

SKIING THE SUN
New Mexico Essays

by

Michael Waterman

*...he will eat dried buffalo meat and frijoles with chili,
and he will be glad to drink water when he can get it.*

From the novel *Death Comes for the Archbishop*,
written by Willa Cather in 1926-1927.

In recent years I have written several essays about New Mexico, only one of which has appeared in print. The truth is that my motivation has not been to tell others these stories and thoughts, but instead to articulate them to myself. That is harder than it looks, at least it is when I do it! After the memorial article about Gian-Carlo Rota appeared in the *Notices of the AMS* (here it is titled “Silhouettes on the Mesa”), several people expressed an interest in Rota, Ulam and Metropolis, so I decided to post this material.

As this appears on my scientific web page, I have only included the essays that relate to science and mathematics, or more accurately, to scientists and mathematicians. The only formulas in the manuscript appear in “Silhouettes” and should be easy to skip over.

The title comes from a 1970s slogan advertising New Mexico, “Ski the Sun,” that referred to the fact that the New Mexico weather was so good that people didn’t need to ski in bad weather. Coming from Idaho where there wasn’t much sunshine in the winter, I was struck by the reluctance of people to venture outdoors in anything but ideal conditions. Then again there is an obvious connection to be made with that incredible ball of fire when the bomb exploded at White Sands on July 16, 1945. Throughout the twentieth century, a complex of nature and science and circumstance brought creative people to New Mexico, all to ski the sun.

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Chapter 1

The Lost Art of Conversation

Gian-Carlo Rota
1932–1999

It was the summer of 1970, and I was in a covered plywood tunnel joining some offices to the computer building at Los Alamos Scientific Laboratories. Complete with security clearance I was inside the fence on a summer grant to approximate the invariant measure associated with n -dimensional continued fractions. Although in the 1960s Stan Ulam and Paul Stein had looked at function iterates on a screen, using a computer to experiment with measures was not yet common. Los Alamos seemed a good place to do the work.

I met Nick Metropolis along this walkway, and he stopped me for an introduction to an elegant man, Gian-Carlo Rota. Nick went on but Gian-Carlo stayed and asked me a question. He spoke English excellently with a rich Italian accent which might have been flavored by the years he spent as a teenager in Ecuador.

“So, who was your advisor and what did you do for your thesis?”

I had heard of this guy and had seen his name at the mast-head of various fancy mathematics journals. I almost asked him how he knew I was a fresh Ph.D. but fortunately thought better of it. Instead I gave him a terse answer: John Kinney was my advisor and I had done such-and-such in my thesis. Gian-Carlo gave me an intense look.

“No, no, no,” he said with some impatience. “I mean: *what did you do for your thesis?*”

I elaborated, and he asked a number of insightful penetrating questions. When he let me go in ten or fifteen minutes, I walked on to get my print-out fairly certain that he now knew more than I did about what I had just published in the journal *Z. Wahrscheinlichkeitstheorie*.

It must have passed muster because after this I actually got to know Gian-Carlo Rota. As you are aware, he was very supportive of other mathematicians, and in the 29 years I knew him, he spent an amazing amount of time listening to people put what they knew (as well as what they thought they knew) onto blackboards while he would inscribe the material into one of his heavy notebooks. He would switch between pairs of glasses during these sessions, and in recent years there were sometimes as many as three pairs hanging from

cords around his neck. Gian-Carlo was as absorptive as a dry sponge, but a good deal more discriminating.

“Ummmhumm,” we all have heard him say, “and just what *is* the connection with covariant functors?” Of course the person may never in their life have considered *that* connection. It all went into Rota’s notebooks which must be a vast treasure-house of digested mathematics.

The translator of us all is gone, died the weekend of April 17, 1999. I spoke with him about two weeks before his death. He had written one of those compact confident flamboyant letters of recommendation for one of our job candidates, and I wanted to say thanks. We compared current teaching loads, displays we loved to make showing that we were each doing our jobs. Freshmen scored higher than graduate students in our contests. He did tell me that out of all of mathematics the thing he most wanted to understand was the maximum entropy principle.

“No-one really knows why it works,” he said.

Gian-Carlo published our first computational biology paper although it was not called computational biology then. It was not called anything at all. We were so far out of the mainstream that a paper with “biological sequence metrics” in the title elicited the following referee report from a Berkeley mathematician: *There is a huge literature on metric spaces. The authors should consult it.* (Of the reviews my papers have received, this one is now my favorite.)

We told Gian-Carlo of our rejection, and showed him the paper and its reviews. Instead of another put-down, Gian-Carlo placed the paper in *Advances in Mathematics*. It still is being quoted, and Bill Beyer, Temple Smith and I have him to thank for its seeing the light of day as soon as it did.

Gian-Carlo Rota ran his journals as a total autocracy. He and only he decided just what papers would be published and exactly when they would appear. When I was recounting my difficulties as editor of a journal, he became impatient with me.

“I have no idea what you are talking about,” Gian-Carlo said. “If you have to have a paper refereed to find out if it should be published, you shouldn’t publish it.”

He had a point. I wonder if an important journal ever again shall be run as he did his journals?

Non-standard analysis was the rage in the early 1970s, and elaborate treatments seemed to appear right and left. It looked to me to be what I would learn to call abstract nonsense. So when one summer Rota announced a series of lectures at LASL on the topic, I decided to attend the first one only. The audience was physicists, chemists, computer scientists, and mathematicians; and the approach was nothing like what I had seen in the journals. He connected infinitesimals with rates of convergence to zero which had their “mates” which were rates of divergence to infinity. His treatment was crystal clear to anyone who knew any real analysis. He gave short crisp lucid proofs of results that took pages in the literature. Sometimes he had the class supply a proof on the spot. “Now you know what Leibnitz knew,” he’d say with pleasure. At the end of the series which I attended faithfully, the class was proving theorems which had not yet appeared anywhere. Gian-Carlo Rota was simply

the most gifted teacher I have ever seen.

