Charlotte Angas Scott (1858–1931)

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BIOGRAPHY

Charlotte Angas Scott was born on June 8, 1858, in Lincoln, England, the second of seven children of Caleb (1831–1919) and Eliza Exley Scott. The only extant information about her mother is references in her father's obituaries. They report that the marriage was "a source of profound happiness" to him and that she died in 1899 when he was on his way home from the United States, where he had attended the International Congregational Council and visited his "eldest daughter at Bryn Mawr" ("Ministers Deceased" 1919).[1]

However, a great deal is known about Caleb and his father, Walter Scott (1779–1858), because they were both ministers of the Congregational Church and presidents of colleges training such ministers. Walter Scott was a hard-driving man who struggled for education of the working classes and against slavery and alcohol consumption. His eighth offspring, Caleb, had had three successful years in business and had obtained two degrees by the age of twenty-three. Since their religion was "Non-conformist," and Cambridge and Oxford Universities required a vow of loyalty to the Church of England, Walter and Caleb developed alternative sources of education for young men of their religion.

Since there were no colleges in England open to women while Charlotte Scott was growing up, and almost no secondary schools either, the support of her family and church was indispensable to her education. In a speech to newly elected deacons, Caleb admonished their wives, "Let the innocent tastes and tendencies of youth not be all repressed and stifled in the iron mould of any conventionalism" (Scott 1865). This was a man who encouraged his family to think and to enjoy life, and Scott's later writing indicates

[1]Patricia Clark Kenschaft, "Charlotte Angas Scott (1858–1931)," in *WOMEN OF MATHEMATICS A Biographic Sourcebook*, Louise S. Grinstein, and Paul J. Campbell, eds. (Greenwood Press, Inc., Westport, CT, 1987), pp. 193–203.

that mathematical games were part of their home entertainment. In 1865 Caleb became principal of the Lancashire Independent College (now called the Congregational College) and thus was able to provide good tutors for an ambitious daughter.

In 1876, at the age of eighteen, she won a scholarship on the basis of home tutoring to the recently opened Girton College. Most of her classmates had never attended a secondary school either. However, secondary schools for girls were springing up in England, so educated women suddenly had career opportunities as teachers. Thus there were eleven students, an unprecedented number, in Scott's entering class at Girton College, the first college in England for women. Life was austere. "When retiring for study after an extremely simple 'tea' in the Commons, they would pick up three things en route to their rooms...two candles, a bucket of coals, and a chamber pot" (Silver 1981).

Girton College had opened in 1869 with five students at a different location and in 1873 had moved to a modest three miles from Cambridge University, thereby enabling its students to attend the lectures of the twenty-two (out of thirty-four) Cambridge professors who were willing to let women listen to them. Such women had to be carefully chaperoned, because until 1894 Cambridge University maintained the "Spinning House," a special prison for prostitutes and "suspected prostitutes," where any unescorted woman would be summarily sent, her entire future thereby ruined. One student of the 1890s told her son-in-law that women attending lectures sat in the back behind a screen, obviously posing special problems to mathematics students (Silver 1981).

Any further instruction was from idealistic, or at least flexible, young tutors. Since the male Cambridge undergraduates received bachelor's degrees with honors by taking the Tripos examinations, the women wanted to pass these examinations too. Three of the first five students had done so in 1872, and songs in the memory of these "Girton Pioneers" were sung during the long dark winter evenings of Scott's student days.

Women would not receive degrees at Cambridge until 1948, but every year after 1872 women applied to take the Tripos exams, and some were given special permission to do so. On nine bitterly cold days in January 1880, Charlotte Scott spent over fifty hours taking the mathematics Tripos. When word leaked out that she had done as well as the eighth man in the entire university, the news permeated England that a woman had succeeded in a "man's" subject.

Because she was female, she could not be present at the award ceremony, nor could her name be officially mentioned. However, a contemporary report says, "The man read out the names and when he came to 'eighth,' before he could say the name, all the undergraduates called out 'Scott of Girton,' and cheered tremendously, shouting her name over and over again with tremendous cheers and waving of hats." The young men of Cambridge gave honor where it was due, even though their elders followed the established rules. At Girton College there were cheers and clapping at dinner, and a special evening ceremony where she was led up an "avenue of students" while they sang "See the Conquering Hero Comes." She stood on "a sort of dais" while an ode written by a staff member was read to her, and then she was crowned with laurels, "while we clapped and applauded with all our might" (Megson and Lindsay 1961, 31).

In 1922 James Harkness, who was only a schoolboy in 1880, remembered that Scott's achievement impressed even him at the time, its widespread impact marking "the turning point in England from the theoretical feminism of Mill and others to the practical education and political advances of the present time" (Putnam 1922). The publicity resulted in pressure on Cambridge University to admit its resident female students to university examinations as a matter of policy, not just special privilege, and to post their names with those of the male students, an important step toward qualifying for jobs. After a year of controversy, this resolution was passed on February 24, 1881. Its national implications are reflected by the fact that at the newly opened college for women at Oxford, the news was proclaimed loudly in the dining room, "We have won! We have won!" (Bradbrook 1969, 55).

Arthur Cayley, a renowned algebraist, was one of those leading the effort for this recognition of women's education, and for the rest of his life, Scott was the recipient of "his kindness" (Scott 1895). She attended his lectures, did her graduate research under him, and obtained her first and only position outside Girton College on the basis of his recommendation. Meanwhile, she was hired as a resident lecturer by Girton College and taught there until receiving her doctorate in 1885.

Although by Scott's time Cambridge University no longer required an oath of allegiance to the Church of England, it would not grant her a degree, because of her sex. Fortunately, the University of London began granting "external" degrees to women in 1876, so Scott took two entirely different sets of examinations from two universities, one to place her with her peers, and the other to obtain degrees. She thus received a B.Sc. in 1882 and a D.Sc. in 1885 from the University of London, both "First Class," the highest possible rank.

Bryn Mawr College, which opened in Pennsylvania in 1885, was dedicated to providing both undergraduate and graduate education of the highest level to women. Since comparable positions for women were virtually nonexistent in Europe, Scott went to Bryn Mawr, becoming its first mathematics department head and the only mathematician on its founding faculty of eight. There was one other woman, a biologist. There were no better options in the world for a woman mathematician during the next forty years, so Scott remained there.

Occasionally her father or brother Walter visited her at Bryn Mawr. Her older sister, with whom she had grown up, died the spring before Scott left for Girton College, and her youngest sister died as an infant; so in her adulthood she was the oldest of five siblings, with two younger brothers and two younger sisters. Her will also mentions her "beloved" sister-in-law, Walter's widow. Walter, who was in the machinery business, died suddenly in Scott's home on August 7, 1918, a great blow to her. One of her sisters worked for a while in an orphanage and then married. The other remained home and cared for her father, Caleb, in his old age. The family was a close and loving one; surviving relatives remember with affection "Auntie Charley [pronounced 'Sharly']."

The early Girton College community had strictly observed the social mores of the time. The existence of the Spinning House left little margin for experimentation, and the prevailing opinion was that personal conservatism was required to promote women's educational and political equality. Charlotte Scott maintained this view throughout her life, disapproving of smoking and makeup, but her disapproval extended equally to both sexes. She bobbed her hair before arriving at Bryn Mawr in 1885, although short hair for women was still controversial in the 1920s. She had at least one close male friend outside her family, Frank Morley, whose time studying mathematics at Cambridge University overlapped hers. He told his son that the social conventions made it more acceptable for her to visit him and his family in Baltimore than for him to visit her, and she did so often.

Scott's relationship with M. Carey Thomas, the first dean of Bryn Mawr College and its president from 1894 to 1922, was always formal, despite the fact that Scott was only one year younger. Thomas had become the first American woman to earn a doctorate in any field (linguistics), in 1882 at the University of Zurich, and had visited Girton College on her way home. A biographer of Thomas says that Scott was hurt by her initial coldness after Scott's lonely trip across the ocean (Finch 1947, 194). Thomas had the impatience of many dynamic reformers, and her correspondence with Scott also includes a confession that mathematics had always been her most difficult subject, suggesting a special tension because of this. In 1906 Scott wrote a letter apparently in response to Thomas's desire to know when a certain student would finish her Ph.D. Patiently she explained, "If it were simply a matter of surveying the field, collating papers and stating the contents clearly, she could do the thesis before June certainly; but to produce an original piece of work is quite another matter..." (Scott Papers). Thomas's lack of knowledge about mathematics is also reflected in much earlier correspondence about the necessity of mathematics journals for the library; Scott was always fighting for her discipline on her home turf.

During Scott's first three years at Bryn Mawr, there was a total of only four serious mathematics students—three undergraduates and one "graduate" student who had studied nothing higher than differential equations before she came. Scott worked intensely, writing her lecture notes "*after*, not before, the lecture... at the end of a busy day... word perfect... knowing that at nine a.m. tomorrow [she would give another lecture].... But the next delivery showed no lack of spontaneity for changes and improvement were made until the notes could be, and as a matter of fact were, used as text-book material" (Maddison and Lehr 1932). Gradually her classes grew larger, and by her ninth year there were six new mathematics students, two undergraduates and four graduates, including her first two successful doctoral candidates. Indeed, three of the nine American women to earn doctorates in mathematics in the nineteenth century studied with her. Her professional correspondence shows her intense involvement with each student, arguing against the doctoral candidacy of one who demonstrated "everything except that one essential, capacity" for doctoral work, and for one who has been discovered to have tuberculosis but has already published good work. She pleads on behalf of a student who inadvertently left a notebook in an examination room, and against those sitting on a fire escape to eavesdrop on a faculty meeting. Former students remembered her kindness and her ability to help them solve their problems.

Her Girton propriety and calm exterior slipped on January 12, 1898, when she wrote to President Thomas:

> I am most disturbed and disappointed at present to find you taking the position that intellectual pursuits must be "watered down" to make them suitable for women, and that a lower standard must be adopted in a woman's college than in a man's. I do not expect any of the other members of the faculty to feel this way about it; they, like (nearly) all men that I have known, doubtless take an attitude of toleration, half amused and half kindly, on the whole question; for even where men are willing to help in women's education, it is with an inward reserve of condescension. (Scott Papers)

The word "nearly" is inserted in small lettering above the handwritten letter. It is indeed unfortunate that Scott's entire correspondence with her family has apparently been lost.

Thomas wrote to her niece in 1932 that "in my generation marriage and an academic career was impossible" (Dobkin 1979, xv), and this fact was basic to Scott's life too. She wanted to build her own house but was unable to find a suitable plot, so in 1894 she moved from a small apartment on the Bryn Mawr campus to a house rented from the college. Her cousin Eliza Nevins joined her to become her companion and housekeeper until Nevin's death in 1928. Others, including an early doctoral student, lived with them occasionally. Scott was a leader among the tenants in campaigning for such mundane matters as access to direct paths and more effective heating of the homes. On February 27, 1901, her own house caught fire; the house was saved, but Scott could not live in it for months afterward.

An even more serious disruption occurred in the spring of 1906, when she developed an acute case of rheumatoid arthritis. After that her ill health and her increasing deafness, which was apparent even in her Girton College days and was complete by the time anyone now living knew her, marred her life significantly. Her publications ceased for two decades, and the doctor recommended outside exercise. Gardening was compatible with her academic duties; and her garden was "brought, year after year, unbelievably, to greater beauty" (Maddison and Lehr 1932). She developed a new strain of chrysanthemum. Her correspondence reveals the zest with which she continued to live. "I am not a Vandal, as you know; but this tree is not good, it simply encourages visitors of objectionable kinds, beginning with scab and continuing accordingly, and any miserable little apples that it does produce are infected with maggots. My wish is to cut it down and dig it up, and then plant a less troublesome tree a few feet away, so as not to spoil the appearance of the slope" (Scott Papers).

Scott maintained her church membership in England for at least a decade after she came to the United States. American mathematicians joked about her leaving for Europe every spring as soon as exams were marked, but this was not literally true. Still, she crossed the Atlantic Ocean often, at a time when each voyage involved at least a week of discomfort and danger. She thus provided an invaluable link between the fledgling mathematical community of the United States and the established centers in Europe.

Scott officially retired in 1924 but remained an extra year at Bryn Mawr to help her seventh and last doctoral graduate complete her dissertation. Then she moved to a large house on the bus line halfway between Girton College and the center of Cambridge University. Her complete deafness made social interactions difficult, even with her next door neighbor, who also happened to be a retired mathematician. Her primary diversion was betting on horses, an activity to which she applied mathematicial statistics. Her doctor, who had introduced her to his own bookie, Mr. Cook, believed that she neither gained nor lost much money. However, he was amused how her Victorian outlook affected her view of Cook. One Christmas when he visited her home, she was extremely agitated. "Dr. Nourse, I am very worried. Do you see that umbrella in the corner? That has been sent to me by Mr. Cook. Of course I couldn't accept it!" The doctor explained that the bookie sent umbrellas to all his women clients and purses to the men and would feel hurt if the presents were returned. "Do you really think I can keep it?" Scott replied, obviously relieved that the umbrella was not an indication of moral turpitude.

On November 10, 1931, she died quietly in Cambridge. She was buried with Miss Nevins in St. Peter's part of the St. Giles's Churchyard in Cambridge near the northwest corner of the chapel. The inscription on a small stone gives only her date of death and age and no indication of her place in the history of mathematics.

Although she seems almost forgotten today, Scott received many honors in her lifetime. Rebière, writing in Paris in 1897, called Scott "one of the best living mathematicians" with no apparent need to justify its claim. She was the only woman starred in the first edition of *American Men of Science* (i.e. considered prominent in mathematics by her contemporaries) and the only mathematician included in *Notable American Women, 1607–1950*.

Her honors at Cambridge in 1880 were informal because she was female, but they had a lasting impact on women everywhere. Later honors by academic institutions were official. She was the chief examiner in mathematics of the College Entrance Examination Board in 1902 and 1903. In 1909 the alumnae of Bryn Mawr honored her with the college's first endowed chair. When she retired, the board of directors of Bryn Mawr College cited her contribution to the college in its first forty years as "second only to that of President Thomas."

On April 18, 1922, the American Mathematical Society met at Bryn Mawr, and about 200 people gathered in her honor. Alfred North Whitehead gave the featured talk on "Some principles of physical science." Although it was his first trip across the Atlantic Ocean, he refused invitations from Harvard and Columbia universities because he did not want competing attractions in Scott's "neighborhood." At the end of his talk Whitehead observed, "A friendship of peoples is the outcome of personal relations. A life's work such as that of Professor Charlotte Angas Scott is worth more to the world than many anxious efforts of diplomatists. She is a great example of the universal brotherhood of civilizations" (Putnam 1922).

WORK

When the New York Mathematical Society opened its membership to people outside New York, Scott immediately responded, and she was one of the major organizers who developed the group into the American Mathematical Society (AMS) in 1891. She served on its council from 1891 to 1894 and again from 1899 to 1902, and was its vice-president in 1905–1906. When Thomas Fiske gave an anniversary talk in 1938 reviewing the first fifty years of the society, he cited the work of about thirty people, of whom Scott was the only woman.

She brought experience as a member of established European societies, including the London Mathematical Society, the Edinburgh Mathematical Society, the Deutsche Mathematiker-Vereinigung, the Circolo Matematico di

Palermo, and as an "honorary member" of the Amsterdam Mathematical Society. She was one of only seventeen Americans who attended the World Congress of Mathematicians in 1900, and she wrote an extensive report of it for the *Bulletin* of the AMS. Since Scott's field (algebraic geometry) was the same as that of both the father and the future dissertation advisor of Emmy Noether*, both of whom attended the congress and must have conferred with Scott there, perhaps it is not coincidence that Emmy Noether switched fields that summer from languages (more common for young women) to mathematics (still largely male-dominated).

In 1899 Scott became coeditor of the eminent *American Journal of Mathematics*, an influential position she held for twenty-seven years. Her own papers were published not only in American journals, but also in the more competitive European publications, where American mathematicians appeared extremely rarely.

Her book, *An Introductory Account of Certain Modern Ideas and Methods in Plane Analytical Geometry*, was published in 1894 and reprinted, essentially without change, thirty years later. Although its title includes the word "introductory," and it was indeed used by many beginners, it took its readers to the edges of research. It was used widely. Cole's review (1896) praised its inclusion of such recent concepts as groups, subgroups, invariants, and covariants. However, even more far-reaching than its subject matter was its obvious "distinction between a general principle and a particular example." Scott was one of the first textbook writers, especially those writing in English, to be "perfectly aware" of this distinction and to teach it to the next generations of college mathematics students. Her other book, a "school" book about plane geometry, was not well received, because she based her development on lines instead of points, an innovation that was not widely adopted.

F. S. Macaulay's obituary of her summarized, "Miss Scott was a geometer who whenever possible brought to analytical geometry the full resources of pure geometrical reasoning" (1932, 232). Her published research, like most mathematical writing of her time, consisted of discussions of various specific mathematical phenomena. Her specialty was the geometric interpretations of algebraic expressions in two variables of degree greater than two, that is, of plane curves neither linear nor quadratic. However, she had a keener sense of the difference between example and proof than most of her contemporaries, playing an important role in the transition to the twentieth-century custom of presenting mathematics via abstract proofs. Her most notable paper may be her 1899 "geometric" proof of a theorem of Max Noether, Emmy Noether's father. Unfortunately for Scott's fame, her particular field fell out of fashion in the twentieth century.

*Cross-reference to other women discussed in the volume is given by an asterisk following the first mention in a chapter of the individual's name.

She was hired by Bryn Mawr College to be department head, to teach ten or eleven hours a week of both graduate and undergraduate courses, and to supervise graduate research. Although her written offer in 1884 said that her hours of teaching would be diminished as her other duties grew, they were still at their original level thirty years later. Committees also absorbed much time. "She would··· sit through a long meeting··· and at just the right moment make a brief, incisive speech which—such was the respect with which her opinion was regarded—often turned the vote from the direction in which it was tending to the side which she supported."

Her impact on mathematics education in the United States was enormous. Although Harvard University had dropped its requirements that all freshmen take a course in addition, subtraction, and multiplication only fifty years earlier, her initial requirements for students entering Bryn Mawr College included passing examinations in arithmetic, plane geometry, and algebra through quadratic equations and geometric progressions. Students who did not pass admission examinations in solid geometry and trigonometry had to pass courses in these subjects before graduation. Mathematics majors were required to take one semester of algebra and the theory of equations, a year of differential and integral calculus, and a semester of differential equations and elements of finite differences. Early Bryn Mawr students took another sequence concurrently in "analytical geometry," one year in two dimensions and another in three.

During her early years at Bryn Mawr, she was distressed at the amount of time she spent writing and grading entrance examinations, so she worked for a nationwide testing service. The College Entrance Examination Board began in 1901, and she was its chief examiner in mathematics in 1902 and 1903, setting standards that have changed little in over eighty years, although, ironically, the name of their promulgator is rarely mentioned.

Scott was a special inspiration to women, who received three times the percentage of American Ph.D.'s in mathematics before 1940 than they did in the 1950s. She herself was the dissertation advisor of seven women, and Bryn Mawr conferred two other Ph.D.'s in mathematics while she was department head. During this time Bryn Mawr College was third only to the University of Chicago and Cornell University, both much larger institutions, in the number of doctorates in mathematics granted to women in mathematics. When she had delivered her talk to the AMS in 1905, nine of the forty-five listeners were women, only two of whom were from Bryn Mawr. It is difficult to measure influence by numbers, but her visibility, her conversations, and her preparation of many women to teach younger women clearly had a major impact on the academic and economic position of women in America.

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EDWARD BURR VAN VLECK

1863–1943

BY RUDOLPH E. LANGER AND MARK H. INGRAHAM

"THE ILLUSTRIOUS SON of a distinguished father and the distinguished father of an illustrious son" was the description given by Dean Holgate, of Northwestern University, of Professor Van Vleck at a dinner of the American Mathematical Society. This description was not only literally true but also symbolically true. Professor Van Vleck was a scholar who to a superlative degree inherited the intellectual and cultural riches of the ages and succeeded in his determination to transmit these enhanced to coming generations.

Edward Burr Van Vleck was born in Middletown, Connecticut, on June 7, 1863. His father, John Monroe Van Vleck, was Professor of Mathematics and Astronomy at Wesleyan University from which he had graduated in 1850 at the age of seventeen and where he taught from 1854 until his death in 1912. Moreover, he frequently acted as president of the University. The Van Vleck Observatory at Wesleyan was named after John Monroe Van Vleck. The Van Vlecks were an ancient family of Maastricht, Holland; and Tielman Van Vleck in 1658 came to America, where he became one of the founders of Jersey City after a period as a notary in New Amsterdam. The family, through the generations, like many other Dutch families, moved up the Hudson Valley—John Monroe Van Vleck being born at Stone Ridge, New York. There was also a large strain of French Huguenot blood in his ancestry. Professor Edward Burr Van Vleck's mother was born Ellen Maria Burr, of Middle-

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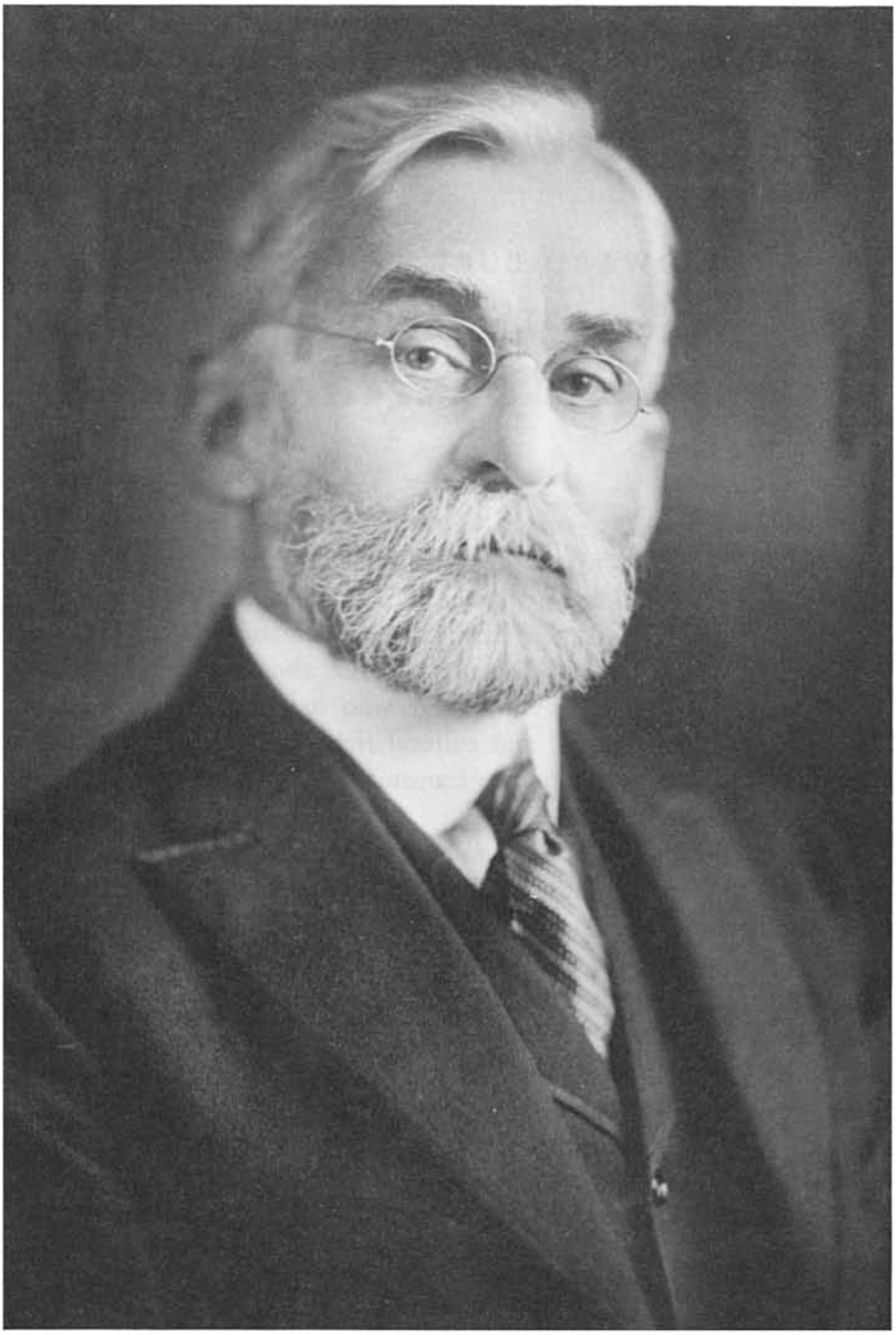

Edward B. Van Vleck

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town, Connecticut, and was chiefly of English descent from stock that had come to New England as early as 1635.

Young Van Vleck's education was in the schools of Middletown and at Wilbraham Academy. He graduated from Wesleyan University in 1884. Endowed with a brilliant mind, blessed with good health, but being quite devoid of athletic skill, he early turned to highly intellectual interests. (His collector's instinct was also shown in youth in his enthusiastic acquisition of stamps.) He found it difficult to decide whether his major interest would be the classics, especially Greek literature, or mathematics. After graduating from Wesleyan, he studied mathematics and mathematical physics at Johns Hopkins University from 1885 to 1887 and taught at Wesleyan from 1887 to 1890. From 1890 to 1893, when he received his Ph.D. degree, he studied at Göttingen, where he formed lifelong friendships with his fellow students (some of them American) and with his major professor, Felix Klein, who had great influence upon Van Vleck as he had upon many others of his students. He always regretted that this period of study had not come somewhat earlier, as from it dates his career as a productive mathematician. The rest of his official career was spent at Wesleyan University, where he was Assistant Professor from 1895 to 1898 and Professor from 1898 to 1905, and at the University of Wisconsin, where he was Instructor from 1893 to 1895 and Professor from 1906 until his retirement as Professor Emeritus in 1929.

Upon his return from Germany in 1893 he married Hester L. Raymond, of Lyme, Connecticut. They had one son, John Hasbrouck Van Vleck, now Hollis Professor of Mathematics and Natural Philosophy and Dean of Applied Science, at Harvard University. Professor Van Vleck's home life was a well from which flowed the quality of his work, his cultural interests, and the influence he had upon his friends—an influence based on intellectual vigor tempered by a fundamental serenity of spirit. Mrs. Van Vleck had much to do not only with her husband's happiness but also with his effectiveness.

Note should be made of three other aspects of his life apart from his research: his interest in literature and the fine arts, his love of travel, and his quality as a teacher.

In connection with the first two of these it must be mentioned that his father late in life had been bequeathed by a brother a considerable estate, part of which Professor Van Vleck inherited. Hence he had means to live graciously, to collect books, etchings, and prints, and to travel extensively. With true Dutch characteristics he was able to combine the love of good living with meticulous care in money matters. He took joy both in giving generously and in investing wisely, but inexactitude, financial or otherwise, went against the grain.

Professor Van Vleck kept abreast of what was published in his field of mathematics, but in spite of this found time for much reading of literature. Often, however, he joked about doing his reading vicariously through Mrs. Van Vleck, who was a prodigious reader. In the graphic arts they shared consuming interest. The etchings of Rembrandt, Seymour Hayden, and Whistler adorned their walls, which however, were always the walls of a home—not those of a museum. Their collection of Japanese prints was notable, and Mrs. Van Vleck became expert in repairing these. Friends from all over America remember with pleasure the occasions when for an hour or so the Van Vlecks would show to small groups some selected prints from their collection.

Travel played a very large role in the life of the Van Vleck family. The guide book and the atlas were ever at hand. (A timetable was not needed in the presence of their son.) The galleries, the churches, and the mountains of Europe were equally familiar. It was perfectly natural for a conversation to turn from point sets to the comparative beauty of the north and south spires of Chartres cathedral. Professor Van Vleck's retirement at sixty-six was associated with both his love of art and of travel for, as he explained to his friends, he wished to retire while he could still enjoy a trip around the world and return to catalogue his Japanese prints. For each of these programs he set aside a year. He apologized for the fact that, because on his return he missed a connection in Chicago, the trip had taken a year and six hours instead of a year. However he had acquired so many prints during the journey that the cataloguing of this collection was prolonged well past the allotted time.

As a teacher Professor Van Vleck had both natural assets and liabilities. He had the gift of exact expression and of clear organization. However, it was difficult for him to understand a slow mind or to pace himself in accordance with the requirements of an average class. In quizzing a small group or an individual he was superb—discovering any lack of apprehension and clarifying difficult points. He was courteous, yet impatient—one of the few dichotomies of a remarkably integrated personality and related to the conflict between his great tolerance of spirit and his own almost puritanical standards of conduct. He was generous in the extreme with his time, but demanded that he see some results for his effort. He was a stimulating teacher and colleague of the gifted; others surpassed him in getting moderately satisfactory results from the average. As chairman of the Department of Mathematics of the University of Wisconsin, he constantly upheld the highest scholarly ideals.

The qualities of insight, exactitude, and consideration when there was a spark worth fanning made his work as editor of the *Transactions of the American Mathematical Society* in its formative years of great and beneficial influence. A mathematical result was not something to be transmitted haphazardly to the public. It should be a part of a great cultural structure and, as such, it should be expressed with precision and elegance. Many young authors gained much from his kindly but incisive suggestions. Moreover, such standards have been transmitted from scholar to scholar, to become traditional for the *Transactions*.

Not only did Professor Van Vleck believe strongly in the unity of mathematics, but he also believed in the unity of the scholars who dealt with that subject; and at the time when it seemed likely that they would divide themselves into regional groups, he was a potent force in keeping the American Mathematical Society a truly national organization.

Professor Van Vleck was interested both in the affairs of the University and in those of the community—an interest that was shown through generous giving and through active participation in committees, boards, etc.

There are many who, in their ideal of the scholar and what the life of the scholar should be, have acquired much from Professor Van Vleck and his family.

As a mathematician Van Vleck won his spurs with the completion of his doctoral dissertation in 1893. He had spent five semesters at Göttingen, where he had found his primary inspiration in Felix Klein. His thesis subject, "The Development of Hyperelliptic Integrals in Continued Fractions," was in the focal center of interest of the day. The hyperelliptic integrals are of the form

$$\int \frac{W(x)dx}{(x-a_1)^{1-\lambda_1}\ldots(x-a_n)^{1-\lambda_n}}$$

$W(x)$ being a polynomial of the degree $(n-2)$ and the $a_1, \ldots, a_n$, $\lambda_1, \ldots, \lambda_n$, being real or complex constants. Work in this field had been initiated by Gauss in connection with the hypergeometric functions, in particular in connection with the function

$\frac{1}{2}\log\frac{x-1}{x+1}$, which is represented by the integral

$$\int \frac{dx}{(x-1)(x+1)}$$

It had been carried forward by others in connection with studies of the polynomials of Lamé and Stieltjes. Such polynomials appear as solutions of linear differential equations of the form

$$\frac{d^2y}{dx^2}+\left(\frac{1-\lambda_1}{x-a_1}+\cdots+\frac{1-\lambda_n}{x-a_n}\right)\frac{dy}{dx}+\frac{W(x)}{(x-a_1)\cdots(x-a_n)}y=0$$

By an extensive and searching analysis Van Vleck greatly broadened and generalized the existing theory, and threw light upon it from several new angles. His approach was both analytic and geometric. From the analytic standpoint, the convergents of the continued fraction developments yield algebraic approximations to the integral. Van Vleck concerned himself with such approximations, both such as were valid in the neighborhood of a single branch point, and such as were simultaneously valid in the neighborhoods of several branch points. His geometric discussion, which was extensive, was based upon the theory of conformal mapping. The irregularities of the algebraic approximants and the distribution of the roots of the polynomial factors that figure in the integral representations of the remainder terms were investigated. The upshot was an extensive coordination and classification of the integrals, and revelations of some deeper lying connections of their theory with the theories of linear differential equations, of groups, of polynomials, etc.

With this important memoir Van Vleck had opened for himself a number of avenues along which investigations were to occupy him for the ensuing decade. The fruits of these researches were a succession of papers, on the roots of Bessel functions and Riemann P-functions, on the classification along group theoretic lines of differential equations that admit two solutions whose product is a polynomial, on criteria for the radii of convergence of power series, on the roots of hypergeometric series, and, most especially and extensively, on the theory of the convergence of continued fractions. Well-known theorems in this last field are his. His extended preoccupation with this field of analysis well qualified him for the role of "Colloquium lecturer" of the American Mathematical Society. Delivered in 1903, his lectures were on the subject of "Divergent Series and Continued Fractions."

In 1907 and 1908 Van Vleck published papers on point-set theory, his primary concern being the analysis of non-measurable sets. That his appreciation of this field of analysis was not transient is evidenced by the fact that he chose in 1915, as retiring president of the American Mathematical Society, to direct his address to the subject of "The Role of the Point-Set Theory in Geometry and Dynamics."

Between 1910 and 1916 Van Vleck's research was concerned with the functional equations of the sine and the theta functions, and with linear difference equations. Although he wrote only one paper on the latter subject, he also treated it in a lecture course at the University of Wisconsin, in a manner that was described by George D. Birkhoff, one of his auditors, as "suggestive and stimulating." Birkhoff and his students subsequently achieved notable advances in this field. It is therefore appropriate to observe Birkhoff's remarks, that "one must look upon Van Vleck as an essential factor in American contributions to linear homogeneous difference equations."

The properties and classifications of groups of linear substitutions in any number of variables were treated by Van Vleck in various papers at different times. Another subject of recurring interest to him was the location of the roots of polynomials. On that he wrote in 1899 and 1903, and again in 1925. He made it the subject of his "Symposium lectures" before the American Mathematical Society in 1929.

Van Vleck was a well-informed and discerning mathematician, and a clear and fluent writer. Some essays in which he reviewed various mathematical developments therefore deserve mention, since they were widely read and appreciated. Among these were his address on the role of point-set theory, which has already been mentioned above, his address on "The Influence of Fourier's Series upon the Development of Mathematics," delivered in 1913 on the occasion of his retirement from a vice presidency of the American Association for the Advancement of Science, and his address "Current Tendencies of Mathematical Research," delivered on the occasion of his investiture with the honorary Doctorate of Science by the University of Chicago in 1916.

His honors were numerous: the degrees of Doctor of Mathematics and Physics from Groningen, Doctor of Science from the University of Chicago, and Doctor of Laws from Clark University and Wesleyan University. He was made "Officier de l'instruction publique" by the French Republic; and, in addition to serving as editor of the *Transactions of the American Mathematical Society,* he was President of the Society, 1913-1915. He was elected to the National Academy of Sciences in 1911.

Dr. Van Vleck died in Madison, Wisconsin, on June 2, 1943, at the age of 80.

KEY TO ABBREVIATIONS

Am. J. Math. = American Journal of Mathematics
Ann. Math. = Annals of Mathematics
Bull. Am. Math. Soc. = Bulletin of the American Mathematical Society
Trans. Am. Math. Soc. = Transactions of the American Mathematical Society

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The Mathematical Work of R. L. Moore: Its Background, Nature and Influence

R. L. WILDER

Communicated by C. TRUESDELL

ROBERT LEE MOORE ("R. L.") was probably one of the most influential American mathematicians of the first half of the 20^{th} century. Whether this was due more to his famous teaching method (the "MOORE Method") or to his creative work in mathematics is debatable; the current folklore seems to credit the former. On the other hand, careful scanning of his published work reveals that while, from a present point of view, it was narrowly oriented in scope, being confined to what he called "Point Set Theory," it contained the germs of a large portion of modern research in both general and algebraic topology.

MOORE did not himself venture into algebraic topology at all. Possessed by dogmatic prejudices, he eschewed algebraic methods, and while a preacher of the necessity of axiomatic foundations, he apparently based his personal ideas and beliefs about mathematics on some kind of absolute intuition whose decrees, once revealed, were not to be tampered with. To him, the Axiom of Choice was a matter of *truth*, not convenience, and to question it in his presence stirred him to anger.

But it is not my purpose to discuss here either his teaching methods or his general philosophy, except insofar as they influenced his mathematical work. My principal concern will be with the published materials outlined in the Bibliography which is appended hereto, and some of the circumstances surrounding and effecting it as revealed by his correspondence and other papers now available in the Archives of American Mathematics at the University of Texas. I shall be concerned both with the origin and evolution of MOORE's ideas as well as with their influence on events in the history of modern mathematics.

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Part I. The Background

When Moore, a native Texan, matriculated at the University of Texas in 1898, he encountered one of the most forceful personalities on the campus (as well as in Americal mathematics), namely George Bruce Halsted. Geometry was finally being put on a satisfactory basis, and Halsted's greatest interest at the time was in geometry, particularly in Hilbert's recently published *Grundlagen der Geometrie* (1899). The outstanding characteristic of this work was its attempt to found the geometry of the plane and three-space on a rigorous axiomatic basis. Not only did Halsted apparently acquaint R. L. Moore with Hilbert's work, but Moore was induced to check one of the axioms (Axiom II 4) for independence. Moore's first piece of research embodied finding that this axiom was actually not independent. Halsted communicated the result to E. H. Moore, head of the mathematical group at Chicago—only to find, however, that E. H. had a few months before established the same result. Nevertheless, it turned out that R. L.'s proof was shorter and more elegant than E. H.'s, and the latter, in a note in the American Mathematical Monthly (vol. 9, 1902, pp. 152–153) termed it "delightfully simple." Halsted wrote up R. L.'s proof in the form of a short note in the same journal.*

After earning his B.A. and M.A. degrees at Texas (both in 1901), and spending the subsequent two years first as teaching fellow at Texas and then as high school teacher in Marshall, Texas, R. L. spent the years 1903–1905 as a graduate student at the University of Chicago. Here he found an atmosphere of research that formed a natural continuation of that which Halsted created. As already mentioned, E. H. Moore was the head of the mathematics department at Chicago, and he had become thoroughly imbued with the ideas of the German school of mathematics, and particularly with the exploitation of the axiomatic method. He had spent a year of study in Göttingen and Berlin after receiving his doctorate at Yale in 1885, and according to two of his biographers (G. A. Bliss & L. E. Dickson), "It seems that the work of Kronecker made the most lasting influence upon him, but in his habits of thought and his later work there are many indications of influences which might be traced to Weierstrass and Klein."** Also at Chicago was Maschke, who is termed by the same authors (*loc. cit*) "one of the most delightful lecturers on geometry of all time." Inevitably these geometric interests helped to foster R. L.'s already formed interest in geometry.

At the time when R. L. arrived in Chicago, O. Veblen had just finished his doctoral work under E. H. Moore and had been appointed an Associate.† In the latter capacity, he seems to have been enlisted by E. H. to assist in

*G. B. Halsted, *The betweenness assumption*, Amer. Math. Mo., vol. **9** (1902), pp. 98–101.
G. A. Bliss & L. E. Dickson, Nat'l Acad. of Sci. Memoirs, vol. **17, 1936, pp. 83–102.
†*Cf.* S. MacLane, Nat'l Acad. of Sci. Memoirs, vol. **37**, 1964.

the supervision of R. L.'s thesis work. The association between VEBLEN and R. L. became quite close at this time, and R. L.'s thesis was, like VEBLEN's, devoted to the axiomatic foundations of geometry. Much of R. L.'s early work was to be closely related to VEBLEN's, *e.g.*, papers 1–3, 5.

As one scans his published papers, one is struck by MOORE's predilection for the axiomatic method. All of his first 10 papers, with the exception of a discussion of DUHAMEL's Theorem (paper 4) were concerned with some kind of axiomatic procedure. Aside from incidental use of the method (as in proving his classical theorem on upper semi-continuous collections of continua, paper 38), 15 of his 66 papers wer based on axioms; the same holds for his major work, his book entitled "Foundations of Point Set Theory." It was mainly in this respect that R. L., throughout his life, showed his Chicago background of the early 1900's. VEBLEN, who started his career at Princeton in 1905, continued his investigations in geometry throughout his life.

The first work that R. L. carried out, when he left Chicago and went to the University of Tennessee for a year, was concerned with axioms for the positive integers and their arithmetic. In this work, never published, he attempted to found the theory entirely on the undefined term *integer* and operations $\oplus$ and $\otimes$, thus skirting the notion of order entirely. Unfortunately one of the axioms was inordinately long and complicated (although quite easily grasped if one had at hand the tools of modern mathematical logic), and extant correspondence between MOORE and VEBLEN leads one to infer that the work was rejected by the *Annals of Mathematics*. In a letter dated April 9, 1906,* VEBLEN, who was now at Princeton and to whom MOORE and evidently sent his manuscript, wrote R. L. as follows:

"Your 'lists' of axioms came back from Huntington** the other day. I doubt if he understands A_6, [the axiom referred to above]. In consequence he was more impressed by the difficulty than by the value of your work.... You ought to write a preamble about your logical aims, condense the proofs as much as possible.... The avoidance of *ordinal counting* and order in any form, the replacing of 'class' by 'statement,' and some account of 'logic of propositions' which you presuppose ought to go into the preface. Have you tried to write postulates of logic?"

Another statement in the same letter from VEBLEN indicates that R. L. was also working at this time on some problems concerning curves: "Why not send me your curve business in its final form? If I can I will try to look up the literature better than you can in your town." What this refers to I have not been able to ascertain. Incidentally there follows a remark that will

*In the R. L. MOORE Collection at the Archives of American Mathematics at the University of Texas; quoted here by permission. For help in locating and obtaining materials from this collection, I am indebted to Professor LUCILLE WHYBURN and Dr. ALBERT C. LEWIS.

**E. V. HUNTINGTON was at that time one of the editors of the *Annals of Mathematics*. R. L. gave two alternative lists of axioms in his paper which he entitled "List 1" and "List 2."

strike a chord in everyone who has ever directed dissertations: "No doubt you are getting your geometry work into final form as quickly as possible? I am anxious to see that work come out as soon as possible." (The first two papers of MOORE, on geometry, were published within the next two years.)

R. L. also sent the "lists" of axioms for the positive integers to E. H. MOORE. A letter from the latter to R. L. indicates that E. H. pleaded the press of other duties and did not render any judgment on the work. However, like VEBLEN, he suggested to MOORE the possibility of delving into logic, specifically advising him to read the articles that had been appearing in the *Mathematische Annalen*. The articles referred to were chiefly concerned with the foundations of the theory of sets, especially with the Axiom of Choice and the Continuum Hypothesis.

Can it be that we have here and in VEBLEN'S urgings, the origin of R. L.'s dislike for such investigations?* As any of his doctoral students can testify, he was a platonist in regard to the Axiom of Choice, regarding it as an absolute principle and not a matter for research regarding its consistency or admissibility. In his book, he did not indicate which theorems were dependent upon the Axiom, but stated it as a general principle in his Preface, to be used wherever needed in the text; this was quite contrary to his custom of giving clear indication in the book regarding which of his set theory axioms each theorem depended upon. This was also evidently one of the matters upon which he disagreed with VEBLEN. The latter, in his presidential address before the American Mathematical Society in 1924 stated, "The conclusion seems inescapable that formal logic has to be taken over by mathematicians. The fact is that there does not exist an adequate logic at the present time, and unless the mathematicians create one, no one else is likely to do so." (*Cf.* MACLANE, *loc. cit.*) It is interesting to note that exactly three years later, Alonzo Church, who may be considered the dean of mathematical logic in this country, received his Ph.D. under VEBLEN'S direction with a dissertation concerning a set theory in which the Axiom of Choice is false.

Another colleague of R. L.'s younger days, who is not perhaps ordinarily thought of as forming one of the early influences on MOORE, was N. J. LENNES. He has probably been principally known as the coauthor, with VEBLEN, of *Introduction to Infinitesimal Analysis, Functions of One Variable*, published in 1907, a work popular among students of function theory and analysis in this country for many years.** Although LENNES was eight years older than R. L., and had received his M.S. degree at Chicago in 1903, he had taken time out to do high school teaching and did not receive his Ph.D. (Chicago) until 1907. He apparently kept in close touch with the Chicago

*See the comments below in Part IIIa concerning MOORE'S feelings about such matters.

**See, for instance, R. C. ARCHIBALD, "A Semicentennial History of the American Mathematical Society, 1888–1938." N.Y., Amer. Math. Soc., 1938, p. 208. At the time when he wrote this book with VEBLEN, LENNES was a high school teacher in Chicago!

mathematical group during the time when R. L. was there, and he and R. L. corresponded with one another after R. L. left Chicago; LENNES was also one of those to whom R. L. set his "Lists" concerning the positive integers. But more important, LENNES was the creator of the topological definitions of *connected, arc* and *simple closed* (JORDAN) *curve* which played such an important part in MOORE's work. As will be noted later, LENNES' 1911 paper was drawn upon by MOORE in one of the latter's most important works.

Aside from these personal influences on R. L., there was of course the literary part of the mathematical environment, which at that time consisted almost predominantly of current papers of HILBERT, VEBLEN, LENNES, FRÉCHET and others, as well as the books of W. H. YOUNG and G. C. YOUNG (1906). A. Schoenflies (1908) and F. HAUSDORFF (1914) which inevitably helped to set the pattern that guided R. L. in his choice of work. Such influences can be more meaningfully brought out in our discussion of MOORE's own papers, to which we now turn.

PART II. THE MATHEMATICAL WORK

Reference will be made to MOORE's papers according to their numbers in the Bibliography; these numbers correspond to those which were assigned to them in my obituray of MOORE.* What follows directly below will essentially be an expansion of the discussion in the obituary, and I will therefore employ the same classification by subject that was used therein.

IIa. Geometry

Only six of MOORE's papers were devoted to what would today be called geomtry (*cf.* the remark in Part I contrasting VEBLEN's geometric work with R. L.'s). MOORE's interests seem to have undergone a gradual change during the period between his departure from Chicago in 1905 and the year 1915. This period turned out to be an almost sterile interval in R. L.'s life, probably partly induced by his apparent lack of success in finding, during this time, an environment that he could consider satisfactory and permanent.** Between 1908 and 1912, he published nothing, and likewise between 1912 and 1915 (he did publish papers on the specific dates mentioned, however); before

*R. L. WILDER, *Robert Lee Moore*, 1881–1974, Bull. Amer. Math. Soc., vol. **82** (1976), pp. 417–427.

