

the idea of "category" of sets. "Category" is a notion in a way parallel to but less quantitative than the measure of sets—that is, length, area, volume, and their generalizations. We quickly established some new results, and the fruits of our conversations during the first few months of our acquaintance were published as two notes in *Fundamenta*. We followed this with an ambitious attack on the problem of the existence of ergodic transformations. The ideas and definitions connected with this had been initiated in the nineteenth century by Boltzmann; five years before work on this had culminated in von Neumann's paper, followed (and in a way superseded) by G. D. Birkhoff's more imposing result. Birkhoff, in his trail-breaking papers and in his book on dynamical systems, had defined the notion of "transitivity." Oxtoby and I worked on the completion to the existence of limits in the ergodic theorem itself.

In order to complete the foundation of the ideas of statistical mechanics connected with the ergodic theorem, it was necessary to prove the existence, and what is more, the prevalence of ergodic transformations. G. D. Birkhoff himself had worked on special cases in dynamical problems, but there were no general results. We wanted to show that on every manifold (a space representing the possible states of a dynamical system)—the kind used in statistical mechanics—such ergodic behavior is the rule.

The nature, intensity and long duration of our daily conversations reminded me of the way work had been done in Poland. Oxtoby and I usually sat in my room, which was rather stark, although I had rented a couple of oriental rugs to furnish it, or in his own, which was even more spartan.

We discussed various approaches to a possible construction of these transformations. With my usual optimism, I was somehow sure of our ultimate success. We kept G. D. Birkhoff informed of the status of our attacks on the problem. He would smile when I talked to him at dinner at the Society of Fellows, partly amused, partly impressed by our single-minded persistence, and partly skeptical, though he really

Harvard Years

1936-1939

ICAME to the Society of Fellows during its first few years of existence. Garrett Birkhoff and B. F. Skinner, the psychologist, were among its original members. Most of the Junior Fellows, as we were called, were in their mid-twenties, mainly budding post-doctoral scholars.

I was given a two-room suite in Adams House, next door to another new fellow in mathematics by the name of John Oxtoby. About my age, he did not have his doctor's degree but was well known at the University of California—where he had done his graduate work—for his brilliance and promise. I took an instant liking to him. He was a tallish, blue-eyed redhead, with a constant good disposition. An attack of polio in his high-school years had severely crippled one leg, so that he had to walk with a crutch.

He was interested in some of the same mathematics I was: in set theoretical topology, analysis, and real function theory. Right off, we started to discuss problems concerning

cated and what seemed to me occasionally rather precious discussions. Another foreign-born member was George Hanfmann, an archaeologist. Hanfmann was obviously a very learned person, and I appreciated his erudition. We shared the same fondness for Greek and Latin literature.

The logician Willard Quine was friendly and outgoing. He was interested in foreign countries, their culture and history, and knew a few words of Slavic languages, which he used on me with great gusto. He already had made a reputation in mathematical logic. I remember him as slim, dark-haired, dark-eyed—an intense person. During the presidential election of 1936 in which Franklin D. Roosevelt defeated Landon, I met him on the stairs of Widener Library at nine in the morning, after Roosevelt's landslide victory. We stopped to chat and I asked him: "Well, what do you think of the results?" "What results?" he replied. "The presidential election, of course," I said. "Who is President now?" he asked casually. This was characteristic of many in academe. I once heard that, during Charles W. Eliot's presidency at Harvard, a visitor to his house was told, "The President is away in Washington to see Mr. Roosevelt"! (This was Theodore Roosevelt.)

I had my meals at Adams House, and the lunches there were particularly agreeable. We sat at a long table—young men and sometimes great professors; the conversations were very pleasant. But often, towards the end of a meal, one after the other would gulp his coffee and suddenly announce: "Excuse me, I've got to go to work!" Young as I was I could not understand why people wanted to show themselves to be such hard workers. I was surprised at this lack of self-assurance, even on the part of some famous scholars. Later I learned about the Puritan belief in hard work—or at least in appearing to be doing hard work. Students had to show that they were conscientious; the older professors did the same. This lack of self-confidence was strange to me, although it was less objectionable than the European arrogance. In Poland, people would also pretend and fabricate stories, but in

ADVENTURES OF A MATHEMATICIAN

had an open mind about our chances. He would check what I told him with Oxtoby, a more cautious person. It took us more than two years to break through and to finish a long paper, which appeared in *The Annals of Mathematics* in 1941 and which I consider one of the more important results that I had a part in.

The chairman of the Society was L. J. Henderson, a famous biologist, author of a book, *The Fitness of Environment*, which enjoyed a great popularity at the time, not only among specialists, but quite generally. L.J., as he was called, was a great Francophile. Indeed, the Society was molded along the lines of the Fondation Thiers in Paris, rather than on the Cambridge or Oxford systems of fellows in the college.

The Society was composed of some five or six Senior Fellows and about twenty-two Junior Fellows.

The Senior Fellows were well-known distinguished professors, like John Livingston Lowes in literature, Samuel Eliot Morison, the historian, Henderson, and Alfred North Whitehead, the famous English philosopher, who had already retired from his professorship at Harvard when I entered the Society. I often had the pleasure of sitting next to him at the traditional Monday-night dinners of the Society.

