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MURRAY GELL-MANN
(b. 1929)

INTERVIEWED BY
SARA LIPPINCOTT

July 17 and 18, 1997

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, particle physics

Abstract

An interview in two sessions, July 1997, with Murray Gell-Mann, Robert Andrews Millikan Professor of Theoretical Physics, emeritus. Dr. Gell-Mann was on the faculty of Caltech's Division of Physics, Mathematics, and Astronomy from 1955 until 1993.

In this anecdotal interview tracing his career to 1960, he begins by recalling his Manhattan childhood during the Depression, family background, early education at Columbia Grammar School. Discusses his undergraduate years at Yale, graduate work at MIT with Victor Weisskopf, courses at Harvard with Norman Ramsey and Julian Schwinger—followed in 1951 by two terms at Institute for Advanced Study, working with Francis Low on a problem in quantum field theory. Summer 1951, University of Illinois, works on complex systems with Keith Brueckner; interaction with John von Neumann.

Joins University of Chicago's Institute for Nuclear Studies, headed by Enrico Fermi; recalls such colleagues as M. L. Goldberger, Leo Szilard, Harold Urey, Gerald Wasserburg; works on dispersion relations and pseudoscalar meson theory

with Goldberger. At University of Illinois, summer 1953, works with Low on elementary-particle field theory, invents the renormalization group; comments on later contributions of Petermann & Stueckelberg, his student Kenneth Wilson.

His early work at Caltech on what was later called S-matrix theory; comments on contribution to superstring theory. Meets future wife, Margaret Dow; travels in Scotland with her, 1954; their marriage. Recruited to Caltech by R. P. Feynman; life in Pasadena; visits Bohr Institute, Copenhagen, summer 1955; Spain, France, and the U.K. Back at Caltech fall 1956, teaches quantum mechanics course. Recollections of Robert and Kitty Oppenheimer, Stewart Harrison. Comments on undergraduate education at Caltech and vain efforts to promote behavioral and social sciences there.

Work at RAND, 1956; paper with Brueckner; objections by Brueckner and Tatsuro Sawada; contributions of Bill Karzas, Don DuBois, Jeffrey Goldstone. *Annual Review of Nuclear Science* article on "last stand of the universal Fermi Interaction" with Arthur Rosenfeld; related work by Marshak & Sudarshan; Feynman's approach; their collaboration; later work by Yang & Lee. Comments on origins of the Eightfold Way. Preoccupation with symmetry, supermultiplets, weak and strong interactions, Yang-Mills theory. Collaboration with Maurice Lévy et al., in France, 1959, on the axial vector current in beta decay.

Administrative information

Access

The interview is unrestricted.

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ORAL HISTORY PROJECT

INTERVIEW WITH MURRAY GELL-MANN

BY SARA LIPPINCOTT

THE SANTA FE INSTITUTE, NEW MEXICO

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ORAL HISTORY PROJECT

Interview with Murray Gell-Mann **by Sara Lippincott**

The Santa Fe Institute, New Mexico

Session 1	**July 17, 1997**
Session 2	**July 18, 1997**

Begin Tape 1, Side 1

LIPPINCOTT: I'd like to start off by talking a little bit about your early life. You were born in Manhattan, around the time the market crashed?

GELL-MANN: Right. A few weeks earlier [September 15, 1929].

LIPPINCOTT: How about your family life? Tell me about your father and mother.

GELL-MANN: I've written a lot about my father.

LIPPINCOTT: He was a teacher of languages, isn't that right?

GELL-MANN: Well, that's what he ended up doing. He had hoped to be a philosophy teacher in a *Gymnasium*—one of the prestigious classical high schools. This was in Austria. He attended a *Gymnasium* and did very well there. Then he went to the University of Vienna, very early in the century. And after the first year, he went to Germany for a year, I think to Heidelberg. The universities were interchangeable, and he would have come back to Vienna after the third year and finished up that way, and with that degree he could have begun a career in the teaching system in Austria. But his parents, who had suffered some financial reverses, were already in New York, and they had problems there. My grandfather had become ill, and they had financial problems stemming from that. So they asked him to come to the United States to help them out.

LIPPINCOTT: Why were they in the United States?

GELL-MANN: As I said, they had had financial reverses, and when Europeans were faced with financial reverses, they didn't know what to do. You couldn't just get a job.

LIPPINCOTT: So they came here seeking their fortune?

GELL-MANN: Seeking a job. They came to the United States, where on arrival as an immigrant you could get some sort of job in a sweatshop. So that's what they did. But it was not working out, and they asked him to come to the U.S. and help them out. So he dropped out of the university after two years and came to Philadelphia, where he was offered a job at an orphan asylum. When I wrote that in my book [*The Quark and the Jaguar* (W. H. Freeman, 1994)], people told me I had to change it to "orphanage," because nobody had ever heard of an orphan asylum. When I was a child, that was a common expression.

LIPPINCOTT: What did he do there?

GELL-MANN: He was a sort of counselor for the children. He knew very little English, but he picked up English, and baseball, from the children, and he spoke English perfectly—no accent and no mistakes. As I said in my book, you could tell he was a foreigner only because he never made mistakes. [Laughter]

LIPPINCOTT: Was he married at the time to your mother?

GELL-MANN: No, they met in New York. He moved later from Philadelphia to New York and got a series of jobs of many different kinds. He tried studying pharmaceutical chemistry at Columbia. And he managed to do it without textbooks—making his own textbook by copying the notes and the lectures and constructing pictures from what he saw under the microscope, reproductions of the microscope slides. He essentially made up his own textbook. He did very, very well in the courses, but he didn't graduate, because he needed a $5 fee and he couldn't figure out how to beg, borrow, or steal the $5 necessary to complete the course.

LIPPINCOTT: While he was doing that, was he working?

GELL-MANN: I don't know—probably. He had a series of jobs. He had a job on Wall Street at one point. And when he was working there, the explosion in front of the Morgan bank occurred. This was in 1920. You know, it was a wagonload of dynamite that exploded in front of the Morgan bank. And his office was right across the street. He was very nearly killed. The window was blown open, smashed, by the explosion, and two jagged pieces of plate glass went by him, one on each side, but they both missed.

He also worked for a toy-importing firm, doing their German correspondence. They wanted him to become an officer of the firm, but he refused. I think he sensed that there was something the matter, and sure enough, the officers went to jail. [Laughter] He had nothing to do with their illegal activities, though.

My parents met in the late 1910s, and they were married secretly in 1919.

LIPPINCOTT: Why secretly?

GELL-MANN: I don't know—their parents weren't ready for it yet or something. And then they had a public wedding on January 1, 1920, and that was their official wedding day. I have the invitation still.

LIPPINCOTT: What was her background?

GELL-MANN: My mother always thought, until about 1940, that she was born in New York. But then she discovered she had been born in Austria and had come over here as a tiny infant. But she had already voted here in numerous elections. [Laughter] So she had to be hastily naturalized. It's a good thing she discovered it when she did, because a year later she would have been an enemy alien. [Laughter] But it's as if she had been born in New York.

Her father died when she was young, and her mother remarried—married a man with six children and had to take care of the younger ones. So it was quite a chore for her mother, but she managed.

My mother did very well in high school, apparently, and wanted to go on to the university. But they wouldn't let her. They said she had to go to work. So she went to Miss

Aub's secretarial school, and at the age of sixteen, or something like that, she became a secretary. And from what I understand, she did quite well as a secretary. She and my father met—I think their mothers introduced them to each other, the least romantic possible way to meet. Anyway, they met sometime in the late teens. I don't know exactly at which stage my father was, in this progression of different things he tried. Eventually, sometime in the 1920s, he opened a language school in New York. And he stayed with that until it failed.

LIPPINCOTT: And it was for teaching immigrants English?

GELL-MANN: Well, one thing was to teach English to immigrants, and to teach English successfully, because my father had learned English perfectly—no accent, no mistakes—and he wanted other immigrants to come at least close to that. But he also taught German. And he hired teachers at one point for Italian, French, Spanish, and Portuguese.

LIPPINCOTT: Did the school fail as a part of the Depression?

GELL-MANN: Well, I think it failed soon after I was born—the market crashed six weeks later. But also, 1929 was the year in which the draconian Immigration Act of 1924 took effect. So there were few immigrants. And if they were there, they had very little money, because following the stock market crash came the Depression. So the schoolroom was largely empty. What the family had was a series of apartments in Manhattan—big apartments, with room for schoolrooms. My father would teach right there in the house. And they were empty when I was one or two, because there were no students, and the thing was obviously failing.

So we moved away from Manhattan. We moved up near Bronx Park, near the Bronx Zoo—near the little bit of hemlock forest that was left of the primeval hemlock forest that had covered the whole of New York at one time. We lived there for a few years, and my father got a job at a bank, which he kept until he retired. He was in charge of the vault. It was not a very intellectually demanding job.

LIPPINCOTT: What did he have to do? Open it?

GELL-MANN: Open it and close it. [Laughter]

LIPPINCOTT: And you had one sibling? That was an older brother?

GELL-MANN: Yes. My brother Benedict is older—something like nine years older. He was born in late November 1920.

LIPPINCOTT: One thing I haven't read about is where you had your elementary education. Was Columbia Grammar School really more of a high school?

GELL-MANN: No, it included everything from kindergarten on. But I didn't start there in kindergarten.

LIPPINCOTT: Where did you start?

GELL-MANN: Well, my mother was a romantic. And she always had dreams for me, of doing great things. Whereas my father was more likely to pooh-pooh things and say, "This is impossible" and "That's impossible."

LIPPINCOTT: Well, he'd had some reverses in his life, and that might have made him—

GELL-MANN: Well, I think maybe it was the other way around: He had reverses to some extent because he didn't believe things were really possible. [Laughter] I can't imagine an American failing to get that pharmaceutical chemistry degree because of a $5 fee. There must be some way to borrow $5.

Anyway, my mother had dreams. And she wanted me to go to a private school. And she dragged me around to a great variety of schools to be tested, because she was convinced that I had special talents or intelligence.

LIPPINCOTT: What made her think so?

GELL-MANN: I don't know. She took me to a lot of places, and nothing worked at the beginning. So I started in public school in the Bronx. And I went through grade 1A. Then 1B gave rise within a few weeks to 2A, and then 3A.

LIPPINCOTT: You whizzed through the first few grades?

GELL-MANN: In the first two terms, I had gotten through 3A and then I did 3B. So that was a year and a half. In a year and a half, I did the first three grades.

Then my mother's dream was realized. She had also taken me to have music lessons at various places—piano lessons—and at one of those places the piano teacher managed to get me an introduction to yet another private school. And that was the Columbia Grammar School—on West 93rd Street, just one building away from Central Park West. And there they gave me, again, a battery of tests. But this time they said yes, they would accept me, and they would give me a full scholarship—which of course was necessary, because my family had no money. They started me in sixth grade. So I skipped four and five.

LIPPINCOTT: How old were you then, when you started sixth grade?

GELL-MANN: It was around my eighth birthday when I started sixth grade. The other kids were mostly around eleven. And then I just continued at Columbia Grammar in the normal way—went through sixth, seventh, and eighth grades, and then four years of high school. They didn't have a junior high system, with seven, eight, and nine compressed into two years. And I'm not sure the public schools had retained that by that time. When my brother went to school, there was a junior high system whereby grades seven, eight, and nine were condensed into two years.

LIPPINCOTT: Was it difficult for you to have classmates who were three and four years older than yourself? Or did you kind of fit in?

GELL-MANN: Well, they bugged me quite a lot, naturally. But I managed.

LIPPINCOTT: You didn't know [physicist and *New Yorker* writer] Jeremy Bernstein when you were there?

GELL-MANN: No. He was there, all right. He was three years behind me. But I didn't know who he was. I knew some of the kids who were one year behind. Like David Wolper. And Steve Ross, who died just a couple of years ago. I knew them slightly. And I knew a few of the kids who were two years behind, like Charles Israel, who is now the head of the bank in Aspen where I bank, and almost my next-door neighbor in Aspen. His brother Justin was in my class. But I didn't know anyone in the class three years behind, and I understand that's where Jeremy was.

LIPPINCOTT: You weren't the least bit interested in physics at that point in your life?

GELL-MANN: Well, I was interested in everything, really, except sports. And even in sports, I was interested in soccer—I liked soccer a lot. In fact, I knew something about sports, actually. I knew all the rules of baseball and football and so on. And I followed the games. So it isn't even true that at that time I was uninterested in sports. In high school, I was the manager of the tennis team. So I guess that's not really true, what I just said. It was later that I completely lost interest in sports, except perhaps soccer.

Anyway, about physics. I tried reading physics books. I had great difficulty with them. They were elementary physics books and in many cases obsolete.

LIPPINCOTT: And you said that connections weren't made between the various fields.

GELL-MANN: Well, that was later. That's true, what you say, but the remark I made was about a physics course I took at Columbia Grammar many years later, in high school. That course was discouraging. We had to memorize the seven kinds of simple machine—the pulley, the inclined plane, the wedge, and so on. Ridiculous! And, as you said, the different subjects were not connected—mechanics, wave motion, sound, light, heat, electricity, magnetism. One would never have known that there were a few simple laws that governed all of these things.

LIPPINCOTT: How about math? Was that what you were interested in, principally?

GELL-MANN: No, not principally. I liked math a lot. I was very anxious to understand advanced math. But there were certain mental blocks that prevented me for a while from going ahead. Maybe they came from my father, who enjoyed teaching math. I think his teaching may have interfered with my learning. [Laughter] He kept telling me that in calculus, dx was a little bit of x. And if it was just a little bit of x, I couldn't understand why you could throw away dx^2.

