intelligence, machine translation of languages, and he had a remarkably prescient vision of the future shape of the discipline.

A division of computer science was established in the mathematics department in 1962 with Forsythe as Director. The first year's faculty consisted of Forsythe and Herriot together with several visitors. The new division received strong support and encouragement from the provost, Fred Terman, and from Al Bowker, who was his assistant and later graduate dean.

The first appointment made specifically in computer science at Stanford was that of John McCarthy in 1963. He had been an assistant professor of mathematics at Stanford for three years after he got his Ph.D. (in differential equations) with Lefschetz at Princeton. Before his return to Stanford he was a professor at MIT, where he had developed LISP and was active in the original work on artificial intelligence. It was McCarthy, in fact, who coined the name "artificial intelligence."

This appointment was soon followed by those of Gene Golub and Niklaus Wirth as assistant professors. Golub is a numerical analyst specializing in numerical linear algebra. Niklaus Wirth was the originator of the programming language Pascal.

In 1964 the division became the department of computer science. Al Bowker, then graduate dean, and I, who had become an associate dean of the

school of humanities and sciences, were involved at the administrative level with the formation of the new department, and Bowker and Terman had played a significant role earlier in establishing computing at Stanford, but it was George Forsythe who was the guiding spirit in the establishment of computer science at Stanford. He chaired the division and then the department of computer science and was also the director of the computation center from 1961 until he was succeeded by Ed Feigenbaum in 1965. Not only did he have a well conceived notion of what should be done, he also had the Quaker knack of building a consensus for getting it accomplished. Although he was a father figure to the members of his department, he considered himself a numerical analyst and sometimes remarked wryly that numerical analysts had gone from being those odd people in a mathematics department to become those odd people in a computer science department.

The following year William F. Miller was appointed jointly as a Professor in the new department and in the Stanford Linear Accelerator Center, and Edward Feigenbaum was appointed to an associate professorship. The year after saw the joint appointment of George Dantzig as professor of computer science and operations research.

I conclude this section by giving the department's roster for the year 1969–1970:

Professors: George B. Dantzig, Edward A. Feigenbaum, George E. Forsythe, John G. Herriot, Donald Knuth, John McCarthy, Edward J. McCluskey, William F. Miller.

Visiting: C. William Gear, James H. Wilkerson.

Associate Professors: Jerome A. Feldman, Robert W. Floyd, Gene H. Golub.

Assistant Professors: Zohar Manna, D. Rajagopal Reddy.

8. OPERATIONS RESEARCH

The discipline of operations research began during World War II with the application of mathematical techniques to solve various problems of optimization. Further research in this area was conducted at the Rand Corporation and other places, and Ken Arrow, who held a joint appointment in economics and statistics at Stanford, and Sam Karlin, who came to Stanford in 1956 as a professor of mathematics and statistics, were associated with some of the activity at Rand.

The first formal course in operations research at Stanford was taught by Gerald Lieberman in 1957. In 1960 the provost, Fred Terman, at the urging of Al Bowker, established a committee to consider the future of operations research as a discipline at Stanford. The committee was chaired by Lieberman,

who held a joint appointment in statistics and industrial engineering, and included K. J. Arrow from economics and statistics, Sam Karlin from mathematics and statistics, James Howell from the Business School, Pete Veinott from industrial engineering, and Harvey Wagner from industrial engineering and statistics. A Ph.D. program in Operations Research was established as a result of this committee's study. This program had a committee in charge with the authority to admit graduate students to the Ph.D. program and to control the curriculum for that program. The following year the program was authorized to make appointments, provided they were joint with an established department. The first such appointment was that of George Dantzig, one of the pioneers in the field and the inventor of the Simplex Method, one of the cornerstones of operations research. He was appointed jointly with computer science.

The program flourished under Lieberman's leadership, but the administrative arrangements, which involved reporting to three separate deans, were cumbersome. Hence the program welcomed the opportunity to become a department in the school of engineering in 1967. The new department's roster for the academic year '67-'68 was the following:

Professors: George B. Dantzig, Donald Iglehart, Rudolf E. Kalman, Gerald J. Lieberman, Alan S. Manne, Arthur F. Veinott.

Associate Professor: Frederick S. Hillier.

Assistant Professor: Richard W. Cottle.

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Studying Under Pólya and Szegő at Stanford ¹

P. C. ROSENBLOOM

In September 1941, I started my graduate work at Stanford after graduating from Pennsylvania. Since there were few graduate students in mathematics then, and I had covered many of the elementary graduate courses by independent reading, I was permitted to register for a rather light course load. This consisted of a course in differential geometry with Uspensky, one in group theory with Manning, and advanced reading and research with Szegő. My teaching load was 6 hours of college algebra and trigonometry, under the supervision of Bacon, at a salary of \$700 for 9 months. After Pearl Harbor that December, there was a tremendous increase in enrollment in mathematics. My load was increased to 9 hours, and Blichfeldt was pressed to come out of retirement to teach again.

It may be of some interest to note that I paid \$25 a month for room and board which, because of inflation, was increased to \$35 the following year. On Sunday evenings I would splurge with a full-course restaurant dinner for 45 cents. Still I saved enough money to pay my expenses to the American Mathematical Society meeting at Vassar in the summer of 1942.

