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TERRY COLE
(1931-1999)

INTERVIEWED BY
SHIRLEY K. COHEN

October 11, 22 & 30, 1996

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Subject area

Chemistry, Jet Propulsion Laboratory

Abstract

Interview in three sessions, October 1996, with Terry Cole, senior faculty associate in the Division of Chemistry and Chemical Engineering and senior member of the technical staff of the Jet Propulsion Laboratory. Cole earned his BS in chemistry from the University of Minnesota in 1954 and his PhD from Caltech in 1958 under Don Yost, on magnetic resonance. The following year he moved to the Ford Scientific Research Laboratory, in Dearborn, Michigan, where he rose to head the departments of chemistry and chemical engineering. In 1980 he joined JPL's Energy & Technology Applications branch; in 1982 he became JPL's chief technologist, and he was instrumental in establishing JPL's Microdevices Laboratory and its Center for Space Microelectronic Technology. Interview includes recollections of Lew Allen's directorship of JPL and a discussion of the origins of the SURF (Summer Undergraduate Research Fellowship) program.

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Terry Cole, ca 1990.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH TERRY COLE

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROGRAM

Interview with Terry Cole
Pasadena, California

By Shirley K. Cohen

Session 1	October 11, 1996
Session 2	October 22, 1996
Session 3	October 30, 1996

Begin Tape 1, Side 1

COHEN: You're about to tell us the story of your life.

COLE: Well, it begins a little over sixty-five years ago. I was born in a very small town next to a small town. I was born in the town of Eagle Harbor, New York, which is a suburb of Albion, New York, in western New York State. Eagle Harbor was a harbor at one time on the Erie Canal—one of my forefathers helped dig the Erie Canal. Eagle Harbor is about halfway between Buffalo and Rochester. There's a stop on the thruway in Batavia, New York, which is a little bit south—about ten miles south of Albion. I was born to Mark W. Cole Sr. and Florence Terry Cole, the product of a second marriage: My father's first wife died of cancer in 1921; he remarried six years later, and I was born on the 28th of March, 1931.

I had five elder half brothers and sisters, all but two of whom are deceased. They were young adults—all but one—when I was growing up. The one nearest my age was actually just going into college. He's the only one of the older siblings who went on and got an advanced degree. Another got a degree from Cornell—a bachelor's degree. My half brother Peter got a degree at MIT [the Massachusetts Institute of Technology] in physics in about 1939 and actually worked at the Rad Lab [Radiation Laboratory]—worked for, you might say, Lee DuBridge, though he was far down the chain.

COHEN: What did your father do?

COLE: My father did many things. He had a college degree. He studied for the ministry and went to Trinity College—the one that's in Hartford, Connecticut. It was an Episcopal school. But he never went on and practiced in the ministry.

He began farming. This was a very rural community; it used to be fruit and table crops grown in Orleans County, of which Albion is the seat. He was very interested in politics. He read a lot. He had an extensive library; it was probably at least a couple thousand books. He wrote extensively to authors. At one time he had a short correspondence with George Bernard Shaw—these postcards with Shaw's tiny, almost microscopic, handwriting.

COHEN: Do they exist anymore?

COLE: I think I may have one of them that survived several moves and shuffles in the family. It had to do with something about my father wanting to produce *Mrs. Warren's Profession* as an amateur production for a church group in Albion, and he wondered if they could produce the play without paying royalties. And Shaw didn't go for that. He said that intellectual property is as real as any property. [Laughter]

My father was interested in theatre. I guess he'd been in theatre groups, perhaps when he was in college. He preserved that interest.

He was interested in politics. He served a term in the New York State Assembly. Around 1912, 1915—just before the First World War—he served on the agriculture committee with a young representative from Putnam County named Franklin Roosevelt. I think he was the first Democrat elected from Orleans County since the Civil War. He served for one term, then went back to farming.

When the economic downturn occurred in the twenties, his eldest son, Ezra, started taking over the farming and he began working for a New York State government agency called the Farmers' Fund. It was, in a way, a precursor of some of the things that occurred under the Roosevelt administration in the 1930s. It was a way for farmers to get low-interest loans. I think he worked for them about four or five years. I think the program terminated. Then he got a job—I think it was selling agricultural chemicals—up until the Depression really hit. And in about 1933 or 1934—that was after I came on the scene—he went to work for what is now the

Bureau of Alcohol, Tobacco, and Firearms, which used to be part of the same agency that ran the Secret Service in those days. So he was a revenuer for probably twenty years. He retired from the government job about 1958 or so. He lived until he was, I think, eighty-seven years old and died in 1961.

COHEN: Were you the only child of the second marriage?

COLE: I was the only child of the second marriage. So in a way I grew up as an only child, with elder aunts and uncles who were actually half brothers and half sisters.

Now, also during the Depression, because the government salaries were very modest—well, perhaps I should start back and say where my mother came from.

She also came from western New York, from the Terry family. The Terry family lived for many years in East Bloomfield, New York—that's south of Rochester. Her father was a druggist, and she grew up one of seven children, all with brilliant red hair. Genetically, it's just shown up again; my son has brilliant red hair. She grew up in a very large family. She was very interested in cooking and wanted to go to college, which she did. She put herself through Columbia University—I believe it was dietetics—working nights in restaurants. She graduated in, I think, 1912. There were not very many women undergraduates at Columbia in 1912.

In the First World War she was the dietician at Fort Niagara, New York, which was where many of the wounded were brought back for long term; it was a veterans' hospital as well as being an army post. At the end of the war, she got a job with the Colonnade Company, headquartered in Cleveland, which ran a chain of cafeterias and a few restaurants in Erie, Cleveland, and Detroit. She was sort of a roving nutritionist, but she was also the troubleshooter. When restaurants got into trouble—not getting enough business, or they had problems—she was the roving troubleshooter who went in for a few months and hopefully repaired some of the problems. She met my father in 1927 because he was traveling as part of the work that he was doing for agricultural chemicals. I didn't hear many stories about their courtship, except that he was taken by her good cooking. [Laughter] They were married in 1928, and I came on the scene about three years later.

About 1933 or 1934, when the Depression was at its worst, my mother decided that since we lived in a very large house that was largely unoccupied because the older children had all

gone, she would open a restaurant, which she did. It was called Four Chimneys, which was the name of the house. The house was built sometime between 1815 and 1820. She would open the restaurant during the summer, when it was traveling weather—from about the first of May until snow flies. So my early experience with chemistry was washing dishes at Four Chimneys.

[Laughter] But it established, I think, a work ethic.

My brother Peter would visit occasionally at home. He and my father had started building a telescope—à la Russell W. Porter's amateur telescope making. And when I got in high school, I took over that project, completed it. By that time, we had moved. But I suppose some of my interest in the availability of science as a career came because I knew that Peter was at MIT, and on one of our trips we visited MIT and saw his lab. He worked for, I think, Professor George Harrison at MIT, doing work on spectroscopy. I think there's a catalog of spectral lines that he published that's quite well known.

So both my father and mother were working, but my mother worked at home.

I started school in a two-room schoolhouse in Eagle Harbor. And when I was in about the fourth grade my mother became dissatisfied with the educational opportunities there, so we commuted from the time I was in fifth grade to Albion. Albion was about three miles away—a town of about 4,000 people and the county seat. I went through high school in Albion. I suppose in terms of influences, one person really stands out as an influence in the high school. And from what I've heard, this is often the case with people who go into science. The name was Charles D'Amico. Charlie's becoming a teacher was also a product of family tragedy and the Depression. He had graduated from Union College and was about to accept a graduate fellowship at Columbia. But his father died. It was a large, Italian family with ten kids. Charlie was the eldest and had to drop out of Columbia. He came home and taught in high school—first science and math. He eventually became the principal of the high school that I went to. But on request he would teach a course in advanced algebra. Occasionally he would substitute teach in physics. So he was a very strong influence. The course in advanced algebra—he really put us through our paces. It was a one-semester course, and we went all the way up to integral calculus. He was very demanding. He said, "You'll do more homework in my class than in all the rest of your classes put together."

COHEN: How big would your class have been?

COLE: Five people. So it was strictly volunteer. The high school was about 450 to 500 people. It was a rural community. I would say probably half, or less than half, of my classmates went on to college or advanced degrees. There were a few.

COHEN: But you were happy there.

COLE: Yes. It was a good place to grow up. It was a safe and pleasant place, near the lake shore.

COHEN: Lots of snow in the winter.

COLE: Yes. Living in western New York, halfway between Buffalo and Rochester, is a good way to build your upper body strength, because you do a lot of shoveling. We did a lot of shoveling at Four Chimneys. The house was quite large—twenty-three rooms, I would say between 5,000 and 6,000 square feet. It had been added onto several times—an old house, full basement and all. And the driveway was almost a tenth of a mile long. So averaging something like fifty to sixty inches of snow every year, that generally meant a lot of shoveling—and a lot of lawn mowing, because we had two-thirds of an acre of lawn. [Laughter] So that was some of the background about my growing up.

I got a chemistry set as a Christmas present. I was able to have sort of a lab in the basement. I got quite interested in photography and the chemistry involved when I was in high school—and even a little bit before that. My father was an avid amateur photographer, along with his other interests. He had some of the first Kodachrome film from Kodak. When he was working for the government, he worked in Rochester generally. At one time he worked in Buffalo. So he always had long commutes—thirty to fifty miles. And in the 1930s, that was a long commute, because it was narrow two-lane roads and no thruways.

COHEN: So it was rather expected that you were going to go on to the university?

COLE: I think my mother certainly expected it. [Laughter] And I think my father did, too.

COHEN: So where did you go?