Gian-Carlo spent much time nurturing his students and others he thought deserved help. He published their papers, suggested them for jobs, nominated them to societies. For example he put me up for Fellow of the Institute of Mathematical Statistics a decade before that establishment noticed my work, and he has done this for many people.

When I met Gian-Carlo I had a job at Idaho State University which he thought to be purgatory although I loved it there. He was highly relieved when I came to LASL in 1975, and occasionally he would reveal his fear that I'd chuck it and head back to those mountains. The outdoors was a domain like Asian food which he totally dismissed. But he could sense the dangerous influence of Idaho and wished to guard me against it. How many of us did he watch over?

One afternoon I was in a car on the mesa Los Alamos sits on. My fellow passengers were Nick Metropolis, Stan Ulam and Gian-Carlo Rota. As I age my dreams are becoming more vivid and that memory is like a dream I had last night. Brilliant sunlight, the origin and destination of our trip completely missing, my total happiness at just being in that car, my profound sense of incredible good luck.

Gian-Carlo visited Los Angeles often after I joined the University of Southern California in 1982. His friend Mark Kac was at the university, and then USC was flush enough to sponsor regular visits. Once I and Curtis Green managed to drag him to a Chinese restaurant where Gian-Carlo made one of his dramatic absurd pronouncements. Instantly Curtis and I broke into laughter. I thought about this later, both of us knowing he was funny and laughing at him. I asked Gian-Carlo about it, saying that some people took such statements seriously and were mightily offended. His answer was an equivalent to the one Jesus makes about those wearing sackcloth and ashes: they have their own reward, leave them to it. Of course there was more going on than that. Gian-Carlo Rota was always poking us with sharp sticks, hoping to get our attention, doing his best to get us to look at something from a new angle. Intellectual fakes and frauds be damned.

As I learned more molecular biology, I put in my turns at the blackboard trying to give an overview of that exciting subject. Of course Gian-Carlo delighted in the combinatorial power of genes and proteins. "We must write a paper together explaining biology to mathematicians," he told me several times. In the early 1980s I was far too busy, and when I finally thought I could find the time, he told me, "No, it is impossible now. You know too much!"

He enjoyed eating good food, and drinking good wine. When asked how he found the nerve to send very expensive wine back, he answered, "If it tastes like ink, why shouldn't I send it back?" As a rural schoolchild I remember getting ink in my mouth, the bitter flat persistent taste. Yes, send the blasted stuff back!

He loved European food and had an understandable fondness for South American cuisine. Asian was out, one of his biggest weaknesses in my estimation. But on Italian or French he was 100% accurate. Often he went for expensive and ornate restaurants; just look at The Palace in Santa Fe, Loche Ober in Boston, Madeo and Valentino in Los Angeles. At Madeo you descend a half-floor underground into plush old-fashioned red luxury where there is a

rolling vat of garlic-and-rosemary baked veal-and-potatoes and plenty of grappa. Often there were relaxed film stars at the other tables, but they did not interest Gian-Carlo. On arrival the head waiter would recognize him, fuss over the table location, and go to fetch the first bottle of champagne. Then the manager would arrive for a long conversation in Italian.

“He’s been here 15 years, and he hasn’t yet learned English,” Gian-Carlo would confide in us as we began with the wine.

“The Mafia owns this restaurant,” he would say, looking around with satisfaction. “They don’t advertise and they don’t have a sign out front.”

Those heavy-duty locations in cities around the globe were rooms in his home, and he entertained many of us in them. What a privilege that was.

Once I was a member of a large dinner in Santa Fe that included the famous Richard Hamming who was visiting the Lab. At the close of dinner Hamming made a show of looking for the bill which he was going to pay. But the waiters ignored Hamming, and he fussed at Rota who said with a slight gesture of his hand,

“It is taken care of. You are my guest.” This went on for a time.

“You must let me pay,” Richard Hamming finally burst out. “I make more money than you do!”

“Yes,” Gian-Carlo answered sympathetically and reassuringly. “Yes, I am sure that you do.” And that was the end of it.

In Los Angeles Gian-Carlo found many restaurants to love. The new California cuisine was just right with him, so long as it didn’t get too Asian. He delighted in the fresh inventive upscale restaurants: Michaels, Citrus, Silvo, Patina.... When three French chefs started Fennel on the bluff in Santa Monica he took to it. Each of the cooks was to spend a couple of weeks in Los Angeles. I do think the idea of toque-topped French chefs rotating through LAX baggage claim has its certain charm. I had dinner at Fennel with my beautiful friend Sheaufang Chang, the brilliant and sardonic David Berlinski, and Gian-Carlo. We all wore our best clothes, and it was a dazzling evening. At times like this Gian-Carlo would sometimes pronounce, “The art of conversation is lost.” And I would sit there feeling fortunate to be somewhere people still carried on the art of conversation.

Part of his time in Los Angeles was spent working on a projected book of character studies. His essay “The Lost Cafe” was to be the first. There he gave an analysis of Stan Ulam which had never occurred to me. I had observed certain things such as Stan’s short attention span, but had thought it was simply that Stan was so much smarter than the rest of us that he was soon bored. And smarter he was! I still wonder about Gian-Carlo’s analysis. He spent a good deal of time with Herb Taylor, an odd eccentric character who hung out at USC around the engineering school, and he worked hard on Taylor’s chapter. I’d love to see it but never had an opportunity. Gian-Carlo asked me if I would allow him to do a chapter on me, and I refused saying there were many things about myself that I was probably better off not knowing. In the recently published *Indiscrete Thoughts* we can see some shadows of his book of characters.