Because he didn't want to talk about limits. It's only in the limit that *dx* goes to zero that you can throw away dx^2, by comparison with *dx*. And since he didn't mention limits when he tried to teach me calculus, I never really understood it. But later, on my own, I understood it.

I liked math a lot, but that was not my principal interest. My principal interests were all in subjects involving individuality, diversity, evolution. History, archeology, linguistics, natural history of various kinds—birds, butterflies, trees, herbaceous flowering plants, and so on—those are the things that I loved. Plus mathematics. Plus all sorts of other things—art, for example, and music. My brother and I, together, were interested in all of these things. We discussed them, went to museums. New York was not a place you could enjoy very much if you didn't have much money, except for the parks and the museums and the libraries. So that's where I was all the time—the parks and museums and libraries, all the free things.

LIPPINCOTT: When it came time to go to college, why did you choose Yale particularly? Did you apply to other places?

GELL-MANN: No. I should have, of course, It's crazy to apply only at one place. But I was very neurotic. I didn't get things done properly. I'm still a little bit that way—I've never changed that much. But I've become somewhat more effective in the course of fifty years.

So I ended up applying really only to one place. I sent for applications to many, many, many colleges. I studied their catalogs; I thought about them; I dreamt about what it would be like to be at all these colleges. But the only one I actually completed the application for was Yale. The reason is that I didn't see any kind of scholarship at the other colleges that would be big enough so that my parents wouldn't have to contribute anything. But Yale had one scholarship called the Medill McCormick Scholarship, which paid enough for room, board, tuition, laundry, and so on and so forth—everything! And it didn't require taking what was called a bursary job—that is, paid student employment. At Yale, every other scholarship recipient except the Medill McCormick Scholar had to have a job. And at my age—fourteen when I applied, fifteen when I got there—it would have been quite a burden, I think, to have a job and the scholarship. Later on I did take a job for a little while—something in the physics lab.

In any case, that one scholarship was what I set as my goal—to be admitted to Yale to get that one scholarship. It was ridiculous! The chances were just about zero that I would be admitted to *the* university I wanted to go to and get *the* one scholarship that would permit me to attend. But around April I was admitted to Yale—April 1944. And in June, I was on a visit to my old music teacher's house in Englewood, New Jersey. I woke up early in the morning, because the radio was blaring, announcing the invasion of Normandy—the 6th of June, 1944. That was the day I rode my bicycle back to Manhattan to our house, over the George Washington Bridge, and when I got home there was a letter in the mailbox from Yale. I tore it open, and it said I'd been awarded that very scholarship.

So I went to Yale. I delayed four months. I don't know whether that was a good idea, but I delayed four months. I had some vague idea of having a job over the summer and making some money. It was crazy, because I was too young—the law didn't really permit me to work. I got plenty of job offers, but the law didn't really permit me to work at any of them. I would have had to falsify my age somehow, which is difficult to do, in order to get the work permit. However, I could work on President Roosevelt's campaign for a fourth term, which I did.

LIPPINCOTT: What did you do? Pass out pamphlets?

GELL-MANN: No, I worked for the Labor Committee—CIO-PAC, I think, or what was later called CIO-PAC, a fairly left-wing Democratic organization. I escorted the various celebrities around—before lunch, after lunch, and so on. Got them glasses of water while they were speaking at lunch. So I got to meet Dorothy Parker, Helen Keller, Frank Sinatra—people like that.

But I want to tell you about the scholarship. Later on—much later, my senior year—I was supposed to graduate in January of 1948. In '47, I went into the bursary office and asked them what I could do about that, because I didn't really want to graduate in January; I wanted to graduate in June. And I said, "Can I have a scholarship for a ninth term and graduate in June?" They said, "Well, we could consider that. But we would recommend that you graduate right now, in June '47, after seven terms, and get a Henry Fellowship to Cambridge." I thought, Wow, Cambridge, England! It was an incredibly romantic idea. I didn't think about the fact that there was no food in England, and so on. [Laughter] I said, "Very good."

So I rushed home and filled out the application for a Henry Fellowship—very uncharacteristically, because typically it would have taken me two years to do such a thing. But

I was one day too late. If they had made the recommendation the previous day, my application would have been considered. But this way they rejected it as having been submitted one day late. So they said, "Sorry we didn't tell you about that in time. Why don't you stay nine terms if that's what you would like, and we'll give you a scholarship for nine terms." So I did stay. I took a lot of silly courses in which it was impossible to get very high grades. So I diluted my academic standing, and I ended up being second instead of first in my class.

LIPPINCOTT: You must have had a huge course load, if you could have graduated in three years.

GELL-MANN: Three and a half. Yes, I took lots of courses. I loved courses. Sometimes I dropped them, but I started out always with lots of courses. And then I kept as many as I could. I loved the idea of learning lots of different things.

LIPPINCOTT: What were some of the silly courses you took?

GELL-MANN: I don't mean that the subject matter was silly. I just mean it was foolish to choose those courses, because they were courses of thousands of students with exams graded by graduate students. And it was impossible to get a good mark in those things; they just didn't give them. One of them was on modern drama. These were so-called gut courses, meaning that they were ridiculously easy for football players to take and pass. They were not courses in which there was any way of doing very well.

But what I wanted to get at was that in this interview in which they said, "OK, you can have nine terms," they suggested that I write a letter to my unknown benefactor, the scholarship donor. And I just couldn't do it, because I looked at the names. McCormick; Medill. This was clearly the family that had produced Joseph Medill Patterson of the *New York Daily News* and Robert McCormick of the *Chicago Tribune*. Both, as far as I could tell from my point of view—both on the wrong side of the war. [Laughter] And I just couldn't think of thanking somebody from that family—writing some obsequious letter thanking somebody from that family. I would have had to say, to be honest, "Thank you very much. This scholarship has made a huge difference to my life. But I can't say that I approve of your family and the way it made its money and the way it's behaved." But I couldn't send a letter like that, I thought. So I didn't; I never wrote the letter.

Thirty years later, in Aspen, in a garden in Aspen, I met Katrina McCormick Barnes, who had donated the scholarship when she was in her twenties. Her parents had died, and she didn't like the McCormick family, to which she belonged. She didn't like the way they made their money. She didn't like her Uncle Bertie [Robert McCormick]. She detested her Uncle Bertie. [Laughter] She hated the whole scene. And she decided she couldn't keep a penny of this tainted money, and she was going to give it all away. [Laughter] She hired a young lady assistant and opened an office and started giving the money away, and one thing she did was to create this scholarship in memory of her younger brother, who had died in his teens and never had a chance to attend—or at least graduate from—a university. And his name was Medill McCormick.

LIPPINCOTT: So you discovered this in Aspen, thirty years later?

GELL-MANN: Thirty years later, yes. She now lives here in Santa Fe. Marcia [Marcia Southwick Gell-Mann] and I were just dining with her a couple of months ago. She's a wonderful person, and her husband, Gordy Barnes, was a wonderful person. He died just a little while ago. I saw them a number of times after that—in the last twenty years, I've seen them a number of times.

LIPPINCOTT: So you've had some serendipity in your life.

GELL-MANN: That was such a remarkable situation! If I'd written that letter that I thought would have been so disgraceful, she probably would have loved it. [Laughter]

LIPPINCOTT: Yes, she probably would have. Before we leave Yale, is there any truth to this Skull and Bones story? Did you break into Skull and Bones?

GELL-MANN: No, I didn't break into Skull and Bones. However, there is considerable truth in the story. I was acquainted with a student who had been involved in a break-in some years earlier with a friend, just for the purpose of seeing what was in there, because that was supposed to be forbidden territory. But I didn't know they had done that; I just knew they had a lot of interest in what went on inside. There were a lot of rumors about what it looked like inside.

So what I did was to seize the occasion when some repair work was being done on my college, which was next door, and I borrowed some ladders and erected them over the back wall of the college, down into the courtyard of Skull and Bones, and then up the side of the main building there.

LIPPINCOTT: Was this at night?

GELL-MANN: No. I did this actually during the day, with the help of a friend. Then I recruited a fellow from another college whom I knew slightly—an Englishman who had been sent over to the U.S. during the war to escape the bombings. His father was a Harley Street surgeon, I believe. He was quite willing to do this job. He had never heard of Skull and Bones, but the idea of engaging in this adventure was something he really liked. And he recruited a friend of his, who later became a very famous documentary filmmaker. The two of them actually did this thing. They used the ladders; they climbed up to the roof. And they were supposed to open a trapdoor in the roof and climb down inside and see what was there. However, from that point, the reality diverged from the scenario.

First of all, they found that since the previous attempt—about which I learned just then, from my other friend—since that attempt, Skull and Bones had put in a lot of bars that had to be sawed through. So we passed up a hacksaw. And for hours and hours and hours, we would walk back and forth in front of this building and listen to the hacksaw, as they were sawing their way in. [Laughter]

They were supposed to come get us when they had gotten in, so that we could come in and look also. But they didn't get in until the morning, when it was light. By this time, my friend and I had fallen asleep in our chairs. And it was then that they came over and said, "It's open. Go in now." And we said, "But the janitor's due in five minutes. It's useless." [Laughter] I never saw the inside. And they did borrow, or steal, some things from there. But that was not my idea. Burglary was not the idea. The idea was to see what was in there.

Now, as to what they found, it's no longer a secret, because Garry Trudeau had it all in his Doonesbury cartoon—namely, the skull of Geronimo, stolen from some fort in the Southwest, and the tombstone of Elihu Yale from Wrexham, labeled "Wrexham, England," although Wrexham is actually in Wales. Everything on the wall was labeled "stolen from" somewhere—all stolen. And there was a huge array of official pins of Yale organizations—all of them stolen. So if my friends stole a few things from there, the only things they stole were things that were marked "stolen." [Laughter] They stole, I think, some of those pins.

However, there was a Bones man back from the war. During the war, they were all gone, I guess. But there was a Bones man back from the war, and he stole it back. He burglarized their rooms in their college and took all those pins back. So in fact there was no net loss.

LIPPINCOTT: We ought to talk a little bit about the academic side of Yale. You became a physics major there.

GELL-MANN: Well, I explained in my book how I took up physics. You know that story.

LIPPINCOTT: Well, there was Professor Henry Margenau?

GELL-MANN: Yes, but that's later. I explained in my book about how I had discussed with my father what to fill in on the application. And I thought archeology or linguistics, or something like that. And he said, "No, no, you'll starve." So I said, "Well, what should I put down?" He said, "Engineering." And I said, "I'd rather starve. Besides, if I design anything, it will fall down, fall apart." Which is true. So later on I took an aptitude exam as a freshman at Yale. My freshman advisor gave me the results, and he said, "According to these results, you can be a lot of things: in science, the arts, humanities. But don't be an engineer." [Laughter]

So then I said, "Well, what do you suggest?" My father said, "What about a compromise? What about physics?" And I said that that course in high school was a disaster. It was the only course in which I did badly in high school, and I hated it. He said, "Oh, that doesn't make any difference. At the university, you'll study quantum mechanics and relativity, and you'll love it. It's marvelous." So I took physics, and after a while I got to like it. And I found that my father was right, in fact—uncharacteristically, he was quite right. Quantum mechanics and relativity were marvelous.

LIPPINCOTT: Nuclear physics was the hot thing then, in the forties?

GELL-MANN: Yes.

LIPPINCOTT: Had a lot of quantum stuff crept into the curriculum at that time?

GELL-MANN: Oh, yes.

LIPPINCOTT: And you were searching around for graduate school. And MIT was the only institution that would give you a scholarship?

GELL-MANN: They didn't give me a scholarship, actually. What happened was, I didn't complete my senior essay at Yale, and I think some of the recommendations were not very strong from the Yale physics people. So Yale actually turned me down in physics. Harvard accepted me but didn't give me any opportunity, as far as I could tell, to make money, and I didn't see an assistantship or a fellowship or anything materializing from Harvard. Harvard claimed later that they had turned my application down, but that's not true. I was accepted; I got a little card saying I was accepted at Harvard, but that's all. I never got a letter saying there was a scholarship, or that I could have an assistantship to pay my tuition or room and board, or anything. Princeton just didn't take me. Yale's Mathematics Department solicited my coming. They wanted me to come back to Yale as a graduate student, but in mathematics. But I didn't want to study mathematics; I wanted to study some kind of science—something that had to do with the real world, rather than with abstraction. Although I loved mathematics, and I would have been very happy to use mathematics. And I did use a little bit of mathematics later. But I didn't want to make that my subject of study. I didn't want my work to be judged on the basis of whether or not it was mathematically sophisticated. I wanted it to be judged on the basis of whether or not it had something to do with the world.

So I took the only situation that offered itself where I could be compensated, and that was MIT. They offered me a job. They wrote and asked me to come. They sort of begged me to come. And they said that Victor Weisskopf would be my advisor and I would have a job working as his assistant. I wouldn't have to do some miserable, grubby work as a teaching assistant; I would work directly for this Professor Weisskopf. Of course, I had never heard of Professor Weisskopf. And I had no idea whether MIT was any good. I had had my heart set on the Ivy League, or possibly Michigan. Somebody had told me that the University of Michigan was really strong.

So I talked to some of the Yale physics teachers, and they said, "Oh, this is wonderful! You don't understand. Viki Weisskopf is one of the best people in the world. You'll love MIT." Well, I didn't believe them. I thought I would commit suicide—that would be the easiest way out. But then it occurred to me that I couldn't then try MIT, whereas if I tried MIT I could perfectly well commit suicide up there, if I didn't like it. [Laughter]

LIPPINCOTT: Yes, that was much more sensible.

GELL-MANN: So I went to MIT, and it was a blast! All those Ivy League people I wanted to be with were there in the graduate school course, just like me. [Laughter] And Viki was a marvelous person, just one of the world's best people. He's still alive, by the way. He lives in Newton, Massachusetts, now, with his second wife.