COLE: When I finished high school, I had a New York State scholarship. I think I was second or third in my class in high school. Regents exams were definitely part of the drill. The scholarship could be applied to most places in New York State, but there were some places it couldn't apply to. On the other hand, my family was not wealthy enough to fill in the rest. So we decided that I should go to a school that offered cooperative work, so I could work half-time. And there was the Rochester Institute of Technology, which was the outgrowth of the old Rochester Mechanics Institute—a very old technical school—and probably started a bit like Throop, but around 1830. So I started at RIT in 1949.

They offered essentially a three-year degree, including work time. It was really the equivalent of only two years, because after the first year you worked half-time. So I worked at Kodak as a co-op student. That was quite often the case in chemistry. I think my original interest was in physics, but there was no physics really offered as an option at RIT. There were electrical engineering and chemistry and chemical engineering and of course photographic technology and a few others. So I decided that chemistry was sort of the second interest of mine. I started in chemistry there and graduated in 1952 with pretty good marks.

I was looking around for another place. I did well enough that several of the professors at RIT, especially the fellow who taught physics and who helped me quite a lot—his name was Raymond Buhler—said, “You're good enough, you ought to go on.” I talked with several of the other faculty. We had had analytical chemistry out of a book by Ivan Koltoff, a famous analytical chemist at the University of Minnesota. So I got several catalogs, and I decided that Minnesota looked like a very interesting place for somebody with a chemical background and an interest in chemistry. So I went to Minnesota, and I figured I could get my bachelor's degree in two years. I had gotten an associate degree in applied science at RIT. I corresponded with Minnesota and entered there in the fall of 1952.

Influences there: I met my first Caltech graduate, who taught me physical chemistry—Bill Lipscomb. I got an A in his physical chemistry course—a very challenging course. He encouraged me to come to Caltech. In fact, he wrote a letter of recommendation to his professor, who was Eddie [Edward W.] Hughes. So that's the family connection to Caltech.

Of course, I knew about Caltech even when I was going to RIT, and actually wrote and asked about a transfer. Well, transfers were very difficult and I didn't have enough credits—especially I didn't have enough humanities credits to make the transition. But eventually I did make it here.

I started in 1954 and went through the usual several weeks of talking to faculty and trying to decide whom to select as an advisor.

COHEN: Had you been out here before?

COLE: No. I had a cousin—still have a cousin—living over in the San Fernando Valley. So I knew somebody who was out here. They met me at the Santa Fe Station in Pasadena with my steamer trunk [laughter] in September of 1954, when I came to Caltech.

The first night was spent over “The Greasy.” One of the dorms for graduate students was on the second floor over the greasy spoon. It was a temporary building from the First World War, located where the bookstore is now—an old clapboard building, sort of creaky, sort of falling down. Smelled of onions and french-fry oil. [Laughter] The first room that I got there was located right over the vent fan from the kitchen, and it was pretty hot in September of 1954. So I decided that I really should look around and see what else was available for housing.

One of the early people I had met in my tours looking around the chemistry department was Floyd Humphrey. Floyd was living at that time in the Athenaeum's loggia—no longer occupied. He had a bunk in the loggia. Residents in the loggia had a cot, blankets, and, I think, three or four drawers in the huge wall of drawers. But there was no room in the loggia. But Floyd said he'd seen some notices for places along Hill Street, so I found a room in the back of, I think, 555 or 535—it's just about across from the President's House. There was a widow lady there who rented out to Caltech students. So that's where I lived.

Now, as I said, Floyd was one of the first people I met touring around the chemistry department. And it seemed to me I was interested in the physical side of chemistry. Floyd was doing some very interesting work in magnetic resonance, working for Don Yost.

Don was one of the first people—perhaps the second person—on the West Coast to do magnetic resonance spectroscopy after Felix Bloch, who had coinvented it with Ed Purcell at Harvard; Bloch was at Stanford. This seemed like something very interesting—combined radio

frequency, microwaves. It was a new field. There were lots of things to do; almost anything you looked at was of scientific interest.

So I talked to Don—and Don was a true character. I wrote a little biography of Don for *Engineering and Science* some time ago and helped John Waugh to put together the National Academy biography of Don. Don grew up in Idaho when it was still a territory and rode to school eight miles on horseback to a one-room territorial school. [Laughter] He was a true pioneer, quite an iconoclast, a great wit. He had wonderful stories about growing up in Idaho. And I decided that this looked like an interesting field and somebody very interesting to work for.

COHEN: Was he of Mormon background?

COLE: No, he was Catholic. I think he converted to Catholicism after he met his wife, Marguerite, when they were undergraduates at Berkeley.

Don took the train to Seattle and a boat south to go to Berkeley. [Laughter] Out-of-state tuition, he told me, was fourteen dollars. He took freshman chemistry from G. N. Lewis. His TA [teaching assistant] was Joel Hildebrand and his lab instructor for freshman chemistry was Richard Tolman. So you can see, he was sort of predestined.

When he went into the navy, he met Marguerite. They got married, he went into the navy during World War I, came out of the navy and then decided that he wanted to attend graduate school. Marguerite was from Salt Lake City, so he registered at the University of Utah and started working for a Professor Bonner, who in less than a year or so said, “You know, Don, you’re so much more interested in the physics part, you really ought to go to this new school that this friend of mine [Arthur Amos] Noyes is starting, out in Pasadena. I’ll give you a letter of recommendation.” So that’s how Don came here and was in the first graduate class in chemistry.

Now, Professor Bonner had two sons who later came to Caltech and used to work on the Yost ranch in Idaho every summer—Lyman and James Bonner. [Laughter] Another family connection.

I started working for Don in magnetic resonance and was searching around for a problem. I was learning the technique and how to do things. Floyd Humphrey and his previous graduate students had built up some apparatus in Don’s lab, which was in the basement of Gates—22

Gates was my lab. I was down in the subbasement, right in the little alcove corner there, where the walkway passes Gates.

Previous people who had worked for Don include John Waugh and Jim Shoolery, both of whom became quite famous in magnetic resonance. James Shoolery eventually became, I believe, the chief of R&D [Research and Development] at Varian Associates. That was the company that for twenty years or more made virtually all of the magnetic resonance equipment—at least in this country. John Waugh went to MIT; he's now a professor at MIT.

COHEN: So you were surrounded by very interesting people.

COLE: Yes. Don had contracted osteomyelitis while working at Los Alamos during the war. He worked on some of the chemistry associated with the bomb, and I believe some of the chemistry of extracting of the transuranic elements, and became very ill, close to death. Apparently he was one of the first people to receive penicillin during the war. He survived, but he was in rather frail health when I worked for him. He also had had two cataract operations, which required him to wear these very thick glasses to see, so he really was not active in the laboratory. But he was a great guy to talk science with, because he had read so extensively and knew so much about just descriptive chemistry. In fact he wrote, I think, the first textbook on the rare-earth elements—a thin volume. There wasn't much known at that time. Yost was a great resource person. You'd go and talk to him and he would ask you great questions.

Now, a little jumping around. What did I do for my thesis? I had met several people in physics, and after about the first term I started eating with two fellows who were in physics: John Thomas Harding, who was a student of John Pellam, a professor of physics specializing in low-temperature physics, and Arthur Muir, who worked for Jesse DuMond. They were living in the playhouse behind Professor Morgan Ward's house on Holliston Avenue. We found it was cheaper to buy groceries for three than to go out and eat. And we were a little bit tired of the menu at the Athenaeum in those days, which was very dull and very repetitive.

So we started eating at the playhouse. John Harding was working on a project that had been inspired by Richard Feynman to try and discover experimental evidence for quantized vortexes in superfluid helium called rotons. Now, the experiment he was doing was a heroic, very difficult experiment. Essentially, he had a carbon microphone—similar to the old carbon

microphones that were used for radios back in the 1920s—immersed in liquid helium. And he was listening to the noise produced by the vortexes in the liquid helium. There was an enormous amount of just background noise. Even when the helium was superfluid, still there was vibration in the rooms and from people walking around. And you were listening for tiny increases in the noise levels.

So we got to thinking. We were talking physics, and I said, “Well, you know, there’s a paper that I’ve read about magnetic resonance and how magnetic nuclei interact with one another.” There’s a theory called Blumberg, Purcell, and Pound, BPP for short, that everybody read in those days—in fact they probably still do, if they’re interested in the history of the subject—on how magnetic nuclei interact with one another and exchange energy. And I said, “You know, there’s a term in there that says that the relaxation rate of a magnetic nucleus depends on relative motion”—Brownian motion, in the case of normal liquids, and maybe the relative motion of helium-3, because we knew that Pellam had a very small sample of an isotope of helium-3, the only helium isotope that has a nuclear spin. Most of the helium is helium-4, and it has no magnetic properties. So we said, “Supposing we made a solution of helium-3 in helium-4 and looked at the helium-3 with nuclear resonance. Could we find when rotons occur that there’s enough of a change in the rate of motion of one helium-3 relative to another to show up in the relaxation rate?” Now, we knew this relaxation rate was going to be very slow, because of these low temperatures. There was not much of a way for these nuclei to get rid of their energy.

So we talked to Pellam, but Pellam said, “Well, you better go talk to Feynman to see if this could actually work.”

So John Harding said, “OK, I’ll go ask Feynman. I’ll go and propose this to him.” This was about the time that Feynman was very interested in superfluidity. Another Feynman story.

John went and talked to Feynman and proposed what we wanted to do. Feynman thought about it. I’ve forgotten whether it was a day or so. He finally told John, “No, I think you’re wasting your time, because the helium-3 and helium-4, they’re different particles. One is a boson and the other’s a fermion. They have different kinds of statistics, and I believe that the rotons in helium-4 will pay no attention whatsoever to the helium-3. There won’t be any influence. You’re sort of chasing something, I think, that has almost no chance. It’s a big risk.”

So we went and thought about it. Well, the story was that Feynman had spent two years as a helium atom, [laughter] he knew so much about it. So we said, “Well, we better drop that.”