Gian-Carlo was one of the worst drivers I have ever known. I tried to drive whenever we went anywhere, but sometimes couldn’t escape being his passenger. Riding with him in Los

Angeles was like being in a tunnel with the nearby drivers swerving and honking and waving their arms and fists at him. He had only a vague sense that he was not with it, and would occasionally wonder at the fuss going on around him. Gian-Carlo did in particular hate the necessity in LA of making left turns across traffic.

“I’m writing a book,” he would announce. “The title will be *Los Angeles on Three Left Turns a Day*. It will be a best seller!” The detached retina he had had in his left eye may have made left turns especially difficult; he never mentioned it.

Once Schützenberger was visiting from Paris, and I learned Gian-Carlo was picking him up at a hotel to bring him to the university. They were late, and I spent a tense hour fearing that there had been an accident. Two of the most famous men in combinatorics in a car with Rota driving! Somehow Gian-Carlo managed to do a lot of LA driving without any accidents or tickets that I learned of. In this connection I found out one of his secrets. He had a second driver’s license in New Mexico in the name “Juan Carlos.” He made me swear not to reveal this fact to anyone, but I think this is the right time to tell the story. He can’t lose his security clearance now! (After I made this public, I learned of the possibility of a third license!)

So now we are here without him. I am not ready for this. Bill Beyer (or rather his daughter Elizabeth) has suggested a pleasant romantic image of Uncle Stan and Gian-Co meeting at the Lost Cafe. Bill has them going at Cantor’s hypothesis and visiting Stan’s inaccessible cardinals, but I know for certain Gian-Co hasn’t yet given up on the mysterious maximum entropy principle. Still I am unprepared for life on earth without my friend. Here I sit in a noisy cafe where I can not make out anything that is said. And I can tell you that being here *tastes just like ink*.

Los Angeles, California

May 1, 1999

Chapter 2

Silhouettes on the Mesa

“I have never known Stan Ulam to last longer than ten minutes of anyone else’s lecture,” Gian-Carlo Rota wrote, mimicking the famous and irreverent first sentence of Chapter 1 of James Watson’s *The Double Helix*. Nevertheless Rota tells of meeting Ulam in New York City in 1964 when Mark Kac prevailed on Ulam to attend a lecture of Rota’s; Ulam made it twenty minutes before bolting, and one need not be an expert on extreme value distributions to know that was a rare event. Kac and Ulam were great mathematicians born in Poland who each came to the U.S. at the beginning of World War II. They both had broad European educations and did not observe boundaries between mathematics and other sciences, let alone between mathematical subfields. It is natural that they each took up with Rota with his multiple languages and wide-ranging intellect.

Soon after New York, Rota was invited to Los Alamos National Laboratory, known as the Lab, the Hill, the Mesa, and most famously as Santa Fe Box 1663 during the war when brilliant men of science, physics especially, worked feverishly to create the atomic bomb. By 1964 Stan Ulam was one of those who retained a regular association with Los Alamos. The Lab at 7400 feet is on a mesa top in ponderosa pines just above the pinion-juniper zone. The crisp clear air has a distinctive incense of cedars, pine, ozone, and sun-baked tuff, and one can see for tens-of-miles. In Santa Fe thirty-five miles distant is Native American and Caucasian culture with good restaurants and art galleries. This exotic high-altitude sun-bleached locale captivated Rota, but surely it was Ulam who kept him coming back. One can find each of them writing about the other in several places such as Ulam’s *Adventures of a Mathematician* and Rota’s *Indiscrete Thoughts*.

Rota soon became part of Los Alamos. He gave lectures that were deeply informative polished works of art that made him known throughout the Lab. The topics were wide-ranging: differential equations, ergodic theory, nonstandard analysis, probability, and of course, combinatorics. I attended the series on nonstandard analysis, and it was the equivalent of a course with an approach that had not yet appeared in print. These notes exist as a Los Alamos report. Over the years Rota helped organize several conferences: History of Computing in the Twentieth Century (1979), Science and the Information Onslaught (1981), and Frontiers of Combinatorics (1998). He was made a consultant of the Lab in 1966 and

a Director's Office Fellow in 1971. When asked what he did, he said "I wish I knew. I manage to snoop around and every once in a while I pop into the Director's office and have a chat with him." (Rota loved and absorbed gossip about mathematicians and scientists!) Director's Fellows could come whenever they chose and could stay as long as they wished. For Rota this meant at least a week in January (Rota hated Boston winters even more than New Mexico winters!) and a month in the summer. As a Fellow he quickly became involved with high-level Lab politics. In the late 1970s he was at a dinner party in my home when a new Director was being chosen. He received so many lengthy telephone calls that I feared he would not get enough to eat.

Other than Ulam his closest collaboration at Los Alamos was with Nick Metropolis, an elegant man who had a long association with the Lab. Metropolis was educated as a physicist at the University of Chicago where he took many mathematics courses. He had a distinguished career as a physicist and pioneer in the development of modern computers; he passed away on October 17, 1999. In wartime Los Alamos he and Feynman repaired Marchant manual calculators to the disapproval of Hans Bethe. In 1945 at von Neuman's invitation Metropolis began to work with the ENIAC, and in 1947 he started a computer research group at Los Alamos that produced the remarkable series of MANIAC computers. At Los Alamos I used the MANIAC II which was a joy. The MANIAC III, based on significance arithmetic, was built at the University of Chicago. For the last twenty years of his career Metropolis worked in mathematics, much of it with Rota. One of their major contributions was in using concepts created for computers such as binary representation of numbers and "carry" operations and applying them to the foundations of real numbers. They brought forward a new idea, distinct from the usual Peano and Dedekind construction. There are four papers on those topics. They also studied the lattice of the faces of the n -cube, and they give an explicit decomposition of the lattice into a minimal number of chains of lattice faces. And they had the good fortune to discover a fact missed by all the early workers in symmetric functions: that every function in three variables is uniquely expressible as a sum of a symmetric function, a skew symmetric function, and a cycle symmetric function [1]. The underlying idea was extended to n variables in several papers, including an introduction of two new classes of symmetric functions.