Some of the Junior Fellows gave me the impression of being a somewhat precious group of young men, as far as manners were concerned. Oxtoby, Willard Quine (really a logician), and I were the only mathematicians among them. Among the physicists there were several who later became very well known, such as John Bardeen, Ivan Gettling, and Jim Fisk. Among the biologists, I remember Robert B. Woodward, the chemist who first synthesized quinine and other important biological substances. Paul Samuelson, the economist who served as advisor to President Kennedy, was there; also Ivar Einerson, a great scholar in linguistics; Henry Guerlach, who became a historian of science; and Harry Levin, in English literature. Levin was rather proustian in his manner. He loved to engage in sophisti-

University seemed relatively untouched by this cataclysm. After the colloquium talk I gave there just before my appointment to the Society of Fellows, I remember Professor William Graustein telling me that at Harvard the professors had not felt the depression at all. This left me wondering at their lack of involvement in the general problems of the country or in the affairs of Massachusetts or even of Cambridge. It was evident that campus life in America meant at least partial isolation from the rest of society. Professors lived almost entirely among themselves and had very little contact with the rest of the professional or creative community as in Lwów. This had both good and bad effects: more time for scholarly work, but very little influence on the life of the country or vice versa. As everyone knows, things changed somewhat after World War II. In the Kennedy administration, for example, Harvardians had a great deal to do with the affairs of government and for a time the influence of scientists became even paramount.

Activities at the Society of Fellows were of course only one facet of my life at Harvard. I had many contacts with the younger members of the faculty at the university and quite often saw and talked with the senior professors and with G. D. Birkhoff himself. His son Garrett, a tall, good-looking, and brilliant mathematician, some two years younger than I, became a friend, and we saw each other nearly every day.

Even though membership in the Society did not require teaching of any kind, Professor Graustein asked me to teach an elementary undergraduate section of a freshman course called Math 1A. (It may even be that the late President Kennedy was for a while a student in this class. I remember a name like that and someone saying that the young man was a rather remarkable person. He left to go abroad in the middle of the term. Years later when I met President Kennedy I forgot to ask him whether he had really taken that course.)

I had given talks and seminars, but not yet taught a regular class, and I found this teaching interesting. The rule for young instructors was to follow very closely the prescribed

the opposite sense. They might have been working frantically all night, but they pretended they never worked at all. This respect for work appeared to me as part of the Puritan emphasis on action versus thought, so different from the aristocratic traditions of Cambridge, England, for example.

The Society's rooms were in Eliot House. We Junior Fellows would meet there on Mondays and Fridays for lunch, and for the famous Monday-night dinners which gathered Junior and Senior Fellows together around a long T-shaped table which was said to be the one featured in Oliver Wendell Holmes' *Autocrat of the Breakfast-Table*. Henderson had secured it from some Harvard storeroom.

President Lowell attended almost every Monday dinner. He was fond of re-creating the Battle of Jutland of World War I, moving knives and forks and saltcellars around on the dinner table to show the positions of the British and German fleets. From time to time he would also betray his doubts and even remorse about the Sacco and Vanzetti case. He would recount it—not so much to defend but rather to restate the position of the court and the subsequent legal steps. He had been a member of one of the review committees.

Good French Burgundies or Alsatian wines accompanied the meals. These were the pride and joy of Henderson, who once told me that if he ever deserved a statue in Cambridge, he would like to be put in Harvard Square with a bottle of wine in his hands, in commemoration of his having been the first person to obtain University funds for a wine cellar. George Homans, one of the Junior Fellows, a descendant of President John Adams, was one of the young men entrusted with the selection and sampling of wines. I considered it a great distinction when I, too, was put on the wine-tasting committee of the Society. This was my very first administrative job in America! The Society is still very much alive today at Harvard, and it continues to hold its Monday-night dinners where former fellows are always welcome.

In 1936 the depression appeared to be ending. Harvard

known ones and to comment on their relative merits. On the whole, it is a harmless if somewhat futile pastime.

I remember that at the age of eight or nine I tried to rate the fruits I liked in order of "goodness." I tried to say that a pear was better than an apple, which was better than a plum, which was better than an orange, until I discovered to my consternation that the relation was not transitive—namely, plums could be better than nuts which were better than apples, but apples were better than plums. I had fallen into a vicious circle, and this perplexed me at that age. Mathematicians' ratings are something like this.

Many mathematicians are also sensitive about what they consider their most beautiful mental offspring—results or theorems—and they tend to be possessive about them. Paradoxically, they also show a tendency to consider their own work as difficult and other work as easier. This is exactly opposite in other fields where the better acquainted one is with something the easier it seems.

Mathematicians are also prone to disputes, and personal animosities between them are not unknown. Many years later, when I became chairman of the mathematics department at the University of Colorado, I noticed that the difficulties of administering N people was not really proportional to N but to N^2 . This became my first "administrative theorem." With sixty professors there are roughly eighteen hundred pairs of professors. Out of that many pairs it was not surprising that there were some whose members did not like one another.