LIPPINCOTT: What kind of work was he doing at that time?

GELL-MANN: Oh, the most important work of his life. He and a graduate student, a very advanced graduate student—a man from Newfoundland called Bruce French—they were learning how to eliminate the infinities from quantum electrodynamics, in connection with the most prestigious problem in physics at that moment, which was to explain the so-called Lamb shift, the relativistic part of the Lamb shift, and make it come out finite, and make it come out correct and in agreement with observation.

But in publishing it, they ran into some peculiar things. Richard Feynman and this person [Julian] Schwinger had some supposedly very advanced methods and were finding a different result. And Bruce said, "We have the right answer. Let's publish it right away." Viki said, "No, I'm frightened. Feynman's getting a different answer, and Schwinger is getting a different answer. And they have their elephant guns; we just have our little pistol. There must be something wrong." Because Viki and Bruce were doing it by this incredibly clumsy, old-fashioned method.

So they delayed, which was really a disaster, because Feynman and Schwinger were both wrong. And they were wrong for a very simple reason—namely, the nonrelativistic calculation had been done by Hans Bethe using this clumsy formalism. And so Viki and Bruce French could relate their relativistic result very easily to the nonrelativistic work of Bethe. Whereas for

Feynman and Schwinger, it was a huge problem, and they kept doing it wrong, to match their elegant relativistic calculations against this clumsy nonrelativistic calculation of Bethe. Whereas Weisskopf and French were using exactly the same method as Bethe, except doing the relativistic part—with the renormalization and everything—doing it perfectly correctly. So they had the right answer; the other people had the wrong answer.

Schwinger never apologized for this. But Feynman wrote an apology. He put a footnote in one of his papers ["Space-Time Approach to Quantum Electrodynamics," *Phys. Rev.* 76:6, 769-89 (1949)], and he arranged a whole bunch of asterisks and little crosses and so on, so that the footnote would come out number 13. And he said, "I'm sorry that an earlier result of mine in matching this relativistic calculation with a nonrelativistic calculation of Bethe gave the wrong answer and this delayed the publication of the work of French and Weisskopf. This footnote is appropriately numbered." [Laughter] Schwinger, of course, never said anything.

LIPPINCOTT: Did you have much interaction with Julian Schwinger?

GELL-MANN: Yes. I was allowed to take any course at Harvard that I wanted. MIT and Harvard had a reciprocal arrangement. So I went over there and signed up for a course that Schwinger was supposed to teach. But Norman Ramsey came in and taught it instead. That was fine—Norman Ramsey's a wonderful man.

LIPPINCOTT: What was the course?

GELL-MANN: I don't remember—some kind of advanced quantum mechanics course.

LIPPINCOTT: But weren't there informal meetings of theoretical physicists at MIT and Harvard?

GELL-MANN: Yes. I just wanted to say, though, that then the next year I took a course actually taught by Schwinger. But I didn't find it very stimulating, because Julian came in very late. And then he started in the upper left-hand corner of the blackboard—started writing equations, very elegant, very beautiful-looking equations. And then he finished in the lower right-hand corner of the last blackboard, and fled. And when he finished, it was lunchtime, so most of us missed lunch—because he came in twenty minutes late and left twenty minutes late, and that twenty minutes was the time in which we had to get to the cafeteria before it closed.

LIPPINCOTT: Well, he was nocturnal, wasn't he?

GELL-MANN: Yes, he was nocturnal.

But I never got anything out of that course. You could memorize those equations, but there was never any question of problems that might come up, difficulties that might come up, resolving difficulties, questions about why he did this rather than that. He just ran through it and left. So, although these lectures were supposed to be elegant, beautiful, marvelous, and we were supposed to admire them, I never got anything out of them. Whereas Viki would come in and stumble around and write some wrong thing on the blackboard. Then he'd erase it and say, "Now, wait a minute. What's the matter with this? How is this supposed to go?" Even if it was something he invented, he would get it all wrong. And then he'd say, "Well, you know, the problem is, today I didn't prepare. Next time I'll come in prepared." Next time he'd come in and write the equations confidently on the blackboard. But they were still wrong. [Laughter] Of course, every student in the class learned how to do it right.

Also, Schwinger was not ever willing to admit anything. If he made a mistake, he would try to conceal it. He would never lay bare any of his thought processes. It was all just "Here's the answer. Take it or leave it."

LIPPINCOTT: Did he stay that way throughout his life?

GELL-MANN: Stayed that way and got much worse. He took to appropriating everyone else's results and writing them in his own notation, and sometimes getting them wrong in the process, as when he appropriated my results. It was discouraging. I never thought much of him. I loved his wife, Clarissa—a really nice person. How wonderful of her to put up with this guy! **[Tape ends]**

Begin Tape 1, Side 2

LIPPINCOTT: What did you do at MIT? What was your thesis on?

GELL-MANN: Well, my dissertation was on nuclear physics. I never did much in nuclear physics after that, but the dissertation was on nuclear physics. It came about this way. The shell model—as studied, say, by Maria [Goeppert-]Mayer and a couple of other people—turned out to be very good at predicting the ground states and low excited states of nuclei. The shell model is the kind of thing you would have if the nucleons—the neutrons and protons in the nucleus—were fairly weakly coupled, just had a tendency for their spin in orbital angular momentum to act as a unit: the so-called J-J coupling. Under those conditions, you would get the shell model just from simple considerations—almost free particles.

But there was a huge amount of evidence that in the nucleus the coupling was actually very strong. And when a particle came in, it sort of got lost in there, in this big messy welter of nuclear interactions. And it took a long, long, long, long time to come back out, by which time it had largely forgotten how it got in. Which is a quite different picture and implies very strong coupling.

So Viki said, "Well, it must be that these pictures are compatible with each other somehow; the coupling must be intermediate. Can you work out anything about what happens with intermediate coupling?" As the coupling gets stronger, what happens to these energy levels of the free particles that you start with?

So I worked on that, and I supplied part of the answer, but I didn't really finish it in the dissertation. The rest of the answer was given a few years later by [Eugene] Wigner, [A.M.] Lane, and [R. G.] Thomas.

LIPPINCOTT: Who are Lane and Thomas?

GELL-MANN: Two young nuclear physicists at the time. One was an Englishman called Tony Lane, a very interesting man but a little bit odd. He used to engage in all sorts of risky behavior—a "mad dogs and Englishmen go out in the midday sun" kind of Englishman. In fact he did that—he went out fishing in India, along the Indian coast, without a hat. Anyway, he was apparently a very bright guy. I never met him. And Thomas was a bright young student in nuclear physics who spent some time at Caltech. But again, I don't think I ever knew him.

The problem was finished later, in 1955 or so, by Wigner, Lane, and Thomas, but I began it.

The dissertation had a lot of little gems in it, which I didn't understand to be gems. They were new results—quite interesting new results—that were publishable. But I didn't know they were. I couldn't tell what was important and what wasn't important, and I didn't want to publish anything that wasn't fantastically important. Of course, that's a very poor attitude. When you pursue something that's fantastically important, you're likely to find nothing. And as I wrote in a recent essay called "The Garden of Live Flowers," you're more likely to discover something really important by having a more modest objective—if the modest objective has been wisely chosen.

Anyway, most of the dissertation research was finished in June 1950, after just two academic years. And then Viki left for France, for Paris, to be a visiting professor. At first he said, "Come with me." And I thought, "Wow, that's a wonderful idea! I have this government scholarship. I could take it to Paris, and it would be just as good in Paris as here. Wow! I'll get to see France! This is going to be fantastic!" I was really excited. And then some weeks later, he said, "No, I've changed my mind. I don't want you to come to Paris with me. I want to go there by myself. I don't want to bring a student and look as if I'm trying to Americanize the place." So I didn't go with him.

Anyway, instead of writing up the results, I wasted the summer and the fall reading *The Tibetan Book of Dead* translations by Evans-Wentz, twelve volumes, and stuff like that. I just frittered away my time. I've done a lot of that in my life—just frittered away my time reading pointlessly.

LIPPINCOTT: It doesn't show—that you've wasted your time.

GELL-MANN: Well, I've wasted *most* of my time during my life. The little bits of time I wasn't wasting maybe I made good use of, but mostly I've just wasted my time. Anyway, I frittered away the summer and fall. And the economic situation got very bad, because it turned out that one of these Atomic Energy Commission fellowships had been given to a Communist. A

Communist! Now, of course, these Atomic Energy Commission fellowships had nothing to do with secret work, but they were given by the agency that did secret work.

LIPPINCOTT: Is that what funded your graduate work?

GELL-MANN: No. First was the MIT assistantship to Viki, and then, after a year, these Atomic Energy Commission fellowships were created. I applied for one and I got it, and it was splendid. The very first batch. It worked beautifully. But then, after a year, they took away the money. They stopped sending the money because this Communist had gotten one. They suddenly required clearances from everybody. It was totally pointless! Just some stupid senator called [Joseph C.] O'Mahoney insisted on this—the O'Mahoney Amendment.

So I didn't have any money. I stayed clandestinely in the graduate house without paying rent, and my friends kept lending me money. They invested in me, basically. I had wonderful friends in graduate school. They all gave me money, and I promised to pay them back when I got my fellowship money.

Well, things went on and on. And they found all sorts of bad things about me. I apparently had known people who were very bad—Communists and so on. I wasn't aware of any of it. I told them that I had had Communist sympathies until I was around fifteen or sixteen. But did they really care about that? Anyway, finally around New Year's Day, it was all resolved. And later—February or so—I got the money. I had to fly down to Washington, talk with these security people, and then it worked out.

But all that time I had been living under these horrible conditions—borrowing money, living illegally in the graduate house. So finally around New Year's Day, I got fed up with this life. I had been accepted at the Institute for Advanced Study, but I couldn't go there until I got my degree. All during that fall, I was supposed to be in Princeton, and I was still at MIT, frittering away my time, not finishing the dissertation.

Finally, New Year's Eve, I got drunk and decided, "This is the end. I just can't go on with this anymore." So I sat down with the liquor bottle and I started to write the dissertation. And I wrote it New Year's Day, January 2nd, January 3rd. By about January 4th, I'd finished it. [Laughter] Drinking all this alcohol, writing the dissertation. Anyway, I finished the damn thing. I had my exam, passed the dissertation exam, left for Princeton, settled with the government, got my money, paid back my friends, got my income from the Institute for Advanced Study, bought a car. All of a sudden, I was a person. I'd gone from being this miserable wreck of a human being all through December to being in February a real person, with my debts paid off, an income from the Institute for Advanced Study, a wonderful office, and a nice little room in a boarding house in Princeton. I was no longer suspected of being a Communist agent. [Laughter]

LIPPINCOTT: And this was all just in the space of two months—January and February, 1951?

GELL-MANN: Yes. Or even just one month. By the end of January, things had cleared up a lot. By February I had a car, and so on.

LIPPINCOTT: Tell me about Princeton. [J. Robert] Oppenheimer was the [institute] director.

GELL-MANN: Yes.

LIPPINCOTT: How were you received there?

GELL-MANN: Well, Oppenheimer and I got along very well. He liked me. He thought I was a good person.

LIPPINCOTT: You were acquainted with him before you got to Princeton?

GELL-MANN: No. Only that he called me on the phone at the graduate house at MIT and said, "What is all this? We don't have any data on you. We don't know how old you are. We don't know anything about you. We're prepared to accept you here, but you have to send in the application form with all these data. And you have to finish your dissertation and come down here." So eventually I did it. My roommates, who were putting me up for free and putting up with my poverty and my obnoxious characteristics, were very impressed that Oppenheimer had called on our phone.

LIPPINCOTT: What kind of work did you do down there?

GELL-MANN: Well, it was very nice. I worked with Francis Low. We were officemates. We worked together for many, many years. That spring, we worked on a rather formal problem together. But it was the kind of thing the Institute for Advanced Study encouraged. Ed [Edwin E.] Salpeter was a student of Hans Bethe, and he and Bethe had written down this relativistic equation for the two-body problem in quantum field theory. It was approximate, but you could imagine what the higher terms would be, so that you could make it exact—except, of course, it would be a long series of things and you couldn't possibly solve it. But anyway, it was an interesting proposal. It looked very pretty. It was fully relativistic. And most ways of solving the two-body problem didn't look like that; they were much uglier.

So Francis Low and I looked at this thing. They'd given us sort of a sketch proof, based on Feynman diagrams. But we supplied a serious basis for that proof. And in the course of that we found a number of little tricks and a number of formulae and a number of ideas which were fruitful later. I was not very proud of this, because all we'd done was solve a formal problem. All we'd done was give some formal background for something that was already known—namely, their equation. If we had shown that it was wrong, and that it should be something else, that would have been exciting. But this way it was just formal.

Viki had taught me that formal victories are not very important victories. And, of course, I had a lot of talent for formal things, but I knew from association with Viki that one shouldn't push it too hard; one should try to find substantive proof, not just formal proof. That's very important. Because I could so easily have spent my life pursuing just formal things. I was very lucky that I went to MIT, with Viki.

Anyway, Francis and I did quite well with this problem. People were quite impressed. And Oppenheimer, who loved formal things, was especially impressed.

LIPPINCOTT: Did you give talks there?

GELL-MANN: Yes, we gave talks there and elsewhere. And then, since I had missed out on the first term, I stayed another term. I stayed through the fall. I probably could have stayed another year, actually. But what happened was, during the fall, I worked on some other thing. I didn't make much progress on it. But I tried a lot of different things. It was quite a useful term, even though I didn't produce much—didn't produce anything, really. But I tried a lot of things. And having tried a lot of things, I learned what roadblock you'd run into in this direction and what you'd run into in that direction, which was quite useful.