**After his year at the University of Tennessee, R. L. held positions at the following institutions: Princeton University, 1906–1908; Northwestern University, 1908–1911; University of Pennsylvania, 1911–1920; University of Texas, 1920–. At Texas, he taught a full-time schedule until 1969, although nominally on half-time after age 70. For details, see the obituary, *loc. cit.* It can also be surmised that during this period, R. L. was coming to the conclusion that axiomatic foundations of geometry, to which he had devoted so much time, was not as fruitful a field of investigation as he would like.

1915, he published only four papers, one of which was his dissertation—not a very promising start for one whose later production belied these early portents. Both E. H. MOORE and O. VEBLEN urged him, in the letters cited from them in Part I, to get his dissertation into publishable form; he was obviously busy with the lists of axioms for the positive integers at that time, however.

By the year 1915, his thoughts had become focused on problems, outside classical geometry, that would lead him into the areas that were destined to form his life's work.

MOORE's work in geometry was chiefly devoted to its axiomatic foundations. His first two papers (1,2) were presented to the American Mathematical Society on April 22, 1905, in combined form under the same title, *Sets of metrical hypotheses for geometry.* DEHN had shown† that HILBERT's original axiom sets I, II, and IV, augmented by the assertion, S, that the sum of the angles of a triangle is two right angles, are not sufficient to yield III (parallels). In paper 1, R. L. showed that any space satisfying I, II, IV and S must nevertheless be a subspace (via the addition of ideal points) of a space in which III holds. It is interesting to note that in the proof MOORE made use of his former mentor HALSTED's book "Rational Geometry."

In his thesis, paper 2, R. L. gives axioms for Euclidean geometry using as primitive notions *point, order* and *congruence.* As already mentioned above, it is closely related to VEBLEN's dissertation*, whose axioms I and III–X it utilizes: alternative sets of axioms are considered, some of them being systems in which ordinary ruler and compass constructions are possible, as well as a set for Bolyai-Lobachevskian geometry. In showing that every circle is a JORDAN curve, he had to use a definition thereof given by VEBLEN in 1905** in terms of order and continuity conditions, the *Lennes* definition not being available at the time he wrote out his proofs. He also considered independence of his axioms. Incidentally, one of the most popular of R. L.'s courses, which he frequently gave during summer sessions at the University of Texas, utilized one of his systems of axioms for geometry (along with his now famous method of teaching).

The paper 5, published in 1915, contained a result surprising for the time. In the paper published by VEBLEN in 1905, which has just been cited above, a proof of the Jordan Curve Theorem was given which purported to hold in a non-metrizable space V satisfying the Axioms I–VIII, X of his thesis. In

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†M. DEHN, *Die Legendre'schen Sätze über die Winkelsumme im Dreieck*, Math. Ann., vol. **53** (1900), pp. 404–439.

*O. VEBLEN, *A system of axioms for geometry*, Trans. Amer. Math. Soc., vol. **5** (1904), pp. 343–384.

O. VEBLEN, *Theory of plane curves in non-metrical analysis situs*, Trans. Amer. Math. Soc., vol. **6 (1905), pp. 83–98.

5, MOORE showed that the space V was actually metrizable, being topologically equivalent to the Euclidean coordinate plane. This paper gives some indication of the trend of MOORE's ideas toward the topological material and methods which were to occupy most of his time in after years.

In paper 17, MOORE applied his by then maturing familiarity with topological point set methods to give an axiomatic foundation of Euclidean and Bolyai-Lobachevskian plane geometry in terms of *point, region* and *motion* as primitives. HILBERT, in his fundamental paper *Grundzüge der Geometrie* of 1903,† had analyzed the transformation group, assuming the underlying space to be a number plane; MOORE's paper analysed the underlying space and the group simultaneously. The paper can be considered as a digression to his first mathematical love, but using the topological tools that had by now become part of his mathematical arsenal.

The review, 18, of the now classical VEBLEN-YOUNG work on projective geometry, seems to be the only review ever undertaken by MOORE. It is mainly devoted to a critical analysis of the foundations, especially as to whether one of the defined terms should really be treated as undefined.

IIb. Analysis

There is evidence that, while R. L. was coping with the problem of his major interests, he tried his hand as the field of Analysis. This was at a time when Analysis was passing from its "classical" stage to the modern form. Utilizing set-theoretic notions, such authors as BOREL and LEBESGUE had introduced newer and more general types of integration and more refined tools for attacking and analyzing problems in the theory of functions. During the period 1911–1912, MOORE presented papers to meetings of the American Mathematical Society under the titles "On the transformation of double integrals"* and "On sufficient conditions that an integral equation of the second kind shall have a continuous solution."** However, these were published only in abstract form. His published papers in Analysis can be considered as papers 4, 6, 7, 16, 25, 30, 34 and 42. The importance of the form of *Duhamel's* Theorem (of wider application than that due to OSGOOD), given in paper 4 was later emphasized by H. J. ETTLINGER, who also gave it generalizations and outlined its use in the study of summable functions.‡ Papers 6 and 7 present sets of axioms in terms of *point* and *limit* for the linear continuum with emphasis in paper 6 on the question of complete independence of the axioms in the sense of E. H. MOORE. However, the statement that the axioms are categorical with respect to point and limit is retracted in paper 7,

†Math. Ann., vol. **56** (1902–1903), pp. 381–422.

*See Bull. Amer. Math. Soc., vol. **17** (1910–1911), p. 513, abstract No. 10.

***Ibid.*, vol. **18** (1911–1912), pp. 217–218, abstract No. 5.

‡H. J. ETTLINGER, *R. L. Moore's principle and its converse*, Comptes Rendus des Séances de la Soc. des Sc. et de Lettres de Varsovie, XIX, 1927 Classe III, 455–460.

in which it is shown how to modify one of the axioms so that the statement concerning categoricalness becomes true.

In Paper 16, in which necessary and sufficient conditions are given that a certain type of FRÉCHET space be compact. MOORE demonstrates that by this time (1919) be has attained a maturity capable of dealing with the most abstract kind of mathematics. In paper 25, he corrects a proposition in the classic "Theory of Functions of a Real Variable" by E. W. HOBSON, and paper 30 is concerned with the relatively uniform convergence introduced by E. H. MOORE, with special reference to functions defined on a measurable set.

IIc. Point Set Theory

We use the term "Point Set Theory" to denote this section in difference to MOORE's own preference,[‡] although current usage would dictate the term "Set-theoretic Topology."

Even though paper 10 is, according to our classification, the first paper of R. L.'s that we place in this category, the ideas and methods used are a natural evolution of both his own and other's previously published set-theoretic ideas. Besides G. CANTOR and HILBERT (particularly the "Grundzüge cited above), there were, in addition to MOORE himself, such mathematicians as O. VEBLEN, N. J. LENNES,[*] SCHOENFLIES, FRÉCHET and F. HAUSDORFF involved in this evolution. MOORE's own paper 5, discussed above, formed with the works of those just cited, a background of which MOORE's later work is a natural extension.

Because of its later influence, especially on MOORE's teaching, we consider paper 10 more in detail. It formed a basis for both his renowned style of teaching and for his later research methods. Its general format was by now classic: Primitive terms (a class S of elements called *points* and a class of subclasses of points called *regions*), axioms, development of the theory (of plane topology) therefrom, and finally independence examples for the axioms. The axiom system used for the proofs was denoted by Σ_1; in a final section he

[‡]R. L. had very strong feelings regarding terminology. If he felt a term was the "right" one, he adhered to its use regardless of majority opinion. This principle led in at least one instance to a terminology somewhat paradoxical after dimension theory had come in; he retained, throughout his life, the term "continuous curve" for spaces which could hardly be called "curves".

[*]See especially abstracts of LENNES' papers in Bull. Amer. Math. Soc., vol. **12** (1905–1906), as well as his paper of 1911, *Curves in Non-Metrical Analysis Situs with an Application in the Calculus of Variations*, Amer. Jour. Math., vol. **33** (1911), pp. 287–326. Such notions are *arc, simple closed curve, connected, accessibility*, as well as relations of a simple closed curve to its complement in the plane, all are introduced in this paper and play a prominent part therein.

discussed modification of Σ_1 denoted by Σ_2 and Σ_3. Of particular interest, both historically and for future developments, was axiom 1 (of both Σ_1 and Σ_2):

There exists an infinite sequence of regions, $K_1, K_2, K_3, \ldots$, such that (1) *if m is an integer and P is a point, there exists an integer n, greater than m, such that K_n contains P*; (2) *if P' and P are distinct points of a region R, then there exists an integer δ such that if $n > \delta$ and K_n contains P, then the closure of K_n is a subset of $R - P'$.*

In a footnote, MOORE remarks that there is a "certain amount of resemblance between Axiom 1 and Veblen's Postulate of Uniformity" stated in the latter's paper of 1905. *Definition in terms of order alone in the linear continuum and in well-ordered sets,* cited above. The Postulate of Uniformity was stated for the linear continuum, and asserted the existence for each point P and integer n, of a segment (= open interval) R_{nP} such that the set $\{R_{nP}\}$ satisfies the conditions:

1) For fixed P, and all n, $R_{nP} \supset R_{n+1P}$;

2) for fixed P, $P = \cap_n R_{nP}$;

3) for every segment R, there exists an integer n_R such that for no P does $R_{n_R P} \supset R$.

Although VEBLEN's postulate may have influenced MOORE's formulation of his Axiom 1, it should be noted how much stronger are the implications of the latter. In VEBLEN's postulate, there is required a sequence of segments (regions) for *every* point P—hence a non-denumerable class for the entire continuum, whereas MOORE's axiom postulates only a denumerable class of regions for the whole space. (Compare MOORE's Axiom 1′ of the axiom system Σ_3, however.) In particular, MOORE's axiom 1 implies the separability of the space, whereas VEBLEN's does not; VEBLEN postulated the separability in a separate axiom. Moreover, as E. W. CHITTENDEN pointed out some eleven years later, "The importance of the regular and perfectly separable, therefore metric, spaces in the analysis of continua is indicated by the fact that nine years before the publication of the discoveries of Urysohn, R. L. Moore assumed these properties in the first of a system of axioms for the foundations of plane analysis situs. This axiom is furthermore of particular interest historically since it yields when slightly modified a necessary and sufficient condition that a topological space be metric and separable."* The modification referred to here consisted only in adding an "Axiom 0" to the effect that "for every region R, there exists an integer n such that $R_n \subset R$." In the same connection, CHITTENDEN noted that MOORE had inferred from the

* E. W. CHITTENDEN, *On the metrization problem and related problems in the theory of abstract sets*, Bull. Amer. Math. Soc., vol. **33** (1927), pp. 13–34.

now well-known theorem of TYCHONOFF** that his Axiom was a sufficient condition for metrizability.†

Attention should also be called to Theorem 4, §3, of LENNES' paper of 1911, referred to above, concerning the existence of sequences of sets of regions closing down uniformly upon closed and bounded sets of points. There is no evidence that I have found, that this had a direct influence on MOORE's thinking in setting up his Axiom 1, although MOORE was thoroughly familiar with LENNES' paper. (For example, in the paper under discussion, MOORE not only uses the definitions presented in LENNES' paper, but makes use of LENNES' theorems 2–9 of §4, which are provable on the basis of MOORE's axioms,‡ and of LENNES' argument for the analysis of MOORE's Theorem 48 (the SCHOENFLIES converse of the JORDAN Curve Theorem in terms of accessibility). From this and other evidence,≠ it is clear that LENNES and MOORE were taking similar approaches to the topology of the plane.

Two year before the publication of paper 10, F. HAUSDORFF had given in his book§ his so-called second countability axiom. It is doubtful that R. L. had even seen this book before submission of paper 10 to the publisher, so that we cannot consider HAUSDORFF's axiom as having contributed to MOORE's thinking in the formulation of Axiom 1.

We shall call attention shortly to the later evolution of Axiom 1 as exemplified in MOORE's book of 1932 (item 51 in the Bibliography). To return to the paper (10) itself, it may be said to represent a culmination of the trends, so far as plane analysis situs is concerned, to be found in the previous works of VEBLEN, LENNES and MOORE himself. A central core of these works was the topological characterization of the basic Euclidean elements, *viz.*, the simple arc, the simple closed curve (S^1), the plane and 2-sphere. In paper 10, the topological characterization of the plane was achieved; in paper 14, MOORE showed that every space that satisfies either of the systems Σ_1 or Σ_2 is topologically equivalent to the number plane. It is interesting that MOORE did not use, in his proof, the result of paper 5, and show that the space satisfied VEBLEN's Axioms I–VIII, XI, but proceeded independently.

Before leaving paper 10, it should be pointed out that the independence examples given for axioms 6 and 7 are not valid—the discovery of which led

The TYCHONOFF theorem states that a necessary and sufficient condition that a perfectly separable HAUSDORFF space be metrizable is that it be regular. See Math. Ann., vol. **95 (1926), p. 139.

†Of importance from an evolutionary standpoint, we note that we shall see later that a replacement of Axiom 1, leading to the notion of "Moore Space," played a part in the discovery, by R. H. BING, of a new and important metrization theorem.

‡See p. 139 of LENNES' paper.

≠For instance, a letter of LENNES to MOORE in the Humanities Research Collection of the University of Texas, dated May 18, 1912.

§F. HAUSDORFF, "Grundzüge der Mengenlehre", Leipzig, Verlag von Vert. u. Comp., 1914, p. 263.

to the elimination of axiom 6, which can be proved from the other axioms.* Also, a notable reduction of MOORE's system Σ_1 was accomplished by ZIPPIN in connection with his characterization of the 2-sphere (see Part III). MOORE was to return later to the axiomatization of the plane (2-sphere) in both his book and paper 53, in the latter of which the undefined terms were *piece* (which may be interpreted as bounded, connected open set) and a relation which he called *imbedded in.*

IId. Continuous Curves

These configurations, defined analytically by C. JORDAN** in 1893, were quickly proved (PEANO, HILBERT, E. H. MOORE, PÓLYA) to encompass not only most of the "thin" geometric entities that were ordinarily considered to be curves, but to comprise a whole host of higher dimensional spaces, including all the Euclidean n-spheres ($n = 1, 2, 3, \ldots$). They very early became a subject for topological investigation, especially by SCHOENFLIES.† The basic problem of giving them a characterization in topological, rather than analytic, terms, was solved independently by H. HAHN and S. MAZURKIEWICA circa 1913‡ using concepts of general topology and in particular that of *local connectedness* ("connectedness im kleinen").

MOORE's first venture in this area—the "arc theorem" for continuous curves, represented by paper 11, turned out to be a "multiple" in that it was independently proved by S. MAZURKIEWICZ and H. TEITZE. However, it was quickly followed by a series of investigations which are reported on in the expository paper 27, *Report on Continuous Curves from the Veiwpoint of Analysis Situs*, published in 1923. Subsequent to this report, MOORE devoted less attention to continuous curves (papers 26, 44 and 45 are exceptions), leaving the field to his students, especially G. T. WHYBURN. To the latter is due the notion of *cyclic element* of a continuous curve, which proved to be a most useful device for analyzing the structure of these curves.≢

A weaker property than local connectedness, "semi-local-connectedness," was formulated by G. T. Whyburn and shown to be capable of replacing local connectedness in a number of situations; it is discussed by him in his book "Analytic Topology." Later, F. B. JONES introduced the notion of *aposyndetic continua.* Although the two notions (semi-locally-connected; aposyndetic) differ at a point, they are equivalent as applied at all points of a continuum.

*R. L. WILDER, *Concerning R. L. Moore's axioms for plane analysis situs*, Bull. Amer. Math. Soc. Vol. **34** (1928), pp. 752–760.

**C. JORDAN, *Cours d'Analyse*, 2 ed., Paris, Gauthier-Villars, 1893, vol. 1.

†See A. SCHOENFLIES, *Die Entwickelung der Lehre von den Punktmanningfaltigkeiten*, II, Leipzig, Teubner, 1908.

‡For references see paper 27, p. 202, footnote†.

≢For a report on the work of WHYBURN and others on cyclic element theory, *see* B. L. MCALLISTER, *Cyclic elements in topology, a history*, Amer. Math. Mo., **73** (1966), pp. 337–350.

A great deal of research has been done on such continua; two reports* of JONES may be consulted for descriptions and citations of results.

These notions can be generalized, using methods of algebraic topology, so that they appear as 0-dimensional cases of certain n-dimensional "avoidability" properties. For details, see my book "Topology of Manifolds," pp. 333f.**

IIe. The Structure of Continua

In the early twenties, work on the topology of general spaces and especially in the theory of continua began to take on a wider geographical spread, notably to Poland, where Sierpinski and others founded a school of set-theoretic topology; the new journal *Fundamenta Mathematicae* was founded there in 1920. As could be expected, duplication of effort was inevitable. In the case of MOORE's papers 28 and 31, which extend a theorem of SIERPINSKI, paper 31 turned out to be a "multiple" with MAZURKIEWICZ; *cf.* footnote 8 of paper 37.† Paper 39 duplicated results of W. GROSS and FRÉCHET. Paper 41 was a contribution to the theory of indecomposable continua, a type of topological configuration which began to receive much attention during the decade of the 1920's.‡

Papers 54–56 are of particular interest in that they display a system of axioms whose list of primitive terms, in addition to *point* and *region*, contains the term *contiguous to*, denoting a relation between points. Presumably a major reason for introducing this notion was for its application to structural properties of a continuum in terms of specialized subsets; for example, if the cyclic elements of a continuous curve C are regarded as "points" and two such points p and q are called "contiguous" if and only if one of the pair p, q is a point (in the ordinary sense) of the other, then C becomes an acyclic continuous curve in terms of its "points". One may wonder why this material has not led to more subsequent research than it has, since the notion of contiguous points could prove fruitful as both a mathematical and physical notion.≠

In papers 57, 59 and 64, MOORE continued his researches in the structure of continua, making special use of concepts such as continua of condensation

*F. B. JONES, *Concerning aposyndetic and non-aposyndetic continua*, Bull. Amer. Math. Soc., vol. **58** (1952), pp. 137–151; and *Aposyndetic continua*, Coll. Math. Soc. Janos Bolyai, 8, Topics in Topology, Keszthely, Hungary, 1972.

R. L. WILDER, "Topology of Manifolds," Providence, R.I., Amer. Math. Soc. Coll. Pub., vol. **32, 1949, 1963.

†Theorem 3 of paper 37 turned out to be false, and was corrected by paper 48.

‡Regarding paper 41, see B. KNASTER & C. KURATOWSKI, *Remark on a theorem of R. L. Moore*, Proc. Nat. Acad. Sci., vol. **13** (1927), pp. 647–649.

≠*Cf.* T. HAILPERIN, *On contiguous point spaces*, Bull. Amer. Math. Soc., vol. **45** (1939), pp. 172–174; E. C. KLIPPLE, *Two-dimensional spaces in which there exist contiguous points*, Trans. Amer. Math. Soc., vol. **41** (1938), pp. 250–276; and K. S. BUTCHER, *A homology theory for multiply connected contiguous point spaces*, Univ. of Michigan Dissertation, 1946.

and upper semicontinuous collections of continua. Upper semicontinuous collections had been introduced in paper 38, where it was shown that if such a collection, G, of disjoint bounded continua fills up a plane E^2 and none of its elements separates E^2, then it is itself a plane in terms of the elements of G considered as "points" and with "limit point" suitably defined. A similar statement holds for the 2-sphere, S^2, and in paper 50, MOORE showed that if the elements of G are allowed to separate S^2, then the resulting configuration, C, in terms of the elements of G as "points," is a *cactoid* (= a continuous curve whose maximal cyclic elements are 2-spheres). In view of the definition of limit for the elements of an upper semicontinuous collection, these elements may be considered as the counter-images of points of C under a monotonic continuous mapping of S^2 onto C. In such terms the theorem was later generalized not only to 2-manifolds and higher dimensional configurations,* but the notion of monotone mapping proved very fruitful in later set-theoretic investiagtions.**

The notion of triod was introduced in papers 46 and 49. One of the most striking results was the impossibility of imbedding an uncountable number of disjoint triods in the plane.

Prime part decompositions, which had been introduced by H. HAHN, were exploited in papers 29 and 35.† It was shown, for instance, that in terms of its prime parts, every bounded continuum is a continuous curve (possibly degenerate). The prime part notion was further extended and generalized by G. T. WHYBURN and the present writer.

Although MOORE did not venture far into the structure of point sets having no compactness properties (an exception is paper 43), some of his students, particularly M. E. (ESTILL) RUDIN, made notable discoveries in the area of connected point sets lacking compactness restrictions, as did also P. M. SWINGLE.‡ Such investigations, it turns out, can be expected to lead into questions of the foundations of set theory.

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* *Cf.* J. H. ROBERTS & N. E. STEENROD, *Monotone transformations of two-dimensional manifolds*, Ann. of Math., vol. **39** (1938), pp. 851–862; R. L. WILDER, *Monotone mappings of manifolds*, Pacific Jour. Math., vol. **7** (1957), pp. 1519–1523; and *Monotone mappings of manifolds, II*, Mich. Math. Jour., vol. **9** (1958), pp. 19–23.

** See, for instance, the report by L. F. MCAULEY, *Some fundamental theorems and problems related to monotone mappings*, Proc. Conf. On Monotone Mappings and Open Mappings, ed. L. F. MCAULEY, Binghamton, State Univ. of N.Y., 1970. pp. 1–36, as well as papers cited in the bibliography thereof. Also, R. C. LACHER, *Cell-like Mappings and their Generalization*, Bull. Amer. Math. Soc., vol. **83** 91977), pp. 495–552.

† Paper 29 contains certain errors which were corrected in the footnote at the bottom of pp. 426–427 of paper 38.

‡ See, for example, M. E. RUDIN, *A property of indecomposable connected sets*, Proc. Amer. Math. Soc., vol. **8** (1957), pp. 1152–1157; and M. E. RUDIN, *A primitive dispersion set of the plane*, Duke Math. Jour., vol. **19** (1952), pp. 323–328.

IIf. Positional Papers

Paper 12 was evidently principally inspired by (1) A. SCHOENFLIES' classic work *Die Entwickelung der Lehre von den Punktmannigfaltigkeiten* already referred to in IId, in which, among other results concerning positional properties of plane continuous curves, conditions were given under which the common boundary of two plane domains will be a simple closed curve, and (2) CARATHEODORY's work on prime ends. Like CARATHEODORY's condition, given for a similar purpose, MOORE's condition of "uniform connectedness im kleinen" applied to one domain alone; otherwise it is much simpler than the CARATHEODORY condition, and in the higher dimensional properties "ulc_n" and "ULC_n" has led to extensive generalizations.* Paper 21 gives examples in three-dimensional space for which neither MOORE's theorem nor the theorem of SCHOENFLIES holds.

In earlier work of ZORETTI, F. RIESZ, SCHOENFLIES and DENJOY, it developed that every closed, bounded totally disconnected plane point set is a subset of an arc. In paper 15, written jointly with J. R. KLINE,** necessary and sufficient conditions were given in order that a plane closed point set should be a subset of an arc. This was later extended to n-dimensional space by E. W. MILLER (*On subsets of a continuous curve which lie on an arc of the continuous curve*, Amer. Jour. Math., vol. **54** (1932), pp. 397–416).

The concept of equicontinuous systems of curves was introduced in paper 20, and in paper 24 was used to characterize both closed 2-cells and open surfaces in three-dimensional space. "Property S," a property weaker than uniform local connectedness yet stronger than local connectedness, was introduced in paper 22; a modification of a notion that SIERPINSKI had used to characterize continuous curves, it was used here to characterize those simply connected plane domains which have continuous curve boundaries.† And with reference to bounded plane domains that are complementary to continuous curves, MOORE proved in paper 23 that their outer boundaries are simple

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*I have recently learned (see J. M. McGREW, *The origins of connectedness im kleinen*, Dissertation, Mich. State Univ., 1976) that A. DENJOY proved the sufficiency part of MOORE's theorem of paper 12 in *Sur l'analysis situs du plan*, Comptes Rendus, Paris Acad., vol. **153** (1911), pp. 423–426. Also, L. E. J. BROUWER proved a theorem narrowly related to the necessity part of MOORE's theorem in *Über Jordansche Mannigfaltigkeiten*, Math. Ann., vol. **71** (1911), pp. 320–327.

**This seems to be the only jointly authored paper in which MOORE was involved.

†Property S was later generalized by the present author to a class of higher dimensional medial properties; for a report thereon, see R. L. WILDER, *A certain class of topological properties*, Bull. Amer. Math. Soc., vol. **66** (1960), pp. 205–239.

closed curves; from this he was able to show that if two points are separated by a continuous curve, C, then they are separated by a simple closed curve of C.

Spirals were introduced (in the plane) in paper 68 and certain results established concerning sets of points on which a spiral may close down; *e.g.*, if M is a compact, totally disconnected point set and p is a point not in M, then there exists an arc from p which spirals down on every point of M but on no point that is not in M. Several of MOORE's later doctoral students found further results concerning this notion.

Others of his positional papers, as their titles indicate, treat plane separation and accessibility. Paper 44 establishes an interesting theorem to the effect that any two points in the complement of a plane continuous curve M can be joined be an arc that does not separate M.

IIg. The Book, "Foundations of Point Set Theory"

From 1919 to 1932, inclusive, MOORE published 38 papers—more than half his total of 67—and his book, the first edition of which appeared in 1932 (see item 51 of the Bibliography). Any doubts about his creativity that may have been harbored by his mathematical colleagues during his earlier years, were certainly by now completely obliterated. His most productive period was from 1919–1926, during which 8 year period he published 32 papers. The book may be considered a kind of culmination of his work in topology, although he published some 17 papers thereafter. However, from 1930 on, he seems to have thrown most of his energies into teaching and the production of doctoral students (of whom 43 were awarded their Ph.D.'s in 1930 and after). Much of his teaching and hence much of the work done by his students after 1930 was based on the ideas of his book. In particular, the new "Axiom 1" introduced in the book is of special importance in future work, as well as of interest in connection with the Axiom 1 of paper 10 (from now on we denote this paper by FPAS). The new Axiom 1 is stated as follows:*

Axiom 1. *There exists a sequence $G_1, G_2, G_3, \ldots$ such that* (1) *for each n, G_n is a collection covering S [the set of all points] such that each element of G_n is a region,* (2) *for each n, G_{n+1} is a subcollection of G_n,* (3) *if R is a region, X is a point of R and Y is a point of R, whether identical with X or not, then there exists a natural number m such that if g is any region belonging to the collection G_m and containing X then the closure of g is a subset of R and, unless Y is X, the closure of g does not contain Y,* (4), *if $M_1, M_2, M_3, \ldots$ is a sequence of closed point sets such that for each n, M_n contains M_{n+1} and, for*

*We use the 1962, revised edition.

each n there exists a region g_n of the collection G_n such that M_n is a subset of the closure of g_n, then there is at least one point common to all the point sets of the sequence $M_1, M_2, M_3, \ldots$.

This axiom may seem to the uninitiated to be rather formidable, but MOORE's method of teaching was such that it was introduced to his students quite naturally and became "second nature" to them. It undoubtedly evolved, in MOORE's thinking, from the "Axiom 1" of FPAS, and was created as a means of accomplishing most of the purposes of the original, while not implying that the space S is metrizable or separable (both of which were derivable from the original axiom of FPAS). Evidently his earlier discovery that VEBLEN's proof of the JORDAN Curve Theorem, which was designed for non-metric spaces, was actually based on axioms which implied the underlying space to be the number plane and hence metrizable (see the discussion of paper 5 above) had some influence on MOORE's desire for such a new axiom, and he hoped that the latter might be sufficiently broad to serve as a basis for plane topology without implying the metrizability.* As with the system FPAS, the new system of axioms incorporating the new Axiom 1, served for years as a basis for his advanced course in point set theory, and, like FPAS, contributed to the evolution and success of his renowned method of teaching.

PART III. INFLUENCES OF MOORE'S RESEARCH ON FUTURE MATHEMATICS

Here we distinguish between the influence of MOORE's *teaching* and MOORE's *research*. The former has long been recognized through both the reputation of the "MOORE method" or, as it is sometimes called, the "Texas method," and the large number (50) of the doctorates awarded under his supervision. The influence of MOORE's research is a more subtle and difficult matter since in most cases a significant result in mathematics usually has a complex and intricate background, no particular element of which can be assigned as *the* influence that motivated the result. The best one can do is to indicate significant areas of mathematics in which the influence of MOORE's research is clearly indicated, even though not necessarily the sole reservoir from which the basic ideas involved were derived. The situation is further complicated by the fact that MOORE was, after all, only one of a growing number of mathematicians in this country, Poland, Russia, Germany, *etc.*, who took part in the building of the foundations of topology. And although one can expect that the early work of MOORE's own students was motivated by him, or by ideas that they got from him, as they developed they naturally

*For this information, I am indebted to Professor F. B. JONES. According to him, although MOORE discovered the axiom in 1926 (see Bull. Amer. Math. Soc., vol. 33 (1927), p. 141), and used it in his Boulder Colloquium Lectures in 1929, he did not succeed in proving the existence of non-metrizable spaces satisfying the axiom until, after the lectures, he was enroute home by rail.

adopted new ideas and methods from other sources. This becomes accentuated as one passes on to the work of later "generations". Even many of those mathematicians loosely designated as belonging to the "MOORE School" prove, on closer examination, to have adopted new philosophies and new interests some of which would not have met with MOORE's approval. Inevitably we shall omit much work, especially of recent years, that could be traced back to work of Moore.

Much of the direct influence of MOORE's individual results on further research has already been commented upon in Part II, so that in this part we shall usually omit further reference to such items.

*IIIa. Moore Spaces.***

The term "MOORE Space" for a topological space that satisfies Axiom 0 (which states that every region is a point set) and parts (1), (2) and (3) of Axiom 1 quoted above from MOORE's book, seems first to have been used by F. B. JONES.[†] The term "complete MOORE space" for a MOORE space which satisfies in addition part (4) of the axiom was justified by the proof by J. H. ROBERTS,[*] that every metric space which satisfies Axioms 0 and 1 is complete in some one of its compatible metrics. As mentioned above, there exist, however, complete MOORE spaces that are not metrizable (a fact first discovered by MOORE himself).

In the fifty years since their introduction, around 300 papers relating to MOORE spaces have been published.[***] These have largely been stimulated by problems concerning metrizability, and particularly by the "JONES conjecture"[‡] that every normal MOORE space must be metrizable, as well as by the question of the actual position of MOORE spaces among the geneal abstract spaces. JONES proved (*loc. cit.*) that if $2^{\aleph_0} < 2^{\aleph_1}$ (the so-called LUSIN hypothesis) then every separable normal MOORE space is metrizable. And in 1951, R. H. BING, in connection with his important work on the metrization problem, showed that if a MOORE space is collectionwise normal, then it is metrizable.[††] It is a corollary that every paracompact MOORE space is metrizable.

**The writer is indebted to both Professor MARY ELLEN RUDIN and Professor F. BURTON JONES for advice and information in composing this section.

†See F. B. JONES, *Concerning normal and completely normal spaces*, Bull. Amer. Math. Soc., vol. **43** (1937), pp. 671–679.

*J. H. ROBERTS, *A property related to completeness*, Bull. Amer. Math. Soc., vol. **38** (1932), pp. 835–838.

***In 1968, a separate subject classification number was assigned to MOORE spaces by the abstracting journal *Mathematical Reviews*.

‡F. B. JONES, *loc. cit.*, p. 676, Here JONES raised the question, "Is every normal MOORE space metric?"

††R. H. BING, *Metrization of topological spaces*, Canadian Jour. Math., vol. **3** (1951), pp. 175–186. The theorem quoted is considered by many to be the most important theorem concerning MOORE spaces.

It may seem paradoxical, in view of MOORE's own feeling regarding logical foundations,[≠] that the "conjecture" has led into problems of mathematical logic. In particular, the independence of the normality of certain MOORE spaces from the usual axioms of set theory has been shown. Such involvement with set theory was, of course, already indicated by JONES' above cited result of 1937. Since then, research in MOORE spaces has proceeded along both topological and set-theoretic lines. Of the former character is a theorem proved by REED & ZENOR[§] to the effect that every locally connected, locally compact, normal MOORE space is metrizable.[§§] Regarding the latter see T. PRZYMUSINSKI & F. D. TALL, *The undecidability of the existence of a non-separable, normal Moore space satisfying the countable chain condition*, Fund. Math., vol. **85** (1974), pp. 291-297.

Recently it has been shown by W. G. FLEISSNER that, assuming the continuum hypothesis, one can construct a normal, non-metrizable MOORE space.[*]

Perhaps the most amazing sequence of results can be put together using theorems of FLEISSNER, P. NYIKOS, K. KUNEN and R. JENSEN: The existence of a strongly compact cardinal (a special kind of measurable cardinal) in a model for set theory implies the existence of an extension in which all normal MOORE spaces are metrizable. On the other hand, if there is a model in which all normal MOORE spaces are metrizable, there is an inner model in which there is a measurable cardinal.[**]

IIIb. Spheres and Manifolds

A subject that occupied much of MOORE's thinking was the characterization, in topological terms, of the plane and 2-sphere; his achievements in this

[≠] It has been well known among MOORE's students that he expressed a violent dislike of questions concerning logical foundations. The present writer recalls vividly one occasion on which he queried of one of MOORE's students whether he had used the Axiom of Choice in obtaining a certain result. MOORE, who was present, turned angrily and exclaimed, "I thought you'd ask that!" Curiously, he was urged in 1906 by both his colleagues E. H. MOORE and O. VEBLEN to study questions of logic. (*Cf.* Part I).

[§] G. M. REED & P. L. ZENOR, *A metrization theorem for normal Moore spaces*, Stud. Top. Proc. Conf. Charlotte, N.C., 1974, pp. 485-488.

[§§] It has been pointed out to me by F. B. JONES that P. S. ALEXANDROFF also discovered MOORE spaces, and that his uniform spaces are, in fact, metacompact MOORE spaces. See P. S. ALEXANDROFF, *Some results in the theory of topological spaces, obtained within the last twenty-five years*, Russian Math. Surveys, vol. **15** (1960), pp. 23-83. See also A. V. ARHANGELSKII, *On a class of spaces containing all metric and all locally bicompact spaces*, Soviet Math., vol. **4** (1963), pp. 1051-1055.

[*] W. G. FLEISSNER, *Normal non-metrizable Moore Space from Continuum Hypothesis or nonexistence of inner models with measurable cardinals*, to appear in Proc. Nat. Acad. Sci., USA.

[**] P. NYIKOS, *A provisional solution to the normal Moore space problem*, Proc. Amer. Math. Soc., vol. **78** (1980), pp. 424-435; W. G. FLEISSNER, *If all normal Moore spaces are metrizable, then there is an inner model with a measurable cardinal.*

regard have been discussed in Part II above. One of the notable and early characterizations of the 2-sphere was obtained by L. ZIPPIN, who showed that a continuous curve which contains at least one simple closed curve and is not disconnected by any arc of such a curve, but is separated by each of its simple closed curves must be a 2-sphere.† It may be noted that the JORDAN Curve Theorem constitutes one of the axioms (#4) in MOORE's book. Later the question was raised by J. R. KLINE as to whether a continuous curve which is separated by each of its simple closed curves, but not by any pair of distinct points in a 2-sphere. this proved a stubborn problem, but was eventually answered by R. H. BING in the affirmative.‡

Characterizations of 2-manifolds were obtained by I. GAWEHN (1927), J. H. ROBERTS (1932), E. R. VAN KAMPEN (1935) and G. S. YOUNG, Jr (1945). The VAN KAMPEN characterization utilized a localization of the ZIPPIN characterization of the 2-sphere, and the YOUNG characterization used the condition that each arc in a 2-manifold has two sides.≢

The difficulties encountered in achieving characterization of n-manifolds of dimension greater than 2 led to the introduction of "generalized manifolds," almost simultaneously and independently by E. ČECH, S. LEFSCHETZ and the present writer,* having the homology properties of the classical manifolds. For the classical dualities, this was shown by both ČECH and LEFSCHETZ; the present author went beyond this in his book** on manifolds, whose last five chapters are devoted to a verification that all the separation, accessibility, and other external and internal homology properties of the classical manifolds are possessed by the generalized manifolds. The generalized manifolds (now often called "homology manifolds") do not, of course, have

†L. ZIPPIN, *On continuous curves and the Jordan Curve Theorem*, Amer. Jour. Math., vol. **52** (1930), pp. 331–350.

‡R. H. BING, *The Kline sphere characterization problem*, Bull. Amer. Math. Soc., vol. **52** (1946), pp. 644–653. For other characterization of the 2-sphere, see the references given by BING in the introduction to this paper.

≢I. GAWEHN, *Über unberandete 2-dimensionale Mannigfaltigkeiten*, Math. Ann., vol. **98** (1927), pp. 321–354; J. H. ROBERTS, *A point set characterization of closed 2-dimensional manifolds*, Fund. Math., vol. **18** (1932), pp. 39–46; G. S. YOUNG, Jr., *Spaces in which every arc has two sides*, Ann. of Math., vol. **46** (1945), pp. 182–193.

*E. ČECH, *Théorie générale des variétés et de leurs théorèmes de dualité*, Ann. of Math., vol. **34** (1933), pp. 621–730; S. LEFSCHETZ, *On generalized manifolds*, Amer. Jour. Math., vol. **55** (1933), pp. 469–504; R. L. WILDER, *Generalized closed manifolds in n-space*, Ann. of Math., vol. **35** (1934), pp. 876–903. The general equivalence of all three types of generalized manifolds did not become evident until later; those of ČECH and LEFSCHETZ were constructed with the precise purpose of duplicating the homology properties of the classical manifolds; that of WILDER for the homological positional properties of the classical manifolds in n-space (see below).

**R. L. WILDER, "Topology of Manifolds," *loc. cit.*

the same homotopy properties as the classical manifolds. We forego later developments such as the applications in the theory of transformation groups.†

IIIc. Upper Semicontinuous Decompositions‡

One of MOORE's primary interests was upper semi-continuous decompositions. In Chapter V of his book (which devoted some 66 papers to their study), he modified Axiom 1 to a form which he called Axiom 1′ in order to derive the following theorem: *If G is an upper semi-continuous decomposition of the space, S, whose elements are compact (and closed) subsets of S, then G (given the quotient topology) also satisfies Axiom* 1′. WORRELL proved≢ that MOORE could just as well have used Axiom 1 itself, since if S satisfies Axiom 1 and G is an upper semi-continuous decomposition of a compact (closed) subset of S, then G satisfies Axiom 1 also.

Upper semicontinuous decompositions were also discussed by G. T. WHYBURN in his book "Analytic Topology", and they continue to be of interest, especially in connection with problems relating to homeomorphisms of spaces and certain decompositions thereof.§

Notable work on upper semicontinuous collections was done by R. D. ANDERSON, who in his dissertation* under MOORE showed that for every continuous curve M (of any dimension whatsoever,) there exists a 1-dimensional continuous curve K in E^3 and an upper semi-continuous collection G of disjoint continua filling up K, which with respect to its elements as points is homeomorphic with M. Later, the same author established theorems concerning dimension-raising monotone mappings,** and also studied continuous collections of continuous curves. Another of MOORE's students, E. DYER, investigated dimension-lowering mappings, and J. H. ROBERTS studied two-to-one transformations.***

We recall that in paper 38, MOORE showed that an upper semi-continuous decomposition of the plane, E^2, into bounded continua that do not separate

†See A. BOREL *et al.*, "Seminar on Transformation Groups," Princeton, University Pr., 1960. (Annals of Math. Studies. No. 48).

‡For valuable help in writing sections IIIc and IIId, the writer is indebted to Professor C. E. BURGESS.

≢J. M. WORRELL, Jr., *Upper semi-continuous decompositions of developable spaces*, Proc. Amer. Math. Soc., vol. **16** (1965), pp. 485–490.

§See, for example, W. T. EATON, *Applications of a mismatch theorem to decomposition spaces*, Fund. Math., vol. **89** (1975), pp. 199–224, and the citations therein.

*R. D. ANDERSON, *Concerning upper semi-continuous collections of continua*, Trans. Amer. Math. Soc., vol. **67** (1949), pp. 451–460.

See, for example, R. D. ANDERSON, *Monotone interior dimensionraising mappings*, Duke Math. Jour., vol. **19 (1952), pp. 359–366.

***See, for instance, E. DYER, *Certain transformations which lower dimension*, Annals of Math., vol. **63** (1956), pp. 15–19; and V. MARTIN & J. H. ROBERTS, *Two-to-one transformations on 2-manifolds*, Trans. Amer. Math. Soc., vol. **49** (1941), pp. 1–17.

E^2 yields the space E^2 again, and we have mentioned results of ROBERTS-STEENROD and WILDER constituting generalizations of MOORE's result to higher dimensions. In 1957, BING described a decomposition of E^3 into points and tame arcs such that the resulting decomposition space is not E^3 and the projection of the non-degenerate elements is zero-dimensional, and EATON, in 1973, described similar examples for all E^n, $n \geq 3$.[†] BING showed further that his example, which is not a manifold, is a Cartesian factor of E^4; that is, its Cartesian product with E^1 yields E^4.[‡] More recently, EATON & PIXLEY, and EDWARDS & MILLER[≠] have shown that any cell-like decomposition of E^3 is a Cartesian factor of E^4 provided the projection of the non-degenerate elements is closed and zero-dimensional.

In 1971, ARMENTROUT[§] extended the result of ROBERTS & STEENROD on cellular decompositions of 2-manifolds, to cellular decompositions of 3-manifolds that yield 3-manifolds. (Examples of BING and EATON, mentioned above, show that it is necessary to require, in dimensions higher than 2, that the decomposition space be a manifold.) SIEBENMANN then extended ARMENTROUT's result to n-manifolds, $n > 4$.[§§] These results on cellular decompositions of manifolds have involved the use of what has become known as "BING's shrinking criterion".[*] EATON developed a criterion called the "mismatch theorem" which has been very useful in determining whether certain types of decompositions of E^3 yield E^3, and CANNON & DAVERMAN have recently extended this concept to higher dimensions.[**]

Summaries of work on decompositions of manifolds, at various stages of its development, can be found in two papers by ARMENTROUT, and also in a paper by CANNON in which he develops properties of cell-like embedding

[†] R. H. BING, *A decomposition of E^3 into points and tame arcs such that the decomposition space is topologically different from E^3*, Ann. of Math., vol. **65** (1957), pp. 484–500; W. T. EATON, *A generalization of the dog bone space to E^n*, Proc. Amer. Math. Soc., vol. **39** (1973), pp. 379–387.

[‡] R. H. BING, *The cartesian product of a certain non-manifold and a line is E^4*. Ann. of Math., vol. **70** (1959), pp. 399–412.

[≠] W. T. EATON & CARL PIXLEY, *S^1 cross a UV decomposition of S^3 yields $S^1 \times S^3$*, Geometric Topology (Proc. Conf., Park City, Utah, 1974) Lecture Notes in Math., No. 438, Springer-Verlag, New York, 1975, pp. 166–194; R. D. EDWARDS & R. T. MILLER, *Cell-like closed 0-dimensional decompositions of R^3 are R^4 factors*, Trans. Amer. Math. Soc., vol. **215** (1976), pp. 191–203.

[§] S. ARMENTROUT, *Cellular decompositions of 3-manifolds that yield 3-manifolds*, Memoirs Amer. Math. Soc., No. 107 (1971).

[§§] L. C. SIEBENMANN, *Approximating cellular maps by homeomorphisms*, Topology, vol. **11** (1972), pp. 271–294.

[*] Originally formulated in R. H. BING, *A homoemorphism between the 3-sphere and the sum of two solid horned spheres*, Ann. of Math., vol. **56** (1952), pp. 354–362; and *A decomposition of E^3 into points and tame arcs such that the decomposition space is topologically different from E^3*, cited above.

[**] W. T. EATON, *Sums of solid spheres*, Mich. Math. Jour., vol. **19** (1972), pp. 193–207; J. W. CANNON & R. J. DAVERMAN, *Cell-like decompositions arising from mismatched sewings. Applications to 4-manifolds.* (Unpublished.)

relations.[†] In the latter paper, CANNON shows that problems on decompositions of manifolds are closely related to taming problems.

Decompositions of manifolds have played a significant role recently in the solution of the double suspension problem and the associated existence of noncombinatorial triangulations of manifolds. Bot CANNON and EDWARDS have shown that the double suspension of each homology n-sphere is a cellular decomposition of the $(n+2)$-sphere. CANNON[‡] established a shrinking criterion for decompositions of manifolds general enough to establish that the double suspension of each homology n-sphere is an $(n+2)$-sphere. More recently, EDWARDS[§] has given a more general condition characterizing the cellular decompositions of n-manifolds $(n \geq 5)$ that have the same n-manifold for decomposition spaces.[§§]

IIId. Positional Properties

MOORE evinced great interest in positional properties of the simple closed curve in the plane and, more generally, of continuous curves in the plane. As pointed out in Part IIe above, this interest seems to have stemmed from a number of earlier works, particularly of SCHOENFLIES and N. J. LENNES. To such properties as accessibility, used by these earlier writers, MOORE added such positional properties as uniform local connectedness and Property S; and along with such investigators as SCHOENFLIES ("Die Entwickelung...") and F. HAUSDORFF ("Grundzüge der Mengenlehre," 1914, p. 335), MOORE expressed interest (Paper 21; also paper 27, pp. 301–302) in the extension to Euclidean 3-space of the positional properties which he and his predecessors had already studied in the plane. Such extensions were made by the present writer, not only to 3-dimensional Euclidean space but, using generalized manifolds, to dimensions greater than 3 (see my book, "Topology of Manifolds," *loc. cit.*).[*]

[†]S. ARMENTROUT, *Monotone decompositions of* E^3, Topology Seminar (Wisconsin, 1965), Ann. of Math. Studies, No. 60, Princeton, University Pr., 1966, pp. 1–25; and *A survey of results on decompositions*, Proc. Univ. of Oklahoma Topology Conf., Norman, Okla., Dept. of Math., Univ. of Oklahoma, 1972, pp. 1–12, J. W. CANNON, *Taming cell-like embedding relations*, Geometric Topology (Proc. Conf., Park City, Utah, 1974), Lecture Notes in Math., No. 438, Springer-Verlag, N.Y., 1975, pp. 77–118.