Among the Harvard mathematicians I knew, I should mention Hassler Whitney, Marshall Stone, and Norbert Wiener. Whitney was a young assistant professor, interesting not only as a mathematician. He was friendly, but rather taciturn—psychologically of a type one encounters in this country more frequently than in central Europe—with wry humor, shyness but self-assurance, a probity which shines through, and a certain genius for persistent and deep follow-through in mathematics.

textbook. Apparently I did not do too badly, for in an evaluation of teachers the student newspaper praised me as an interesting instructor. Soon after the beginning of the course, G. D. Birkhoff came to inspect my performance. Perhaps he wanted to check my English. He sat at the back of the room and watched as I explained to the students how to write equations of parallel lines in analytic geometry. Then I said that next we would study the formulae for perpendicular lines, which, I added, were "more difficult." After the lecture Birkhoff came to me and commented, "You've done very well, but I would not have said that perpendicular lines are more difficult." I replied that I believed on the contrary that students would remember better this way than if I said everything was easy. Birkhoff smiled at this attempt at pedagogy on my part. I think he liked my independence and outspoken ways, and I saw him rather frequently.

Shortly after I arrived in Cambridge, he had invited me to dine at his house. It was my first introduction to strange dishes like pumpkin pie. After dinner, which was pleasant enough, I got ready to leave and G. D. took my overcoat to help me into it. This sort of courtesy was unheard of in Poland; an older man would never have helped a much younger one. I remember blushing crimson with embarrassment.

I frequently ate lunch with his son Garrett, and we often took walks together. We talked much about mathematics and also indulged in the usual gossip that mathematicians love. Surely it is a shallow theme to evaluate how good X or Y is, but it is a characteristic of our tribe. The reader may have noticed that I practice this, too. Mathematics being more in the nature of an art, values depend on personal tastes and feelings rather than on objective factual notions. Mathematicians tend to be rather vain—though less so than opera tenors or artists. But as every mathematician knows some special bit of math better than anyone else, and math is such a vast and now more and more specialized subject, some like to propose linear orders of "class" among the better-

Marshall Stone, whom I had met when he came through Warsaw with von Neumann and Birkhoff in 1935 on the way back from the Moscow Congress, had had a meteoric career at the university, although he was only thirty-one years old. Already a full professor, he was quite influential in the affairs of the department and of the university for that matter. He wrote a classic work, a comprehensive and authoritative book on Hilbert space, an infinitely dimensional generalization of the three-dimensional or n -dimensional Euclidean space, mathematically basic to modern quantum theory in physics. He was the son of Harlan Stone, Chief Justice of the Supreme Court. It is said that his father proudly said of Marshall's mathematical achievements, "I am puzzled but happy that my son has written a book of which I understand nothing at all."

And there was Norbert Wiener! I met him at a colloquium talk I gave during my first year at Harvard. I was lecturing on some problems of topological groups, and mentioned a result I had obtained in Poland in 1930 on the impossibility of completely additive measure defined in all subsets of a given set. Wiener, who always sat at lectures in a semi-somnolent state except when he heard his name (at which he would suddenly jump up, then sit back in a very comical way) interrupted me to say, "Oh! Vitali has proved something like that already." I replied that I knew Vitali's result and that it was much weaker than mine because it required an additional property—namely equality for congruent sets—whereas my result did not make any such postulate and was a much stronger, purely set theoretical proof. After the lecture he came to me, apologized, and agreed with my statements. This was the beginning of our acquaintance.

I had heard of Wiener before this meeting, of course, not only about his mathematical wizardry, his work in number theory, his famous Tauberian theorems, and his work on Fourier Series, but also about his eccentricities. In Poland, I had heard through Jozef Marcinkiewicz about his book with

Paley on the summability of Fourier transforms. Raymond Paley, one of the most promising and successful young English mathematicians, died in a mountaineering accident at a tragically young age. Marcinkiewicz was a student of Antoni Zygmund. He visited Lwów as a post-doctoral fellow and patronized the Scottish Café, where we discussed Wiener's work, since he had worked in trigonometric series, trigonometric transforms, and summability problems. Marcinkiewicz, like Paley, whom he resembled in genius and in mathematical interests and accomplishments, reportedly was killed while an officer in the Polish army in the 1939 campaign at the beginning of World War II.

Absentminded and otherworldly in appearance, Wiener nevertheless could make an intuitive appraisal of others, and he must have been interested in me. Great as the difference in age between us was (his forty to my twenty-six years), he would seek me out occasionally in my little apartment in Adams House, sometimes late in the evening, and propose a mathematical conversation. He would say, "Let's go to my office, where I can write on the blackboard." This suited me better than staying in my rooms, from which it would have been difficult to put him out without being rude. So he drove me in his car through darkened streets to MIT, opened the building doors, turned on the light, and he started talking. After an hour or so, although Wiener was always interesting, I would almost fall asleep and finally manage to suggest that it was time to go home.

