An Interview with Stan Ulam

Anthony Barcellos

Stanislaw M. Ulam spent the winter quarter of the 1978–79 academic year at the Davis campus of the University of California. As Distinguished Visiting Professor of the College of Letters and Science, Ulam presented weekly seminars that touched on such topics as probability, finite state automata, evolutionary genetics, and coding theory. Professor Ulam's audiences reflected the broad span of his interests: mathematicians, physicists, engineers, and biologists, among others.

Reminiscing about his career in an interview with the *Two-Year College Mathematics Journal* during his Davis sojourn, Stan Ulam displayed a disarming modesty and personal charm. It was difficult to perceive in him the intimidatingly self-confident man described in his autobiography *Adventures of a Mathematician* [1]. The discussion touched on Ulam's origins in Poland, his work on the Manhattan Project during World War II, and the course of his subsequent research and interests. Drawing on his experiences in several scientific disciplines, Ulam spoke on the current status of mathematics and sciences and speculated on their possible development.

Barcellos: *Was your decision to become a mathematician a conscious one, or something that developed over a period of time?*

Ulam: At the age of ten I was interested in astronomy, then in physics, and finally in mathematics. By the time I was fifteen I was reading number theory; there was a fascinating book by Sierpinski—in Polish, of course. And then I read about set theory. At that time I thought that if it's possible at all, or practical, to become a mathematician, I would want to be one. Of course, from the practical point of view, it was very difficult to decide on studying mathematics—only mathematics—at the university because of the exigencies of a career: there were very few positions. To make a living in mathematics was very, very difficult.

So I entered an engineering school, the Polytechnic Institute, and ordered a so-called "general faculty" which actually contained a lot of mathematics courses. Then Professor Kuratowski, a very famous topologist, certainly influenced some of my early choices in topics in mathematics. I met other mathematicians more my contemporaries—although a few years older—like Mazur, and Banach. Banach was a professor at the University, but he gave courses at the Polytechnic Institute.

Very soon, just because—perhaps by luck—I managed to solve a few problems which were open, I became more sure of myself and decided to study—instead of electrical engineering—mathematics itself, come what may. I continued my work, continued writing my papers, and by the time I received my doctorate I had nine or ten papers published.

B. *What were the fields of study that interested you most at first and how have those changed over the years?*

U. Well, set theory—and topology. That slowly, of course, changed with the years —one should say decades, almost. But I have been interested in probability theory and always, so to say, platonically interested in theoretical physics.

B. *During your career the work you've done has been both in very abstract mathematics and in various applied fields. Do you yourself perceive a fundamental difference between pure and applied math?*

U. I really don't. I think it's a question of language, and perhaps habits. Even between pure mathematics and theoretical physics the thinking process bears many similarities. As I try to say in my seminar here in Davis: Mathematicians start with certain facts—which we call axioms—and deduce consequences, theorems. In physics, in a sense, it's the other way around: The physicists have a lot of facts, lots of relations, formal expressions, which are the results of experiments; and they search for a small number of simple laws—we could call them axioms in this case—from which these results can be deduced. So in some ways it's an inverse process, but the course of thinking about it and the intuitions have great resemblance in both cases. And the question of habits, so-called rigor, which mathematicians require is often absent in physics. If one is tolerant, however, you could say that what physicists do is quite rigorous, but with different primitive notions than the ones too naively pursued.

Now you actually didn't ask me about physics so much as about applied and pure mathematics. Even in applied mathematics the really good work is not merely a service type activity, but invention of new tools, new methods, new applications. For somebody like Gauss, you know, distinctions are really very hard to perceive; he was perhaps the greatest number theorist who ever lived, and then he did some marvelous applied work—the method of least squares, for one thing.

B. *Currently you're a professor at the University of Florida at Gainesville, professor emeritus of the University of Colorado at Boulder, and you're still a consultant for the Los Alamos Scientific Laboratory of the University of California. How do you divide your time these days?*

U. I'm afraid most of the time I'm just staying at home, because in Florida I spend three or four months at most. I don't stand humid heat very well, so come April I usually leave. Now Los Alamos is very near where I live in Santa Fe (New Mexico). In Colorado I still have an office. Lots of written material is there because there's no room in my house for tons of written material.

B. *What do you work on at the Los Alamos Scientific Laboratory?*

U. Mainly problems that are not concerned with weapons. I did some work on nuclear propulsion of space vehicles, and general mathematical studies—also, what you might call mathematical biology. It's a little presumptuous term, perhaps at the present time, but people are beginning to use mathematics even conceptually for biological schemata observation. Not many solvable equations appear in practical applications; there is very little done on that.