In the summer of 1951, between my two terms, I went to the University of Illinois. I went to a classified project, which didn't have much reason to be classified—at something called the Controls Systems Laboratory; this was connected with building some kind of air defense system in Korea. But we actually had nothing to do with the air defense system and nothing to do with Korea. We were concerned with something very much like what the Santa Fe Institute studies today, although this was forty-six years ago. We were interested basically in complex systems and how they operate and how they're controlled. It was a very interesting summer.

LIPPINCOTT: Who was there with you?

GELL-MANN: Well, Keith Brueckner and I both went there. I knew him well from the Institute for Advanced Study. He was on his way to a job at Indiana University, which was the only assistant professorship available that year, and I was going back to the institute in the fall for another term, as I explained.

Sid [Sidney M.] Dancoff was there. But he was already fatally ill that summer. He was only thirty-seven or something like that. But he was the one who brought us there, to this project, to think about all these things. And there were all sorts of interesting visitors—the biologist Colin D. Hendrigh, whom I knew much later. And lots of other biologists—Luca Cavalli-Sforza.

LIPPINCOTT: Did you work with biologists?

GELL-MANN: Well, they were visiting mostly, and giving talks. In the meantime, while listening to lectures, I did work a little bit on quantum field theory. I got some interesting results—which, again, I didn't realize were new, and which I didn't publish until years later when other people had already found them. That was my usual pattern. I found things much earlier than other people, but I delayed publishing them for a year or two, usually. By that time, they were joint, because somebody else was also working on them. It was true of these representations. Many people called them the Lehmann representation, but of course I found it a year or two before, during this summer.

Keith Brueckner and I worked on a problem of how to purify computations done by a computer in which the individual elements were extremely unreliable—so unreliable that they approached random performance. Now, for vacuum tubes that was not such an insane idea, because vacuum tubes have a lot of problems. We didn't know that the transistor was going to be invented then—that individual elements in computers were going to become very, very reliable. But we looked at the opposite case, in which each element is extremely unreliable, asking what one would do to purify the signal. Well, the fairly obvious thing to do was to take three different copies and have a majority vote. So we constructed conceptually a little majority voter. And then by majority voting over and over and over and over again, we would purify the signal. And we got exponential purification. But we didn't show it in a very convincing manner—that we were actually getting this exponential purification.

Just then, John von Neumann came through as a consultant for a day, and we showed him what we were doing. He looked at it in a more sophisticated way than we did. I mean, he approximated our cubic equation by a linear approximation to the cubic equation and got the answer and saw that it was exponential. We said, "Yes, that's fine. That's about what we're doing." And he said, "Oh, this is very, very interesting." And later on he went to Caltech and gave a series of lectures in which he presented all this material. And he thanked us, not for proposing the problem—for constructing what was essentially the solution to the problem—but for inventing this little majority voter thing, which actually anybody could invent in five minutes. [Laughter]

And when I saw this article, instead of being furious at Neumann for giving us so little credit I was flattered that he had mentioned us at all. It was a funny reaction. This is interesting to you, perhaps, because it involves Caltech. That's a very famous lecture ["Probabilistic Logics and the Synthesis of Reliable Organisms from Unreliable Components"], and it's been read over and over by lots of people. And there we were, in this footnote. But I realize now that we should have gotten a lot more credit. [Laughter]

LIPPINCOTT: When did you hear from the University of Chicago?

GELL-MANN: Well, you see, a guy who had been my officemate at MIT—a postdoc named Murph [Marvin L.] Goldberger—had gone on to Chicago. He was offered many assistant professorships, including Harvard. But he had been a graduate student at Chicago, and he decided to go back there to teach with [Enrico] Fermi. He admired Fermi greatly, while recognizing his limitations. But he loved to work there, with Enrico. And he wangled me an instructorship at the University of Chicago.

The Institute for Advanced Study people were very surprised that I didn't ask them for any help in getting a job. They said, "Murray, it's getting late in the term. You really better look into someplace to work. Or do you want to stay longer? We can probably arrange for you to stay longer. You really better look into this. Or don't you want us to write some letters?" So I said, "Well, actually I have a job." [Laughter] And they said, "What!?"

The problem was, it was only an instructorship, it wasn't an assistant professorship. They'd run out of assistant professorships everywhere, because after the war all these people were coming back who had gone through their graduate training, and they were taking all the jobs. There was only *one* assistant professorship, and that was the one Keith Brueckner got at Indiana University. But Murph wangled me this instructorship at Chicago.

So I went in to see Frank [Chen Ning] Yang. And I said, "You had this job, didn't you, for a year?" He said, "Yes." Same job. So I said, "Well, tell me, first of all, how many instructors are there? Are there ten or twelve?" He said, "No, you'll be the only one." I said, "Oh, that's nice." [Laughter]

LIPPINCOTT: But then you'd be teaching your head off, I guess.

GELL-MANN: Well, my next question was, "Will I be teaching my head off?" He said, "No, probably one course during one quarter." I said, "Well, that's pretty nice for an instructorship." [Laughter] "But then there's no chance of being promoted?" Because often that's true of instructorships—say, at Harvard, where your chance of becoming an assistant professor or an associate professor is virtually zero. He said, "No, it's virtually a hundred percent." [Laughter] So I called Murph and said, "I accept the job."

LIPPINCOTT: And was everything that Frank Yang said true?

GELL-MANN: Yes.

LIPPINCOTT: And this was the Institute for Nuclear Studies that you were in?

GELL-MANN: Now called the Fermi Institute for Nuclear Studies, since Enrico died. Yes, that was where I was, with Murph, with Enrico, with Harold Urey, with Leo Szilard. Those were the people I had lunch with every day. Gregor Wentzel. And every Monday, we theoretical physicists had a meeting—Fermi, Murph, Wentzel, and I, and sometimes one or two other people. Oh, and Ed Adams.

LIPPINCOTT: This was about the time that Urey and [Stanley L.] Miller did that wonderful—

GELL-MANN: Yes, I remember when Miller did that experiment with the lightning and had little bugs crawling out of his lab. And there was this other student of Urey named Gerry [Gerald J.] Wasserburg—who came to Caltech later. He was a graduate student, and he blew himself up. I was there. It was at night. And I was there in an office in the next building, and I ran over to see what had happened. I thought maybe the building had blown up. Well, a large part of the building *had* blown up. And here was this graduate student, covered with blood. So I said, "Come on, I'll take you to the hospital. But I have a new car. Please don't bleed too much." [Laughter] He remembered that.

LIPPINCOTT: Was he all right then?

GELL-MANN: Well, they sewed him up at Billings Hospital.

LIPPINCOTT: Was there much excitement about the Urey-Miller experiment at the time?

GELL-MANN: Well, yes. Lots of exciting things were going on. We were excited about all of them. Urey would talk about the moon every day at lunch—whether the moon was cold or hot, or ever had water. You know, they said there was no water, there couldn't have been any water, so how did all those grooves get there? They were not made by rivers. What happened? And so on and so on. It was very interesting. And Szilard had all sorts of interesting things going in biology. It was a very exciting time. We would all meet together once a week.

LIPPINCOTT: Where would you meet?

GELL-MANN: Some big conference room. And we had sort of a Quaker meeting—anybody could get up and say anything.

LIPPINCOTT: It was unstructured?

GELL-MANN: Yes. People would ask Enrico questions and he would answer them, if he knew the answer. Almost always he knew the answer, because he had solved almost all physics problems at one time or another and put down the answers in his notebook. He could usually find some calculation he had done in the past and respond—or come close to responding—to the question.

LIPPINCOTT: There were a lot of new particles being discovered at this time. And that's what you were doing?

GELL-MANN: Well, no. The particles actually had been discovered years before, but Caltech was finding more of them. None of them had been discovered at Caltech. Though in the earlier days they had been—for instance, [Carl] Anderson found the positron and then the muon. But now what happened was that other people—[George D.] Rochester and [Clifford C.] Butler, and [Louis] Leprince-Ringuet—had found new particles. And what Caltech did was to find the second through the thirtieth, or the second through the fiftieth, or the second through the fifth, or something, of these particles. And that was Anderson's lab. Some of them were called "hooks and forks." And he had a little note on his blackboard that said, "What have you done about 'hooks and forks' today?" Because every day he had to make some progress on hooks and forks. And he had all these students and young associates, like [Robert] Leighton and [Eugene] Cowan and so on, who helped him. So it was those particles that I thought about.

LIPPINCOTT: And you were thinking about strangeness there, at Chicago?

GELL-MANN: But that was not my principal activity. I was working on a lot of other things, but in one tiny corner of my mind I was worried about strange particles.

LIPPINCOTT: OK. We're in Chicago. And you worked on dispersion relations with Murph Goldberger?

GELL-MANN: Yes. And I worked with him on an explicitly relativistic form of field theory without closed loops, which we never did anything with. I think it might have been useful. But the theory we were working with—pseudoscalar meson theory—was not the correct theory of the world, so it really doesn't matter whether it would have worked or not. The theory was not a serious theory. The correct theory turned out to be the theory of quarks and gluons.

And I worked on the renormalization group. So there were a lot of things.

LIPPINCOTT: But there was an important paper, published in 1953.

GELL-MANN: It was a little tiny letter to the editor.

LIPPINCOTT: In *Physical Review*? I have the title: "Isotopic Spin and New Unstable Particles." [*Phys. Rev.* 92:3, 833-4 (1953)]

GELL-MANN: Yes. The original title was "Isotopic Spin and Strange Particles," but they wouldn't use the word "strange" or "curious." We had this long correspondence. They insisted that I call them "new unstable particles."

LIPPINCOTT: Wasn't this your first big impact on the world of physics?

GELL-MANN: I guess so. Well, people probably paid attention to that proof, or whatever we called it, of the Bethe-Salpeter equation.

Anyway, also in 1953, I returned to the University of Illinois for the summer—this time to work on elementary particle physics field theory with Francis Low. You remember that Francis Low and I were officemates at the institute, and I was now an assistant professor at Chicago and he was an assistant professor at Illinois. Illinois had offered me the job first, actually. But then Chicago met the offer, so I decided to stay at Chicago, because it was a big-time place. Enrico was there and all these other people. Later on, when Enrico died [1954], I wasn't so interested in staying on in Chicago. I didn't like Chicago very much.

But anyway, in ’53 I spent the summer in Urbana. The temperature reached 104 degrees day after day. But we found a laboratory, related to the laboratory where I had worked two years before, that was air-conditioned. And so we spent a lot of time visiting that lab and working in air-conditioned rooms. Then we would come back out to the hot office and think some more, and then go back into the cool office. I didn’t notice the temperature because we were so fascinated by what we were doing.

We were constructing the rules that governed what would happen to field theory at very, very high energies, or very high momenta. In the course of that, we invented what other people later called the renormalization group, which has been of enormous importance in physics, and more recently in other sciences, and is used very extensively here at Santa Fe Institute.

Around the same time—maybe a couple of months earlier—some less convincing work but related work was done by [André] Petermann and [Ernst C. G.] Stueckelberg in Switzerland. But nobody remembers today that we invented the renormalization group. Nobody credits us with it anymore—nor Petermann and Stueckelberg either, for that matter. It’s credited entirely to people who revived it ten, twenty years later. My student, Ken [Kenneth G.] Wilson, who got his PhD with me at Caltech—he never learned very much from me; he was extremely bright—Ken Wilson applied the renormalization group to condensed matter theory and critical phenomena in general. And that’s remembered. But that we invented it twelve years before—that is not remembered.

LIPPINCOTT: Was this because you didn’t publish?

GELL-MANN: No, we published it. [“Quantum Electrodynamics at Small Distances,” *Phys. Rev.* 95:5, 1300-12 (1954)] As usual, I publish late. Fourteen months later we published it, in late 1954, even though we had found it in July ’53. Because we delayed fourteen months, it came out a little bit later than this less convincing but related work by Petermann and Stueckelberg. But it was still twelve years or so ahead of all these other things. And I’m very proud of that work, the renormalization group. We had all those equations, which for some reason were later named after other people. We had the equation for the variation of the charge with momentum. And we showed how the various possibilities for that equation would lead to different situations. And for some reason it’s now called the Callan-Symanzik equation.

Anyway, that was some very interesting work.

In the meantime, as you mentioned, Murph Goldberger and I had done a number of things. But in particular, we worked on dispersion relations. And just after I got to Caltech [in 1955], I had the idea that you could use the dispersion relations and the unitarity relation to construct a version of quantum field theory on what we call the mass shell, in which the energy and momentum would obey the regular equation—energy squared minus momentum squared equals mass squared, where the velocity of light was equal to one. And instead of going off the shell—as in all the other work that had been done in expressing and describing field theory—we would stay on the shell all the time, but at the price of going to imaginary momenta, complex momenta, and so on. And this work is what led to what people call S-matrix theory.

But I didn't call it S-matrix theory. What I did was to give a talk in a Rochester meeting [Sixth Annual Rochester Conference, April 1956] not long after I got to Caltech, describing the possibility of getting field theory in this way, on the mass shell. I described it very modestly, in terms of perturbation theory. But it was obvious that since it was true to all orders of perturbation theory, it could be applied exactly, and it would give an exact alternative way of doing field theory on the mass shell. And then I mentioned, almost as a joke, that [Werner] Heisenberg had discussed how you might guess the S-matrix in this manner.

Then, five years later, Geoffrey Chew put forward this same idea, which Murph and I had been begging him to accept for five years, and called it S-matrix theory. Only he said. . . . It isn't that he didn't credit us—he said we had helped him think about it, and so on—but the point is, he denied that this was just a reformulation of field theory and said it was an entirely new approach to physics called S-matrix theory. And he credited Heisenberg with this, whereas, as far as I was concerned and many other people, it was just field theory redone in a different manner.