Wiener seemed childish in many ways. Being very ambitious about his place in the history of mathematics, he needed constant reassurance about his creative ability. I was almost stunned a few weeks after our first encounter when he asked me point blank: "Ulam! Do you think I am through in mathematics?" Mathematicians tend to worry about their diminishing power of concentration much as some men do about their sexual potency. Impudently, I felt a strong temptation to say "yes" as a joke, but refrained; he would not have understood. Speaking of that remark, "Am I

through," several years later at the first World Congress of Mathematicians held in Cambridge, I was walking on Massachusetts Avenue and saw Wiener in front of a bookstore. His face was glued to the window and when he saw me, he said, "Oh! Ulam! Look! There is my book!" Then he added, "Ulam, the work we two have done in probability theory has not been noticed much before, but see! Now, it is in the center of everything." I found this disarmingly and blessedly naive.

Anecdotes about Wiener abound; every mathematician who knew him has his own collection. I will add my story of what happened when I came to MIT as a visiting professor in the fall of 1957. I was assigned an office across the hall from his. On the second day after my arrival, I met him in the corridor and he stopped me to say, "Ulam! I can't tell you what I am working on now, you are in a position to put a secret stamp on it!" (This presumably because of my position in Los Alamos.) Needless to say, I could do no such thing.

Wiener always had a feeling of insecurity. Before the war he used to talk about his personal problems to J. D. Tamarkin, who was a great friend of his. When he was writing his autobiography, he showed a voluminous manuscript to Tamarkin. Tamarkin, whom I had met in 1936 and with whom I became quite friendly, told me about Wiener's manuscript and how interesting it was. But he also expressed the opinion that Wiener might be sued for libel for many of his outspoken statements. He spoke almost with disbelief about Wiener's text and how he tried to dissuade him from publishing the book in that form. What finally appeared apparently was considerably toned down from the original version.

Another memory I have of Wiener concerns his asking me to go with him to South Station in Boston to meet the English mathematician G. H. Hardy who was coming to the States for a visit. He knew I had met Hardy in England. We collected another mathematician, perhaps it was Norman Levinson, and picked up Hardy at the train. Wiener, who

prided himself on his knowledge of the Chinese, their culture and even their language, invited everybody for lunch at a Chinese restaurant. Immediately he started talking Chinese to the waiter, who seemed not to understand a word. Wiener simply remarked, "He must be from the south and does not speak Mandarin." (We were not quite convinced that this was the complete explanation.) It was a very pleasant lunch with much mathematical talk. And after lunch Wiener who had picked up the check discovered that he had no money. Fortunately we found the few necessary dollars in our pockets. Wiener scrupulously reimbursed us later.

It was said that Wiener, although he considered his professorship at MIT quite satisfactory, was very disappointed that Harvard never offered him a post. His father had been a professor at Harvard, and Norbert wanted very much to follow in his footsteps.

Although G. D. Birkhoff was at least ten years his senior, Wiener felt a rivalry with him and wanted to equal or surpass him in mathematical achievement and fame. When Birkhoff's celebrated ergodic theorem proof was published, Wiener tried very hard to go him one better and prove an even stronger theorem. He did manage it, but the strengthening was not as simple or as fundamental as G.D.'s original proof. Here again is an example of the competitive nature of some mathematicians and the sources of their ambition.

I think Wiener had marvelous talents as a mathematician—that is perspicacity and technical genius. He had a supreme general intelligence but, in my opinion, not the spark of originality which does the unusual unrelated to what others have done. In mathematics, as in physics, so much depends on chance, on a propitious moment. Perhaps von Neumann also lacked some of the "irrational," though with his wonderful creativity, he certainly went to and achieved the limits of the "reasonable."

There are several ways in which Wiener and von Neumann intersected in their interests and in their feelings

about what was important both in pure mathematics and its applications, but it is difficult to compare their personalities. Norbert Wiener was a true eccentric and von Neumann was, if anything, the opposite—a really solid person. Wiener had a sense of what is worth thinking about, and he understood the possibilities of using mathematics for seemingly more important and more visible applications in theoretical physics. He had a marvelous technique for using Fourier transforms, and it is amazing how much the power of algorithms or symbolism could accomplish. I am always amazed how much a certain facility with a special and apparently narrow technique can accomplish. Wiener was a master at this. I have seen other mathematicians who could do the same in a more modest way. For instance, Steinhaus obtained quite penetrating insights into other fields, and his student, Mark Kac, now at Rockefeller University, surpassed him. Antoni Zygmund in Chicago, another Pole, is a master of the great field of trigonometric series. Several of his students have obtained epoch-making results in other fields—for example, Paul Cohen, who did this in set theory, the most general and abstract part of mathematics.

I don't think Wiener was particularly fond of combinatorial thinking or of working on foundations of mathematico-logical or set theoretical problems. At the beginning of his career, he may have gone in this direction, but later he applied himself to other fields and to number theory.

Von Neumann was different. He also had several quite independent techniques at his fingertips. (It is rare to have more than two or three.) These included a facility for symbolic manipulation of linear operators. He also had an undefinable "common sense" feeling for logical structure and for both the skeleton and the combinatorial superstructure in new mathematical theories. This stood him in good stead much later, when he became interested in the notion of a possible theory of automata, and when he undertook both the conception and the construction of electronic computing machines. He attempted to define and to pursue some of the

formal analogies between the workings of the nervous system in general and of the human brain itself, and the operation of the newly developed electronic computers.

