* The text of "Vita" consists solely of quotations from Stan's autobiography, *Adventures of a Mathematician*, which are reproduced with permission from Charles Scribner's Sons, an imprint of Macmillan Publishing Company. *Adventures* was assembled by Françoise Ulam from hours of recorded reminiscences. The selections reprinted here not only chronicle Stan's life but also highlight those areas of his work that are featured in Part II of this volume.

Vita*

POLISH YEARS

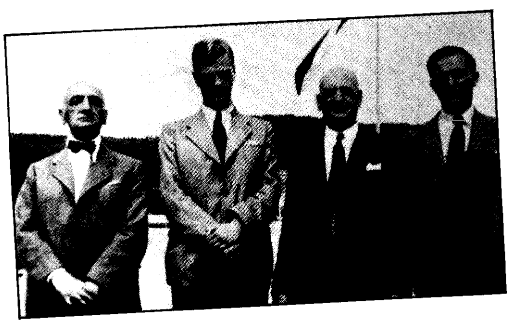
1909 Born April 13 in Lwow, Poland, then part of Austro-Hungarian Empire

1916 Russian troops occupy Lwow. Family moves temporarily to Vienna

1918 Family returns to Lwow,now part of Republic of Poland. Ukrainians besiege the city

1919 Enters gymnasium

1927 Matriculates from gymnasium. Enters Lwow Polytechnic Institute



Father (left) and uncle Szymon seeing Stan and his young brother, Adam, off for the last time at Gdynia, Poland, 1939

Passport photo, 1935

My father, Jozef Ulam, was a lawyer. He was born in Lwow, Poland, in 1877. At the time of his birth the city was the capital of the province of Galicia, part of the Austro-Hungarian Empire. When I was born in 1909 this was still true . . . My mother, Anna Auerbach, was born in Stryj, a small town some sixty miles south of Lwow, near the Carpathian Mountains. Her father was an industrialist who dealt in steel and represented factories in Galicia and Hungary.

In November of [1918] the Ukrainians besieged the city . . . Our house was in a relatively safe part of town, even though occasional artillery shells struck nearby . . . Many of our relatives came to stay with us . . some thirty of them, half being children. There were not nearly enough beds, of course, and I remember people sleeping everywhere on rolled rugs on the floor . . . Strangely enough, my memories of these days are of the fun I had playing, hiding, learning card games with the children for the two weeks before the siege was lifted . . . For children wartime memories are not always traumatic.

At the age of ten in 1919 I passed the entrance examination to the gymnasium. This was a secondary school patterned after the German gymnasia and the French lycees. Instruction usually took eight years. I was an A student, except in penmanship and drawing, but did not study much.

Around [that time] so much was written in newspapers and magazines about the theory of relativity that I decided to find out what it was all about . . . This interest became known among friends of my father, who remarked that I “understood” the theory of relativity . . . This gave me a reputation I felt I had to maintain, even though I knew that I did not genuinely understand any of the details. Nevertheless, this was the beginning of my reputation as a “bright child.”

I had mathematical curiosity very early. My father had in his library a wonderful series of German paperback books–*Reklam*, they were called. One was Euler’s *Algebra*. I looked at it when I was perhaps ten or eleven, and it gave me a mysterious feeling. The symbols looked like magic signs: I wondered whether one day I could understand them.

In high school, I was stimulated by . . . the problem of the existence of odd perfect numbers. An integer is perfect if it is equal to the sum of all its divisors including one but not itself. For instance: $6 = 1 + 2 + 3$ is perfect. So is $28 = 1 + 2 + 4 + 7 + 14$. You may ask: does there exist a perfect number that is odd? The answer is unknown to this day.

Poincare molded portions of my scientific thinking. Reading one of his books today demonstrates how many wonderful truths [remain], although everything in mathematics has changed almost beyond recognition and in physics perhaps even more so, I admired Steinhaus’s book almost as much, for it gave many examples of actual mathematical problems.

In 1927 I passed my three-day matriculation examinations and a period of indecision began. The choice of a future career was not easy. My father, who had wanted me to become a lawyer so I could take over his large practice, now recognized that my inclinations lay in other directions . . My parents urged me to become an engineer, and so I applied for admission at the Lwow Polytechnic Institute as a student of either mechanical or electrical engineering.

Vita

POLISH YEARS

1928 Writes his first paper, published in Fundamenta Mathematicae *in 1929*

1931 Attends mathematical congress in Wilno

1932 M.A. from Polytechnic Insitute

1933 D. Sc. from Polytechnic Institute

In the fall of 1927 I began attending lectures at the Polytechnic Institute in the Department of General Studies, because the quota of Electrical Engineering already was full. The level of the instruction was obviously higher than that at high school, but having read Poincare and some special mathematical treatises, I naively expected every lecture to be a masterpiece of style and exposition, of course, I was disappointed.

Soon I could answer some of the more difficult questions in [Kuratowski's] set theory course, and I began to pose other problems. Right from the start I appreciated Kuratowski's patience and generosity in spending so much time with a novice. Several times a week I would accompany him to his apartment at lunch time, a walk of about twenty minutes, during which I asked innumerable mathematical questions . . . Between classes, I would sit in the offices of some of the mathematics instructors. At that time I was perhaps more eager than at any other time in my life to do mathematics to the exclusion of almost any other activity.

At the beginning of the second semester of my freshman year, Kuratowski told me about a problem in set theory that involved transformations of sets. It was connected with a well-known theorem of Bernstein: if 2A = 2B, then A = B, in the arithmetic sense of infinite cardinals. This was the first problem on which I really spent arduous hours of thinking. I thought about it in a way which now seems mysterious to me, not consciously or explicitly knowing what I was aiming at. So immersed in some aspects was I, that I did not have a conscious overall view. Nevertheless, I managed to show by means of a construction how to solve the problem, devising a method of representing by graphs the decomposition of sets and the corresponding transformations. Unbelievably, at the time I thought I had invented the very idea of graphs.

Joint mathematics-physics meeting, Lwów, 1930. Stan is number 10

Panorama of Lwow in the 1970s

Kuratowski and a photo of Banach, circa 1968

It was Mazur (along with Kuratowski and Banach) who introduced me to certain large phases of mathematical thinking and approaches. From him I learned much about the attitudes and psychology of research. Sometimes we would sit for hours in a coffee house. He would write just one symbol or a line like y = f(x) on a piece of paper, or on the marble table top. We would both stare at it as various thoughts were suggested and discussed. These symbols in front of us were like a crystal ball to help us focus our concentration.