Innumerable people gave Rota private lectures, which he carefully inscribed in one of his heavy notebooks. "It's my job," he would say with pride. It was much more than a passive activity, and here is an example of one of those exchanges. Jim Louck, a Los Alamos physicist, listened to Rota lecture in the late 1960s on the set $M_{m,n}(\alpha, \beta)$ of $m \times n$ matrices with nonnegative integer entries having vector row sum $\alpha = (\alpha_1, \alpha_2, \dots, \alpha_m)$ and vector column sum $\beta = (\beta_1, \beta_2, \dots, \beta_n)$ with $\sum_i \alpha_i = \sum_j \beta_j = N$. During the lecture, Rota remarked that he knew of no physical applications of the set $M_{m,n}(\alpha, \beta)$. During this same period, physicists were very active in developing explicit unitary irreducible representations of the general unitary group for physical applications, and one of the popular physical models for this theory was a collection of independent harmonic oscillators as realized through the Heisenberg algebra of creation and annihilation operators. Many physical problems can be modeled in this way because of the generality of the property that quantum states can be

created from the ground state by the action of the creation operators, the ground state itself being defined by its annihilation by the action of the annihilation operators. The simplest of such models is a system of mn identical 1-dimensional harmonic oscillators, which may also be viewed as n oscillators, each of which is an m -dimensional isotropic oscillator in Euclidean m -space. If the total energy of such a system is N energy quanta, and the number of these quanta associated with the motion of all n oscillators in the i -th direction is the nonnegative integer α_i , while the number of quanta associated with the j -th component oscillator of each of the m -dimensional oscillators is the nonnegative integer β_j , then α and β are respectively, the row and column sums of the $m \times n$ matrix $(a_{i,j})_{1 \leq i \leq m, 1 \leq j \leq n}$. Here $a_{i,j}$ is the number of energy quanta associated with oscillator (i, j) in the set of mn 1-dimensional oscillators. In this way, the set $M_{m,n}(\alpha, \beta)$ enters almost universally into the physical theory of quantum systems. It was this observation, which emerged after Rota's lecture, that led to thirty years of interactions among Louck, L.C. Biedenharn, and Rota. Louck and Biedenharn gave many informal presentations on the tensor operator theory they had created. "Rota never really bought it," Louck told me, and he and Biedenharn wrote no joint papers with Rota. But when Rota's student W.Y.C. Chen came to the Lab, Rota said, "Go to Los Alamos and look up Jim Louck. He's a gold mine for mathematicians." Chen was delighted to find this to be true, and he and Louck went on to mine that rich ore in an on-going series of papers.

Biology is another area that Rota helped along although he did not entirely buy into biology either. (I refer especially to his doubts about Darwin's theory of evolution.) When my first paper on sequence matching was rejected, Rota placed it in *Advances in Mathematics*. It is still being quoted, and I (along with Bill Beyer and Temple Smith) have Rota to thank for the timely appearance of that paper. "There are so few people working on those problems," he said many years later. A few years ago David Torney began to give Rota lectures about his work that arose in classification of DNA sequences. The result was an elegant joint paper on probability set functions and help in organizing a conference.

It is of course impossible to list all Rota's interactions. Some of the most unexpected (to me at least) are those relating to Rota's interest in philosophy. David Sharp is a multi-talented mathematical scientist who shared Rota's passion for philosophy. Their dialogue "Mathematics, Philosophy, and Artificial Intelligence" in *Los Alamos Science, No. 12* is fascinating. Rota had a tremendous impact on students who took his philosophy classes. Mark Ettinger and David McComas are two of those MIT physics students who went to the Lab because of Rota. McComas went on to become Director of the Center for Space Science and Exploration.

Rota served on the Advisory Board for Non-Proliferation and International Security (there is a Lab Division of that name), but it is next to impossible to learn any details. While he did write short classified reports on national security issues, they are not available to "unclassified eyes." At Los Alamos this activity, just as with almost everything else there, has gone under various names, but it is often called "the Spook Shop." It will be many years before much more is known. For example I am curious about whether Rota's relationship with the Spook Shop or the National Security Agency came first.

Let me return to Rota's vital connection with Ulam. The fascinating essay "The Lost

Café,” the final version of which appeared as Chapter VI of *Indiscrete Thoughts*, is a sketch of Ulam’s life with details of his health, work habits, mathematical abilities, and state of mind, and some of it was far from complimentary. “The Lost Café” was very controversial at the Lab, with the Ulam family, and elsewhere. “It’s a scandal,” Rota told me with evident satisfaction. The editor Palombi writes, “... one does not say this kind of thing about great men.” I can almost hear Rota use those exact words! I believe “The Lost Café” is filled with respect and love, but it is radical. Among other things Rota writes that Ulam was lazy. I like a remark Carson Mark made at a reception at Los Alamos, “Ulam was thinking all the time,” and I doubt that Rota would have disagreed. I believe Ulam’s widow remains bitter about the article and has not forgiven Rota, not even after his death. And at Los Alamos and elsewhere there are resentments, grudges, and judgments; although Rota would say “We should tell it like it is,” I have not space to list them here.

In winter, snowstorms come to northern New Mexico, and the following day dawns clear with deep-blue sky and subzero temperatures. Every snow crystal reflects light, and the vast landscape is dazzling. Rota planned a Los Alamos article entitled “The Desert is Covered with Snow.” It too would have dazzled, and just as likely, shocked and upset some. We can never know all that we have lost, what Gian-Carlo Rota would have revealed to us about mathematics and about ourselves.

