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An Interview with Stan Ulam and Mark Kac

Mitchell Feigenbaum

I am what is called a mathematical physicist. I take this to mean the utilization of—and sometimes the attendant construction of—mathematics in a context posed by physical reality. Now I suppose that statement would fail to distinguish mathematical physics from mathematics or from physics; after all, numbers and geometry, the stuff at the core of all mathematics, have been abstracted from the context of the physical world. And physics is the hard science, of necessity drawing sharp conclusions only from its mathematical language. Newton had to invent the calculus to extend Galileo's algebraic kinematics to a general framework, and yet Newton is always viewed as a physicist. Evidently the boundary between these disciplines is ultimately blurred, although at a given time in development, the attitudes of the various practitioners can be distinct.

Having exposed my view of no hard distinction between physics and mathematics, I have also exposed a main thrust of the nature of the discussion I had in mind in the following interview. That is, I wanted to explore the (personal) "philosophical" views of just what connections are in the back of theorists' minds that drive the work they perform. It is hard, in understatement, to know a creator's internal vantage point from the technical products in print.

Los Alamos is fortunate in the presence—either on a temporary or permanent basis—of a number of great individuals. I count as one of my fortunes that being here has allowed my coming to know Mark Kac and Stan Ulam. A mutual interest in discussing these matters has, of course, allowed the possibility of this interview. Moreover, these gentlemen embody a tradition of technical education and a viewpoint toward science that, in starting some fifty years ago in a "different" world, are in ways at variance with the more "modern" tradition. Above all, I wanted to explore just what these differences might entail.

As a brief background—both will provide more detail themselves—Kac and Ulam are both internationally known and successful mathematicians. And

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as shall be evident from the interview, both also have a strong enthusiasm in science. Kac has been a pioneer in the development of mathematical probability as well as in its applications (largely to statistical physics). In particular, the modern method of quantization proceeds through a device often called the Feynman–Kac path integral. Similarly, Ulam has made diverse contributions to the various twentieth century branches of mathematics while simultaneously involving himself in a range of theoretical and technological scientific applications. In particular, his name has been associated with the development of the Monte Carlo method of numerical simulation.

A technically oriented reader will find himself disappointed if he expects to hear in any detail of the work they are known for. Rather, what is offered are the reflections of these men, toward the latter parts of their careers, on how they have seen education, mathematics, and science evolve in spirit over the course of their professional lives. Also, their attitudes toward the content and range of their subject will be viewed. It is a regrettable consequence of the medium of the written word that the rich inflection of voice and gesticulation of hand that so often color and amplify the words of these men are not available to the reader. Nonetheless, I hope some of their characteristic charm and humor is conveyed.

FEIGENBAUM: Would each of you give a brief biographical sketch? Stan, would you like to start?
ULAM: My name is Ulam, Stan Ulam. Stanislaw is the real first name. I was born in Poland. I received my doctorate in mathematics from the Polytechnic Institute in Lwów, ages ago. During the early thirties I visited some foreign centers of mathematics. In 1935 I received an invitation to come to Princeton for a few months, to the Institute for Advanced Study. I was not clever enough to see what was coming, really. Stupidity made me not even make such plans; but then I received an invitation from this famous, very world-famous mathematician, one of the great mathematicians of the century, John von Neumann, who was actually only about six or seven years older than I; and so I decided to visit the United States for three months. Of course, there were no planes. I had to go to some port in France to catch a boat to New York. I spent a few weeks in Princeton, and one day at a von Neumann tea, G. D. Birkhoff, who was the dean of American mathematics, was present. He knew a little about my work, apparently from his son, who was about my age, and he asked me when I would come to Harvard. Then I went back to Poland. But the next fall I returned to Cambridge as a member of the so-called Society of Fellows, a new Harvard institution. I was only twenty-six or so. I started teaching right away: first, elementary courses and then quite advanced

courses. And then I became a lecturer at Harvard in 1940. But every year during that time I commuted between Poland and the United States. In the summers I visited my family and friends and mathematicians. In Poland the mathematical life was very intense. The mathematicians saw each other often in cafés such as the Scottish Café and the Roma Café. We sat there for hours and did mathematics. During the summers I did this again. And then in '39, I actually left Poland about a month before World War II started. It was very lucky in a sense. My mother had died the year before the war, and my brother, thirteen years younger, was more or less alone. My father, a lawyer, was busy; he thought it would be good for my brother to come to the United States, too, to study at the university. My brother was seventeen at the time and he came with me in 1939. I enrolled him at Brown University in Providence, which was not too far from Cambridge.

Then in 1940 I became an assistant professor at the University of Wisconsin in Madison. While there—it was in the spring or summer of 1943—I received an inquiry from John von Neumann whether I would be interested in doing some very important war work in a place which he couldn't name, and I was to meet him in Chicago in some railroad station to learn a little bit more about it. I went there; and he couldn't tell me where he was going; and there were two guys, sort of guards, looking like gorillas, with him. He discussed with me some mathematics, some interesting physics, and the importance of this work. And that was Los Alamos at the very start. A few months later I came with my wife, but that is another story. I could talk for hours about the impressions of the trip, of arriving for the first time in a very strange place. But that is already in some books, including my own autobiography. What else would you like to know?

FEIGENBAUM: Why don't you quickly say something about your work?

ULAM: I have been publishing mathematics papers since I was eighteen. Though not very common, neither was it too unusual, because very often mathematicians start very early. I got my Ph.D., as I told you, in Poland. And in this country I published papers as a lecturer at Harvard and at Wisconsin, but the work here in Los Alamos was mainly physics, of course. I had always had some interest in physics, and I had read a lot of relativity, quantum theory, etc. It had been a platonic interest in the sense that most of my early papers were in pure mathematics.

FEIGENBAUM: Mark, would you now say something, as you put it, as Stan's younger colleague?