Beginning with the third year of studies, most of my mathematical work was really started in conversations with Mazur and Banach. And according to Banach some of my own contributions were characterized by a certain "strangeness" in the formulation of problems and in the outline of possible proofs. As he told me once some years later, he was surprised how often these "strange" approaches really worked.

He [Banach] enjoyed long mathematical discussions with friends and students. I recall a session with Mazur and Banach at the Scottish Cafe which lasted seventeen hours without interruption except for meals.

These long sessions in the cafés were probably unique. Collaboration was on a scale and with an intensity I have never seen surpassed, equaled or approximated any where--except perhaps at Los Alamos during the war years . . . Needless to say such mathematical discussions were interspersed with a great deal of talk about science in general (especially physics and astronomy), university gossip, politics, the state of affairs in Poland: or to use one of John von Neumann's favorite expressions, the "rest of the universe." The shadow of coming events, of Hitler's rise in Germany and the premonition of a world war loomed ominously.

The second big congress I attended [of mathematicians from the Slavic countries] was held in Wilno in 1931 . . . At the congress I gave a talk about the results obtained with Mazur on geometrical isometric transformations of Banach spaces, demonstrating that they are linear. Some of the additional remarks we made at the time are still unpublished. In general, the Lwow mathematicians were on the whole somewhat reluctant to publish. Was it a sort of pose or a psychological block?

If I had to name one quality which characterized the development of this school, made up of the mathematicians from the University [of Lwow] and the Polytechnic Institute, I would say that it was their preoccupation with the heart of the matter that forms mathematics. On a set theoretical and axiomatic basis we examined the nature of a general space, the general meaning of continuity, general sets of points in Euclidean space, general functions of real variables, a general study of the spaces of functions, a general idea of the notions of length, area and volume, that is to say, the concept of measure and the formulation of what should be called probability.

In 1932 I was invited to give a short communication at the International Mathematical Congress in Zurich. This was the first big international meeting I attended, and I felt very proud to have been invited. In contrast to some of the Polish mathematicians I knew, who were terribly impressed by western science. I had confidence in the equal value of Polish mathematics. Actually this confidence extended to my own work. Von Neumann once told my wife, Francoise, that he had never met anyone with as much self-confidence-adding that perhaps it was somewhat justified.

By 1934 I had become a mathematician rather than an electrical engineer. It was not so much that I was doing mathematics, but rather that mathematics had taken possession of me . . . At twenty-five, I had established some results in measure theory which soon became well known. These solved certain set theoretical problems attacked earlier by Hausdorff, Banach, Kuratowski, and others. These measure problems again became significant years later in connection with the work of Godel and more recently with that of Paul Cohen. I was also working in topology, group theory, and probability theory. From the beginning I did not become too specialized. Although I was doing a lot of mathematics. I never really considered myself as only a mathematician. This may be one reason why in later life I became involved in other sciences.

[Nevertheless] ever since I started learning mathematics I would say that I have spent—regardless of any other activity--on the average two to three hours a day thinking and two to three hours reading or conversing about mathematics.

Vita

PRINCETON

HARVARD

WISCONSIN

1934 Postdoctoral travels and studies in Vienna, Zurich, Paris, and Cambridge (England)

1935 Scottish Book originates

Returns to Poland. Receives letter of invitation to Institute for Advanced Study in Princeton

December: Sails to America

1936–39 Academic years with Harvard Society of Fellows. Summers at home in Poland

1939 Leaves home for the last time in the fall of 1939, accompanied by his young brother, Adam

1939–40 Lecturer at Harvard

1940--41 Instructor at University of Wisconsin. Meets C. J. Everett. Works with him on ordered groups and projective algebras

1941 Becomes American citizen. Tries to volunteer in the U.S. Air Force

1941-43 Assistant Professor at University of Wisconsin

Stan in Warsaw, 1972

Oxtoby and Stan at Harvard, circa 1936

In 1934, the international situation was becoming ominous. Hitler had come to power in Germany. His influence was felt indirectly in Poland. There were increasing displays of inflamed nationalism . . . and anti -Selmitic demonstrations . . . For years my uncle Karol Auerbach had been telling me: "Learn foreign languages!" Another uncle, Michael Ulam, an architect, urged me to try a career abroad. For myself, unconscious as I was of the realities of the situation in Europe, I was prompted to arrange a longish trip abroad . . . to meet other mathematicians . . . and in my extreme self-confidence, try to impress the world with some new results. My parents wcrc willing to finance the trip.

It was only toward the end of 1934 that I entered into correspondence with von Neumann. He was then in the United Statesc a very young professor at the Institute for Advanced Study in Princeton. I wrote him about some problems in measure theory. He had heard about me from Bochner.,and in his reply he invited me to come to Princeton ford few months, saying that the Insititue could offer me a $300 stipend. I met him [in Warsaw] shortly after my return from England . . . Von Neumann appeared quite young to me. although he was . . . some fivc or six years older than I . . . At once I found him congenial. His habit of intermingling funny remarks, jokes, and paradoxical anecdotes or observations of people into his conversation, made him far from remote or forbidding.

[At the Institute] I went to lectures and seminars, heard Morse, Veblen, Alexander, Einstein, and others, but was surprised how little people talked to each other compared to the endless hours in the coffee houses in Lwow there was another way in which the Princeton atmosphere was entirely different from what I expected: it was fast becoming a way station for displaced European scientists. In addition, these were still depression days and the situation in universities in general and in mathematics in particular was very bad.

One of the luckiest accidents of my life happened the day G. D. Birkhoff came to tea at von Neumann's house while I was visiting there . . . We talked and, after some discussion of mathematical problems, he turned to me and said, "There is an organization at Harvard called the Society of Fellows. It has a vacancy. There is about one chance in four that if you were interested and applied you might receive this appointment."

I came to the Society of Fellows during its first few years of existence . . . I was given a two-room suite in Adams House, next door to another new fellow in mathematics by the name of John Oxtoby . . . 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 the idea of "category" of sets. "Category" is a notion in a way parallel to but less quantitative than the measure of sets . . . We quickly established some new results, and the fruits of our conversations . . . 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.

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 . . . 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 . . . 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.

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 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.