Acknowledgment. I am grateful to those who assisted me in putting this segment together, especially Bill Beyer, Jim Louck, and Nikki Cooper, who all provided essential material.

This essay with very minor differences appears as part of a longer memorial article for Gian-Carlo: Beschler, E.F., Buchsbaum, D.A., Schwartz, J.T., Stanley, R.P., Taylor, B.D., and Waterman, M.S. (2000) Gian-Carlo Rota (1932–1999) *Notices of the AMS*, February 2000, **47** 203-216.

1. N. Metropolis, G.-C. Rota, and J.A. Stein, Symmetry classes: functions of three variables. *Amer. Math. Monthly* **98**, (1991) 328-332.

Chapter 3

Summer Fishing

Before I moved to Los Alamos, New Mexico in 1975, I spent three summers there, an Idaho refugee working at Los Alamos National Laboratory. And I had been up in the Jemez, which is a huge volcanic land-island in northern New Mexico. What Laboratory employee didn't take that twisting drive up precipitous Route 4 and gaze into the Valle Grande? The Valle is one of the world's largest caldera, 30 to 35 miles in diameter, and it is now a magnificent cattle ranch. Of course I had done a little more than just take a car ride, and in 1970 carrying a Camp Trials backpack, had circled the backcountry of Bandelier National Monument before a big fire in the 1980s ravaged the large pines at the upper end of the Monument and before Cochiti Dam backed the muddy Rio Grande up the canyon to make a lake. So I could claim some direct experience with the country, maybe even a little more than some of those who had spent years working behind the security fence at the Laboratory.

During my first summer in New Mexico in 1970, Bob Schrandt, a programmer in T-Division, found that I fished for trout, and he took me up past the Valle, then down the Jemez River which brushes by the road to then swing away in a several-mile-long loop before coming back under the highway. Bob and I would park at the highway crossing and fish upstream in the evenings, the severe brightness of the day softening, small brown trout beginning to rise making delicate dimples on the water. Bob was a fly fisherman of the Eastern school, using small carefully cast flies. I had been catching big trout in large rough crashing Idaho rivers with spinning equipment and considered those New Mexico trips pleasant social outings, although I did pick up a little fly-fishing knowledge in spite of myself. And we caught a few small fish which fitted with the water and the flies, although I kept my own council about that. That was almost all the fishing we had in New Mexico, so why complain about it?

Bob told me a nice story about fishing in the Jemez during the war. One of the great European physicists, Fermi perhaps, asked why people went fishing as the eating of trout could not be enough reason to spend all the time it apparently took to catch them. He received a long explanation about imitation of insects by tying feathers on hooks and of the difficulty of presenting the flies to the trout in a way that convinced them to take. After the fishing enthusiast finally ran down there was a pause. Then the great man said, "I think I

see what is going on. You are trying to determine who is smarter, you or the fish.” Bob and I both knew in our cases the fish were almost always smarter.

Once we went up the North Fork of the Jemez, and it was much the same with a little less water. And we took most of a day and went on a longer trip into the Jemez to Fenton Lake, where we rented a boat and rowed out into heavy weeds and caught trout that came up in the channels. I am not inclined to fishing from boats and we didn't repeat that adventure, although because the fish were a little bigger there we would sometimes say, “Well, we *could* go back out to Fenton....”

This did not quite exhaust all the available trout fishing; there were two little lakes, usually muddy, on the Santa Clara Indian Reservation toward Española where we could pay a fee to cast our lines. Those lakes were definitely not worth taking any time off from work.

Twice we drove past Española up the Rio Grande to the Taos Box Canyon. It was beautiful country, raw and rugged. We drove to the Box on a bumpy rock-and-dirt road and parked at the bridge. A thunderstorm caught us on the road once, the sluice of huge drops overwhelming the wipers, and we were forced to stop in the road, the road bank beside us eroding away with mud and rocks falling onto the road. The sagebrush-ozone smell of late summer New Mexico air after a crashing storm. The water was never quite clear enough to catch trout, as thunderstorms were usually raging somewhere up river and the violent rains caused the water to color. The Rio had a nice size compared to the Jemez River, and I enjoyed roll casting across the river to the water running against the volcanic canyon wall.

The most pleasant aspect of these fishing trips was Bob Schrandt himself, a truly gentle person who was in awe of the great men he was working with, of Carson Mark, Stan Ulam, Paul Stein and Nick Metropolis. He puffed his pipe as he drove through the wonderful evening air and talked of computers and trout. I believe he was a deeply contented man, much as my grandfathers had been. I very much liked his company.

Chapter 4

The Day I Met Temple Smith

Written on the Occasion of His Sixtieth Birthday

I was an innocent mathematician until the summer of 1974. It was then that I met Temple Ferris Smith and for two months was cooped up with him in an office at Los Alamos National Laboratories. That experience transformed my research, my life, and perhaps my sanity.

My first job after receiving a Ph.D. was at Idaho State University. The first summer I spent at Los Alamos and then I had two summers with NSF grants, all to work on iteration of functions. But by 1974 I had burned out my mathematics funding and was invited by Bill Beyer at Los Alamos to join an NSF-funded Summer Project to study molecular biology and evolution. Stan Ulam and Bill had begun this work already and someone named Temple Smith was to join us. Temple had already worked with these people, and although he had a Ph.D. in physics, he was considered by Bill and Stan to be an expert on biology.

At the Labs biology was intellectually isolated from the “real thing” which was pretty much physics and explosions. To emphasize this distancing, the life sciences were (and remain to this day) located in a building across a deep canyon from the Laboratory complex. I recall walking on the bridge high over the dramatic cut through the mesa to attend the first biology lecture of my life. Mind you I don’t follow much of a serious lecture in mathematics, but this was of another order. I just didn’t get it. Then the lecture ended, the audience drifted away, and Bill Beyer introduced me to an incredibly intense character, Temple Smith. Even then he had a moustache, and we both had more hair on our heads than we do today.