The upshot of the story is, about a year or so later Professor Bill Fairbank up at Stanford did the nuclear resonance of the solution. And he didn't find rotons, but what he found was that when you get below the point at which the superfluid transition occurs, the two fluids separate; they're not completely mixed—you get two layers. The signal from helium-3 disappeared out of his coil. And this was a major discovery, because nobody had predicted that helium-3 and helium-4 could not mix. Normally, they're completely soluble in one another. Here was a case where two isotopes, driven by this difference in statistics, had separated. If you had the solution and if you saw any signals at all and you cooled it through the transition point, you would have seen it. It was absolutely a no-brainer. [Laughter] Which taught me something very valuable: Never neglect doing an experiment that a theorist tells you won't work. [Laughter] Because theorists can usually tell you something about probabilities, but it's the unexpected that the theories don't predict, or that haven't been thought through far enough to say, “Ah, there should have been a phase separation!” And of course, once the measurement was made, all the theorists working on helium said, “Oh, well, of course, if you'd asked us if there was going to be a phase separation, we would have predicted it.” [Laughter]

Good lesson. But we were still interested in doing magnetic resonance at very low temperatures. And Pellam had a setup with helium. He could liquify helium; he had a Collins machine here. He had a large magnet in his lab, which was actually better than the magnet that we had in Don Yost's lab. The magnet that I started with in Don Yost's lab was inherited from [Carl] Anderson's B-29 flights looking for cosmic ray particles. It had a rather strange design that was just right for you to put square photographic plates between the poles so that he could look for spiraling magnetic particles.

So we moved all the electronics and all of the microwave gear from Gates over to East Bridge. Down in the basement of East Bridge was where the cryogenics lab was. There was an interesting phenomenon that had been noted by Herbert Broida, who started the physics department at UCSB [University of California, Santa Barbara]. Broida had worked at the National Bureau of Standards for many years—as had Pellam—both with an interest in low temperatures. They had discovered that when you put a microwave discharge through nitrogen gas, you could produce a phenomenon that is seen sometimes in the upper atmosphere, called air

glow. It has to do with excited states of nitrogen molecules. It's a yellowish-green glow. And people are very interested in studying this in the laboratory. It was thought that perhaps the microwave discharge was a way to make single nitrogen atoms. Normally, you don't find very many nitrogen atoms, because you have, I think, 216 kilocalories of binding energy in the nitrogen triple bond, so nitrogen is normally a diatomic molecule.

We thought, Well, look, we could probably set up a microwave discharge, and maybe if we condense this material at liquid helium temperature, this would be a way of making nitrogen atoms. And paramagnetic resonance would be an ideal way to determine that they were there, and we could identify them as nitrogen atoms because the nitrogen-14 has a nuclear spin of 1, so there would be lines to see.

So we set up that experiment. Many trials and tribulations in getting it set up, learning how to work with helium. I probably have to look at my notebook, which I have not done. It would probably have been in the spring of 1956, about a year after I got married. We saw our beautiful spectrum—big lines, tremendous lines. So I knew I had a thesis at that time.

Yost and Pellam were both on my committee, and both read my thesis ["Studies of free radicals at 4.2 degrees K by electron paramagnetic resonance" (1958)]. One was a chemist and one was in physics.

Actually, about that time the chemistry department was interested in hiring somebody new in physical chemistry. Don Yost was getting ready to retire; he was approaching emeritus status. There was a seminar by a candidate for the physical chemistry post coming down from the Shell Development Lab at Emeryville—Harden McConnell. So I went to hear Mac's first seminar at Caltech. Very impressive, of course, as he always is, striding back and forth at the podium. [Tape ends]

Begin Tape 1, Side 2

COLE: Mac's seminar was very, very impressive. I think he came to Caltech in the following year. He was hired. And I was in the process of finishing up my thesis—actually, had finished my thesis, I guess. I had my final oral on the day after *Sputnik* was launched. [Laughter] So I didn't make any headlines at all.

Shortly before that, I went over and welcomed Mac, and he said, “I need some help in setting up my laboratory. How would you like to become my first postdoc?” That’s how I began my career with McConnell. I worked for him from October ’58 until April of ’59. I helped him set up his lab, met a number of friends that are still very close to me. There were two other postdocs that he hired within a few months afterwards—Hanan Heller, an A. A. Noyes research fellow in chemistry, and Richard Fessenden.

Together with McConnell, we started working on free radicals. McConnell, Norm Davidson, and I actually pursued this idea of using low-temperature stabilization. We found that, in fact, nitrogen atoms, because they have high electronic spin, in crystalline condensed nitrogen actually have what’s called zero field splitting, which was a new observation for an atom in a crystalline solid—an atom of that low molecular weight. So we published that. The nitrogen atom work was published.

Then we started looking at other ways to produce radicals in low-temperature materials. We decided the methyl radical was a very interesting chemical species. And we found that by freezing methyl iodide at nitrogen temperatures and hitting it with X rays—we borrowed one of Sten Samson’s X-ray cameras and set up a way that we could actually irradiate the sample in the electron spin resonance cavity. We were able to generate methyl radicals, and we were able to show from the size of the interaction of carbon-13—we got carbon-13-labeled methyl iodide—we were able to show that the methyl radical is planar. We actually had an argument for several years with [Linus] Pauling, because Pauling didn’t think that methyl radicals should be planar. But I think the experimental evidence finally carried the day.

So we became interested in other kinds of simple radicals, where the unpaired electron was in a pi orbital. McConnell had been working on a theory that said that if you had conjugated bonds in an extended pi orbital, there were places in that chain where the spin of the electron was pointing down. Averaged over the entire molecule, spin is pointing up, let’s say, given a direction for the electron spin. But there were places he called negative spin density. And we looked around for a number of compounds. We were pretty much unsuccessful at looking at low-temperature materials. So McConnell said, “Well, why don’t we look at some materials where we can just crystallize them?”

It turned out that malonic acid was very interesting material. Two carboxyl groups—one at each end—stabilized the material, and there’s a single CH₂ group in the middle. So we were

able to irradiate malonic acid and make essentially a fixed radical. I want to make some corrections in the way I led through that.

First of all, we were able to show that the spin coupling with the nucleus of a carbon atom in the malonic acid was negative, which had been under some dispute for a long time. Later on, when I moved on to Ford [Ford Motor Company], I continued working on this and actually was able to do one of the conjugated radicals.

Perhaps I should step back and fill in a little bit of history, going back to Minnesota. I think the second or third night I was at Minnesota, one of the people I had struck up an acquaintance with at the orientation session and who was staying in the dorm room next to mine—we decided to go out to the place where all the undergraduates eat at Minnesota, which was called Dinkytown, down Washington Avenue from the university. We went to a Greek restaurant and decided to have a little bit of pseudo-ethnic food. We got to talking and exchanging. He came from some small town in Minnesota, and I was telling him where I came from and that I'd worked at Kodak. And suddenly this girl in the next booth sort of craned her head around the edge of the booth, and she said, "I heard you say you worked at Kodak." And I said, "Yes, I used to work at Kodak as a co-op student." She said, "Well, then, you know Randy Houck?" Well, Randy Houck was my officemate at Kodak. [Laughter]

We started exchanging stories about him, because he's rather a humorous fellow. And one thing led to another, and we started dating. By the time I graduated, we were very much going steady. I think we were as good as engaged. But she was in nursing education and had to stay another year at Minnesota to finish her degree. So I came west. We spent a year apart. And then on July 16, 1955, we got married, and Margaret came out here.

TERRY COLE
SESSION 2
October 22, 1996

Begin Tape 2, Side 1

COHEN: There were several things you wanted to mention before we leave Caltech.

COLE: Well, yes, because it's a sidelight on some of the personalities that have made Caltech what it is.

For a number of years there was a fair amount of dispute between my research advisor, who was Don Yost, and Linus Pauling. It's interesting that they were both part of the first graduate student class in chemistry. So they had known one another I guess since about 1922 or 1923.

Don worked at Los Alamos during the war and came back to Caltech shortly after the war. And I think he was worried about the dominance that Pauling was having in the division [Division of Chemistry and Chemical Engineering]. He was obviously the greatest chemist of the twentieth century. But I think Don felt that Pauling's strength really was in research rather than as chairman of the division. And what he told me was that when Noyes had retired as chairman they had set up two positions. One was chairman of the division and the other was director of the laboratories. The idea was to have sort of a dual role—that the chairman would have cognizance over the academic doings of the division, especially having to do with the hiring of young outstanding people. And the executive officer would make the wheels run and keep things operating, having to do with lab space and equipment.

Apparently Pauling, according to Yost, wanted to be—and became—both chairman of the division and executive officer. And I think Don felt that this was too much power concentrated in one individual. Pauling became chairman and director of the laboratories before the Second World War and continued after that for many years. But I think Don objected to the fact that Pauling was so busy with his scientific enterprises that he in fact left most of the executive officer-ing to James Holmes Sturdivant, who had not been elected as director of the laboratories. And he [Yost] felt that that was, I think, a misuse of the office—that it should have

been an elected position. Yost felt that Pauling was imposing too much regulation—that things were becoming overregulated and overbureaucratized. I believe it was before my tenure under Yost that Pauling had said that research advisors should read and approve of theses. Don objected to that, on the principle that Caltech graduate students are adults and they are in a competitive game: I will read the thesis, and I will make comments to the doctoral candidate, but I will not approve it, because it is the job of the examining committee to examine and approve the thesis. They are going to read it as persons not personally involved with the student, and they will bring their best scientific judgment to bear. And that is what the purpose of a final oral exam is all about—to examine the thesis, and if it is suitable, to approve it.