[‡]J. W. CANNON, *Shrinking cell-like decompositions of manifolds, codimension three*, Annals of Math., vol. **110** (1979), pp. 83–112.

[§]R. D. EDWARDS, *The topology of manifolds and cell-like maps*, Proc. Int'l Cong. of Mathematicians, Helsinki, 1978, pp. 111–127.

[§§]See also the summary by J. W. CANNON, *The recognition problem: What is a topological manifold*? Bull. Amer. Math. Soc., vol. **84** (1978), pp. 832–866.

[*]It is another amusing sidelight that this work ran into another of MOORE's dislikes. Analogous to his aversion to logical studies of the consistency and independence of principles such as the Axiom of Choice, he opposed strongly the introduction of algebraic methods into point set theory. There is nothing novel about this type of attitude, of course; one may recall the intense

These extensions were characterized by the usse of homological methods. Work of another type, more geometric in nature, may be said to have been inspired by the example of a wild 2-sphere in 3-dimensional Euclidean space E^3, discovered in 1924 by J. W. ALEXANDER.** The SCHOENFLIES extension theorem had established that there are no wild simple closed curves in E^2. The discovery of "wildness" in E^3 led to a considerable amount of work in the 1950's and 1960's, in which R. H. BING and his students had a leading role, on conditions under which a 2-sphere S is tamely embedded in E^3; that is, such that the embedding of S is equivalent with that of the round sphere. Much of this work has been summarized by BURGESS & CANNON and by BURGESS.† Some of the methods were initiated with MOISE's work, about 1950, on the triangulation of 3-manifolds.‡ Important key results, in the middle and late 1950's, were proofs of DEHN's lemma, the sphere theorem, and the loop theorem by PAPAKYRIAKOPOLOUS,≠ the development of approximation theorems for 2-spheres in E^3 by BING,§ and a solution of a SCHOENFLIES problem for $(n-1)$-spheres in E_n by BROWN.§§ Some of the fundamental properties of manifolds of dimension 2 and 3 are developed in a recent book by MOISE.***

Much of the work that was done for dimension 3 in the 1950's and 1960's has been extended to higher dimensions in the late 1960's and the 1970's. KIRBY & SIEBENMANN**** proved trinagulation theorem for n-manifolds, $n > 4$. DAVERMAN has recently presented a summary of work on embeddings on $(n-1)$-spheres in E^n, $n > 4$.*****

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antagonism between advocates of "pure" geometric as opposed to analytic methods in geometry (resulting, for instance, in the famous geometer STEINER threatening to cease publishing in CRELLE's *Journal* if it continued to accept PLÜCKER's analytical papers).

J. W. ALEXANDER, *An example of a simply connected surface bounding a region which is not simply connected*, Proc. Nat. Acad. Sci., vol. **10 (1924), pp. 8–10.

†C. E. BURGESS & J. W. CANNON, *Embeddings of surfaces in E^3*, Rocky Mountain Jour. Math., vol. **1** (1971), pp. 259–344; C. E. BURGESS, *Embeddings of surfaces in Euclidean three-space*, Bull. Amer. Math. Soc., vol. **81** (1975), pp. 795–818.

‡E. E. MOISE, *Affine structures in 3-manifolds*, V. *The Triangulation theorem and Hauptvermutung*, Ann. of Math., vol. **56** (1952), pp. 96–114.

≠C. D. PAPAKYRIAKOPOULOS, *On Dehn's lemma and the asphericity of knots*, Ann. of Math., vol. **66** (1957), pp. 1–26.

§R. H. BING, *Approximating surfaces with polyhedral ones*, Ann. of Math., vol. **65** (1957), pp. 456–483; *Approximating surfaces from the side*, Ann. of Math., vol. **77** (1963), pp. 145–192.

§§M. BROWN, *A proof of the generalized Schoenflies theorem*, Bull. Amer. Math. Soc., vol. **66** (1960), pp. 74–76.

***E. E. MOISE, "Geometric Topology in Dimensions 2 and 3," Graduate texts in Mathematics, vol. **47** N. Y. Springer-Verlag, 1977.

****R. C. KIRBY & L. C. SIEBENMANN, *On the triangulation of manifolds and the Hauptvermutung*, Bull. Amer. Math. Soc., vol. **75** (1969), pp. 742–749.

*****R. J. DAVERMAN, *embeddings of $(n-1)$-spheres in n-space*, Bull. Amer. Math., vol. **84** (1978), pp. 377–405.

Some important key results are (1) proofs, developed independently by CERNAVSKII and DAVERMAN,[†] showing that an $(n-1)$-sphere is tame in E^n $(n>4)$ if its complement is 1-ULC and (2) recent joint work by ANCEL & CANNON showing that any $(n-1)$-sphere in $E_n (n>4)$ can be approximated with a tame sphere.[‡]

While many of the results in higher dimensions are similar to those in dimension 3, the methods in many cases are quite different. Proofs of many of the theorems on embeddings of 2-spheres in E^3 depended upon properties of E^2. On the other hand, much of the work for higher dimensions depended upon STALLINGS' engulfing theorem,[≠] and generalizations of it.[§] Thus, except for accessibility and separation properties and the SCHOENFLIES theorem, which are valid in all dimensions, work similar to what is mentioned above has not yet been done for 3-spheres in E^4.

CONCLUDING REMARKS

It is inevitable that the interpretations of historical events will reflect the prejudices of the historian. However, a good historian will try to avoid this, and will strive to be as impartial and factual as possible in the light of his own weaknesses. I am all to aware of the latter, and it is possible that my interpretation of MOORE's place in the history of topology is both inadequate and colored by my own acquaintance with the man. During my years of study and teaching at the University of Texas (1921–1924), I shared an office with Dr. MOORE for a whole year, and came to know his personality well. His was a forceful personality, but despite our areas of disagreement, we always retained a deep affection for one another as persons. When I left Texas in 1924, I felt I had a good idea of how MOORE would like to see the subjects, to which he had contributed, grow in the future.

In what I have written above, I have tried to emphasize the areas that MOORE liked best, although not neglecting at least to mention those in which methods that he disliked came into play. This will account for the greater detail that I have given to Parts IIIc and IIId, since I judge, possibly wrongly, that MOORE was rather intrigued by the outcome of these studies in the decomposition of continua and the positional properties in higher dimensions.

[†] A. V. CERNAVSKII, *Coincidence of local flatness and local simple-connectedness for embeddings of $(n-1)$-dimensional manifolds in n-dimensional manifolds when $n > 4$*, Mat. Sbornik, vol. **91** (133), (1973), 279–286 = Math. USSR Sbornik, vol. **20** (1973). 297–304; R. J. DAVERMAN, *Locally nice codimension one manifolds are locally flat*, Bull. Amer. Math. Soc., vol. **79** (1973), pp. 410–413.

[‡] F. D. ANCEL & J. W. CANNON, *The locally flat approximations of cell-like embedding relations*, Annals of Math., vol. **109** (1979), pp. 61–86.

[≠] J. R. STALLINGS, *The piece-wise linear structure of Euclidean space*, Proc. Cambridge Phil. Soc., vol. **58** (1962), pp. 481–488.

[§] R. H. BING, *Vertical general position*, Geometric Topology, (Proc. Conf., Park City, Utah, 1974), Lecture notes in Math., No. 438, N.Y., Springer-Verlag, 1975, pp. 16–41.

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Anna Johnson Pell Wheeler (1883–1966)

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Biography

Anna Johnson Pell Wheeler was the daughter of Swedish immigrants, Andrew Gustav and Amelia (Friberg) Johnson, who came to the United States in 1872 from the same Swedish parish—Lyrestad in Skaraborglän, Wästergotland. Settling originally at Union Creek in Dakota Territory, they lived in a dugout hollowed from the side of a small hill, and the father tried to eke out a living as a farmer. In 1882 he moved his ever-growing family to the nearby town of Calliope (now Hawarden), Iowa, where Wheeler was born on May 5, 1883, the youngest of three surviving children. Her sister Esther, to whom she was very close, was four years older, and her brother Elmer was two years older. Around 1891 the Johnsons moved to Akron, Iowa, where her father became a furniture dealer and undertaker.[1]

The earliest extant records indicate that Wheeler was sent to the Akron public school. Though there appears to have been no tradition of academic achievement in the family, in the fall of 1899 Wheeler enrolled at the University of South Dakota, where her sister had already been studying for a year. After one year as a "sub-freshman" making up entrance requirements, she fulfilled the degree requirements in three years. Her main interest—mathematics—was evinced early in her college career. One of her mathematics professors at South Dakota, Alexander Pell, recognized her talent for mathematics and actively coached her into a mathematical career.

Obtaining an A.B. degree from South Dakota in 1903, Wheeler won a scholarship to the University of Iowa. She completed a master's degree the

[1]Louise S. Grinstein and Paul J. Campbell, "Anna Johnson Pell Wheeler (1883–1966)," in *WOMEN OF MATHEMATICS A Biographic Sourcebook*, Louise S. Grinstein and Paul J. Campbell, eds. (Greenwood Press, Inc., Westport, CT, 1987), pp. 241–246.

following year, taking five mathematics courses and a philosophy course. Simultaneously, she taught a freshman mathematics course and wrote her master's thesis, "The extension of the Galois theory to linear differential equations." The quality of her work was high, and she was elected to the Iowa chapter of the scientific society Sigma Xi. Winning a scholarship to Radcliffe, she earned a second master's degree in 1905. She stayed at Radcliffe an additional year on scholarship, enrolling in courses with such noted mathematicians as Maxime Bôcher, Charles Bouton, and William Osgood.

In 1906 she applied for and won the Alice Freeman Palmer Fellowship offered by Wellesley College to a woman graduate of an American college. A stipulation of the fellowship was that she agree to remain unmarried throughout the fellowship year. Wheeler used the funds to finance a year's study at Göttingen University, then the worldwide center of intense mathematical activity. While at Göttingen, Wheeler attended lectures given by the mathematicians David Hilbert, Felix Klein, Hermann Minkowski, and Gustav Herglotz, and the astronomer Karl Schwarzschild. Of these professors, she was most influenced by Hilbert and his work.

Throughout Wheeler's years of graduate study at Iowa, Radcliffe, and Göttingen, her former teacher, Alexander Pell, kept in touch with her. He was very proud of her progress and achievements. His first wife having died in the interim, he and Wheeler finally decided to marry, despite her family's objections to the twenty-five-year age differential. In July 1907, when her fellowship expired, they were married in Göttingen. They then returned to South Dakota, where Pell had been promoted to the position of first dean of the College of Engineering. During the fall term of 1907–1908, the young wife taught two courses at South Dakota—theory of functions and differential equations. Still, she wanted the Ph.D.; and in the spring of 1908, she decided to return to Göttingen alone to complete her doctoral work.

By the late fall of 1908, Wheeler had almost completed the requirements. The final examination for the Ph.D. was imminent. Evidently, some conflict of unknown origin arose between her and Hilbert, and she returned to America in December 1908 with a thesis (written independently of Hilbert) but no degree. She rejoined her husband in Chicago, where he had moved after academic policy disagreements forced his resignation from the University of South Dakota. His new position involved teaching at the Armour Institute of Technology.

Undeterred by the turn of events in Göttingen, Wheeler enrolled immediately at the University of Chicago. After a year's residency, during which she studied under the mathematician E. H. Moore, the astronomer Forest Moulton, and the astronomer/mathematician William Macmillan, she received a Ph.D. magna cum laude. The thesis accepted by her advisor, Professor Moore, was the one she had written initially for the Göttingen degree.

After receiving the Ph.D., she sought a full-time teaching position. Unfortunately, the large midwestern universities were reluctant to hire women. In the fall of 1910, she taught part-time at the University of Chicago. When Pell suffered a paralytic stroke in the spring of 1911, she substituted for him at the Armour Institute of Technology, another institution that did not want to hire women on a full-time basis.

In the fall of 1911, a vacancy opened at Mount Holyoke College. She applied for it and was accepted. Hired initially as an instructor, she was promoted to associate professor in 1914. However, Wheeler's years at Mount Holyoke (1911–1918) were not easy ones. Teaching loads were heavy. She felt compelled at all costs to continue her research work, and she had to take care of her husband, who never fully recovered from his stroke.

In 1918 Wheeler decided to resign from her position at Mount Holyoke College and accept an associate professorship at Bryn Mawr College. She felt that Bryn Mawr offered great potential for her career advancement. The possibility of teaching advanced mathematics to graduate students intrigued her, and there was the prospect of being promoted to chairperson when Charlotte Angas Scott* retired. Professionally, her career at Bryn Mawr was successful. She became chairperson in 1924 and full professor in 1925. Except for brief periods, Wheeler remained at Bryn Mawr as chairperson and teacher until her own retirement in 1948.

Wheeler's personal life during the Bryn Mawr years was not a consistently happy one. She lost her father in 1920 and her husband several months later. There was a brief but happy second marriage, followed by the death of her second husband in 1932. In 1935 her mother died. Later that same year, Emmy Noether*, her colleague and new-found friend, also died suddenly. All of these events took their toll on Wheeler.

During Wheeler's second marriage, to Arthur Leslie Wheeler, a classics scholar, the coupled lived in Princeton. Wheeler gave up her administrative duties at Bryn Mawr but continued lecturing on a part-time basis. She had more time to devote to her own research and could participate in the stimulating mathematical environment at Princeton University. Summers the Wheelers spent in the Adirondacks at a place they built and called "Q.E.D.," a name appropriate in the light of both of their careers. Following her husband's death, Wheeler returned to live and work full-time at Bryn Mawr.

Retirement for Wheeler in 1948 did not mean withdrawal from all mathematical activity. Despite recurring severe bouts of arthritis, she kept abreast of new developments and attended mathematical meetings. She remained in contact with many of her students, taking great pride in their achievements.

*Cross-reference to other women discussed in the volume is given by an asterisk following the first mention in a chapter of the individual's name.

She traveled, spending most of her summers in the Adirondacks, where she enjoyed various outdoor activities.

Wheeler suffered a stroke early in 1966. Never recovering, she died a few months later, on March 26, at the age of eight-two. According to her wishes she was buried beside Alexander Pell, in the Lower Merion Baptist Church Cemetery at Bryn Mawr.

Wheeler was highly respected professionally during her lifetime. Of the 211 mathematicians ever starred in *American Men of Science*, only three were women. One of them was Wheeler. Such starring was an honor reserved for those considered prominent in their field of activity by their contemporaries. In 1926 she was elected to Phi Beta Kappa. She received honorary doctorates from the New Jersey College for Women (now Douglass College of Rutgers University) (1932) and Mount Holyoke College (1937). In 1940 she was singled out as one of the one hundred American women to be acclaimed by the Women's Centennial Congress as having succeeded in careers not open to women a century before.

Work

When Wheeler was studying at Göttingen, the most influential mathematician there was David Hilbert. In the early 1900s, Hilbert's work and interest evolved around integral equations, and he attached a great deal of importance to the subject. As a result, many mathematicians at Göttingen and throughout the world, among them Wheeler, were inspired to pursue further investigations in this area. Numerous papers were published. As the years passed, interest declined, and many of the results obtained passed into relative obscurity. An outgrowth of the work on integral equations was the development of a field in mathematics known as functional analysis, dealing with transformations, or operators, acting on functions.

Wheeler's research work spanned this period when the study of integral equations per se was at its peak of popularity and functional analysis was in its infancy. She regarded her work as being centered on "linear algebra of infinitely many variables." Her interest derived from possible applications of linear algebra to both differential and integral equations. Particularly noteworthy were her results on biorthogonal systems of functions. Some of the results she published were extended and generalized in the work of her own doctoral students at Bryn Mawr.

In 1927 Wheeler herself attempted to summarize her work and its overall importance in a series of invited lectures on the theory of quadratic forms in infinitely many variables. Unfortunately, these so-called Colloquium Lectures, presented during an American Mathematical Society meeting, were never published; but a detailed outline of the topics covered is found in an abstract written by T. H. Hildebrandt. In all the years that the Colloquium

Lectures have been given at American Mathematical Society meetings, only three lecturers have been women: Wheeler in 1927, Julia Robinson* in 1980, and Karen K. Uhlenbeck in 1985.

Wheeler drew accolades for her teaching throughout her career. Despite personal pressures and research commitments, she found time and even money to give to her students. Frequently she would invite graduate students to visit her summer home, where she provided them with encouragement and research time. Students felt free to talk to her about both personal and academic problems. Often she would take students to professional meetings at neighboring colleges and universities and urge them to participate actively.

As an administrator, Wheeler strove to enhance the national and worldwide reputation of the Bryn Mawr mathematics department. She tried to create an atmosphere in which students and faculty had ample opportunity for professional growth and development. When the Depression cut into available funds at the college, she nonetheless reduced teaching loads whenever possible so that faculty could find time for research.

Wheeler was instrumental in offering professional and political asylum at Bryn Mawr to the eminent German-Jewish algebraist Emmy Noether. A group of qualified Bryn Mawr students was assembled to take part in advanced algebraic seminars with Noether. Wheeler laid plans to involve Noether in an exchange of graduate mathematics courses with the Univeristy of Pennsylvania. Unfortunately, these plans never materialized because of Noether's unexpected death following surgery in 1935, less than two years after her arrival in America.

Wheeler did not confine her professional activities to her own research or to Bryn Mawr College. She was an active participant in such national professional organizations as the American Mathematical Society and the Mathematical Association of America. From 1927 to 1945 she served as an editor of the *Annals of Mathematics*. She worked on a College Entrance Examination Board committee which formulated basic guidelines for testing the mathematical potential of college-bound students (1933–1935). She was among those who petitioned for the establishment of the *Mathematical Reviews* in 1939, when the German abstract and review journal *Zentralblatt für Mathematik und ihre Grenzgebiete* became a victim of Nazi policy.

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Norbert Wiener: A Survey of a Fragment of His Life and Work

P. R. MASANI*

Contents

*I am grateful to Professor J. Benedetto of the University of Maryland for his helpful suggestions in reducing the size of a longer version of this paper.

Norbert Wiener
1894–1964
Photograph taken in the late 1920s.
(Photograph courtesy of the MIT Museum.)

1. Wiener the Man

Wiener used to lunch at the Faculty Club in the Sloan Building of MIT. Around noon he would walk from his office to the club and back again. During one such walk he encountered an old friend whom he had not seen for a long time. It was a balmy day. They chatted amiably, admired the trees, the Charles and its sailboats. At last, they said goodbye. But as the friend departed, Wiener, looking bewildered, stood still. "By the way," he asked, "which way was I headed when we met?" "Why, Norbert, you were headed towards your office," the friend replied. "Thanks," said Wiener, "that means I have finished lunch."

With the substitution "Walker Memorial" for "Sloan Building," and minus my embellishments (balmy weather, etc.), this story is true. The encounter occurred in 1929 with Ivan A. Getting, then a physics freshman and an organist, whom Wiener had met previously at a demonstration of a new electric organ.[1] Dr. Getting also tells me of a tennis practice in which, after failing to connect with any of nearly 100 serves from him, Wiener suggested that they might exchange racquets.

At a garden party at the Statistical Institute in Calcutta, we were standing near a table when someone (whom I had not met) approached to pick up some refreshments. Wiener introduced himself, and got the response, "I am Abraham Matthai." "Matthai," said Wiener, "that's the name Matthew in Malayalam." Dr. Matthai was a statistician. Believing that he had just met Wiener, I approached him later to tell him of Wiener's lectures. Matthai laughed: it had been his third encounter with Wiener, and the third time he had learned about "Matthew".[2]

Such absent-mindedness, quirkishness and idiosyncrasy, amusing and even endearing, were punctuated unfortunately by recurrent manifestations of petulance, emotional instability, and irrational insecurity and anxiety. This was the source of his uneven relationships with some colleagues. Three rather distinct descriptions of Wiener have been penned by colleagues. A good "first look" is portrayed by Hans Freudenthal:

> In appearance and behavior, Norbert Wiener was a baroque figure, short, rotund, and myopic, combining these and many qualities in extreme degree. His conversation was a curious mixture of pomposity and wantonness. He was a poor listener. His self-praise was

[1]During World War II, Dr. Getting was appointed Director of the Radar and Fire Control Division at the MIT Radiation Laboratory, and he enlisted the services of mathematicians such as Ralph Phillips, Witold Hurewicz and others. Several allied victories in the air war are attributed to his work on radar. He retired a few years ago as Vice-president of the Aerospace Corporation in Los Angeles.

[2]For more reminiscences of Wiener, both amusing and serious, see Dr. Brockway McMillan's recent article in this series {**M10**}. Numbers in braces refer to the list of (non-Wiener) references at the end of the paper.

> playful, convincing, and never offensive. He spoke many languages but was not easy to understand in any of them.[3] {**F2**, p. 344}

N. Levinson has described his experiences as a student in one of Wiener's postgraduate courses:

> As soon as I displayed a slight comprehension of what he was doing, he handed me the manuscript of Paley–Wiener for revision. I found a gap in a proof and proved a lemma to set it right. Wiener thereupon sat down at his typewriter, typed my lemma, affixed my name and sent it off to a journal.... He convinced me to change my course from electrical engineering to mathematics. He then went to visit my parents, unschooled immigrant working people living in a rundown ghetto community, to assure them about my future in mathematics. He came to see them a number of times during the next five years to reassure them until he finally found a permanent position for me. {**L5**, pp. 24–25}

This little story is more telling of Wiener, the man, than the earlier description. But Levinson hastens to add:

> If this picture of extreme kindness and generosity seems at odds with Wiener's behavior on other occasions, it is because Wiener was capable of childlike egocentric immaturity on the one hand and extreme idealism and generosity on the other. {**L5**, p. 25}

When from personality and character we turn to Wiener's mind, D. J. Struik's observations are germane:

> ... the first impression was that of an enormous scientific vitality, which the years did not seem to affect. The second was to a certain extent complementary, and that was of extreme sensitivity. Complementary indeed, since a man with heart and mind so close to nature and the technique of his time must have had very fine antennae; he sees, or believes he sees, he feels, or believes he feels, where others remain unresponsive. {**S4**, p. 35}

The historian of science, G. de Santillana, said of Wiener, "In his reactions he was a child, in his judgements a philosopher." Indeed, the transformation was striking. One recalls Leonard Bernstein's talks on television about that disorderly, unhappy and irritable individual called Beethoven, and of the mental metamorphosis that occurred when he picked up his musical pen. With Wiener too, all traces of immaturity and eccentricity vanished when he picked up his scholarly pen. This writer has had the good fortune to

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[3]In this otherwise apt description, the ambiguous term "wantonness" is totally inappropriate, and is perhaps indicative of inadequate acquaintance with the English language.

study all the 250-odd publications of Wiener. He can raise his right hand and say that in this corpus he has found only three, viz. [**47b**, **48d**, **49h**][4] in which Wiener-noise damps out the Wiener message. Wiener's Manuscript Collection (MC)[5] in the MIT Archives comprises 900 folders, among which is Wiener's correspondence with about a thousand individuals, ranging from an Attica prisoner to leaders of industry and labor, and some of the world's great minds.[6] Among the few letters this writer has scanned, he came across only one (to Dr. Frank Jewett, in September 1941, in which Wiener tenders his resignation from the National Academy of Sciences) that was intellectually confusing.

Wiener's life work, its enormous range notwithstanding, exhibits a coherence of thought from start to finish reminiscent of a great work of art. Unfortunately, because of space limitations, we shall be able to convey only a fragment of this piece of art. Nonetheless, writing this paper has been a pleasure.

2. The Nurturing Intellectual Environment

The climate at home was extraordinarily conducive. Wiener's father, Leo (1862–1939), a Tolstoian romantic and humanist who left Russia in his youth, was a genius, a scholar, a great linguist who spoke forty languages, and a Harvard professor. Leo had very definite ideas as to how children had to be trained so as to bring out their fullest potentialities. Norbert, being a precocious child, was subjected to a most vigorous and intense training primarily at home under Leo's direct tutelage, but he was also encouraged to read on his own, to have the run of libraries and museums and explore the countryside.

Unfortunately, certain prejudices and tempermental weaknesses of the parents affected this training with rather devastating effects on Wiener's emotional life. Apart from unnecessary harshness in training, he was led to believe that he was a gentile. The sudden revelation at age fifteen (1911) that this was a lie was shattering:

> The wounds inflicted by the truth are likely to be clean cuts which heal easily, but the bludgeoned woulds of a lie draw and fester. [**53h**, p. 147]

The "black year of my life" was his description of 1911.

[4]The numbers in square brackets refer to the Wiener papers cited at the end of the paper. Other references are in braces. For the papers [**47b**, **48d**], and [**49h**] a book review he wrote for *The New York Times*, see Wiener's *Coll. Works*, IV, pp. 748–750, 764–766 and 996–1000, cf. {**M5**}.

[5]Numerals prefixed by MC are to this Collection.

[6]It is good to report that this correspondence, now on microfilm, is being studied by Dr. Albert C. Lewis of The Bertrand Russell Editorial Project, McMaster University, Ontario, Canada, and may see the light of day within a few years.

Even so, the overall benefits were enormous. By the time he had finished school at age eleven and college at age fifteen, he had done a colossal amount of reading, had exposed himself to some of the world's greatest minds, and had formed a more or less coherent attitude towards the external world. He had also come to love American democracy, especially as it is practiced in the New England small towns, and was deeply patriotic, as his futile attempts to enlist in World War I testify. Furthermore, Wiener acquired a strong sense of duty that he retained throughout his life, cf. e.g. [**60e**]. The year 1911 notwithstanding, he received a Ph.D. from Harvard in philosophy in 1913 at the age of eighteen, and was awarded a John Thorton Kirkland Traveling Fellowship by Harvard. Without doubt Leo Wiener was Norbert Wiener's first great mentor.

The second very favorable factor in Wiener's environment was the extremely healthy intellectual climate that prevailed in world science during his postdoctoral and later years, roughly between 1914 and 1933. This had much to do with the publication of the *Principia Mathematica* (PM) by Bertrand Russell and Alfred North Whitehead during the years 1910 and 1913 {**W3**}. Let us see how this affected Wiener.

The German logician G. Frege's attempts in 1893 to reduce arithmetic to logic had failed: his system allowed the antinomy concerning the Russell class $R = \{X\colon X \notin X\}$. In 1910 Whitehead and Russell succeeded in attaining Frege's objective: they kept the antinomies at bay by adhering to the canons of type that Russell had introduced in 1903 {**R2**}. In fact the PM salvaged the entire Cantorian theory of sets and the Dedekind theory of numbers. Moreover, to use Gödel's words, the subject "was enriched by a new instrument, the abstract theory of relations," on which is based the theory of measurement {**G2**, p. 448}. Russell's use of recursive definitions brought to the forefront the idea of *recursion*, which when set is a proper metamathematical footing by K. Gödel, A. Church, A. M. Turing and others had revolutionary ramifications on mathematical philosophy and, via the work of C. Shannon, J. von Neumann, Wiener and others, on automata theory and industrial technology. It ushered in the age of automatization.

Wiener's Harvard thesis was on "A comparison between the treatment of the algebra of relatives by Schroder and that by Whitehead and Russell." Wiener decided to spend his Harvard overseas traveling fellowship to study mathematical philosophy with Russell at Cambridge. In his thesis Wiener had missed the philosophical import of the theory of types. Relearning it from Russell's lectures was an eye-opener:

> For the first time I became fully conscious of the logical theory of types and of the deep philosophical considerations it represented. [**53h**, p. 191]

In Russell, Wiener had found his second great mentor.

To Wiener, the Russellian hierarchy was more than a convenient tool to keep off the antinomies. Indeed Zermelo (1908) and later von Neumann (1925) and Bernays (1937) kept off the paradoxes more effectively by cutting down the hierarchy to just two: "element and non-element," "set and class," "sets formally expressible, and sets not so expressible." For Wiener, however, it was the hierarchical classificatory attitude behind the Russellian doctrine that was stimulating. For instance, in later years he assigned types 1, 2, 3, to automatons A, B, C, in case B could evaluate the performance of A, and C that of B. Likewise, he assigned types 1, 2, 3,... to the military categories: tactics, strategy, general considerations to use in framing strategy,....

Wiener was wont to see the Russell antinomy behind many a situation where most of us might see none. Roughly he saw it whenever trouble ensued from two variables becoming equal or near equal. He took just as readily to the more Cantorian "paradoxes of the superlative," which yield self-contradictory concepts such as the set of all sets. A favorite self-contradictory concept was "the totally efficient slave."[7] Wiener used such paradoxical concepts tellingly to illustrate phenomena such as the Roman household in which the Greek philosopher-slave becomes the real master.

This love notwithstanding, Wiener was not able to marshall the full potency of the paradoxes in the disciplined and creative way in which K. Gödel was able. His only contribution to axiomatic set-theory was his type-theoretical definition of the ordered pair as a set **[14a]** (age nineteen). This simplification completed a line of thought of C. S. Peirce: it showed that three primitive constants, $\downarrow$, $\forall$, $\in$ suffice for logic and mathematics. It also simplied the theory of types, cf. Quine {**Q1**, p. 163}.

During the years 1914–1920 Wiener did a lot of work on mathematical and general philosophy. The best of this extends the theory of measurement in the PM, Vol. III, to quantities, the range of whose values is bounded, e.g. the intensity of "redness" of a red patch, and to relations such as "seem louder than" **[14b, 15a, 21a]**. There was also a 101-page paper on Kant's theory of space **[22a]**.

A remarkable sequel to the PM appeared in 1919 in the *Tractatus logico-philosophicus* by Ludwig Wittgenstein {**W6**}. The novel ideas in this work led the philosophers of the Vienna Circle to formulate the main theses of *logical empiricism*: (i) the analyticity, or devoidness of factual content, of all logical and mathematical statements; (ii) the hypothetical character of all empirical ones; (iii) the paramount importance of mathematical concepts in the formulation of general hypotheses of the sciences, and of logical and mathematical theorems in the transition from such hypotheses to verifiable

[7]Self-contradictory, because to be fully efficient one has to be free.

experimental and observational statements. The resulting faith in the intimacy of logic-mathematics on the one hand and physics in the wide sense on the other, despite a clear-cut separation between the two, is perhaps best illustrated by the words of Einstein:

> As far as the laws of mathematics refer to reality they are not certain; and as far as they are certain they do not refer to reality. {**E2**, p. 28}[8]

This was the faith which dominated the intellectual climate of the period in which Wiener began research. It affected him in concrete ways. Russell urged him to adopt the broadest standpoint, to concentrate not just on the foundations but also to look at the frontiers of mathematics as well as of theoretical physics. This advice brought Wiener into contact with G. H. Hardy, then a young don. Hardy was without question Wiener's third and perhaps last great mentor. It also exposed Wiener to Bohr's atomic theory, the work of J. W. Gibbs on statistical mechanics and the Einstein–Smoluchowski papers on the Brownian motion.

In this free and clean atmosphere a good physicist could extol the virtues of mathematics without a feeling of having let down his regiment. Thus what the French physicist and Nobel Prize-winner, Jean Perrin, wrote in 1913 was music to Wiener's ears:

> Those who hear of curves without tangents or of functions without derivations often think at first that Nature presents no such complications nor even suggests them. The contrary, however, is true and the logic of the mathematicians has kept them nearer to reality than the practical representations employed by physicists. This assertion may be illustrated by considering certain experimental data without preconception. {**M2**, pp. 5, 6}[9]

These then were the kind of messages that entered into the nonlinear transducer we call Wiener, messages which are hard to come by today. Before considering later inputs, and there were many, let us see the messages that emerged from this transducer.

3. The Leap from Postulate Theory to the Brownian Motion and Potential Theory

After Wiener had left the U. S. Army in early 1919, and joined the Mathematics Department of MIT in the fall of 1919, his intellectual interests began

[8]It should be clear from this quotation that we are interpreting logical empiricism in the broadest way. This is necessary in the light of the criticism levelled by Professor Quine {**Q2**} and others against narrower interpretations of the thesis, cf. Carnap {**C2**}.

[9]For the rest of this quotation, see Mandelbrot {**M2**}.

to move away from the philosophical foundations of mathematics towards its superstructure. He finalized his long paper on his 1915 Docent Lectures at Harvard on geometry and experience [**22a**]. But concurrently he began to focus on the postulates of specific systems then engaging the curiosity of mathematicians.

Space permits us to comment only very briefly on the papers on postulation [**17a**, **20a–e**, **21b**, **22b,c**, **23g**]. They bear the impress of E. V. Huntington and M. Fréchet. Wiener's objective was to do what Sheffer (and in fact C. S. Peirce) had done for the truth-functional sentential calculus, viz. have just one primitive operation, and then study "all the sets of postulates in terms of which the system may be determined" [**21b**, p. 1]. Thus in [**20b**] Wiener introduced a single binary connective $*$ subject to 7 postulates, and showed its equipollence to the usual postulate system for a field $\mathbb{F}$. In the paper [**22c**] on topology, his object was to place postulates on the primitives X, $\sum$, that would make X a topological space, and $\sum$ the group of its homeomorphisms. Thus, a "limit-point" a of a set E is defined by

$$f \in \sum \ \& \ \forall x \in E \backslash \{a\}, \ f(x) = x \ \Rightarrow \ f(a) = a.$$

In [**22b**] Wiener characterized the linear continuum in this manner, departing thereby from Huntington, Veblen and R. L. Moore. Wiener's postulational interests also included metric affine and vector spaces [**20e**, **22c**]. In [**23g**] he assumed for the first time that the metric is complete, thus defining a Banach space, but he focused on the analysis of vector-valued functions, puny stuff in relation to the deep work that Banach had begun a little earlier. There is no need to start speaking of "espaces du type BW".

Among the propellants that steered Wiener away from such work into deeper waters, was his reading of the treatises of Osgood, Volterra, Fréchet and Lebesgue during the summer of 1919, his meetings with Fréchet in Europe, and above all his conversations with the young mathematician I. A. Barnett. From the latter he learned of the potential importance of probabilistic questions in which the events are curves, such as the paths traced by a swarm of flying bees, and of the possibility of using infinite-dimensional vector spaces in their analysis.

Wiener began in earnest. Spotting the papers of Daniell, he tried to adapt them to his needs. He started with a sequence $(\pi_n, w_n)_{n=1}^{\infty}$, where π_n is a finite partition of a fixed set X, and w_n is a function on π_n to $\mathbb{R}_+$. He then took the class

$$\mathscr{L} := \{F : F \in \mathbb{R}^X \ \& \ \exists n \geq 1 \ni F \text{ is } \pi_n\text{-simple}\},$$

and for any F in $\mathscr{L}$, defined its mean-value $M(F)$ by

$$M(F) := \left\{ \sum_{t\in\Delta\in\pi_n} f(t)w(\Delta) \right\} \Big/ \left\{ \sum_{\Delta\in\pi_n} w(\Delta) \right\}.$$

With the assumption that π_{n+1} is a refinement of π_n, and w is finitely additive, $M(F)$ becomes independent of n, and is unique. Wiener subjected the π_n, w_n to further conditions (Kolmogorov's marginals in disguise) and showed that $(\mathscr{L}, M)$ then fulfills Daniell's conditions. Consequently, there is an extension $(\overline{\mathscr{L}}, \overline{M})$, $\mathscr{L} \subseteq \overline{\mathscr{L}}$, $M \subseteq \overline{M}$, $\overline{\mathscr{L}}$ being the class of "summable" functions and $\overline{M}$ the "Daniell" integral. Wiener showed that F is in $\overline{\mathscr{L}}$ if F is "uniformly continuous," i.e., $\inf_{n\geq 1} \sup_{\Delta\in\pi_n} \mathrm{Osc}(F, \Delta) = 0$. This work **[20f]** was to provide a firm mathematical footing for a venture into physics.

Wiener's first object of attack, to wit turbulence, was suggested by the then fresh paper of Sir Goeffrey Taylor {**T1**}. When these attempts failed, Wiener tried his hand on something else he knew, vaguely akin to turbulence, viz. the Brownian movement as conceived by Einstein in his fundamental 1905 paper {**E1**}.

Recall what Einstein had done. He had assumed that there is a positive number τ such that a time-interval of length τ is, in his words,

> ... very small compared to the observed interval of time, but nevertheless, ... such ... that the movement executed by a (colloidal) particle in consecutive intervals of time τ are ... mutually independent phenomena. {**E1**, p. 13}

From this premise Einstein derived the result that the displacements in disjoint intervals are normally distributed, that "the mean (square) displacement is ... proportional to the square root of the time" {**E1**, p. 17}, being given by the equation

$$\bar{d}_t^2 = \frac{RT}{3\pi a\mu N} \cdot t, \tag{1}$$

where T is the temperature, R is the gas constant ($pv = RT$), μ is the viscosity of the liquid, N is Avogadro's number, and a is the radius of the colloidal particle.

Wiener's concern, unlike Einstein's, was with the nature of the curve followed by a single particle. He therefore made the idealization that Einstein's conditions prevail for all positive lengths τ. To this idealized Brownian motion, "an excellent surrogate for the cruder properties of the true Brownian motion" **[56g**, p. 39], Wiener was able to apply the theorem proved in **[20f]**. The nexus is clear from §§3, 4 of **[24d]**. Here Wiener defined the sequence $(\pi_n, w_n)_{n=1}^{\infty}$ so that it not only fulfills the premises of the **[20f]** theorem, but the extensions $(\overline{\mathscr{L}}, \overline{M})$ of the resulting $(\mathscr{L}, M)$ also have the following additional properties. Write

$$(2) \qquad \begin{cases} \mathfrak{X} := \{x\colon x \text{ is continuous on } [0,1] \text{ to } \mathbb{R} \ \&\ x(0) = 0\} \\ \mathscr{B}(\mathfrak{X}) := \{B\colon B \text{ is a Borel subset of } \mathfrak{X}\}. \end{cases}$$

Then $\overline{\mathscr{L}}$ is the class of $\mathscr{B}(\mathfrak{X})$ measurable functions on $\mathfrak{X}$ to $\mathbb{R}$, and writing $\mu(B) := \overline{\mathscr{L}}(1_B)$ for $B \in \mathscr{B}(\mathfrak{X})$, μ has all the properties of what today we call *Wiener measure.* (We cannot, unfortunately, state Wiener's ingenious definitions of π_n and w_n since they involve a lot of notation.)

Excellent commentaries on the papers **[21c, 21d, 23d, 24d]** on the Brownian motion by K. Ito (*Coll. Works*, I), M. Kac {**K1**} and J. Doob {**D3**} are available. However, so overwhelming have been the effects of this work on the development of analysis and probability theory, and later on communication theory, that a little more must be said.

It was only in the 1930s that Wiener was able to unearth what is buried in these papers, and there is a lot, as Kolmogorov's important 1933 work {**K3**} suggested. By mapping the space $\mathfrak{X}$ (cf. (2)), with Wiener measure onto the interval $[0,1]$ with Lebesgue measure, Wiener characterized the Brownian motion as the *stochastic process* $\{x(t,\alpha)\colon t \in [0,1],\ \alpha \in [0,1]\}$, governed by the conditions: (i) the increments $x(b,\cdot) - x(a,\cdot)$ are normally distributed random variables with mean 0 and variances $\sigma^2(b-a)$, i.e.,

$$(3) \qquad \int_0^1 |x(b,\alpha) - x(a,\alpha)|^2\, d\alpha = \sigma^2(b-a), \quad \sigma = \text{const.},$$

the abstract formulation of Einstein's equation (1); (ii) for nonoverlapping intervals $[a,b]$, $[c,d]$ the increments $x(a,\cdot) - x(b,\cdot)$ and $x(c,\cdot) - x(d,\cdot)$ are stochastically independent, cf. {**D2**}. Equivalently we may characterize it as the process for which $x(t,\cdot)$ is normally distributed with zero mean and such that

$$(4) \qquad \int_0^1 x(s,\alpha)x(t,\alpha)\, d\alpha = s \wedge t, \quad s,t \in [0,1].$$

Wiener showed that for almost all α in $[0,1]$, the trajectories $x(\cdot,\alpha)$ are continuous everywhere but differentiable nowhere. Although the functions $x(\cdot,\alpha)$ are of "extremely sinusoidity," and definitely not of bounded variation, Wiener was able to define for any f in $L_2[a,b]$, a "Stieltjes" type integral

$$g(\cdot) = \int_a^b f(t)\, dx(t,\cdot) \quad \text{on } [0,1],$$

and to enunciate the beautiful properties of the random variables $g(\cdot)$ so obtained. Thus Wiener opened up the whole area of probability theory we nowadays call *stochastic integration.* These developments occur in the later works **[33a, 34a, 34d]** done in collaboration with Paley and Zygmund.

The Brownian motion also penetrates deeply into the nonstochastic parts of mathematical analysis. An interesting example is afforded by the initial value problem of the one-dimensional heat equation with potential term. Its solution can be expressed as an integral over $C[0,\infty)$ with respect to Wiener measure. This was discovered in 1948 by Mark Kac under the stimulus of R. P. Feyman's cognate result in nonrelativistic quantum theory, and has led to widespread use of functional integration in both mathematical analysis and field physics.

Since 1950 the ideas of Perrin and Wiener have been receiving considerable enlargement in the researches of Dr. Benoit Mandelbrot and others into phenomena marked by intrinsic irregularities that persist even as we improve the accuracy of the scale of observation. Such are the jagged lines of cracks in rock filaments, for instance. To deal with such irregularities, Dr. Mandelbrot has singled out sets whose Hausdorff dimension exceeds the topological dimension, calling them *fractals*, {**M2**, **M3**}. But it seems clear that this definition is too restrictive. Roughly speaking, fractals emerge after an infinite number of iterations of a step which involves breaking up a set as well as changing the scale. It would seem best to treat the term "fractal" as an undefined, governed by certain postulates. Some significant hints as to this appear in Professor Cannon's 1982 lectures {**C1**}, unfortunately still unpublished. Cannon has found that cognate ideas are useful in the theory of topological manifolds, especially of the hyperbolic type. Thus Wiener's idealized Brownian motion has turned out to be the progenitor of a growing variety of fractals encountered in physics as well as in pure mathematics.

During the early 1920s Wiener often consulted O. D. Kellogg, the Harvard authority on potential theory. Within a very short time these conversations brought him to the frontiers of the subject. Wiener then wrote six papers within a space of three years, which revolutionized the field. In the words of the French authority M. Brelot, he "initiated a new period for the Dirichlet problem and potential theory" {**B8**, p. 41}.

The *Dirichlet problem* is to determine the steady-state temperature distribution $u(\cdot)$ in region R, given its distribution $\phi(\cdot)$ on the boundary S, i.e., to find the function $u(\cdot)$ satisfying the Laplace equation

$$\Delta u = \frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} = 0$$

on R, and such that for any s in S, $u(r) \to \phi(s)$ as $r \to s$. The solution $u(\cdot)$, which depends both on the shape of S and the boundary-distribution ϕ, was known for smooth surfaces S and continuous ϕ. But, as Wiener learned from Kellogg, the issues are extremely complicated when the surface S has sharp dents and corners. This mathematical complexity is reflected in the physical instabilities which occur when chambers have such crooked surfaces, as Wiener noted. An illustration for the electric potential is the glowing of nails and pointed objects observed by sailors during thunderstorms.

Wiener's great contribution was to show that no matter how rough the surface S, the Dirichlet problem has a "solution" in a genuine but nonclassical sense, and to introduce several ideas of lasting value to accomplish this. As this work is very technical and has been commented on by Brelot {**B8**} and in greater detail in the *Collected Works*, I, it will suffice to say just the following. In his papers Wiener introduced the now central and crucial concepts of *capacity* for arbitrary sets and a *generalized solution* for the Dirichlet problem, as well as the criterion of regularity of the solution. To solve the Dirichlet problem, Wiener and H. B. Phillips considerably advanced the finite-difference technique of solving partial differential equations that has become standard forty years later with the advent of computers, cf. §7B.

Mrs. L. Lumer, commenting on [**24a**], writes, "the notion of capacity is perhaps Wiener's most important and long lasting contribution to potential theory" {*Coll. Works*, I, p. 393}. Indeed, this notion has been repeatedly generalized by Frostman, Choquet and others. It may therefore be worthwhile to recall what Wiener showed, viz. if B is a compact set in $\mathbb{R}^q$, $q \geq 2$, then there exists a $\mathbb{R}_{0+}$-valued countably additive measure μ on the Borel subsets of B with the property that $\int_B \mu(dy)/|x-y|^{q-2}$, $x \in \mathbb{R}^q$, tends to 1 on B and tends to 0 as $x \to \infty$. Wiener defined the capacity of B by cap $B := \mu(B)$.

An important link between Wiener's work on Brownian motion and potential theory was discovered by S. Kakutani in 1944 {**K2**}. In $\mathbb{R}^2$, for instance, it expresses the solution u of the Dirichlet problem as an integral $\int_0^1 \dots d\alpha$, where the integrand involves, apart from the boundary function ϕ, the time $\tau(x,y,\alpha)$ at which the Brownian path initially at $(x,y) \in R$ crosses S. It has initiated a new approach to potential theory.

4. From Communications Engineering to Generalized Harmonic Analysis and Tauberian Theory

Wiener was extremely fortunate in finding at MIT a forward-looking electrical engineering department, led by Professor Dougald C. Jackson in the early 1920s and Dr. Vannevar Bush later on. Wiener had an early flair for things electrical, and got along splendidly with the engineers who often sought his advice on mathematical methodology.