There's little odds and ends. I'm actually now trying to write a book on unsolved problems. It's a sequel to the book on this subject which I wrote twenty years ago. [2]

B. *To stay with the subject of Los Alamos, how did it feel when you came from a background in theoretical mathematics to join a large group of engineers and physicists working on the very practical problems of the Manhattan Project and the atomic bomb?*

U. It was fascinating. I must say it felt very good in the sense that it was interesting. It wasn't really—in the group in which I found myself—very "engineering" in the ordinary sense, because the thing was so new and so unknown in many aspects that it was almost like a purely theoretical discussion. The problems really had mathematical interest even though the crux was always the physics of it. That was one of the most interesting periods of my life, intellectually. There was a realization of the possible enormous changes which could be brought about through use of nuclear energy.

B. *How do you feel about the consequences of the Los Alamos work? Are you satisfied with what has happened?*

U. It's hard to say—satisfied from what point of view? It's hard to say, certainly, "satisfied" or "dissatisfied" with facts of nature. These things exist. I believe—some people say—that the advent of nuclear bombs prevented a third world war and will hopefully prevent such an unimaginable catastrophe—actually surpassing by orders of magnitude all the horrors of the past war. Some people say that, and perhaps it's true. Let's hope. Now other uses of nuclear energy are beneficial: use of radioisotopes and even use as energy sources. I myself believe strongly in the use of fission reactors for producing energy. It seems to me that there could be safeguards for disposing of the waste. After all, there are several hundred reactors running even now, mainly for energy of some sort; there hasn't been a single major accident. Some people say it's by far the safest way to produce energy—safer than coal, in terms of production of coal and the use of coal.

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After an unsatisfying postwar stint at the University of Southern California, Ulam was invited to return to Los Alamos. Russian acquisition of the atomic bomb spurred efforts at Los Alamos to perfect the "super," as the hydrogen bomb was called in its development stage. Ulam agreed that the matter was urgent and was an important part of the research work. He provided the key that finally made possible the ignition of a thermonuclear device.

Ulam lays no claim to the title "Father of the H-Bomb" which Edward Teller has willingly worn. Teller exerted himself strenuously on behalf of the bomb's development and was its self-appointed champion on all fronts. He was continuing the struggle to salvage his plans in the face of increasingly negative theoretical results when Ulam produced his vital contribution. As Ulam tells it, in his *Adventures of a Mathematician*:

> . . . Teller continued to be very active both politically and organizationally at the moment when things looked at their worst for his original "super" design, even with the modifications and improvements he and his collaborators had outlined in the intervening period.
>
> 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 . . . .
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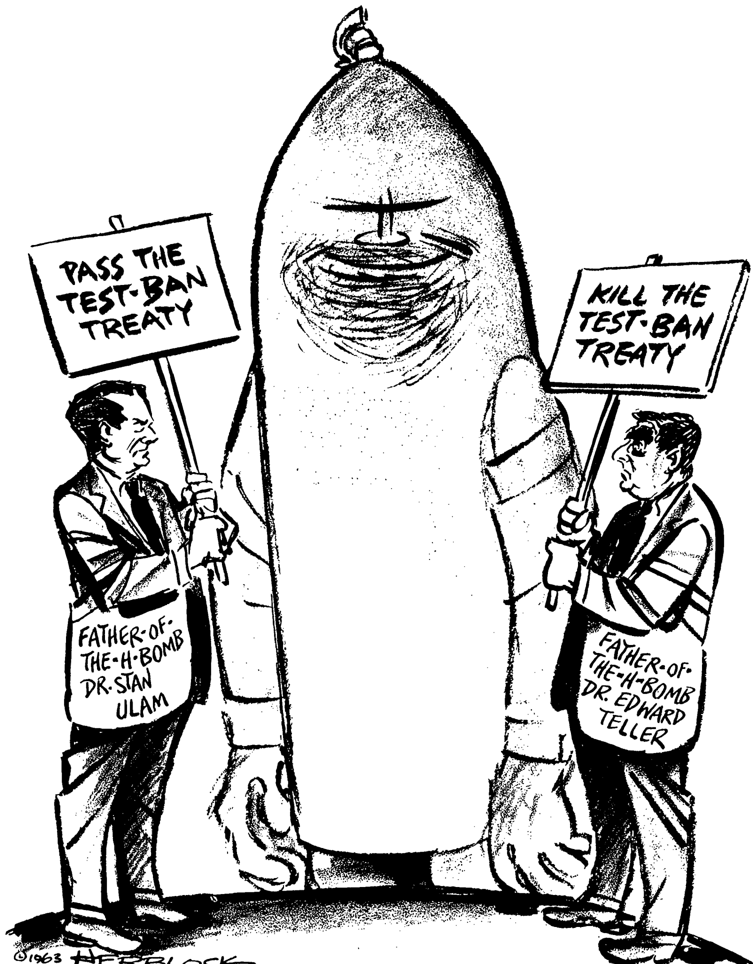
> The next morning I spoke to Teller. At once Edward took up my suggestions . . . I wrote a first sketch of the proposal. Teller made some changes and additions, and we wrote a joint report quickly . . . . The report became the fundamental basis for the design of the first successful thermonuclear reactions and the test in the Pacific called "Mike." [1, pp. 219–20]