Each summer between 1936 and 1939, I returned to Poland for a full three months. The first time, after only a few months' stay in America, I was surprised that street cars ran electricity and telephones worked. I had become imbued with the idea of America's absolute technological superiority and unique "know-how." My main emotional reactions were, of course, related to reunion with my family and friends, and the familiar scenes of Lwow, followed by a longing to return to the free and hopeful "open-ended" conditions of life in America.

I had to go to the American consulate in Warsaw each summer I was in Poland to apply for a new visitor's visa in order to return to the United States. Finally, the consul said to me. "Instead of coming here every summer for a new visa, why don 't you get an immigration visa'?" It was lucky that I did, for just a few months later these became almost impossible to obtain.

Claire, at 14 months, and Franchoise, Los Angeles, 1945

[My brother] Adam and I were staying in a hotel on Columbus Circle [in New York] . . . It must have been around one or two in the morning when the telephone rang . . . my friend the topologist Witold Hurewicz began . . . "Warsaw has been bombed, the war has begun." That is how I learned about the beginning of World War II . . . Adam was asleep; I did not wake him. There would be time to tell him the news in the morning. Our father and sister were in Poland, so were many other relatives. At that moment, I suddenly felt as if a curtain had fallen on my past life . . . There has been a different color and meaning to everything ever since.

Birkhoff helped me to secure the job [at the University of Wisconsin] . . . Almost at once I met congenial, intelligent people not only in mathematics and science, but also in the humanities and arts . . . So I found Madison not at all the intellectual desert I had feared it would be . . . I was given a light teaching load . . . But the very expression . . . implied physical effort and fatigue—two things I have always been afraid of lest they interfere with my own thinking and research.

Something else happened to make Madison most important to me. It was there that I married a French girl, who was an exchange student at Mount Holyoke College and whom I had met in Cambridge, Francoise Aron. Marriage, of course, changed my way of life, greatly influencing my daily mode of work, my outlook on the world, and my plans for the future.

It was in Madison that I met C. J. Everett. . . [He] and I hit it off immediately. As a young man he was already eccentric, original, with an exquisite sense of humor, wry, concise, and caustic in his observations. He was totally devoted to mathematics . . . I found in him much that resembled my friend Mazur in Poland, the same kind of epigrammatic comments and jokes . . . We collaborated on difficult problems of "order"- the idea of order for elements in a group. In our mathematical conversations, as always, I was the optimist, and had some general, sometimes only vague ideas. He supplied the rigor, the ingenuities in the details of the proof, and the final constructions. Everett exhibited a trait of mind whose effects are, so to speak, non-additive: persistence in thinking, Thinking continuously . . . for an hour, is at least for me—and I think for many mathematicians—more effective than doing it in two half-hour periods. It is like climbing a slippery slope. If one stops, one tends to slide back. Both Everett and Erdos have this characteristic of long-distance stamina.

I was asked to run the mathematics colloquium, which took place every two weeks . . . The colloquium was run differently from what I had known in Poland, where speakers gave ten- or twenty-minute informal talks. At Madison they were one-hour lectures. There is quite a difference between short seminar talks like those at our math society in Lwow, and the type of lecture which necessitates talking about major efforts. The latter were better prepared, of course, but their greater formality removed some of the spontaneity and stimulation of the shorter exchanges.

Harvard Society of Fellows, 1938

Vita

LOS ALAMOS

1943 Receives invitation to join the Manhattan Project

1944 February 4: Arrives in Los Alamos

Joins Edward Teller's group in the Theoretical Division under Hans Bethe

Teller's group is transferred to Fermi's Division

COSMOS CLUB
WASHINGTON 5 D C

November 9, 1943

Von Neumann wrote this letter to Stan in longhand. It is reproduced here in typewritten form tc decrease its size and increase its legibility.

Dear Stan,

I am very glad that Mr. Hughes ''and all he stands for'' have come through. I told them about you, because you wrote me several times in the past that you definitely wanted a war job, and because this is a *very real* possibility, where you could dc very effective and useful work.

The project in question is exceedingly important, probably beyonc all adjectives I could affix to it. It is very interesting, too. and the theoretical (and other) physicists connected with it are probably the best group existing anywhere at this moment. It does require some computational work, but there is no doubt, tha everybody will be most glad and give you all the encouragement you can wish in doing original research on the subject, for whic there is ample opportunity. I can also assure you of my own cooperation in this respect.

The secrecy requirements of this project are rather extreme, anc it will probably necessitate your and your families essentially staying on the premises (except for vacations) as long as you choose to be associated with it.

To repeat: If you want war work, this is probably a quit exceptional opportunity.

I may be able to give you a better idea orally, and I would be glad to do so, if we can meet somewhere before you answer---but suppose that there is no time. (I will be in Princeton on 13-15 and in Washington on 16-17 of this month.)

So you really count on a quite short war? I don't see that from a purely technical standpoint Germany need be broken before next fall.. Of course a collapse may come any day from now on for moral and political reasons, but I can't [know] how to judge that, without knowing much more about the present state and efficiency of the Nazi political machine

And there is still a year's worth of Asiatic war after that Anyhow, qui vivra verra ...

It seems that Morgenstern's and my book on ''games'' will be ou in 3 months or so.

Best regards, and looking forward to seeing you soon---here o ''there''---

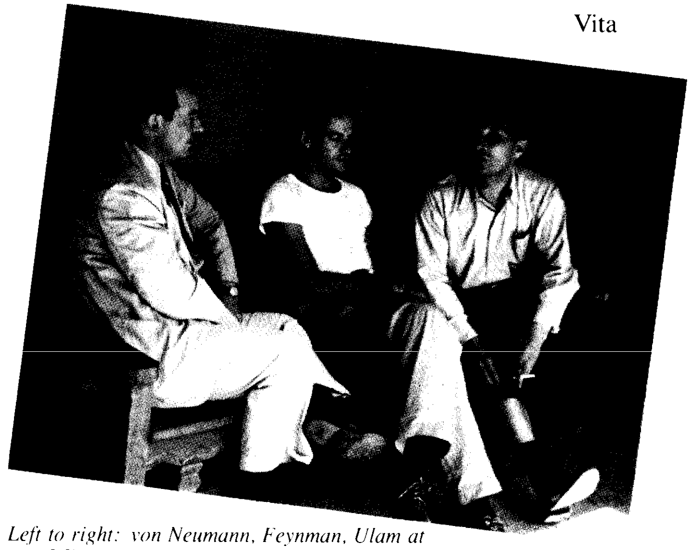
Von Neumann, circa 1950

During the late spring of 1943, I wrote to von Neumann about the possibility of war work. . . I received an official invitation to join an unidentified project that was doing important work, the physics having something to do with the interior of stars. The letter inviting me was signed by the famous physicist Hans Bethe.