The first theoretical task that Wiener undertook at the behest of the engineers was the rigorization of the 1893 operational calculus of Oliver Heaviside. The thought underlying his rigorization is that "when applied to the function e^{nit}, the operator $f(d/dt)$ is equivalent to multiplication by $f(ni)$" [**26c**, p. 550]. Given an arbitrary function f, he dissected it into a number of frequency ranges, and applied to each range that expansion of $f(d/dt)$ which converged on this range.

By making these moves, Wiener in effect produced an embryonic form of the *theory of distributions* that was to come twenty-five years later. In §8 of his paper, which deals with the operational solution of second-order linear partial differential equations in two variables, Wiener wrote:

> ... there are cases where u must be regarded as a solution of our differential equation in a general sense without possessing all the orders of derivatives indicated in the equation, and indeed without being differentiable at all. It is a matter of some interest, therefore, to render precise the manner in which a non-differentiable function may satisfy in a generalized sense a differential equation. [**26c**, p. 582]

In this he anticipated Laurent Schwartz. Moreover, as Professor Schwartz tells us, by 1926 Wiener had seen farther than what all others had seen before 1946:

> Il est amusant de remarquer que c'est exactement cette idée qui m'a poussé moi-même à introduire les distributions![10] Elle a tourmenté de nombreux mathématiciens, comme le montrent ces quelques pages. Or Wiener donne une très bonne définition d'une solution généralisée; j'en avais, dans mon livre sur les Distributions, attribué les premières définitions à Leray (1934), Sobolev (1936), Friedrichs (1939), Bochner (1946), la définition la plus génerale étant celle de Bochner; or la définition de Wiener est la même que celle de Bochner, et date donc de ce mémoire, c'est-à-dire de 1926, elle est antérieure à toutes les autres. {*Coll. Works*, II, p. 427}

In [**26c**] and its sequel [**29c**], Wiener also took the important step of introducing the concept of *retrospective* or *causal operator* thus initiating the theory of *causality and analyticity*: the study of how one-sided dependence in the time domain leads to holomorphism in the spectral domain. In essence he defined an operator T on a space of signals f on $\mathbb{R}$ to be *causal*, if and only if for each t,

$$f_1 = f_2 \text{ on } (-\infty, t) \Rightarrow T(f_1) = T(f_2) \text{ on } (-\infty, t).$$

It follows easily that the transfer operator of a time-invariant linear filter with convolution weighing W, i.e., the filter which yields for input f the output g:

$$(1) \qquad g(t) := (W * f)(t) := \int_{-\infty}^{\infty} W(t-s) f(x)\, ds,$$

[10]Voir l'introduction de mon livre sur les distributions, p. 4.

will be causal, if and only if $W = 0$ on $(-\infty, 0]$. Thus in the causal case, (1) gets amended to

$$g(t) := (W * f)(t) = \int_{-\infty}^{t} W(t-s) f(s)\, ds, \quad t \text{ real.} \tag{2}$$

Equivalently, the Fourier transform $\hat{W}$ is holomorphic on the lower half-plane ($\hat{W}(\lambda) := (1/2\pi)\int_{-\infty}^{\infty} e^{-it\lambda} W(t)\, dt$).

Causal operators are important in engineering, since the transfer operator of a physically realizable filter must obviously be causal. In recent years Wiener's basic ideal of causality has been extended to cover more general "time domains," cones and the like, cf. Foures and Segal {**F1**} , Saeks {**S1**}, and it has also shown up in the so-called *dispersion relations* of quantum mechanics.

The mid 1920s also saw Wiener's embarkation on generalized harmonic analysis. This came from his discernment of a certain parallel between the developments of electrical engineering and of mechanics. In the latter the consideration of uniform motion gave way to that of simple harmonic motion and then periodic motion, notably planetary motion, which, with the rise of statistical mechanics, in turn gave way to the study of highly random movements such as the Brownian motion. In electrical engineering there was first the direct current ("uniform level"), and then came the alternating current of one or several frequencies ("periodic level"). This corresponded to the stage of *power engineering*, the study of generators, motors and transformers, in which the central concept is *energy*. For this study fairly classical mathematics sufficed. But with the advent of the telephone and radio came *communications engineering* in which the central entity is the irregularly fluctuating current and voltage, which carries the *message* ("everything from a groan to a squeak"), and which is neither periodic nor pulse-like (i.e., in L_2). Thus the voltage curve of a busy telephone line has the same kind of local irregularity and overall persistence that Wiener had encountered in the Brownian motion, and he began to associate the communication phase of electrical engineering with the statistical phase of mechanics. For these phases, new and more difficult mathematics was required. He set about to find it, spurred on by his engineering friends.

A Fourierist at heart, Wiener assigned to the notions of orthogonal expansion, linearity and pure tone, a central place. We have harmonic analysis and synthesis when the pure tones are identified with the sinusoidal functions $\cos \lambda t$, $\sin \lambda t$, of different frequencies λ, i.e., in complex notation with the continuous characters e_λ:

$$e_\lambda(t) := e^{i\lambda t} = \cos \lambda t + i \sin \lambda t, \quad t \in \mathbb{R},$$

of the additive group $\mathbb{R}$ of real numbers, their acoustical realizations being the sounds of tuning forks. Wiener attributed great significance to such analysis for the following reasons.

Our faith is that the laws of Nature are invariant under time and space translations. Such invariant laws together with Huygens' Principle give PDEs with time-independent coefficients, and the propagators $U(t,s)$ they yield are invariant under time translations; i.e., $U(t,s) = T(t-s)$, T being a function on $\mathbb{R}$, and the $T(t)$ commute with the translation operators in the space variables. For small oscillations both the PDEs and the $T(t)$ are linear. The characters of the group $\mathbb{R}^3$ are eigenfunctions of linear operators having this commutation property; i.e., with $e_\lambda(s) := e^{i(\lambda' s)}$, $\lambda, s \in \mathbb{R}^3$, we have

$$T(t)(e_\lambda) = \alpha(t,\lambda)e_\lambda,\ \lambda \text{ in } \mathbb{R}^3; \quad \alpha(t,\lambda) := \{T(t)(e_\lambda)\}(0),$$

where $\alpha(t,\lambda)$ is a real or complex number. If the initial $(t = 0)$ disturbance f of the medium can be represented as a linear combination of the characters, $f = \sum c_\lambda e_\lambda$, then from the linearity of $T(t)$, it follows at once that the disturbance at instant t is given by the elegant formula:

$$T(t)(f) = \sum c_\lambda \alpha(t,\lambda)e_\lambda.$$

Thus the problems of expressing functions as combinations of characters, and finding the "Fourier coefficients" c_λ for a given f—in short, *harmonic analysis and synthesis*—are exceedingly important.

Wiener believed that signals of wide varieties are harmonically analyzable, and that for this, the wider class of irregular and persisting curves must be properly demarcated and the averaging operations drastically altered. In this research, the earlier work on the rigorous demarcation of functions having convergent Fourier series being of no avail, Wiener sought his ideas from nonestablishment "radicals" such as Lord Kelvin, Lord Raleigh, Sir Oliver Heaviside, Sir Arthur Schuster and Sir Geoffrey Taylor, who were interested in the harmonic analysis of allied random phenomena in acoustics, optics and fluid mechanics.

To leave history aside, Wiener considered the class S of complex-valued measurable functions f on the real axis $\mathbb{R}$ for which the *auto-covariance function* ϕ:

(3) $$\phi(t) = \lim_{T\to\infty} \frac{1}{2T} \int_{-T}^{T} f(s+t)\overline{f(s)}\,ds$$

exists and is continuous on $\mathbb{R}$. This very large class S includes the almost periodic functions of H. Bohr and A. S. Besicovitch and of course the periodic functions originally analyzed by Fourier. Wiener, spurred by the needs of communication engineers, set out to develop a harmonic analysis for this class [**30a**], guided by Schuster's work.

This generalized analysis has two parts. In the first, one seeks the Fourier associate of ϕ, and in the second, the Fourier associate of f itself. Today, after Bochner's theorem on positive-definite functions, the first part is rather elementary. We have

$$\phi(t) = \int_{-\infty}^{\infty} e^{it\lambda}\, dF(\lambda), \tag{4}$$

where F is a nonnegative distribution function on $\mathbb{R}$, called the *spectral distribution* of f. In the second, Wiener defined for each f in S a *generalized Fourier transform* s by

$$s(\lambda) := \lim_{A\to\infty} \frac{1}{\sqrt{2\pi}} \left(\int_{-A}^{-1} + \int_{1}^{A} \right) \frac{f(t)e^{-it\lambda}}{-it}\, dt + \frac{1}{\sqrt{2\pi}} \int_{-1}^{1} f(t) \frac{e^{-it\lambda} - 1}{-it}\, dt. \tag{5}$$

Wiener, however, defined F, not by (4) but by the analogue of (5) with ϕ replacing f, and only with much difficulty recovered (4), cf. [**30a**, (5.40)]. Both F and s are clearly defined in [**30a**], after much groping extending back to [**25c**].

Very ingeniously, Wiener used the stochastic integral

$$f(t,\alpha) = \int_{-\infty}^{\infty} W(t-\tau)\, d_\tau x(\tau,\alpha), \quad \alpha \in [0,1], \quad W \in L_2(R),$$

of the Brownian motion $x(\cdot,\cdot)$ to show that for almost all α, $f(\cdot,\alpha) \in S$ and for this f the spectral distribution F is absolutely continuous with $F'(\lambda) = \sqrt{2\pi}|\widetilde{W}(\cdot)|^2$, a.e. where $\widetilde{W}$ is the (indirect) Fourier–Plancherel transform of W [**30a**, §13].

The concepts of covariance ϕ of a signal f, the spectral distribution F and their interconnection have an interesting history. The fact that Einstein was a participant was revealed only in 1985, when a remarkable two-page heuristic note of his, dealing with f, ϕ, F', and carrying a version of (4), came to light, cf. {**M6**}. Einstein was unaware of the work of Schuster. The genesis of the different ideas went as follows:

Spectral density F' ("periodogram"): Schuster, 1889; Einstein, 1914.

Covariance ϕ: Einstein, 1914; Taylor, 1920.

Spectral distribution F: Wiener [**28a**].

Interconnections: inverse of (4) with F', Einstein, 1914

(4) itself with F, Wiener [**30a**].

All the work was done independently.[11]

[11] For an interesting, stochastic process interpretation of Einstein's note, and its links to Khinchine's work of 1934, see A. M. Yaglom {**Y1**}.

The s function was exclusively Wiener's, and he felt certain that the following Bessel-type identity between f and s should prevail:

$$\lim_{T\to\infty} \frac{1}{2T} \int_{-T}^{T} |f(t)|^2\, dt = \lim_{h\to 0} \frac{1}{4\pi h} \int_{-\infty}^{\infty} |s(\lambda + h) - s(\lambda - h)|^2\, d\lambda. \tag{6}$$

Wiener showed that its correctness hinged on that of the simpler identity for nonnegative g:

$$\lim_{T\to\infty} \frac{1}{T} \int_{0}^{T} g(t)\, dt = \lim_{h\to 0} \frac{2}{\pi h} \int_{0}^{\infty} g(t) \frac{\sin^2 ht}{t^2}\, dt, \tag{7}$$

an identity of which he seems to have become aware as early as 1925. But Wiener had a hard time proving it. The stalemate broke dramatically during 1926 when Wiener was at Göttingen and Copenhagen on a Guggenheim Fellowship. At Göttingen he met an acquaintance, the British number-theorist I. A. Ingham, and learned from him for the first time that the identity (7) was, as they say, "Tauberian" in nature, and that his mentors Hardy and Littlewood were authorities on such matters. This was news to Wiener. Another important contact was with the Tauberian theorist Dr. Robert Schmidt of Kiel, whom Wiener met at a German mathematical meeting at Düsseldorf after the summer of 1926.

Following Ingham's advice, Wiener studied the work of Hardy and Littlewood, and noticed that they too were concerned with the equality of two means. Then, he suddenly saw that a simple logarithmic change of variables would reduce the integrals occurring in Schmidt's theorems to convolution integrals with which he was familiar from his electrical studies, and that the real problem was to find a theorem for convolution integrals. He then proceeded to find such a theorem, by a novel and hard attack in which no classical Tauberian theorem was used [**28b**, **32a**]. Wiener's final result has a beautifully simple enunciation. Write

$$(W * f)(t) := \int_{-\infty}^{\infty} W(t-s) f(s)\, ds, \quad t \text{ real}. \tag{8}$$

Here $W \in L_1$ and $f \in L_\infty$. Regard $g = W * f$ as the response of an ideal convolution filter W^* with weighing W subject to the input signal f. When will the output g tend to a limit as $t \to \infty$? Wiener's Tauberian theorem gives the answer: *feed the signal f into another filter W_0^* with a nowhere vanishing frequency response, i.e., where the weighing W_0 in L_1 is such that its Fourier transform $\hat{W}_0$ vanishes nowhere. If the resulting response g_0 has a limit as $t \to \infty$, so will the original response g.* Wiener also proved a "Stieltjes" form of this theorem. It may be looked upon as its analogue when the input is a real or complex-valued measure μ over $\mathbb{R}$ and the filter performs the convolution

$$(W * \mu)(t) := \int_{-\infty}^{\infty} W(t-s) \mu(ds). \tag{9}$$

All the classical Tauberian theorems, even the deepest, can be recovered from Wiener's two theorems: one has only to pick W, W_0, f and μ intelligently and change variables. This applies also to (7) and therefore to the Bessel-type identity (6) on which rests the appropriateness of Wiener's generalized Fourier transformation. Thus generalized harmonic analysis was put on a sound footing. But many new theorems were also uncovered. Among these, perhaps the most interesting was the one on the Lambert series, which bears on the analytic theory of prime numbers. It led Wiener and his Japanese former student, Professor S. Ikehara, to a simpler proof of the celebrated Prime Number theorem, to wit

$$\lim_{n\to\infty} \frac{\pi(n)\cdot\log n}{n} = 1,$$

where $\pi(n)$ is the number of primes not exceeding n. For this proof, all one has to know about the Riemann zeta function is that it has no zeros on the line $x = 1$ in the complex plane, and the traditional appeal to contour integration is avoided.

As the enormous ramifications of the memoir **[32a]** (e.g. on Banach algebras) are dealt with extensively in the *Collected Works*, II, we shall say no more about them. The memoir **[30a]** on G.H.A., however, has only recently been assimilated in the framework of abstract analysis. The Wiener class S is a *conditionally* linear subspace (cf. *Coll. Works*, II, pp. 333–379) of the Marcinkiewicz Banach space:

$$\mathfrak{M}_2(\mathbb{R}) := \left\{ f\colon f \in L_1^{\text{loc}}(\mathbb{R}) \;\&\; \|f\|^2 := \overline{\lim_{T\to\infty}}\, \frac{1}{2T}\int_{-T}^{T} |f(t)|^2\,dt < \infty \right\}.$$

K. S. Lau {**L1**} has shown that the f in S with $\|f\| = 1$ are extreme points of the unit ball of $\mathfrak{M}_2(\mathbb{R})$.[12] Wiener's generalized Fourier transform s of a function f in S gets a nice interpretation in terms of helices in the Hilbert space $L_2(\mathbb{R})$. Since the middle 1960s, J. P. Bertrandias in France has subjected $\mathfrak{M}_2(\mathbb{R})$ to extensive analysis from the standpoint of S and its subspaces, cf. {**B3**} and also {**B1**}. J. Benedetto, Benke and Evans have just announced a full-fledged generalization of the Tauberian identity (6) of G.H.A. to functions on $\mathbb{R}^n$ {**B2**}.

Wiener went on to show that G.H.A. has a propaedeutic role in optics [**28d**, **30a** (§9), **53a**]. In Maxwell's theory, the flux of electromagnetic energy at a fixed point P in a medium traversed by light, through a small surface at P perpendicular to the direction of propagation, is proportional to $|E(t)|^2$, where $E(t)$ is the electric vector at P at the instant t in question. This led

[12] Recently, Lau has obtained an interesting extension of BMO spaces by considering the class in which the Marcinkiewicz $\overline{\lim}$ is replaced by sup, cf. {**L2**}.

Wiener to regard $|f(t)|^2$ as the energy at instant t of the signal f in the class S and to regard

$$(10) \qquad \phi(0) = \lim_{T\to\infty} \frac{1}{2T} \int_{-T}^{T} |f(t)|^2 \, d\tau,$$

cf. (3),as the *total-mean-power* or "brightness" of the signal f.

Central to Wiener's clarification of optical ideas was his tacit interpretation of the *photometer* as an instrument which, when impinged with a light signal f in S gives the reading $\phi(0)$. The justification of this came in 1932 when von Neumann {**V1**} noted that instruments have a response time T, and measure not the input $f(t)$ but time-averages $(1/T)\int_{t-T}^{t} f(\tau)\,d\tau$. In the case of the photometer, the enormity of T from the standpoint of atomic chaos allows us to let $T \to \infty$, and equation (10) follows from the ergodic theorem. Next Wiener considered the *Michelson interferometer*, equipped at the output end with a photometer (camera, eye, etc.). A simple calculation shows that when the input $f = E$ (electric field), the observed intensity at the output is

$$\phi(0) + \phi(\Delta l/c),$$

where Δl is the difference between the lengths of the two arms and c is the speed of light. By turning the screws, i.e., changing Δl, we can find $\phi(x)$, for any given (not too large) x. Thus, as Wiener noted, the Michelson interferometer is an analogue computer for the covariance function of light signals.

By extending G.H.A. to vector-valued signals, Wiener defined the *coherency matrix* of a set of light signals $(f_1, \dots, f_q)$, as the spectral distribution matrix F of its matricial covariance functions

$$\Phi = [\phi_{ij}], \quad \phi_{ij}(t) = \lim_{T\to\infty} \frac{1}{2t} \int_{-T}^{T} f_i(t+\tau)\overline{f_j(\tau)}\, d\tau.$$

This also allowed him to deal with polarization (cf. [**30a**, §9] for details).

Elsewhere, we have traced how these ideas bear on the polarization of light and on the question as to why two candles are twice (and not four times) as bright as one {**M4**, §4}. With the reasonable hypothesis that (macroscopically observed) light signals are trajectories of a stationary stochastic process, Wiener was able to justify what physicists such as Schuster and Raleigh knew intuitively but could not formulate rigorously. His ideas have found a place in the standard repertory in optics, e.g. in the treatise by Born and Wolf {**B7**}. But the full significance of some of this work, begun in 1928, emerged only with the advent of lasers, masers and holograms. Sir Dennis Gabor spoke of the coherency matrix as a "philosophically important" idea, adding that "it was entirely ignored in optics until it was reinvented ... by Dennis Gabor in England in 1955 and Hideya Gamo in Japan in 1956." He pointed out that the matrix theory of light propagation had been initiated by Max von Laue in 1907, and covers the transmission of information, and that "the entropy in optical information is a particularly fine illustration of the role of entropy in the Shannon–Wiener theory of communication" (*Coll. Works*, III, pp. 490–491).

5. Max Born and Wiener's Thoughts on Quantum Mechanics

An inequality in classical harmonic analysis, sometimes referred to as "the time-frequency uncertainty principle," reads: if f is in $L_2(\mathbb{R})$ and its L_2-norm $|f|_2$ is 1, and if the integrals $\int_{-\infty}^{\infty} |tf(t)|^2\,dt$ and $\int_{-\infty}^{\infty} |\lambda\tilde{f}(\lambda)|^2\,d\lambda$ are finite, $\tilde{f}$ being the (indirect) Fourier–Plancherel transform of f, then

$$\int_{-\infty}^{\infty} |tf(t)|^2\,dt \cdot \int_{-\infty}^{\infty} |\lambda\tilde{f}(\lambda)|^2\,d\lambda \geq \frac{1}{4}, \tag{1}$$

cf. e.g. Weyl {**W1**, p. 393}. It follows that if a sound oscillation f of intensity (or loudness) l, and centered around $t = 0$, is of short duration, then the first factor on the left being small, the second factor will have to be large, i.e., the oscillation f will comprise a whole range of frequencies, and will not be a pure tone. Conversely, if the oscillation is approximately pure, i.e., its frequencies are all clustered around a single frequency λ_0, then the second factor will be small; the first factor will now have to be large, i.e., the oscillations spread over a long interval of time.

In practical terms, a pure tone of only momentary duration cannot be created. Likewise in optics, it is impossible to produce a light ray passing through a definite point A in a definite direction. (For to ensure that the light passes through A, we will have to interpose in its path a screen with pin hole at A; but the latter will cause the emergent light to diffract, i.e., to spread out in a conical beam, and not along a definite straight line.) Wiener found other such instances in which precision in the determination of a quantity inexorably results in an uncertainty in the value of another.

These ideas formed the contents of Wiener's seminar talk on harmonic analysis at Göttingen in the summer of 1924 [**56g**, p. 106]. At that very time Max Born and Werner Heisenberg were grappling with the failure of the classical laws in atomic radiation, and were becoming gradually aware of the limitations afflicting the simultaneous determination of complementary quantities such as the position and momentum of atomic particles. This interest in uncertainty at Göttingen led Wiener to learn the quantum theory, and to collaborate with Max Born in the fall of 1925 when the latter came to MIT as Foreign Lecturer.

Heisenberg had just enunciated his matrix mechanics to cover the motion of a closed system with discrete energy levels or frequencies, and Born raised the question as to its substitute for nonquantizable systems with continuous spectra, such as rectilinear motion, in which no periods are possible. They wrote a joint paper on this [**26d**]. In it appears for the first time the idea that *physical quantities correspond to linear operators on function space.* They introduced the operator Q defined by

$$Q(f)(\cdot) = \lim_{T\to\infty} \frac{1}{2T} \int_{-T}^{T} q(s,\cdot) f(s)\, ds$$

as a replacement for the Heisenberg matrix for configuration, and obtained commutation laws governing differential operators; they then proposed more general operators on function space. Unfortunately, the function space is left unspecified. It appears to be a subclass of S comprising at least the Besicovitch almost periodic functions on $(-\infty, \infty)$.

In two more papers [**28d**, **29e**] on the subject as well as in G.H.A. [**30a**], Wiener pointed out that the linear-quadratic relationship prevailing in quantum theory also occurs in the branches of classical physics, e.g. white light optics or communication engineering, that demand generalized (rather than ordinary) harmonic analysis. In the theory of white light, for instance, the fundamental Maxwell equations are linear, but pertain to the intrinsically nonobservable quantities $E(t)$ and $H(t)$, whereas the observable intensities are defined in terms of the squares of their amplitudes. Since all observation at this sub-Hertzian level is necessarily photometric, to take an observation amounts to reading $\phi(0)$, where ϕ is the covariance function of the light signal f, cf. §4(10). But an apparatus which reads covariances destroys phase relations. For instance, if $f(t) = \sum_1^n a_k e^{i\lambda_k t}$, then $\phi(t) = \sum_1^n |a_k|^2 e^{i\lambda_k t}$. The phases of the complex numbers a_k are gone. Feeding this "observed light" into another optical instrument will not produce the same response as feeding in the unobserved light f. Thus observation affects the signal and thwarts prediction, much as in quantum mechanics. If we think of the light beam as a vector-signal, then in Wiener's words:

> ... if two optical instruments are arranged in series, the taking of a reading from the first will involve the interposition of a ground-glass screen or photographic plate between the two, and such a plate will destroy the phase relations of the coherency matrix of the emitted light, replacing it by the diagonal matrix with the same diagonal terms. Thus the observation of the output of the first instrument alters the output of the second. [**30a**, p. 194]

Thus in vague analogy with the quantum situation, in Wiener's white light optics, observation has the effect of diagonalizing an operator, and of enlightening the mind only by killing off a lot of information.

The role of the Born–Wiener paper [**26d**] in the history of quantum mechanics is alluded to in Whittaker's history {**W4**, vol. II, p. 267}, and discussed much more fully in J. Mehra and H. Rechenberg's recent comprehensive history of the subject {**M10**, Ch. 5}. The idea of a Hilbert space, still embryonic, was just not in Wiener's consciousness at that time. But the paper [**26d**], its limitations notwithstanding, had "an immediate impact on Heisenberg" as Mehra and Rechenberg point out, and they conclude:

> At a time when just a few physicists struggled to develop a consistent theory of quantum mechanics, the Born–Wiener collaboration not only indicated the way for handling the problem of aperiodic motion but also contributed to the physical interpretation of the theory. {**M11**, p. 246}

In the late 1940s an interesting use of Wiener's Brownian motion in quantum theory was revealed by M. Kac's analysis of the "path integral" defined in R. P. Feynman's important thesis. This stems from the deep resemblance between the "path integration" employed in quantum mechanics and integration over the space $C[0,\infty)$ with respect to the "Wiener measure" induced over this space by the Brownian motion stochastic process. With imaginary time it allows us to use the Brownian motion to prove theorems on Hilbert spaces germane to quantum field theory. We refer the reader to E. Nelson's commentary in the *Collected Works*, III, pp. 565–579.

In the early 1950s Wiener himself realized that the intriguing appearance of probabilities as squares of amplitudes in quantum mechanics was explainable in Gibbsian terms by use of his Brownian motion. This work, done in collaboration with A. Siegel and J. Della Riccia, is complicated and remains unfinished [**55c**, **56c**, **63a**, **66a**]. Since it is hardly understood, a short justification of its validity for pure quantum states may be in order.

Recall that the *states* of a quantum mechanical system are countably additive probability measures μ on the lattice $\mathscr{L}$ of projections P on a separable complex Hilbert space $\mathscr{H}$ (cf. G. Mackey {**M1**}). It follows from Gleason's fundamental theorem {**G1**} that to any pure state μ (i.e., extreme point of the state space) corresponds a unit vector ψ in $\mathscr{H}$ such that

$$(1) \qquad \mu(P) = |P\psi|^2_{\mathscr{H}}, \quad P \in \mathscr{L}.$$

Given any quantum mechanical system for which the Hilbert space $\mathscr{H}$ is $L_2(\mathbb{R}^q)$, Wiener and Siegel exhibit a probability space $(\Omega, \mathfrak{A}, \rho)$ and for any pure state μ, a function M_μ on the lattice $\mathscr{L}$ to the σ-algebra $\mathfrak{A}$ such that

$$(\mathrm{I}) \qquad \mu(P) = \rho\{M_\mu(P)\}, \quad P \in \mathscr{L}.$$

Unfortunately, we do not have the space to define this crucial mapping $M_\mu(\cdot)$.[13] With it Wiener and Siegel fulfill their goal of showing that the probability appearing in quantum mechanics, as the square of the absolute value of the complex-valued function $P\psi$ in $L_2(\mathbb{R}^q)$, is the probability of a

[13] It will appear in a new book on Wiener by this writer.

well-defined subset of a well-understood probability space $(\Omega, \mathfrak{A}, \rho)$, viz. the space of q-parametrized Brownian movement—a generalized Wiener space.

Unfortunately, it is not clear from the Wiener–Siegel work if an equality of the type (I) is available for impure states μ. Furthermore, Wiener and Siegel do not discuss the physical relevance of the Brownian motion as yielding "hidden parameters" in quantum mechanics, although this notion is central to their approach. Nor do they discuss how their use of the Brownian motion fits in with that of Bohm, de Broglie, Vigier and others.

Efforts to pass beyond the present viewpoint of quantum physics are most desirable, for as de Broglie has pointed out, the history of science teaches us

> that the actual state of our knowledge is always provisional and that there must be, beyond what is actually known, immense new regions to discover. {**B4**, p. x}.

Professor Nelson points out that it is "a deep drive within science" that impels such efforts, for without them science would die. See §C for Nelson's commentary, *Collected Works*, III, pp. 575–576, for more.

6. Ergodic Theory, Homogeneous Chaos, Statistical Mechanics, Information, and Maxwell's Demon

> It is one of the greatest triumphs of recent mathematics in America, or elsewhere, that the correct formulation of the ergodic hypothesis and the proof of the theorem on which it depends have both been found by the elder Birkhoff of Harvard. [**38e**, p. 63]

It was Eberhard Hopf, of Potsdam Observatory, who during his visit to MIT in 1931, aroused Wiener's interest in Birkhoff's theorem. Wiener's fascination with the theorem becomes understandable, for recasted in Wienerian terms, it reads:

Ergodic Theorem. *Let $\{f(t,\cdot) : -\infty < t < \infty\}$ be a complex-valued strictly stationary stochastic process such that $f(0,\cdot) \in L_2(\Omega, \mathscr{B}, P)$. Then for P almost all ω in Ω, the signal $f(\cdot,\omega)$ belongs to the Wiener class S, and its covariance function $\phi(\cdot,\omega)$ satisfies the equality*

$$(1) \qquad \phi(\tau,\omega) = E_{\mathfrak{J}}(f(\tau,\cdot)\overline{f(0,\cdot)})(\omega), \quad \tau \in \mathbb{R},$$

where $E_{\mathfrak{J}}\{\cdot\}$ is the conditional expectation with respect to the σ-algebra $\mathfrak{J}$ of invariant sets in $\mathscr{B}$. In case the process is ergodic, i.e. $\mathfrak{J}$ is trivial,

$$(2) \qquad \phi(\tau,\omega) = E\{f(\tau,\cdot)\overline{f(0,\cdot)}\}(\omega), \quad \tau \in \mathbb{R},$$

where $E\{\cdot\}$ is the (unconditional) expectation.

Apart from the equalities (1) and (2), Birkhoff's Theorem validated Wiener's long cherished belief that

$$(3)\qquad \lim_{T\to\infty}\frac{1}{T}\int_{-T}^{0} f(t+\tau)\overline{f(t)}\,dt = \lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{T} f(t+\tau)\overline{f(t)}\,dt.$$

Thus past observations of the signal f suffice for the estimation of its covariance function ϕ, and this function gives information intrinsic to the stochastic process from which the signal hails. The fact that, for this, ergodicity has to be postulated, did not deter Wiener, for the assumption of ergodicity was just one of many idealizations that his scientific philosophy permitted. Moreover, von Neumann's celebrated 1932 theorem on the distintegration of regular measure-preserving flows over complete metric spaces into ergodic sections {**V2**}, gave Wiener a good excuse to deal almost exclusively with the ergodic case, see for instance [**61c**, pp. 55–56].

Wiener's own research in this area began when Paley and Wiener demonstrated the existence of a flow T_t, $t \in \mathbb{R}$, which preserves Lebesgue measure over $[0, 1]$ and is ergodic, and such that for almost all α in $[0, 1]$,

$$x(b+t,\alpha) - x(a+t,\alpha) = x(b, T_t\alpha) - x(a, T_t\alpha), \quad t \in \mathbb{R},$$

where $x(\cdot,\cdot)$ is the Brownian motion stochastic process, cf. the book [**34d**, §40]. The combined use of this result with Birkhoff's theorem appreciably simplifies certain proofs in the memoir [**30a**] on G.H.A., but it is also very significant in several other contexts.

In the paper [**39a**] Wiener, apart from deducing Birkhoff's theorem from von Neumann's mean ergodic theorem, extended the ergodic theorems to measure-preserving flows with several parameters, i.e., flows T_λ, where $\lambda \in \mathbb{R}^n$, $n > 1$, thereby making them available in the study of spatial or spatio-temporal *homogeneous random fields.* For the latter fields, $\lambda = (x, y, z, t)$ represents the space-time coordinates of an evolving random process, in a certain quantity $f(\lambda, \omega)$ of which we are interested. In collaboration with A. Wintner, Wiener also proved that almost all signals f, which emanate from an ergodic stationary stochastic process in L_2, are cross-correlated with the characters e_λ, $e_\lambda(t) = e^{it\lambda}$, i.e., possess generalized Fourier coefficients [**41a, b**]; more fully, for P almost all ω in Ω and *every* λ in $\mathbb{R}$,

$$\lim_{T\to\infty}\frac{1}{2T}\int_{-T}^{T} f(t,\omega)e^{-i\lambda t}\,dt$$

exists. They also gave conditions under which such a signal $f(\cdot,\omega)$ will be a Besicovitch almost periodic function. These results are significant in view of the presence of lots of functions in the unrestricted Wiener class S that are not cross-correlated with the characters e_λ.

In an earlier paper [**38a**] Wiener extended the Birkhoff theorem to homogeneous chaoses. A *homogeneous chaos* (nowadays termed a *stationary random* *measure*) is a finitely additive measure μ on a ring $\mathscr{R}$ of subsets of a group X, such that all its values $\mu(A)$, (A in $\mathscr{R}$) are real- or complex-valued random variables over a probability space $(\Omega, \mathscr{B}, P)$, and furthermore the random variables $\mu(A)$ and $\mu(A+x)$, have the same probability distribution over $\mathbb{R}$ or $\mathbb{C}$, for different x in X. A simple instance is afforded by the measure μ defined by

$$\mu(a,b](\alpha) = x(b,\alpha) - x(a,\alpha), \quad \alpha \in [0,1],\ a \leq b. \tag{4}$$

where $x(\cdot,\cdot)$ is Wiener's Brownian motion. For any real-valued homogeneous chaos μ on a ring $\mathscr{R}$ over $X = \mathbb{R}^n$, and any measurable function f on $\mathbb{R}$, Wiener proved that if

$$\int_\Omega f\{\mu(A)(\omega)\} \cdot \log^+ |f\{\mu(A)(\omega)\}| P(d\omega) < \infty,$$

then for all A in $\mathscr{R}$ and P almost all ω in Ω,

$$\lim_{r\to\infty} \frac{1}{v(r)} \int_{V(r)} f\{\mu(A+t)(\omega)\}\, dt$$

exists, where $v(r)$ is the volume of the ball $V(r)$, center 0, radius r, in $\mathbb{R}^n$, $t = (t_1, \ldots, t_n)$ and $dt = dt_1 \ldots dt_n$. Furthermore, this limit is equal to the expectation $E[f\{\mu(A)\}]$ in case the chaos μ is "ergodic," which Wiener defined to mean

$$\lim_{|t|\to\infty} P\{\omega\colon \mu(A)(\omega) \in G \;\&\; \mu(A+t)(\omega) \in H\} = P\{\omega\colon \mu(A)(\omega) \in H\},$$

for any Borel subsets G and H of $\mathbb{R}$. This was a far-reaching extension of the Ergodic Theorem.

Wiener now felt that he had the right viewpoint and equipment to tackle the problems of statistical mechanics, in particular the problem of turbulence that had evaded him in 1920. He had written the paper **[38a]**, we spoke of, in this hope. In it he represented arbitrary random functions by sums of multiple stochastic integrals of Brownian motion. A joint paper with A. Wintner on the discrete chaos **[43a]** followed. While the importance of the pure mathematical side of these papers is beyond question, their import in statistical mechanics is still in doubt. These issues are discussed in *Collected Works*, I, especially in the comprehensive survey by Drs. McMillan and Deem.[14] On the other hand, a present school of thought, let by Professor J. Bass in Paris, holds that it is more pertinent to regard a turbulent velocity simply as a function in the Wiener class S rather than as a trajectory of some hypothetical stochastic process. This has revived an interest in **[30a]** and has brought to light some hitherto unnoticed connections of Wiener's G.H.A. to H. Weyl's earlier work on *equidistribution*, and to the so-called Monte-Carlo method. We would refer the interested reader to the *Collected Works*, II, pp. 359–372, and the references therein, and to Bass {**B1**} and Bertrandias, et al. {**B3**}.

[14]For more on these questions, see Dr. McMillan's recent article in this series {**M10**}.

At about this time, the concept of entropy began to permeate Wiener's work. From the Second Law of Thermodynamics (Inaccessibility Principle, in Carathéodory's elegant treatment), it follows that for a thermodynamic system α with a state space $\mathscr{S}$ and empirical temperature $\theta_\alpha(s)$ in state s, the Pfaffian equation $dQ = 0$ (Q = heat) has an integrating divisor of the form $T\{\theta_a(\cdot)\}$; i.e., there exists a function $T(\cdot)$ on $\mathbb{R}$ such that

$$dQ/T\{\theta_\alpha(\cdot)\} = dS_\alpha(\cdot) \quad \text{on } \mathscr{S},$$

where $S_\alpha(\cdot)$ is a function on $\mathscr{S}$. The function $T\{\theta_\alpha(\cdot)\}$ on $\mathscr{S}$ is called the *absolute temperature* and $S_\alpha(\cdot)$ the corresponding *entropy* of the system. With the standardization, $T(t) = c \cdot t$, c = const., obtained by taking the empirical temperature $\theta_\alpha(s)$ as that measured by the perfect gas thermometer, it can be shown *that the entropy $S_\alpha(\cdot)$ of the system α cannot decrease in any adiabatic transformation, and must increase in all non-quasi-static adiabatic ones.*

The law of increasing entropy imposes on events an ordering, past → future, determined by increase in entropy. It thus provides the foundation for the objectivity of *anisotropic time*, or "time with an arrow" as Eddington used to say. Many phenomena that interested Wiener, such as controlled experiment, communication, memory and learning, hinge on the anisotropy of time.

Recall that the molecular kinetic theory asserts first that disorder at the atomic level engenders at the microscopic level phenomena governed by probabilistic laws such as the Brownian motion, and second that what the causal phenomena at the macroscopic level exemplify are statistical stabilities emerging from the cooperation of an enormous number of irregular impulses. Guided by this and by the intrinsically stochastic aspect of subatomic phenomena (quantum theory), Wiener held that there is a random element in the very texture of Nature, and that *the orderliness of the world is incomplete.* We can no longer regard the universe as a strictly deterministic system, the state of which at any instant t is determined exclusively by its states at all previous instances $t' < t$. It was Wiener's position that we still have a cosmos: the Principle of the Uniformity of Nature still reigns, but at a stochastic level. It is the probability measures, engendered by the statistical aspect of Nature, that remain invariant under time-translations. Ergodic considerations become paramount. (See [**50j**, *Introd.*] and [**55a**, pp. 251–252].)

Wiener also realized that the time-concept that emerged from the contingent nature of the cosmos had more to it than mere anisotropy. The indeterminism of the new physics opened up the possibilities of noise and orderliness, freedom, innovation, growth and decay, error and learning. Stochastic prediction and filtering rest on the possibilities of contingency, innovation and noise, as does modern control theory, military science, meteorology and a host of other fields. The writings of the French philosopher H. Bergson on time and evolution, though somewhat diffuse, where poignant in stressing these nonextensive (nonspatial) novelty-creating aspects of time. Wiener therefore spoke of *Bergsonian time* in contrast to *Newtonian time*, when he wanted to emphasize the last aspect of time, cf. [**61c**, Ch. I].

The transmission of messages via a medium (or channel) is a statistical phenomenon in Bergsonian time, for we have to deal with a collection of messages (such as those that cross a telephone exchange) not prescribed by definite laws but only by a few statistical rules. Generally speaking, the more informative a message, the longer it will be, and the more the energy needed to transmit it. Clearly, a proper numerical measure of the *informative-value of a transmitted message* is needed. Wiener and Shannon provided such measures suitable for telephony and telegraphy, respectively.

For a recipient of a message, who knows the probability distribution p of the different outcomes of a repeatable experiment being performed far away, the *informative value* of the message "the outcome x has occurred" is deemed to be

$$I(x) := -\log p\{x\} = \log[1/p\{x\}].$$

(This definition of $I(x)$ meets the reasonable requirement that the more a message removes uncertainty, the greater its informative value.) Let X be the set of all atomic outcomes of the experiment. Then what concerns the transmission engineer are not the individual values $I(x)$, but rather their average:

$$\text{(1)} \qquad \operatorname{Inf}(p) := E[-\log p\{\cdot\}] = -\sum_{x \in X} p\{x\} \cdot \log p\{x\}.$$

We may call this *the average informative value of the probability distribution p*. It gives the average energy (and average cost) of transmission. This definition, in which X is finite or at most countable, is due to Dr. C. E. Shannon in 1947 or 1948 {**S3**}, who was at the Bell Telephone Laboratories and was concerned with the energy-efficient coding of telegraphic messages over noisy channels. It presaged his deep work on channel capacities, encoding and decoding.

In the summer of 1947 Wiener was led to the same problem for an absolutely continuous probability distribution p over the real line $\mathbb{R}$, by the needs of filter theory. Since an infinite sequence of binary digits is required to transmit a real number, a limiting approach, starting with the Shannon concept, will assign an infinite average information to such a p. Wiener's starting point was the observation that we may forget all digits after a fixed number because of noise. By an argument, very obscurely presented, he arrived at the following definition for the *average informative value of p*:

$$\text{(2)} \qquad \operatorname{Inf}(p) = E\{-\log p'(\cdot)\} = -\int_{\mathbb{R}} p'(x) \cdot \log p'(x)\, dx,$$

where $p'(\cdot)$ is the probability density, cf. [**61c**, p. 62, (3.05)].[15] He too took logarithms to the base 2. This definition is used in the theory of information processing for continuous time. Wiener's Inf, unlike Shannon's, can become negative. Nevertheless both Inf's have essential common features.

The nexus between communications engineering and statistical mechanics, which Wiener had dimly discerned in the mid 1920s (cf. §4) is deep indeed, for the Shannon–Wiener concept of information has turned out to be a disguised version of the *statistical entropy* to which Boltzman was driven seventy years earlier. The demonstration is hard, and the final result linking the Boltzman entropy of the gas in the complexion λ to the Shannon average information of an associated probability measure p_λ, is impossible to state here since we would have to define Boltzman's concepts of the complexion of a gas and of its statistical entropy. Be it noted, however, that to prove his H-theorem, $d\,\mathrm{Ent}(s_t)/dt \geq 0$, Boltzman had to replace his summation by an integral. This integral is precisely the Wiener average information of a multidimensional probability distribution, cf. [**61c**, p. 63] and Born {**B6**, pp. 57 (6.25), 165}.

Whereas the Boltzman entropy of a gas is best interpreted as a measure of "internal disorder," Shannon's average information is most naturally interpreted as a measure of "uncertainty removed". Their equality has suggested the term *negentropy*, or *measure of internal order*, as a substitute for the term *information* in certain contexts.

The cogency of this viewpoint has become clear from the work of Szilard, Brillouin and also Wiener [**50g**, **52a**] on the Maxwell demon.[16] To perform its miracles, the demon must receive information about impending molecular movements, and for this, electromagnetic radiation must be available in the gas. But each time the demon draws information from a photon of light, it degrades its energy and (by Planck's law) also its frequency, and creates an equal amount of entropy. The entropy of the matter-radiation mixture is not reduced. For the mixture itself, nothing miraculous occurs.

But Wiener's imaginative mind was not satisfied with this rather easy disposal of the demon. While the demon fails in its overall mission, it still scores a local success: it enhances the negentropy in its immediate neighborhood by degrading the photons of light. Does not such enhancement occur when a piece of green leaf uses sunlight to produce portions of a molecule of a

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[15]Wiener omitted the minus sign, but this is of little consequence since the integral can take any real value for different p.

[16]Dr. J. R. Pierce {**P1**, pp. 198–200, 290} defines this demon as "a hypothetical and impossible creature, who without expenditure of energy, can see a molecule coming in a gas which is all at one temperature, and act on the basis of this information". Seated in a cup of water with an insulating partition having a tiny door, it can by intelligently opening and shutting the door warm the water on one side of the partition and cool it on the other.

carbohydrate from the carbon dioxide and water in its midst, and to release a molecule of oxygen, following the chemical formula:

$$\text{Light} + nCO_2 + nH_2O \xrightarrow[\text{phyll}]{\text{chloro-}} (CH_2O)_n + nO_2?$$

If so, a piece of green leaf in an environment of carbon dioxide and water, irradiated by the sun, is a thermodynamic machine studded with "Maxwell demons" (particles of chlorophyll), all of whom enhance negentropy *locally* by degrading the sunlight impinging on them.

Briefly, no demons, no life. But Wiener also noticed the temporariness of the demon's local successes. It could perform locally only as long as it had usable light, i.e., light from a source at a temperature higher than that of the gas. In a gas-radiation mixture in equilibrium, it will be as helpless as in a gas devoid of light. Ultimately, it too will fall into equilibrium and its intelligent activities cease. In short, it will die. Wiener felt that these reflections on the demon impinged on the biological issues of life, decay and death. See **[50g]**, and Brillouin {**B9**}.

7. The Limitations of this Survey

What we have surveyed so far is roughly 70 percent of Wiener's mathematical work and 25 percent of his work in the empirical realms. What has been left out comprises work that has had a marked impact on contemporary life and thought. Because of space limitations, only its synoptic description is possible.

A. Work on the **Hopf-Wiener integral equation**, in collaboration with E. Hopf [**31a**], and the further exploration of the underlying idea of *causality and analyticity* with R.E.A.C. Paley [**33a**, **33e**, **34d**]. The factorization and other techniques introduced in [**31a**] and [**34d**] have had enormous ramifications, cf. the commentary of J. Pincus is in *Collected Works*, III.

B. Work on **electrical networks and analogue and digital computers** from 1926–1940. This was preceded by an early flair for things electrical, a fascination with Leibniz's *Ars Characteristica*, and by practical experience as computer at the U.S. Army Proving Grounds in Aberdeen, Maryland, in 1918–1919. In 1926 he conceived the *optical integraph*, an analogue computer for convolutions $y = \int_0^a f(t)g(x-t)\,dt$. This was put in-the-metal with better and better designs by Bush's junior colleagues, starting with K. E. Gould {**G3**} in 1929 and ending with Hazen and Brown {**H2**} in 1940. In the early 1930s came the *Lee–Wiener network* {**L3**}.

More remarkable was Wiener's 1940 letter and memorandum to Bush on *mechanical solution of PDEs*. Filed away, it began to surface only in the late 1970s. It is printed in *Collected Works*, IV [**85a**, **b**] and in the *Annals of the History of Computing* {**M7**} with comments by B. Randell and by S. K.

Ferry and R. E. Saeks. Wiener's proposed machine is digital, and embodies a discrete quantized numerical algorithm, Turing machine architecture, binary arithmetic and data storage, an electronic arithmetic logic unit, and a multitrack magnetic tape. Wiener was about fifteen years ahead of his time in both his recommendation of magnetic taping, and his emphasis on attaining "several thousand times the present speed" with only a slight increase in cost. Such speeds were attained only by the first generation of transistorized computers (IBM 7090, etc.) in the late 1950s.