KAC: I was also born in Poland, although it was not clear that it was Poland. Because, in fact, where I was born, it was czarist Russia, and where Stan was born, it was Austria. In addition to other uncertainties connected with my birth is that my date of birth is not entirely right either, because under the czars they used the Julian calendar. So my birth certificate says I was born on August 3rd, and I maintain this fiction, but in reality I was born on the 16th. I was born 170 kilometers—that is 100 miles— almost directly east of where Stan was born. Nevertheless, within those 100 miles were two completely different worlds, because Poland had not existed as an independent country for 150 years. It was partitioned among Austria, Germany, and Russia, and the cultures of the occupying powers had made an enormous imprint. In my part of the world, nobody spoke Polish; my mother never learned to speak Polish. Anyway, I was born. After an evacuation in 1915 somewhat deeper into Russia, we returned to Poland in 1921, and then I went for my first formal schooling in Polish. Polish was actually the fourth language I learned. I first spoke Russian, because that was the language that everyone spoke; then, when we came back home after the evacuation, my parents engaged for me a French governess, a French lady who was a widow of a White Russian officer. For three years she came for half a day, and we'd conjugate French verbs, and I hated it. Then my father was briefly a principal of a lay Hebrew school. It was not a religious school, but all the subjects were taught in Hebrew, so I learned Hebrew, which I promptly forgot. Then, finally in 1925, at the age of eleven, I entered a Polish school, a very well-known Polish school, the Lycée of Krzemieniec. The town where I was born had a certain part in Polish history, one of the reasons being that one of the two great Polish romantic poets, Juliusz Slowacki, was born there (almost every Polish child would know the name). In addition, another very famous citizen of that town is Isaac Stern, whose parents were wise to take him out of Poland when he was only nine months old. After secondary school education I went to the university in the same town where Stan was born and where he studied, except he was in the Engineering school, which had, remarkably enough, a division that was devoted to pure science, that is to say, mathematics and physics.

I went to the regular university and I was, and still am, five years younger. At that time Stan was already a legend—and to me looked infinitely old. He was only twenty-two and I was seventeen. I met him for the first time, briefly, and it will be a fiftieth anniversary of that event next year, when he was awarded his doctorate in 1933. (Actually, I thought it was this year, but he corrected me, and he ought to know better when he got his doctorate.) I graduated, got my doctorate, in 1937, and unlike Stan I wanted to get out of Poland very badly. I did not know the disaster was going to be of the magnitude it turned out to be, but it was obvious that Europe, especially eastern Europe, was not the place to stay. But it was not very easy to get out in those days.

Now, two episodes I have recalled because Mitchell and I have been tracing back the autobiographical part. In 1936, maybe '37, just before the time I got my doctorate, I was trying desperately to get out of Poland, and I would read *Nature*, because in *Nature* there would be ads of various positions. Most positions required being a British subject, but one of them (at that time, by the way, I knew not a word of English) was an ad for a junior lecturer in the Imperial College of Science and Technology at the salary of 150 pounds per annum, which in those days was about 750 dollars. Even then that was not very much money, and I thought that no self-respecting British subject would ever want to apply for a job like this. So I spoke to my teacher, Hugo Steinhaus, and asked whether it would be a good idea to apply, and he, partly in jest, partly seriously, said, "Well, let's estimate your chances of getting the job. I would say it is 1 in 5000. Let's multiply this by the annual salary. If this comes out to be more than the cost of the postage stamp, then you should not apply. If it is less than the cost of the stamp, you should." Well, it turned out to be a little bit less than the cost of the stamp, so I wrote. I got a letter from them later on saying that unfortunately the job was filled, so there had been after all a British subject who wanted the 150 pounds per annum. Many, many years later when I was in England, I was invited to give a lecture at the Imperial College of Science and Technology, and I said to them, "You know, you could have had me for 150 pounds per annum." I believe that they actually looked up and found the correspondence. This anecdote reminds me that, when I finally decided to come to the United States, it was very difficult to get visas, because already the German refugees were coming. It was a terrible time, and I managed to get only a visitor's visa for a six-month period. The Consul made me buy a round-trip ticket just to make sure that I would return. The return portion of the ticket I still have, and it was for a boat that was sunk in the early days of the second world war. A memento.

It was Hugo Steinhaus, my teacher and my friend, a very well-known Polish mathematician, who tried very hard to help me get out. And finally he succeeded in a very simple way by helping me get a small fellowship to go abroad to Johns Hopkins University. It is curious how small things change one's life, and in effect possibly save one's life. I applied for that scholarship in 1937, immediately after getting my doctorate and did not get it. I thought it was a tremendous injustice, but I got it a year later; that saved my life because if I had gotten it a year earlier, I would have been compelled to go back. This way the war caught me in this country and literally saved my life. I was at Johns Hopkins when the war started, and then I got an offer to Cornell, where I spent twenty-two very happy years. (Mitchell is going to be my successor there.) In fact, my whole family, that is, my acquired family in the United States, my wife and both my children, are native Ithacans. And I have actually lived in Ithaca longer than in any other place in the world.

ULAM: So it is the converse of Odysseus.

KAC: When I left Cornell I was forced to make a very brief speech, and I said, "Like Ulysses I, too, am leaving Ithaca, the only difference being I'm taking Penelope with me." That was how it was. I was then for twenty years at Rocky U, Rockefeller University, in New York City and then decided to spend my declining years, as it were, where there is more sun and less ice. So I am now at the University of Southern California, a little bit west of here.