Temple was from a small university in Marquette Michigan which was as isolated and anti-intellectual as my own in Pocatello Idaho. He was not happy there and had come to New Mexico to do some earth-shaking research that would spring him from the Upper Peninsula of Michigan. So he had a lot at stake. I didn’t know these things at the time, but anyone could see that he was so tense that sparks seemed to be coming off him.

After Bill introduced us he said that we should get organized. At this point Temple took over. He was as short as Bill was tall, but suddenly he seemed to be the only one doing

anything. He pulled a little blackboard from behind a dusty curtain and started lecturing us about biology: what it was, what was important, what was going on. It made quite a tangle. Somewhere in there by implication was what we should work on, but if truth be told he didn't know what that was either. He simply was exerting his substantial will in order to make something happen. I was totally confused. Temple spoke loudly and rapidly and so far as I was concerned he could have been speaking in the language of one of the pueblos from the Rio Grande Valley below us. Amino acids, PAMs, Jukes, hydrophylic, nucleoside, beta pleated sheets. What were these things? Where was the mathematics?

Temple went on and on, working off a winter's claustrophobia, dispelling Marquette and its dreary students. Bill was a patient man, older than we were and one of those kids who was interested in science in high school, in today's language a nerd if ever one did exist. Occasionally he would try to make sense of something, but there was no stopping Temple. The contrast between Bill's high squeaky voice and Temple's booming barking roar was quite remarkable. One of Bill's interruptions would just deflect Temple in a different direction. I sat there in an awkward uncomfortable chair wondering what the Hell I had gotten into, vowing if I ever got back to the mountains and trout streams of Idaho I'd never leave again. I wish I remembered how long Temple went on—it seemed forever.

Later we settled for the summer into that little window-less office behind the security fence, and I learned enough to help formulate a few problems. "The right problem" is still the key to our business, and we did make sense of some things. Two papers came from that summer: one on sequence alignment and one on molecular evolution. Unlike today with more rapid pathways to publication, they did not appear until 1976 and 1978. Both are still being quoted in 1999.

Bill Beyer tried to bring more biology into the project and he choose an Indian geneticist by the name of Singh. This man didn't have a security clearance, and we only met with him once or twice a week. He had the narrow mind of the classically educated British geneticist and never knew what we were trying to do. But Bill agonized over his exclusion, and repeatedly sighed, "Poor Singh. Poor poor Singh." We listed Singh as co-author of one of the papers, but it was for Bill as much as for poor Singh. Bill wore a salt-and-pepper crew cut in a style that had been out of fashion for 15 years and that was not to come back into fashion for another 15 years. Bill never blinked in matters of style and was there waiting when crew cuts were again cool. In my opinion the National Science Foundation got their money's worth with that grant; they should draft a memorandum of thanks to Bill Beyer for pulling it all together.

I don't think Temple had on his western wear that first day, but I want to end this by saying something about those clothes. He wears boots and a big hat and gripper-snapper shirts, and imagines himself to be dressed like a cowboy. People from Patterson New Jersey and Toulouse France think so too. For those of you not from the rural American West, I can assure you that he just doesn't pass. Once while I was sitting in a Santa Fe hotel lobby with a woman from West Texas, Temple came through in that get-up. "And what the Hell was that supposed to be?" she asked me, quite startled. But Temple is more of a Westerner than I'm making it sound. If you sleep enough nights out under big Western skies, carry

packs over enough miles of trail, scale the North Face of enough mountains, you do become a Westerner. It is just that with the West as with Biology, Temple Ferris Smith has taken a traditional activity and then has come up with his own unique creation. We wouldn't have gotten here without him!

Los Angeles, California
February 1999

Chapter 5

Nick the Greek

Nicholas Metropolis
1915–1999

I was invited to Los Alamos Scientific Laboratories in the fall of 1969 to present a lecture on my work. I had no real interest in pursuing a research career, so I thought then, and went there to give my first presentation outside of the university where I received my Ph.D. simply because my advisor John Kinney had a connection at Los Alamos. Bill Beyer planned to bring me to the Lab the next summer, and there I was, behind the security fence with a visitor's badge dangling from my shirt collar, giving my account of a discovery I had made in order to receive a doctorate. It was really an analogy that had its roots in two papers, one by C.J. Everett at Los Alamos and the other by Alfred Renyi, the renowned Hungarian mathematician.

At the close of my lecture there were a few questions, one from a short striking man. Even then Nick had a shock of white hair, and accompanying his slightly rumped checkered sports coat, there was a chunk of turquoise instead of the tie some might have expected. He had already lived in New Mexico for nearly 25 years. As soon as I could I asked Bill Beyer, "Was that C.J. Everett?"

"Oh no, C.J. never comes to public lectures!" Bill answered in surprise that I did not know such a well-known and obvious fact. "That was Nick Metropolis." (It turned out that C.J. Everett was a recluse at the Lab, and he avoided most human contacts.) Then Bill introduced me to Nick, and I fell permanently under the spell of that elegant man. Nick Metropolis had style, from that lump of turquoise at his throat to his turn of phrase and quick wit, and he was one of those humans who are entirely at ease. I was never as close to him as I became with Gian-Carlo Rota, or even with Stan Ulam, but I felt his warmth and approval, and knew first-hand of his lively interest in a wide variety of scientific topics.