C. The work on **anti-aircraft fire control** with Mr. Julian Bigelow (1940–1943). The shift from deterministic to stochastic prediction culminated in Wiener's book on *time-series* [**49g**]. The understanding of its relationship to Kolmogorov's monumental work on stationary sequences {**K4**}, and subsequent work by Wiener and others on the multivariate and nonlinear cases brought into being the **theory of prediction**. This subject has wide ramifications in functional analysis, as the commentaries of P. Muhly and H. Salehi in the *Collected Works*, III, show.

The Wiener–Bigelow work has another large component, on *filtering, control and regulation.* This, as is well known, has revolutionized the field of communications engineering, cf. the extensive commentaries of T. Kailath in *Collected Works*, III, and Y. W. Lee {**L4**}.

D. In an important military document in 1942 appear the words:

> ... we realized that the "randomness" or irregularity of an airplane's path is introduced by the pilot; that in attempting to force his dynamic craft to execute a useful maneuver, such as a straight-line flight, or a 180-degree turn, the *pilot behaves like a servomechanism*, attempting to overcome the intrinsic lag due to the dynamics of his plane as a physical system, in response to a stimulus which increases in intensity with the degree to which he has failed to accomplish his task.[17] (emphasis added)

These observations of Bigelow and Wiener, when integrated with the thought of the neurophysiologist Dr. Arturo Rosenblueth, led to the joint paper on *teleology* [**43b**] which opened the field of **cybernetics** [**48f**, cf. **61c**]; cf. also Ashby {**A1**}. This led to further work often in conjunction with Drs. W. McCulloch and W. Pitts in the following areas:

(a) Self-learning and reproducing servomechanisms; organization and homeostasis [**58i**, **62b**];

(b) Neural nets, and the proximity of the brain and the electronic computer {**S2**, **M8**}, and [**61c**, **53d**];

[17] N. Wiener: *A. A. Directors, Summary Report*, June 1942, Department of Defense, see p. 6, para. 1, cf. *Coll. Works*, IV, p. 170.

(c) Pattern recognition ("Gestalt") [**61c**, pp. 133–139];

(d) Brain-rhythms and electro-encephalography;

(e) Sensory and muscular-skeletal prosthesis.

These contributions are commented on extensively by Drs. John Barlow, R. W. Mann, W. Ross Ashby, H. von Foerster and this writer in *Collected Works*, IV, and all the relevant Wiener papers are cited therein.

E. **Physiological work** on muscle clonus, heart flutter and fibrillation, spike potential of axons and synaptic excitation, done in collaboration with Dr. A. Rosenbleuth and others. It will suffice to refer to the survey of Dr. Garcia Ramos in *Collected Works*, IV.

F. In his book, *The Nerves of Government*, the political scientist, K. Deutsch, quotes the following words of Wiener:

> Communication is the center that makes organizations. Communication alone enables a group to think together, to see together, and to act together. All sociology requires the understanding of communication. {**D1**, p. 819}

Wiener's work on the **cybernetical aspects (communication and control) of social organization** can be classified as follows:

(a) *The difference between long-time and short-time institutions.* Low-probability events (benevolent "acts of Grace," or malevolent "acts of God" in insurance parlance) become important in long-time prediction and planning. A greater faith in the benevolence of God, and a different system of investment is required in the planning of long-time institutions (cities, universities, cathedrals) than in the management of short-time ones. This led Wiener to the concept of the *long-time State* [**62c**]. The *control of the means of communication* being "the most effective and most important" of all "homeostatic factors in society" (cf. [**61c**, p. 160]), Wiener felt that their control should be entrusted to the long-time institutions: the churches, universities, academies, etc. Their entrustment to short-sighted profit makers is pernicious [**61c**, pp. 161, 162], [**50j**, pp. 131–135].

(b) *The capitalist market as an n-person game.* Far from being a homeostatic process, the capitalist market is a highly volatile one, with recurrent down-sides and a propensity to misuse the channels of communication, and to accept mass gullibility and an indifferent system of education [**61c**, pp. 158–160], [**50j**, pp. 132–134].

(c) *Military theory.* The von-Neumann-Morgenstern game theory {**V3**} is of service in military contests only at the lowest level. Operations at higher levels proceed by strategic evaluations based on the analysis of enemy time-series [**60d**]. Continual reconnaisance, essential to good military planning, is not possible with units that operate on different "time scales" (cf. [**60d**,

p. 721]). The atomic bomb is a bad weapon from this standpoint [**UP1**, p. 6]. This evaluation of Wiener's ideas on military theory differs somewhat from that of S. Heims {**H3**}.

(d) *Observer-observed coupling in the social fields.* Wiener's sharp perception of the difference between such coupling in the natural and social sciences (cf. Bohr {**B5**}) led to his skepticism concerning much of the economics and sociology that is dressed up in classical mathematical garb. He felt that non-classical branches of mathematics such as game theory and fractals were more appropriate to these fields [**61c**, p. 163], [**64e**, pp. 90–91].

Wiener was more than a great theoretician. Ever since 1943, when he first surmised the occurrence of growing automatization in the American economy ("the Second Industrial Revolution"), he made efforts to alert American labor to its social consequences and emphasize the need for ever-expanding education and retraining. But his efforts fell on deaf ears. His eventual correspondence with Walter Reuther of the United Auto Workers led to the formation of a Council on Science and Labor in 1952. It never got off-ground. Today, American society is paying the penalty for its disregard of Wiener's far-sighted wisdom, and for its pathetic condescension to the debasement of its schools, cf. Lynd {**L6**}.

8. Wiener's Place in the Philosophia Perennis

Wiener's cybernetically inspired conception of history enabled him to lay bare the illusory aspects of certain basic beliefs now in vogue, and to unveil more balanced and realistic attitudes on issues of human survival.

Thus Wiener saw the speciousness of the belief in "unlimited human progress" that came from the French Enlightenment and Marxism. Wiener's religious thought rests on the analogy he drew [**50j**, p. 11] between entropy and St. Augustine's negative evil {**A2**, vol. 1}. "The paradox of homeostasis is that it always collapses in the end" [**UP2**, p. 103]. The life of man is further afflicted by his corrupt inclinations. In crying over spilt milk, beating about the bush and venting greed, man, his cerebral cortex notwithstanding, is less intelligent than the puppy and the elephant. Man's murderousness has grown with his knowledge and understanding, and so-called "rational self-interest," far from redounding to the common good, becomes a gateway to avarice and to the spoilation of man and earth [**61c**, p. 158]. In the words of T. S. Eliot, "Sin grows with doing good."[18] Wiener's writings wisely emphasize the fact that man is not "animal rationale" but "animal symbolicum," and a corrupt one at that, and wisely observe that the well-balanced tragic attitude depicted in Greek mythology is more conducive to the human welfare than

[18]For an interesting early exchange of views between T.S. Eliot and Wiener, whose backgrounds have interesting parallels, see *Coll. Works*, IV, pp. 68–75.

the anxiety-ridden attitude of many a modern man hankering after success and progress, [**50j**, pp. 40, 41, 183, 184].

The fact that the human species is severely handicapped led Wiener to view the chief function of science in human life as prosthetic: "That of maintaining a rapport with the environment, which will enable us to face our environment and its changes as we come to them" [**UP2**, p. 102]. The insights of Pythagoras, Plato, Aristotle, Aquinas, Newton, Kant, and (after non-Euclidean geometry and mathematical logic) of Whitehead and Einstein have enlarged our understanding of the scientific methodology, and of the organic enterprise we call science. But its evolution notwithstanding, this enterprise has an enduring integrity stemming from its all-time prosthetic value. From this standpoint, the savage, who formulates his observations of Nature in animistic terms, is trying to understand Nature in order to overcome his handicaps, and is thus pursuing science. This wholesome concept of science is entirely antithetical to the view that it is a "game against Nature"—a sad confusion of the disparate activities of inquiry and contest.

The arts too, Wiener felt, subserve a homeostasis in human life, and he did not attribute much significance to their differences with the sciences. Indeed, for mathematics the difference vanishes:

> Mathematics is every bit as much an imaginative art as it is a logical science. [**23a**, p. 269]

cf. also [**29h**]. Unlike Halmos {**H1**}, however, Wiener was speaking of all mathematics, the pure corresponding to the *presentative* aspect of art, and the applied to its *representative* aspect. He cited Einstein's general theory (which Halmos disqualifies as "matho-physics") as a magnificent example of both forms of art:

> This double aspect of Einstein's work, and indeed of all physics, may serve as a final link between mathematics and the arts. As is well known, most of the arts possess both a presentative and a representative aspect. A painting has beauty not merely as a study in abstract design but as a representation of the outer world. ... Thus mathematics, too besides the beauty of inner structure, has a further beauty as a representation of reality. This is most clear in mathematical physics but even in the purest of pure mathematics, mathematical physics often serves as a valid if unconscious guide. Many a pure mathematical study is an impression of some chord of the physical world. [**29h**, p. 162]

To Wiener the creative activities in both fields appeared as manifestations of a spirit seeking objectification, cf. [**29h**, pp. 130–131]. This attitude towards the aesthetic impulse brought Wiener very close to the religious view of art of the scholastics Dante, Meister Eckehart, and St. Bonaventura, cf. Coomaraswamy {**C3**}.

In religion also Wiener saw a homeostatic and prosthetic factor. The survival of physics depends on its ceaseless quest for ideal concepts: particles without volume, perfect liquids, the electromagnetic field, the momentum-energy tensor, and so on. The belief that daily living too is enhanced by inclusion of ideal, nonphysically representable elements is the religious view of life. The average family placed in a social system, almost invariably exploitative, needs "acts of grace" for its successful survival, no less than a long-time institution such as a city, cf. §7F(a). Uncorrupted religiosity promotes individual acts of grace and thus serves a very high homeostatic and prosthetic purpose. But Wiener was wary of rigid creeds, and of course saw in corrupted religious establishments an anti-homeostatic factor.

Thus, unlike many contemporaries, Wiener did not let the revolutions of thought that make (and partition) the history of science obscure the vision of its fundamental continuity. There was nothing "anti-Euclidean" in non-Euclidean geometry or Mengenlehre or fractals, and nothing "anti-Newtonian" in relativistic mechanics.[19] Indeed, the notion of fractal has roots extending to Aristotle, as Mandelbrot has indicated {**M3**, p. 406}, and Wiener's own cybernetical ideals go back at least to Leibniz, if not to Plato's *Georgias*, as S. Watanabe has suggested (cf. *Coll. Works*, IV, p. 215). A similar perception of continuity marked Wiener's vision of history as a whole. An admirer of both the sixteenth-century Renaissance and the eighteenth-century French Enlightenment, he was blinded by neither. The removal of medieval teleology from post-Renaissance science was a boon, but Wiener contributed to its useful restoration in a modern scientific setting [**43b**]. The same remark applies to his view of the long-time State [**62c**], cf. {**A2**, vol. 2, **C4**}. The facile division of history into three ages, viz. of superstition, religion and science, is shallow. The symbiotic relation of religion and science has been well expressed by Einstein: "Science without religion is lame, religion without science is blind {**E4**, p. 26}, cf. also H. Weyl {**W2**, pp. 89, 214}.

A unique and significant aspect of Wiener's writings is the underlying thought that the incomplete and contingent cosmos revealed by science merits the same feeling of awe that Einstein expressed in the words, "Intelligence is manifested throughout all Nature" and in his references to Spinoza's God {**E3**}. The stochastic aspect is not an impairment. This Pythagorean faith

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[19]Wiener saw no parallels between developments in twentieth-century science and those in twentieth-century art. His disdain of the latter is indicated by his favorite title "The Emperor's New Clothes" for many a piece of modern painting. On the other hand, Wiener did sense a resemblance between nineteenth-century mathematics and the German Romantic movement, cf. [**29h**].

sustained Wiener's ability to fuse the transcendent and the abstract with the practical and concrete. In his words:

> ... so often in my work, the motivation that has led me to the study of a practical problem has also induced me to go into one of the most abstract branches of pure mathematics. [**56g**, p. 192]

Unlike Wigner {**W5**}, Wiener found nothing perplexing or "unreasonable" about the efficacy of mathematics in the sciences. In fact, the investigations of the remarkable medical men surrounding him (all disciples of Russell, cf. {**R3**}), showed that mathematical relation-structure is all that is preserved in the course of sense-observation and the subsequent neurological transitions that constitute cognition {**M8**, **M9**, **R1**}. The paramountcy that Pythagoras, Plato, Roger Bacon, and Galileo accorded to mathematics was not misplaced.

Wiener's towering stature in the history of science rests not only on his unusual ability to discern so much unity amid such wide apparent diversity—he lived, in Struik's words, "the life of the unity of science"—but in his appreciation of its continuity. He was a revolutionary-traditionalist in the best sense of the word. He incorporated in the edifice of human wisdom the new stochastic storey without impairing the total architecture.

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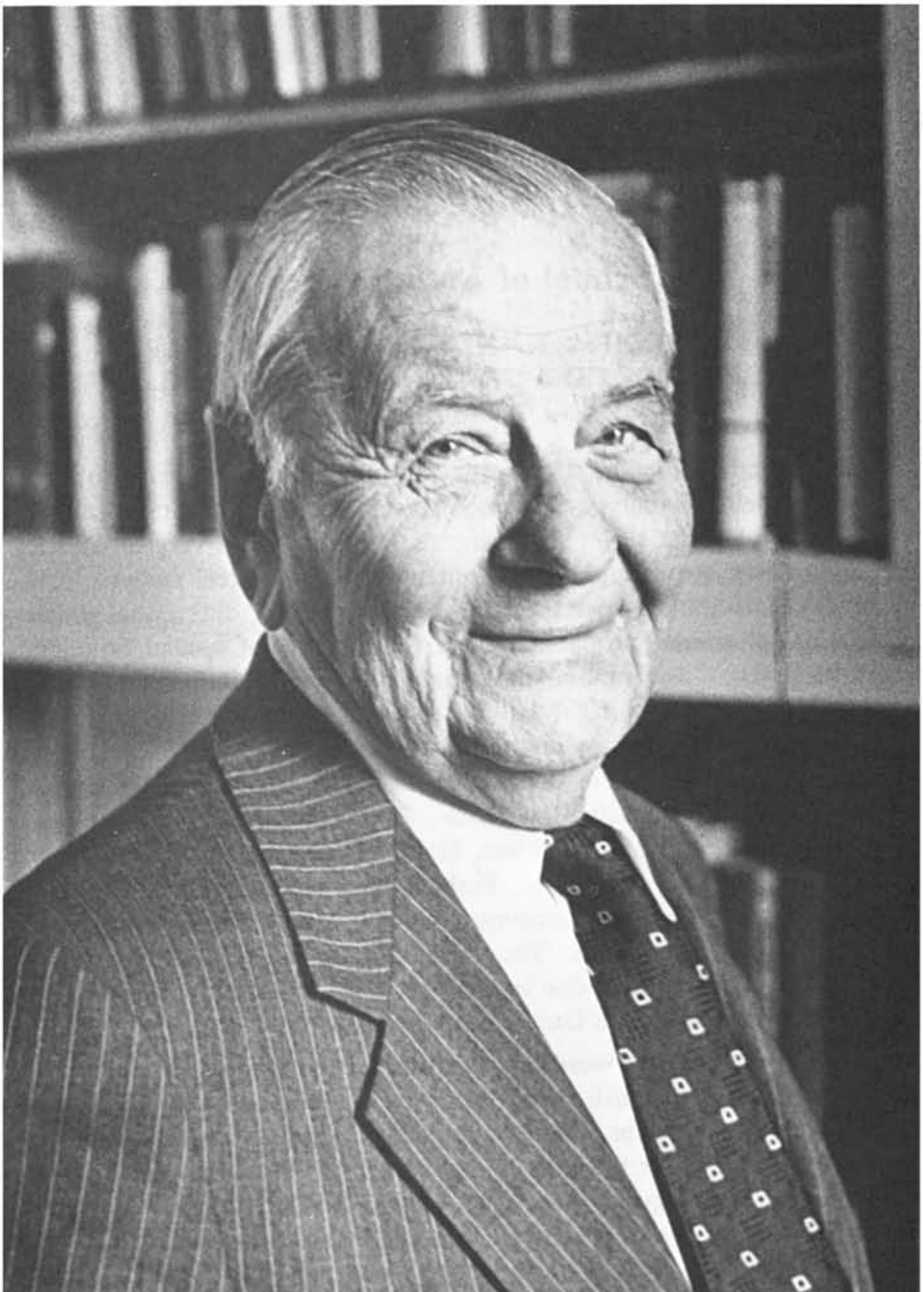
The School of Antoni Zygmund

RONALD R. COIFMAN AND ROBERT S. STRICHARTZ
WITH THE HELP OF GINA GRAZIOSI AND JULIA HALLQUIST

To most mathematicians, the words "harmonic analysis" bring to mind a narrow subfield of analysis dedicated to very technical and classical subjects involving Fourier series and integrals. In fact, it is a very broad field that draws from, inspires, and unifies many disciplines: real analysis, complex analysis, functional analysis, differential equations, differential geometry, topological groups, probability theory, the theory of special functions, number theory, ... Several mathematicians have contributed to the breadth and influence of harmonic analysis. We mention only a few names of those who were active in this century before the second World War: Bernstein, Besicovitch, Bochner, Bohr, Denjoy, Fejér, Hardy, Kaczmarz, Kolmogorov, Lebesgue, Littlewood, Lusin, Menschov, Paley, Plancherel, Plessner, Privalov, Rademacher, F. and M. Riesz, Steinhaus, Szegö, Titchmarsh, Weyl, Wiener, G. C. and W. H. Young. Perhaps it is even appropriate to mention that Cantor's theory of transfinite numbers has its origin in a problem involving trigonometric series. The present status and prominence of harmonic analysis, however, is due in large part to Antoni Zygmund and the school that he created in the United States.

We shall first say a few words about Antoni Zygmund and try to explain why he was able to establish such a large and influential school. By doing this, we shall also describe, briefly, the field of harmonic analysis and the vision Zygmund had for this discipline. We then present a two-generation "mathematical genealogy" of Zygmund's students and their students. We do this for two reasons. First, we believe that this is the most concrete evidence we can provide for gauging the influence Zygmund had. Second, such a compilation may be a most useful document for a historian of mathematics.

Antoni Zygmund was born in Warsaw, Poland, on December 26, 1900. After completing high school, he enrolled in the University of Warsaw in 1919. A few months later, he enlisted in the Polish army where he served during the creation of the state of Poland. He returned to Warsaw when



Antoni Zygmund
1987
(Photograph courtesy of University of Chicago News and Information.)

the fighting ceased and graduated from the University in 1923. He studied with Aleksander Rajchman and devoted himself to the study of trigonometric series. He and Rajchman wrote some joint papers on summability theory. Another of his teachers was Waclaw Sierpiński with whom he published a paper in 1923. While still a student, he met Saks, who was three years older. Saks had a significant influence on Zygmund. They wrote some joint papers and later produced an excellent text on the theory of functions.

He began his teaching career at the Warsaw Polytechnical School. From 1926 to 1930, he held the position of "Privat Dozent" at the University of Warsaw. During these years in his native city, Zygmund's mathematical activity (mostly in the field of trigonometric series) was intense. He spent the academic year 1929–1930 in England as a Rockefeller Fellow at the Universities of Oxford and Cambridge. There he met both Hardy and Littlewood as well as others who shared his scientific interests. In particular, it was there that the seeds of an important collaboration with R. E. A. C. Paley were sown. He also met Norbert Wiener with whom he and Paley later wrote a seminal paper that showed the important relationship probability has with the theory of Fourier series. During the ten months in England, he wrote ten papers.

In the summer of 1930, Zygmund was appointed Associate Professor of mathematics at the University of Wilno. He stayed there until March 1940, when, together with his wife and son, he managed to escape from occupied Poland. The ten-year period in Wilno was a remarkably productive one. His unique ability to integrate ideas from many fields and his sense of direction on various subjects are evident from his publications during this decade. His collaboration with Paley pointed the way to the many connections between the theory of functions and the study of Fourier series. With Paley and Wiener, he showed the important ties between this last topic and probability theory. In Wilno, he discovered a brilliant youth, Josef Marcinkiewicz. It is one of the many tragedies of the second World War that this very talented man died in the spring of 1940 when he was serving as an officer in the Polish army. Together with Marcinkiewicz, Zygmund explored and pioneered in other fields of analysis. This effort included an important paper on the differentiability of multiple integrals (another young mathematician, Jessen, was involved in this research as well). Much of the subsequent study of functions of several real variables depends on the ideas in this work. Perhaps the most important achievement of this period was the publication of the first edition of his famous book *Trigonometrical Series*. In this book, one can find practically all the important results that were known on this subject, as well as its connections with other disciplines. In addition to the topics we have already mentioned, the book includes subjects and points of view that were new at that time. In particular, one should keep in mind that it was during this period that much of modern functional analysis was developed in Poland by Banach and others. In Zygmund's book, one can find the treatment of function spaces and operators on them that is much in the spirit of this new topic. It was in this work that the importance of the M. Riesz Convexity Theorem, as a tool for studying operators, was made evident.

Thanks to the efforts of J. D. Tamarkin, Norbert Wiener, and Jerzy Neyman, in 1940 he received an offer of a visiting professorship at M.I.T. as well as a visa to the United States. The American academic world, at that time, was facing many problems. Zygmund had to start his American career from the beginning. From 1940 to 1945, he was an assistant professor at Mount Holyoke College. During this period, he was also granted a leave of absence to spend the academic year 1942–1943 at the University of Michigan. This, too, was a prolific period for Zygmund. He produced eleven papers. His collaboration with Raphael Salem began at this time. A little-known fact is that one of these papers, with Tamarkin, contains the elegant proof of the M. Riesz Convexity Theorem that is known as the "Thorin proof." This proof gave birth to the "complex method" in the theory of interpolation of operators. Thorin did obtain his proof earlier (in 1942), but he did not publish it until 1947. Zygmund acknowledged Thorin's priority and always referred to the result involved as the "Riesz-Thorin Theorem." All this was done despite the very heavy teaching schedule (by modern standards) of nine hours per week. We should add that often, during his career in Poland, Zygmund had comparably heavy teaching duties.

In 1945, Zygmund accepted an associate professorship at the University of Pennsylvania where he stayed until 1947. In that year, he was invited to join the faculty at the University of Chicago where he spent the rest of his career. This was the beginning of an exceptional period for Zygmund and, more generally, for mathematics. Under the leadership of its chancellor, Robert M. Hutchins, the University of Chicago became a world leader in many academic fields. In particular, Hutchins hired Marshall H. Stone who built an exceptional department of mathematics in the ensuing years. In addition to Zygmund, he brought many distinguished mathematicians to this department. S. Mac Lane, S. S. Chern, and A. Weil were some of the senior men that joined well-known professors already in the department: A. Adrian Albert, E. P. Lane, and L. M. Graves. The more junior newcomers who came developed into well-known leaders in their fields. I. Kaplansky, P. Halmos, and I. E. Segal were some of these. Distinguished visitors from all over the world spent various periods of time at the University of Chicago. J. E. Littlewood, M. Riesz, L. Hormander, S. Smale, and R. Salem represent only a very small and arbitrarily chosen sample of this group. In addition to all this, a large number of extraordinary graduate students came to Chicago to study with this illustrious group.

Zygmund flourished in this atmosphere. Many of the talented young people who came to study in Chicago became his students. In addition, he went to Argentina in 1949 on a Fulbright fellowship where he discovered two outstanding students, Alberto Calderón and Mischa Cotlar. Both went to Chicago and soon earned their Ph.D.'s with him. Calderón soon became Zygmund's collaborator, and their joint work is of such importance that many refer to the school we are discussing as the "Zygmund–Calderón school." Though this name appropriately classifies an important portion of harmonic analysis, it does not cover all that should be referred to as the "Zygmund school."

It is important to realize the following unique features of this school. When Zygmund came to Chicago, the "trend" in mathematics was very much influenced by the Bourbaki school and other forces that championed a rather abstract and algebraic approach for all of mathematics. Zygmund's approach toward his mathematics was very concrete. He felt that it was most important to extend the more classical results in Fourier analysis to other settings, to show the connections of this field to others (as we have already indicated in this article) and to discover methods for carrying this out. He realized that fundamental questions of calculus and analysis were still not well understood. In a sense, he was "bucking the modern trends." In retrospect, his approach proved to be very successful. This is seen not only by what we state here (his achievements and the two-generation genealogy that includes more than 170 names), but by the fact that the very concrete problems posed by Zygmund, with well-defined scope, attracted many of the very gifted students in Chicago to work with him.

Zygmund continued making important contributions. Perhaps the most significant is the second edition of his book *Trigonometrical Series*. This two-volume work, published in 1959, includes all that was in the earlier edition in addition to most of the development in the field that occurred in the twenty-five years after the first edition was written. This was a tremendous effort for Zygmund. He complained to J. E. Littlewood that writing this book cost him at least thirty research papers. Littlewood replied that the book was worth more than twice that many good papers. His work with Calderón, of course, was of paramount importance. Even before he met Calderón, he often said that "the future of harmonic analysis lies in several dimensions." The Calderón–Zygmund theory is a giant step in this direction. They developed a theory of "singular integral operators" that has led to many advances in the theory of partial differential equations and many other fields.

By 1956, Zygmund had trained the three students, Calderón, Elias M. Stein, and Guido Weiss, who were to form the backbone of the Zygmund school, not only because of their research contribution, but because of the large number of students they have trained, a total of seventy-three to date (a number that will probably increase to seventy-seven by the time this article is printed). He continued having students until 1971. Even after that date, however, he was active mathematically. Soon after coming to Chicago, he organized a weekly seminar that consisted of a one-hour presentation of a current topic followed by an informal hour of discussion. This discussion was open to anyone who wanted to present an idea or formulate a problem. This "Zygmund Seminar" continued under his leadership through the seventies and early eighties.

We have described, briefly, some of Zygmund's work, vision, and influence in the study of Fourier series and integrals. We indicated that he was a pioneer in showing how this field was connected with the theory of functions, probability theory, functional analysis, analysis in higher-dimensional Euclidean spaces, and partial differential equations. A more thorough biography would indicate an even broader vision. He showed the importance of certain function spaces: $L \log L$, the weak type spaces, the space of smooth functions (he was most proud of this creation). He paved the way to other topics in higher dimensions by being the first to establish important results in the theory of Hardy spaces involving analytic functions of several variables. By writing a beautiful paper on the Marcinkiewicz Interpolation Theorem (after Marcinkiewicz's death), he led the way to "the real method" in the theory of interpolation of operators. His collected works have been compiled and include more than 150 publications. We give a precise reference to this volume at the end of this article, where we cite some other works containing relevant historical material.

Zygmund's personality contributed greatly to the influence he had on his students and colleagues. He was gentle, generous, and friendly. His interests always extended way beyond mathematics. Literature and current events occupied a considerable amount of his attention. The beginning of each day was devoted to a thorough reading of the New York Times, and he ended the day engrossed in a book; but mathematics was his passion. His outlook on life and his considerable sense of humor almost always were connected with mathematics. Once when walking past a lounge in the University of Chicago that was filled with a loud crowd watching TV, he asked one of his students what was going on. The student told him that the crowd was watching the World Series and explained to him some of the features of this baseball phenomenon. Zygmund thought about it all for a few minutes and commented, "I think it should be called the World Sequence." On another occasion, after passing through several rooms in a museum filled with the paintings of a rather well-known modern painter, he mused, "Mathematics and art are quite different. We could not publish so many papers that used, repeatedly, the same idea and still command the respect of our colleagues." His judgements of others, however, was usually kind. Once, when discussing the philosophy of writing letters of recommendation, he said to one of his students, "Concentrate only on the achievements, and ignore the mistakes. When judging a mathematician you should only integrate f+ (the positive part of his function) and ignore the negative part. Perhaps this should apply more generally to all evaluations of your fellow men." Despite his considerable achievements, he always considered others as his equal and made his students feel at ease with him. He was always easy to approach and encouraged students to come and talk with him. His office was often filled with students and colleagues.

The Genealogy

The following is a list of all of Zygmund's Ph.D. students in the U.S. in chronological order. Under each student, indented, is a list of all his or her students (through 1987), also in chronological order. Each entry lists the current affiliation if known, the date the Ph.D. was granted, the university granting the Ph.D., and the thesis title. Zygmund also had four Ph.D. students in Poland: L. Jasmanowicz, L. Lepecki, J. Marcinkiewicz, and K. Sokol-Sokolowski; the last three are deceased.

Before presenting this list, let us make a few observations about such a genealogy. Such a list has to be terminated somewhere. We have chosen to limit ourselves to the second generation since the influence of Zygmund as a teacher would be quite diluted by the third generation. We are aware that there are quite a few mathematicians who either totally or partially retrained under Zygmund and his students, but do not show up on our list. One of us (Coifman), for example, was a student of Karamata, but studied intensively under Guido Weiss and, later, Calderón and Zygmund. We are also aware that a Ph.D. student may have more than one advisor. For example, when Calderón and Zygmund were at the University of Chicago together, they had common students. A consequence is that those officially listed as Zygmund students have their students on our list, while those listed as Calderón students do not. A similar situation occurred at Washington University between Coifman and Weiss (the Coifman students do not appear on our list). To the best of our knowledge, our list reflects the advisor-student relation that was given to us by the departments of mathematics involved. We know that there are many who have made significant contributions to the Zygmund school but who are not mentioned here. We offer our apologies to them for this and ask for their understanding.

The students of Zygmund are listed in boldface. The second generation's names are indented and are listed below the name of their advisor.

Acknowledgments

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ZYGMUND'S PH.D. STUDENTS IN THE U.S

Nathan J. Fine
Retired, Pennsylvania State University
Ph.D. 1946, University of Pennsylvania
"On the Walsh Functions"

Justin J. Price
Purdue University
Ph.D. 1956, University of Pennsylvania
"Some Questions about Walsh Functions"

Anthony W. Hager
Wesleyan University
Ph.D. 1965, Pennsylvania State University
"On the Tensor Product of Function Rings"

William A. Webb
Washington State University
Ph.D. 1969, Pennsylvania State University
"Automorphisms of Formal Puiseux Series"

Ching-Tsu Loo
Ph.D. 1948, University of Chicago
"Note on the Properties of Fourier Coefficients"

Alberto Calderón
Buenos Aires, Argentina
Ph.D. 1950, University of Chicago
I. "On the Ergodic Theorem"
II. "On the Behavior of Harmonic Functions at the Boundary"
III. "On the Theorem of Marcinkiewicz and Zygmund"

Robert T. Seeley
University of Massachusetts, Boston
Ph.D. 1959, M. I. T.
"Singular Integrals on Compact Manifolds"

Irwin S. Bernstein
City College, CUNY
Ph.D. 1959, M.I.T.
"On the Unique Continuation Problem of Elliptic Partial Differential Equations"

Israel Norman Katz
Washington University, Dept. of Systems, Science and Math.,
St. Louis, Missouri
Ph.D. 1959, M.I.T
"On the Existence of Weak Solutions to Linear Partial Differential Equations"

Jerome H. Neuwirth
University of Connecticut
Ph.D. 1959, M.I.T.
"Singular Integrals and the Totally Hyperbolic Equation"

Earl Berkson
University of Illinois
Ph.D. 1961, University of Chicago
I. "Generalized Diagonable Operators"
II. "Some Metrics on the Subspaces of a Banach Space"

Evelio Tomas Oklander
Deceased
Ph.D. 1964, University of Chicago
"On Interpolation of Banach Spaces"

Cora S. Sadosky
Howard University
Ph.D. 1965, University of Chicago
"On Class Preservation and Pointwise Convergence for Parabolic Singular Operators"

Stephen Vági
DePaul University
Ph.D. 1965, University of Chicago
"On Multipliers and Singular Integrals in L_p Spaces of Vector Valued Functions"

Nestor Rivire
Deceased
Ph.D. 1966, University of Chicago
"Interpolation Theory in S-Banach Spaces"

John C. Polking
Rice University
Ph.D. 1966, University of Chicago
"Boundary Value Problems for Parabolic Systems of Differential Equations"

Umberto Neri
University of Maryland
Ph.D. 1966, University of Chicago
"Singular Integral Operators on Manifolds"

Miguel De Guzmán
Universidad Complutense de Madrid
Ph.D. 1967, University of Chicago
"Singular Integral Operators with Generalized Homogeneity"

Carlos Segovia
Universidad de Buenos Aires
Ph.D. 1967, University of Chicago
"On the Area Function of Lusin"

Keith William Powers
Ph.D. 1972, University of Chicago
"A Boundary Behavior Problem in Pseudo-differential Operators"

Alberto Torchinsky
Indiana University
Ph.D. 1972, University of Chicago
"Singular Integrals in Lipschitz Spaces of Functions and Distributions"

Robert R. Reitano
Senior Financial Officer for John Hancock
Ph.D. 1976, M.I.T.
"Boundary Values and Restrictions of Generalized Functions with Applications"

Josefina Dolores Alvarez Alonso
Florida Atlantic University
Ph.D. 1976, Universidad de Buenos Aires
"Pseudo Differential Operators with Distribution Symbols"

Telma Caputti
Universidad de Buenos Aires
Ph.D. 1976, Universidad de Buenos Aires
"Lipschitz Spaces"

Carlos Kenig
University of Chicago
Ph.D. 1978, University of Chicago
"H_p Spaces on Lipschitz Domains"

Angel Eduardo Gatto
DePaul University
Ph.D. 1979, Universidad de Buenos Aires
"An Atomic Decomposition of Distributions in Parabolic H_p Spaces"

Cristian E. Gutierrez
Temple University
Ph.D. 1979, Universidad de Buenos Aires
"Continuity Properties of Singular Integral Operators"

Kent Merryfield
California State Univ., Long Beach
Ph.D. 1980, University of Chicago
"H_p Spaces in Poly-Half Spaces"

F. Michael Christ
UCLA
Ph.D. 1982, University of Chicago
"Restriction of the Fourier Transform to Submanifolds of Low Codimension"

Gerald Cohen
Ph.D. 1982, University of Chicago
"Hardy Spaces: Atomic Decompostion, Area Functions, and Some New Spaces of Distributions"

Maria Amelia Muschietti
National University of La Plata, Argentina
Ph.D. 1984, National University of la Plata
"On Complex Powers of Elliptic Operators"

Marta Urciuolo
National University of Cordoba, Argentina
Ph.D. 1985, University of Buenos Aires
"Singular Integrals on Rectifiable Surfaces"

Bethumne Vanderburg
Ph.D. 1951, University of Chicago
"Linear Combinations of Hausdorff Summability Methods"

Henry William Oliver
Professor Emeritus Williams College (Retired 1981)
Ph.D. 1951, University of Chicago
"Differential Properties of Real Functions"

George Klein
Ph.D. 1951, University of Chicago
"On the Approximation of Functions by Polynomials"

Richard P. Gosselin
University of Connecticut
Ph.D. 1951, University of Chicago
"The Theory of Localization for Double Trigonometric Series"

Richard Montgomery
University of Connecticut, Groton
Ph.D. 1973, University of Connecticut
"Closed Sub-algebra of Group Algebra"

Leonard D. Berkovitz
Purdue University
Ph.D. 1951, University of Chicago
I. "Circular Summation and Localization of Double Trigonometric Series"
II. "On Double Trigonometric Integrals"
III. "On Double Sturm-Liouville Expansions"

Harvey Thomas Banks
Brown University
Ph.D. 1967, Purdue University
"Optimal Control Problems with Delays"

Lian David Sabbagh
Sabbagh Associates, Inc.
Ph.D. 1967, Purdue University
"Variational Problems with Lags"

Thomas Hack
Ph.D. 1970, Purdue University
"Sufficient Conditions in Optimal Control Theory and Differential Games"

Jerry Searcy
Ph.D. 1970, Purdue University
"Nonclassical Variational Problems Related to an Optimal Filter Problem"

Ralph Weatherwax
Ph.D. 1972, Purdue University
"Lagrange Multipliers for Abstract Optimal Control Programming Problems"

William Browning
Applied Math. Inc.
Ph.D. 1974, Purdue University
"A Class of Variational Problems"

Gary R. Bates
Murphy Oil
Ph.D. 1977, Purdue University
"Hereditary Optimal Control Problems"

Negash G. Medhim
Atlanta University
Ph.D. 1980, Purdue University
"Necessary conditions for Optimal Control Problems with Bounded State by a Penalty Method"

Jiongmin Yong
University of Texas, Austin
Ph.D 1986, Purdue University
"On Differential Games of Evasion and Pursuit"

Victor L. Shapiro
University of California at Riverside
Ph.D. 1952, University of Chicago
"Square Summation and Localization of Double Trigonometric Series"
"Summability of Double Trigonometric Integrals"
"Circular Summability *C* of Double Trigonometric Series"

Aaron Siegel
Deceased
Ph.D. 1958, Rutgers University
"Summability *C* of Series of Surface Spherical Harmonics"

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Kenyon College
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"Green's Formula, Linear Continuity, and Hausdorff Measure"

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"A Theorem on the Uniqueness of (C_{11}) Summability of Walsh Series"

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Ph.D. 1965, University of Oregon
"Capacity of Sets and Harmonic Analysis on the Group 2^{ω}"

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"The Uniqueness of Hermite Series Under Poisson–Abel Summability"

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"Boundary Behavior of Random Valued Heat Polynomial Expansions"

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"Uniqueness Theory of the Haar and Walsh Series"

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"Hankel Transforms and Generalized Axially Symmetric Potentials"

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"Removable Sets for Pointwise Solutions of Elliptic Partial Differential Equations"

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"Spherical summability of Conjugate Multiple Fourier Series and Integrals at the Critical Index"

Richard Escobedo
Ph.D. 1971, University of California at Riverside
"Singular Spherical Harmonic Kernels and Spherical Summability of Multiple Trigonometric Integrals and Series"

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Ph.D. 1973, University of California at Riverside
"Uniqueness of Laguerre Series Under Poisson-Abel Summability"

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"Trigonometric Approximation, Fréchet Variation, and the Double Hilbert Transform"

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"The Dini Condition and a Certain Nonlinear Elliptic System of Partial Differential Equations"

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"Isolated Singularities and Regularity of Certain Nonlinear Equations"

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"An Extension to n-dimensions of Certain Nonlinear Equations"

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"Second and Higher Order Quasilinear Ellipticity on the N-torus"

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"Continuity of Potential Operators in Spaces with Weighted Measures"

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I. "The Central Limit Theorem for Orthonormal Systems"
II. "The Walsh Functions"

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II. "A Convergence Theorem"
III. "On Some High Indicies Theorems"

Syed A. Husain
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"Convergence Factors and Summability of Orthonormal Expansions"

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"On the Littlewood-Paley and Lusin Functions in Higher Dimensions"

James R. McLaughlin
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"On the Theorem of Fatou and Stepanoff"

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Ph.D. 1974, Syracuse University
"On the Degree of Approximation of a Function by Partial Sums of its Fourier Series"

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Ph.D. 1974, Syracuse University
"Bounded Variation and its Generalizations"

Arthur D. Shindhelm
Ph.D. 1974, Syracuse University
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"Topics in Generalized Bounded Variation"

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Ph.D. 1986, Syracuse University
"Functions of Generalized Bounded Variation and Fourier Series"

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"Functions of Generalized Bounded Variation.
Summability of Fourier Series"

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Ph.D. 1955, University of Chicago
"On an Extension of the Cesàro Method of Summability to the Logarithmic Scale"

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Princeton University
Ph.D. 1955, University of Chicago
"Linear Operators on L_p Spaces"

Stephen Wainger
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Ph.D. 1962, University of Chicago
"Special Trigonometrical Series in K-Dimensions"

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Washington University in St. Louis
Ph.D. 1963, University of Chicago
"Smoothness and Differentiability Conditions for Functions and Distributions on E_n"

Robert S. Strichartz
Cornell University
Ph.D. 1966, Princeton University
"Multipliers on Generalized Sobolev Spaces"

Norman J. Weiss
Queens College, CUNY
Ph.D. 1966, Princeton University
"Almost Everywhere Convergence of Poisson Integrals on Tube Domains Over Cones"

Daniel A. Levine
Ph.D. 1968, Princeton University
"Singular Integral Operators on Spheres"

Charles Louis Fefferman
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"Inequalities for Strongly Singular Convolution Operators"

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Ph.D. 1970, Princeton University
"Fourier Analysis on Matrix Space"

Lawrence Dickson
Ph.D. 1971, Princeton University
"Some Limit Properties of Poisson Integrals and Holomorphic Functions on Tube Domains"

Steven G. Krantz
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Ph.D. 1974, Princeton University
"Optimal Lipschitz and L_p Estimates for the Equation $\bar{\partial} u = F$ on Strongly Pseudo-Convex Domains"

William Beckner
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"Inequalities in Fourier Analysis"

Robert A. Fefferman
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Ph.D. 1975, Princeton University
"A Theory of Entropy in Fourier Analysis"

Israel Zibman
Ph.D. 1976, Princeton University
"Some Characteristics of the n-Dimensional Peano Derivative"

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Ph.D. 1975, Princeton University
"Some Character Identities for Semisimple Lie Groups"

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Ph.D. 1977, Princeton University
"Fourier Analysis on the Heisenberg Group"

Duong Hong Phong
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Ph.D. 1977, Princeton University
"On Hölder and L_p Estimates for the $\bar{\partial}$ Equation on Strongly Pseudo-Convex Domains"

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Ph.D. 1978, Princeton University
"A Local Version of Real Hardy Spaces"

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Ph.D. 1978, Princeton University
"L_p Estimates for the Wave Equation"

Meir Shinnar
Ph.D. 1978, Princeton University
"Analytic Continuation of Group Representations"

Robert Michael Beals
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Ph.D. 1980, Princeton University
"L_p Boundedness of Certain Fourier Integral Operators"

David Saul Jerison
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Ph.D. 1980, Princeton University
"The Dirichlet Problem for the Kohn Laplacian on the Heisenberg Group"

Charles Robin Graham
University of Washington
Ph.D. 1981, Princeton University
"The Dirichlet Problem for the Bergman Laplacian"

Allan T. Greenleaf
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Ph.D. 1982, Priniceton University
"Prinicipal Curvature and Harmonic Analysis"

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Ph.D. 1985, Princeton University
"Probabilistic Square Functions, Martingale Transforms and A Priori Estimates"

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University of Chicago
Ph.D. 1985, Princeton University
"Oscillatory Integrals and Spherical Harmonics"

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"Small Time Local Controllability"

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Ph.D. 1986, Princeton University
"The Szegö K Kernel as a Singular Integral Kernel on a Weakly Pseudo-Convex Domain"

Peter N. Heller
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"Analyticity and Regularity for Nonhomogeneous Operators on the Heisenberg Group"

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"Maximal Function Estimates for Meromorphic Nevanlinna Functions"

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"On L_p and Hölder Estimates for the $\bar{\partial}$-Neumann Problem on Strongly Pseudoconvex Domains"

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Ph.D. 1987, Princeton University
"Riesz Means and Multipliers for Hermite Expansions"

Hart F. Smith
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Ph.D. 1988, Princeton University
"The Subelliptic Oblique Derivative Problem"

William J. Riordan
Ph.D. 1955, University of Chicago
"On the Interpolation of Operations"

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Ph.D. 1956, University of Chicago
"Singular Integrals"

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"On Certain Classes of Function Spaces and on the Interpolation of Sublinear Operators"

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"On Boundary Behavior of Temperatures in Several Variables"

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"Operators Acting on Lorentz Spaces"

Robert Ogden
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"Harmonic Analysis on the Cone Associated with Noncompact Orthogonal Groups"

Robert William Latzer
Ph.D. 1971, Washington University
"Non-Directed Light Signals and the Structure of Time"

Richard Rubin
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"Harmonic Analysis on the Group of Rigid Motions of the Euclidean Plane"

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"Interpolation Theorems on Generalized Hardy Spaces"

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"Multiplier Operators for Expansions in Spherical Harmonics and Ultraspherical Polynomials"

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"Harmonic Analysis of a Second Order Singular Differential Operator Associated with Non-Compact Semi-Simple Rank-One Lie Groups"

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"The Molecular Theory of $H^{p,q,s}(H^n)$"

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"Certain Hardy-Type Spaces that can be Characterized by Maximal Functions and Variations of the Square Functions"

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"Function Spaces Generated by Blocks Associated with Spheres, Lie Groups and Spaces of Homogeneous Type"

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Ph.D. 1957, University of Chicago
"The Law of the Iterated Logarithm for Lacunary Series and Applications to Hardy-Littlewood Series"

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"Topics in the Theory of Uniqueness of Trigonometric Series"

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"Prime Geodesic Theorems"

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Ph.D. 1958, University of Chicago
"On Certain Singular Integrals"

Eileen L. Poiani
Saint Peter's College, Jersey City, NJ
Ph.D. 1971, Rutgers University
"Mean Cesàro Summability of Laguerre and Hermite Series and Asymptotic Estimates of Laguerre and Hermite Polynomials"

Hsiao-Wei Kuo
Ph.D. 1975, Rutgers University
"Mean Convergence of Jacobi Series"

Ernst Adams
Ph.D. 1981, Rutgers University
"On Weighted Norm Inequalities for the Riesz Transforms of Functions with Vanishing Moments"

Efrem Herbert Ostrow
California State University, Northridge
Ph.D. 1960, University of Chicago
"A Theory of Generalized Hilbert Transforms"

Richard O'Neil
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Ph.D. 1960, University of Chicago
"Fractional Integration and Orlicz Spaces"

Jack Bryant
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"Spaces Formed by Special Atoms"

Marvin Barsky
Beaver College, Glenside, PA
Ph.D. 1964, University of Chicago
"On Repeated Convergence of Series"

Chao Ping Chang
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"On Certain Exponential Sums Arising in Conjugate Multiple Fourier Series"

Eugene Barry Fabes
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Ph.D. 1965, University of Chicago
"Parabolic Partial Differential Equations and Singular Integrals"

Max Jodeit
University of Minnesota
Ph.D. 1967, Rice University
"Symbols of Parabolic Singular Integrals and Some L_p Boundary Value Problems"

Julio Bouillet
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Ph.D 1972, University of Minnesota
"Dirichlet Problem for Parabolic Equations with Continuous Coefficients"

Stephen Sroka
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Ph.D. 1975, University of Minnesota
"The Initial-Dirichlet Problem for Parabolic Partial Differential Equations with Uniformly Continuous Coefficients and Data in L_p."