Well, this goes on to this day. And superstring theory, that brilliantly conceived candidate for a universal theory of all the particles and all the interactions, invented by John Schwarz and André Neveu in 1971 and then developed by John Schwarz in collaboration with a whole series of other collaborators—superstring theory is still formulated on the mass shell in the manner that we proposed back in 1954 and '55 and '56. And there's still hope that one can put it into ordinary field theory form someday. So this whole matter is still a very live issue now, so many years later—more than forty years later.

So again, that's work of which I am exceedingly proud. It opened up the way for this whole set of ideas. I contributed to it in other ways also, but always as a distant observer. I didn't invent superstring theory or anything like it. But all this was foreshadowed by things that were going on in '54 and '55 and '56. And when I spoke at Glasgow in the summer of 1954, I gave two talks. One of them was on strangeness, and the idea of strangeness—possible schemes you could construct around strangeness. And the other was on the work with Murph on dispersion relations on the mass shell. But the idea of getting the whole of field theory out of dispersion relations and unitarity and so on—crossing relations, the unitarity relation, and the dispersion relations, all things on which I'd worked with Murph—that idea came after I had left Chicago.

I stayed at Chicago until the late summer of '54, and then I took a term off to go to Columbia, where they made me a visiting associate professor and offered me a tenured associate professorship. And Chicago by that time was offering me tenure also.

LIPPINCOTT: Was Columbia tempting?

GELL-MANN: I really didn't like the idea of living in New York. This was just around my twenty-fifth birthday.

LIPPINCOTT: You got married around this time.

GELL-MANN: No. I didn't get married until 1955. But I had already fallen in love with Margaret [Dow].

LIPPINCOTT: Where did you meet her?

GELL-MANN: Well, I had seen her briefly at Princeton. This was when I was at Chicago but I was visiting the institute to talk about some work, and I met her there in the spring of '54, briefly. But then the way we really met was in the summer of '54. I was going to the conference in Glasgow, and after the conference I would be going to the Isle of Skye with Roger Hildebrand, a colleague at Chicago, and then coming back to Glasgow and spending some time poking around in Scotland.

What happened was that as I was leaving for London, I was at LaGuardia field. I phoned up Princeton to see if I could reach Margaret, and I was told, “No, she’s gone. You can’t talk to her. There’s no way to reach her.” And so on and so on and so on. Finally I got through to Gwen Groves, General [Leslie] Groves’s daughter, who was her best friend. Gwen and she had very similar jobs. They were both assistants to classics professors—in Margaret’s case, an old lady archeologist; a classics professor in the case of Gwen. They saw a lot of each other; they were inseparable.

So when Gwen got on the phone, I figured I’d finally find out where Margaret was. And Gwen said, “Well, I’m not supposed to tell you. But actually, she’s left for LaGuardia field to fly to London, because her mother is very, very ill and probably dying of cancer. She’s in a big hurry to get to London, and there’s no way you can reach her.”

I said, “Thank you.” I went to every airline and looked at the passenger manifest—which of course you can’t do today. And I found her on TWA. She was expected, so I left her a little note, and I said, “I’m getting into London a little before you. I’ll meet you.”

And I met her. And we went to a phone immediately, and she phoned Royal Leamington Spa. Her mother was already dead.

It was a very weird, old-fashioned family. And of course I was familiar with that kind of nonsense—not talking about things. They never mentioned cancer, and they never told her mother that she was fatally ill. And they didn’t send for Margaret, because that would have conveyed to the mother that she was very ill. They waited until it was too late. It was very Victorian.

So I fed her some bad ravioli or something in the airport restaurant. And I saw her to her train at Paddington Station. We said goodbye. And I was hoping to see her again.

Just as she left for the train, I said, “You know, I’m going to be in Scotland. And it would be so nice if you could come and join me in Scotland. We could do something together—look for birds, sea birds.” Because when I met her in Princeton, I had told her that I was going to go look for puffins. I desperately wanted to see an Atlantic puffin. And every girl I had met before that would have said, “What’s a puffin?” Whereas what Margaret did was to draw me a beautiful little sketch of a puffin standing on a rock ledge, looking out at the sea. And I still have it.

So now I said, as she was leaving on the train to go join her father and brother in mourning, "Why don't you come to Scotland for a little while, and we'll go look for puffins together."

Well, I went to Skye with Roger. And then, after a couple of days, in my little inn on the Isle of Skye, one morning I was at breakfast. And the servant brought in a silver salver with a little tiny letter on it. It was from Margaret, and she said, "Having spent a week with my father and brother mourning, your invitation to Scotland sounds better and better. [Laughter] Can I meet you in Glasgow on such and such a day?" So naturally I called up and said "Yes, fine! And I'll meet you at dawn when your train arrives in Glasgow." So she arrived, and I fed her some milk and a Toblerone, which she had never had. I took her to my hotel and let her sleep for an hour or two. And then we caught another train, and we left for Stranraer, in Galloway. And there we were going to see a puffin, but it didn't work out. However, we had some amusing adventures in Stranraer. Finally, we decided to go somewhere else, and so we went up to the Isle of Mull. Did you see the film called *I Know Where I'm Going*? Well, we weren't very far from Pennycross House, where they made that film. I loved that film. Well, we were right near there. We couldn't stay there, because it's a private house first of all, and second, there's no road to it. But we were nearby, and we went to Iona. It's a long, very romantic story.

It was Glasgow Fair time, which is not a fair at all. It's two weeks in August when everyone in Glasgow goes somewhere else. The city empties. And this was when I was trying to find a hotel room on the Isle of Mull. We were in Oban, where the train had come in, and we were about to take the ferry to the Isle of Mull, but we had no place to go on the Isle of Mull. So I was calling all these places, and everybody said, "No, it's Glasgow Fair holiday and you haven't got a chance." But finally somebody said, "You know, there's a place called the Argyll Arms in Bunessan." And I thought, "Well, all right, I'll try that." So I went to the tourist bureau person where I was phoning, and I said, "Have you heard of the Argyll Arms?" She said, "Oh, yes. In Bunessan. But you'll not like it; it's not attractive." So I said, "That's all right. If it's not attractive, it's OK, because there's nothing else. What's the phone number?" And I looked in the phone book, and she said, "It'll not be in there." Finally, some lady who was walking by took pity on me and said, "You know, if it's called the Argyll Arms, it'll be listed under 'Campbell.'" Because the Duke of Argyll is the head of the Campbell clan. The only reason anybody would call a pub Argyll Arms is because the publican is named Campbell. So I looked up "Campbell" in Bunessan in the phone book, and I called up. And sure enough, they had two rooms. So we boarded the ferry. And when we got there, to this hideously unattractive pub, it was the cutest thing you could possibly imagine. [Laughter] A little tiny pub by a loch called Loch na Làthaich—not a sea loch but an internal one. We heard the corncrake calling from the swamp. I'd never heard a corncrake before. I'd always imagined myself listening to a corncrake calling from the swamp. And Margaret had the most delightful little tiny room under the eaves. And there was a pretty teenager called Morag, who told us everything about the whole area. There were almost no people in Bunessan; they'd all left for somewhere else. The place was completely deserted. The islands and the highlands were depopulated; everybody had gone somewhere else for jobs. For the few people who remained, it was incredibly romantic. This was a look back into a much earlier time.

And we went over to Iona, and on other sea trips. We went to all sorts of other places together. We saw Erraid, the beach in Robert Louis Stevenson's *Kidnapped*. And Rona, the port of the seals. And we saw Staffa, where Fingal's Cave is. It was just marvelous. We didn't actually get to see Fingal's Cave—the weather was too bad. But we saw Staffa in the distance.

LIPPINCOTT: And that was the summer of '54?

GELL-MANN: '54, yes. So then, when I got back and went to Columbia, I kept calling Princeton when Margaret was due to come back, saying, "Well, has she come back? Did she show up?" She had visa problems. Finally she came back, in very late October. And by November 11 we were engaged—the same day her father died. He died very shortly. He didn't have anybody to quarrel with. For several months he hadn't had anybody to quarrel with, and it killed him. The two of them were constantly battling, fighting. On horrible terms! But they loved each other besides. He couldn't live without her.

LIPPINCOTT: Where were you married?

GELL-MANN: Right there in Princeton—in the house where Margaret had a room. She had a room with the people who ran the cafeteria—Dick and Mary Sleet—and she worked in the cafeteria to supplement her income. I don't think she needed the money. I think it was just that she wanted to do a favor for her friends who owned the house where she had a room. Since they ran the cafeteria, she served as a cashier in the cafeteria just for fun. And I realized when I first met her that I had seen her there a year earlier, in the cafeteria, and wondered who this lovely young lady was. Because I had visited Princeton in the fall of '53, and I had seen her this way, from a great distance. Margaret's hair was ash blonde, dirty blonde, honey blonde—whatever you call it—and she had grayish blue eyes. She had a short haircut; she looked like Audrey Hepburn.

So I was at Columbia when we got engaged, and then I asked Chicago if I could have another term's leave, so I could be in Princeton in the spring, so I could be with Margaret. And they said, "Sure, go to the Institute for Advanced Study." The Institute for Advanced Study was very happy to have me back. So I spent the spring term at the Institute for Advanced Study, and on Margaret's twenty-fourth birthday, April 19th, we were married in the living room of the Sleets' house. We left from the wedding for a night in New York and then set off across the country for Caltech. So we never really had a honeymoon.

Because Caltech had by now made me an offer. Margaret and I had gone out there together in December. Then she went back to Princeton and I stayed until after New Year's and gave some lectures there. [Richard] Feynman was our host; he showed us around. We went down to Palomar. I said I wanted to see Palomar, so he drove us down to Palomar.

LIPPINCOTT: Had you known Feynman before this?

GELL-MANN: Yes, I'd met him. I met him at a meeting, a couple of meetings. And in Chicago we had had some money for inviting a visitor, and I told Fermi we should invite Feynman. He said, "That's a great idea," and he invited Richard, who spent ten days or so. We used up all our money on him, and it was great.

They also offered me a job at Berkeley. But Berkeley was in the midst of this horrible situation with the oath. I didn't really feel like going to Berkeley.

LIPPINCOTT: What is the oath?

GELL-MANN: You never heard of the loyalty-oath controversy at Berkeley?

LIPPINCOTT: Oh, the loyalty oath—McCarthy.

GELL-MANN: Well, it was McCarthy times; 1954 was the year of the Army-McCarthy hearings, which I watched in Chicago before I left. And it was the year when Oppenheimer lost his clearance—the day his clearance expired, they took it away.

Berkeley wanted to look me over. But I wasn't terribly interested in Berkeley. I thought, Wow, it would be nicer to go to Caltech! I didn't realize all the things I wouldn't like about Caltech; I just thought about the things I would like. It was a very exciting place right then. They were doing all this work on the strange particles. Dick [Feynman] was there, and I thought Dick was not like these bogus people—like Schwinger, and so on. He was genuine. I just thought of Caltech as a terrific place where the people got on with one another. You didn't have a batch of fascists, the way you did at Berkeley, who were on bad terms with the people who weren't fascists. The political climate seemed to be good. The theoretical physics intellectual climate seemed to be good, if it was mainly Feynman, because I admired him. And the experimental physics sounded great. Margaret was not very happy about the idea, because she thought it was hot and not at all like England.

MURRAY GELL-MANN

SESSION 2

July 18, 1997

Begin Tape 2, Side 1

LIPPINCOTT: We're at December 1954. You're visiting Caltech.

GELL-MANN: Late December 1954. Margaret and I were engaged at that time—not yet married. We visited Caltech, and Dick Feynman showed us around—took us to Palomar at our request and to various amusing things. Then we drove up the coast to Berkeley, where I was to go to a meeting, and where they were interested in offering me a job.

Then Margaret flew home to Princeton, and I went back down to Pasadena just after New Year's, and I gave some seminars and a colloquium at Caltech. Dick was very impressed, because he hadn't met anyone before who had discovered results that he hadn't known about—in quantum mechanics and in field theory. So we got on very well. We discussed all these things. He suggested some further refinements, actually, on what I was doing.

LIPPINCOTT: Did he talk to you about coming to Caltech?

GELL-MANN: Well, I happened to mention at some point that I was looking for a job somewhere. I was planning to move from Chicago. I didn't want Margaret to have to live in Chicago. And Fermi, who was the principal attraction, had just died. So apparently Dick acted on that information and went to see Bob [Robert F.] Bacher, who was the chairman of the physics division. And in the few days involved, they cooked up an offer—something that couldn't be done now, of course. There are all sorts of federal laws and various practices that impede it. It would take months, and maybe a year. But then, by the time I left, they had an offer—to be an associate professor with tenure at Caltech, and at quite a decent salary.

LIPPINCOTT: You had tenure right away?

GELL-MANN: Well, I would have had tenure in any case. At Columbia. And Chicago had promoted me to associate professor with tenure. It was just a question of where I wanted to go—Berkeley, or Caltech, or Columbia, or stay in Chicago.

LIPPINCOTT: And you said that the political atmosphere at Berkeley wasn't to your liking?

GELL-MANN: Well, there was a lot of conflict still, in the aftermath of the oath controversy. And there were a number of very right-wing people involved—and a number of people who were not like that. Many people had resigned and gone elsewhere. Or they had left under pressure and sued the university to get their jobs back and get back pay. And later on, a group of them won this lawsuit, actually. But I didn't want to go into that kind of situation.

Caltech I viewed as a very friendly place, because I thought the people were all friendly to one another. There were no big political conflicts—in the sense of national politics—as there were at Berkeley.