Wiener, somewhat hemmed in by the childishness and naiveté of his personality, was perhaps psychologically handicapped by the fact that, as a child, his father had pushed him as a prodigy. Von Neumann, who also began rather young, had a much wider knowledge of the world and more common sense outside the realm of pure intellect. Furthermore, Wiener was perhaps more in the tradition of talmudistic Judaic scholarship, even though his opinions and beliefs were very libertarian. This was quite conspicuously absent from von Neumann's makeup.

Johnny's overwhelming curiosity included many fields of theoretical physics, beginning with his pioneering work—his attempt to form a rigorous mathematical basis for quantum theory. His book, *Die Mathematische Grundlagen der Quantum Mechanik*, published over forty years ago, is not only a classic, but still the "bible" on the subject. He was especially fascinated by the puzzling role of the Reynolds number and the seeming mystery of sudden onsets of turbulence in the motions of fluids. He had discussions with Wiener on the perplexing values of this number which is "dimensionless"—a pure number expressing the ratio of the inertial forces to the viscous forces. It is of the order of two thousand, a large number. Why is this so and not around one, or ten, or fifty? At that time, Johnny and I came to the conclusion that actual detailed numerical computations of many special cases could help throw light on the reasons for the transition from a laminar (regular) to a turbulent flow.

He told me of another discussion he had with Wiener and their different points of view: Johnny advocated, in order to establish models for the working of the human brain, a numerical digital approach through a sequence of time steps, while Wiener imagined continuous or "hormonal" outlines. The dichotomy between these points of view is still of great interest and, of course, by now has been trans-

formed and deepened by the greater knowledge of the anatomy of the brain and by more work in the theory of automata.

The relationship between G. D. Birkhoff and von Neumann was curious. Birkhoff did not really have complete admiration for or appreciation of von Neumann's genius. He probably could not appreciate the many kinds of mathematics von Neumann was pursuing. He admired his technical brilliance, but G.D.'s tastes were more classical, in the tradition of Poincaré and the great French school of analysis. Von Neumann's interests were different. Birkhoff had ambitions to produce something of great importance in physics, and he made a few technically interesting but not conceptually important contributions to the general theory of relativity. He lectured several times on such subjects in Mexico, stimulating a small school of relativists there. Von Neumann's interests lay in the foundations of the new quantum theory's more recent developments. Theirs were differences of interests, of approaches, and of value systems. Birkhoff appreciated probing in depth more than exploring in breadth. Von Neumann, to some extent, did both. There was, of course, about a quarter-century's difference in age between them, as well as in background and in upbringing. Also, von Neumann never quite forgave G.D. for having "scooped" him in the affair of the ergodic theorem: Von Neumann had been first in proving what is now called the weak ergodic theorem. By a sheer virtuoso kind of combinatorial thinking, Birkhoff managed to prove a stronger one, and—having more influence with the editors of the *Proceedings* of the National Academy of Sciences—he published his paper first. This was something Johnny could never forget. He sometimes complained about this to me, but always in a most indirect and oblique way.

In addition to the elementary mathematics courses which I taught during my first year in the Society, I was asked to add advanced courses gradually. I liked this, for the best way to learn a subject is to try to teach it systematically.

Then one gets the real points, the essentials. One was an important undergraduate course in classical mechanics, Math 4 if I remember its former name. Another was Math 9, a course on probability.

At the time I had no precise idea what grades meant: A, B, C, D, or F. But I had rigid standards. I remember an otherwise quite good student, who protested receiving the grade of "C." Some other professors intervened, but I stubbornly, perhaps foolishly, stood my ground. Now I tend to be more lenient, and when I give a "C" or "D" the students really deserve an "F" or worse!

Tamarkin, who was a professor at Brown University, asked me to teach a graduate course in his place while he took his sabbatical leave for a term. I decided to give the course on the theory of functions of several real variables. It included a lot of new material—much of it my own recent work—and I was rather proud of it. Every Friday I went to Providence by train, taught the course, spent the weekend with Tamarkin at his home, returning to Cambridge on Sunday. When I mentioned the contents of the course to Mazur when I went home to Lwów for the last time during the summer of 1939, he liked it very much. He liked the material, the way it was organized, and said he would love to give such a course himself, all of which pleased me and encouraged me.

Tamarkin was a most interesting person. He was of medium height, very portly—I would say some thirty pounds overweight. He was quite nearsighted, a constant cigar-and-cigarette-smoker, and generally extremely jovial. As I got to know him better, I discovered the wonderful qualities of his mind and character.

Before World War I, he had written some mathematical research papers on the work of G. D. Birkhoff and even improved some of the latter's results a bit, which led to a certain animosity in their relations. Yet when he came to the United States, Birkhoff helped him secure his position at

ADVENTURES OF A MATHEMATICIAN

Brown, which had a notable mathematics faculty, including James Richardson, Raymond C. Archibald, and others. Richardson was a gentleman of the old school. Archibald was an eminent historian of mathematics, who established the famous mathematical library of Brown, one of the best in the country.