On the terrace of Fuller Lodge. Clockwise from lower left: Davis, Metropolis, Ulam, an unidentified person, McMillan, and de Hoffman

Left to right: Metropolis, Ulam, Morgenstern, Carol Stein, Brillouin, Paul Stein

Enrico Fermi



Left to right: von Neumann, Feynman, Ulam at Bandelier. 1949

Finally I learned that we were going to New Mexico, to a place not far from Santa Fe. Never having heard about New Mexico, I went to the library and borrowed the Federal Writers' Project Guide to New Mexico. At the back of the book, on the slip of paper on which borrowers signed their names. I read the names of Joan Hinton, David Frisch, Joseph McKibben, and all the other people who had been mysteriously disappearing [from Madison] to hush-hush war jobs without saying where. I had uncovered their destination in a simple and unexpected fashion. It is next to impossible to maintain absolute secrecy and security in war time.

[Upon my arrival at Los Alamos. Johnny] took me aside and . . . told me of all the possibilities which had been considered, of the problems relating to the assembling of fissionable materials, about plutonium (which did not yet physically exist even in the most microscopic quantities at Los Alamos). I remember very well, when a couple of months later I saw Robert Oppenheimer running excitedly down a corridor holding a small vial in his hand, with Victor Weisskopf trailing after him. He was showing some mysterious drops of something at the bottom of the vial. Doors opened, people were summoned, whispered conversations ensued, there was great excitement. The first quantity of plutonium had just arrived at the lab.

It *is* one thing to know about physics abstractly, and quite another to have a practical encounter with problems directly connected with experimental data . . . 1 found out that the main ability to have was a visual, and also an almost tactile, way to imagine the physical situations. rather than a merely logical picture of the problems . . . Very few mathematicians seem to possess [such an imagination] to any great degree.

Strangely enough, the actual working problems did not involve much of the mathematical apparatus of quantum theory although it lay at the base of the phenomena, but rather dynamics of a more classical kind—kinematics, statistical mechanics, large-scale motion problems, hydrodynamics, behavior of radiation . . . Compared to quantum theory the project work was like applied mathematics as compared with abstract mathematics.

[Edward] Teller, in whose group I was supposed to work. talked to me on that first day about a problem in mathematical physics that was part of the necessary theoretical work in preparation for developing the idea of a "super" bomb. as the proposed thermonuclear hydrogen bomb was then called . . . Teller's problem concerned the interaction of an electron gas with radiation This was the first technical problem in theoretical physics I had ever tackled in my life . It was a messy little job. Edward was not satisfied with my rather elementary derivations.

After this first work on Edward's problem, I spread out my interests to other related questions, one being the problem of statistics of neutron multiplication. This was more tangible for me from the purely mathematical side. I discussed such problems of branching and multiplying patterns with David Hawkins.

A discussion with von Neumann . . . [in] early 1944 took several hours, and concerned ways to calculate the course of an implosion more realistically than the first attempts outlined by him and his collaborators. The hydrodynamical problem was simply stated, but very difficult to calculate—not only in detail, but even in order of magnitude . . . In this discussion I stressed pure pragmatism and the necessity for attempting to get a heuristic survey of the general problem by simpleminded brute force—that is, more realistic, massive numerical work . . . With the available computing facilities, the accuracy of the necessary numerical work could not be satisfactory. This was one of the first reasons for pressing for the development of electronic computers.

Fermi was short, sturdily built, strong in arms and legs, and rather fast moving. His eyes, darting at times, would be fixed reflectively when he was considering some questions . . . He would try to elucidate other persons' thoughts by asking questions in a Socratic manner, yet more concretely than in Plato's succession of problems.

I think he had a supreme sense of the important. He did not disdain work on the so-called smaller problems: at the same time, he kept in mind the order of importance of things in physics. This quality is more vital in physics than in mathematics. which is not so uniquely tied to "reality." Strangely enough, he started as a mathematician. Some of his first papers with very elegant results were devoted to the problem of ergodic motion. When he wanted to, he could do all kinds of mathematics. To my surprise, once on a walk he discussed a mathematical question arising from statistical mechanics which John Oxtoby and I had solved in 1941.

[Fermi] could be also quite a tease. I remember his Italian inflections when he taunted Teller with statements like "Edward-a how corn-a the Hungarians have not-ii invented anything'?"

Clockwise from lower left: An unidentified person, Mark, Matthias, Ulam, Evans, Cowan, Metropolis

Vita

LOS ALAMOS

1945 July 16: Trinity Test

September: Moves to Los Angeles as Associate Professor at University of Southern California

1946 January: Acute attack of encephalitis

April: Attends secret conference at Los Alamos

May: Returns to Los Alamos

1947 C. J. Everett joins the Laboratory

Seminars on the Monte Carlo Method and hydrodynamical calculations

Beginning of heuristic studies on electronic computing machines

Ulam and Everett develop theory of multiplicative processes

1949 Russian atomic bomb test

Truman directs AEC to proceed with work on the hydrogen bomb

One thing that relieved the repetition and alternation of work, intellectual discussions, evening gatherings, social family visits and dinner parties, was when a group of us would play poker about once a week. The group included Metropolis, Davis, Calkin, Flanders, Langer, Long. Konopinski, von Neumann (when he was in town), Kistiakowski sometimes, Teller, and others. We played for small stakes; the naivete of the game and the frivolous discussions laced with earthy exclamations and rough language provided a bath of refreshing foolishness from the very serious and important business that was the raison d'etre of Los Alamos.

The Trinity test. Hiroshima. V-J Day, and the story of Los Alamos exploded over the world almost simultaneously with the A-Bomb. Publicity over the secret wartime Project tilled the newspapers and its administrative heads were thrown in the limelight.