LIPPINCOTT: Did you know [Caltech president Lee A.] DuBridge at the time?

GELL-MANN: I met him in the course of these visits. And he was, of course, a delightful person. So that seemed very good. And Caltech was the center of experimental work connected with the sort of theoretical work I was doing. I didn't realize that that wouldn't be true for very long.

LIPPINCOTT: Why wasn't it true? Are you talking about [Carl] Anderson?

GELL-MANN: Well, Anderson's group was producing these sensational results, but that didn't continue for very long. And then it was no longer the center of research. Research was being done with accelerators and no longer with cosmic rays. And the synchrotron, which was producing very important results, was a low-energy machine. It was upgraded a couple of times, but, again, it never became a really important frontier instrument.

LIPPINCOTT: So, CERN [European Council for Nuclear Research] and SLAC [Stanford Linear Accelerator Center] got to be the two big—

GELL-MANN: Right. And I can say, jumping ahead, that the following year, when I became a full professor, I suggested repeatedly to the meeting of full professors that we immediately start a visiting team that would go to one of the great accelerators, work there, and accomplish frontier research in elementary particle physics. And they pooh-poohed it. Willie [William A.] Fowler, with whom I almost never agreed on anything, said, "What would be the point of that? How silly! Why don't we have an accelerator here?"

Well, they made a feeble attempt, actually, to get the next big machine in Southern California. But it was not successful.

LIPPINCOTT: Did they actually propose it?

GELL-MANN: Yes. It was proposed at one point; they were talking about it. They discussed some sites, and so on. But it didn't work out. I figured that what was practical, really practical, and didn't depend on a long-shot attempt to snag the world accelerator for Southern California, was to build a visiting team. Which, of course, universities did at that time. And Caltech finally ended up doing it many years later—the work of Frank Sciulli, Barry Barish, and so on. But anyway, when I suggested it in 1956, it was not regarded as an intelligent suggestion by my colleagues.

We'll return now to January '55. Just after New Year's, I gave some talks, and they cranked up an offer, which after a while I accepted.

LIPPINCOTT: Did you and Margaret drive across country?

GELL-MANN: We drove, yes. And a few days later we arrived in Pasadena.

LIPPINCOTT: Where did you live when you first arrived?

GELL-MANN: Well, first I called John Pelham, whom I had met at New Year's, and he suggested a motel on Huntington Drive, near Rosemead—in Arcadia or some neighboring community. We stayed there a few nights, and then we found an apartment in a ridiculous place called Fireside Manor Lanai—the combination of this English-sounding name with this Hawaiian word at the end was so ridiculous. It appealed to Margaret's English sense of humor, of course. It was in

South Pasadena, on Raymond Hill. And it was one of many such rather silly places over there—full of these young people jumping from the lanai into the swimming pool, making a huge amount of noise, and drinking a great deal. [Laughter] And there we set up housekeeping. Margaret cooked meals for us. And we explored the area, which was quite discouraging, with the smog and the miserable slums and so on. We weren't very pleased with it. And then after a month, we broke the lease and went off to Europe.

LIPPINCOTT: When were you to start teaching?

GELL-MANN: In the fall. We had arrived in April—or probably early May—and after a month or so, school was out. And we went off to Europe. The woman who ran this silly Fireside Manor Lanai said, "Europe. I don't understand why anyone goes to Europe. There isn't anything they have in Europe that we don't have right here in Southern California." Margaret loved that. [Laughter]

LIPPINCOTT: Where did you go in Europe?

GELL-MANN: We went to Copenhagen, to the Bohr Institute. Niels Bohr wasn't there, and Aage Bohr wasn't there. But there were some interesting people—it was interesting to be there.

LIPPINCOTT: Was that your first visit there?

GELL-MANN: Yes. It's my only visit there—well, except for a couple of days in later years. It was the only extended visit. I was there for several weeks, and I practiced my Danish on people. Speaking Danish is difficult, because Danes are not accustomed to foreigners speaking their language. And they have a very narrow band pass. You have to get it very accurate, or they don't realize you're speaking Danish at all. Eventually I got through and I could talk with people. But it's not easy. It's a funny language. They like to say about the pronunciation that if Dutch is a throat disease, then Danish is a stomach disease.

LIPPINCOTT: Were they still *schwindlig* there?

GELL-MANN: Were they still what?

LIPPINCOTT: It means "dizzy."

GELL-MANN: *Svimmel* is "dizzy" in Danish.

LIPPINCOTT: But didn't Niels Bohr say that if quantum theory didn't make you *schwindlig*, you didn't understand it?

GELL-MANN: *Svimmel*. *Svimmel*. In Danish, what he said was [quotes Bohr in Danish], which means, "If one believes one can think about quantum physics without becoming dizzy, that shows only that one has not understood anything whatever about it." And what [UCSB theoretical physicist] Jim Hartle and I would like to do is to change that situation. That's our ambition. So that you can think about quantum physics without becoming dizzy. That's what we would like to accomplish.

LIPPINCOTT: Do you think that's possible?

GELL-MANN: Yes. I think we've accomplished a great part of it already.

LIPPINCOTT: So you came back to Caltech in the fall.

GELL-MANN: Well, let's continue with what we did during that summer. We were in Denmark, and then we went down to France. And then we went to the Pyrenees, and we stayed there for a while. We went over into Spain. That was difficult, because Franco's government was angry at Britain over Gibraltar, so British people needed visas and special permission, and so on, to go to Spain. So when we drove up to the frontier post, which was at Roncesvalles, the place where Charlemagne and his army were attacked by the Basques and where Roland died—what a romantic place!—the frontier post's guard said, "Well, you know, it's not really legal. You have a British passport, we can't really let you in." I said, "We're only going to lunch at Pamplona and coming back." And he said, "Well, all right. I'll hold on to the passport here, and then you come and pick it up later."

So we went to Pamplona. We saw some new birds in the Pyrenees. We came back from lunch. It was a lot of fun, very nice. But when we got to the frontier post, there was somebody else there. And he said, "Well, this is not legal. This is an infraction." And I said, "Well, I don't know what to do about it. But we certainly can't go into some lengthy thing; we have to get home tonight to our French hotel. So if there's a fine or something, can I just pay it on the spot?" So he said, "Yes. I think we can do that. Five pesetas." So for twelve and a half cents I bought us out of this illegal situation. We got back the passport. [Laughter] And we drove back into France. It was an interesting summer.

Unfortunately, we had a car crash. The brakes failed in the rain in France, and our little Hillman Minx got badly smashed up.

LIPPINCOTT: Were you hurt?

GELL-MANN: Margaret was slightly hurt. She was in the death seat, of course, and her knee was banged a little bit. And she had some windshield in her forehead. But it wasn't serious. She recovered completely.

So we had to drive this wreck back to England, and leave it there to be repaired and then shipped over to the United States. Then we came home, and I started teaching. And I believe that for the first few years, I taught the quantum mechanics course.

LIPPINCOTT: Yes, I have it here: "Principles of Quantum Mechanics."

GELL-MANN: And I've met many people who remember my quantum mechanics course. They rather liked it, apparently. Although I wasn't very satisfied with my teaching. I thought it was very sloppy and not very good. But maybe the sloppiness itself contributed something to the course, as it did with my teacher Viki Weisskopf. I don't know.

LIPPINCOTT: Well, maybe the students couldn't tell it was sloppy.

GELL-MANN: Well, no, they could tell. But I think what happened was, they could see my thought processes. I was exposing my thought processes—just what I said Julian [Schwinger] never did—and they liked that. So there are a number of people who've told me they got quite a bit out of that course—although I doubted it at the time.

LIPPINCOTT: You also taught a seminar on theoretical physics.

GELL-MANN: Did I do that then? Or later on?

LIPPINCOTT: Yes. The very first year you were there. I looked it up in the catalog. Physics 205 all three terms, and Physics 238 all three terms. So you were busy.

GELL-MANN: I don't believe I actually did that. I was probably listed as doing it. I doubt if I actually did. It doesn't sound like me. But catalogs never lie. So perhaps I really did it.

I had an office on the second floor of Bridge [Norman Bridge Laboratory of Physics]. Which is where Robert Oppenheimer had been, he told me. He said that at one point he had had that office. He used to come down from Berkeley for the spring term and bring all his students in his jalopy. Having a jalopy was a sign of great wealth, of course, during the Depression.

Robert was a great friend of Charlie Lauritsen, who built that 1MEV accelerator, which was a huge accelerator at that time. They used it for medical purposes, and Stewart Harrison, a local radiologist from England, supervised the medical aspects in the use of the accelerator—this was in the late 1930s. And Stewart Harrison married Kitty Puening, who had been married to an American Communist [Joe Dallet] who fought in the Spanish Civil War. She was then married to Stewart, who had I think some left-wing leanings of his own. And then she and Oppenheimer met at a party, because Oppenheimer, Stewart, and Charlie were all involved in this 1MEV accelerator—Robert doing some theory and Charlie being responsible for the accelerator itself and the experiments, and Stewart for the medical applications. And apparently they fell in love at first sight, and Robert stole her away from Stewart. He was her third husband, and she was Robert's only wife.

Kitty—although I liked her very much—could not have been a very successful wife, because she was alcoholic and moody. But she was a lot of fun.

LIPPINCOTT: Well, they liked martinis.

GELL-MANN: Yes, that's right, they liked martinis. Well, they had very rigid ideas about martinis. Martinis must not be shaken or the gin would be bruised. And it was extremely important to make them with a very high ratio of gin to vermouth, and then refrigerate them. Never, never put them in a cocktail shaker. That was a thing only barbarians did. Anyone barbarous enough to do that would never be invited to the home of the Oppenheimers. So they were very rigid about martinis.

Those are things I learned at Princeton, of course, rather than at Caltech. But at Caltech I got to know Stewart and his next wife, Helen Harris, who was a delightful woman. Stewart gained enormously, I think, by having Kitty stolen away from him and marrying Helen. [Laughter]

LIPPINCOTT: Did you know Roger Sperry [Hixson Professor of Psychobiology] at that time?

GELL-MANN: I never knew Roger Sperry very well. Looking back, I realize that I should have known him a lot better, because he was doing these fascinating things, which I heard about—but I didn't know him personally. Very late in his life, when I did have the opportunity to know him, he was off on some rather strange tracks.

LIPPINCOTT: He became religious?

GELL-MANN: Sort of. I don't know if it was exactly religious—but mystical, anyway. I don't know about that part, but earlier on he was doing marvelous stuff.

Anyway, when we returned from Europe that September, we rented a house in Monrovia on Hidden Valley Road—a house belonging to a woman who I guess had just been divorced. And we got to know the neighbors there. We made friends among the neighbors—friendships that lasted for years. Of course, we were very far from Caltech. The commute was long.

LIPPINCOTT: Yes. Why so far?

GELL-MANN: Well, it was just easier to get a good place cheap, far away. And younger people, in general, tended to live very far away, because that's where they could get a respectable place for their money. And I liked being out in the boondocks, where there was some wildlife and so on. There was even a mountain lion. We could hear it screaming sometimes, late at night. I went out at night sometimes to try to find it, but we never did. But our next-door neighbors were returning home very late from a party once, when a dark-colored dog ran ahead of them up Hidden Valley Road. And when it got to an oak tree, it climbed the oak tree—walked right up the bark—something dogs don't do very often. They concluded that it was a mountain lion. [Laughter]

LIPPINCOTT: Did you teach undergraduates [at Caltech]?

GELL-MANN: There were some undergraduates in the quantum mechanics course. I never taught a specifically undergraduate course.

LIPPINCOTT: Did you find them smart?

GELL-MANN: Not particularly. I've never shared this notion. I mean, there are a lot of bright kids at Caltech, but I never shared this notion that somehow it was an inspiring experience to teach Caltech students and teaching students elsewhere was uninspiring. For example, I teach now at the University of New Mexico, and I suppose the number of very bright students is smaller—it must be. But I don't notice any tremendous difference. And for some reason, I enjoy it more. Maybe that's because Marcia and I drive down together and drive back together and so on.

I never believed much in classroom teaching. I think it's silly. What I do—and what I did later at Caltech—really doesn't belong in that category. What I did was to try to present new results—a sort of seminar—discussing things with the students, wrestling with new problems in front of them, making mistakes, coming back the next time and saying, "Well, we could do it differently; let's improve on what we said last time"—that sort of thing. That's useful. Teaching a course on stuff that's already known I find just stupid.

LIPPINCOTT: But somebody has to do it.

GELL-MANN: No, nobody has to do it. It's a completely unnecessary process. It stems from the practice in the Middle Ages of publishing books in a scriptorium, where the lector—the reader—

would read the manuscript of the theology text, or whatever it was, and a room full of *scriptores* would copy it. That was publishing. Then, at the universities, the students, who were mostly very poor, would study the same theology text. And they were too poor to buy copies produced by this scriptorium method, which was very expensive, so the professor would be his own lector and would read his notes out, and the students would copy them down. And that way they would have copies of the theology text. We're still doing that today, despite the fact that printing came to Europe in the fifteenth century. And there's absolutely no point in it. The students can read textbooks. If they get stuck, they can come to the professor in some sort of tutorial session, maybe even in a group, and ask questions about what they're stuck on. The professor can do other tasks for which a human is important, like suggesting ways of thinking, suggesting problems, suggesting reading, and so on. But why the professor has to write out on the blackboard all this stuff that's in all these books, I don't know. Think of a typical Monday morning, say, on the East Coast of the United States, where in, say, two hundred colleges and universities somebody is starting to write Maxwell's equations on the board. [Laughter] It's so stupid.