[A]s a result of my work on the hydrogen bomb, I became drawn into a maze of involvements. [I]n some circles I became regarded as Teller's opponent, and I suspect I was consulted as sort of a counterweight. Some of these political activities included my stand on the Test Ban Treaty and testimony in Washington on that subject. The cartoonist Herblock drew in the *Washington Post* a picture of the respective positions of Teller and me in which I fortunately appeared as the "good guy." [1, p. 251]

It's A Wise Father That Knows His Own Bomb



—from Straight Herblock (Simon & Schuster, 1964)

However, the role he played in the establishment of the nation's nuclear policy appears to loom less large in his mind now than at the time he penned his autobiography:

B. *You figured very strongly in the debate on the management of nuclear resources.*

U. No, not really. I was once involved in some testimonies about testing in the atmosphere—the Test Ban Treaty. My friends' and my own opinion was that atmospheric testing was not necessary. And finally the U.S. Senate ratified by an overwhelming margin an agreement not to have nuclear explosions in the atmosphere.

B. *You were viewed at that time as something of a counterweight to Edward Teller.*

U. No, there were many people who argued for the test ban, and I think Teller was a minority.

B. *In the scientific community?*

U. In the scientific community acquainted with the appropriate technology. On the whole people thought that atmospheric testing was not necessary. I don't know how Teller feels about it now.

B. *You mentioned your belief in the safety of nuclear reactors. Do you think that the general public has any real understanding about these issues, that they're properly educated on them and have sufficient information to make reasoned judgments?*

U. I think that they do not have any information. Too much is the result of emotion. In this case, unfortunately, I think that it slows down the attempts of the United States to become independent in production of energy. I do not know why one does not build many more reactors.

B. *This leads me to the broader question of the current status of mathematics, science, and technology. So many people have become very anti-technology and feel that it's responsible for most of our problems. Do you see any likelihood of this trend reversing in the near future?*

U. I don't know. I'm not a prophet. Certainly what you say is true, but many of the phenomena that have been going on are due, it seems to me, to feelings of inadequacy—individuals who are baffled by the facts of science. I think some of this is one of the reasons for the unrest in the world—feelings of inadequacy. I don't know how to counteract this or how to proceed in education to make people feel better about the fact—which is now, I think, unavoidable—that one does need special technological and scientific frameworks to organize the world with its enormous population, and so many demands and ultimate shortage of the old type of fuels.

B. *This is a very popular question to ask mathematics professors: Do you have any opinions on the "disastrous failure" or the "qualified success" of the new math?*

U. Yes, I had some feelings about the new math right away. I thought that in principle, ideally, it was an interesting thing to attempt to instill or inculcate in children a sort of more abstract way of reason. Unfortunately, in practice that requires very special teachers. More than that: Many people—including, for example, myself—need examples, practical cases, and not purely formal abstractions and rules, even though mathematics consists of that. They need contact with intuition. Variety almost by itself confuses the student. I think a great problem is teaching mathematics as a question of grammar rather than the structure. Sometimes, especially with teachers who themselves are not too good at it, it was a negative change and discouraged, I think, many bright children from going into more mathematical things. [This formalism] was a big problem. That's how I felt in the beginning and I think that by now it uses much less of this.

B. *To what mathematical questions would you most like to know the answers?*

U. Well, I'll tell you. It's very strange; it's a question I ask myself—exactly what you ask me now. And it's very hard to give just one or two. But I certainly would like to know the outlines of a future basis of set theory—so to say, the sequel of the discovery of Gödel of undecidability. Then there are problems in number theory; some mathematicians, of course, mention Riemann's hypothesis, or Goldbach's conjecture. Are there "theorems" of this type which will be proved undecidable on the basis of present systems of number theory?

What interests me more now is not any special theorems, but rather whether the shape of mathematics will change: Will there be "large" theorems such that individual theorems will be left out as exercises, or corollaries? Well, I have to make it more precise.