Angel Gutierrez
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Ph.D. 1979, University of Minnesota
"A Priori L_p-Estimates for the Solution of the Navier Equations of Elasticity, Given the Forles on the Boundary"

Gregory Verchota
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Ph.D. 1982, University of Minnesota
"Layer Potentials and Boundary Value Problems for Laplace's Equation on Lipschitz Domains"

Patricia Bauman
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Ph.D. 1982, University of Minnesota
"Properties of Non-Negative Solutions of Second Order Elliptic Equations and Their Adjoints"

Russell Brown
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"Layer Potentials and Boundary Value Problems for the Heat Equation in Lipschitz Domains"

Richard Lee Wheeden
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Ph.D. 1965, University of Chicago
"On Trigonometirc Series Associated with Hypersingular Integrals"

Edward P. Lotkowski
Ph.D. 1975, Rutgers University
"Lipschitz Spaces with Weights"

Russell T. John
Ph.D. 1975, Rutgers University
"Weighted Norm Inequalities for Singular and Hypersingular Integrals"

Douglas S. Kurtz
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Ph.D. 1978, Rutgers University
"Littlewood–Paley and Mulitplier Theorems on Weighted L_p Spaces"

J. Marshall Ash
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Ph.D. 1966, University of Chicago
"Generalizations of the Riemann Derivative"

P. J. O'Connor
Ph.D. 1969, Wesleyan University
"Generalized Differentiation of Functions of a Real Variable"

I. Louis Gordon
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"Perron's Integral for Derivatives in L_r"

Yorham Sagher
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Ph.D. 1967, University of Chicago
"On Hypersingular Integrals with Compex Homogeneity"

Michael Cwikel
Israel Institute of Technology

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Ph.D. 1967, University of Chicago
"On Differentiation and Derivatives in L^r"

Leo Frank Ziomek
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Ph.D. 1967, University of Chicago
"On the Boundary Behavior in the Metric L_p of Subharmonic Functions"

William C. Connett
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"Formal Multiplication of Trigonometric Series and the Notion of Generalized Conjugacy"

Thomas Walsh
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"Singular Integrals of L^1 functions"

Marvin J. Kohn
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"Riemann Summability of Multiple Trigonometric Series"

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Ph.D. 1971, University of Chicago
"On the Best Values of the Constants in the Theories of M. Riesz, Zygmund, and Kolmogorov"

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Richard Askey started to work with special functions with I. I. Hirschman while an undergraduate at Washington University. He received his Ph.D. in 1963 from Princeton University as a student of S. Bochner. After two years at the University of Chicago, learning more analysis from A. Zygmund and his colleagues, he went to the University of Wisconsin. There he progressed backwards from proving norm inequalities, to proving positivity, and is now trying to evaluate and transform series and integrals, and so discover new results for future handbooks.

Handbooks of Special Functions

RICHARD ASKEY

1. BACKGROUND

Special functions are functions that satisfy certain differential equations, or difference equations, or are given by certain series or integrals. To be a special function, the function must arise often enough so that someone gives it a name, and then others use this name.

Some special functions were discovered so long ago it is probably impossible to determine who discovered them. For example, there are early books dealing with spherical trigonometry, but I do not know who introduced trigonometric functions and derived their fundamental properties. These functions and logarithms were once widely represented in books of tables, but these have now been replaced by pocket calculators.

In the eighteenth century, a number of other special functions were discovered. Euler found the gamma function and used it to evaluate a beta integral. He also considered the differential equation that is now called the hypergeometric equation and found both integral representations for solutions and series expansions for one solution. Bessel functions were studied by a number of people. See Watson [**45**] for some references.

Elliptic integrals were also studied by a number of people, from Fagnano and Euler to the systematic treatment by Legendre.

Starting with Gauss, systematic treatments of a number of functions were given. Gauss considered the hypergeometric function

$$y = {}_2F_1\left(\begin{matrix}a,b\\c\end{matrix};x\right) = \sum_{n=0}^{\infty}\frac{(a)_n(b)_n}{(c)_n n!}x^n \tag{1.1}$$

where the shifted factorial $(a)_n$ is defined by

$$(a)_n = \Gamma(n+a)/\Gamma(a). \tag{1.2}$$

The function defined in (1.1) is also written as ${}_2F_1(a,b;c;x)$.

Euler had studied this function and discovered some instances of what Gauss called contiguous relations. Two series given by (1.1) are said to be contiguous if they have the same power series variable, if two of their parameters agree and if the third differs by one. Euler discovered some contiguous relations when he found a continued fraction representation for

$${}_2F_1\left(\begin{matrix}a,b+1\\c+1\end{matrix};x\right)\Big/{}_2F_1\left(\begin{matrix}a,b\\c\end{matrix};x\right). \tag{1.3}$$

He stated this for integrals rather than series, but eventually he discovered the identity of these different representations of this function. However, he did not systematically explore these contiguous relations. Gauss did. He showed that ${}_2F_1(a,b;c;x)$ and any two functions contiguous to it are linearly related. There are $15 = (6 \cdot 5)/2$ such relations, and he stated all of them. Kummer read Gauss's paper very carefully, even uncovering the existence of quadratic transformations from a list of expansions Gauss gave. He systematically studied the equation (1.4),

$$x(1-x)y'' + [c-(a+b+1)x]y' - aby = 0. \tag{1.4}$$

First he treated (1.4) with all three parameters free, then with one restriction when quadratic transformations exist, then with one free parameter when these transformations can be iterated, and finally in the confluent case of

$${}_1F_1\left(\begin{matrix}a\\c\end{matrix};x\right) = \lim_{b\to\infty}{}_2F_1\left(\begin{matrix}a,b\\c\end{matrix};\frac{x}{b}\right). \tag{1.5}$$

For historical accounts of hypergeometric series see [**5**, **7**, **12**].

Gauss treated theta functions and some special elliptic functions just as systematically, but he did not publish his work. Thus it was Abel and Jacobi who first published on elliptic functions in 1827 and 1828. This work was so striking, and so obviously important, that it is not surprising that many people studied these functions and wrote their own accounts of them. Eventually, so many results were found that it became useful to include a compilation of results as well as the formal development. For example, volume 4 of Tannery and Molk [**41**] contains more than seventy pages of formulas in addition to the more than sixty pages of formulas in volume 2 of [**41**].

Shortly after the turn of the century, E. T. Whittaker wrote *A Course in Modern Analysis* [**46**]. The second edition was coauthored with G. N. Watson [**47**], and with minor revisions this is still in print. This book has two parts. The first is a text in complex variables. The second is a treatment of the special functions that seemed important at the turn of the century. In the next section, I will contrast the treatment of special functions by Whittaker and Watson and that contained in *Higher Transcendental Functions* [**13, 14, 15**].

The first of the real handbooks was *Funktionentafeln mit Formeln und Kurven* by E. Jahnke and F. Emde [**20**]. This book has gone through many editions and is also still in print. In §3 this will be compared with [**1**], which was thought by the authors to be a modern version of Jahnke and Emde [**21**].

2. Whittaker and Watson and the Bateman Project

Harry Bateman was an English applied mathematician who spent most of his professional life in the United States, first at Bryn Mawr College, then five years at Johns Hopkins University, and finally at California Institute of Technology. Truesdell has written a very interesting account of Bateman's life and work [**44**]. The only aspect of his work that concerns us here is his work on special functions. In much of his work, Bateman regularly used special functions. He was a collector of facts about special functions and recorded useful facts about them on cards which he stored in shoe boxes. He had planned to write a many-volume work on special functions, treating their properties in many different ways. This project was so large that it did not really get started.

After Bateman's death, someone at California Institute of Technology approached E. T. Whittaker to ask for the recommendation of a person who could look at Bateman's cards and notebooks to see if they could be reworked for publication. Whittaker recommended a younger colleague, Arthur Erdélyi, who went to Pasadena for the year 1947–1948 to study the material. His conclusion was that it would be possible to write a series of useful books, but not on the scale proposed by Bateman. After a year in which he returned to Edinburgh, he returned to Cal. Tech. to head a large project which led to the publication of five books. Two of these were tables of integral transforms, and while this type of table is useful, none of the many that have been done have been very influential. The other three books, under the title *Higher Transcendental Functions*, have been very influential, so it is worthwhile considering their contents in some detail. To aid in this, the material will be compared with the material in the second half of Whittaker and Watson. To quote Erdélyi's first sentence in the first volume: "The work of which this book is the first volume might be described as an up-to-date

Arthur Erdélyi
ca. 1935
(Photograph courtesy of The Archives, California Institute of Technology.)

version of *Part II. The Transcendental Functions* of Whittaker and Watson's celebrated 'Modern Analysis'."

The chapters in Part II of Whittaker and Watson and the number of pages are:

	Chapter	Pages
XII	The Gamma Function	30
XIII	The Zeta Function of Riemann	16
XIV	The Hypergeometric Function	21
XV	Legendre Functions	35
XVI	The Confluent Hypergeometric Function	18
XVII	Bessel Functions	31
XVIII	The Equations of Mathematical Physics	18
XIX	Mathieu Functions	25
XX	Elliptic Functions. General Theorems and the Weierstrassian Functions	33
XXI	The Theta Functions	29
XXII	The Jacobian Elliptic Functions	45
XXIII	Ellipsoidal Harmonics and Lamé's Equation	43

Each of these chapters deals with a specific class of functions except for Chapter XVIII. This one primarily deals with Laplace's equation, and one of the main results is a proof of the addition formula for Legendre polynomials.

The general form of these chapters is the following. The specific functions being treated are introduced, and a systematic and careful treatment of some of their main properties is given. The chapter closes with a few references and a large number of problems. As many people have observed (but not in print as far as I know), most references to Whittaker and Watson are to a problem in one of these chapters. Some of these problems were Tripos problems, but most were taken from papers. The facts given in these problems are often very important. It is really this aspect of Whittaker and Watson, the listing of important facts, that Erdélyi and his coauthors use as a model for most of the chapters in *Higher Transcendental Functions* [**13, 14, 15**]. Also, most chapters have an outline of the development of the functions being treated, and many more references are given.

Here is a listing of the chapters and their lengths in these three books. The lengths of the corresponding chapters in these two works are not a good indicator of the amount of material contained in each. However the length of the treatment in each of these works is a good indication of the relative importance as seen in the 1910s and around 1950.

Higher Transcendental Functions

	Chapter	Pages
I	The Gamma Function	55
II	The Hypergeometric Function	64
III	Legendre Functions	62
IV	The Generalized Hypergeometric Series	20
V	Further Generalizations of the Hypergeometric Function	46
VI	Confluent Hypergeometric Functions	48
VII	Bessel Functions	48
VIII	Functions of the Parabolic Cylinder and of the Paraboloid of Revolution	18
IX	The Incomplete Gamma Functions and Related Functions	20
X	Orthogonal Polynomials	79
XI	Spherical and Hyperspherical Harmonic Polynomials	32
XII	Orthogonal Polynomials in Several Variables	30
XIII	Elliptic Functions and Integrals	90
XIV	Automorphic Functions	43
XV	Lamé Functions	47
XVI	Mathieu Functions, Spheroidal and Ellipsoidal Wave Functions	76
XVII	An Introduction to the Functions of Number Theory	39
XVIII	Miscellaneous Functions	20
XIX	Generating Functions	55

The last chapter was "based on an extensive list of generating functions compiled by the late Professor Harry Bateman" [**15**, p. 228], and so provides an indication of one type of book that Bateman had planned. I have owned this volume since its publication in 1955, and have never found this chapter particularly helpful. Not every topic can be appropriately treated in a handbook, and this chapter is a good illustration of one that does not work, at least the way it was organized here.

The most striking change from Whittaker and Watson to *Higher Transcendental Functions* is the greatly expanded treatment of hypergeometric functions. A hypergeometric series is a series

$$\sum_{n=0}^{\infty} c_n \tag{2.1}$$

with term ratio a rational function of n. Explicitly, this is usually taken as

$$\frac{c_{n+1}}{c_n} = \frac{(n+a_1)\cdots(n+a_p)x}{(n+b_1)\cdots(n+b_q)(n+1)} \tag{2.2}$$

and the series (2.1) is usually written as

$${}_pF_q\left(\begin{matrix}a_1,\dots,a_p\\ b_1,\dots,b_q\end{matrix};x\right) = \sum_{n=0}^{\infty} \frac{(a_1)_n\cdots(a_p)_n}{(b_1)_n\cdots(b_q)_n}\frac{x^n}{n!}. \tag{2.3}$$

A hypergeometric function is the analytic continuation of (2.3). Chapter II treats the case $p = 2$, $q = 1$. Legendre functions are the special case of this case when one of the parameters has been restricted so that a quadratic transformation exists. Confluent hypergeometric functions are the case $p = 1$, $q = 1$ or $p = 2$, $q = 0$. The case $p = 2$, $q = 0$ comes from a series that diverges, but there are integral representations that satisfy the appropriate differential equation, and are limits of the case $p = 2$, $q = 1$. Bessel functions come from $p = 0$, $q = 1$. Parabolic cylinder functions are sums of two confluent hypergeometric functions, and incomplete gamma functions are special cases of confluent hypergeometric functions, as is the error function. The chapters on orthogonal polynomials, spherical harmonics and orthogonal polynomials in several variables are also about hypergeometric functions. There are a few pages in the chapter on generalized hypergeometric series that deal with basic hypergeometric series, but the rest of this chapter and the chapter on further generalizations of the hypergeometric function deal with hypergeometric functions in one or several variables. Thus much more than half of these two books deals with hypergeometric functions. When one reads accounts of the development of special functions in books on the history of mathematics, one does not see the important role played by hypergeometric functions, and most mathematicians are unaware as well. Their importance was starting to be appreciated by the end of the last century. For example, in his 1893 lectures at Evanston, F. Klein [27] wrote:

> Next to the elementary transcendental functions the elliptic functions are usually regarded as the most important. There is, however, another class for which at least equal importance must be claimed on account of their numerous applications in astronomy and mathematical physics, these are the hypergeometric functions, so called owing to their connection with Gauss' hypergeometric series.

Klein was just referring to the case $p = 2$, $q = 1$. There are now many more applications of these functions in mathematics, and some other hypergeometric functions are also very useful. I will illustrate this by considering orthogonal polynomials. In Whittaker and Watson, the only orthogonal polynomials that are treated in detail are Legendre polynomials, with short sections on ultraspherical polynomials $C_n^{\nu}(x)$, Hermite polynomials which are not called by name and are denoted by $D_n(x)$, and one problem giving the interior asymptotics of Jacobi polynomials without mentioning their name. Jacobi polynomials are now given by

$$(2.4)\qquad P_n^{(\alpha,\beta)}(x) = \frac{(\alpha+1)_n}{n!}\,{}_2F_1\left(\begin{matrix}-n, n+\alpha+\beta+1\\ \alpha+1\end{matrix}; \frac{1-x}{2}\right)$$

and satisfy the orthogonality relation

$$(2.5)\qquad \int_{-1}^{1} P_n^{(\alpha,\beta)}(x)P_m^{(\alpha,\beta)}(x)(1-x)^\alpha(1+x)^\beta dx = 0, \quad m \neq n,\ \alpha,\beta > -1.$$

Legendre polynomials are the special case $\alpha = \beta = 0$, and ultraspherical polynomials are the polynomials when $\alpha = \beta = \nu - \frac{1}{2}$ after they have been renormalized. Hermite polynomials are the limiting case when $\alpha = \beta \to \infty$ after the change of variables $x \to x\alpha^{-1/2}$. They are orthogonal with respect to $\exp(-x^2)$ on $(-\infty, \infty)$.

All of these polynomials and Laguerre polynomials, which are orthogonal on $(0,\infty)$ with respect to $x^\alpha e^{-x}$, are the main polynomials treated in the chapter on orthogonal polynomials in *Higher Transcendental Functions.* The authors had Gabor Szegö's great book *Orthogonal Polynomials* **[37]** to draw on, and so they had the work of a real expert on this subject to use as Whittaker and Watson did not. However, the chapter on orthogonal polynomials contains information about some other sets of polynomials which the authors thought would be useful. They were right. To explain these polynomials, and why they thought they might be useful, here is a very brief account of the classical polynomials of Jacobi, Laguerre, and Hermite, and their discrete analogues.

Jacobi polynomials and their limiting cases of Laguerre and Hermite polynomials have a number of common properties. They satisfy second-order Sturm-Liouville differential equations of the form

$$(2.6)\qquad a(x)y'' + b(x)y' + \lambda_n y = 0, \qquad y = p_n(x)$$

where $a(x)$ and $b(x)$ are independent of n and λ_n is independent of x. The derivatives $q_n(x) = p'_{n+1}(x)$ are also a set of orthogonal polynomials. Finally, they satisfy a Rodrigues' type formula

$$(2.7)\qquad w(x)p_n(x) = K_n\frac{d^n}{dx^n}\{w(x)[A(x)]^n\}$$

where K_n is independent of x and $A(x)$ is a polynomial which is independent of n.

Each of these three properties along with orthogonality with respect to a positive measure can be shown to lead to the same polynomials, Jacobi, Laguerre, and Hermite, after a linear change of variable and renormalization. These facts are often taken to mean that these are the only orthogonal polynomials with enough structure to be really useful. However, discrete versions of these polynomials had been found, starting with a discrete extension of Legendre polynomials found by Tchebycheff [**42**]. The polynomials found by Tchebycheff and others can be represented by hypergeometric series with the polynomial variable now appearing in a parameter spot rather than as the power series variable. Here are the discrete polynomials known before 1940, and an orthogonality.

Hahn polynomials (discovered by Tchebycheff [**43**])

(2.8)
$$Q_n(x) = Q_n(x;\alpha,\beta,N) = {}_3F_2\left(\begin{matrix}-n, n+\alpha+\beta+1, -x\\ \alpha+1, -N\end{matrix};1\right), \quad x, n = 0, 1, \dots, N,$$
$$\sum_{x=0}^{N} Q_n(x)Q_m(x)\binom{x+\alpha}{x}\binom{N-x+\beta}{N-x} = 0, \qquad m \neq n \leq N,\ \alpha, \beta > -1.$$

Meixner polynomials

(2.9)
$$M_n(x) = M_n(x;\beta,c) = {}_2F_1\left(\begin{matrix}-n, -x\\ \beta\end{matrix};1-c^{-1}\right), \quad \beta > 0,\ 0 < c < 1,$$
$$\sum_{x=0}^{\infty} M_n(x)M_k(x)\frac{(\beta)_x}{x!}c^x = 0, \quad k \neq n.$$

Krawtchouk polynomials

(2.10)
$$K_n(x) = K_n(x;p,N) = {}_2F_1\left(\begin{matrix}-n, -x\\ -N\end{matrix};\frac{1}{p}\right), \qquad n, x = 0, 1, \dots, N,\ 0 < p < 1,$$
$$\sum_{x=0}^{N} K_m(x)K_n(x)\binom{N}{x}p^x(1-p)^{N-x} = 0, \qquad m \neq n \leq N.$$

Charlier polynomials

(2.11)
$$C_n(x) = C_n(x;a) = {}_2F_0\left(\begin{matrix}-n, -x\\ -\end{matrix};-\frac{1}{a}\right), \quad a > 0,$$
$$\sum_{x=0}^{\infty} C_n(x)C_m(x)\frac{a^x}{x!} = 0, \quad m \neq n.$$

The analogue of the derivative is

(2.12)
$$\Delta f(x) = f(x+1) - f(x).$$

Then

(2.13)
$$\Delta Q_n(x;\alpha,\beta,N) = \frac{-n(n+\alpha+\beta+1)}{N(\alpha+1)}Q_n(x;\alpha+1,\beta+1,N-1)$$

is an analogue of

(2.14)
$$\frac{d}{dx}P_n^{(\alpha,\beta)}(x) = \frac{n+\alpha+\beta+1}{2}P_{n-1}^{(\alpha+1,\beta+1)}(x).$$

Chapter 10 in [14] contains enough on most of these polynomials so that people who looked for information about one of these polynomials would probably become aware that others had studied them before. In particular, the three term recurrence relation which all orthogonal polynomials satisfy was included for each of these polynomials except for the general Hahn polynomials, where it was given only in the case $\alpha = \beta = 0$. For the general Hahn polynomials, the discrete Rodrigues' formula was given, as was an explicit formula and a reference to a paper that contained the recurrence relation. The orthogonality was given in a slightly too general form, since the claimed measure only has finitely many moments.

As an illustration that enough was included so that people became aware of these polynomials, one only needs to look at some papers of Karlin and McGregor. See [23] and [24], and also the long and impressive paper of Karlin and Szegö [25]. Of course Szegö had been aware of most of these polynomials, although he seems not to have known that Tchebycheff found the general Hahn polynomials as well as the special case when $\alpha = \beta = 0$ which Szegö mentioned in [37]. See [2] and my comments in [40, pp. 866–869] for more information and references about these polynomials.

Markoff found another discrete analogue of Legendre polynomials, and there were other polynomials of a related nature found by Stieltjes, Wigert and Geronimus. In 1949, W. Hahn [18] found a wider class of orthogonal polynomials where theorems like those mentioned above hold. He used the q-difference operator Δ_q defined by

$$\Delta_q f(x) = \frac{f(x) - f(qx)}{(1-q)x}. \tag{2.15}$$

He found all sets of orthogonal polynomials $p_n(x)$ for which

$$r_n(x) = \Delta_q p_{n+1}(x) \tag{2.16}$$

is a set of orthogonal polynomials. These polynomials are basic hypergeometric analogues of all the polynomials mentioned above.

A basic hypergeometric series is a series $\sum c_n$ with c_{n+1}/c_n a rational function of q^n for a fixed number q. There are two choices of q that occur most frequently, $|q| < 1$ and q an integer power of a prime.

The most general set of polynomials in the class of Hahn is

$$Q_n(x; a, b, c) = \sum_{k=0}^{n} \frac{(q^{-n}; q)_k (q^{n+1}ab; q)_k (x; q)_k}{(aq; q)_k (cq; q)_k (q; q)_k} q^k \tag{2.17}$$

where

$$(a; q)_k = \prod_{j=0}^{k-1} (1 - aq^j). \tag{2.18}$$

Hahn found the second order q-difference equation all the polynomials satisfy, but only worked out the orthogonality for the polynomials

$$p_n(x;a,b:q) = \sum_{k=0}^{n} \frac{(q^{-n};q)_k(q^{n+1}ab;q)_k}{(qa;q)_k(q;q)_k}(qx)^k. \tag{2.19}$$

There are uses of these polynomials that are quite important. Here are two types of uses that are not mentioned in *Higher Transcendental Functions* with the exception of the classical case of symmetric Jacobi polynomials arising in the study of spherical harmonics, which occurs in Chapter XI.

Let S^{n-1} be the surface of the unit ball in R^n. Consider the functions

$$u(x_1,\dots,x_n) = r^k U\left(\frac{x_1}{r},\dots,\frac{x_n}{r}\right)$$

where u is harmonic in R^n and $r = (x_1^2 + \cdots + x_n^2)^{1/2}$. Thus U is a function defined on the unit sphere. If u is a polynomial of degree k, then U is called a spherical harmonic. When $n = 2$ and $k \geq 1$, there are two linearly independent choices for U, say $\cos k\theta$ and $\sin k\theta$. When $n = 3$, there are $(2k+1)$ linearly independent functions U, one being $P_k^{(0,0)}(\cos\theta) = P_k(\cos\theta)$ and the others being functions of two variables which are a product of a function that is the jth derivative of $P_k(x)$ times an algebraic function of x, $j = 1,2,\dots,k$ and then times two linearly independent functions of one variable which arise in the same way on the unit circle. This pattern continues for spheres of all dimensions, and a nice outline is given in Chapter XI. The zonal spherical harmonic, or spherical function, is the symmetric Jacobi polynomial $P_k^{((n-3)/2,(n-3)/2)}(\cos\theta)$.

The essential property that makes this work is that the sphere is a two-point homogeneous space. On the sphere there is a metric which is invariant under the rotation group, and given two points on the sphere with distance d between them, and a second pair also separated by distance d, there is an element of the rotation group that takes the first pair to the second. There are other compact connected two-point homogeneous spaces, and on each it is possible to construct similar zonal harmonics. These spaces are real, complex, and quaternionic projective spaces, and a two-dimensional projective space over the Cayley numbers. These spaces go back to work in the last century, primarily by E. Cartan. The theorem that there are no others was proved by Wang. In 1929, E. Cartan [**8**] found the zonal spherical functions on complex projective spaces. His spherical functions are the Jacobi polynomials $P_k^{(n,0)}(x)$, $k = 0,1,\dots,$ when the space is $(n+2)$-dimensional complex projective space. This was not widely known since most people who knew something about Jacobi polynomials knew little about complex projective spaces, and those who knew Cartan's work knew very little about hypergeometric functions and Jacobi polynomials. That situation has changed, and there are now a number of people who are comfortable with both of these.

One thing this has led to is an addition formula for Jacobi polynomials. I used to say that it was about ninety years between discovery of the addition formula for Legendre polynomials and Gegenbauer's discovery of the addition formula for ultraspherical polynomials, and about another ninety years until the discovery of the addition formula for general Jacobi polynomials, so one could guess how long it would be before another addition formula was found. The obvious guess was far too large, for it was less than ten years from the Jacobi case, which was found by Šapiro **[35]** in the case $(\alpha, \beta) = (\alpha, 0)$, and by Koornwinder **[28]** independently in this case and then extended to the general case, and when Dunkl **[11]** found the addition formula for symmetric Krawtchouk polynomials. He was able to do this because there are other important compact two-point homogeneous spaces, and they have spherical functions that not only can be found, but they are orthogonal polynomials and can be found among the polynomials outlined above. The appropriate space for the symmetric Krawtchouk polynomials is the unit cube in R^N, i.e., the space of sequences $(\varepsilon_1, \varepsilon_2, \ldots, \varepsilon_N)$ with $\varepsilon_i = 0$ or 1, and the distance to $(0, 0, \ldots, 0)$ is defined to be the number of 1's. This is called the Hamming distance, and this space is the natural setting to start the study of coding theory. It took many years before people developing coding theory realized that the polynomials they had constructed were not new, but once this realization was made then it was only a short while before the analogy with Legendre and Laplace's work on spherical harmonics was recognized and used as a model of what to look for. Next, this was done for other discrete two-point homogeneous spaces, including a finite field version of some of the spaces, so that many Krawtchouk, Hahn, q-Krawtchouk, and q-Hahn polynomials arise in this way. See Stanton **[36]** for a nice survey and many references.

There is a second way in which Legendre polynomials arise in a group setting. They are elements of matrix representations of $SU(2)$. Jacobi polynomials and Krawtchouk polynomials also arise in this fashion. When the tensor product of two representations of $SU(2)$ are decomposed, the coefficients can be written as a ${}_3F_2$ times a more elementary function, and this ${}_3F_2$ can be transformed to be either a Hahn polynomial or a dual Hahn polynomial (the polynomials that arise when n and x are interchanged). In physics, these coefficients are called $3 - j$ symbols, Clebsch-Gordan coefficients, or Wigner coefficients. The $6 - j$ symbols, or Racah coefficients, can also be transformed to be orthogonal polynomials times a more elementary function, and these polynomials fit into a slightly wider class of classical type orthogonal polynomials. Instead of using the finite difference operators Δ or Δ_q, divided difference operators need to be used. The resulting orthogonal polynomials are either ${}_4F_3$'s or ${}_4\phi_3$'s or special or limiting cases of them. In particular, all the polynomials mentioned above and some ${}_2F_1$'s and ${}_3F_2$'s not mentioned are also in this wider class of classical polynomials.

This is not the end, for recently a Hopf algebra extension of $SU(2)$ has been discovered. It is called a quantum group and is denoted by $SU_q(2)$. The matrix representations have elements that are Hahn's q-extension of Legendre and Jacobi polynomials, and the Clebsch-Gordan and Racah coefficients are basic hypergeometric extensions of the classical results. Koornwinder has used this setting to obtain an addition formula for the q-Legendre polynomials of Hahn. See [29] and references in this paper. Drinfield [**10**] and Jimbo [22] discovered quantum groups, and Woronowicz [**48, 49**] was the first to realize that one could compute explicitly in $SU_q(2)$.

I have outlined this material to point out what has to be done to write a really useful handbook. One needs to know the applications of the past, so that the formulas that were useful in the past are contained. They will probably be useful in the future. However, to make the book more useful, one has to include some results that have not been used yet, or have not been used very much yet. Erdélyi and his coauthors, W. Magnus, F. Oberhettinger, and F. Tricomi, did a better job of predicting the future than Whittaker and Watson did, but then their aims were different. Whittaker and Watson were writing a text, and while not everyone appreciates it [**19**], there are many others who learned analysis from it and have gone on to make many important discoveries. However, one needs to realize that the second half of Whittaker and Watson is the part that was closest to both Whittaker and Watson's main interests, and so it is the part where their detailed knowledge shows to best advantage.

Szegö [**39**] wrote a review of [**13**] and [**14**]. After starting with the sentence: "These two volumes compiled by the Bateman Manuscript Project represent a stupendous accomplishment," he went on to write:

> The difficulties of such a compilation as was planned by Bateman and is carried out in the present work are enormous. They are due not so much to the vastness of the pertinent material but rather to the intrinsic difficulty of formulating and following clear and consistent principles in organizing it.

Szegö also mentioned that it seemed likely that Erdélyi played a central role in these books, even though Erdélyi's Foreword is modestly silent about Erdélyi's share. I wrote a comment about this work as an addendum to an article on Erdélyi written by A. G. Mackie [**30**]. I sent a copy to W. Magnus for comments. His reply included the following:

> You are absolutely right in assuming that Erdélyi was not only the editor but simply the soul of the Bateman Project. Nevertheless he never interfered directly with the work of his coauthors. He simply put in the final touches where necessary. May I add that it was Erdélyi's idea to include functions of number theory

> and automorphic functions into the Project. I wrote these chapters upon his request — but it would not have occurred to me to include them. What you call Erdélyi's foresight was, in part, his strong sense of responsibility. He knew that the Project was a rare opportunity to serve the mathematical community, and he took his task very seriously.

In a number of articles in these three volumes, *A Century of Mathematics in America*, there have been comments about the supremacy of mathematics in the United States. One should remember that there were areas where knowledge in the United States was not very strong, even with the many immigrants. Special functions was one. Erdélyi was brought over from Scotland. In 1943, Magnus and Oberhettinger had written a smaller handbook of special functions [33] which was a transitional book from Whittaker and Watson to *Higher Transcendental Functions*. They were brought from Germany. Tricomi came over from Italy. He was the only one to return to Europe at the end of the Bateman Project, although Erdélyi eventually returned to Edinburgh. Mathematics is such a broad field, and certain traditions are probably absent in every country, so it is not surprising that there are gaps in the detailed knowledge of some parts of mathematics in every country. In particular, consider multiple hypergeometric functions. In the United States, the most important work was started by some physicists. In particular, the work of Biedenharn and Louck and various coworkers is important. See [6] for references. Eventually some mathematicians started to work in this area. See Milne [34] and Gustafson [17]. These papers deal with well poised multiple hypergeometric series and other series with a lot of structure. They arise from various groups. In England, Ian Macdonald has used affine Lie algebras to discover some important multiple theta functions [31], and some multidimensional beta integrals [32]. In Japan, Aomoto has introduced some important multiple hypergeometric integrals [3]. There is a lot of other work on multiple hypergeometric functions in Japan, most of it connected with partial differential equations. Surprisingly, as far as I know, the only course on multiple hypergeometric functions given in the United States in at least twenty years was given by a Japanese mathematician who was a visiting professor at the University of Minnesota. See [26] for the notes on this course.

In the Soviet Union, Gelfand and some coworkers have introduced a more general class of multiple hypergeometric integrals via integrals over Grassmannians. See [16] and many later papers.

There is a connection between Macdonald's work and some of the work in the United States, but otherwise, except for one important paper of Aomoto [4], the groups in Japan, the United States, and in USSR are working independently, and no group really understands what the others are doing. There are some young people in the Netherlands starting to work in a different way. Each group knows the reasons why they care about their work and is usually ignorant about the reasons behind the other work. All of this work is very important, but I am the first to say that I understand little of it. The next generation of handbooks will be much richer because of it, and also because of all the work on basic hypergeometric functions. Much of the work on basic series goes back to work done by Ramanujan and earlier by Rogers. A year and a half ago, Gelfand told me he expects work on q-problems (including q-series) to be at the center of mathematics in fifteen years. He is working hard to try to learn it. His statement is probably too strong, but this area is clearly much more important than Erdélyi and his coworkers thought that it would be. However, they included an outline of Hahn's work, and that was what led me to be interested in this topic.

My copies of **[13]** and **[14]** have had to be rebound, which is one of the strongest statements I can make about how useful I have found them. They are not perfect, but they were a significant improvement on what was done before. This was partly because of added knowledge, and it was partly the decision of the authors to include material they thought would be useful in the future. Their guesses about what would be useful were quite good, given the few hints they had to work with.

3. Jahnke and Emde and the Bureau of Standards Handbook of Mathematical Functions

Jahnke and Emde wrote the first handbook of special functions. It was written for a different group of users than *Higher Transcendental Functions.* This is best seen by looking at the book and seeing the numerical tables and graphs. The lack of graphs is a weakness in **[13, 14, 15]**, but there were other books to use for numerical data. That was much less true in 1909, so it was worthwhile using half the space for numerical tables and another fifteen percent for graphs. This left only slightly more than one third of the 174 pages for text and formulas. However, the graphs have led to one very nice theorem which mathematicians discovered and proved years before it would be rediscovered by a couple of physicists. In both the first edition **[20]** and in the one which was widely available in the United States after 1945 **[21]**, there is the following picture of Legendre polynomials. See Figure 26 on page 82 in **[20]** and Figure 64 on page 118 in **[21]**

This graph has been recomputed by Paul Nevai using Mathematica. Nevai's graph is given in Figure 1. Almost forty years after the first publication, J. Todd looked at this graph carefully and suggested that the kth maximum of $|P_n(x)|$ is a decreasing function of n. Here the maximums are counted from the right. This was proved by G. Szegö **[38]**, and many years

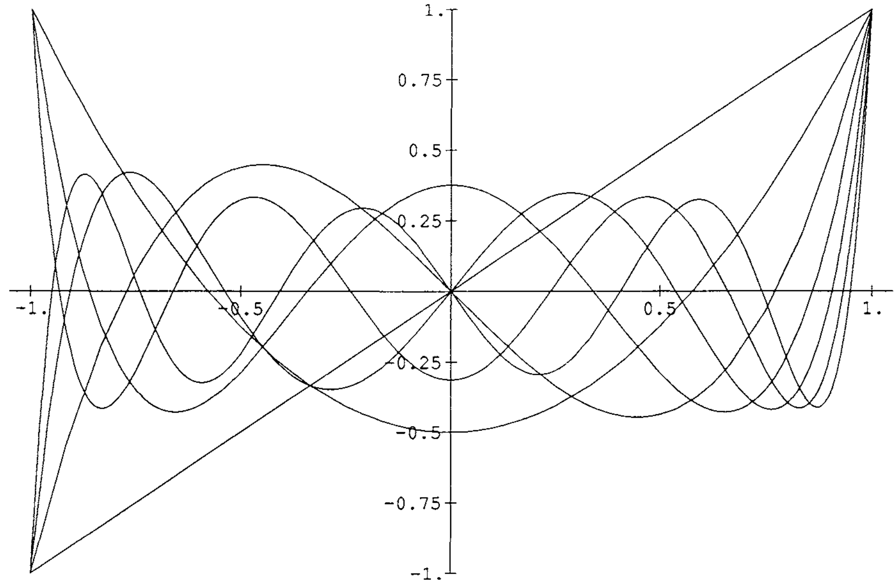

FIGURE 1. Graph of $P_n(x)$, $n = 1, 2, \ldots, 7$.

later Corneille and Martin [9] rediscovered this set of inequalities. It is reassuring that eventually someone looked seriously at this graph, but dismaying that it took almost forty years. Once Szegö proved Todd's conjecture, Todd and Szász obtained similar results for related functions. About twenty years ago, I tried to extend these results to the Jacobi polynomials

$$\frac{P_n^{(\alpha,\beta)}(x)}{P_n^{(\alpha,\beta)}(1)}.$$

Since these inequalities were known for $\alpha = \beta \geq -\frac{1}{2}$ and for $\alpha \geq \beta = -\frac{1}{2}$, it seemed clear they should be true for $\alpha > \beta > -\frac{1}{2}$. That is still unproven. The case $\beta = -1$, $\alpha = 0$ turns out to be very interesting. First

$$\frac{P_n^{(0,-1)}(x)}{P_n^{(0,-1)}(1)} = \frac{P_n^{(0,0)}(x) + P_{n-1}^{(0,0)}(x)}{2},$$

so it is just the average of two adjacent Legendre polynomials. Second, a graph of these functions up to $n = 7$ suggests that the monotonicity in Figure 1 is reversed. This partly explains why the cases when $-\frac{1}{2} < \beta < \alpha$ are so hard. The corresponding graph, again computed by Nevai, is in Figure 2.

With this as background, consider the graphs contained in the chapter on orthogonal polynomials in [1]. Figures 22.4 and 22.8 seem similar. In fact, they are identical. In Figures 22.2 and 22.3, it is very hard to see the monotonicity of the zeros as a function of the varying parameter α or β. This can be seen in Figure 22.5. I find it impossible to determine even by looking at

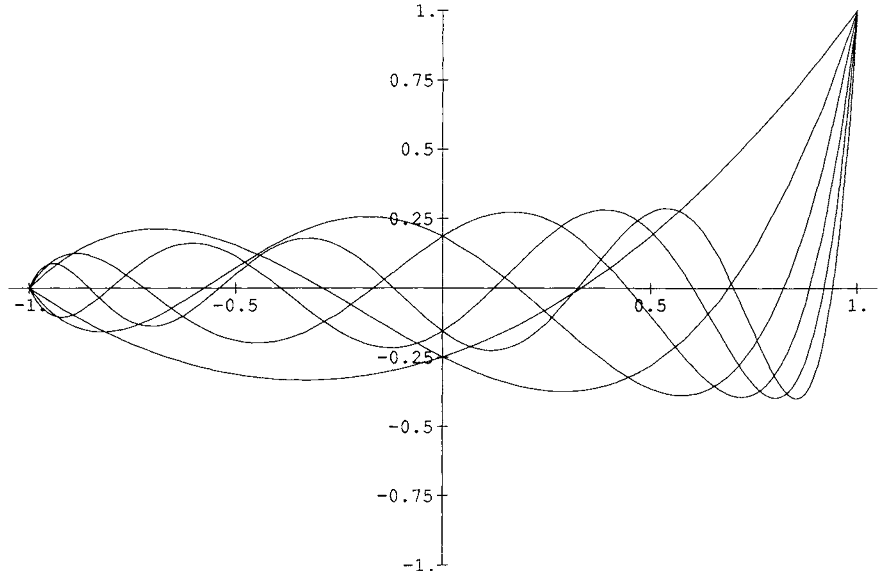

FIGURE 2. $\frac{P_n(x)+P_{n-1}(x)}{2}$, $n = 2, 3, \ldots, 7$.

the graphs that the left minimum values in Figures 22.2 and 22.3 are negative, as they are. The rest of the graphs are reasonable, although I suspect that Figure 22.5 would have been more informative qualitatively if the polynomials had been normalized to be 1 at $x = 1$ and only the right-hand side had been printed, which is all that is necessary by symmetry. Graphs are very useful to give qualitative information, but much less useful for quantitative information.

The chapter on orthogonal polynomials in [**1**] was written by someone who was not an expert on them, and it shows. The chapter starts with the definition of orthogonal polynomials but restricted to an absolutely continuous measure. Then there is the sentence:

> These polynomials satisfy a number of relationships of the same general form.

Four are listed, a differential equation, a three-term recurrence relation, a Rodrigues' formula, and their derivatives forming an orthogonal set. Unfortunately, only one of these holds for general orthogonal polynomials, the three-term recurrence relation. The others hold only for Jacobi, Laguerre, and Hermite polynomials, as was stated in §2.

There are a number of problems with the list of formulas. Some are incorrect; others are stated in a way that is inappropriate; and others are not interesting enough to justify space in a book where important results were omitted because of a lack of space. Here is one of each type. Formula 22.13.5 is

$$\int_{-1}^{1}(1-x^2)^{-1/2}P_n(x)dx = \frac{2^{3/2}}{2n+1}.$$

This is clearly wrong, since $P_n(x)$ satisfies

$$P_n(-x) = (-1)^n P_n(x),$$

and thus the integral vanishes when n is odd. When n is even, the integral is

$$\int_{-1}^{1}(1-x^2)^{-1/2}P_{2n}(x)dx = \left[\frac{\Gamma(n+\frac{1}{2})}{\Gamma(n+1)}\right]^2.$$

The correct form of 22.13.5 is

$$\int_{-1}^{1}(1-x)^{-1/2}P_n(x)dx = \frac{2^{3/2}}{2n+1}.$$

Formula 22.13.1 is given as

$$2n\int_0^x (1-y)^\alpha(1+y)^\beta P_n^{(\alpha,\beta)}(y)dy$$
$$= P_{n-1}^{(\alpha+1,\beta+1)}(0) - (1-x)^{\alpha+1}(1+x)^{\beta+1}P_{n-1}^{(\alpha+1,\beta+1)}(x).$$

It should be given as

$$2n\int_x^1 (1-y)^\alpha(1+y)^\beta P_n^{(\alpha,\beta)}(y)dy = (1-x)^{\alpha+1}(1+x)^{\beta+1}P_{n-1}^{(\alpha+1,\beta+1)}(x)$$

since this is more compact, easily implies the stated formula, and $P_n^{(\alpha,\beta)}(0)$ can be summed only when $\alpha = \beta$ or $\alpha + \beta = 0$. Thus one does not want to use $P_n^{(\alpha,\beta)}(0)$ unless one has to. Unfortunately, this formula first appeared in **[14]**.

Finally, 22.14.10 is

$$P_n^2(x) - P_{n-1}(x)P_{n+1}(x) < \frac{2n+1}{3n(n+1)}, \qquad -1 \le x \le 1.$$

The real interest in inequalities about $P_n^2(x) - P_{n-1}(x)P_{n+1}(x)$ is Turán's inequality

$$P_n^2(x) - P_{n-1}(x)P_{n+1}(x) > 0, \qquad -1 < x < 1$$

and extensions of it. From asymptotics it is easy to see that

$$P_n^2(x) - P_{n-1}(x)P_{n+1}(x) = O(n^{-1}),$$

and a more exact upper bound is unlikely to be useful unless it is a sharp bound, and even then I doubt it will be useful. The asymptotic behavior is of interest, but that is easy to determine from asymptotics of the separate terms.

There is a simple test one can use to check the quality of a chapter on the classical orthogonal polynomials. The ultraspherical polynomials can be defined by

$$(1-2xr+r^2)^{-\lambda} = \sum_{n=0}^{\infty} C_n^\lambda(x) r^n.$$

This reduces to $1 = 1$ when $\lambda = 0$, so

$$C_n^0(x) = 0, \qquad n = 1, 2, \ldots .$$

Here is how Szegö **[37]** handled the case $\lambda = 0$. He wrote:

$$\text{(4.7.8)} \qquad \lim_{\lambda \to 0} \frac{C_n^\lambda(x)}{\lambda} = T_n(x), \quad n = 1, 2, \ldots ,$$

where

$$T_n(\cos\theta) = \cos n\theta.$$

He does not introduce $C_n^0(x)$.

In *Higher Transcendental Functions* the solution is as follows, skipping irrelevant material. The authors start with:

$$\text{(3)} \qquad C_n^\lambda(1) = \frac{(2\lambda)_n}{n!}.$$

The standardization (3) fails when 2λ is zero or a negative integer. The only exception in the range $\lambda > -\frac{1}{2}$ is $\lambda = 0$, and for this we standardize according to

$$\text{(5)} \qquad C_0^0(1) = 1, \quad C_n^0(1) = \frac{2}{n}$$

and we have

$$\text{(6)} \qquad C_n^0(x) = \lim_{\lambda \to 0} \lambda^{-1} C_n^\lambda(x).$$

In **[1]**, the solution is

$$\text{(22.3.14)} \qquad C_n^0(\cos\theta) = \frac{2}{n} \cos n\theta.$$

I think Szegö's solution is better than either of the others, since there is no reason to use $C_n^0(x)$, and it is probably confusing to introduce it with a different normalization. Actually, I like

$$\lim_{\lambda \to 0} \frac{C_n^\lambda(x)}{C_n^\lambda(1)} = T_n(x), \qquad n = 0, 1, \ldots$$

or

$$\lim\nolimits_{\lambda \to 0} \tfrac{n+\lambda}{\lambda} C_n^\lambda(x) = 1, \quad n = 0,$$

$$= 2T_n(x), \quad n = 1, 2, \ldots ,$$

better than Szegö's limit (4.7.8), but this is a matter of preference. The worst solution was formula 22.3.14 in **[1]**.

Some of the chapters in [**1**] are first rate, while others are poor. The problem is partly the choice of some of the authors, but more the absence of someone to direct the whole project who had the wide and detailed knowledge of Arthur Erdélyi. This book has been a best seller, both for the U. S. Federal Government, which is surprising considering their poor distribution system (the very low price led to the high sales), and also in the paperback edition published by Dover. It is a shame that the quality was not uniformly high as, for example, it was in Olver's chapter on Bessel functions.