In addition to the books, one could have films of the very best teachers—say, a half dozen of the very best teachers in the country, or in the world, doing this. In case students can't read, or for some reason want. . . . Some people want an aural rather than a purely visual presentation. OK, fine, we can do that with films. But what on Earth is the point of hiring these people all doing the same thing on Monday morning in all these different places—some of them better, some of them worse? It's just silly! As long as it's known and it's in books and it's written down and it's codified, what on Earth is the point of teaching it by this method? The students should be weaned from that kind of thing when they're ten or eleven—twelve, maybe. There's no need for it. And then in American schools, of course, they're told to read pages so-and-so and there will be a quiz. In Europe, they get over that at a fairly early age. Margaret was appalled at the idea that adult students were given quizzes and homework assignments. In European universities, there are lectures, but they are a cultural seasoning for the course, which consists of having the student read, study, do problems, and so on, and consult with the teacher when it's done. It makes so much more sense. Anyway, I think courses are just plain stupid.

LIPPINCOTT: How about the intellectual climate at Caltech? You became a little bit disabused?

GELL-MANN: Well, all the time I was there—I was there for thirty-seven years, not including my terminal leave from 1992 to 1993—and during most of that time I begged Caltech to get involved in behavioral and social sciences, to get involved in archeology and linguistics and psychology and evolutionary biology even. I like to say that given the amount of evolutionary biology at Caltech, it might as well be Bob Jones University. There is a clever geologist—I mention him in my book, a very interesting man with a Japanese wife. What's his name . . . [Joseph] Kirschvink—cherry finch! He's in the geology division and he studies evolutionary biology and the evolutionary record, but very few Caltech people do.

LIPPINCOTT: Well, they did go in those directions a little bit later.

GELL-MANN: Well, after I left, all sorts of things may have happened. In fact, the president [Thomas E. Everhart, Caltech president 1987-1997] wrote me and said, "Gosh, these complaints you have about Caltech—maybe we should do something about them." But I had to leave in order for that to happen.

LIPPINCOTT: Roger Sperry being the exception, I guess.

GELL-MANN: Roger Sperry was an exception, very much so. But, you know, they never really replaced him. They got another psychologist at one point, but he died—he drowned. A very interesting man; his son was in my daughter's class in school. But he died shortly afterward, and they never replaced him. Now, of course, things are a little better. Christof Koch [Troendle Professor of Cognitive & Behavioral Biology] is trying to work on psychology.

LIPPINCOTT: And [biophysicist John] Hopfield was there for a while. But now he's gone.

GELL-MANN: I didn't pay much attention to John's work. I mean real psychology. [Laughter]

LIPPINCOTT: Not neurobiology?

GELL-MANN: Well, it wasn't even neurobiology. It was some sort of attempt to impose ideas from solid-state physics on neurobiology. And the simplicities of solid-state physics didn't actually apply to neurobiology.

LIPPINCOTT: Well, we could get way far afield talking about that. We could talk about [British mathematical physicist Roger] Penrose and his strange ideas about—

GELL-MANN: Oh, that's different! John Hopfield is a serious scientist, and he's done extremely good work. I just felt that it was an inadequate approach to the brain and the mind to rely exclusively on the work that Hopfield and his associates were doing—sort of a narrow approach and not sufficient. But it was serious scientific work. He's a really serious scientist. John has accomplished a great many good things. He's certainly not a phony. Roger Penrose, when he abandoned cosmology to work on these questions, turned into a pure crank. It's completely different!

LIPPINCOTT: OK, let's get off Penrose and go back to the fifties and what was going on in physics at Caltech then.

GELL-MANN: Well, my work at RAND in 1956 gave rise to some actual papers. The RAND physics people were trying to extend the Fermi-Thomas model of atoms, an approximate description of what happens to the electrons in heavy atoms. It's called

What happened was, I looked at this problem of the next term in the description of the electrons after free electrons. And then the term after that. And the term after that was called the correlation energy. There were many separate contributions to the correlation energy, but some of them diverged. So it was necessary to look at higher and higher approximations and sum all these terms in order to get something converging, which would contain the log of the density. In other words, you couldn't just expand in powers of the density parameter—the cube root of the density. You had to include some logarithms. And the way these logarithms appeared was that if you tried to make a pure power series, you got infinity, logarithmic infinity. But then if you summed up a lot of infinite terms, they gave a finite result.

I was very familiar with this from ordinary field theory. And here there was some sort of an analog of field theory. So I worked out a way of using Feynman diagrams, as if in field theory, for this problem and summing up the divergent terms to get an answer. But I was stuck on a technical point. And Keith Brueckner came through for a day as a consultant and got me unstuck on this technical point. So I included his name on the paper and sent it in ["Correlation Energy of an Electron Gas at High Density," *Phys. Rev.* 106:2, 364-8 (1957)]. And it was quite funny, because Brueckner and a Japanese called [Tatsuro] Sawada, who was working with him, wrote an attack on our paper shortly afterward, saying that we had omitted a very important term—the plasmon term. So I went back to RAND, and Bill Karzas and I studied the problem and looked at the propagator we had written down—because we had constructed these fake Feynman diagrams for the problem. Later on, these were called Goldstone diagrams, because Jeffrey Goldstone wrote a long treatise on how to do this. But my student Don DuBois did the same thing and calculated the higher corrections—probably earlier than Goldstone, but they're named after Goldstone.

Anyway, we noticed in this propagator that there was a pole for the plasmon, which meant that we had included the plasmon in the calculation. Brueckner and Sawada were counting twice—they were adding in a plasmon contribution to a term that already included the plasmon contribution. So we sent a letter to them. But I had envisaged, as I say in this paper that I'm going to give you now ["Reminiscences," *Philosophical Magazine B* 74:5, 431-4 (1996)], a nightmarish correspondence in the columns of *Physical Review* between Gell-Mann and Brueckner and Brueckner and Sawada, attacking each other. [Laughter] One of the authors being the same person. [Laughter]

LIPPINCOTT: That would have been fun!

GELL-MANN: Anyway, that was something interesting in 1956. And Don DuBois, who was my student, worked out the whole diagram business and calculated the next term. He then became a leading staff member at Los Alamos. He's now retired, and he has the office across the hall from me at Los Alamos.

LIPPINCOTT: I didn't know you had an office at Los Alamos.

GELL-MANN: Oh, yes. And right across the hall is Don DuBois.

In the meantime, my daughter Lisa was born, in 1956, and I was thinking a lot about her. She was a very interesting little child—very bright, and pretty, and a pleasure to be around.

LIPPINCOTT: Did you do any work at that time with Feynman on the weak interaction?

GELL-MANN: He and I worked together on a lot of things, and subsequently we did work on that subject. But what you're referring to is different. That was just a joint publication. What happened was this . . . **[Tape ends]**

Begin Tape 2, Side 2

GELL-MANN: Let me tell you what happened. I've written about it many times, and it works like this. In 1956 and '57, when it was proposed and confirmed that parity was violated in weak interactions, an attractive theoretical model became available—which I had been toying with, actually, for a number of years. And that was a vector and axial vector picture of the weak interactions, where there was just a single current, with vector plus axial vector terms. The interaction would then be vector plus axial vector times vector plus axial vector. And the cross terms would violate parity, while the square terms would preserve parity. Since parity was known to be violated, this was an acceptable scheme.

But experiments in nuclei on beta decay, sponsored by Mrs. [Chien-Shiung] Wu at Columbia and done by [Brice] Rustad and [Stanley] Ruby, and various other experiments, suggested that this vector/axial-vector interaction was wrong and that the actual interaction was scalar and tensor, which made it a much uglier theory.

Well, in 1957, Art [Arthur H.] Rosenfeld and I were working at Caltech, writing a review article on the weak interaction for *Annual Review of Nuclear Science*. And I began to discuss with him how this was an attractive idea and maybe the experiments were wrong. It made for a universal Fermi interaction, and one that was perfectly compatible with an intermediate boson of spin one, which would carry the interaction. It was very appealing, but we were still worried about all these experiments that contradicted this hypothesis.

At that time, Robert Marshak and his student, George Sudarshan, came to visit. Art and I went to meet with them, and I think with Felix Boehm, at RAND, in Santa Monica. We had lunch together. And they told us about their work, in which they had figured out that these experiments could really be wrong—that they could be criticized and perhaps were actually wrong—lending a lot of strength to this very beautiful, fantastically simple theory of the weak interations. I liked that, and I liked the strengthening of confidence that this might work.

Art and I had written a section in our *Annual Review* called "The Last Stand of the Universal Fermi Interaction," in which we described how, if all these experiments really were wrong, we could have this beautiful theory. They asked whether we were going to publish any more on it. And I said, "Well, I don't think so. I don't suppose we will. This is what we're going to say, and it says most of it." We always took this very modest approach, and this delaying approach, to publication. I don't know why. [Sighs]

So they started writing something also. They started writing on their ideas, which went further, in the sense that they criticized some of these experiments and showed how they might be wrong.

Then I went away with Margaret on a vacation in Northern California. We were completely out of range of any communication, off in a tent somewhere in the Siskiyou Mountains. When I came back, Richard Feynman had returned from Brazil. And Felix Boehm had told him that I thought the interaction was vector and axial vector. Whereupon Richard said, "Oh, my God! If that's true, then you can have this beautiful theory. I have just thought of this fantastically beautiful theory!" And then he wrote up this fantastically beautiful theory and gave some rather weird reason for it to be true—some kind of second-order equation, or I don't know what it was, some strange point of view that he adopted. He always liked to adopt an eccentric point of view in everything, and here he had some weird reason why this theory was preferable. It actually didn't mean much.

He wrote up an article and was about to send it off for publication when I came back from vacation. And I was horrified, because it seemed so absurd for him to assume that I had this idea of vector and axial vector and hadn't realized the rest of it. He didn't know about Marshak and Sudarshan. So I showed him the little paragraph that Art Rosenfeld and I had written, and of course it was in a totally different spirit from Feynman's. What we had written was: "Look. If these experiments are really wrong, then this beautiful theory could be correct.

This is the last stand of the universal Fermi interaction. The numbers come out right, and everything is really beautiful." Whereas his said, "I have discovered this fantastic theory! And this beautiful theory, which I have just discovered, says that such-and-such. And furthermore, it's the only really good theory because of this . . ." and then he presented this rather weird justification.

So I said, "Well, God, if you're doing that, I have to write up our work, because we had this idea before, and not from this point of view." And then Bob Bacher said, "Well, I don't think it's such a great idea for both of you to write papers on the same thing from the same institution. Why don't you collaborate on one paper?"

Well, this was very tricky, because Feynman's rationale—which I found utterly unconvincing, and still do—was the central part of what he had written, and to undo it was very difficult. So it stayed in there, and I felt very unhappy about that. I still feel very unhappy about it.

So I contributed a lot of other ideas, about how to extend the theory to other parts of the world—strange particles and all sorts of other things. But this was done in haste, and I really would have liked more time to think about those things and get them right. Anyway, we published it together ["Theory of the Fermi Interaction" (with R. P. Feynman), *Phys. Rev.* 109:1, 193-8 (1958)], and it came out in a book in January '58, or February. But in December '57 there was a very important international meeting in Stanford, and we both went there, and we both gave talks, back to back. Richard talked about the theory itself, and I talked about implications and things going beyond the theory. And it's a pity that my talk wasn't written down, because I included a lot of very interesting stuff. Such as that the intermediate boson, if there was an intermediate boson—which seemed very much indicated by this theory, a spin-one intermediate boson—if there really was one, then the muon would decay into electron plus gamma at a rate that was experimentally impossible. Unless there were two kinds of neutrinos, which we called "red" and "blue." And this Feynman and I had discussed, but when I wanted to write it up he for some reason withdrew his collaboration on it. He didn't like the red and blue neutrinos. And I got discouraged and didn't write it up myself—which is a pity, because it's true. And it's credited to [Chen Ning] Yang and [Tsung Dao] Lee, who worked on it years later. And this calculation of the *mu* into *e* plus *gamma* is credited to Gerald Feinberg, who also worked on it a little bit later.

Anyway, that was a pity. And I don't know why Richard didn't want to go on with that. It's a pity he didn't go along with publishing that. As it is, to refer to it, you just have to refer to a talk at that American Physical Society meeting, but the talk isn't written.

LIPPINCOTT: No proceedings?

GELL-MANN: No. This was a talk. The American Physical Society at that time had talks. And the talks were referred to, but they were just listed by title in the program.

At Gatlinburg in the fall of '58, Sidney Bludman came up with the red and blue neutrinos—a year after I wanted to publish it. And Richard made fun of him and said it was the dumbest idea he'd ever heard, although Richard participated a year earlier in thinking of this idea. He also said it was really stupid because it couldn't be tested by experiment. I didn't see any way of testing it immediately by experiment, but I assumed that eventually somebody would test it by experiment. Richard claimed that couldn't be done, for some reason.

What Yang and Lee noticed a year or two later was rather simple. It was that as you went up in energy, the weak interaction cross-sections increased, until you went way above the mass of the intermediate boson. Ordinary accelerators would produce neutrino beams, and the interactions would become sufficiently nonweak at high energies that you could actually detect neutrinos in these neutrino beams. And therefore you could test whether or not the neutrinos that came from muon decay could first initiate beta decay. And of course the experiment *was* done, around 1961, and it verified red and blue neutrinos.

LIPPINCOTT: Where was that done?

GELL-MANN: At CERN, I believe. Anyway, all this I find rather sad.

LIPPINCOTT: Did Feynman ever come around?

GELL-MANN: Oh, of course. He was a serious scientist. When the facts proved him wrong, he would accept it. It may have taken a little while sometimes, but he always accepted the facts.

Anyway, in those years I was thinking a lot about approximate symmetries—going beyond isotopic spin and forming very approximate families, which we can call supermultiplets.