Szegő met me once a week to discuss my progress. My assignment was to do problems in Pólya-Szegő and to read Titchmarsh's *Theory of Functions*. This weekly meeting gave me a feeling of responsibility to have something to report so as not to waste Szegő's time. I have found that such regular meetings have the same effect on my own students and so have followed this practice ever since. Often Szegő would discuss the ramifications of the problems and related results in the recent literature. He would point out natural questions for further investigations.

We had a weekly seminar. Since I was the only serious graduate student in mathematics, the members were Szegő, Schaeffer, Hille (on sabbatical), Forsythe (then an instructor), Doyle (an assistant professor), and myself.

¹ Reprinted with permission from *Gabor Szegő: Collected Papers* (Birkhauser; Boston-Basel-Stuttgart, 1982) Vol I, pp. 12-13.

Hille returned to Yale the following year, but Pólya and Spencer joined the department then. Hille lectured on the Nevanlinna theory and on the Gelfond-Schneider solution of Hilbert's problem (later written up for the Monthly). I was assigned to present Brun's twin prime theorem and Ahlfors's thesis. The second year Spencer lectured on multivalent functions, and Schaeffer on Schiffer's variational method in conformal mapping. That was when their collaboration on univalent functions began.

I was given a desk in Schaeffer's office and had many opportunities for informal discussions with him. I earned a little extra money by babysitting for him, and also for Spencer the next year.

Szegő gave an evening course for engineers, sponsored by the Army as part of the defense program, on functions of a complex variable. He gave me the job of writing up the notes. His lectures were models of clarity and well spiced with physical applications. It was instructive to observe his careful attention to motivation without any compromise on mathematical standards.

At that time at Stanford, the language examinations for doctoral candidates in the sciences were administered by the scientists. That year Ogg, of the chemistry department, was in charge. When I called him, he said I should just come over to his office with some mathematics books in French and German. At the sight of Kuratowski's book, he said, "I've never studied topology. Read me some of that." After hearing several pages of topology, he tested me in German with Bieberbach's function theory. I have never encountered such a rational procedure at any other university since then!

Every couple of weeks Szegő would invite me home for dinner. It was a charming family circle with his wife, his son Peter, and his daughter Veronica. Since Mrs. Szegő had a degree in chemistry and Peter, a high school student, was already interested in engineering, the conversation at the dinner table often concerned science and mathematics and frequently became quite technical. I first heard the proof of Picard's theorem, starting with the construction of the modular function by conformal mapping, over the Szegős' dinner table. Szegő had an orchard behind his house and would often invite me to pick a basketful of fruit for my landlady to preserve.

At my preliminary examination, which was then oral at Stanford, Szegő asked me to prove Picard's theorem. I had never expected such a question but couldn't deny acquaintance with it since I had heard it from his own lips. He pushed me, in Socratic style, to stumble through it.

My first paper, on Post algebras, written as an undergraduate, had been accepted by the *American Journal*. Szegő said I could submit it as my thesis, and he would arrange for an outside expert to judge it, since there was then no logician at Stanford. But I didn't want to miss the chance to learn analysis from him. I had started, in the winter of 1941, to try to do for the sections of the power series for the error function what Szegő had done for

the exponential series in 1924. During my first week on this, I made some stupid mistakes which are still embarrassing to recall. Szegő was patient and kind, and encouraged me to persist.

When I broached the question of a thesis topic, he said I should wait until Pólya came. He visited in the spring of 1942 and, at Szegő's home, suggested that I try to prove the results on sections of power series for entire functions which Carlson had announced in 1924, but whose proofs had never been published.

Szegő continued our weekly meetings, but I met with Pólya more irregularly. At one point, when I was floundering, Pólya suggested that I try to apply problems 107–112 in the first volume of Pólya-Szegő, and these turned out to be crucial. I was soon able to handle entire functions of infinite order and the radial distribution of the zeros of sections of entire functions of finite order. But I was having trouble with the angular distribution. Then one day, Szegő had lunch with me and constructed a heuristic argument on a paper napkin. This hint enabled me to overcome my difficulties. When I told Pólya my results, he invited me to his home. The whole evening he pestered me with questions about why my proof worked, whether my solution was a special trick or an instance of some systematic method. I learned more from that one evening than from any other single experience in my career.

In January 1943, I received an offer of an instructorship at Brown, providing that I could start in February. Szegő arranged for me to take my final orals immediately, even though my thesis wasn't written yet. I was asked to outline my main results thus far, and then the committee probed me with general questions. Pólya began asking me to give the definition of Gaussian curvature in terms of the area of a spherical map by the normals. I protested that I had never studied it, but he insisted that I try to work it out at the blackboard. He said, "It is not forbidden to learn something from an examination!"

Szegő had a broad and deep knowledge of general theories, but he preferred to work on concrete problems which test the power of these theories. He had an amazing technical facility. The late C. Loewner used to call him a virtuoso.

It is perhaps impossible to impart this technique to others, but he did influence me by his taste, his ways of looking at and attacking problems, and his insights into the general significance of particular results. I hope he also influenced me by his broader concern for my progress as a teacher, my teaching load, my material welfare, and even such acts as giving me a ticket to Bartók's concert at Stanford. I don't know whether he was able to continue giving such personal attention to students when so many more began to come to Stanford in the late '50s, but I was lucky to have had this opportunity when it was possible.