Tamarkin was interested in Polish-style mathematics and had heard about some of my results in the theory of Banach spaces. He had a quality which perhaps only a small number of mathematicians possess: he was extremely interested in the works of others and less egocentric than most. He was also interested in what was going on in other fields besides his own, whereas most mathematicians—even the best ones—are often deeply immersed in their own work and do not pay much attention to what others around them are doing. Tamarkin befriended me and encouraged me in my work.

He was Russian, not of Jewish origin exactly, but a Karaite. The Karaites were a sect of Semitic people not subject to the usual restrictions on Jews in Russia, the reason being that they claimed they were absent from Palestine when Jesus was condemned to death, and this exempted them. This claim was accepted by the Russian governors. They also had something in common with the ancient Khazars, people of a mysterious sixth- or seventh-century kingdom in southern Russia, a pagan tribe whose king decided to adopt a new religion. He selected Judaism after having asked Christian, Moslem, and Jewish representatives to explain their beliefs. Tamarkin believed he was one of their descendants. He had escaped from Leningrad after the Russian Revolution in a manner not unlike that of George Gamov some ten years later—over the ice of Lake Ladoga to Finland.

While I was at Harvard, Johnny came to see me a few times, and I invited him to dinner at the Society of Fellows. We would also take automobile drives and trips together

HARVARD YEARS

during which we discussed everything from mathematics to literature and talked without interruption while still paying attention to our surroundings. Johnny liked this kind of travel very much.

Once at Christmas time in 1937, we drove from Princeton to Duke University to a meeting of the American Mathematical Society. On the way, among other things we discussed the effect that the arrival of increasingly large numbers of refugee European scientists would have on the American academic scene. We stopped at an inn where we found a folder describing a local Indian Chief, Tomo-Chee-Chee, who apparently had been unhappy about the arrival of white men. As an illustration of our frequently linguistic and philological jokes, I asked him why it was that the Pilgrims had “landed” while the present European immigrants and scientific refugees merely “arrived.” Johnny enjoyed the implied contrast and used this in other contexts as an example of an implied value judgment. We also likened G. D. Birkhoff’s increasing qualms about the foreign influence to the Indian Chief’s. Continuing our drive, we managed to lose our way a couple of times and joked that it was Chief Tomo-Chee-Chee who had magically assumed the shape of false road signs to lead us astray.

This was the first time I visited the South, and I was much taken by the difference in atmosphere between New York, New England, and the southern states. I remember a feeling of “*déjà vu*”: the more polished manners, the more leisurely pace of life, and the elegant estates. Something seemed familiar, and I wondered what it was. Suddenly I asked myself if it could be the remnants of the practice of slavery, which reminded me of the traces of feudalism still visible in the country life of Poland. I was also surprised to see so many black people, and their language intrigued me. At a gas station, one of the Negro attendants said, “What would you like now, Captain?” I asked Johnny, “Does he think I might be an officer and calls me Captain as a compli-

ment?" Similarly, the first time I heard myself called "Doc," I wondered how the porter knew that I had a Doctor's degree!

As we passed the battlefields of the Civil War, Johnny recounted the smallest details of the battles. His knowledge of history was really encyclopedic, but what he liked and knew best was ancient history. He was a great admirer of the concise and wonderful way the Greek historians wrote. His knowledge of Greek enabled him to read Thucydides, Herodotus, and others in the original; his knowledge of Latin was even better.

The story of the Athenian expedition to the island of Melos, the atrocities and killings that followed, and the lengthy debates between the opposing parties fascinated him for reasons which I never quite understood. He seemed to take a perverse pleasure in the brutality of a civilized people like the ancient Greeks. For him, I think it threw a certain not-too-complimentary light on human nature in general. Perhaps he thought it illustrated the fact that once embarked on a certain course, it is fated that ambition and pride will prevent a people from swerving from a chosen course and inexorably it may lead to awful ends, as in the Greek tragedies. Needless to say this prophetically anticipated the vaster and more terrible madness of the Nazis. Johnny was very much aware of the worsening political situation. In a Pythian manner, he foresaw the coming catastrophe.

It was during this trip also that for the first time I sensed that he was having problems at home. He exhibited a certain restlessness and nervousness and would frequently stop to telephone to Princeton. Once he came back to the car very pale and obviously unhappy. I learned later that he had just found out that his marriage to Marietta was definitely breaking up. She would leave him shortly thereafter to marry a younger physicist, one of the frequent guests at the numerous parties which the von Neumanns gave in Princeton.

On the way back from the meeting I posed a mathematical problem about the relation between the topology and the purely algebraic properties of a structure like an abstract group: when is it possible to introduce in an abstract group a topology such that the group will become a continuous topological group and be separable? "Separable" means that there exists a countable number of elements dense in the whole group. (Namely, every element of the group can be approximated by elements of this countable set.) The group, of course, has to be of power continuum at most—obviously a necessary condition. It was one of the first questions which concern the relation between purely algebraic and purely geometric or topological notions, to see how they can influence or determine each other.