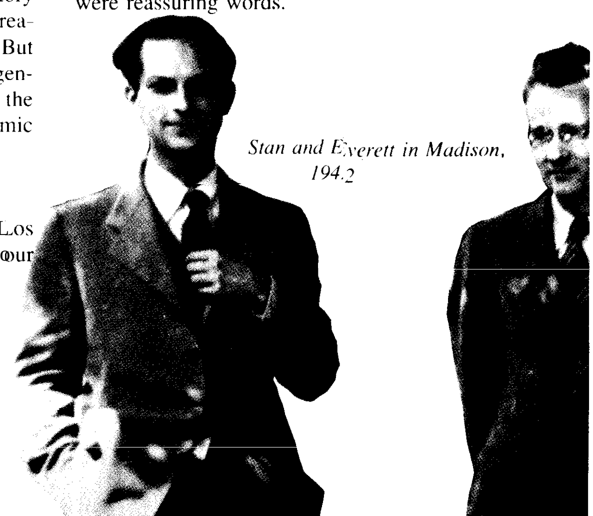
As I was reading [such items,] something else flashed through my mind. a story of a "pension" in Berlin before the war . . . Or-w man was taking most of the asparagus that was on the platter. Whereupon another man stood up shyly and said: "Excuse me, Mr. Goldberg, we also like asparagus!" And the expression "asparagus" became a code word in our private conversations for trying to obtain an unduly large share of credit for scientific work or any other accomplishment of a joint or group character. Johnny loved this story . . . We would plan to write a twenty-volume treatise on "Asparagetics through the Ages" . . . But levities like these could hardly alleviate the general feeling of foreboding upon entering into the era of history that would be called the Atomic Age.

In early September of 1945, I went to Los Angeles to look for housing and to prepare our move from Los Alamos.

It was a stormy day; on the walk from the bus to the house in Balboa the violent winds almost choked me. That same night I developed a fantastic headache . . . The following night . . . I noticed that my speech was confused, that I was barely able to form words. I tried to talk but it was mostly a meaningless mumble—a most frightening experience . . . A severe attack of brain troubles began, which was to be one of the most shattering experiences of my life . Many of the recollections of what preceded my operation are hazy. Thanks to what Francoise told me later I was able to put it together . . . She feared I was dying and made a frantic telephone call to the surgeon, who decided the operation should be performed immediately. This probably saved my life: the emergency operation relieved the severe pressure on my brain which was causing all the trouble . . . The illness was tentatively diagnosed as a kind of virus encephalitis. But the disquietude about the state of my mental faculties remained with me for a long time. even though I recovered speech completely.

Many friends came to visit me . . . Metropolis came all the way from Los Alamos. His visit cheered me greatly. I found out that the security people in Los Alamos had been worried that in my unconscious or semi-conscious states I might have revealed some atomic secrets.

As I was preparing to leave [the hospital], . . . Erdos appeared at the end of the hall . . . In the car on the way home from the hospital, Erdos plunged immediately into a mathematical conversation. I made some remarks, he asked me about some problem, 1 made a comment, and he said: "Stan, you are just like before." These were reassuring words.



Stan and Everett in Madison, 1942

In the days that followed we had more and more mathematical discussions and longer and longer walks on the beach. Once he stopped to caress a sweet little child and said in his special language: "Look, Stan! What a nice epsilon." A very beautiful young woman, obviously the child's mother, sat nearby, so I replied, "but look at the capital epsilon." This made him blush with embarrassment.

Two seminar talks I gave shortly after my return [to Los Alamos] turned out to have good or lucky ideas and led to successful further developments. One was on what was later called the Monte Carlo method. and the other was about some new possible methods of hydrodynamical calculations. Both talks laid the groundwork for very substantial activity in the applications of probability theory and in the mechanics of continua. [Both ideas required extensive machine computation.]

Computing machines came about through the confluence of scientific and technological developments. On one side was the work in mathematical logic, in the foundations of mathematics, in the detailed study of formal systems, in which von Neumann played such an important role; on the other was the rapid progress of technological discoveries in electronics which made it possible to construct electronic computers.

Almost immediately after the war Johnny and 1 also began to discuss the possibilities of using computers heuristically to try to obtain insights into questions of pure mathematics. By producing examples and by observing the properties of special mathematical objects one could hope to obtain clues as to the behavior of general statements which have been tested on examples.

The Gamow cartoon

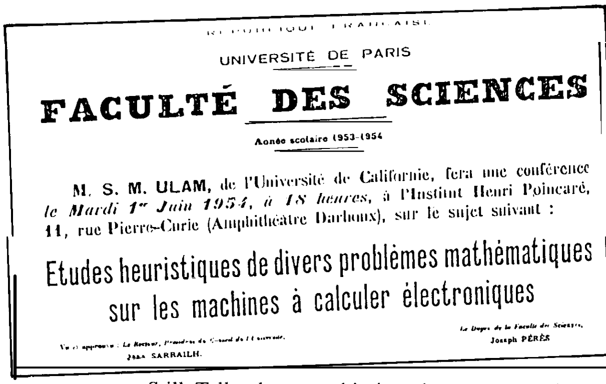
It was in 1949.. . that George Gamow, whom I had met briefly in Princeton before the war. came to Los Alamos for a lengthy visit . . . There was nothing dry about him. A truly "three-dimensional" person, he was exuberant, full of life, interested in copious quantities of good food, fond of anecdotes, and inordinately given to practical jokes.

Banach once told me, "Good mathematicians see analogies between theorems or theories, the very best ones see analogies between analogies." Gamow possessed this ability to see analogies between models for physical theories to an almost uncanny degree . . . It was along the great lines of the foundations of physics. in cosmology, and in the recent discoveries in molecular biology that his ideas played an important role. His pioneering work in explaining the radioactive decay of atoms was followed by his theory of the explosive beginning of the universe, the "big bang" theory (he disliked the term by the way), and the subsequent formation of galaxies.

Shortly after President Truman's announcement directing the AEC to proceed with work on the H-Bomb, E. O. Lawrence and Luis Alvarez visited Los Alamos from Berkeley and started (discussions with Bradbury and then with Gamow, Teller, and myself about the feasibility of constructing a "super." This visit played a part in the politics of this enterprise.

Several different proposals of ideas existed on how to initiate the thermonuclear reaction. using fission bombs as starter. One of Gamow's was called "the cat's tail." Another was Edward's original proposal. Gamow drew a humorous cartoon with symbolic representations of these various schemes. In it he squeezes a cat by the tail. I spit in a spittoon. and Teller wears an Indian fertility necklace, which according to Gamow is the symbol for the womb, a word he pronounced "vombb." This cartoon has appeared among the illustrations in his autobiography. My *World Line,* published by The Viking Press in 1970.