This was a point of contention between Yost and Pauling, and I believe there was one incident relating to the issue. I think this was related to me secondhand by Floyd Humphrey. There was this thesis that was submitted to the divisional office, and it said, “Read by Don Yost.” And it was sent back by the chairman saying, “Please approve.” And Don sent it back again, saying, “Read once more, but not approved. You can’t regiment me.” [Laughter] Which was expressive of Don. He was very much the pioneer spirit, having been raised up in Idaho when it was still a territory, and had no truck with overregulation and bureaucratization. So I was certainly conscious of that during my time.

COHEN: Were they civil to each other if they would meet?

COLE: I think that they were. But generally, they wouldn’t attend one another’s seminars. It was well known that there was friction between them. And I think it probably showed up in sort of an icy calm. It’s unfortunate that it happened, because they were both outstanding scientists. But Don was not to be regimented. And I think he thought that Pauling had overstayed his tenure as chairman—that he should have given it up and should not have delegated so much authority.

Let’s pass on from that to early 1959. Verner Schomaker was getting ready to leave Caltech and go to work as director of the Union Carbide Research Lab in Poughkeepsie, New York. He had suggested that perhaps I should either come with him or I certainly should go and meet the people at Union Carbide who were forming the lab and get an idea of what it was like. So I signed up for an interview trip and made the arrangements. And before I left, I had a visit in

my lab down in 22 Gates, where the registrar's office is now, down in the basement. A long lanky fellow with a brush cut who spoke very slowly and somewhat hesitantly—sort of a Gary Cooperish fellow—by the name of Jim Zimmerman stopped by. Jim Zimmerman was then working at the Ford lab [Ford Scientific Research Laboratory] and he was on a recruiting trip. He had done extensive work in low-temperature physics.

Jim said that Ford was forming a new research lab, since Ford hadn't had any research labs. They'd had an engineering center for many years. Would I be interested in coming, at least on an interview trip? And it turned out that the dates and the timing were right, so I made a dual interview trip. I went to New York City, talked to the Union Carbide people there, visited the site. At that time, it was no more than a site. It had been bulldozed clear and there were stacks of structural steel and bricks and construction equipment. So that the plans there were only plans.

Then I visited Detroit on my way back, and was met by Jim. I went to the Ford lab, and there I met Jack [Jacob E.] Goldman. Prior to his arrival at Ford, Jack had been head of the magnetics lab at Carnegie Tech—that's before it became Carnegie Mellon—through the early part of the 1950s. He had been recruited to come to Ford by Michael Ference. Mike Ference had been a professor of physics at the University of Chicago and was recruited to come to Ford by Andrew Kucher. Andy Kucher was one of the originals at Ford. He was an engineer. I believe he might have had a bachelor's degree in mechanical engineering and worked for old Henry Ford for a number of years, and was now vice president of engineering at Ford in Dearborn.

Andy Kucher had always had a fascination with science, although he was certainly not a scientist—at that time he was pretty much an administrator. But he had convinced young Henry Ford II, who was at that time president of the company, that Ford should have a basic research lab, inasmuch as General Motors had just opened the Technical Center up in Warren, Michigan. And this would be a real basic research lab. At that time, in the early 1950s, it was considered very fashionable, if not almost required, for major corporations to have high-class basic research labs, pretty much patterned on Bell Labs and IBM, and perhaps General Electric, which were the premiere industrial labs at that time. He recruited Ference to come and start it.

One of Ference's first recruits was Jack Goldman. Jack is a real ebullient fellow. I would say he was sort of the Sol Hurok of condensed matter physics, in that he knew everybody

in the field. He always used to brag that he had played poker with and won money from all of the American physics Nobel Prize winners—at least prize winners in condensed matter physics. And it was his tradition to go to the March meeting of the APS [American Physical Society], which is where all the condensed-matter, solid-state physicists congregate, and to have a running poker game there in his hotel room for days and days on end. A very gregarious, tremendously enthusiastic person. And one of the great scientific recruiters of the last few decades.

He had managed, after being at Ford probably only a few months, to recruit many of the outstanding people who had been at Carnegie in the magnetism lab: Anthony Arrott and Hiroshi Sato. And in what was really a coup at that time, he'd recruited Al Overhauser—that's Albert W. Overhauser of the Overhauser effect—who was at that time a young professor of physics at Cornell. He was getting top people. This was shortly after Al Overhauser had graduated his second graduate student, who was John Hopfield. So, another Caltech connection.

This was considered quite a coup because Al, while he was still a postdoc at Illinois, had theoretically predicted a thing called the Overhauser effect, which was essentially a way to cool atomic nuclei by putting energy into electron spins. It was widely thought by some experts, when it was first proposed, to violate the second law of thermodynamics. And it was confirmed within a year after his prediction. It was one of those few cases where a theoretician actually predicts an experimental effect and is vindicated completely by the experiment. So he was thought to be a young tiger in this area.

COHEN: He must have been offering high salaries to get these people to come?

COLE: They were. Essentially, Jack's approach was, "We want you to come here. We'll provide you with all the money and all the resources you need. And what we want from you is the best science that you're capable of. And you choose the topic." So it was carte blanche. It was an era that probably won't be duplicated again for a very long time. And for somebody either starting out or early in their career, it was just wonderful, because the U.S. automobile companies then virtually owned a license to make money. There was no Japanese competition in the 1950s. The public was still starved for automobiles, because there was still a lingering shortage from the Second World War. And big cars with lots of chrome and big engines that made big profits for the company, so that there was lots of money. One never had to write a

proposal to the government. It was the last thing on their mind, to ever take a dime from the government because, of course, the auto companies suspected the government. We were provided with outstanding equipment, plenty of money, and essentially you just had to tell your supervisor or your department manager a few months ahead of time how many more hundred thousand dollars you needed next year. [Laughter] And at least write it up as an internal proposal for a few pages, and make a list of equipment.

Of course, there was a downside, which was that not being at a university, we didn't have the continuous stimulation of young challenging minds and junior faculty. But nevertheless, it was a wonderful place to work. There was actually a whole lab that was created to do mostly magnetic resonance. And another one, just across the hall in the scientific lab building, to do lasers. Some of the very first laser work in the country was done there. The first time that lasers were generating multiples of their frequency—laser harmonics—a lot of the early nonlinear work was done there.

Other people who were working in magnetic resonance with me at the same time were Hanan Heller, who had been a postdoc with McConnell. He started shortly after I had started with McConnell. And we had actually done some work together on free radicals made by X-radiation and crystals. So he was in that group. Arnold Silver, who is now head of superconducting research at TRW. John Lambe, who was at JPL [Jet Propulsion Laboratory] with me for a number of years and is now retired. And Toshimoto Kushida. Toshi had been hired from the University of Hiroshima, having just finished a postdoc at Harvard with Nicolaas Bloembergen.

We plunged into various aspects of magnetic resonance. Hanan and I worked on the problems of looking at the remains of high-energy radiation in polymers and organic crystals. John Lambe became involved in doing double resonance, where you do both electron spin or paramagnetic resonance and nuclear magnetic resonance at the same time. Arnold Silver was interested in doing nuclear magnetic resonance and looking at the atomic structure in glasses.

COHEN: Now how could this possibly be related to the making of automobiles?

COLE: It really didn't matter. As long as it was publishable in the very best journals and people were invited to give talks. To a large extent, it was looking for intellectual reflected glory, I suppose. And it was thought, in a way, stylish. It was the thing that large corporations did.

Now, Bell Labs actually got tremendous payoff, and I think so did IBM and General Electric in the long run. Those labs had been going for, I suppose, on the order of fifty years before the Ford lab was set up, so there'd been a very long-term investment. And certainly in the case of Bell Labs, what came out of it was the transistor and essentially most of modern electronics. Certainly major improvements in lasers—all of those things flowed from the investments of millions of dollars over decades. In terms of the country's economy, it was a tremendously worthwhile investment.

In addition to the physics department, there were other departments at the Ford Scientific Lab that dealt much more with the business of the company. There was a first-rate metallurgy department. Now, that made a lot of sense. In some universities, metallurgy and solid-state physics are combined—they're in the same department—because they both deal with matter in the condensed state. At Ford, there was a separate metallurgy department, and they worked on what was thought of as fairly fundamental research in metallurgy: Why do metals, when you stress them repeatedly, why do they develop fatigue cracking? Obviously important for durability in automobiles. They were very interested in corrosion, because that's a chronic problem in the automobile industry. And also in the metallurgy of metal cutting: How do you make cutting tools that wear longer?

COHEN: But you guys were just sort of a bauble, in some sense?

COLE: Yes. We had a chemistry department. They worked, at first, mainly on polymers, but then on inorganic chemistry. They eventually hired Bernard Weinstock, from Argonne National Laboratory; before that, he had been at Los Alamos during the war. He was an expert in fluorine chemistry—gaseous compounds, mostly of metals, that are made by reacting them with fluorine. After a short time at Ford he became interested in atmospheric chemistry, and he and Hiromi Niki did some of the very first work on the oxides of nitrogen chemistry that was key to unraveling some of the problems of smog. So, in a way, that recruitment was very propitious, because later on it gave Ford the credibility to say, "We are actually doing fundamental research

in the area of pollution, and when we speak, we speak with the authority of someone who's publishing in the refereed journals and is well recognized as an expert in the area.”

While we started out doing magnetic resonance in, I think, the summer of 1964, John Lambe was working on this double resonance. He was looking at silicon. You know, when you put a little bit of phosphorus in silicon, you can make it conducting. And he was looking at the interaction between the electron spin and the nuclear spin—silicon-29 has a spin. And just before they published their paper, they decided to get one more sample of phosphorus-doped silicon from Dow Chemical. And to make sure that it was what they had ordered, John asked his technician, Dick Ager, to make a resistivity measurement—electrical resistance. Now, it's tough to make a good electrical contact with silicon, because it's covered with silicon dioxide on the outside; it's a naturally forming insulating layer. But you can break through that if you use the element indium. And you can use indium as a solder, and you can make good electrical contact.