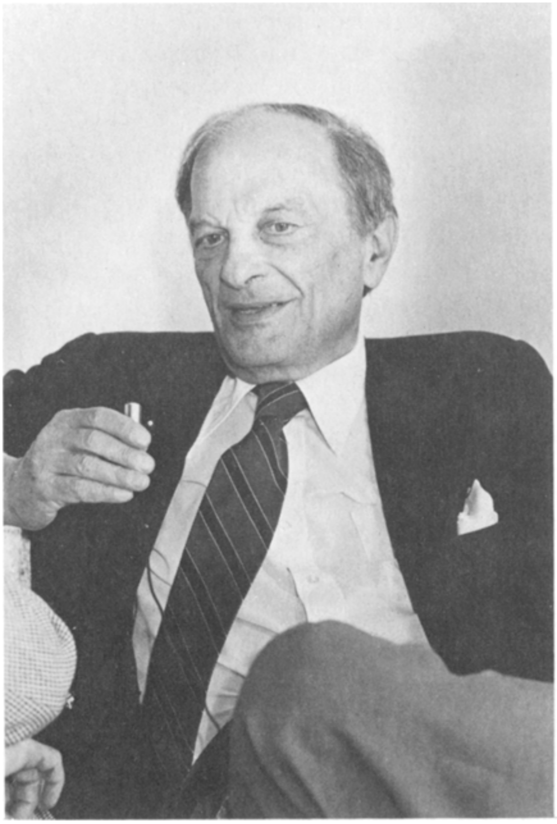
FEIGENBAUM: I guess it's time to interrupt you from these reminiscences. Stan, perhaps you can say something about how you became interested in mathematics?

ULAM: As a young boy at the age of ten, I was very interested in astronomy and then in physics. I was reading popular books on astronomy; there weren't as many, and they were not as beautiful ones as now with incredible illustrations, but still, that was my passion. An uncle gave me a little telescope for my birthday when I was eleven or twelve. By then I was trying to understand the special theory of relativity of Einstein, and I think I had a pretty good qualitative idea of what it was all about. Then, later, I noticed that I needed to know some mathematics, so I went beyond what was given in the high school, gymnasium, as it was called. Students started gymnasium at age ten and went to age eighteen. When I was fourteen, I decided to learn more mathematics by myself, and I was sixteen when I really learned calculus all by myself from a book by Kowalevski, a German not to be confused with Sonia Kowaleska, a famous nineteenth century Russian woman mathematician. Then I read also about set theory in a book by Sierpinski, and I think I understood that. We had a good professor in high school, Zawirski, who was a lecturer in logic at the university. I talked to him about it then and when I entered the Polytechnic Institute.

FEIGENBAUM: He was teaching at the high school?

ULAM: Yes, he was teaching in high school to make money, because lecturers earned hardly any money at the university. When I entered the university, I attended a course by Kuratowski, a freshman professor who had just come from Warsaw. He was only thirty-one years old; I was eighteen. He gave an elementary course on set theory, and I asked some questions; then I talked to him after classes, and he became interested in a young student who evidently was interested in mathematics and had some ideas. I was lucky to solve an unsolved problem that he proposed.

FEIGENBAUM: Stan, did you feel at that point that your interests were



changing from astronomy and physics and relativity toward mathematics?
ULAM: No, in fact, even now I don't think the interests have changed. I am interested in all three. Of course, I did much more work in pure mathematics than in applications or in theoretical physics, but my main interests remain. I have to make a confession: nowadays I don't read many technical mathematical journals—rather, I read what is going on in astronomy and astrophysics or in technical physics in *Astrophysics Journal* and *Physics Today*. It always seems to me much more understandable. You know, this specialization in each science, especially in mathematics, has proceeded much apace the last few years. Mathematics is now terribly specialized, more so than, say, physics. In physics there are more clearly defined central problems than in mathematics itself. Of course, mathematics still has many important problems, fundamental ones.
FEIGENBAUM: You feel that this specialization is unfortunate?

ULAM: Oh, yes. Both of us have very similar views, it turns out, about science in general and about mathematics and physics in particular.
FEIGENBAUM: Mark, how did you begin in mathematics?
KAC: Stan and I are running in parallel. Actually my interest in mathematics also began very young, and probably I romanticize a little. (I was saying to Mitch that if you try to think of something that happened sixty years ago, it is not always infinitely reliable.) My father had a degree in philosophy from the University of Leipzig in Germany and knew mathematics. He also later got a degree from Moscow in history and philology, so he knew, among other things, all the ancient languages. Anyway, he earned a living during the war by giving private tutorials in a little one-room apartment, and among other things he tutored in elementary geometry. I heard all these incredible things: from a point outside a straight line you can drop a perpendicular and draw one and only one parallel, and such and such angles are equal. I was four years old, five maybe, and all these wonderful, ununderstandable sounds, in what seemed like ordinary language, impressed me. I would absolutely pester him to try to tell me what it was; in self-defense he began to teach me a little bit of elementary geometry, and somehow the structure, that there is such a fantastic tight structure of deduction, impressed me when I was a very young boy. In fact, at that time my father despaired because at the same time I was exceedingly bad learning multiplication tables. That one could know how to prove theorems of elementary geometry without knowing how much seven times nine was seemed more than slightly strange. That was the beginning of my interest in mathematics, but like Stan the interest in science came almost at the same time, primarily by reading popular books. One book, available in Russian translation, was called a *Short History of Science* and was by an English lady whose name was Arabella Buckley, or something of the sort. It was fascinating! I then later read Faraday's *Natural History of the Candle*, which is one of the great books. In school, when I finally went to the gymnasium, as it was called, I was equally interested and equally good in mathematics and physics, but finally decided on mathematics.

Actually, an event during the summer before my last year at the gymnasium, among other things, influenced my decision. Here's how it was. My mother had envisaged that I would pursue something sensible like engineering, but in the summer of 1930 I became obsessed with the problem of solving cubic equations. Now, I knew the answer, which Cardano had published in 1545, but what I could not find was a derivation that satisfied my need for understanding. When I announced that I was going to write my own derivation, my father offered me a reward of five Polish zlotys (a large sum and no doubt the measure of his skepticism). I spent the days, and some of the nights, of that summer feverishly filling reams of paper with formulas. Never have I worked harder. Well, one morning, there it was—Cardano's formula on the page. My father paid up without a word, and that fall my mathematics teacher submitted the manuscript to *Mlody Matematyk (The Young Mathematician)*. Nothing was heard for months, but as it turned out, the delay was caused by a complete search of the literature to ascertain whether I had not in fact "rediscovered" a derivation. They found that my derivation was, after all, original, and so it was published. When my gymnasium principal, Mr. Rusiecki, heard that I was to study engineering, he said, "No, you must study mathematics; you have clearly a gift for it." So you see. I had very good advice.