Many years later, after I had been a visitor in the summers of the first half of the 1970s and then a full-time employee in the Statistics Group from 1975 until 1982, I came to Los Alamos as a member of a committee to evaluate a computational biology group there. We

were more than once smugly told, “We are part of T-Division, the first Division at the Lab, and we are very proud of that.” It struck me that I had been supported by people like George Bell and Nick Metropolis when I was far from the Theoretical Division, and the notion that Nick would care where I was sitting in the Laboratory rather than about the quality and importance of my work was absurd. That Lab was elitist is certain, but for the old-guard that elitism was based on scientific contributions, not organizational charts.

Nick’s career contained marvelous triumphs: the war-time efforts at Los Alamos, the creation of some of the first digital computers including the MANIAC series (in 1970 the MANIAC II still had vacuum tubes), the paper with Paul Stein that first discovered the period-doubling that Mitch Feigenbaum went on to make famous, and the astonishingly wide-spread Metropolis algorithm that is so useful for really hard optimization problems. Nick seamlessly combined his physics insights with mathematical elegance to make his great contributions.

Newcomers to New Mexico always glory in the clear air. Once Nick told me that the air had been clear in 1945, but that it was steadily getting worse. Then you could really see into the distance, he said. When I lived in Santa Fe and commuted to Los Alamos, sometimes when I topped the rise out of Santa Fe after a snowstorm, the air was totally clear. I could then see snow-covered Mt. San Antonio all the 70 miles to Colorado, and once made a special trip just to see that isolated land-mass up close.

Due to a marriage of their relatives, Nick became acquainted with the Lebanese investment banker Jaqui Safra. An advisory board was created by Jaqui with the name Global Pursuits. I was invited to join and it was an awesome crew: Gain-Carlo Rota, Daniel Kleitman, Danny Hillis, Robert Sokolowski, and Nick. Once a year or so we would meet for one day in some luxury hotel, and along with the stimulating sessions, we would eat and drink extremely well. One evening in early winter walking down a Boston street, I told Nick of my astonishment and pleasure at the beginning of a Woody Allen film to hear Nick giving a lecture on physics on an unwatched television set in the living room. It fit perfectly, and I told him teasingly that I was happy to finally know a movie star. “I live in Los Angeles now,” I said, “but the first person I happen to know who is in the movies is from New Mexico!” He came back with a story about his connection with Woody Allen, and then of his appearance in the film *The Ox*. “I have my movie career to think of now,” he said with gentle irony and pleasure at the turns of life.

On a brief visit to Los Alamos during the late 1980s, Nick saw me in the lunchroom. He called me over and told me a story about “a friend of mine” who had seen him and Jim Louck having lunch as they so often did. Nick did not know my friend’s name, but the incident was vividly recalled. My friend came up and told them how surprised he was to see them. “I thought you both were dead,” he said. Nick was still irritated at Temple Smith for that breach. I was surprised Nick even cared, and knew that Temple had not said it to upset them.

Some years ago, Nick began to suffer from senile dementia. Not even his appointment as a permanent Emeritus of the Laboratory could save him from his slow decline. Due to a bitter divorce in 1977, all details of which I am ignorant, not even his children were in

contact with him, and only old friends such as Bill Beyer and Jim Louck went to see him in the care facility. Global Pursuits met no more, and I cannot imagine it without his sparkling presence. The idea of a person so elegant-and-dignified in appearance-and-thought sliding into unconsciousness is not a pleasant one.

One pleasure of mine has been a rumor Gian-Carlo passed to me, that Nick Metropolis had dated Georgia O’Keeffe. I did not try to verify its truth because I did not want to learn it was not true. O’Keeffe was older than Nick—about 25 years older—but this did not show the story false: she was famous for her fondness for younger men. When I tried to slip the rumor into a short essay about Gian-Carlo I wrote for *Notices of the American Mathematical Society*, the editor balked, and before I making a big fuss, I decided I should finally check on the truth of the story. Bill Beyer said he had only heard it from me, and his wife Ann said she was positive it was false. I gave in to my narrow-minded prudish editor, and then the next day an e-mail came from Bill.

“Yes, the O’Keeffe story is true. I have a copy of Nick’s own words for it. So you were right the first time”

It turns out that, in an unpublished essay about his life, Nick tells of spending many evenings at Abiquiu with Georgia O’Keeffe, “talking philosophy.” Now, Nick, I can see the twinkle in your Greek eyes as you tell me this, and you know that I will guess the truth. It is only right that a child of immigrant parents from Chicago who worked to save our country during the big war and who helped bring the electronic age into being should have this, only right that with such elegance and grace and wit you should spend crisp high-altitude evenings at Abiquiu with another startling original American. I am certain you both had a glorious time!

Some of my own great good fortune has run out, as it must for all of us. I, a child of the isolation and mud of southwestern Oregon, I knew Stan Ulam, Gian-Carlo Rota, and Nick Metropolis, I stood in the same room with them, I ate at the same table, I rode in their car across the mesas. And I met totally urban Mark Kac at Los Alamos as well when he came to see his old friend Stan Ulam. Heavy brilliant sunlight of summer-time Los Alamos, thunderstorms gathering over the Jemez. Stan went first in 1984, but not until after Mark Kac lured him back to Los Angeles, the scene of Stan’s post-war illness and despair, and I was able to drive him around the city I had become as accustomed to as I had the mesas of northern New Mexico. Not long after that Mark was gone too. Now in one year 1999 there has been the shocking loss of Gian-Carlo, and then the more expected but no less saddening exit of Nick the Greek. It is indeed the end of the century.