So Dick put the canonical four probes on this little bar of silicon about two millimeters in diameter and measured the resistance. It was one ohm, which is what they were looking for. They then put the sample into the magnetic resonance instrument and cooled it down to four degrees Kelvin. All these measurements had to be made at liquid helium temperature.

Suddenly, before they even turned the magnet on, they began to see what looked like magnetic resonance spectra coming out of their apparatus. They noticed that even with the power off, if they rotated the magnet, these lines seemed to move around, move back and forth in the spectral domain. They turned the magnet on. They were seeing not dozens of lines, which was not unusual, but thousands, tens of thousands of lines. A mystery! They had looked at the spectrum in a nearly identical sample just before. How could something change that much? They took the sample out; they thought there was something wrong with the apparatus. The signals all went away, so it was obviously something in the sample.

Now, John Lambe, I think, showed great scientific judgment. At that point, he said, “Forget about the double resonance for a while. This has got to be important. Let's find out what's happening here.” So they made many experiments. They found out that if you took the indium off the silicon, the signals went away. They hired a fellow, Robert Jaklevic, from Notre Dame, who was good at making thin films of metal. So they made thin films of indium. Then they made thin films of lead. And they found out that they could get the same effects. They

brought in Al Overhauser, our theoretical top gun. And he was scratching his head, proposing a number of things.

About that time, another one of the Caltech recruits to Ford, Jim Mercereau, who eventually came back to Caltech—he had been a PhD student with John Pellam, one of my co-advisors—was off at one of the periodic low-temperature conferences. It was in Moscow. He got back about a month after this discovery was made. And he said, “You know, at this conference there was this young fellow. I think he’s just a graduate student. His name is Brian Josephson, and he’s come forward with this remarkable theory that if you have two pieces of superconductor that are joined by what was called a weak link, where the electron wave function had a bottleneck, that you should see effects in the conductivity of that junction that are quite remarkable.” This was the Josephson effect, and they had been observing it. When Mercereau reported on the nature of the effect, it was quite clear that that’s what was happening. And there was very rapid development. The resonance lab was turned around. And for several years, it did mostly superconducting work. That’s where the acronym SQUID comes from—Superconducting Quantum Interference Device; it was coined by Arnie Silver in the summer of 1964, even though later IBM said that they invented it. [Laughter]

Unfortunately, eventually the group of Mercereau, Zimmerman, Silver, and Lambe—one of whose diagrams shows up in the third volume of Richard Feynman’s physics book as being such a beautiful example of quantum interference—broke up because of one of these unfortunate disputes over intellectual property. It was pretty clear that these effects could be turned into devices, and that the devices might actually make somebody some money. So there were several patent disclosures written. And somehow between the initial rough draft writing and the final legal submission, some names got eliminated. And this led to a very bitter dispute between Mercereau and all the other members of the group. It was so bitter that people threatened to resign from Ford. Eventually, Jack Goldman, who was the impresario, had to split the group up. And he moved Jim Mercereau out to the Aeronautics laboratory.

In probably the very early 1960s, Ford had purchased Philco Corporation. One of Philco’s subsidiaries was Aeronautics, out here on the West Coast. They also owned Western Development Lab in San Jose and Aeronautics in Newport Beach.

COHEN: Wasn’t it Mercereau who left [out?] all the names on it, or something?

COLE: It was never completely clear to me, but as far as most of the other members of the group [were concerned], Mercereau was the guy in the black hat. Whether this had started out as something that a lawyer had done and Mercereau hadn't stopped it, or hadn't even realized that it occurred, nevertheless feelings became very, very bitter.

So the group minus Mercereau kept on with this kind of research at Ford for a number of years—in fact, right up until the present day. Bob Jaklevic is the last survivor of that group, who has finally retired from Ford.

COHEN: So you were well ensconced and living in Dearborn.

COLE: Yes. And by that time, Margaret and I were beginning to have a family. We had three children. Vallance was born in 1961, Catherine was born two years later, and four years after that Sarah Jane was born.

Dearborn actually was a very good place to raise a family. It was a divided city. There was an East Dearborn and a West Dearborn, separated by Ford Motor Company and its world headquarters and the giant River Rouge plant. East Dearborn was somewhat more proletarian than West Dearborn. The managers lived in West Dearborn and people who worked on the assembly line and some of the foremen lived in East Dearborn. It was the ultimate company town. I suppose the assessed valuation of Ford Motor Company in that town must have been \$5 or \$6 billion, because there must have been 400 or 500 acres of total land, if not more. Most of the central third of the city was Ford property, which meant that it was a very stable town. It had been stabilized, of course, by old Henry, the original Henry Ford, and his chief of security, Harry Bennett, who was one of Al Capone's gunmen that Henry had hired as security chief. They had their own private police force, which essentially ran the town. That, of course, all faded away with the new management structure when old Henry gave up the presidency.

Nevertheless, with all of that wealth being generated in the town, there was of course plenty of money. The schools were just superb, in a very stable, nice neighborhood. I lived two and a half miles from my work. I've always been very lucky, and somewhat selective. I always like to live near where I work, so I can go back to the lab at night if need be.

The Ford lab also did work in energy conversion and energy storage. Their principal invention was the sodium-sulfur battery, which was a new concept brought forward by a man named Joseph Kummer. He looked at the problem of energy storage for automobiles: How can you have an adequate battery? Because the battery is really the problem in making an electric automobile. Gasoline is such a beautiful energy storage medium; it outranks lead acid batteries by a factor on the order of 100 in the amount of energy you can store in a given volume or given weight. So he said, "Well, one of the problems is lead is very heavy—lead acid storage batteries are basically heavy." The amount of power you can store per unit mass is low, and another problem is they wear out as you discharge and recharge them. The electrodes are made of solid material—either lead dioxide or lead itself—and as you dissolve the electrodes and then replat them out, nature keeps making mistakes. You get dendrites growing in the lead, and you get the lead dioxide flaking off and falling to the bottom. And eventually the battery wears out—not because the chemistry has gone bad but because there's physical degeneration of these organized solids. He thought, Why not turn things around? Why not have liquid electrodes and a solid electrolyte? He had originally thought that one could use glass as an electrolyte—there are kinds of glass in which sodium and potassium ions move around relatively rapidly—and use sodium and sulfur. Sodium melts around the boiling point of water, and sulfur melts a bit above that. So by having a warm or hot battery, you could have liquid electrodes. You could make sodium sulfide by reacting sodium with sulfur. And everything could be kept liquid except the electrolyte, which only acts as sort of a subway to allow electrons and ions to move back and forth. And that could probably be done forever. Whereas with a liquid, the liquid is highly disorganized, so you can discharge and remake the electrodes over and over again.

COHEN: Was this going on outside of your work? I don't quite understand why you're bringing this in right now.

COLE: Well, to show a little bit about what Ford research lab actually did for the company. Also, in a way, it's a background of how I eventually ended up out here.

Jack Goldman pushed the sodium-sulfur battery. He made really extravagant claims, saying that Ford would be making perhaps 100,000 electric cars based on the sodium-sulfur battery in five years. The sodium-sulfur battery was a great invention, but it was enormously

difficult to reduce it in practice so it would have been durable enough to put in an automobile. In fact, people are still working on that problem. There are commercial sodium-sulfur batteries, but they're not in automobiles. This kind of claim, though, stuck in the throats of management at Ford. Ford liked the idea of the research lab, but they really didn't like the guys in the research lab mucking around in the business of the company and making claims which managers felt somehow they were either going to have to make good on or be embarrassed. I think when these claims didn't pan out, Jack Goldman found out fairly quickly through the usual management channels that his career was rather limited from that time on at Ford—that he could probably give up any dreams he had of becoming a vice president.

So about 1969, Jack got a better offer. He got an offer from Xerox to come to Rochester and take over as vice president for research and development for the entire Xerox Corporation. Well, not only, I'm sure, did he get tremendous financial remuneration but here was a company that was in those days considered *the* high-tech company in the world, probably because they had taken the invention of Chester Carlson, turned it into a brand new technology, and everybody wanted Xerox machines.

Jack had always had a dream at Ford that Ford would allow him to set up a West Coast laboratory—because he thought as good as he was at recruiting people to come to Dearborn and Detroit, he could do even better if he had a lab on the West Coast, preferably near Palo Alto or in the Bay Area somewhere. Well, when he went to Xerox, I think that was probably a quid pro quo for him accepting the job, that he got to set up a laboratory on the West Coast. And at Dearborn, he had tried for many years to hire George Pake, who was, I think, provost of Washington University at that time—an old friend of his in physics. But he was unable to recruit him to Dearborn. He was able when they set up the Xerox Palo Alto Research Center—Xerox PARC.

Eventually, Xerox PARC, as you may know, invented the personal computer, the laser printer, the mouse, the graphical user interface that is now on Macintoshes and is also on Windows. They set up everyone else, partly because the lab was set up on the West Coast and really divorced from company life and the management in Rochester. All of these inventions that underpin most of the personal computer industry never made a nickel for poor old Xerox. Eventually, they tried to buy a computer company; that lasted for a while. But all in all, it was a

tremendous lab, done in Jack's style, very well run, with a tremendous amount of talent. But it was a case where there was very, very negligible coupling with the company.

So what happened back at Ford—Ford needed to recruit someone new to run their laboratory. And they recruited W. Dale Compton, just like the famous Compton effect. He was associated with the University of Illinois at that time. He came to the Ford lab as director in, I believe, 1970. About a year after that, he recruited me to become head of the chemistry department at Ford. Well, I had a lot of misgivings about that, because I really enjoyed doing scientific research. But the lab was going through one of its periodic crises when the company wasn't making quite as much money. The Japanese were beginning to sell cars in America. So after a bit of arm twisting, I did take over. And that's how we come back to Joe Kummer. One of the things that we continued developing when I was manager was the sodium-sulfur battery. We also worked on the chemistry of air pollution—Bernie Weinstock worked on that. And Joe Kummer worked on the sodium-sulfur battery.