A first committee was formed to organize all work on the 'super' and investigate all possible schemes for constructing it. The committee's work was directed by Teller, as chairman, Gamow and myself . Both Gamow and I showed a lot of independence of thought in our meetings and Teller did not like this very much. Not too surprisingly, the original 'super' directing committee soon ceased to exist.


RÉPUBLIQUE FRANÇAISE

UNIVERSITÉ DE PARIS

FACULTÉ DES SCIENCES

Année scolaire 1953-1954

M. S. M. ULAM, de l'Université de Californie, fera une conférence *le Mardi 1er Juin 1954, à 18 heures*, à l'Institut Henri Poincaré, 11, rue Pierre-Curie (Amphithéâtre Darboux), sur le sujet suivant :

Etudes heuristiques de divers problèmes mathématiques sur les machines à calculer électroniques

Vu et approuvé : Le Recteur, Président du Conseil de l'Université, Jean SARRAILH.

Le Doyen de la Faculté des Sciences, Joseph PÉRÈS

Vita

LOS ALAMOS

Hand calculations by Ulam and Everett suggest that ignition scheme for "super" won't work

Results of calculations confirmed by von Neumann and Evans on Princeton computer

Ulam suggests new approach to ignition

Teller suggests a related approach and presents it to the General Advisory Committee

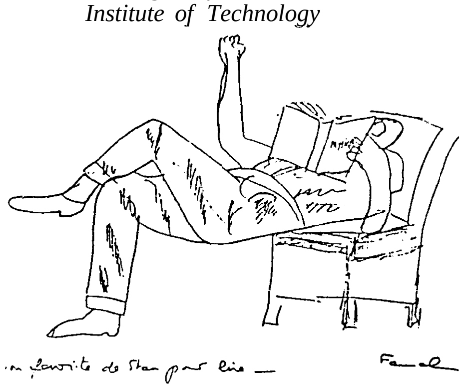
1951–52 Fall semester: Visiting Professor at Harvard

Begins serious discussions of cellular automata with von Neumann

1952 Summer: Studies nonlinear systems with Fermi and Pasta

1955 Fermi dies

1956–57 Visiting Professor at Massachusetts Institute of Technology



Still Teller kept on hinting that not enough work was being done on his original scheme for the ignition of the "super" . . . In collaboration with my friend Everett one day I decided to try a schematic pilot calculation which could give an order of magnitude, at least, a "ballpark" estimate of the promise of his scheme . . . Before we started this calculation of the progress of a thermonuclear reaction (burning in a mass of deuterium or deuterium-tritium mixture), Everett and I had done a lot of work on probability questions connected with the active assemblies of uranium and with neutron multiplications. We worked out a theory of multiplicative processes, as we called it. (Now the preferred name is "branching processes.'') . . . Our calculation . . . threw grave doubts on the prospects of Edward's original approach to the initial ignition conditions of the "super."

As the results of the von Neumann-Evans calculation on the big electronic Princeton machine slowly started to come in, they confirmed broadly what we had shown. There, in the course of the calculation, in spite of an initial, hopeful-looking "flare up," the whole assembly started slowly to cool down. Every few days Johnny would call in some results. "Icicles are forming," he would say dejectedly.

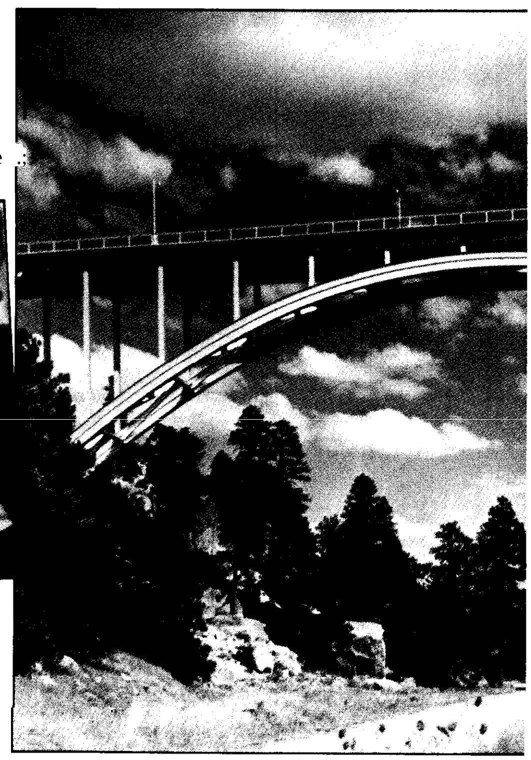
Perhaps the change came with a proposal I contributed. I thought of a way to modify the whole approach by injecting a repetition of certain arrangements. Unfortunately, the idea or set of ideas involved is still classified and cannot be described here.

A sketch by Françoise of Stan's favorite reading position

At once Edward took up my suggestions, hesitantly at first, but enthusiastically after a few hours. He had seen not only the novel elements, but had found a parallel version, an alternative to what I had said, perhaps more convenient and generalized. From then on pessimism gave way to hope . . . Teller lost no time in presenting these ideas, perhaps with most of the emphasis on the second half of our paper, at a General Advisory Committee meeting in Princeton which was to become quite famous because it marked the turning point in the development of the H-bomb.

Contrary to those people who were violently against the bomb on political, moral or sociological grounds, I never had any questions about doing purely theoretical work . . . I felt that one should not initiate projects leading to possibly horrible ends. But once such possibilities exist, is it not better to examine whether or not they are real? An even greater conceit is to assume that if you yourself won't work on it, it can't be done at all . . . When I reflected on the end results, they did not seem so qualitatively different from those possible with existing fission bombs. After the war it was clear that A-bombs of enormous size could be made. The thermonuclear schemes were neither very original nor exceptional. Sooner or later the Russians or others would investigate and build them.

The Oppenheimer Affair, which grew out of the violent hydrogen-bomb debate-even though the animosity between Strauss and Oppenheimer had personal and perhaps petty origins—greatly affected the psychological and emotional role of scientists.



Oppenheimer's opposition to the development of the H-bomb were not exclusively on moral. philosophical. or humanitarian grounds. I might say cynically that he struck me as someone who, having been instrumental in starting a revolution (and the advent of nuclear energy does merit this appellation), does not contemplate with pleasure still bigger revolutions to come . . .

It seems to me this was the tragedy of Oppenheimer. He was more intelligent, receptive, and brilliantly critical than deeply original. Also he was caught in his own web, a web not of politics but of phrasing. Perhaps he exaggerated his role when he saw himself as "Prince of Darkness, the destroyer of Universes." Johnny used to say, "Some people profess guilt to claim credit for the sin."