Nick, I will always remember the afternoon sunlight slanted across the wide Rio Grande Valley as you drive off the mesa into huge shadows and turn north before the river, up and down through the arroyos, turn again northwest at the Rio Grande Cafe in Española, then go upstream along the Rio Chama past big cottonwoods and pastures and beanfields and adobe houses, and scenes a new American painter was sculpting onto canvas, dry stony draws with stark skulls and thirsty bones and voluptuous flowers projected over them, then head up the rise to Abiquiu, check the bottle of Burgundy under the coat on the seat beside you, and then feel the cool rush of air into the open windows as you descend into the cottonwoods

at the river's edge, you know ahead are the powerful oxbows of the Chama, and then that long run of land where the black volcanic knob of Perdenal juts out of the eroded edge of the Jemez mountains, but you are not going that far around this ancient collapsed volcano. That's where you will always be, just about to stop the car beneath sandstone cliffs and make the walk through the soft early darkness on the path of small stones to that large weathered door.

Los Angeles, California
October 23, 1999

Chapter 6

Introduction to the First Ulam Lecture

The Human Genome and Beyond by Eric Lander

The task of introducing the First Stanislaw Ulam Lecture in Computational Biology in Santa Fe, New Mexico gives me a great deal of pleasure. We had hoped to have Stan's widow Françoise Ulam join us this evening, but she is recovering from a minor operation. But their granddaughter Rebecca Ulam Weiner, a student at Harvard, is visiting her grandmother and is here in the audience.

To steal a phrasing from a political debate, *I knew Stan Ulam*. He was one of the great mathematicians of this century. His early work was in pure mathematics, in the so-called Polish School, and Stan's name is on an amazing number and variety of mathematical objects. From point set topology to branching processes, from functional analysis to complexity theory, from the foundations of probability to chaos, from computers to physics, Stan looked, saw, and started us out on some amazing intellectual journeys. Monte Carlo methods and cellular automata are two more that come to mind. He died over a decade ago, but we are still working on his problems. For some of you who might wish to know more, he wrote an autobiography *Adventures of a Mathematician*. For details of his early mathematics, I recommend another book called *The Scottish Book* which Dan Mauldin edited with much help from Bill Beyer who is here tonight.

Stan came to the U.S. just before the war as a Harvard Fellow. Then he went to Madison, Wisconsin where I believe he was unhappy, something to do with the weather and the Midwest. Then he came to New Mexico to work on the Manhattan Project. Except for a stint as chair of the mathematics department at Boulder Colorado, he spent the rest of his life in New Mexico.

It was at Los Alamos of course where I met Stan the summer of 1970, along with many others. Several people from Los Alamos are here this evening, including Nick Metropolis. (Yes, he is *that Metropolis* whose method is scattered about our transparencies at this

conference.) Los Alamos was a pretty amazing place, very interdisciplinary as a matter of course. It was a given, no hyphenated-program-projects requiring people from different fields to speak to one another, no proclaiming signs on the door.

I don't want to cut into the evening's lecture, but I cannot resist giving my naive picture of Ulam the mathematician. There are sometimes tiresome discussions in mathematics and computer science about "depth," and great emphasis is given to what is called "technical." I don't think this was in Stan's vocabulary or frame-of-reference. Obviously Stan saw very deeply, but it was without effort I think. I believe that he just wandered about his world, gazing at this and that, sometimes moving something a bit to see what it was. Occasionally he reported back to us. It seemed to me he thought he was going slowly, but the rest of us thought it was lightening.

Around 1970, Stan met Temple Smith in Colorado. I don't know if Stan already had the idea that there was mathematics to be found in the new biological sciences, but he brought this notion to Los Alamos. He (and Temple Smith in numerous visits) stirred up things, and Bill Beyer got involved. It was Bill who got me going in what is now called Computational Biology (on an NSF summer grant, to give funders credit too). So it is fitting that the First RECOMB conference is in Santa Fe, Stan's home town.

I was fortunate enough to get my first choice to deliver the first Ulam Lecture: Eric Lander. Some of you know what a furious schedule he has, and appreciate our good fortune to have him here. People speak of "Lander sightings," and somewhere there may be someone whose job it is to keep a database of these occurrences. I must admit I had my moments until he appeared this afternoon. The Poisson distribution was invented to characterize just such rare events as these. I can hear Sam Karlin saying, "Yes, but it is an inhomogeneous stochastic process!" Whatever, we are in luck tonight.

Eric has had a non-linear career path. He went to New York's Stuyvesant High School, then was a mathematics undergraduate at Princeton, after which he went to Oxford as a Rhodes Scholar where he wrote a thesis in algebraic combinatorics. Then he took a job at Harvard. I know you are asking: *when does the non-linear part begin?* Well, surprise: his job was in the Harvard Business School. Which I assure you is *not* in the business of supporting pure mathematics. Fred Mosteller has a long record of such unexpected brilliant hires. Eric was fully on-track then. (Or off-track if you wish to look at it that way.) He spent some time looking into neurobiology, and then he and David Botstein found each other and the rest is history most of you know, or should know. (I was fortunate enough to recommend them to one another, and I want full credit for setting up something that appears on hindsight to be written in the stars!) They tooled up genetic mapping as it is needed for modern molecular genetics. Eventually Eric became a real biologist, running a large lab that has done a great deal of mouse mapping and genetics, with a major effort on the human genome. Yesterday we heard about the human genome mapping effort from his lab. On the side, in his spare time seemingly, he has stirred up legal issues about DNA typing. It is hard to describe the Human Genome Project without repeatedly talking about Eric.

Like Stan Ulam, Eric Lander is someone who moves very very fast, at least from the point of view of us mortals. I wonder sometimes about what Stan Ulam would have had to

say about all the exciting new discoveries in biology. And what would he have had to say to Eric Lander about Eric's maps and genes. I very much wish to have heard that conversation, and they did not miss each other by so many years. I want to be there, in a room listening to them go, watching their gestures. I am not at all sure I could have kept up, but it would have been remarkable to witness.

Instead it is too late for that conversation, but tonight we have the very next best thing, a genuine Eric Lander lecture.

Introduction by Michael Waterman.

Santa Fe, New Mexico

January 21, 1997