Along the way, Kummer also invented another device using this solid electrolyte concept. There was a remarkable material that had been developed by the Japanese during the war; it was a variant of aluminum oxide. Now, there's an alpha alumina and there's a gamma alumina. And the Japanese thought that they had discovered a beta alumina. It turned out that the beta alumina was really a compound that was about twenty-percent sodium oxide as well as aluminum oxide. So it really was a sodium aluminate. And it had the remarkable property that sodium ions were tremendously mobile in this material. Although it was a hard, white, ceramic material that melts somewhere around 2,000 degrees centigrade, nevertheless it conducted sodium—well, not quite as well as sodium ions, almost but not as well as seawater, but better than any other ionic conductor as far as I know that's yet been found. That was the basis for making the sodium-sulfur battery, and later on it became the basis for what at JPL we call the alkali metal thermal electric converter. Because it turned out that you could take away the sulfur in the sodium-sulfur battery. If you could provide a way to condense sodium vapor, you could essentially force sodium under heat and pressure through the beta alumina. And in a way, the beta alumina acted like an electrostatic piston. So you could expand sodium vapor through this membrane and get electrical work out directly. So you put heat in one end, a little bit less heat out the other end, and got electricity. No moving parts. Interesting invention.

Well, after about five years as manager of chemistry, and then as manager of chemical engineering, Ford . . . [tape ends]

Begin Tape 2, Side 2

COLE: After about five or six years as manager of the chemistry, and later the chemical engineering, departments, I decided that I really didn't enjoy the management aspect of things. I certainly didn't enjoy the downside of management, when you have to disappoint your friends or even move them or transfer them. And you have to worry about a lot of details of a department.

So I got a chance through a connection with John Baldeschwieler to become a Sherman Fairchild scholar [at Caltech]. And I came out in the summer of 1976 for a six-month stay, to work on the magnetic resonance of sodium in beta alumina with Bob Vaughan, who was at that time professor of chemical engineering and certainly one of the rising stars at Caltech. While I was working on that—Bob Vaughan had a very active group—I was invited to a dinner party at Baldeschwieler's home, and one of the other guests was Bruce Murray. Bruce was interested in the work we were doing in energy conversion.

COHEN: Was he head of JPL at that time?

COLE: Yes, he had just become director. He was interested in moving the JPL into the national problem of the energy crisis that was going on because of the Arab oil boycott. In Bruce's subtle way, I think he said, "Well, why don't you stop wasting your life at Ford and come out and work at JPL?" [Laughter]

It certainly was an interesting offer. It didn't take right away, but we kept in contact. So I finished up my fellowship here. I had met at that time another young faculty member, named Ahmed Zewail. And we had gotten interested in ways to trap sunlight and concentrate it without using lenses. We called it the luminescent solar concentrator. Sunlight comes into a transparent material that contains, let's say, a fluorescent material, like laser dye. It excites the laser dye. The laser dye fluoresces, but because of the geometry of the optics seventy-five percent of the light that's fluoresced gets trapped inside of a flat plate, like a flat plate of this plastic that surrounds the globes here. And it can only come out at the edges, according to optics. So you

can get a concentration ratio theoretically that's the ratio of the edge area to the top area, which could be very large.

Ahmed and I started working on that before I left in mid-1977 to go back to Ford, and we kept up a correspondence on that. I went back and had decided before I came out for the Fairchild that I really would like to work on this thermal electric converter. I got permission from Dale Compton to go back into research. So I started working with Neil Weber, who was one of the sodium-sulfur battery group who had initially worked on this thermal electric converter, which at that time we called the sodium heat engine. I worked with him and with Thomas Hunt, another Caltech graduate—a Pellam graduate, as a matter of fact, a low-temperature physicist—on developing this.

We were moving along fairly well. But it seemed that really the offer from JPL was pretty attractive. And I really hadn't been able to give up totally my management duties. Early in 1980, I came out for a much more extended interview trip at JPL and talked to the people in Energy and Technology Applications [E&TA], which was a branch of JPL that was on Foothill Boulevard. It was run by Bud [Harris] Schurmeier. I made the decision in the early part of that year that now was the time, perhaps, to leave Ford. The energy crisis and the emissions crisis were forcing a change in the style and relevance of the laboratory at Ford. It appeared that time horizons were shortening from a decade or more to a year or so, or perhaps even less. It was a chance to get back to the Caltech community, to the West Coast that both Margaret and I had always enjoyed and visited when possible.

In July of 1980, I arrived here. Because our eldest daughter was going into senior year in high school in the fall of 1980, we decided that Margaret would stay on in Dearborn until Catherine completed her high school. By that time, my son was enrolled at Kalamazoo College in Michigan.

I lived up on Wilson—I think it was 241 Wilson. It was an apartment house that was owned by the lab. I started working at E&TA doing planning and also starting to work with Perry Bankston up at the main part of the lab to bring the technology of this alkali metal thermal electric converter to JPL. It looked as though this was a kind of technology that, because it simply required a heat source—and on board spacecraft such as *Voyager* we have radioactive isotopes, which are pure heat sources—it might be able to replace the so-called Sebeck effect devices, which are no more than thermocouples and which are still used, although at a very low

efficiency. Comparatively speaking, it appeared—and it is now confirmed—that one should be able to get at least three times the efficiency out of this device as out of the conventional technology.

The work going on in E&TA included a heavy amount of solar-related work. There was solar thermal—there were some large parabolic dishes that the lab owned that were out at Edwards [Air Force Base], where we were developing engines that could be put at the focus of a large parabolic, perhaps 10-meter-diameter parabolic reflector. And also, we were working heavily on photovoltaics. At that time, there was no Solar Energy Research Institute; that was set up later on, in the eighties. Because JPL had had a history of using photovoltaics on spacecraft, it had probably had more experience with practical use, where you had to make these devices work and work reliably under extreme conditions. There wasn't a Department of Energy; it was the Energy Research and Development Administration. So at that part of JPL, we worked for ERDA.

We essentially ran the photovoltaic program for ERDA. The idea was not only to do research; in fact, there was relatively little basic research. But the emphasis was on commercializing—transfer of technology. But it was an interesting kind of transfer. Yes, we used our experiences and our knowledge of solid state physics and how to connect these devices and make them operate under adverse conditions. But we really managed the program, in that we would let contracts to industry for innovations. Let's say Union Carbide developed a new process for refining silicon for solar cells to lower the price. We would bring at least a pilot version of that process in-house in one of the buildings over on Foothill and try and reproduce their results and validate that, in fact, this could be done. And not only that, but we began hiring economists in those days. We hired Rich O'Toole, and we had Roger Noll from campus as our consultants. They would make an economic analysis of this process and see how it might fit into an overall photovoltaic economy. So we would pass judgment on the technical merits. We were sort of the disinterested third party that looked at the claims of contractors submitting contracts and submitting ideas to ERDA. And we were also in charge of looking at the overall economics and trying to jump start—

COHEN: Between JPL and the economists at Caltech?

COLE: Yes. The idea was to develop enough of an industry that eventually one could produce electricity at a cost-competitive price. The long-term goal was to be able to produce electricity for fifty cents a kilowatt-hour. Now, that still was much, much, more expensive than using coal-fired or oil-fired plants. But it was getting close enough that the economists could see that there were niche markets, remote operations, in places like Alaska—places where putting up long high-voltage transmission lines involved huge capital costs that made it uneconomical. That program really lasted, and I think the lab developed some valuable technology. There was really no photovoltaic energy in terms of the number of kilowatts or megawatts that were produced and sold. I think by the time that our association with that ended, which would have been in the 1982-to-1983 time frame, we were up to something like five to ten megawatts a year of manufactured and sold solar photovoltaic systems.

In addition to that, we worked on automated coal mining, developed something called the coal pump for moving coal fossil fuel around, worked on fuel conversion—being able to take biomass and chemically convert it to methanol or other combustible fuels.

COHEN: Were there any other Caltech people involved in this besides the economists?

COLE: Yes, I believe George Gavalas was involved in some of the fuel conversion work. Shortly before that, of course, Ford had given a grant to Caltech. This was because Mr. Iacocca was called up in front of Senator Muskie's committee back in probably about '73 or '74 and questioned about why Ford hadn't gone forward with more experimental types of engines that were claimed to be more fuel efficient. And in response to one of Muskie's questions, Iacocca said, "Senator, Ford Motor Company will put up a half a million dollars a year. And we will ask a government laboratory, an outstanding place, to do a technical study of what engine would be the best choice for us to improve the efficiency of our automobiles." And JPL got the contract. In addition to that, Ford decided to put up a half million dollars for long-range energy-related research at the Caltech campus. That was while I was still back at Ford. And I was tapped because I was a Caltech alum. I was the official Caltech liaison person. I got tapped to sort of administer that, along with John Baldeschwieler in chemistry, who helped me make the contacts here and helped to decide who were the deserving people to get it.

The new engine study was actually done at JPL before I came out here permanently, but it was something that I was at least involved in at Ford and I was knowledgeable about.

COHEN: So who actually got the contract on the campus?

COLE: Well, let's see. Several people, because it was rather like the President's Fund in terms of size. I think George Gavalas had one. Lee Silver had one, because he was studying the occurrence of uranium family elements—why they are where they are—and in those days we were still thinking that nuclear power was going to be an important component of the overall energy economy of the country. I think Professor Clayton [??] got one. And I think there was one to Harry Gray, because Harry was interested in the chemistry of solar energy conversion. I think we gave out about six or seven out of this half a million dollars.