Computers were brand-new; in fact the Los Alamos MANIAC was barely finished. The Princeton von Neumann machine had met with technical and engineering difficulties that had prolonged its perfection.

As soon as the machines were finished, Fermi, with his great common sense and intuition, recognized immediately their importance for the study of problems in theoretical physics. astrophysics, and classical physics. We discussed this at length . . . After deliberating about possible problems. we found a typical one requiring long-range prediction and long-time behavior of a dynamical system. It was the consideration of an elastic string with two fixed ends. subject not only to the usual elastic force but having, in addition, a physically correct small non-linear term.

Our problem turned out to have been felicitously chosen. The results were entirely different qualitatively from what even Fermi. with his great knowledge of wave motions. had expected. The original objective had been to see at what rate the energy of the string. initially put into a single sine wave (the note was struck as one tone). would gradually develop higher tones with the harmonics, and how the shape would finally become "a mess" both in the form of the string and in the way the energy was distributed among higher and higher modes. Nothing of the sort happened. To our surprise the string started playing a game of musical chairs, only between several low notes, and perhaps even more amazingly. after what would have been several hundred ordinary up and down vibrations, it came back almost exactly to its original . . . shape.

I know that Fermi considered this to be, as he said. "a minor discovery." And when he was invited a year later to give the Gibbs Lecture, he intended to talk about this. He became ill before the meeting.

These were the days of defense research contracts. Even mathematicians frequently were recipients. Johnny and I commented on how in some of their proposals scientists sometimes described how useful their intended research was for the national interest, whereas in reality they were motivated by bonafide scientific curiosity and an urge to write a few papers. Sometimes the utilitarian goal was mainly a pretext. This reminded us of the story of the Jew who wanted to enter a synagogue on Yom Kippur. In order to sit in a pew he had to pay for his seat, so he tried to sneak in by telling the guard he only wanted to tell Mr. Blum inside that his grandfather was very ill. But the guard refused, telling him: "Ganev, Sie wollcn beten" [''You thief! You really want to pray'']. This, we liked to think, was a nice abstract illustration of the point.

Jim Tuck and Stan

Just after Johnny was offered the post of AEC Commissioner and before he accepted and became one in 1954 we had a long conversation. He had profound reservations about his acceptance because of the ramifications of the Oppenheimer Affair . . . In a two-hour visit to Frijoles Canyon one afternoon he bared his doubts and asked me how I felt about it. He joked, "I'll become a commissionnaire." (In French the term is used to mean errand boy.) But he was flattered and proud that although foreign born he would be entrusted with a high governmental position of great potential influence in directing large areas of technology and science.

Our usual conversations were either about mathematics or about his new interest in a theory of automata. These conversations had started in a sporadic and superficial way before the war at a time when such subjects hardly existed. After the war and before his illness we held many discussions on these problems. I proposed to him some of my own ideas about automata consisting of cells in a crystal-like arrangement.

It is evident that Johnny's ideas on a future theory of automata and organisms had roots that went back in time, but his more concrete ideas developed after his involvement with electronic machines. I think that one of his motives for pressing for the development of electronic computers was his fascination with the working of the nervous system and the organization of the brain itself. After his death some of his collaborators collected his writings on the outlines of the theory of automata.

Von Neumann's reputation and fame as a mathematician and as a scientist have grown steadily since his death. More than his direct influence on mathematical research. the breadth of his interests and of his scientific undertakings. his personality and his fantastic brain are becoming almost legendary.

Now Banach, Fermi, von Neumann were dead —the three great men whose intellects had impressed me the most. These were sad times indeed.

Vita

LOS ALAMOS

BOULDER

1957 February 8: Von Neumann dies in Walter Reed Hospital

1957--67 Research advisor, with John Manley, to Norris Bradbury, Director of Los, Alamos

1960 Publication of A Collection of Mathematical Problems

1961-62 Full semester: Visiting Professor at University of Colorado

1963 Winter quarter: Visiting Professor at University of California, San Diego

1965–77 Visiting Professor of Mathematics, and later, Chairman of Mathematics Department, at University of Colorado. Spends vacations in Santa Fe

1967 Retires from Los Alamos but maintains loose connection with the Laboratory as a dollar-a-year consultant

1968–75 Professor of Biomathematics at University of Colorado Medical School

1972–73 Sabbatical year at Massachusetts Institute of Technology, University of Paris, and Los Alamos

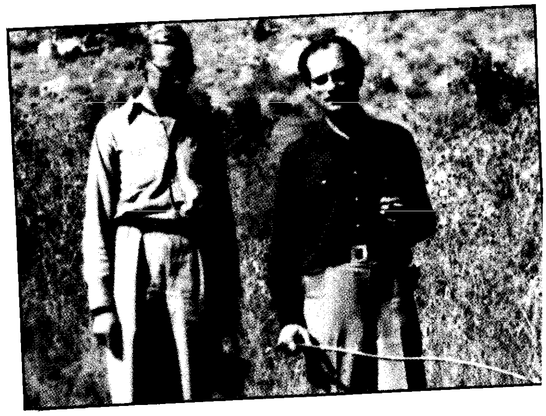
As a result of my work on the hydrogen bomb, I became drawn into a maze of involvements. These had to do precisely with government science and with work as a member of various Space and Air Force committees. Also, in some circles I became regarded as Teller's opponent, and 1 suspect I was consulted as a sort of counterweight. Some of these political activities included my stand on the Test Ban Treaty . . . The cartoonist Herblock drew in the *Wu.shington Post* a picture of the respective positions of Teller and me in which I fortunately appeared as the "good guy."

The idea of nuclear propulsion of space vehicles was born as soon as nuclear energy became a reality . . . I think Feynman was the first in Los Alamos during the war to talk about using an atomic reactor which would heat hydrogen and expel the gas at high velocity. A simple calculation shows that this would be more efficient than expelling the products of chemical reactions.

I became involved with two such projects . . . The first was Project Rover, a nuclear-reactor rocket which was being designed in Los Alamos already quite a few years before the Russian Sputnik. but with very limited funds. The second was a space vehicle, later named Orion. Around 1955 Everett and I wrote a paper about a space vehicle propelled by successive explosions of small nuclear charges . . .