We both thought about ways to do it. Suddenly, while we were in a motel I found a combinatorial trick showing that it could not be done. It was, if I say so myself, rather ingenious. I explained it to Johnny. As we drove Johnny later simplified this proof in the sense that he found an example of a continuum group which is even Abelian (commutative) and yet unable to assume a separable topology. In other words, there exist abstract groups of power continuum in which there is no possible continuous separable topology. What is more, there exist such groups that are Abelian. Johnny, who liked verbal games and to play on words, asked me what to call such a group. I said, "nonseparabilizable." It is a difficult word to pronounce; on and off during the car ride we played at repeating it.

Mathematicians have their own brand of "in" humor like this. Generally speaking, they are amused by stories involving triviality of identity of two definitions or "tautologies." They also like jokes involving vacuous sets. If you say something which is true "in vacuo," that is to say, the conditions of the statement are never satisfied, it will strike them as humorous. They appreciate a certain type of logical non sequitur or logical puzzle. For instance, the story of the Jewish mother who gives a present of two ties to her son-in-law.

ADVENTURES OF A MATHEMATICIAN

The next time she sees him, he is wearing one of them, and she asks, "You don't like the other one?"

Some of von Neumann's remarks could be devastating, even though the sarcasm was of an abstract nature. Ed Condon told me in Boulder of a time he was sitting next to Johnny at a physics lecture in Princeton. The lecturer produced a slide with many experimental points and, although they were badly scattered, he showed how they lay on a curve. According to Condon, von Neumann murmured, "At least they lie on a plane."

Some people exhibit an ability to recall stories and tell them to others on appropriate occasions. Others have the ability to invent them by recognizing analogies of situations or ideas. A third group has the ability to laugh and enjoy other people's jokes. I sometimes wonder if types of humor could be classified according to personality. My friends and collaborators, C. J. Everett in the United States and Stanislaw Mazur in Poland, each had a wry sense of humor, and physically and in their handwriting they also resemble each other.

Generally von Neumann preferred to tell stories he had heard; I like to invent them. "I have some wit; it is a tremendous quality," my wife says I once told her. When she pointed out that I was bragging, I promptly added, "True. My faults are infinite, but modesty prevents me from mentioning them all."

In addition to "in" jokes, mathematicians also practice a form of "in" language. For example, they use the word "trivial." It is an expression they are very fond of, but what does it really mean? Easy? Simple? Banal? A colleague of my friend Gian-Carlo Rota once told him that he did not like teaching calculus because it was so trivial. Yet, is it? Simple as it is, calculus is one of the great creations of the human mind, with beginnings dating back to Archimedes. It was "invented" by Newton and Leibnitz, and amplified by Euler, Lagrange, and others. It has a beauty and an impor-

HARVARD YEARS

tance going far beyond most of the mathematics of our present culture. So what is "trivial"? Certainly not Cantor's great set theory, technically very simple, but deep and wonderful conceptually without being difficult or complicated.

I have heard mathematicians sneer at the special theory of relativity, calling it nothing but a technically trivial quadratic equation and a few consequences. Yet it is one of the monuments of human thought. So what is "trivial"? Simple arithmetic? It may be trivial to us, but is it to the third-grade child?

Let us consider some other words mathematicians use: what about the adjective "continuous"? Out of this one word came all of topology. Topology may be considered as a big essay on the word "continuous" in all its ramifications, generalizations, and applications. Try to define logically or combinatorially an adverb like "even" or "nevertheless." Or take an ordinary word like "key," a simple object. Yet it is an object far from easy to define quasi-mathematically. "Billowing" is a motion of smoke, for example, in which puffs are emitted from puffs. It is almost as common in nature as wave motion. Such a word may give rise to a whole theory of transformations and hydrodynamics. I once tried to write an essay on the mathematics of three-dimensional space that would imitate it.

Were I thirty years younger I might try to write a mathematical dictionary about the origins of mathematical expressions and concepts from commonly used words, imitating the manner of Voltaire's *Dictionnaire Philosophique*.

Adventures of a Mathematician

S.M. Ulam



Preface to the 1991 Edition by William G. Mathews and Daniel O. Hirsch
Note on S. M. Ulam's Mathematics by Jan Mycielski
Postscript by Françoise Ulam

HISTORY OF SCIENCE

The autobiography of mathematician Stanislaw Ulam, one of the great scientific minds of the twentieth century, tells a story rich with amazingly prophetic speculations and peppered with lively anecdotes. As a member of the Los Alamos laboratory from 1944 on, Ulam helped to precipitate some of the most dramatic changes of the postwar world. Showing an experimental flair that distinguished him from other mathematicians, he was among the first to use and advocate computers for scientific research. Ulam also originated ideas for the nuclear propulsion of space vehicles and made fundamental contributions to many of today's most challenging mathematical projects.

With his wide-ranging interests, Ulam never emphasized the importance of his contribution to the research that resulted in the hydrogen bomb. Now William Mathews and Daniel Hirsch reveal the true story of Ulam's pivotal role in the making of the "Super," in their historical introduction to this behind-the-scenes look at the minds and ideas that ushered in the nuclear age. Jan Mycielski sheds new light on Ulam's mathematical originality, and a postscript by Françoise Ulam increases our pleasure in the company of her witty, brilliant, constantly surprising husband.