COHEN: And that was supposed to continue for some years?

COLE: It actually continued for two years. It was in parallel with the new engine study. I think the new engine study, looking back on it, probably had very little impact eventually on Ford. I think the study at JPL was done with good intent and very good engineering insight, but what they recommended essentially was a mix of Stirling engines and diesels, as I recall.

However, I think the Achilles heel of the study was that there were really at JPL at that time no people who understood manufacturing engineering. Although for the industry of the country, manufacturing engineering is very, very important, there are very, very few people who are qualified manufacturing engineers. They generally are trained within corporations. They started out perhaps as mechanical or electrical engineers. These are people who can look at a potential product like a new kind of engine and tell you—well, if you bought fifty acres in a cornfield in Iowa, here's how much it would cost you to build the plant, here are the number and kinds of workers that you'd have to hire, here's how much it's going to cost to manufacture this. And they can tell you whether this design is manufacturable, how many stations are in the production line, and eventually come up with a cost per engine.

JPL didn't have manufacturing engineers. So their recommendations made a lot of sense, you might say, looking at it from physics and thermodynamics: Yes, these engines at least on

paper appeared to be more efficient. However, when you looked at the problems of manufacturing them, which the company did, they essentially said, “Look, this is a study which is mostly theory. And the theory only takes us partway. We have to be able to make and sell these engines at a profit—or sell these cars at a profit. And the problem with these choices is that we see currently no way to manufacture them in an economic way.”

So the study was useful, but I think to some extent flawed, just because the lab lacked, never having had to manufacture—we don’t mass manufacture spacecraft—

[Tape ends]

TERRY COLE**SESSION 3****October 30, 1996****Begin Tape 3, Side 1**

COHEN: I think we're about to start talking about your permanent residency here at JPL and Caltech. What year are we talking about now?

COLE: 1980. I think July 1st of 1980 was my first official day on the job at JPL as the technologist for what was referred to as E&TA—which was Energy and Technology Applications. That was a part of JPL that was housed over at the corner of Foothill and Altadena Drive. There were five large business and light laboratory buildings that were rented by JPL to do work appropriate to the then-perceived energy crisis. And JPL's work centered really in two areas. One was solar energy, both solar photovoltaic (solar cells) and some solar thermal—taking solar energy, concentrating it, heating a fluid, and making electricity.

There was some work that was also carried on on things like improving the fuel supply by looking at biomass: How could you turn scrap wood, wood chips, and that sort of thing into methanol and use it as a fuel? There was some work trying to help the automation of coal mining. That was a relatively small part of the effort.

The photovoltaic program, overall, was I think probably on the order in those dollar days of at least \$50 million a year. It was a substantial program. And JPL really led the photovoltaics the way later the Solar Energy Research Institute has been leading them. In those days, the DOE [Department of Energy] did not have a Solar Energy Research Institute. And JPL, because of its experience in using solar cells on spacecraft, was perceived as being one of the national centers of excellence—although those words weren't used very often in those days—centers of excellence in photovoltaic technology. JPL's assignment was to create a solar photovoltaic industry.

COHEN: Are you in charge of this project?

COLE: No. Throughout my career at JPL, I've studiously avoided any managerial assignments at all. I did my managing career at Ford, had some fun, but then I decided after several years that I could work better in what I like to call a catalytic mode. So I'm an advisor and I try to get new things started, try to do a little matchmaking and a little planning. And if I can wedge it in, to do a little research in the lab.

COHEN: Yes, but given the nature of JPL, you had to have a group. I mean, there is a hierarchy there.

COLE: Oh, there is a hierarchy. The E&TA branch, or program, at JPL was under the direction of Bud [Harris] Schurmeier. Bud, an extraordinarily cordial and warm person, had been, I think, a Caltech graduate in aeronautics and had worked at JPL since, I think, the days of, and perhaps even before, *Explorer*—late fifties, early sixties—and had risen through the ranks. He had managed a couple of programs. And I think he was one of the early project managers on the *Voyager* project. So he was in charge of this whole photovoltaic effort. And I essentially reported to him as a technologist. I was really introduced to JPL—my introductory colleague was Bob Mackin. We were not quite officemates; we were down-the-hall officemates for about two years when I was in E&TA.

Now, overall the technology at the lab had an advisory structure, and JPL did have a chief technologist at that time, who was John Pierce. John was a Caltech graduate back in the thirties. He had a very distinguished career at Bell Labs and had more or less invented the communications satellite while he was at Bell Labs. Then he came back here, and I think he was professor of electrical engineering and communication [John Pierce was professor of engineering, 1971-1980—ed.]. And then going into what would have been his third career, he went to JPL as the chief technologist in more or less a planning and strategy function, supporting Bruce Murray, who was the director of JPL at that time. He had a group of technologists from various program offices or divisions. We used to meet on a regular basis and try and at least coordinate planning activities and maybe catalyze one another. John, of course, subsequently, has gone off to his fourth career at the music school and computer science school at Stanford. And he is working on developing a thirteen-tone scale for music.

So our role in E&TA was to not just do R&D. In fact, the research part of it was rather a small part. There was quite a bit of development. And the idea was, in order to develop an industry you had to do more than do long-range basic research on silicon or other materials as solar cells. You needed to actually inspire and catalyze the private sector. So a big part of our task was to act as more or less an honest intellectual broker. Private firms such as Siliconics or Union Carbide, or one of the companies that were participating in the largely government-subsidized solar program at that time, would develop, let us say, a new process for refining silicon to lower the price to make single crystal solar cells. JPL would often let such a contract so that the DOE money would pass through JPL. But in addition, we were supposed to be the honest broker who could validate. We had to have enough equipment and enough skilled people at the Foothill facility so that we could, at least in small scale, replicate the process. We could do it in our lab as an independent check on whether in fact the claims made by the proposing company were valid. That was interesting, because I think for the first time that meant that JPL had to do real economic analysis. We had Roger Noll, and we hired Rich O'Toole—I think he's also a Caltech grad—both with economics as their starting point. And we even continue some of that expertise today, because now the cost of missions is a real driving factor. So we have to do some consideration.

COHEN: But you had no in-house economists to do all this?

COLE: We don't have a real economics group. At that time, I would say with Roger, who was here on the campus, and Rich O'Toole, and some others—there were enough people, perhaps three or four people, that you could say we had a small group who could do economic and production cost analysis.

During that time, on the side, I was doing some energy research of my own. I think I spoke previously about an energy converter, which was called the sodium heat engine, which we retitled at JPL the alkali metal thermal electric converter [AMTEC] to give it a nice, mellifluous acronym. AMTEC is about to fly sometime early next year—the first space flight for this new energy converter. So we got a small amount of money from NASA and started a very small, essentially one-person, total of one work year in 1980, working primarily with Perry Bankston,

who is now a section manager. It's actually C. Perry Bankston, for Clyde, but he doesn't like the name Clyde.

In addition, I was collaborating with Ahmed Zewail on the luminescent solar concentrator, which I spoke about earlier.

COHEN: So you proceeded with this, among other things.

COLE: Both of those were topics that I was pursuing as my own scientific interest, doing some publication and working with graduate students.

COHEN: So you had graduate students?

COLE: Well, in Zewail's lab, Joe Perry was one of them. Joe is now a group supervisor at JPL. Albert Highe was another one of the graduate students. He went off to industry, and I think he still works at Raychem, a plastic company in the Bay Area.

COHEN: How much time were you spending here on the campus?

COLE: In those days, since I was very close, it was probably thirty percent of my time. I had an office actually in the subbasement of Noyes, and worked closely, as I said, with Ahmed. This was after, as I mentioned before, I had worked closely with Bob Vaughan. Then, of course, in 1979, while coming back from a conference at Lake Geneva, Wisconsin, Bob Vaughan was killed tragically in the DC-10 crash in Chicago. In fact, I gave him a ride to the airport that day. There was a conference on ionic conductors—the basic material for the AMTEC. Bob had gotten very interested in doing NMR [nuclear magnetic resonance] of those materials. In fact, that's what I had done in collaboration with him when I was out here in '76. Bob had given a paper at the conference, and we were going back. I was going back to Detroit, because I was still in Dearborn. And I dropped him off at the airport, along with Satish Khanna, who works at JPL and had worked at Ford. Bob wanted to get back in a hurry to Los Angeles, and so went on standby for the flight that crashed. Meanwhile, Satish found an old classmate of his from the University of New Delhi; they decided to have an extra beer, and he missed the plane. I believe

Caltech lost also Dick Schuster [Caltech Development Office's director of foundations], and there may be one other.

Back to about 1982, at the beginning of the Reagan years, the prospect for energy research changed significantly, because the Reagan Administration didn't believe that there was a long-term problem and began to cut back the funding on energy research. Actually, that lasted probably until 1984 or 1985, but it began a slow reduction over the next four years.

COHEN: And that affected you directly?

COLE: Indirectly it did. It certainly affected the lab directly, because our funding started to drop off. Also at that time, Congress had decided that there should be a Solar Energy Research Institute. Ground was broken in Golden, Colorado, and space was leased. And some of the work began to migrate in that direction. We had worked during that time pretty closely with Sandia National Laboratory, which also had a role in energy conversion—a fairly substantial one, especially in solar thermal. They had a very large solar thermal test facility, a huge field of mirrors, probably 300 meters square. And I actually did an astronomy experiment there.

We took one of the luminescent solar concentrators that's good for concentrating poorly focused light and put it at the focus of the solar thermal test facility. We did this at night. We focused on the Crab Nebula and tried to look for pulses essentially of high-energy cosmic rays coming into the upper atmosphere and producing flashes of Cherenkov light. This was not a very good focusing telescope. The spot of a star would be about a meter in diameter at the focus. But we had enormous area—something like 8,000 square meters of glass. It was the equivalent to *all* of the optical telescopes on earth.