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In his autobiography Ulam addresses at greater length the topic of the future "shape" of mathematics. Remarking on the overthrow of the set-theoretical assumptions on which modern mathematics was founded, Ulam strives to express a sense of the new and broader concepts of "true" and "false" which may be formulated to replace the deficient current notions:

> Gödel, the mathematical logician at the Institute for Advanced Studies in Princeton, found that any finite system of axioms or even countably infinite systems of axioms in mathematics, allows one to formulate meaningful statements within the system which are undecidable—that is to say, within the system one will not be able to prove or disprove the truth of these statements. Cohen opened the door to a whole class of new axioms of infinities. There is now a plethora of results showing that our intuition of infinity is not complete. They open up mysterious areas in our intuitions to different concepts of infinity. This will, in turn contribute indirectly to a change in the philosophy of foundations of mathematics, indicating that mathematics is not a finished object as was believed, based on fixed, uniquely given laws, but that it is genetically evolving. This point of view has not yet been accepted consciously, but it points a way to a different outlook. Mathematics really thrives on the infinite, and who can tell what will happen to our attitudes toward this notion during the next fifty years? Certainly, there will be something—if not axioms in the present sense of the word, at least new rules or agreements among mathematicians about the assumption of new postulates or rather let us call them formalized desiderata, expressing an absolute freedom of thought, freedom of construction, given an undecidable proposition, in preference to true or false assumption. Indeed some statements may be undecidably undecidable. This should have great philosophical interest. [1, pp. 283–4]

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U. I think that computers will bring about great changes in the aspect of both stating and proving theorems. Finally, the most interesting thing is the schema of the human brain itself. What kind of mathematics will our gradually acquired knowledge of the workings of the brain suggest? That's what I think is the most fascinating of all. Some Greek poet said, "There are many wonders, but the greatest of all is human thought." Was it Aeschylus?

B. *That reminds me of Johnny von Neumann—his classical learning.*

U. A most remarkable man. Since his death the application of his work and his influence is growing steadily. He was recognized very much during his life, but his very great fame started developing really, I think, after 1957 when he died. Too bad he didn't live to see the enormously increasing role of computers. He was an early prophet of this.

B. *Recently the four-color theorem was proved with computer assistance. How do you feel about this?*

U. In some cases it might be that this sort of thing will become more frequent. Certainly I believe in the heuristic or experimental value of computers where one by working examples will get intuitions about the more general fact. Ultimately the computers will be able to make formal proofs and operate symbolically the way we do now in thinking about mathematics. There's no question at all. Now there are computers playing fair games of chess. They have a sort of 2000 rating. [This ranking would correspond to a very good amateur.]

B. *Do you think it would be fair to call such computers increasingly intelligent? Or does the word have any meaning in this context?*

U. Well, I think that actual intelligence is very difficult to define even for people. Don't you agree? There's so many different types of what you might call intelligence in individuals. Some people have intelligence in certain directions and are very dumb in some other directions. Isn't that true? Usually if you call a person intelligent it's sort of faint praise. One wants more. "He's intelligent." That's not such a great compliment.

B. *Your mention of computers and heuristic reasoning, working out special cases and examples to give one a feeling for things, naturally brings to mind your Monte Carlo method.*

U. Yes, about the time when I left Los Alamos just when the war ended, I had the first thoughts about it. When I came back to Los Alamos I developed it some more and then, mainly in collaboration with von Neumann, I established several regions of application.

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One may choose a computation of a volume of a region of space defined by a number of equations or inequalities in spaces of a high number of dimensions. Instead of the classical method of approximating everything by a network of points or "cells," which would involve billions of individual elements, one may merely select a few thousand points at random and obtain by sampling an idea of the value one seeks. [1, p. 200]

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B. *What do you want to do now?*

U. What does anyone want to do? Enjoy a few more years of normal life. I want to write this book (of problems), finish it. I am still—*still*—thinking about problems. It's also interesting to see what's happening in foundations of physics, in particle physics, and the very strange phenomena in astronomy. Also in biology, tremendous things will happen, maybe more rapidly now than in any other science.

B. *I have one more fairly prosaic question.*

U. It's the answers which are prosaic. Oscar Wilde said, "No question is indiscreet; the answer might be indiscreet."

B. *I would like you to cite, if you can, the things you like best among what you've done. You've mentioned the Monte Carlo method and everyone seems to recognize that as important.*

U. I know, but intellectually it wasn't a great deal.

B. *What do you like most that you've done?*

U. You mean, sort of narcissism?

B. *Yes, something of that sort.*

U. It's hard. I don't compare things, but a few I thought were—by luck—not unimportant, not totally unimportant. I believe in the role of luck in scientific research. I like some works I did in collaboration with other people. I wrote many joint papers with Mazur, Schreier, Banach, Borsuk, Hyers, Everett, Oxtoby, etc. In general I somehow like to talk to people and work together.

B. *Is it that you have the "habit of luck" yourself or that you associate with people who do?*

U. That's a strange thing. Some people say, "Ah, it cannot be luck because why does it happen several times in a row?" and so on. I don't know; that's a good question. But, clearly, it's not a question of the power of the brain alone. The times must be right, and by chance you come upon something. Even somebody like Einstein, or, as people say, Newton. Who was it that said that Newton was so lucky because only once can you discover the fundamental laws of the universe? Actually there are infinitely many fundamental laws, perhaps. Certainly luck plays a role, even at the highest level, not to mention the level of a working mathematician like myself.

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