S. M. Ulam (1909–1984) was born in Poland and was a key member of the now legendary Polish School of Mathematics. In the United States from 1935 on, he received many academic appointments and honors and authored many articles, essays, and mathematical books, including *Analogies Between Analogies* (California 1990).

William G. Mathews is Professor of Astrophysics at the University of California, Santa Cruz. **Daniel O. Hirsch** is President of the Committee to Bridge the Gap, located in Los Angeles. **Jan Mycielski** is Professor of Mathematics at the University of Colorado. **Françoise Ulam** is a resident of Santa Fe, New Mexico.

Cover illustration: S. M. Ulam, 1938. Pencil sketch by Zygmund Menkes
Cover design: Robert Ross

University of California Press
Berkeley 94720

ISBN 0-520-07154-9

ULAM Adventures of a Mathematician

CALIFORNIA

In memory of my parents

University of California Press
Berkeley and Los Angeles

University of California Press
Oxford, England

Copyright © 1991 by
The Regents of the University of California

Preface copyright © 1983 S. M. Ulam

Copyright © 1976 S. M. Ulam

Library of Congress Cataloging in Publication Data
Ulam, Stanislaw M.
Adventures of a mathematician.

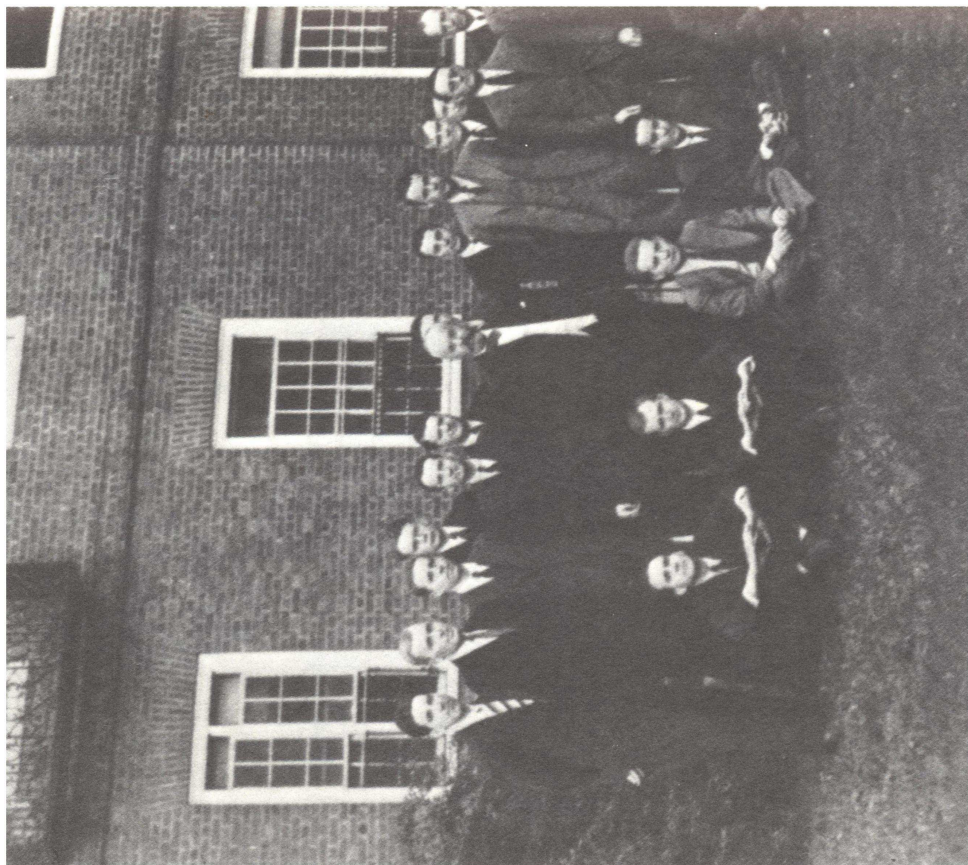
Bibliography: p.
Includes index.

I. Ulam, Stanislaw M. I. Title.
QA29.U4A33 510'.92'4 [B] 75-20133
ISBN 0-520-07154-9

2 3 4 5 6 7 8 9

Printed in the United States of America

The paper used in this publication meets the minimum requirements of American National Standard for Information Sciences—Permanence of Paper for Printed Library Materials, ANSI Z39.48—1984 ∞



The Harvard Society of Fellows, Cambridge, 1938
Left to right, seated: George Homans, Jim Fisk, Paul Samuelson, John Snyder, James Miller, Ivan Getting, Willard Quine, Robert Woodward, George Hass
Standing, first row: James Baker, Kenneth Murdock, Paul Ward, George Haskins, L. J. Henderson, John Ferry, George Hanfmann, Charles Curtiss, Alfred North Whitehead, John Livingston Lowes, Talbot Waterman, Tom



Chambers, Samuel Eliot Morison, John Miller, Conrad Arensberg, David Griggs, William Whyte
Back row: F. Edward Cranz, Reed Rollins, Harry Levin, Frederick Watkins, John Oxtoby, E. Bright Wilson, Richard Howard, Albert Lord, Garrett Birkhoff, Craig LaDrière, Stan Ulam, Orville Bailey
(Harvard University)