At the university I actually thought of possibly starting physics, but physics in Lwów was very poor, theoretical physics especially. Mathematics was extremely good and very lively, so it was very easy to get involved in a tremendously exciting and energetically developing subject rather than struggle with a subject in which there was not really much activity. I took, naturally, courses in the physics department and took some exams in theoretical physics, but my interest, real interest, in physics was kindled considerably later.

FEIGENBAUM: I have the impression that somehow science and mathematics have similarly cross-fertilized in your minds and that you have—I think you have conveyed this feeling—some kind of intuition that is very important toward the way that you view mathematics.

KAC: Yes, this may be of interest to modern readers, and I am sure that Stan will confirm what I say. We belong to an academic generation that was only a little bit removed from the heroic times in the great centers of mathematics, Göttingen and Paris. There the distinction between mathematics and physics was not made as jurisdictionally sharp as it is now. The great mathematicians of that era, Poincaré and Hilbert, both made extremely important contributions to physics, Poincaré especially. Our teachers were taught physics and knew it. Banach, for instance, who is primarily known as the creator of the school of functional analysis and who is probably the greatest Polish mathematician of all times, taught mechanics. He wrote a very good textbook on it. The whole distinction of now you are a physicist, so you do this, now you are a mathematician, so you do that, was intellectually blurred. There were, of course, people who were more concrete, and others who were more abstract, and people who were more interested in this or that. But there wasn't any of this kind of professionalism, nor the almost union card distinctions that are prevalent now, so that it was easy, not only because our makeups were conducive to do this, but also because nobody told me that I should not study physics because if I didn't study just mathematics, I'd never catch up. The idea of catching up, of something running away, never existed. Isn't that so?

ULAM: Absolutely. You are talking about a very long time ago, fifty years ago, and you know—some time ago I had this thought—my life, and Mark's too, occupies more than almost two per cent of the recorded history of mankind. You see, fifty or sixty years is that much. That it is a sizeable fraction of the whole history that we know about is a strange and very terrifying thought. Things have changed in many ways, not only in technology but in attitudes.

FEIGENBAUM: Here is a question. When you mention that there is something negative in your minds about specialization and that you have this connection in your minds between physics and mathematics, is there some kind of a special intuition that you think comes from these two things working together? Do you feel that's an important ingredient?

ULAM: You see, it depends very much on the person. Some mathematicians are more interested in the formal structure of things. Actually, for people in general there are two types of memory that are dominant, either visual memory or auditory memory, and seventy-five per cent (this Mendelian fraction) supposedly have visual memory. Anyway, some people have a very purely verbal memory, more toward the logic foundations and manipulation of symbols, rather than toward imagining physical phenomena. When somebody mentions the word pressure to me, I sort of see something, some kind of confined hot or turbulent material.

KAC: I cringe.

ULAM: Right, but other people, von Neumann for example, are more logically minded. To him pressure was, so to say, a term in an equation. I rather suppose that he did not visualize situations where pressure would do this or that, but he was also very, very good in physics. Certainly there are different attitudes in ways of thinking. Some mathematicians are more prone to the physical. Also, we don't really know too much about this. It could be a question of accidents in your childhood and in your youth or of the way you learned things.

FEIGENBAUM: Do you think that this kind of intuition that you have is more special to yourself? I mean by that, if you think back to when you started doing mathematics, were more people then like yourself rather than more formal.

ULAM: No, no. I don't think so. Many mathematicians that I knew at that time were different from Mark Kac and myself in their attitude toward physics. Even now in this country, I would say ninety per cent or more of mathematicians have less interest in physics than we do.

KAC: Partly, of course, it is educational. I think the education in this country has been, especially higher education, singularly bad. For instance,

it is perfectly possible for a young man to get a doctorate in mathematics in a reputable school, like Harvard, without ever having heard of Newton's laws of motion.
ULAM: I was on a committee of the American Mathematical Society when I discovered that you could get a Ph.D. at Harvard and other places without knowing Newton's laws of motion, which were actually one of the central motives for the development of calculus, you might say. That is how it is now.
KAC: We were exposed to chemistry, to physics, to biology; there were no electives when you were in secondary school. Secondary schools in Europe, in Poland, in France were in a certain sense harder than the university because you had to learn a prescribed curriculum. There was no nonsense. If you were in a certain type of school, you had to take six years of Latin and four years of Greek and no nonsense about taking soul courses or folk music, or all that. I have nothing against taking such courses, except that it has become a substitute. You had to take physics, you had to learn a certain amount of chemistry, of biology, and if you didn't like it, so it was. But if there was some kind of resonating note in you, then you were introduced to it early. At the university you really specialized, although not entirely; every mathematician had, for example, to pass an exam in physics and even, God help me, go through a physics lab. That was one of my most expensive experiences because, being rather clumsy, I broke more Kundt's tubes than I could afford. Stan made an extremely important point to which I can bring a little extra light. I heard probably one of the last speeches by von Neumann. It was in May 1955. (In October of that year, while I was in Geneva on leave, it was discovered that he had incurable cancer, and he died then sometime later in 1957.) He was the principal banquet speaker at the meeting, I believe, of the American Physical Society in Washington. I was there, and I went to the meeting, and after the speech we had a drink together. His speech was, "Why Am I Not a Physicist?" or something of the sort. He explained that he had contributed technical things to physics; for example, everybody knows what a density matrix is, and it was von Neumann who invented density matrices, as well as a hundred other things that are now, so to speak, textbook stuff for theoretical physicists. But he, nevertheless, gave a charming and also moving talk about why he was not really a physicist, and one thing he mentioned was that he thought in terms of symbols rather than of objects; I am reminded that his friend Eugene Wigner hit on it correctly by saying that he would gladly give a Ph.D. in physics to anyone who could really teach freshman physics. I know what he meant. I would attempt, I wouldn't be very good at it, but I would attempt to teach a first semester course in quantum mechanics, and I would probably teach it reasonably well. But I would not