Wigner invented that term, for something slightly different, in 1936 and ’37. And I use that term sometimes myself for this slightly different physical concept. The idea was to put these particles, which were already in isospin families, into bigger families. It’s much like classification in biology. But here it has dynamical consequences and dynamical origins.

I tried one scheme, which I called “global symmetry.” It didn’t work. And then, in the fall of 1960, I tried the eightfold way, which did work. So those years were occupied with thinking hard about this problem—higher symmetries—and trying to connect it with the other problem, which was to incorporate the strange particles into the weak interaction theory. And there Feynman and I worked together quite a bit during those years—’57 to ’60. We had a lot of conversations, when we were both around. I mean, he was away sometimes; I was away sometimes. But when we were both around, we had quite a lot of conversations on the subject.

These two subjects are intimately related. And it’s very, very complex how my thoughts shifted from one possibility to another possibility. I discuss this in great length in these papers that I gave you. But I didn’t give you one more, which I’ll try to find, which is a Fermilab paper—one of those Fermilab history conferences.

LIPPINCOTT: The Catalonia talk?

GELL-MANN: Catalunya. In Catalan, it’s Catalunya; in English, it’s Catalonia. Anyway, in that talk I talk about the hole in the doughnut—all the regrets, and so on. And in this talk I talk positively, about how we progressively understood more and more things—as a community. It’s from my own point of view, but I include the work of other people.

Anyway, during that period, I just thought a great deal about these questions of higher symmetry, which was mainly a matter of the strong interaction, and how to extend the weak interaction theory to strange particles, and related questions. In the course of all that work, I postulated, as a number of other people did, that the Yang-Mills theory—or a broken version, a symmetry-violating version, of the Yang-Mills theory—would be the basic theoretical tool for understanding things. This has the very important characteristic—which I mention in the “Garden of Live Flowers” talk—that once you understand the symmetry, the dynamics is an easy step.

Now, throughout science, it's true that studying the structure, and understanding something about the structure, usually precedes a deep dynamical understanding of what's going on. So [Alfred] Wegener was able to point to continental drift when he looked at the shapes of the continents, and qualitative arguments were supplied from various biological and geological discoveries to support it. But there was no mechanism, and American geologists rejected it. When I got to Caltech, I used to argue with the geologists at Caltech about it. And they were unanimous—with, I guess, one exception—in condemning the idea of continental drift. They all said it was stupid and wrong; they had all been taught it was stupid and wrong; and they taught their students that it was stupid and wrong. And I kept saying it was true. The continents do drift; they had once fit together, just as Wegener said. He was absolutely right. They were absolutely wrong. What I'm trying to get across is this point: that just seeing that would naturally precede seeing a more detailed way of how it happens. And it did precede it—by forty-five years or something like that. And that's very common in science.

But what's peculiar about elementary particle physics is that the dynamics is so closely linked to the symmetry that once you understand symmetry properties, you're likely to have the dynamics handed to you on a silver platter, through the magic of Yang-Mills theory—or general relativity, which really operates the same way. General relativity is also a gauge theory, where the symmetry is gauged. And once you gauge the symmetry, you get the general relativistic theory of gravitation. So Einstein's work can be looked at in the same way. And that's a special magic of elementary particle physics, which I discuss in that paper.

So I was thinking—and so were some other people in elementary particle theory—about these three problems together: how to generalize the weak interaction; how to put the strongly interacting particles, the hadrons, into supermultiplets—superfamilies, if you like; and finally, what the dynamics of the weak and the strong interactions would be like, assuming both were probably based on Yang-Mills theory.

Now, as I explain in the Catalunya paper, the difficulty is that if you try to do Yang-Mills theories for both the strong and the weak interaction in the same space, they don't fit. They clash with each other. And a necessary condition for making the whole thing work was to realize that there would be color. With a color variable, which operated in a completely different space from the flavor and spin variables, it was possible to have a Yang-Mills theory—a broken one—for the weak interaction in one space, with charges operating in one space; and a Yang-Mills theory for the strong interaction, using quarks, operating in another space—the space of color. Then everything was fine. But what we were trying to do all those years—or the way I was approaching it, anyway—was to cope with both these problems at the same time, trying to cram them into the same space. And that would never work. So it was a frustrating set of years, and it went on beyond the time when I got the SU(3) symmetry right—the eightfold way. These problems persisted, even after that. And it wasn't until we had quantum chromodynamics for the strong interaction, and the electroweak dynamics—you could call it quantum flavordynamics if you want—for the electrical and weak interactions together, that it all fell into place. And that gave us the Standard Model [of particle physics]. But that wasn't until 1972-73.

So, there were various stages in this process, which lasted some fifteen years or so. The first stage would be the time when I was groping—1957 to 1960—for some clarity. And in 1960 I got some of the things right.

LIPPINCOTT: Was that the kind of work you were doing in France with [Maurice] Lévy?

GELL-MANN: Well, that was something different. I was working on so many things—that was this partially conserved axial current. It was related, of course, but it was an excursion into a slightly different problem. And I describe that also, in some of this literature.

LIPPINCOTT: I want to mention that you were elected to the National Academy [of Sciences] in 1960.

GELL-MANN: Yes. I heard a rumor in '59 that I would be elected. And Margaret said she would come to one of two things—either Washington for the meetings in Washington at which I might be elected to the National Academy, or to Yale, where I was invited to receive an honorary degree at a very young age. At that point in 1959, I was not yet thirty; I was twenty-nine. Yale, where I couldn't get into the graduate school of physics, was now offering me an honorary degree at this incredibly young age.

Well, Margaret made the wrong choice, I guess. What she said was that I would get lots of honorary degrees in my life later on, but election to the National Academy was a unique event, so she would come to that. I said, "But it's not certain. Maybe they won't elect me." She said, "Oh, they will." But they didn't—not until the following year, in 1960. I think Julian

Schwinger spoke against it in 1959, because he was afraid somebody would be elected who was younger than he was when he was elected. Anyway, I was disappointed—only because of Margaret, because she came to Washington thinking this would probably happen, and it didn't happen until the following year, when she wasn't there.

I remember things that happened at the ceremony at Yale. It was funny. First of all, Harold Morowitz and his wife Lucille came to greet me outside Woolsey Hall, essentially in their pajamas and bathrobes.

LIPPINCOTT: Why is that?

GELL-MANN: Well, because nobody was told. It's a secret who's going to get the honorary degree. And they didn't know until that morning that I was going to be there. So they came rushing down to make sure they didn't miss congratulating me. And Harold said, "I think they're sorry." [Laughter] It was kind of a funny remark.

LIPPINCOTT: Sorry for not having accepted you as a graduate student in physics? I guess they were.

GELL-MANN: I don't know. I have no idea. But anyway, it was a wonderful thing. I was really delighted. And the Dean of Yale College, William DeVane, said, "My God, this is certainly the first time anyone from Yale has gotten an honorary degree who was an undergraduate while I was dean." [Laughter] The whole thing was quite amusing. It was very nice. We had dinner at the house of the president, Whit [Alfred Whitney] Griswold.

Kingman Brewster did a lot for science while he was Yale's provost [1960-1963]. When he was president [1963-1977], he did less. He thought he had done enough as provost, which wasn't true. But while he was provost, he really improved science at Yale, because the Yale administration had a tendency to look down on science and engineering as necessary evils—that really important things were English and history and so on. Not that I'm against English and history, of course. But the idea that science isn't an important part of culture, or technology isn't an important part of culture, is ridiculous.

Anyway, at the dinner, I got into a long, deep discussion with [Elia] Kazan, the moviemaker. It was really a lot of fun. Then Griswold came over and said, "I have to break this up. I don't know what this unholy alliance of science and the arts is all about. You've got to stop it." [Laughter] I thought that was a typical Yale attitude.

And in '59 we went to France. I got a National Science Foundation fellowship of some kind to go to France. And then I got a Guggenheim I think at the same time—or maybe the Guggenheim was later. Anyway, I got that, and it was really stupid, because I had a Sloan Fellowship, and I could have used the Sloan Fellowship to pay for my trip to France and my staying there all year. They would have allowed me to do all sorts of wonderful things free. And I stupidly asked for a leave from the Sloan Fellowship, which I regarded as something to pay for postdocs. And instead I took this National Science Foundation Fellowship, which was a rather niggardly thing.

LIPPINCOTT: Were you on leave from Caltech?

GELL-MANN: I was on leave from Caltech. But it was really a dumb move. I learned later that the people who were on the Sloan grants could get round-the-world tickets for themselves and their families, and visit Kuala Lumpur and Port Moresby, and so on, as well as Paris or wherever they were going. It would have been incredible. My trip to Africa, which I made the next summer, could have been included in the fellowship. I didn't know any of that, so I stupidly took the National Science Foundation fellowship and took a leave from the Sloan Foundation fellowship.

But anyway, we did that. I was invited by two institutions—actually three, and I accepted all of them. So I had an office in the Collège de France, and I had an office at the École Normale. And then, when my friends moved out from the École Normale to the new campus at the University of Paris in Orsay, I had an office at Orsay, in addition to the one at the Collège de France. I didn't have all three at once. The one at the Collège de France was where I did a lot of work, especially during the fall and early spring. It was very quiet there; there was almost nothing going on. You could think. But every day we went out for lunch. I met some friends and I would go out for lunch and drink a lot of wine. And after lunch, my work deteriorated, because of all the wine.

LIPPINCOTT: Jeremy Bernstein was there?

GELL-MANN: Jeremy was there and he followed me around to Palestine, and so on. And Maurice Lévy was in residence, and he and I used to work together.

There's a comic aspect about it: I was invited to give a series of talks in the winter—I think they were talks pertaining somehow to the University of Paris, I'm not sure. Anyway, they were given physically at the Institut Henri Poincaré, and a lot of students showed up. But you know how it is in France, especially at that time. Students would never ask questions—completely unlike American audiences. If you were the number-one student and you asked a question, implying that there was something you didn't know, the number-two student might know it and would feel superior and that would be terrible. And likewise for the number-two student and the number-three student, and so on and so forth. So nobody asked anything.

But I talked about a variety of issues. One very curious thing was this: When Maurice and I would do our work together, we would speak French, and we would write all the equations in French. It was very good, because I learned how to say very easily, without any problems, all the things that you say in a lecture on mathematical physics. But every time I said about a numerator and a denominator, or about two terms on opposites sides of an equation, that these two terms canceled, I would say, "ces deux termes s'annihilent"—annihilate each other, which is perfectly correct. And Maurice would carefully say, "ces deux termes se chancelent." I had never heard that. It's like the English word "cancel." I didn't think it was right, but he said it so often. So, "cancel each other" instead of "annihilate each other." Well, in English it's perfectly all right; in fact, "cancel" is what we say. But in French it seemed a bit weird. And every time I said "s'annihilent," he would say it the other way. Eventually, I concluded that this was some kind of slang phrase that all the physicists used and that it would be amusing if I used it myself. But that conclusion was just what he was trying to produce.

So one day at the Institut Henri Poincaré, when I was lecturing I said this instead of "s'annihilent," which I had been saying up until then. Everybody drew in their breath—*ooooh*! Only one person was rolling in the aisles, and that was Lévy. [Laughter] He just lost control of himself, he was laughing so hard. He had set me up. He admitted it freely. All those months, he had planted this wrong phrase in my vocabulary, hoping that I would use it in my lecture. [Laughter]

LIPPINCOTT: I can see that it's stayed with you over the years.

GELL-MANN: Oh, yes. I wouldn't have forgotten that. Well, if you read someday that he was attacked by Algerian terrorists, you'll know that I was taking revenge. He was a *pied-noir*.

LIPPINCOTT: Did you have any discussion about this afterward with him?

GELL-MANN: Do you mean, did I hit him very hard? No, I didn't say anything at all. It was not very nice, but it was amusing.

LIPPINCOTT: Then you went to Uganda to look at gorillas?

GELL-MANN: Wait a minute. There's a lot that happened in France.

I worked with Lévy and we wrote a paper ["The Axial Vector Current in Beta Decay," *Nuovo Cimento* 16:4, 705-25 (1960)]. And it became clear that this paper was a very special kind of thing, and that the work had much more general implication. So I redid it, with him and with [Sergio] Fubini and [Walter] Thirring and Jeremy Bernstein. So it was a very international paper. Jeremy used to come over to discuss it. And then I think Jeremy and I published something else, too, but I don't remember very well, also on the same general topic—it's called partially conserved axial current, and it was actually even more profound than we were describing. And again, the description is attributed to Jeffrey Goldstone—these are called Goldstone bosons, these things we were working on.

We worked on a number of little field theory models, which were later very important in the history of the subject—the sigma model and the nonlinear sigma model. They were not so original—there are aspects of these things that appeared in the literature in various places. But the particular way we worked with them, and the way we treated them, was new. And the sigma model and the nonlinear sigma model both became very important tools for theoretical investigation.

I wanted to include Feynman, but he didn't like the idea of the nonlinear expression—the square roots. He didn't think that square roots had any place in field theory. We had this model—and it was just a model, not a real, literal theory. It was a model, illustrating certain important features of theory which we wanted to explore. But he didn't want to have any part in it, so he withdrew his name. And then he withdrew his name from the other one as well. So it was just Gell-Mann and Lévy and Thirring and Bernstein and Fubini.

Then in the spring, I worked principally in Orsay, listening to the nightingales. The school was in the woods then. They've pretty much chopped them down by now.

LIPPINCOTT: What did Margaret do while you worked?

GELL-MANN: Oh, Margaret poked around and enjoyed Paris. At first we were stuck in some miserable suburb, but by Christmastime we had moved to the city. After that, it was fine. **[Tape ends]**