4. Summary

In his article on Bateman [**44**, p. 429], Truesdell comes close to asserting that Erdélyi and his coworkers made an error in writing a handbook rather than a treatise. I agree completely with Truesdell when he laments the loss of Bateman's cards from the famous shoeboxes, but disagree with him about the relative importance of handbooks and treatises. Both handbooks and treatises are needed, and treatises are usually more restricted in topic, and so are easier to write. However, they are not easy to write, or we would have more good ones, but the same goes for good handbooks. I use special functions in most of my mathematics, and a fairly large percentage of my work is directly on special functions themselves. Of necessity, I have a fairly good knowledge of what exists in both the systematic treatises of special functions and the best handbooks. My copies of Szegö's *Orthogonal Polynomials* and Watson's *Bessel Functions* have had to be rebound, just as have the first two volumes of *Higher Transcendental Functions*, as was mentioned earlier. These two types of books serve different purposes even for a heavy user and not just for an occasional user. Once the amount of useful knowledge becomes so large that one cannot remember it all, or even remember where it is located, then it is necessary to have help in trying to find the useful facts one needs. Handbooks are one solution. There is talk about trying to make all this material available in a large computer system. This would be very useful, but it should not be the only source. Paper in books often becomes brittle and information is lost as the book disintegrates. The rate of disintegration of computer systems will almost surely be much faster than that of paper since systems change so rapidly. So for the foreseeable future, handbooks of special functions will be useful, and because of scientific and mathematical developments, they need to be redone every so often. Thirty to fifty years is probably the right time interval, so it is time to consider what we can do to help make this useful information more accessible to the mathematical and scientific communities.

George Andrews made another comment about computers versus books. Books permit easier browsing and so more easily lead to unexpected discoveries, interactions, and comparisons. I agree completely.

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Commemorative Meeting for Alfred Tarski
Stanford University—November 7, 1983

PATRICK SUPPES, JON BARWISE,
AND SOLOMON FEFERMAN, SPEAKERS

INTRODUCTION

The meeting was held in memory of Professor Alfred Tarski, who died at the age of 82 on October 28, 1983. Tarski was one of the most important logicians of the twentieth century and his influence stretched over a period of more than fifty years. He established his reputation in the latter part of the

Alfred Tarski
(Photograph by Steve Givant, 1972.)

1920s through the 1930s at the University of Warsaw, came to the United States in 1939, and obtained a position in the Department of Mathematics at U.C. Berkeley in 1942, where he became a professor of mathematics in 1946. At Berkeley, Tarski established a leading center for the research and teaching of mathematical logic. Commemorative meetings were held not long after his death at both Stanford and Berkeley. The meeting at Stanford was sponsored by the Departments of Mathematics and Philosophy, where it was chaired by Professor Solomon Feferman, a former student of Tarski and a member of both departments. After some brief opening remarks, he introduced Patrick Suppes, Professor of Philosophy and Statistics, and Director of the Institute for Mathematical Studies in the Social Sciences, who told of his long experience with Tarski as a friend and colleague; Professor Suppes' remarks, which emphasized Tarski's personal characteristics and scientific style, are summarized below. He was followed by Professor Jon Barwise of the Department of Philosophy and (then) Director of the Center for the Study of Language and Information, who spoke about Tarski's work on the theory of models and model-theoretic semantics, which has been important in logic, philosophy, and linguistics. Feferman concluded the meeting with reminiscences about his experiences in the 1950s as a student of Tarski, and he enlarged on the topics of Tarski's work and interests. The talks of Barwise and Feferman are reproduced essentially as presented, though edited for this publication.

Patrick Suppes [Summary]

Sitting in on Tarski's seminars in the 1950s was a vicarious learning experience. Tarski had a passion for clarity, and he would halt seminar reports by students if at any point they failed to meet his standards. He would not let them proceed until they could present the material in a completely satisfactory, clear, and exact form; this could be very painful, though it was never a personal matter, and most students benefited by the experience.

Also to be emphasized is the elegance of his thought and talk, the strongly aesthetic feelings that came forth in his presentations and writings. He was really a dazzling lecturer and could explain technical subjects to wide audiences in a very clear and accessible way, starting with very simple ideas, gradually building up a full picture. His papers seem much simpler than they really are, because of his passion for organization and clarity. English was Tarski's fifth language (actually seventh, if one counts Greek and Latin); in Russian-occupied Poland he had studied in Russian at the classic *Gymnasium*, and also gained a command of French and German at the same time. His prewar papers were in Polish, French, and German, while he published entirely in English after his emigration to the United States. Tarski had the unusual ability to both lecture and write superbly well. He was also a pleasure to talk to in person because of the breadth of his interests, not only in mathematics, but also in philosophy, art, literature, and politics; his ideas were always well developed and articulated.

The final point to be emphasized—and which accounts for so much of Tarski's influence—was his unflagging energy and enthusiasm for work and ideas. He communicated this to his students, along with a positive attitude in stimulating a large body of work. Many of his students have gone on to become well-known in their own right, working in a variety of directions, but all carrying the distinctive stamp of Tarski's concerns for clarity and organization.

Jon Barwise

I first met Alfred Tarski in the summer of 1963. I was just on my way to graduate school here at Stanford, and was lucky enough to be able to attend the Berkeley International Model Theory Symposium. That symposium was quite an eye-opener for me. As an undergraduate I had been brought up on one view of logic; at the Berkeley symposium I found something dramatically different—namely, Tarski's kind of logic.

I met Tarski at the symposium and heard him lecture there. In fact, that was the only lecture I ever heard him give. I talked to him perhaps a half dozen times over the years, for about five minutes each. I'm far from an expert on Alfred Tarski, either personally or in terms of any kind of scholarly knowledge of his work, so I felt somewhat overwhelmed when I was asked to speak here. But upon reflection I realized that Alfred had an enormous impact on me and on my whole generation of logicians, and that this impact should not go unacknowledged here. Tarski's view of logic has changed the way all of us think about the subject. Indeed, together with Kurt Gödel and Steve Kleene, Tarski was one of the founders of modern mathematical logic. And Tarski is the person who turned model theory into a theory.

The term "mathematical logic" is rather confusing. Sometimes it is used to mean the logic of mathematics, that is, the logic of mathematical activity. Another name for this subject is "meta-mathematics." But "mathematical logic" can also be used to refer to a branch of applied mathematics—the use of mathematical tools to study logic per se, say as it arises in computer science or wherever. Then, of course, you can combine the two meanings and look at using the tools of mathematics to study the logic of mathematical activity itself. That, in fact, is the way the phrase is most commonly used: most mathematical logic consists in the use of mathematical tools to study the logic of mathematical activity. And it's that subject that I think Gödel, Kleene, and Tarski created. Gödel, working largely in isolation, made his contribution through a relatively small number of unquestionably seminal papers, papers which laid the foundations for the whole enterprise. Logicians just didn't think about things the same way after these papers. Kleene and Tarski made their contributions in other ways.

In thinking about the contributions of Kleene and Tarski, it seems to me that there is a remarkable parallel between the two men. But I would like to mention three or four aspects of this parallel.

The first is their concern for, and contributions to, conceptual analysis, that is, to the mathematical analysis of some given intuitive concept. Tarski's main tool was set theory. As an example of his contributions to conceptual analysis, probably the most famous is his work on the notion of truth. In this work, Tarski is a crucial link between logic before the thirties, and modern logic. The older logic focused on "logical systems," axioms and rules of proof. It was clear in much of the work that axioms and rules of proof were about something—that is, they were about mathematical objects. The axioms were supposed to be "true" and the rules of proof were supposed to preserve "truth." But it was Tarski who singled out this notion of truth and gave it a mathematical formulation in its own right.

It is impossible to mention Tarski's work on truth without mentioning Gödel's. For Gödel's work on the Completeness Theorem was also part of the bridge to the past. But, Gödel used the notion of truth implicitly. Tarski pulled the notion up out of the background and made it a core notion in mathematical logic.

You see, in mathematics there is the idea that a given mathematical discourse is about some particular mathematical domain, not about everything there is. Typically it is about something like the natural numbers, or the real numbers, or the elements of some field. Bringing out these domains, isolating them as objects in their own right, and developing the notion of truth in a domain, is what Tarski accomplished in his analysis.

This is the basis of model theory. Only after this piece of work can one ask the kinds of questions that Tarski asked, questions about cardinalities of models of some theory (for example, the Löwenheim-Skolem-Tarski Theorem), or about preservation of truth between different models of some theory (for example, the Łos-Tarski Theorem). It's only after you make the notion of domain and truth precise that there is any hope of proving such results. Thanks to Tarski's work on conceptual clarification, notions and results which were once very confusing to think about have achieved the ultimate compliment: they are either proved or assigned as homework exercises in every course on model theory.

This element of conceptual analysis in Tarski's work is one of the first contributions toward making any theory a branch of mathematics. The second, of course, is asking the right questions and getting answers, that is, proving results. I've mentioned two of Tarski's important results already, results which are basic tools in the tool box of any logician. But these are just two of a host of theorems due to Alfred Tarski, theorems in all parts of mathematical logic. For example, parallel to the definition of truth is his theorem on the Undefinability of Truth. On the one hand, he shows how to define truth for a mathematical structure by stepping outside that structure. But he also shows that if you try to have a single universe of mathematics, where things are either true or not, then the theory will not be adequate for defining its own notion of truth. These are two sides of truth, both of which Tarski helped explicate, through conceptual analysis, on the one hand, and through an important theorem, on the other.

Besides their work on conceptual analysis, and on proving theorems, there is a third and equally important aspect to the work of Tarski and Kleene, something that sets them apart from their contemporaries in logic. Each of them built up a school, in two senses of the word: a school of mathematical thought, and a center of research in logic. In Tarski's case, the former is model theory, the latter is the school of mathematical logic at the University of California at Berkeley. (For Kleene, it was recursion theory and logic at the University of Wisconsin.) Tarski's influence on both of those is enormous. If you look at a list of Tarski's students (which number more than 20), their students, and their students' students, and so on, you'll find an enormous number of the currently practicing logicians. And if you look across the bay at Berkeley itself, you'll find the logic group he founded still flourishing; indeed, its leadership in model theory and set theory has never been in serious jeopardy.

What made Tarski so special? What led him to become one of the three founders of modern logic? I think three things: his work on conceptual analysis, his asking (and often answering) the right mathematical questions once notions were precise, and his unselfish dedication to building up a school of logic. Lurking behind all of these we find the same thing, and another trait shared with Kleene: a boundless passion for mathematical clarity and rigor, both in his own work and of those around him. If there is anything he would want to pass on to those who follow, I think it would be that passion.

Solomon Feferman

I'll conclude this meeting with some remarks about my experiences in the fifties as a student of Tarski and subsequently as a colleague, and about my growing appreciation of his fundamental role in the development of the field of mathematical logic.

I began work as a graduate student in Berkeley in 1948 and before that, I had had just one (rather odd) course in logic. But I felt that it was a subject that I could be interested in—except that I had no idea of what one really did in studying logic, what there was to be done, or who the people were that one did it with. One reason I went to Berkeley was that I got a teaching assistantship there; I had applied to several places. I had heard dimly of Tarski. I really had no idea that I'd be coming to a place where one of the leaders of the field was working away. But there he was, and he was offering his course on metamathematics that year so I enrolled in it. I think this was an experience which regrettably happens only rather rarely, that you find right then and there what it is that you've been looking for all along. There was no question in my mind that this was the subject that I wanted to be working on, that this was the way of viewing it that I was looking for, and that this was what I would want to be following in the years to come.

Tarski was an extremely effective and powerful lecturer. Pat Suppes gave some sense of the personal passion and energy he always conveyed. If you've never seen him, I'd like to give a bit of a physical description: he was a small man, compact, with very intense eyes, balding, and a very prominent forehead with marked veins. If you have a picture of Picasso, that gives a kind of approximation to what he was like, a small person having enormous intensity and vitality, and prodigiously productive. He wrote in a very big, bold hand and spoke with a Polish accent. He was a bit old-fashioned, one had the sense; he stood out in that respect in Berkeley, as a kind of master of the old school.

Before long, I started attending seminars that Tarski gave and I worked very hard in those. I made one contribution that I spent a lot of time working up, and looking back it was probably some minor exercise on Boolean algebras, but he complimented me on my presentation, which was quite encouraging. Pretty soon it became clear to me and my fellow students that Tarski would be the one that I would want to work with for my Ph.D., though everybody said that Tarski was a very difficult person to work for. He was indeed a very demanding professor and had very high standards for his students. With all the students that did succeed under him, there are also a number who unfortunately were left by the wayside. So one had to have a certain amount of courage to enter into this course of studies, and it did take me a while to approach him. But when I did, he was very nice and made it easy for me, and said "Of course"; it seemed like quite a natural and normal thing that we would proceed in this way, and he proposed things that I would read and study. And so in the following years, besides regularly attending his seminars, I took more of his courses, including set theory and algebra and eventually became course assistant to him in some of these same courses and so followed his method of organization and development. I never ceased to be impressed with what an extraordinary lecturer he was and how he managed to start off so simply in certain ways and gradually build up, putting each brick in place to end up with a solid edifice. He seemed to have an endless fund of knowledge about all parts of logic and other fields of mathematics, particularly algebra. There was no subject that came up on which he wouldn't have some information and views. Especially in informal gatherings he loved to talk about literature and art, and politics as well, a favorite subject of his on which he had very strong feelings.

Besides Tarski's "superhuman" aspects, there was also a humanity about him, and I want to tell some little anecdotes that give a flavor of that side. He lectured frequently in Berkeley's Dwinelle Hall, many of whose rooms had small podiums; he always seemed to have an uncertain relationship with material objects and among them were these podiums which he'd constantly be backing into or almost backing off of, and one was always afraid of what would happen. But even though he'd teeter there, he never did fall off. And often, because of the forcefulness with which he wrote on the board, the chalk would explode in his hand. And then there was the business about the cigarettes. Since he was an inveterate chain smoker, he smoked while lecturing and there was always the cigarette and the chalk—and it looked like he was going to smoke the chalk and write with the cigarette! But somehow he always managed to put each one in the right place....

In the period that I was a student I saw a field being transformed in front of my eyes; it was quite amazing. Tarski's interests then were primarily in model theory, but I soon learned that he didn't just sit with one subject. While I'd be thinking, "Well, we're doing such and such these days," suddenly he'd come in and start talking about something entirely different. What I discovered was that he had a series of maybe a dozen or fifteen topics over the years that he kept circulating through, in set theory and model theory and algebra, particularly, but in other fields as well, even geometry. And he'd just go from one to another. He would work on a group of problems—and push them—by himself, with his colleagues, with students. He would see them tied up to a certain point, and then when he was satisfied with that, he'd just move on to the next thing that he had sitting around. And he'd just keep pulling things out of his desk drawers, all sorts of notes on topics that he had developed to some extent or another in the past.

But in the fifties especially, model theory was a very strong interest of his and that was a period in which the sort of things that Jon Barwise was talking about—the fundamentals of model theory—were being built up by him and his students and colleagues. Among these was Leon Henkin who came to Berkeley, and among others elsewhere, one should mention Abraham Robinson who was at the same time very influential in helping to give model theory the importance it has today. One of the main results that Tarski obtained back in the thirties but didn't publish until 1948 (with the help of J.C.C. McKinsey), was on a decision procedure for real algebra and geometry. This was really a paradigm solution to a problem of applying logic to questions of algebraic interest, and it turned out to be extremely important in various ways in model theory and applications of model theory to algebra and in computational uses of algebra today.

I want to say something in general terms about Tarski's scientific style and interests; some of this will overlap, of course, with what the previous speakers had to tell us. Besides model theory, he was interested in all kinds of algebras of logic, in Boolean algebra, relation algebras (which go back to Peirce and Schröder), and his own cylindric algebras; then there were algebras of topology, such as closure algebras, which turned out to be very useful for novel interpretations of intuitionistic logic. He wrote two books—one on cardinal algebras and one on ordinal algebras, which are less familiar and certainly not in fashion now, but which I think hold a lot of very useful and interesting material. In general, he liked the approach taken to the subject of universal algebra along the lines developed by people like Garrett Birkhoff.

In the thirties he had been particularly interested in developing metamathematics as a body of mathematical work. That is typified in his volume of selected papers, *Logic, semantics, metamathematics* (now in a paperback second edition), in which you will find besides the famous paper on the concept of truth and papers on definability, a number of papers on the calculus of systems, which I think were quite important. Also in the thirties he applied a lot of effort to set theory, particularly the role of the axiom of choice in cardinal arithmetic, and the set-theoretical structure of ideals in Boolean algebras. Finally, he kept returning over the years to the application of the method of quantifier elimination in order to obtain decision procedures for a variety of algebraic and mathematical theories. By way of complementary work, in 1953 he published the very influential *Undecidable theories*, with A. Mostowski and R.M. Robinson.

Overall in terms of describing his scientific style and his approach, the thing that I would mainly emphasize is that unlike people like Russell and Hilbert and Brouwer, he had no philosophical prejudices about the foundations of mathematics; he wanted to use mathematics fully in the development of mathematical logic, and he did this to complete advantage, by working within set theory without restriction. Now there's a curious side to this, and that concerns the question as to what his own philosophy of mathematics was. In conversation with me and others, he seemed to say that he really did not believe in set theory—that he really did not believe it was about something—and he treated it rather formally. If you read the article about Tarski in the *Encyclopedia of philosophy* by his first student Andrzej Mostowski, he brings out the same point. Mostowski says he's puzzled about this and doesn't know what to make of it but there it is and maybe eventually we'll find out. Well, I don't know if we ever will but I think what's quite amazing is that you could not tell he had that viewpoint from his own work, since here was someone who used set theory to its fullest and for all one knew, really believed everything he did with it. Maybe it was more a pragmatic attitude, but he certainly did very well with that.

On the other hand, this set theoretical emphasis limited Tarski's understanding in certain respects; he really had no feeling for proof theory and none for constructivity that I ever observed. What he did have was a very strong motivation to make logic mathematical, and at the same time to make it of interest to mathematicians. He struggled with that in many ways; sometimes he tried to force things into a certain mold that he thought would be the only way in which mathematicians would accept the material. Though it wasn't always necessarily the right way to go, one way or another he did certainly help attract the interest of mathematicians. He had a very strong feeling—I would almost call it ideological—for axiomatics and for the algebraic approach to logic. He would axiomatize and algebraicize whenever he could. And it's amazing how much of that he did. He had an extraordinary sense for rigor, exactitude, and organization, and he kept working and reworking his papers. By helping him with a few of these I could see the process he went through to bring them to final form. Yet at the same time, he was extraordinarily prolific, and his papers number in the hundreds. To get a sense of the extent of his contributions you should look at his *Collected works* published by Birkhäuser in four thick volumes; the final volume has a complete bibliography.

Also worth looking at is the volume published by the AMS of the *Proceedings of the Tarski Symposium* held in 1971 for his seventieth birthday. The variety of presentations to be found there gives a sense of how influential he was on so many people in so many directions. And also to be emphasized in terms of his influence were his energy, his drive, the fact that he kept pushing people, and particularly, that he had an enormous fund of problems to suggest and that his choice of problems was extremely good. He didn't just say, "Well, try this or that." He really thought about what the problems were that one ought to work on at a given time and how they ought to be pursued. I think almost all of them have been attacked or solved in one way or another. There are only a few that come to mind to which I think the answer is still unknown—one of them has to do with the decision problem for free groups with at least two generators, and another with the decision problem for the real field with exponentiation. Tarski was certainly instrumental in building up logic in Berkeley as one of the top centers in the world for the study of mathematical logic and in assembling there a faculty which was quite exceptional and continues to be exceptional, having leaders in many areas of mathematical logic.

Finally, Tarski was a prime mover behind a series of very important conferences. He was a leading participant in one extremely important conference in 1957 held at Cornell, which brought together people from all parts of logic. Then in 1960 we had here at Stanford the first Congress for Logic, Methodology, and Philosophy of Science that he, Pat Suppes and Ernst Nagel instituted, and that has met regularly at international points ever since. Just this last year [1983] we had the seventh such congress in Salzburg, and it is an increasingly important ongoing affair.[1] In 1963 there was a theory of models conference in Berkeley, and in 1967 an enormous set theory conference in UCLA. The sixties were a time of great development in the field of set theory and infinitary logic, to which Tarski and his students and colleagues contributed a great deal.

To conclude, I want to say that I feel Tarski was a leader in the best sense of the word. It is true that he maintained his dominance in his own school and in the group of people around him. But he did not suppress anybody; rather, he encouraged them and helped them develop the best they could offer to the field. He valued their contributions and gave everybody their proper share of the territory that they had helped to explore together.

[1] The eighth Congress of Logic, Methodology, and Philosophy of Science was held in Moscow in 1987.

Julia Bowman Robinson (1919–1985)

CONSTANCE REID WITH RAPHAEL M. ROBINSON

BIOGRAPHY

Julia Bowman Robinson was the first woman mathematician to be elected to the National Academy of Sciences and the first woman to be president of the American Mathematical Society (AMS). Her mathematical work was most often centered on the border between logic and number theory.

"I think that I have always had a basic liking for the natural numbers," she once said, recalling that her earliest memory was of arranging pebbles in the shadow of a giant saguaro on the Arizona desert, where she lived as a small child. "We can conceive of a chemistry which is different from ours, or a biology, but we cannot conceive of a different mathematics of numbers. What is proved about numbers will be a fact in any universe."

She was born Julia Bowman on December 8, 1919, in St. Louis, Missouri, the second daughter of Ralph Bowers Bowman and Helen Hall Bowman. Shortly after her second birthday, her mother died. Her father found that he had lost interest in his machine tool and equipment business, and a year later, when he remarried, he decided to retire. The family lived first in Arizona and then in San Diego.[1]

When Julia was nine years old, she contracted scarlet fever, which was followed by rheumatic fever. After several relapses she was forced to spend a year in bed at the home of a practical nurse. She had been in the fifth grade when she fell ill, and by the time she recovered she had missed two additional years of school. After a year of tutoring, she returned as a ninth grader.

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[1]Constance Reid, with Raphael M. Robinson, "Julia Bowman Robinson (1919–1985), in *WOMEN OF MATHEMATICS A Biographic Sourcebook*, Louise S. Grinstein and Paul J. Campbell, eds. (Greenwood Press, Inc., Westport, CT, 1987), pp. 182–189. Copyright ©1987 by Louise S. Grinstein and Paul J. Campbell. Reprinted with permission.

She now knew that mathematics was the school subject which she liked above all others, and she persisted with it at San Diego High School in spite of the fact that by her junior year all the other girls had dropped the subject. When she graduated in 1936, she was awarded the honors in mathematics and the other sciences which she had elected to take, as well as the Bausch–Lomb medeal for all-around excellence in science.

At the age of sixteen, she entered San Diego State College, now San Diego State University. It had recently been a teachers' college and, before that, a normal school. Emphasis was still largely on preparing teachers. By this time the savings that her father had counted on to support his family in his retirement had been almost completely wiped out in the Depression of the 1930s. At the beginning of Julia's sophomore year, he took his own life. In spite of the family's straitened circumstances, she was able to continue her education, tuition at that time being only \$12 a semester. When her older sister was hired as a teacher in the San Diego school system, money became available for Julia to transfer to the University of California at Berkeley for her senior year.

"I was very happy, really blissfully happy, at Berkeley," she later recalled.

> In San Diego there had been no one at all like me. If, as Bruno Bettelheim has said, everyone has his or her own fairy story, mine is the story of the ugly duckling. Suddenly, at Berkeley, I found that I was really a swan. There were lots of people, students as well as faculty members, just as excited as I was about mathematics. I was elected to the honorary mathematics fraternity, and there was quite a bit of departmental social activity in which I was included. Then there was Raphael.

"Raphael" was assistant professor R. M. Robinson, who taught the number theory course which she took during her first year at Berkeley. In the second semester there were only four students in the class—she was again the only woman—and he began to invite her to go on walks with him. In the course of these he told her about various interesting things in modern mathematics, including Kurt Gödel's results: "I was very impressed and excited by the fact that things about numbers could be proved by symbolic logic. Without question what had the greatest mathematical impact on me at Berkeley was the one-to-one teaching that I received from Raphael."

At the end of the first semester of her second graduate year at Berkeley, a few weeks after Pearl Harbor, she and Raphael Robinson were married. There was a rule at Berkeley that members of the same family could not teach in the same department. Since Julia already had a mathematics department teaching assistantship—she was teaching statistics for Jerzy Neyman—this rule did not immediately apply. Later, the prohibition did not concern her, since, now that she was married, she expected and very much wanted to have a family. In the meantime, while the United States was engaged in World War II, she and other mathematics faculty wives worked for Neyman in the Berkeley Statistical Laboratory on secret projects for the military.

When Julia finally learned that she was pregnant, she was delighted—and very disappointed when, after a few months, she lost the baby. She was then advised that because of the buildup of scar tissue in her heart (a result of the rheumatic fever), she should under no circumstances become pregnant again.

For a long time she was very depressed because she could not have children, but during the year 1946–1947, when she and Raphael were in Princeton, she took up mathematics again at his suggestion. The following year, back in Berkeley, she began to work toward a Ph.D. with Alfred Tarski, the noted Polish-born logician, who had joined the Berkeley faculty during the war. Her thesis, "Definability and decision problems in arithmetic," was accepted in June 1948.

The same year that she received her Ph.D., she began to work on the Tenth Problem on David Hilbert's famous list: to find an effective method for determining if a given Diophantine equation is solvable in integers. The problem was to occupy the largest portion of her professional career. As in the case of her thesis problem, the initial impetus came indirectly from Tarski, who had discussed casually with Raphael the problem whether, possibly using induction, one could show that the powers of 2 cannot be put in the form of a solution of a Diophantine equation. Not realizing, initially, the connection with the Tenth Problem, which she said later would have frightened her off, she began to work on solving Tarski's problem. When she found that she could not do so, she turned to related problems of existential definability.

During 1949–1950, when Raphael had a sabbatical, she worked at the RAND Corporation in Santa Monica. It was there that she solved the widely discussed "fictitious play" problem (see below). See did not, however, stop working on problems of existential definability relevant to Hilbert's Tenth Problem, and in 1950 she presented her results in a ten-minute talk at the International Congress of Mathematicians in Cambridge, Mass.

Following a frustrating and unsuccessful experience with a problem in hydrodynamics for the Office of Naval Research, she threw herself into Adlai Stevenson's presidential campaigns (1952 and 1956) and Democratic party politics for the next half dozen years.

In the summer of 1959, Martin Davis and Hilary Putnam proved a theorem which turned out to be an important lemma in the ultimate solution of the Tenth Problem. They sent a copy of their work to Julia, some of whose methods they had utilized.

"Her first move, almost by return mail, was to show how to avoid the messy analysis," Davis recalls. "A few weeks later she showed how to replace the unproved hypothesis about primes in arithmetic progression by the prime number theorem for arithmetic progressions... [She] then greatly simplified the proof, which had become quite intricate. In the published version, the proof was elementary and elegant."

By the time that the Davis-Putnam-Robinson paper appeared in 1961, she was forced by the deterioration of her heart to undergo surgery for the removal of the buildup of scar tissue in the mitral valve. After the operation her health improved dramatically. During the years that followed, she was able to enjoy many outdoor activities, particularly bicycling, which she had had to forego since childhood. She still found, however, that teaching one graduate course a quarter at Berkeley, as she did on occasion, was about all she could manage.

With Yuri Matijasevič's unexpected solution of Hilbert's Tenth Problem at the beginning of 1970 and the recognition of the crucial importance of Julia's work in the solution, many honors began to come to her. In 1975 she became the first woman mathematician to be elected to the National Academy of Sciences and, somewhat tardily, a full professor at Berkeley (with the duty of teaching just one-fourth time). In 1978 she became the first woman officer of the AMS and in 1982 its first woman president. She was also elected president of the Association of Presidents of Scientific Societies, a position she later had to decline because of ill health. In 1979 she was awarded an honorary degree by Smith College, and the following year she was asked to deliver the Colloquium Lectures of the AMS. It was only the second time a woman had been so honored (Anna Pell Wheeler* was the first, in 1927). In 1983 she was awarded a MacArthur Fellowship of $60,000 a year for five years in recognition of her contributions to mathematics. In 1984 she was elected to the American Academy of Arts and Sciences.

Even after Matijasevič's solution, Hilbert's Tenth Problem continued to pose interesting questions. She collaborated on two papers with Matijasevič, whom she had come to know personally on a 1971 trip to Leningrad. For the Symposium on Hilbert's Problems at De Kalb, Illinois, in May 1974, she also collaborated with Davis and Matijasevič on a paper concerning the positive aspects of the negative solution to the problem. It was her last published paper, the business of the AMS occupying most of her time and energy during the next decade. She was also frequently active during this period with problems of human rights.

At the 1984 summer meeting of the AMS in Eugene, Oregon, over which she was presiding, she learned that she was suffering from leukemia. After a

*Cross-reference to other women discussed in the volume is given by an asterisk following the first mention in a chapter of the individual's name.

remission of several months in the spring of the following year, she died on July 30, 1985.

WORK

Julia Robinson's dessertation was written under the direction of Alfred Tarski. He characteristically suggested many problems in class and in conversation, and she pursued those that particularly interested her. Her dissertation contained several results, the most interesting of which will be discussed here.

It follows from the work of Gödel that there can be no algorithm for deciding which sentences of the arithmetic of natural numbers are true. The sentences referred to in this context are those using the concepts of elementary logic, variables, and the operations of addition and multiplication. Since the theorem of Lagrange that every natural number is the sum of four squares can be used as a definition of natural numbers in the ring of all integers, it follows that the arithmetic of integers is also undecidable. On the other hand, Tarski had previously shown that the arithmetic of real numbers is decidable. In all three of these cases the same sentences are used; only the range of the variables is different.

The question raised by Tarski was whether the arithmetic of the rational numbers is decidable or undecidable. If an arithmetical definition of the integers in the field of rational numbers could be given, the undecidability would be proved. Such a definition was given in Julia Robinson's thesis (1949).

The first breakthrough was the observation that if M is a rational number, expressed as a fraction in lowest terms, then the denominator of M is odd if and only if $7M^2 + 2$ can be expressed as a sum of three squares of rational numbers. This follows easily from the classical result hat a natural number is the sum of three squares of integers if and only if it does not have the form $4^a(8b + 7)$.

This result led her to study the theory of quadratic forms. If one quadratic form could be used to eliminate the prime 2 from the denominator, perhaps other forms could be used to eliminate other prime factors. (If all prime factors could be eliminated from the denominator, the rational number would be an integer.) Other ternary quadratic forms were located which served this purpose. In the end the prime 2 was handled in a different way, in combination with other primes, so that the original observation does not appear in the dissertation.

There remained the problem of combining all the required conditions in one formula. It was impossible, in the language used, to describe the various quadratic forms which were needed. She resolved this difficulty by using a larger class of forms which could be described but which would not eliminate any integers.

In this way she used the theory of ternary quadratic forms in a successful attack on a problem of logic. In a later paper, she extended the result to fields of finite degree over the rationals ("The undecidability..." 1959).

Her dissertation exemplifies the fact that her main field of interest lay on the borderline between logic and number theory; however, she wrote two papers completely outside of this field. One was a small paper on statistics (1948), written before her dissertation when she was working in the Berkeley Statistical Laboratory. The other was an important paper on game theory (1951), written when she was working at the RAND Corporation. This latter paper solved one of a list of problems for which RAND had offered monetary prizes (although as an employee she was not eligible for the prize).

George W. Brown had proposed a method of finding the value of a finite two-person zero-sum game, sometimes called the method of fictitious play. Two players are imagined as playing an infinite sequence of games, using in each game the pure strategy which would yield the optimal payoff against the accumulated mixed strategy of the opponent. Brown noted that the value of the game lay between these optimal payoffs for the two players and conjectured that they would converge to the value of the game as the number of plays increased. Julia's paper, "An iterative method of solving a game," verified Brown's conjecture. It is still considered a basic result in game theory.

Several of her papers played an essential role in the negative solution of Hilbert's Tenth Problem, which asked for an algorithm to decide whether a Diophantine equation has a solution. The first of these was "Existential definability in arithmetic" (1952). The problem studied was whether various sets are existentially definable in the arithmetic of natural numbers. The set of composite numbers is existentially definable, but at the time it was not known whether the set of primes is, as was later established. In this paper she proved that binomial coefficients, factorials, and the set of primes are existentially definable in terms of exponentiation, and that exponentiation in turn is existentially definable in terms of any function of roughly exponential growth.

At the time these results seemed somewhat fragmentary, but they took on added importance after the publication of a joint paper with Davis and Putnam (1961). In this paper it is proved that every recursively enumerable set is existentially definable in terms of exponentiation. It follows that there is no algorithm for deciding whether an exponential Diophantine equation (that is, a Diophantine equation in which exponentiation as well as addition and multiplication is allowed) has a solution in natural numbers. In view of her earlier proof that exponentiation is existentially definable in terms of any function of roughly exponential growth, the negative solution of Hilbert's Tenth Problem was reduced to finding an existential definition of such a function. That was finally done by Matijasevič at the beginning of 1970.

Later she collaborated with Matijasevič (1975) in proving that there is no algorithm for deciding whether a Diophantine equation in thirteen variables has a solution in natural numbers. (Matijasevič has since reduced the number of variables to nine.)

Among her other works are two papers dealing with general recursive functions (1950, 1968), as well as one on primitive recursive functions (1955) and one on recursively enumerable sets (1968). The 1950 paper on general recursive functions was her first paper after the dissertation. In it she starts from the characterization of general recursive functions as those obtained by adjoining the μ-rule to the rules used to obtain primitive recursive functions, and then asks what restrictions can be placed on the defining schemes. One result is the proof that all general recursive functions of one variable can be obtained from two special primitive recursive functions (one of which is rather complicated) by composition and inversion. In the later paper, she showed that this same class of functions can be obtained from the zero and successor functions by composition and a new scheme which she calls general recursion.

Other papers include one giving an expository treatment of the class of hyperarithmetical functions (1967) and one giving a finite set of axioms for number-theoretic functions from which the Peano axioms can be derived (1973).

Her Colloquium Lectures, delivered in 1980, have not been published. The first, which was introductory, discussed Gödel's work and the concept of computability. The second dealt with work related to Hilbert's Tenth Problem and included a new proof, due to Matijasevič, of the undecidability of exponential Diophantine equations. The third treated the decision problem for various rings and fields; and the fourth, nonstandard models of arithmetic.

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John Wermer received his Ph.D. at Harvard in 1951, and his thesis advisor was George Mackey. His mathematical interests were strongly influenced by Arne Beurling, with whom he studied at Harvard and in Sweden. He taught at Yale from 1951 to 1954, and since then he has been at Brown. He has worked on operator theory, Banach algebras and complex function theory.

Function Algebras in the Fifties and Sixties[1]

JOHN WERMER[2]

1. INTRODUCTION

This essay is a very personal survey of a chapter of mathematical history in which I participated, the study of Function Algebras in the U.S. in the period 1950–1970. For obvious reasons the survey is very incomplete, as is the bibliography. For a balanced view of the subject the interested reader can consult three excellent works: *Introduction to Function Algebras* by A. Browder, W. A. Benjamin, Inc. (1969), *Uniform Algebras* by T. W. Gamelin, Prentice Hall, Inc. (1969), and *The Theory of Uniform Algebras* by E. L. Stout, Bogden and Quigley, Inc. (1971).

Starting in the early 1950s a band of American mathematicians went to work on some questions in complex analysis which came from two sources: the theory of polynomial approximation on compact sets in the complex plane, and the theory of commutative Banach algebras. The American mathematicians included Richard Arens at UCLA, Charles Rickart at Yale, Ken Hoffman and Iz Singer at MIT, Andy Gleason at Harvard, Hal Royden at Stanford, Errett Bishop at Berkeley, Irv Glicksberg at the University of Washington, Walter Rudin at Rochester and the University of Wisconsin, and the author at Brown. They and their students began to develop a theory of Function Algebras which formed a new link between classical Function Theory and Functional Analysis. Their inspiration came largely from the Soviet Union.

[1] A good discussion of many of the topics of this article, as well as a very extensive bibliography, is given in the article by G. M. Henkin and E. M. Čirka, *Boundary Properties of Holomorphic Functions of Several Complex Variables*, Plenum Publishing Corporation (1976).

[2] I am grateful to Andy Browder and Peter Duren for helpful comments for this article.

In the 1940s I. M. Gelfand and his coworkers had built a theory of commutative Banach algebras in which they had shown that such an algebra, if it has a unit and its radical is zero, is isomorphic to an algebra $\mathfrak{A}$ of continuous complex-valued functions on a compact Hausdorff space $\mathfrak{M}$. The points of $\mathfrak{M}$ are identified with the maximal ideals of $\mathfrak{A}$. G. Šilov had shown that among all closed subsets of $\mathfrak{M}$ there exists a smallest set $\check{S}$ with the property that if m is in $\mathfrak{M}$, then for each f in $\mathfrak{A}$

$$|f(m)| \leq \max |f(x)| \text{ taken over } \check{S}.$$

$\check{S}$ is called the *Šilov boundary* of the algebra.

A simple model for this is given by the *disk algebra* $A(D)$ consisting of all functions which are analytic in the open unit disk: $|z| < 1$ and continuous in the closed disk D: $|z| \leq 1$. Here the maximal ideal space $\mathfrak{M}$ can be identified with D and the Šilov boundary with the unit circle: $|z| = 1$. The natural norm on $A(D)$ is given by $\|f\| = \max |f(z)|$, taken over D.

The question arises: let $\mathfrak{A}$ be an arbitrary semi-simple commutative Banach algebra with unit, such that $\check{S}$ is nontrivial, i.e., $\check{S}$ is strictly smaller than $\mathfrak{M}$. Does there exist an *abstract function theory* for $\mathfrak{A}$, i.e., do the functions in $\mathfrak{A}$ behave on $\mathfrak{M}\backslash\check{S}$ like analytic functions (as in the example of the disk algebra)? Furthermore, does $\mathfrak{M}\backslash\check{S}$ possess *analytic structure*, i.e., can we find subsets of $\mathfrak{M}\backslash\check{S}$ which can be made into complex manifolds on which the functions in $\mathfrak{A}$ are analytic? If enough such analytic structure could be shown to exist, this would explain the Šilov boundary in terms of the maximum principle of analytic function theory.

In 1952 a brilliant achievement by the Soviet Armenian mathematician S. N. Mergelyan provided a second source of inspiration. Mergelyan showed in **[48]** that if X is a compact set in the z-plane $\mathbb{C}$ such that $\mathbb{C}\backslash X$ is connected, then every function which is continuous on X and analytic on the interior of X can be uniformly approximated on X by polynomials in z. This result can be read as a statement about a certain Banach algebra. We let $P(X)$ denote the uniform closure on X of the polynomials in z and we put on $P(X)$ the supremum norm over X. Then $P(X)$ is a Banach algebra, the maximal ideal space $\mathfrak{M}$ coincides with X, and the Šilov boundary $\check{S}$ coincides with the topological boundary of X. Mergelyan's theorem yields that a function φ defined and continuous on $\mathfrak{M}$ belongs to $P(X)$ if and only if φ is analytic on $\mathfrak{M}\backslash\check{S} = \operatorname{int}(X)$ in the natural analytic structure which $\operatorname{int}(X)$ inherits from $\mathbb{C}$.

2. Uniform Algebras

For the problems mentioned above, of constructing an abstract function theory for $\mathfrak{A}$ and of exhibiting analytic structure on $\mathfrak{M}\backslash\check{S}$, it seemed natural to take the norm on the algebra $\mathfrak{A}$ to be a uniform norm. The "Function

Algebras" to be studied where then as follows: we fix a compact Hausdorff space X and an algebra $\mathfrak{A}$ of continuous functions on X such that $\mathfrak{A}$ is closed in the algebra $C(X)$ of all continuous functions on X, contains the constants, and separates the points of X. If we put on $\mathfrak{A}$ the uniform norm over X, $\mathfrak{A}$ is then a Banach algebra. $\mathfrak{M}$ is a compact space in which X lies embedded, as proper subset in general, and $\check{S}$ is a closed subset of X.

Such algebras were baptised *uniform algebras* by Errett Bishop in 1964. He thought the name sounded good, and it has stuck. One says that $\mathfrak{A}$ is a uniform algebra *on* X. Uniform algebras are plentiful in nature. Here are some examples:

(i) Let Y be a compact set in $\mathbb{C}^n$, the space of n complex variables. Let $P(Y)$ denote the uniform closure on Y of polynomials in the complex coordinates $z_1, \ldots, z_n$. Then $P(Y)$ is a uniform algebra on Y.

The disk algebra is a special case. For $n = 1$ and so $Y \subset \mathbb{C}$, Mergelyan's theorem tells us which functions belong to $P(Y)$.

(ii) Let Σ be a finite Riemann surface with boundary $\partial\Sigma$ and denote by $A(\Sigma)$ the algebra of functions continuous on Σ and analytic on $\Sigma \backslash \partial\Sigma$. $A(\Sigma)$ is a uniform algebra on Σ.

(iii) Let K be a compact set in $\mathbb{C}$ and let $R_0(K)$ denote the space of rational functions whose poles lie in $\mathbb{C} \backslash K$. Let $R(K)$ denote the uniform closure of $R_0(K)$ on K. Then $R(K)$ is a uniform algebra on K.

(iv) Let H^∞ denote the algebra of all bounded analytic functions on the open unit disk. By Fatou's theorem, H^∞ is embedded in L^∞ of the unit circle, and L^∞, in turn, is isomorphic to $C(X)$ for a (complicated) space X. H^∞ is a uniform algebra on X.

(v) The Stone–Weierstrass theorem yields that the only uniform algebra on a compact space X which is closed under complex conjugation is the full algebra $C(X)$.

A first indication that it might be possible to do abstract function theory on a uniform algebra A was the proof that *representing measures* always exist. By a representing measure for a point m in $\mathfrak{M}$ is meant a probability measure μ on the Šilov boundary $\check{S}$ such that for all f in A

$$f(m) = \int f \, d\mu.$$

Arens and Singer in [5] and John Holladay in his Yale thesis (1953) proved that such a μ exists.

In the case of the disk algebra $A(D)$, μ is unique for a given m and is the Poisson measure on the circle, corresponding to m. In general, μ is far from unique.

A representing measure μ is multiplicative on A, i.e.,

$$\int fg\,d\mu = \left(\int f\,d\mu\right)\cdot\left(\int g\,d\mu\right). \quad \text{for all } f, g \text{ in } A,$$

and conversely, each multiplicative probability measure is the representing measure for some point m in $\mathfrak{M}$.

In 1953 in **[64]** Šilov made another fundamental contribution to Banach algebra theory by introducing the use of analytic functions of several complex variables into the theory. Let $\mathfrak{A}, \mathfrak{M}$ be as above. Suppose that $\mathfrak{M}$ is disconnected, i.e., $\mathfrak{M} = \mathfrak{M}_1 \cup \mathfrak{M}_2$ where $\mathfrak{M}_1, \mathfrak{M}_2$ are disjoint closed sets. Šilov showed that $\exists$ e in $\mathfrak{A}$ with $e^2 = e$ such that $e = 1$ on $\mathfrak{M}_1$ and $e = 0$ on $\mathfrak{M}_2$.

Not long after, Arens and Calderon in **[4]** and L. Waelbroeck in **[68]** developed a functional calculus for analytic functions of n variables acting on n-tuples of elements of a commutative Banach algebra.

Another application of several complex variables to Banach algebra theory was the algebraic description of the first cohomology group of the maximal ideal space, independently by R. Arens in **[3]** and H. Royden in **[58]**. They showed that for $\mathfrak{A}, \mathfrak{M}$ as above, $H^1(\mathfrak{M}, \mathbf{Z})$ is isomorphic to the quotient group of the group of units of $\mathfrak{A}$ by the subgroup of elements $\exp(y)$ with y in $\mathfrak{A}$.

3. Gleason's Program

Andrew Gleason launched the earliest attacks on the problem of analytic structure in the maximal ideal space of a uniform algebra.

In the case of the disk algebra $A(D)$ those maximal ideals m corresponding to an interior point of the disk, say the point a, have the algebraic property of being *simply generated*: every f in the ideal m can be written in the form: $f = g(z - a)$ with g in $A(D)$. Maximal ideals corresponding to boundary points of D are not simply generated. Gleason obtained the following striking result: *Let A be a uniform algebra and fix m in $\mathfrak{M}$. Suppose that the ideal m is finitely generated in the algebraic sense. Then some neighborhood U of m in $\mathfrak{M}$ can be given the structure of an analytic variety such that every h in A is analytic on U.*

He lectured on this result in the mid-fifties, and published it in **[29]**.

In another direction, Gleason observed the following: with $A, \mathfrak{M}$ as before, let m_1, m_2 be two points in $\mathfrak{M}$. Then $|f(m_1) - f(m_2)| \leq 2$ whenever f belongs to the unit ball of A. It may happen that there exists $k < 2$ such that $|f(m_1) - f(m_2)| \leq k$ whenever f belongs to this unit ball. In the case of the disk algebra, this occurs whenever m_1 and m_2 lie in the open unit disk. This suggests the following general definition: for m_1, m_2 in $\mathfrak{M}$, put $m_1 \sim m_2$ whenever $\exists$ such a $k < 2$, or, in other words, whenever the distance from m_1 to m_2 in the dual Banach space of A is less than 2. Gleason showed that $\sim$ is an equivalence relation on $\mathfrak{M}$. (Since $2+2=4$, the transitivity of the relation $\sim$ is not evident!) He called the equivalence classes under $\sim$ *the parts of* $\mathfrak{M}$.