know how to teach a freshman course in physics, because mathematics is, in fact, a crutch. When you feel unsafe with something, with concepts, you say, "Well now, let's derive it." Correct? Here is the equation, and if you manipulate with it, you finally get it interpreted, and you're there. But if you have to tell it to people who don't know the symbols, you have to think in terms of concepts. That is in fact where the major breach between the two—how to say—the two lines of thought come in. You are either like von Neumann, and I am in that sense closer to him, or you are like Ulam, who when you say pressure, feels it. It is not the partial derivative of the free energy with respect to volume; it is really something you feel with your fingers, so to speak.

FEIGENBAUM: But isn't it nonetheless true that any good mathematician has a very strong conceptual understanding of the things he is working on? He isn't just doing some succession of little proofs.

KAC: Well, the really good ones, yes. But then, you see, there is a gamut, a continuum. In fact, let me put this in because I would like to record it for posterity. I think there are two acts in mathematics. There is the ability to prove and the ability to understand. Now the actions of understanding and of proving are not identical. In fact, it is quite often that you understand something without being able to prove it. Now, of course, the height of happiness is that you understand it and you can prove it. The next stage is that you don't understand it, but you can prove it. That happens over and over again, and mathematics journals are full of such stuff. Then there is the opposite, that is, where you understand it, but you can't prove it. Fortunately, it then may get into a physics journal. Finally comes the ultimate of dismalness, which is in fact the usual situation, when you neither understand it nor can you prove it. The way mathematics is taught now and the way it is practiced emphasize the logical and the formal rather than the intuitive, which goes with understanding. Now I think you would agree with me because, especially with things like geometry, of which Stan's a past master, seeing things—not always leading neatly to a proof, but certainly leading to the understanding—ultimately results in the correct conjecture. And then, of course, the ultimate has to be done also—because of union regulations, you also have to prove it.

ULAM: Let me tell you something. It so happens that I have written an article for a jubilee volume in honor of this gentleman here, Mark Kac, on his whatever anniversary, a volume which has not yet appeared. But the article is about analogy and the ways of thinking and reasoning in mathematics and in some other sciences. So it is sort of an attempt to throw a little light on what he was just talking about. These things are intertwined in a mysterious way, and one of the great hopes, to my mind, of progress, even in mathematics itself, will be more formalizing or at least

understanding of the processes that lead both to intuition and to then working out not only the details but also the correct formulations of things. So there is a very, very deep problem and not enough thought has been really given to it, just cursory remarks made.
FEIGENBAUM: Do you have a hope that people will be able to formalize these things, the serious components?
ULAM: It is now premature, but some partial understanding of the functioning of the brain might appear in the next twenty years or even before—some inklings of it, more than is known at present. That is a marvelous prospect. You see, if I were a very young man, maybe I would be working more in biology or neurology, that is to say the anatomy of the brain, and trying to understand its processes. Mark and I, driving to the Laboratory this morning from Santa Fe, were discussing how children learn to talk and use the phrases they hear—learn to use them correctly in different contexts with changed elements. It is really a mysterious thing.

FEIGENBAUM: Let's pick up on the last thing you said—that maybe there is a chance of understanding how the brain works. When you say that, what comes to my mind is that there are problems that *in principle* you can think of—for example, fully developed turbulence in a fluid and perhaps the brain. It might be that these problems really will rely on an immense number of details, and maybe there won't be any nice theories such as we've known how to write so far, and you really just have to put all these details on a computer. Do you have any thoughts about that and what it implies for the limitations of future mathematical effort?
ULAM: Well, actually, computers are a marvelous tool, and there is no reason to fear them. You might say that initially a mathematician should be afraid of pencil and paper because it is sort of a vulgar tool compared with pure thought. Indeed, say thirty years ago, professional mathematicians were a bit scared, as it were, of computers, but it seems to me that for experimentation and heuristic indications or suggestions, it is a marvelous tool. In fact, the meeting* that is going on right now, to a large extent, is possible because so much has been discovered experimentally.
FEIGENBAUM: That is absolutely true.
ULAM: So in physics, experiments lead finally to problems and to theories. Experimentation in mathematics could be purely mental, of course, and it was largely so over the centuries, but now there is an additional wonderful tool. So in answer to your question about understanding the brian, yes, it seems to me, indeed.
FEIGENBAUM: Certainly one has learned now, or is at the first stage of really learning, how to do experiments on computers that can begin to furnish intuition for problems that otherwise were impenetrable. The new intuition then enables you to write a more analytical theory. Do you think there are problems that are so complex that you won't be able to get that kind of a handle on them? For example, maybe memory in a brain has no global structure, but rather entails nothing more than a million different distinctly stored things, and then you wouldn't write any theory for it but rather only simulate such a system on a computer. Do you think there may be some limitation to what kinds of things you can analyze?
ULAM: It depends on what you call theory. I noticed you said the analytical method; it means that by habit and tradition you think that is the only way to make progress in pure mathematics. Well it isn't. There may be some eventual super effect from the use of computers. I was involved from the beginning in computers and in the first experiments done