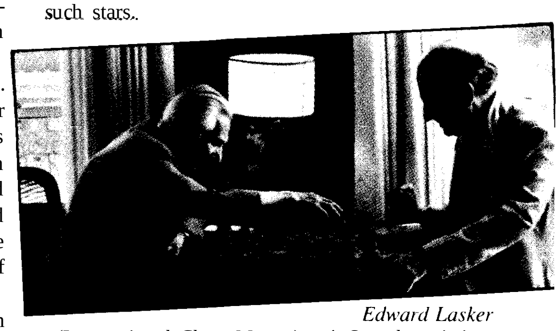
When it was decided to do something in earnest about Project Rover, Wiesner named a Presidential Committee to look into the matter. I was one of its members . . . The committee wrote a report which by faint praise, essentially condemned Project Rover to a de facto death by proposing to make it a purely theoretical study without funds for experimental work or any investment in construction. The physicist Bernd Matthias was the only member of the committee who joined me in writing a dissenting opinion.

Morgenstern and Stun ut Bundelier, 1949



At the same time, I was continuing my own work. After Fermi's death Pasta and I decided to continue exploratory heuristic experimental work on electronic computers in mathematical and physical problems . . .

The problem of clusters of stars was I think the first study of this nature using computers. We took a great number of mass points representing stars in a cluster. The idea was to see what would happen in the long-range time scale of thousands of years to the spherical-looking cluster whose initial conditions imitated the actual motions of such stars.



Edward Lasker (International Chess Master) and Stan, late sixties

While such astrophysical calculations were going on, I began in an amateurish way to work on some questions of biology. After reading about the new discoveries in molecular biology which were coming fast, I became curious about a conceptual role which mathematical ideas could play in biology.

Rota and Stan, 1973

President Kennedy in Los Alamos

In 1960 my book, *[A Collection* of Mathe*matical Problems] was* published. Many years ago Francoise asked Steinhaus what it was that made me what people seemed to consider a fairly good mathematician. According to her, Steinhaus replied: "C'est l'homme du monde qui pose le mieux les problemes." Apparently my reputation, such as it is, is founded on my ability to pose problems and to ask the right kind of questions.

[In 1964] I met Gian-Carlo Rota, a mathematician who is almost a quarter-century younger than I . . . Our relationship is not built on our age difference. Rota claims that he is greatly influenced by me. So I coined the expression "influencer and influence." Rota is one of my best influencers . . .

Rota's personality is compatible with mine. His general education. active interest in philosophy (he is an expert 011 the work of Edmund Husserl and Martin Heidegger). and. above all, his knowledge of classical Latin and ancient history. have made him fill the gap left by the loss of von Neumann.

During the Los Alamos years I frequently took time off to return to academic life and around 1965 I started visiting the University of Colorado on a more regular basis In 1967 I decided to retire from Los Alamos and accept a professorship in Boulder . . . The University of Colorado was flourishing . and the mathematics department experienced an explosive growth in size and quality. Besides Boulder was sufficiently close to Los Alamos . . . so I could continue as a consultant and visit frequently . . . The mathematics department was acquiring excellent researchers . . . [among them] a younger, brilliant Pole, Jan Mycielski, a student of Steinhaus, whom I invited to accept a professorship.

In 1967 . . . Mark Kac and I were invited to write a long article [for *Britannica Perspectives,]* . . . a semi-popular presentation of modem ideas and perspectives of . . . the great concepts of mathematics . Since then it has appeared separately under the title *Mathematics and Logic.*

Mark Kac had also studied in Lwow, but since he was several years younger than I (and I had left when only twenty-six myself, 1 knew him then only slightly . . . After the war he visited Los Alamos, and we developed our scientific collaboration and friendship . . . Mark is one of the very few mathematicians who possess a tremendous sense of what the real applications of pure mathematics are and can be . . . He was one of Steinhaus's best students.

[After I retired from the University of Colorado, we] sold our Boulder house and bought another one in Santa Fe, which has become our base. From Santa Fe I commute three or four times a week to the Los Alamos Laboratory. Its superb scientific library and computing facilities allow me to continue working . . . Dan Mauldin, a professor at North Texas State University [and I] are now collaborating on a collection of new unsolved problems. This book will have a different emphasis from that of my Collection of Mathematical Problems. The new collection will deal more with mathematical ideas connected to theoretical physics and biological schemata.

In the short span of my life great changes have taken place in the sciences . . . Sometimes I feel that a more rational explanation for all that has happened during my lifetime is that I am still only thirteen years old, reading Jules Verne or H. G. Wells. and have fallen asleep.

Mark Kac

It is still an unending source of surprise for me to see how a few scribbles on a blackboard or on a sheet of paper could change the course of human affairs. I became involved in the work on the atomic bomb, then in the work on the hydrogen bomb, but most of my life has been spent in more theoretical realms.

Vita

BOULDER

SANTA FE

1974--84 Winter trimesters: Graduate Research Professor, University of Florida

1975 Retires from University of Colorado

1979-84 Visiting Professor of Biomathematics, University of Colorado Medical School

1980-84 Visiting Professor at Neuroscience Institute of Rockefeller University

1982-83 Fall semester: Visiting Professor at University of California, Davis

1984 Dies in Santa Fe on May 13

At the time of his death, Stanislaw M. Ulam was an elected Fellow of the American Academy of Arts and Sciences *and an elected* member of the National Academy of Sciences and the American Philosophical Society. *He* sat *on* the Board of Governors and the Scientific Advisory Committee of the Weizmann Institute of Science (Rehovot. Israel) and the Board of the Jurzykowski Foundation (New York. New York). *He belonged* to the Polish Mathematical Society, the American Mathematical Society, the American Physical Society, and the American Association for the Advancement of Science.

He *held honorary degrees* from the University of New Mexico, the University of Pittsburgh, and the University of Wisconsin *and was recipient of* the Sierpinski Medal, the Heritage Award, and the Polish Millenium Prize.

He had been a member and/or chairman of the Committee on Innovations of the National Academy of Sciences, the Committee on Applications of Mathematics of the National Research Council, the Visiting Committee for Mathematics and the Visiting Committee for Applied Mathematics of Harvard University, the Gibbs Lecture Committee of the American Mathematical Society, and the Mathematics Research Committee of the Mathematical Association of America.

He *had served as consultant to President* Kennedy's Science Advisory Committee, Air Force General Twining's Space Research Committee, IBM Corporation, General Atomic Corporation, North American Aviation Corporation, Hycon Corporation, and other organizations.

Building in Lwow, in which the Ulam family resided, photographed after the war