For the case of the disk algebra $A(D)$, the open unit disk is one part and each point on the unit circle is a one-point part. For the case of the bi-disk algebra $A(D^2)$ which consists of all functions which are continuous on the closed bi-disk $D^2 = \{|z| \leq 1\} \times \{|w| \leq 1\}$ in $\mathbb{C}^2$ and analytic on the open bi-disk, the maximal ideal space $\mathfrak{M} = D^2$, and the parts are as follows: the open bi-disk is one part, each disk: $z = z_0, |w| < 1$ and each disk: $|z| < 1, w = w_0$ with $|z_0| = 1$ and $|w_0| = 1$ is a part; the remaining parts are the one-point parts on the distinguished boundary $\{|z| = 1\} \times \{|w| = 1\}$ of D^2. Thus the parts here are complex manifolds of dimensions 2, 1, and 0.

Gleason lectured on these ideas, [**28**], at the Conference on Analytic Functions at the Institute for Advanced Study in Princeton in September, 1957. This was a marvelous meeting. The people there interested in Banach algebras included R. Arens, R. C. Buck, L. Carleson, A. Gleason, K. Hoffman, S. Kakutani, Lee Rubel, H. Royden, I. Kaplansky, L. Waelbroeck, and myself. Many of the giants of function theory gave talks, both on one and several complex variables, and tolerated those of us who didn't know much about either one or several complex variables. The two weeks of the conference were for us enormously stimulating and provided the germ of much later work on Function Algebras.

Kakutani had studied H^∞ as a Banach algebra, and reported on his work in [**41**]. At the conference, he discussed the boundary behavior of a bounded analytic function in terms of normed ring theory, [**42**].

Earlier, Kakutani had raised the following basic question about H^∞ as a ring: the open unit disk is naturally embedded as an open subset Δ of the maximal ideal space $\mathfrak{M}$ of H^∞, and so its closure $\overline{\Delta}$ is contained in $\mathfrak{M}$. The set $\mathfrak{M}\backslash\overline{\Delta}$ was called the "Corona".

Is the Corona empty, i.e., is Δ *dense in* $\mathfrak{M}$? Suppose that the answer is "Yes" and consider an n-tuple of functions f_j in H^∞ with $\sum_{j=1}^n |f_j| \geq \delta$ on Δ, where δ is a positive constant. Then $\sum_{j=1}^n |f_j| \geq \delta$ on $\mathfrak{M}$ and so the f_j have no common zero on $\mathfrak{M}$. Hence the ideal generated by the f_j is contained in no maximal ideal of H^∞ and so is the whole ring. It follows that there exist g_j in H^∞, $j = 1, \dots, n$, such that

$$\sum_{j=1}^n f_j g_j = 1.$$

The problem of the existence of the g_j under the given assumption on the f_j turned out to be a very deep problem. This "Corona problem" was solved by Lennart Carleson in [**22**], and it follows that the Corona is indeed empty. Carleson's result and his method of proof has had a major impact on analysis. All this is treated in John Garnett's book mentioned in Section 7 below.

A breakthrough in the understanding of the maximal ideal space of H^∞ occurred at the conference, in the form of the birth of I. J. Schark, [62]. Schark's paper exhibited analytic structure in $\mathscr{M}\backslash\Delta$ for the first time. Schark never published again, since his name was put together from the initials of participants at the conference. So Schark did not perish; he vanished.

In his talk, Gleason formulated the following *Conjecture: Let* m_1, m_2 *be two points in the maximal ideal space* $\mathfrak{M}$ *of a uniform algebra. Then a necessary and sufficient condition for* m_1 *and* m_2 *to be in the same part of* $\mathfrak{M}$ *is that* m_1 *and* m_2 *can be connected by a finite chain of analytic images of the unit disk, contained in* $\mathfrak{M}$. A second idea Gleason introduced in [28] was the notion of a *Dirichlet Algebra.* The real parts of the functions belonging to a uniform algebra A on a space X can be viewed as "harmonic" on $\mathfrak{M}\backslash X$, as can uniform limits on $\mathfrak{M}$ of sequences of such functions. Gleason called A a *Dirichlet Algebra on* X if every real continuous function on X is the restriction to X of such a harmonic function, or, equivalently, if the real parts of functions in A form a uniformly dense subspace of the real continuous functions on X.

The disk algebra $A(D)$ may be viewed as a uniform algebra on the circle $|z| = 1$, with norm the supremum norm on the circle, rather than as a uniform algebra on the disk. $A(D)$ is a Dirichlet algebra on the circle.

Gleason wrote in [28] about Dirichlet algebras: "It appears that this class of algebras is of considerable importance and is amenable to analysis." It turned out subsequently that this preliminary judgment was right on target. At the time, in September 1957, Gleason's ideas were sufficiently strange and novel that I (and many of us, I imagine) did not fully grasp their significance.

4. The Summer of 1959 in Berkeley

In the summer of 1959 a lot of people working on Functional Analysis gathered, rather informally, in Berkeley. My wife Kerstin and I took our two boys, two and five years old, put them in our Chevy and drove across the country. It had been hot when we left the East Coast and got steadily hotter as we drove west until suddenly, as we came into Berkeley, a discontinuity occurred and we were in a cool and lush paradise, the sky blue, the air balmy, and all garden flowers blooming wildly.

I had along with me a recent paper by Henry Helson and David Lowdenslager, [33], in which they studied certain spaces of functions given by Fourier series on the torus. Earlier, Arens and Singer in [6], and Mackey in [47], had given a group-theoretic approach to analytic functions, based on the following observation: A Fourier series $f(x) = \sum_n c_n e^{inx}$ on the unit circle is the boundary function of a function analytic in the unit disk if and only if $c_n = 0$ for $n < 0$. Replacing the circle by the torus, one may consider Fourier series $f(\vartheta, \varphi) = \sum_{n,m} c_{nm} e^{in\theta} e^{im\varphi}$ in two variables. One specifies a half-plane S in the lattice $\mathbf{Z}^2$ and regards f to be "analytic", relative to S, if $c_{nm} = 0$ outside of S. An interesting example is obtained by taking S to be the set of points (n, m) in $\mathbf{Z}^2$ with $n + m\alpha \geq 0$, where α is a fixed irrational number. Helson and Lowdenslager showed in **[33]** that a series of classical boundary value theorems of function theory have counterparts for functions "analytic relative to S". Their results were dramatic and their proofs made elegant use of L^2-methods. Their paper stirred Solomon Bochner's interest, as he had looked at related questions at an earlier time. He showed that their proofs depended only on two properties: first, that for fixed S the class of S-analytic functions continuous on the torus is an algebra, and second, that the real parts of the functions in this algebra are dense in the real continuous functions on the torus. The group structure on the torus entered only through these properties. So Bochner, quite independently of Gleason, was led to the same Dirichlet algebras **[19]**. Thus it turned out that certain basic results about boundary-functions of analytic functions in the disk remain true, when properly stated, for an arbitrary Dirichlet algebra. How does this look?

For the case of the disk algebra, the measure $\frac{1}{2\pi}\,d\theta$ is the representing measure for the origin. For $p \geq 1$,the Hardy space H^p is defined as the closure of $A(D)$ in L^p on the circle with respect to this measure. Let now A, on X, be a Dirichlet algebra and fix m in $\mathfrak{M}$. Let μ be the unique representing measure for m on X, for the algebra A. We define $H^p(\mu)$ as the closure of A in $L^p(X, \mu)$. For f in $H^p(\mu)$, $f(m)$ is defined as $\int f\,d\mu$. One then has, for instance, the following:

THEOREM 1. *Let A, m, μ be as above. Fix a nonnegative function w on X which is summable with respect to μ. A necessary and sufficient condition for w to have a representation*

$$w(x) = |f(x)|^2 \text{ a.e.-}d\mu \text{ on } X$$

for some f in $H^2(\mu)$ with $f(m) \neq 0$ is that

$$\int \log w \cdot d\mu > -\infty.$$

THEOREM 2. *Let W be a closed subspace of $H^2(\mu)$ invariant under multiplication by elements of A, i.e., such that $f\varphi \in W$ whenever $\varphi \in W$ and $f \in A$. Assume also that 1 is not orthogonal to W. Then there exists a bounded function E_0 in W with $|E_0(x)| = 1$ a.e.-$d\mu$ such that*

$$W = \{E_0 g | g \in H^2(\mu)\}.$$

Theorems 1 and 2, in the case when A is the disk algebra, are classical results of, respectively, Szegö and Beurling.

When I realized, in Berkeley, how all these things fitted together I got quite excited. John Kelley and Errett Bishop had been studying Dirichlet algebras, and tutored me in the subject, and I also had the benefit of talking to Helson about his work with Lowdenslager. So I was able to prove the truth of Gleason's conjecture about parts, for the case of Dirichlet algebras, in the following form: *Let A be a Dirichlet algebra, $\mathfrak{M}$ its maximal ideal space and P a part of $\mathfrak{M}$. Then either P is a single point, or P is an analytic disk, i.e., P is the one-one image of the disk $|\lambda| < 1$ by a continuous map ψ such that $h \circ \psi$ is analytic on $|\lambda| < 1$ for each h in A* [72].

When we left Berkeley to go home at the end of August, we ran into several people at gas stations and so on, whom we had met upon arriving, who had noted the Rhode Island plates on our car and had told us that they themselves came from the East. When they realized we were going back, they were amazed: "You've seen California and you're going back East!" they said. My five-year-old son said, "Let's go home to America!" (meaning Providence, Rhode Island).

5. Errett Bishop and the General Theory of Uniform Algebras

Dirichlet algebras were almost too good to be true. The general uniform algebra is much less tractable, largely due to the nonuniqueness of representing measures for fixed points m in $\mathfrak{M}$. However, a series of results about general uniform algebras was discovered, with important applications to many questions in analysis. In this general theory, the unquestioned leader was Errett Bishop. Bishop was on the faculty at Berkeley from 1954 to 1965 and then on the faculty of the University of California at San Diego until his untimely death in 1983.

He was one of the most remarkable people I have known. He was a mathematician of amazing insight and penetration, absolutely fearless and with a profound commitment to mathematics. In his last years he was somewhat isolated in the mathematical community, because of his absolute dedication to constructive methods in mathematics.

In the period about which I am writing, Bishop's work and personal contact with him was enormously stimulating to the rest of us, and led to much work by other people, both jointly with him and independently of him. There was the famous joint work by Bishop and Karel de Leeuw on the Choquet boundary and by Bishop and Phelps on Banach spaces. Stolzenberg and Bishop worked closely together on polynomially convex hulls, as did Rossi and Bishop on problems about complex manifolds. My own work on analytic structure in maximal ideal spaces, e.g. in the joint paper [7] with Aupetit, and work on the same problem by Gamelin in [27], grew out of Bishop's rich paper **[15]**. And so on.

Here I can only mention a few of Bishop's contributions to the general theory of function algebras. The interested reader is referred to **[16]**, **[18]**,

[56] for more extensive discussions of his work. Further, his collected papers appear in **[76]**.

(i) *Peak points.* A point x_0 in X is called a *peak point* for the uniform algebra A on the space X if $\exists\ f$ in A with $f(x_0) = 1$ and $|f(x)| < 1$ on $X \backslash \{x_0\}$. In **[11]** Bishop showed that for X metrizable peak points exist and the set M of all peak points is a *minimal boundary* for A in the sense that for each g in A there is some point x in M with $g(x) = \|g\|$, and, of course, no proper subset of M has this property. It follows that M is a dense subset of the Šilov boundary $\check{S}$. Moreover, if m is a point of $\mathfrak{M}$ there exists a representing measure for m which lies on M.

The existence of peak points had earlier been observed by Gleason (unpublished).

Bishop was able to apply the notion of peak point to the problem of rational approximation. Let X be a compact subset of $\mathbb{C}$. As in Section 2 above, we write $R(X)$ for the uniform closure on X of those rational functions which are analytic on X. When does $R(X) = C(X)$? Clearly, for this to happen the interior of X must be empty. When X has connected complement in $\mathbb{C}$, Mergelyan's theorem shows that this is also sufficient. However, in general the condition is not sufficient, and to show this Mergelyan in 1952 in **[48]** constructed the following set S: remove from the closed unit disk $|z| \leq 1$ a countable family of disjoint open disks: $|z - a_j| < r_j$, $j = 1, 2, \ldots$ such that $\sum_j r_j < \infty$, and denote by S the closed set that remains. By Cauchy's theorem, the complex measure dz on the union of the circles $|z - a_j| = r_j$ together with the unit circle annihilates $R(S)$, and hence $R(S) \neq C(S)$. If A_j, r_j are chosen so that the interior of S is empty, we have the desired example. For obvious reasons, S is called a *Swiss Cheese.* It turned out that, in fact, Mergelyan had *rediscovered* the Swiss Cheese; in 1938 the Swiss mathematician Alice Roth had given such an example. The Swiss Cheese has been very useful to people constructing counterexamples in the study of Function Algebras. My colleague Bob Accola told me that Function Algebras *is* the study of the Swiss Cheese, but this is not strictly correct.

Let now X be an arbitrary compact subset of $\mathbb{C}$. Bishop showed the following: $R(X) = C(X)$ *if and only if each point x in X is a peak point for the algebra* $R(X)$. An extension of this result was found by Donald Wilken in **[74]**. A peak point is always a one-point part of the maximal ideal space. For $R(X)$ the maximal ideal space is precisely X. Wilken showed that *each part of X is either a one-point part, or has positive 2-dimensional Lebesgue measure.*

(ii) *The antisymmetric decomposition.* If A is a uniform algebra on X, a subset Y of X is called a *set of antisymmetry* if every function in A which is real-valued on Y is constant on Y. As example we may take X to be the solid cylinder $\{|z| \leq 1\} \times \{0 \leq t \leq 1\}$ and A to be the algebra of all continuous functions on X which are analytic on each slice: $t = t_0$, $|z| < 1$. Then each disk: $t = t_0$, $|z| \leq 1$ is a set of antisymmetry. For a general uniform algebra A on X Bishop showed in **[13]**: *Let* $\{Y_\alpha\}$ *be the family of all maximal sets of antisymmetry. Then the* Y_α *give a closed partition of* X *and a continuous function* f *on* X *belongs to* A *if and only if each restriction* $f|_{Y_\alpha}$ *belongs to the restriction* $A|_{Y_\alpha}$.

If the Y_α are the points of X, one recovers the Stone–Weierstrass theorem. A less complete result had been obtained earlier by Šilov, **[63]**. Bishop's result reduces the study of general uniform algebras to the study of *antisymmetric* such algebras, i.e., uniform algebras which contain no nonconstant real-valued function.

(iii) *Jensen measures.* The representing measure $\frac{d\theta}{2\pi}$ for the origin for the disk algebra $A(D)$ satisfies Jensen's inequality:

$$\log|f(0)| \leq \int_0^{2\pi} \log|f(e^{i\theta})| \frac{d\theta}{2\pi}$$

for each f in $A(D)$.

Let A be a uniform algebra and fix m in $\mathfrak{M}$. Can a representing measure μ be found for m which satisfies such an inequality? Arens and Singer had shown this to be true in certain cases. In **[15]** Bishop showed it in general: *Let* A *be a uniform algebra,* m *a point of* $\mathfrak{M}$. *There exists a representing measure* μ *for* m *such that*

$$\log|f(m)| \leq \int \log|f|\, d\mu$$

for each f *in* A.

Such a measure μ is called a *Jensen measure* for m. Jensen measures have turned out to be very useful.

6. Irving Glicksberg and Orthogonal Measures

Let A be a uniform algebra on the space X. A complex measure ν on X is called *orthogonal* to A if

$$\int f\, d\nu = 0 \text{ for every } f \text{ in } A.$$

We write $A^\perp$ for the family of all such measures. If we know $A^\perp$, then we can tell, using the Hahn–Banach theorem, whether a given function h in $C(X)$ belongs to A: $h \in A$ if and only if

$$\int h\, d\nu = 0 \text{ for each } \nu \text{ in } A^\perp.$$

The classical theorem of F. and M. Riesz identified all the orthogonal measures for the disk algebra. Frank Forelli's work in **[25]** gave a function-algebraic approach to this result.

In a series of papers [**30**], [**31**], [**32**], the last jointly with me, Glicksberg analyzed measures orthogonal to a uniform algebra. He applied his results to obtain elegant new proofs of Bishop's results on general uniform algebras, as well as to problems in interpolation, approximation, and so forth.

In [**10**] and [**12**] Bishop considered a compact set X in $\mathbb{C}$ with $\mathbb{C}\backslash X$ connected and looked at the measures ν on X orthogonal to the algebra $P(X)$. He showed that such a measure ν always arises from a certain analytic differential $g(z)dz$ on the interior of X. In [**32**] Glicksberg and I adapted these ideas to Dirichlet algebras. Let A be a Dirichlet algebra on a space X. For each m in $\mathfrak{M}$, let λ be the representing measure for m and let $H^1(\lambda)$, as earlier, denote the closure of A in $L^1(X,\lambda)$. If $k \in H^1(\lambda)$ and $\int k \cdot d\lambda = 0$, then $k \cdot d\lambda$ is orthogonal to A, since if f is in A,

$$\int f(k d\lambda) = \left[\int f \cdot d\lambda\right]\left[\int k \cdot d\lambda\right] = 0.$$

Hence we get "obvious" orthogonal measures for A by forming convergent series

$$\sum_i k_i \cdot d\lambda_i$$

where each λ_i is a representing measure and $k_i \in H^1(\lambda_i)$ and $\int k_i d\lambda_i = 0$. We showed that every complex measure ν in $A^\perp$ has a representation

$$\nu = \sum_i k_i \cdot d\lambda_i + \sigma, \tag{1}$$

with k_i, λ_i as above and such that σ is orthogonal to A and is singular with respect to every representing measure for A.

As an application, we took a compact plane set X with connected complement and boundary ∂X, and took $A = P(X)$. By the classical Walsh–Lebesgue theorem, $P(X)$ is a Dirichlet algebra on ∂X. In this case, one can show that every measure σ appearing in (1) vanishes. (1) then quickly implies Mergelyan's theorem on polynomial approximation on X mentioned earlier.

Lennart Carleson in [**23**] gave an ingenious new proof of the Walsh–Lebesgue theorem, and went on to give a proof of Mergelyan's theorem, also based on Bishop's ideas.

Irv Glicksberg was an unusual person. He was ever cheerful, with unlimited enthusiasm and unfailing generosity. He enjoyed every bit of good mathematics that he met up with, and it usually stimulated new ideas in him. He was a delightful, indefatigable correspondent, a fanatic photographer, and fond of jaunty headgear. Politically, he was a staunch liberal, and so he found plenty to get mad about in the last twenty years. On most other questions he had a tolerant point of view.

I was planning to spend a year at the University of Washington with him in 1983, when I was shocked to hear of his death.

7. Function Algebras at MIT and at Brown

In the late fifties and early sixties, Iz Singer and Ken Hoffman presided over a very fruitful mathematical activity at MIT. Their students during that time included Andrew Browder, Hugo Rossi, and Gabriel Stolzenberg, each of whom made important contributions to the study of Function Algebras.

Hoffman and Singer jointly in **[38]** answered a series of questions on Function Algebras which had been posed by Gelfand. In [37] and [39] they studied *maximal* uniform algebras on a space X, i.e., algebras A such that if B is a closed subalgebra of $C(X)$ which contains A, then either $B = A$ or $B = C(X)$.

A major open problem at that time was to prove a *local maximum modulus principle* for function algebras. If z_0 is a point in the domain of analyticity of a function F and U is a neighborhood of z_0, then $|F(z_0)| \leq \max |F|$ taken over the boundary of U. The corresponding statement for a uniform algebra A with maximal ideal space $\mathfrak{M}$ should be this: fix m in $\mathfrak{M}$ and let U be a neighborhood of m whose closure lies in $\mathfrak{M}\backslash\check{S}$. Then $|f(m)| \leq \max |f|$ taken over the boundary of U, whenever $f \in A$. *Is this true*? We all tried to prove this, but, lacking insight into several complex variables, we had no luck. At last Hugo Rossi showed how to do it. I remember the excitement of a late evening phone call, Singer to Rossi when Rossi was in Princeton, where he told us about his proof. The secret was a clever use of the solution of the Cousin problem in n complex variables. Rossi's paper on this is **[55]**, in 1960. Much of what has been found about uniform algebras since then has depended on this local maximum modulus principle.

The local maximum modulus principle, as well as various examples of maximal ideal spaces which had been worked out in the meantime, as well as Gleason's conjecture about parts, all encouraged an effort to prove, in general, the existence of analytic structure in $\mathfrak{M}\backslash\check{S}$. One way to test this question was to look at examples in n complex variables. Let X be a compact set in $\mathbb{C}^n$ and let $P(X)$ be defined as in example (i) in Section 2 above. The maximal ideal space $\mathfrak{M}$ of the uniform algebra $P(X)$ has a natural identification with the so-called *polynomially convex hull* $\widehat{X}$ of X, which had come up in the 1930s in the work of K. Oka, **[51]** and **[52]**. $\widehat{X}$ consists of all points $z^0 = (z_1^0, \ldots, z_n^0)$ in $\mathbb{C}^n$ such that

$$|Q(z^0)| \leq \max |Q| \text{ over } X$$

for every polynomial Q on $\mathbb{C}^n$.

If analytic structure exists on $\mathfrak{M}\backslash\check{S}$, then there must be complex analytic varieties contained in $\widehat{X}\backslash X$. Gabriel Stolzenberg in the winter of 1960–1961 constructed a set X on the boundary of the bi-disk: $|z_1| \leq 1$, $|z_2| \leq 1$ in $\mathbb{C}^2$ such that neither one of the coordinate projections $z_1(\widehat{X})$ and $z_2(\widehat{X})$ contains any open subset of the plane, while at the same time $\widehat{X}$ contains the point $(0,0)$ and hence is larger than X. Then $\widehat{X}$ contains no analytic variety, for else $\widehat{X}$ would contain some proper analytic disk Δ and then either $z_1(\Delta)$ or $z_2(\Delta)$ would have nonvoid interior [**65**].

Thus the hope for analytic structure in $\mathfrak{M}\backslash\check{S}$ in the general case was gone forever. It was a heavy blow. From the perspective of today, almost thirty years later, I should say that Stolzenberg's example taught us that the story of polynomially convex hulls is much subtler than we had thought, but that some satisfactory understanding of these hulls is starting to emerge at the present time.

Stolzenberg himself made other incisive studies of polynomially convex hulls in the sixties, in [**66**] and [**67**].

In addition to the people just mentioned, MIT had in this period a number of junior faculty and academic visitors working on Function Algebras and related matters. These included Stephen Fisher, Ted Gamelin, John Garnett, Eva Kallin, and Donald Wilken. There was lively interaction between the Analysis Seminar at Brown, run by Andy Browder and myself, and these MIT people. Hoffman and Singer were good friends of mine, and much of my own work arose from conversations with them and others of the group.

Once, after Ken Hoffman and I had finished a particularly long-lasting and noisy mathematical conversation at my home in Providence, my three-and-a-half-year-old son came into the room, waving his arms and spouting a stream of nonsense syllables. "I am talking mathematics!" he told us.

Among the Ph.D. students working on Function Algebras who wrote their theses at Brown were Andy Browder and Robert McKissick, both borrowed from MIT; further Mike Voichick, John O'Connell, Bernie O'Neill, and Richard Basener (my students), Al Hallstrom, Jim Wang, and Kenny Preskenis (Browder's students), and Tony O'Farrell (Brian Cole's student). Stu Sidney (Gleason's student) and Lee Stout (Rudin's student) were part of this same mathematical generation, as were Mike Freeman and Laura Kodama, Bishop's students. H. S. Bear (John Kelley's student) is in this group, and Barney Weinstock (Hoffman's student) came somewhat later. Larry Zalcman was an MIT graduate student in this period. He wrote the volume *Analytic Capacity and Rational Approximation* [**75**], which gave a very valuable exposition in English of the recent work of Vituškin and his school on the algebra $R(X)$, example (iii) in Section 2 above.

Andy Browder joined the Brown department in 1961. One result of Browder's thesis concerned the topology of *polynomially convex sets*, i.e., sets X which coincide with their polynomially convex hull: *let X be such a compact set in $\mathbb{C}^n$.* Then the kth Čech cohomology of X with complex coefficients vanishes for $k \geq n$ [**20**]. It follows in particular that if Y is a compact orientable n-manifold in $\mathbb{C}^n$, then $\widehat{Y}$ is larger than Y. Identifying the set of "new points" $\widehat{Y}\backslash Y$ has turned out to be a difficult problem, only partially solved even for 2-manifolds in $\mathbb{C}^2$.

Eva Kallin joined the Brown faculty in 1965. B. Weinstock and G. Stolzenberg also taught at Brown for some years in the sixties. Kallin had written her thesis at Berkeley, with J. L. Kelley, and in it she had solved the following famous problem: *if a function belongs locally to a uniform algebra A, must it belong to A?* More precisely, if A is a uniform algebra and if a function f continuous on $\mathfrak{M}$ has the property that each point m in $\mathfrak{M}$ has a neighborhood U such that $f|_U = F|_U$, for some F in A, does then f belong to A? Kallin **[43]** gave a counterexample. G. Šilov who had earlier published an erroneous proof of the result, sent her a congratulatory postcard. Another result of Kallin's concerned the "n balls problem": consider n closed disjoint balls $B_1, \dots, B_n$ in $\mathbb{C}^N$. Is their union polynomially convex? For $n = 1$ or 2 one sees at once that the answer is "Yes". For $n = 4$ the answer is unknown as of today. Kallin showed in **[44]** that the answer is "Yes" for $n = 3$.

One other major line of research at MIT at that time was Hoffman's work on the algebra H^∞ of bounded analytic functions on the unit disk. H^∞ is a uniform algebra. Its maximal ideal space, $\mathfrak{M}(H^\infty)$, is as mysterious a compact space as an analyst is likely to encounter. In his book *Banach Spaces of Analytic Functions,* which was published in 1962 by Prentice-Hall, Hoffman devoted Chapter 10 to H^∞ as a Banach algebra.

That book as a whole was a milestone. It showed to the world of classical analysts and to the world of functional analysts that they were brothers and sisters rather than strangers (as many had thought). One source of this recognition was for Hoffman, as it was for myself and many others, the towering figure of Arne Beurling who in his own work had combined classical and abstract analysis in essentially new ways.

One observation which Hoffman made was that H^∞ is *almost,* but not quite, a Dirichlet algebra on its Šilov boundary $\check{S}(H^\infty)$. For a uniform algebra A on a space X write $\log|A^{-1}|$ for the space of all functions $\log|f|$ such that f and f^{-1} both belong to A. Hoffman called A *logmodular* if $\log|A^{-1}|$ is uniformly dense in the real continuous functions on X. Dirichlet algebras are logmodular (trivially), but not conversely. Logmodular algebras still enjoy the property that representing measures for points in $\mathfrak{M}$ are unique. H^∞ is a logmodular algebra on $\check{S}(H^\infty)$. In **[35]** Hoffman developed the theory of logmodular algebras and showed that they enjoyed almost all the pleasant properties of Dirichlet algebras. In particular, their Gleason parts were either points or analytic disks. This last result raised the question of describing the Gleason parts of H^∞ explicitly. In the paper **[36]** Hoffman solved this very difficult problem, making use of the deep work of Lennart Carleson in **[21]** and Donald Newman in **[49]**.

H^∞ as a Banach algebra and, in particular, as a subalgebra of L^∞ on the circle has, since Hoffman's work, been the subject of intensive investigation. This theory is closely connected with the modern theory of bounded linear operators on a Hilbert space. An exposition of the work on H^∞ from a function-theoretic point of view is found in John Garnett's book *Bounded Analytic Functions*, published by Academic Press in 1981. In particular, this book treats the Corona Problem, mentioned in Section 3 above, including the remarkable new solution of the problem by Tom Wolff.

A further development in the abstract direction came in the work of Lumer in **[46]**, where Lumer makes as his only hypothesis on a uniform algebra the uniqueness of the representing measures for the points of $\mathfrak{M}$. Other extensions of this theory are given by P. Ahern and D. Sarason in **[1]** and by K. Barbey and H. König in **[8]**.

8. Yale

I taught at Yale from 1951 to 1954 and at Brown after that. Yale provided a superb environment for a young analyst. The senior people in analysis, Rickart, Kakutani, Dunford, and Hille were very active, friendly and encouraging, and the junior people, Jack Schwartz, Henry Helson, Bill Bade, Bob Bartle, Frank Quigley, and myself had a very lively time in the analysis seminar. We all taught calculus, Math 12, out of Ed Begle's book, which is based on the axioms of the real number system. The combination of axioms, Yalies, and ourselves made a heady brew. Our wives were sick of conversations about Math 12 which went on at all department parties. The normal teaching evaluation which each of us got from our freshmen was, "While undoubtedly a brilliant mathematician, Mr. X just can't get it across".

Rickart early on saw the possibilities of an abstract function theory in his papers **[53]**, **[54]**, etc., and through the work of his students. Talking with him and with his student John Holladay got me to thinking about Function Algebras. One day in early 1953 Rickart showed to Kakutani and me a recent paper by the Russian mathematician Leibenson, **[45]**, in which Leibenson raised the following question: Let Γ denote the unit circle $|z| = 1$ and let A denote the disk algebra, viewed as a subalgebra of $C(\Gamma)$. Suppose φ is a function in $C(\Gamma)$ which is not in A. Is the closed algebra generated by φ and A then all of $C(\Gamma)$? He showed that it was if φ is real or if φ satisfies a Lipschitz condition.

Some months earlier I had heard about an intriguing recent result of Rudin: given an algebra of functions continuous in the closed disk $|z| \leq 1$. *Suppose every F in the algebra attains the maximum of its modulus on the boundary $|z| = 1$. Then if one schlicht function belongs to the algebra, every F in the algebra is analytic in $|z| < 1$.*

I did not then know Rudin's proof and spent a week of hard work, making up a proof of Rudin's theorem. My proof was function-algebraic in spirit and rather more complicated than Rudin's own, in **[60]**. When I saw Leibenson's question, I realized that I could use similar function-algebraic arguments to answer it. I showed that *every closed subalgebra of* $C(\Gamma)$ *which contains* A *either equals* A *or equals* $C(\Gamma)$.

I now asked myself what other closed subalgebras of $C(\Gamma)$ have this property of being "maximal" in $C(\Gamma)$. Let us consider a simple closed curve γ on a Riemann surface which is a torus, such that γ bounds a region Ω on this torus. Then the boundary functions on γ of all functions analytic on Ω and continuous on $\Omega \cup \gamma$ make up such a maximal subalgebra of $C(\gamma)$. Also $C(\gamma) \cong C(\Gamma)$. Since Ω need not be of the type of the disk, we have a new maximal subalgebra of $C(\Gamma)$. It was clear then that one should prove that if Σ is any finite Riemann surface with nice boundary $\gamma = \partial\Sigma$, then the algebra $A(\Sigma)$ of functions analytic on $\Sigma \backslash \partial\Sigma$ and continuous on Σ is a maximal subalgebra of $C(\gamma)$. With the kind help of Maurice Heins at Brown, I proved this for the case that $\partial\Sigma$ is a single contour in **[70]**. Hal Royden proved the general case in **[57]**.

The algebra considered by Leibenson, generated by φ and A on Γ, evidently is generated by the two functions: φ and z. Let now φ and ψ be any two functions continuous on Γ which together separate points on Γ, and denote by $[\varphi, \psi]$ the closed subalgebra of $C(\Gamma)$ which they generate. Of course, $[\varphi, \psi]$ may equal $C(\Gamma)$, e.g. if $\varphi = \bar{z}$, $\psi = z$. Suppose that $[\varphi, \psi]$ is a proper subalgebra of $C(\Gamma)$. Then we might expect that Γ lies embedded as the boundary curve $\partial\Sigma$ of some finite Riemann surface Σ such that φ and ψ extend analytically from $\partial\Sigma$ to Σ. In that case $[\varphi, \psi]$ would contain only boundary functions of functions analytic on Σ, and hence be a proper subalgebra of $C(\Gamma)$.

I badly wanted to prove that this is what happens. In the case that φ and ψ are real-analytic on Γ and hence can be viewed as defined and analytic in a little annulus containing Γ, I finally did prove it by the end of 1956, in **[71]**.

One can look at this question geometrically, by considering the image X of Γ in $\mathbb{C}^2$ under the map (φ, ψ). Then X is a simple closed curve in $\mathbb{C}^2$ and the hypothesis that $[\varphi, \psi] \neq C(\Gamma)$ is equivalent to the statement that $P(X) \neq C(X)$. The desired conclusion, the existence of a finite Riemann surface in which Γ is embedded, then becomes the existence of a finite Riemann surface Σ in $\mathbb{C}^2$ having X as its boundary.

In this language, and replacing $\mathbb{C}^2$ by $\mathbb{C}^n$, the problem is then as follows. *Given a simple closed curve* X *in* $\mathbb{C}^n$ *with* $P(X) \neq C(X)$. *Show that there exists a finite Riemann surface* Σ *in* $\mathbb{C}^n$ *(possibly admitting singular points) which has* X *as its boundary.* One expects that the finite Riemann surface Σ equals $\widehat{X}$, the polynomially convex hull of X. All this turned out to be true, as long as the curve X has some regularity. I proved it when X is a single real-analytic curve, Stolzenberg did the case when X is the union of finitely many differentiable closed curves **[67]**, and Herbert Alexander **[2]** did the case when X is merely rectifiable. Bishop's ideas in **[14]** and **[15]** played an important role in this work.

One application of this theory of function algebras on the circle came in the work of Royden in **[59]** on the maximum principle for bounded analytic functions on an open Riemann surface.

Suppose now that we replace the circle Γ by the unit interval I and study the closed subalgebras of $C(I)$ which are uniform algebras on I. The corresponding geometric problem in $\mathbb{C}^n$ is to identify the polynomially convex hull of a Jordan arc in $\mathbb{C}^n$. When J is a regular Jordan arc, satisfying the same smoothness conditions we imposed on the closed curve X above, it turned out that $\widehat{J} = J$, i.e., J is polynomially convex. We expect this, since intuitively we feel that "J cannot bound anything". Furthermore, when J is a regular Jordan arc, $P(J) = C(J)$, i.e., every continuous function on J is a uniform limit on J of polynomials in $z_1, \dots, z_n$. The proof uses both the fact that J is polynomially convex, and that J is smooth, and was given by H. Helson and J. Quigley, in greater generality, in **[34]**.

However, when J is merely topologically a Jordan arc, i.e., homeomorphic to the interval I, J may fail to be polynomially convex. Examples of this were given by me for $n = 3$, **[69]**, and by Rudin for $n = 2$, **[61]**.

The Peak Point Conjecture and Cole's Thesis. As we saw in Section 5 above, Bishop had shown, for an arbitrary compact plane set X, that $R(X) = C(X)$ whenever each point of X is a peak point of $R(X)$. The Peak Point Conjecture was the statement that if A is a uniform algebra on X and if every point of $\mathfrak{M}$ is a peak point of A (in which case, of course, $\mathfrak{M}$ and X coincide), then $A = C(X)$. A related conjecture, due to Gleason, was the statement that $C(X)$ is characterized as a uniform algebra on X by the fact that each part of its maximal ideal space is a single point.

During the 1960s many people tried to settle these conjectures without success. In his remarkable thesis at Yale in 1968, Brian Cole (Rickart's student) disproved both of these conjectures. His procedure was to make repeated adjunction of square roots to a given uniform algebra A so as to end up with an algebra $\widetilde{A}$ which is such that every function in $\widetilde{A}$ has a square root in $\widetilde{A}$. The proof given by Cole may be found in the appendix to A. Browder's book mentioned above in Section 1. Cole's thesis settled a series of other questions as well, and stimulated much further work.

In particular, Richard Basener at Brown was able to modify Cole's construction so as to obtain a compact set X lying on the sphere $|z_1|^2 + |z_2|^2 = 1$ in $\mathbb{C}^2$ such that $R(X)$ provides another counterexample to the peak point conjecture. Here $R(X)$ denotes the uniform closure on X of rational functions in z_1, z_2 which are analytic on X.

Brian Cole joined the Brown department in 1969.

9. Function Algebras on Smooth Manifolds

Let X be a compact set in $\mathbb{C}^n$. Under what conditions on X does $P(X) = C(X)$? Since the maximal ideal space of $C(X)$ is X and the maximal ideal space of $P(X)$ is $\widehat{X}$, a necessary condition for this is that X be polynomially convex.

Suppose now that X is a compact smooth manifold in $\mathbb{C}^n$ with or without boundary, and that X is polynomially convex. Does it follow that $P(X) = C(X)$? As we saw in Section 8, the answer is "Yes" in the case that X is a circle or an arc.

Let k denote the real dimension of X. For $k \geq 2$, it is clear that a new condition enters. If for instance Y is the 2-dimensional disk: $z_1 = \lambda$, $z_2 = \lambda$, $|\lambda| \leq 1$ in $\mathbb{C}^2$, then $\widehat{Y} = Y$ and $P(Y)$ contains exclusively functions analytic on Y. To rule out such a situation, one may consider the tangent space T_x to X for each point x in X. T_x is a k-dimensional real subspace of $\mathbb{C}^n$. If X is a complex-analytic manifold, as in the example, or if merely X contains a complex-analytic submanifold passing through x, this will show up by the presence of a *complex-linear subspace of* $\mathbb{C}^n$ in T_x.

We call such a subspace a *complex tangent* to X at x, and we call the manifold X *totally real* if it has no complex tangents. In 1968–1969 a breakthrough occurred. It was shown, under various conditions of smoothness on X, and arbitrary k, that *if* Σ *is a smooth totally real manifold in* $\mathbb{C}^n$ *and* X *is a compact and polynomially convex subset of* Σ, *then* $P(X) = C(X)$.

The real subspace $\mathbb{R}^n$ of $\mathbb{C}^n$, consisting of all points $(x_1, \ldots, x_n)$ with all x_j real, is evidently a smooth totally real manifold, and every compact subset of $\mathbb{R}^n$ is polynomially convex. So one recovers the Weierstrass approximation theorem.

The above theorem was proved in R. Nirenberg and R. O. Wells, [**50**], L. Hörmander and J. Wermer, [**40**], and E. M. Čirka, [**24**], and the method of proof in these papers was based on Hörmander's solution of the $\overline{\partial}$-problem. Much further work on this problem, with weakened smoothness conditions and simpler, more elementary proofs, was done later on by Weinstock, Berndtsson, Harvey and Wells, and others.

I had earlier, in [**73**], proved the result for the case $k = 2$ when X is a 2-dimensional smooth disk, and M. Freeman, in [**26**], had settled the case of general smooth 2-manifolds. The method used by myself and by Freeman depended on the use of the Cauchy transform of a plane measure, and did not generalize to the case $k > 2$.

Bishop disks. Suppose that Σ is a smooth manifold which does have complex tangents. What then? If the dimension k of $\Sigma > n$, elementary linear algebra shows that Σ has complex tangents at every point. In his paper *Differentiable Manifolds in Complex Euclidean Space* in [**17**], Errett Bishop showed the following: *Assume $k > n$. Fix x in Σ and assume that the dimension of the largest complex-linear subspace of T_x is $k - n$. Then if U is any neighborhood of x on Σ, there exists an analytic disk in $\mathbb{C}^n$ whose boundary lies in U.*

Suppose now that X is a compact set lying on Σ which contains some open subset of Σ. It follows that $\widehat{X}$ contains a multitude of analytic disks whose boundaries lie in X. Every function in $P(X)$ is then analytic on each of these disks.

These "Bishop disks" have turned out to play an important role in the study of analytic continuation in several complex variables.

10. Hanover, N.H., Palo Alto, New Orleans, Yerevan

Many of us got together in the summers at a succession of meetings devoted at least in part to Function Algebras. The atmosphere was rather relaxed some of the time. The conference at Dartmouth College in Hanover was held in 1960. It was organized by Terry Mirkil et al. and was supported by the NSF. One weekday during the meeting Matt Gaffney showed up from Washington, representing the NSF, to see how the conference was going. At the Dartmouth math department he found *none* of the mathematicians, only one of the wives, looking for her husband. The rest of us were out in the lovely countryside. I myself was with Karel de Leeuw and Siggi Helgason on a sailboat on a lake. There were no dire consequences.

In 1961 there was a one-month conference in analysis at Stanford, under the auspices of the American Mathematical Society, and many of us were there and gave talks.

A conference fully devoted to Function Algebras was held at Tulane University in April 1965, organized by Frank Birtel et al. Most people interested in the subject attended, and a volume of the proceedings was published, *Function Algebras*, edited by F. Birtel, Scott-Foresman and Co. (1966).

In September, 1965 a number of us went to a big conference on analytic functions in the Soviet Union in Yerevan. We had a chance to meet and talk with many of the Russians who had similar interests, and I was very much struck by the warmth and friendliness of our hosts. The group around Shabat, Vitushkin, and Mergelyan was very active and doing fundamental work in approximation theory. It included E. Gorin, A. Gončar, E. M. Čirka, S. Melnikov, and E. P. Dolzhenko.

In those days the Russians were not party to an international copyright agreement, so they could freely translate foreign books into Russian. When an author came to the Soviet Union, he got his royalty for the translation in rubles. In this way Hoffman and I got some rubles. They had translated an article of mine so I got enough for one bottle of Armenian cognac and one fur hat. Hoffman's book had been translated, so he was amply supplied with rubles for cognac.

Gončar threw a party for many of us at his family home in Yerevan. The party was very high-spirited with singing, piano-playing and a greater density of liquor bottles on the table than I have ever seen. Of course many toasts were drunk. My toast to Mergelyan was "on your beautiful work which has inspired us all". V. P. Havin responded, with a toast to Mergelyan: "Your work has inspired not only the Americans."

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Addendum: Concepts and Categories in Perspective

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Doing mathematical research is known to be hard. Writing on the history of mathematics is not hard in the same way, but it is difficult. Part of the difficulty is that of picking the right things to bring out. As a small example of this sort of difficulty, I would like to observe now that my article in the first part of this series, under the title "Concepts and Categories in Perspective" seems to have missed some relevant perspectives.

My discussion of the role of "Theories" in §18 should have included the notion of a "sketch". A sketch S is a directed graph with a distinguished class of diagrams, as well as distinguished classes of cocones and cones (a cone consists of edges from a common vertex). A sketch S serves as a theory, if we take a model of S in a category A to be an assignment of an object of A for each vertex and an arrow of A for each edge, all so that all the distinguished diagrams become commutative in A, while the distinguished cones become limit cones and the cocones colimits in A. The notion of a Lawvere theory is a special case of a sketch. There are several variants of the definition, but the notion is due to Ehresmann (**MR 39** #278; A. Bastiani and C. Ehresmann **MR 48** #2211). At that time (1972) sketches did not attract much attention; Ehresmann published voluminously with many different elaborate definitions which were hard to sort out, especially because he and his students were hardly in touch with other workers in categories. His student C. Lair developed sketches further in his thesis (1970, **MR 53** #10888) and in later papers, such as Guitart and Lair (**MR 84h** #18012). The subsequent publication of Ehresmann's carefully annotated collected works (e.g., **MR 86i** #01059) has made his contributions more accessible. More recently the notion of a sketch was an essential part of the discussion of theories in the 1986 book by Barr–Wells (Toposes, Triples, and Theories, **MR 86f** #18001). By the efforts of John Gray and others, it now turns out that sketches are especially effective in aspects of computer science, partly because they are usually small and so are easy to put on a computer and more essentially because they readily handle many-sorted theories and their initial algebras, as used for data types.

This is a case in which a concept became relevant by way of its use in a different field.

My previous article reported the remarkable use of sheaves by Lawvere–Tierney to formulate Cohen's proof by forcing of the independence of the continuum hypothesis. I did not note that this is by no means the only case where toposes explicate forcing. Thus Marta Bunge showed (**MR 51** #2908) that this approach would also handle the famous Solovay–Tennenbaum proof by forcing (**MR 45** #3212) of the independence of the Suslin hypothesis. Then in 1980 Freyd used sheaves to get an amazingly simple new proof of the independence of the Axiom of Choice (**MR 82i** #03079). Subsequently, Scedrov has developed the connection of such forcing with classifying toposes in his memoir (**MR 86d** #03057). For some time I have thought that this connection with set theory calls for still further analysis of the relation between set-theoretic and sheaf-theoretic forcing, but I omitted to say so.

My article also tried to list the initial researches on categories in various countries—but I missed one such start (the first in the USSR?): An article by A. I. Mal'cev on "Defining relations in categories" (**MR 20** #3805). It was written shortly after his famous paper on the "Mal'cev operations", so evidently grew out of his interests in universal algebras.

This is by no means the only missed reference. My list of contributions from the "Swiss School" should certainly have included the work of F. Ulmer, as for example in his influential 1968 paper on "Properties of dense and relative adjoint functors" (**MR 36** #5190). Under the German school, I should add U. Oberst (1968), on the homology of categories (**MR 37** #1440). Walter Tholen has pointed out to me that I managed to hide the considerable activities in Rumania, where there was lively contact with mathematics in France. I had mentioned three Rumanians (I. Bucur, N. Popescu, and A. G. Radu) under the rubric "The Grothendieck School" and two more, M. Jurchescu and A. Lascu, under the heading "Eastern Europe". There should surely have been a separate title "Rumanian School", with these and additional entries such as

C. Bănică and N. Popescu (1963), Exactness of functions (**MR 33** #2700).

C. Năstăsescu and C. Nita (1965), Noetherian objects (**MR 34** #1374).

My most grievous omission is that of Yoneda. In the 1950s his work on homological algebra was right at the forefront, as I well knew from several contacts with him in Paris in 1955, and as in his basic 1954 paper on "Ext via long exact sequences" (**MR 16**, p. 947). Somewhere in this period, no one knows exactly where, he formulated the lemma stating that all the natural transformations from the hom functor $\hom(-, A)$ to a contravariant set-valued functor F are given by the elements of the set $F(A)$—a lemma which continues to play a basic role and which rightly bears his name. In these cases, as so often, the boundaries between fields (here between homological algebra and categories) are both indistinct and permeable.

History is difficult in part because the connections that matter are usually numerous, often hidden, and then subsequently neglected.