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* *"Order in Chaos," a conference on the mathematics of nonlinear phenomena. Sponsored by the Center for Nonlinear Studies at Los Alamos National Laboratory, May 24–28, 1982.*

in Los Alamos. Even in pure number theory there were already tiny little amusements from the first. A time may come, especially because the overspecialization of mathematics is increasing so much that it is impossible now to know more than a small part of it, that there will be a different format of mathematical thinking in addition to the existing one and a different way of thinking about publications. Maybe instead of publishing theorems and listing them there will be a sort of larger outline of whole theories, and individual theorems will be left to computers or to students to work out. It is conceivable.
KAC: Slaves.
ULAM: Mathematics, which hadn't changed much in its formal aspect in the last 2000 years, is now undergoing some change. The great discoveries of this century, Gödel's, are of tremendous philosophical importance to the foundation of mathematics. Gödel proved there are statements that are meaningful but that are not demonstrably true or false in a given system of axioms. Hilbert, of course, was *the* great believer of the formal system for all mathematics. He said, "We will understand everything, but it all depends on what basis." That is no longer so. You see, the axiom systems themselves change as a result of what you learn by physical experimentation or by mental experimentation. I think Mark probably has a different perspective.
KAC: I don't want to step out too far because I am a believer in one of Wittgenstein's dicta: that about things one knows nothing, one should not speak. I wish more people followed this dictum. Well, computers play a multiple role: they are superb as tools, but they also offer a field for a new kind of experimentation. Mitchell should know. There are certain experiments you cannot perform in your mind. It is impossible. There are experiments that you can do in your mind, and there are others you simply can't, and then there is a third kind of experiment where you create your own reality. Let me give you a problem of simple physics: a gas of hard spheres. Now nature did not provide a gas of hard spheres. Argon comes close, but you can always argue that maybe, because of slight attractive tails, something is going to happen. There is no substance—nature was so mean to us that there is no gas of hard spheres. And it poses very many interesting problems. It is child's play on the computer to create a gas of hard spheres. True, the memories are limited, so that, as a result, we can't have 10^{23} hard spheres, but we can have thousands of them, and actually the sensitivity to Avogadro's number is not all that great. We can really learn something about reality by creating an imitation of reality, which only the computer can do. That is a completely new dimension in experimentation. Finally, I may be misquoting him, but a very famous contemporary biologist, Sidney Brenner, who gave a lecture at Rockefeller

University while I was still there, said that perhaps theory in biology will not be like that of physics. Rather than being a straight deductive, purely mathematical analytical theory, it may be more like answering the following question. You have a computer, and you don't know the wiring diagram, but you are allowed to ask it all sorts of questions. Then you ask the questions, and the computer gives you answers. From this dialogue you are to discover its wiring diagram. In a certain sense, he felt that the area of computer science—languages, theories of programming, what have you—may be more of a model for theorizing in biology than writing down analytic equations and solving them.

FEIGENBAUM: A more synthetic notion.

KAC: Yes. In fact, I think we will go even farther in this direction if we introduce, somehow, the possibility of evolution in machines, because you cannot understand biology without evolution. In fact, my colleague Gerry Edelman, whom you know very well and who is a Nobel laureate in biochemistry, is now "into the brain" and is trying to build a computer that has the process of evolution built into it so that you evolve programs: you start with one program that evolves into another, etc. It is an attempt to get away from the static, all-purpose Cray, or whatever it is, and to endow the computer with that one extraordinary, important element of life, namely evolution. I also feel like Stan; if I were younger—Si la jeunesse savait; si la vieillesse pouvait,—as you say in French,* I'd also get into biology. Those are fantastically challenging problems, and they are problems that call for formulation, not only for solution. That's also exciting, to be present at the creation, to formulate the problem.

ULAM: I might add something to it. In fact, to some extent, the differences we talked about between mathematicians and physicists, or the bent of mind, is of that sort. I also wrote, a very crude picture, about the following system: mathematicians start with axioms and draw consequences, theorems. Physicists have theorems or facts, observed by experiment, and they are looking for axioms, that is to say, laws of physics, backwards. Just as you said, the idea is to deduce this system of laws or axioms from which the observed things would follow. Actually the so-called Monte Carlo approach is a little that way, even in problems of a very prosaic, very down-to-earth nature. You manufacture your own world, as you say, of hard spheres, or what have you.

FEIGENBAUM: Mark, I want to turn to something that you mentioned yesterday. You offered a quotation that "axiomatization is the obituary of a great idea." In context, you were talking about how sometimes you can

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* *"If youth only knew; if age only could."*

sort of overkill the mathematics and leave it dead in some way, as opposed to letting it speak for itself and be alive. Will you amplify on the soul of mathematics?
KAC: I will try. There is, of course, axiomatization and axiomatization. If, indeed, we think of the process of natural sciences as the discovery of what we call laws of nature that you can say are its axioms, then, to the contrary, such a discovery is a birth announcement. But, for instance, take geometry: that's one of the oldest, best known parts of human knowledge and, in fact, one of the great achievements of the Greeks. Euclid is probably being given most of the credit, but it was a communal affair, this axiomatization (axiomatization in the sense that from a simple number of seemingly self-evident statements, one can deduce and create a whole world of facts). Then it turned out there were cracks in this edifice; suddenly there were certain concepts that were not fully axiomatized. The ultimate axiomatization of geometry came with Hilbert in 1895, 2000 years after Euclid. That was an obituary in a certain sense, because then it (axiomatization or geometry) could be relegated essentially to a computer. Once the subject becomes so well organized that every single thing can be reduced to a program, then there is nothing more to be done. In fact, Gödel gave hope by proving that reduction is impossible in the somewhat wider system of mathematics, that always, no matter how large, how complex a system is, there will be statements that you won't be able to prove or to disprove. That means there is always the possibility of creation, another axiom, or something or other. There is this tendency among mathematicians of trying to understand through axiomatization.
ULAM: And in physics this is nonsense.
KAC: There are people who still try to axiomatize thermodynamics. The very last thing anybody should be doing is axiomatizing thermodynamics. I mean, first of all, most physical theories, though thermodynamics, I must say, is one of the most durable ones, are only temporary. They change; they evolve. So why the heck should one axiomatize something that the next day is going to be obsolete? But, on the other hand, many mathematicians who are trained formally feel there is no other way to perceive a subject but by strict axiomatization. And worse yet, they try to teach little children in schools like that. To teach geometry through the complete systems of axioms is stupid. Teaching geometry is to tickle a young man's or a young woman's imagination in solving all the wonderful problems. It should not be work to prove that if A is between B and C, and D is between A and C, then D is between B and C. You'll just draw a picture, and it is trivially evident.
ULAM: Take the new math, for instance.
KAC: I could speak hours against new math.

ULAM: It's waning, isn't it?
KAC: Yes, that's flogging a dead horse.
FEIGENBAUM: Do you think that this idea of people's just being trained from a purely axiomatic viewpoint is a growing phenomenon, or has it always been so amongst mathematicians and scientists?
KAC: I really don't know. I know only a very few people.
FEIGENBAUM: You alluded to that situation in saying it's now taught, for example, in terms of new math, although you say that the new math is dying.
KAC: It was true for a while because, somehow, a group of mathematicians sold this idea to poor high school teachers, who didn't even understand what it was all about and who then taught geometry and other things only through axioms. There are two principles of pedagogy which have to be adhered to. One is, "Tell the truth, nothing but the truth, but not the whole truth." That I had from a former colleague who is now unfortunately deceased. The other one is, "Never try to teach anyone how not to commit errors they are not likely to commit." Now, to give you an example. New math spends an awful lot of time in second grade, God forbid, in trying to tell the little kids that you write a little three and you write a big three, and yet the little three and the big three symbolize the same thing because it is the cardinal number of a set of three elements. Correct? That is sheer idiocy. If a kid is logically sophisticated and is bothered by it, then I would take him aside and give him special training, but to create confusion in the mind of a child who is perfectly willing for a while to know that this three and this three, even though one looks bigger than the other, represent the same thing—leave it be! I know it sounds a little funny, but I feel very strongly about it. The need for precision, for logic, must be not imposed from outside. It must be coming from within. If somebody really feels uncomfortable, then he or she has an enormously highly developed sensitivity to finer logical points.
ULAM: I try to make jokes about it. If you print a page of mathematics or anything else, it is not invariant, because if you look at it upside down, it looks different. So the idea in new math was to write in such a way that no matter what angle you look at it, it is the same. That's an ultramathematical point of view.
FEIGENBAUM: Another question I was thinking about was, in reminiscing back to the Scottish Café, what was the excitement for mathematics? Was there some feeling at that time that there was a scheme of understanding things that would continue into the future?
KAC: Stan, you are much more strongly connected with the Scottish Café.
ULAM: I don't think so really. People were so immersed in the actual problems. Occasionally there would be some kind of speculation about the

more remote future. For example, in Lwów, my home town in Poland, Banach, this famous mathematician whom I think you mentioned earlier, decided to have a big notebook kept in the Scottish Café where we assembled every day. It was a book in which problems to be solved, remarks, and ideas were written down. It was kept in the Café, and the waiter would bring it when we came in. A lot of interesting problems were written up. The book, by the way, is being published by Birkhäuser. I guess I started to say that occasionally there would be some speculation. The mathematician Mazur once said, for example, "There must be a way to produce automatic arrangements which will reproduce themselves." That was long before von Neumann actually went into this whole complex of problems and found one way to do it. Speculations of this sort appeared sporadically, but on the whole it was a more down-to-earth, mathematically defined collection of problems which interested us in various fields, such as functional analysis and set theory, fields which were in those days still young.

KAC: But aging already.

ULAM: Perhaps.

KAC: It is difficult to say. Functional analysis, of course, was Banach's creation, and partly Steinhaus's. Toward the end of my student career, it was Banach, himself, I felt, and also Mazur, who began to look for other worlds to conquer.

ULAM: The nonlinear program of studies.

KAC: That's right. Banach also was reading. I can remember because I was once in his office over some trivial matter, and he was reading Wiener's early papers on path integrals. I agree with Stan, though I was less of a habitué of the Scottish Café. First of all, my teacher, Steinhaus, frequented a more elegant establishment where there were special things to eat, and all that. Secondly, I was financially somewhat less affluent than Stan—I was, as Michael Cohen, one of our mutual friends, says, independently poor. And it did cost a little to visit in the Café. What happened primarily was that people discussed problems of interest and then people thought about them. If, indeed, nothing immediate came out of the problem, nothing that appeared to be interesting and promising, then it would be recorded in the notebook. Actually, very few problems in the book proved to be completely trivial. Many of them had a very noble history. Papers were written on many of them, and some are still unsolved. In fact, I want to make a kind of a footnote here. It is so remarkable that the Poles did not publish this book; rather, it has been published in the United States through the efforts, really, of a very remarkable young friend of ours by the name of Dan Mauldin, who is a professor of mathematics at, of all the impossible places, North Texas State University in Denton, Texas. He is a first-rate mathematician, and he has the Polish soul with regard to mathematical problems. It would be interesting to interview him, because he was on his way to becoming an All-American linebacker on the famous Longhorn team, and he gave it up for mathematics.

ULAM: Yes, he was on the Texas football team and played in championship games.

KAC: And then to the disgust of his coach, in his senior year, when he would really do tremendous things, he gave up football and started worrying about set theory.

ULAM: He was offered a car and money.

KAC: A house and everything. It's rather interesting what passions mathematics can engender.

ULAM: One thing you forgot to say—one motive in mathematics is the feeling that you can do something by yourself. I think it is present in almost all mathematicians. One motive for doing mathematics is that suddenly you feel the ability that you are good at something. Very human. Nothing wrong with that feeling.

KAC: Very human, in fact. Actually, I don't think it is really either understood, or perhaps not even understandable at all, how some problems generate passion. Some of them, by the way, ultimately prove to be of relatively little importance. I remember one in connection with Stan. Stan generates problems and conjectures at probably the highest rate in the world. It is very difficult to find anybody in his class in that. Many of them we discuss. He came with one and said, "Look, I thought of the following modification of Fibonacci numbers." With ordinary Fibonacci numbers you start with 1 and 1 and add them, obtaining 2 as the third member of the sequence. Then you add 2 and 1, obtaining 3, then 3 and 2, which gives 5, etc. In other words, the $(n+1)$th member of the sequence is the sum of the nth member and the $(n-1)$th member. Symbolically, $a_{n+1}=a_n+a_{n-1}$ with $a_1=a_2=1$. But in Stan's idea, the formula for a_{n+1} is now $a_{n+1}=a_n+$ either $a_1, a_2, \ldots, a_n$, each taken with probability $1/n$. My God, it is interesting as a coffee house conversation, but for some strange reason, it caught me, and I worked on it, and I even found the mean of a_n, and even the variance. And the variance is given by a tremendous formula with a square root of 17 in it. It even appeared as a little Los Alamos report. I probably spent, easily, a week of hard work on it. Why? I have no idea except I couldn't let the damned thing alone.

ULAM: What you did with the Fibonacci-like rule was beautiful work, and it has a certain simplicity, like the problem itself. And the solution was unexpected because a_n grows exponentially, not with respect to n, but with respect to the square root of n...

KAC: Square root of n, with a complicated constant. There is a point to it because in constructing the sequence, you need at every stage to know *all* the preceding terms—a highly non-Markovian affair. At the time when I was playing with it, it was almost like being an alcoholic. You know it isn't good for you.

ULAM: Another interesting problem is still unsolved—Fermat's. The sum of two squares can be a square, but the sum of two cubes cannot be a cube, and so on. Nobody can prove it for arbitrary powers. Of course, for cubes, quartics, and so forth, but in general, nobody has been able to do it. It seems like a silly little puzzle, and yet so many people worked on it that as a matter of fact some of the efforts to solve it gave rise to much of the modern algebra. This is a strange thing. The mathematical ideal theory and other algebraic theories came from efforts to solve this silly puzzle.

KAC: So you never can tell. You never can tell. Usually these puzzles, the good ones, generate some tremendous things later on, while others of them die. It is very much like survival of the fittest.

ULAM: Or some kind of mysterious thing about the problems that makes them important in the future. It is impossible to tell logically.

FEIGENBAUM: You are almost saying that the problems have a teleological spirit to them and that you don't necessarily realize their unique position at the time they're done.

ULAM: No, one shouldn't be completely mystical, but one day maybe a little will be understood. There must be some...

KAC: Oh, come on, let's be mystical! Why not?

ULAM: So far we are.

FEIGENBAUM: One last question. Have you ever had long-range hopes of finding a good way to analyze a problem and then seen these hopes realized over many years? I think in physics very often there are programs that are set out. Someone has an idea, there is a way you can do the problem, and a lot of people will work on it, perhaps over ten years; sometimes it pans out and sometimes it doesn't.

KAC: I think the best example of that is the recent solution of the classification of all simple groups, finite groups. That is really one of the few genuinely collective efforts in mathematics, including the computer by the way, and that was a program, too, because there were various breakthroughs, understandings came from various places. Well, when it became clear that the problem of classifying simple groups probably could be solved, then an enormous human machinery was created to solve it. In general, mathematicians, even much more than theoretical physicists, tend to be loners. They are collaborative, but basically there are very few papers with, say, more than three coauthors. It would be interesting to plot a graph: by the time it is five authors, the graph hits zero.

ULAM: In mathematics it is zero. It is not uncommon in physics. In

answer to your question, Mitch, Newton said something like—I have to paraphrase it, "If I have achieved something in my life in science, it is because I have thought so long and so much about these problems."
FEIGENBAUM: He also said that if he was able to see further than other people, it was because he was standing on the shoulders of giants.
KAC: Sidney Coleman paraphrased that with, "If I was able to see farther, it was because I was surrounded by midgets."
FEIGENBAUM: What are the things that you have done that you feel most warm towards?
KAC: To begin with, I was always interested in problems rather than in theories. In retrospect the thing which I am happiest about, and it was done in cooperation with Erdös, who also occasionally comes to Los Alamos, was the introduction of probabilistic methods in number theory. To put it poetically, primes play a game of chance. And also some of the work in mathematical physics. I am amused by things. Can one hear the shape of a drum? I also have a certain component of journalism in me, you see; I like a good headline, and why not? And I am pleased with the sort of thing I did in trying to understand a little bit deeper the theory of phase transitions. I am fascinated, also, with mathematical problems, and particularly, as you know as well or better than I, the role of dimensionality: why certain things happen in from three dimensions on and some others don't. I always feel that that is where the interface, will you pardon the expression, of nature and mathematics is deepest. To know why only certain things observed in nature can happen in the space of a certain dimensionality. Whatever helps understand this riddle is significant. I am pleased that I, in a small way, did something with it. And you, Professor?
ULAM: I don't know. I think I was sort of lucky in a number of instances and not so clever. Dumb but lucky. Originally I worked in set theory and some of these problems are still being worked on intensively. It is too technical to describe: measurable cardinals, measure in set theory, abstract measure. Then in topology I had a few results. Some can be stated popularly, but we have no time for that. Then I worked a little in ergodic theory. Oxtoby and I solved an old problem and some other problems were solved in other fields later. In general I would say luck plays a part, at least in my case. Also I had luck with tremendously good collaborators in set theory, in group theory, in topology, in mathematical physics, and in other fields. Also some common sense approaches like the Monte Carlo method, which is not a tremendously intellectual achievement but is very useful, a few things like that.
KAC: I must interrupt because it's time for the afternoon session, but let me end by saying that it is the deserving ones who are also lucky. ■

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