COHEN: Did any of the astronomers here become interested in that?

COLE: Well, actually Trevor Weekes, who is an astronomer at the Mount Hopkins Observatory, was my collaborator. He was the one who was doing the high energy. Some of that work still continues, although not at Sandia. The solar tower proved to be just too erratic in its tracking to be even a good light bucket. But it was fascinating to be up on the top of this 200-foot tower and

focus on a star. To be able to read newspapers by starlight—if you pointed it at a very bright star like Vega. An interesting sidelight.

Anyway, back to JPL. Bruce Murray resigned in 1982. And in 1982, then-General Lew Allen, retiring as chief of staff of the air force, came on board as the director. Shortly after that, John Pierce decided to retire again and enter a new career at Stanford. And Lew Allen decided that there should continue to be a chief technologist at JPL. And so he asked me to come up to the Oak Grove facility and become the lab's overall chief technologist.

So I took over running the division technologists' meeting. And we continued with what became known as the Technology Board, where we'd look at the long-range planning and opportunities to be catalysts.

COHEN: So you were back, in some sense, in management again.

COLE: Back in management, or at least planning and catalysis. I was able to continue my work with the energy converter—AMTEC. But it became a little bit more difficult to continue the collaboration with Ahmed, although shortly, I think early in 1983, we managed to recruit Joe Perry to come to JPL. And he did continue some of that research, and is still working on modifications of laser dyes even today with Seth Marder, who's a member of the Beckman Institute and also is a JPLer.

In any case, probably the most interesting thing that happened in the early eighties was on July 23rd, 1983. Lew Allen attended a meeting of the trustees' JPL committee, which was held in Washington. That meeting was a joint meeting with Burton I. Edelson. Burt Edelson was at that time the associate administrator of NASA for space sciences. Space sciences is the part of NASA that sponsors most of the JPL-NASA work.

Prior to that meeting, [Caltech trustee] Mary Scranton had written a letter to Edelson, essentially posing the question: Is there any role in addition to its work exploring the solar system by robotic means, its overall mission—is there any new facet of this mission that JPL could take on as a new enterprise? Edelson later told me that he gave very serious thought to this letter while on a fishing vacation in New England. And toward the end of the meeting he said, “Yes, I've been giving a lot of thought to this letter. I would like to see JPL take an initiative to become a microelectronics center of excellence for all of NASA. NASA doesn't

have any one of its centers with a specific role to be good in microelectronics. Each center does have some sort of a role—Goddard is observing the earth, Ames is aeronautics, Johnson is a manned space flight center, etc.”

When he came back from the meeting—I think it was the end of the first week in August—Lew called me in and said, “Burt Edelson has proposed that we might get into microelectronics as a center of excellence. I’m not sure I agree with this at all. In fact, I know that we are not very good in this area. We don’t have a lot. But I’d like you to take some time, scour the lab, and report back to me. Could we do this? What would it take to become a true center of excellence? What kind of investment? What kind of hiring would we have to do? And what’s a rough outline plan?”

So I began talking with some of my friends. And by that time a very old colleague of mine from Ford—one of the early members of the group that became known at JPL as the Ford Mafia—John Lambe, had come to JPL. He was one of the fellows who had worked on inventing some of the superconducting devices at Ford. He was then working on some surface physics at JPL. One afternoon, John Lambe and I spent the whole afternoon in his office, essentially with feet up on the desk, or at the blackboard. And we essentially designed in that one afternoon what became to be known as the AMP—Advanced Microelectronics Program—which later became the Center for Space Microelectronic Technology. That’s now an \$80-million-a-year program at JPL and I think fairly widely known. It has heavy collaborative interaction with faculty members here and with other national labs and industry.

Essentially, what we decided was that we needed to define what we would be unique in. We decided that it was clear that we couldn’t compete with industry in producing computer chips or memory chips. We weren’t going to compete with Motorola and Intel. It was senseless to do that, because we were forbidden—since we’re a government-sponsored lab—to compete with industry. And the Defense Department was investing billions of dollars in improving conventional electronics.

But we decided there were some areas where NASA really did have a problem. As an example, the submillimeter wave. It would be very interesting to have a submillimeter wave observatory in space, because the atmosphere doesn’t transmit waves between a tenth of a millimeter and a millimeter in wavelength at all well. In fact, it’s practically black. If you could put a telescope—sort of based on the Hubble Telescope philosophy—outside the earth’s

atmosphere, you could observe this range of the spectrum, which is very interesting, especially to chemists, because most of the low-molecular-weight gaseous molecules that are in interstellar clouds throughout the universe emit radiation—a very characteristic radiation. So that by looking out there, you can do analytical chemistry. What is there? How much of it is there? What's the pressure? And what's the temperature?

So here was an area where one did not have detectors. There were no good detectors that you could use if you went out there. Going out there, it seemed as though one could do that. Launch vehicles were available but there were no detectors. It was known, however, that in principle you could use weak junctions of superconductors to make detectors that would look in that wavelength.

We said, in addition to that, that indicates that there are areas of the electromagnetic spectrum where detectors are either inadequate or nonexistent. JPL goes into outer space, and we're a remote sensing laboratory. Our job is to go to the solar system and also to look outward beyond the solar system in all wavelengths and gather information by gathering photons. So let's make sure that we have a high level of expertise in detector technology or, as it came to be called, focal plane technology. What do you put at the focal plane with a telescope? Or a camera?

COHEN: Now, you didn't have anybody working in this area at that time?

COLE: We did. We had some people. There was a section. I think it was called the Electronic Materials Section at that time. Because of the photovoltaics, we had hired a number of solid-state physicists. We had Frank and Paula Grunthaner. They are both Caltech graduates. Frank is a Harry Gray student, and I think Paula might have been Jim McCaldin's student. John Lambe had come from Ford. About that time, we were about ready to hire Bob Terhune—no relation to Charles Terhune, who was the deputy director of the lab at that time. We had Henry Stadler. In fact, Satish Khanna had worked at Ford—he'd gotten his degree in conducting polymer materials at the University of Pennsylvania. So we had a small cadre of people—Joe Maserjian, and others. These were good people who were publishing, who were recognized by their colleagues as being substantial people.

At this time, Carver Mead had developed the whole idea of custom microchips. You didn't have to take what were produced by the hundreds of thousands or millions by industry but you could in fact design your own. Custom chips required specialized software and workstation computers. So that was the second facet of this AMP program—to transfer that technology from what was emerging from academic research and just beginning to be used in industry. And this gave JPL the capability to do that. That was more or less bootstrapping ourselves into an area.

COHEN: Did Carver Mead talk to you about this?

COLE: Oh, yes, yes. As time went on, during August and September of that year, I spent quite a bit of time. I talked with Amnon Yariv, Tom McGill, Jim McCaldin, Carver Mead.

COHEN: So there seemed to be a very close relationship with Caltech on this particular thing?

COLE: Oh, yes. There was a lot of consultation. There were people on the faculty, some people who were quite skeptical that JPL would be able to pull it off, because they looked at us as a project lab; we did not have at that time a strong, long-range research reputation. And they thought it would be difficult to hire qualified people. And of course we had no facilities to speak of, other than what we were using as part of the solar photovoltaic. There were some very primitive solid state physics silicon laboratory facilities. They were many years out of date—and out of date comes in two or three years in this field.

A third area that we thought was interesting was the area of photonics—that is, eventually using lasers for remote sensing, doing something like LIDAR [Luminescent Intensity Detection and Ranging], where you could do chemical analysis with lasers. But also eventually moving up in frequency to use lasers for interplanetary communication. Jim Lesh had already begun a small effort in laser communication and had worked, in fact, with John Pierce, to at least demonstrate in the laboratory that you could send a bit of information with less than one photon. Which sounds as though you were cheating nature, but in fact it can be done.

And finally a fourth area came about because in I think it was 1982, shortly after he published his very important paper in the *Proceedings* of the National Academy, we invited John Hopfield to give a talk—a Technology Board seminar—at JPL on emergent collective

computational behavior, which came to be known as neural nets. So we got John very early, and we got him talking to some of the people at JPL. In fact by the end of 1983 John Lambe had built the first working Hopfield neural network, essentially out of Radio Shack parts. He said, “That’s a very interesting theoretical concept. I think I can build one of those.” So we were very early in the neural net game.

And about that time, the team of [Geoffrey] Fox and [Charles L. (Chuck)] Seitz had produced a very novel device called the Cosmic Cube. The Cosmic Cube was sixty-four IBM PC computer circuit boards tied together with a communication system. And Fox and Seitz were able to demonstrate that this computer, which probably cost a few thousand dollars if you added up what all the parts cost, not counting the graduate student labor, could outrun a half a million dollar DEC VAX minicomputer. So it looked as though it was quite a bargain—difficult to program, but nevertheless if you had very intelligent people, like Caltech graduates students, you could do some surprising things for a little amount of money.

The story, as I understand it, is that Fox and Seitz at that time thought that the Cosmic Cube, which really sat on the top of a desk with no cover over it and fans blowing on it to keep it cool, really wasn’t the kind of machine that you wanted to make available to even the Caltech public. You had to sort of nurse it along. And it really wasn’t an easily usable machine. Their idea was that new chips were coming out in the commercial field all the time. And the whole idea here was to take commercially mass-produced silicon chips and compete with much higher-cost minicomputers, and eventually supercomputers, by having a team of computers working all on the same problem simultaneously. It’s now called parallel, but Geoffrey used to like to call it concurrent processing, because of the particular way problems were attacked. And some degree of parallelism was already available in most supercomputers.

In any case, they wanted to build a larger version, put it in a nice cabinet, have a monitor screen and a keyboard, and make it look more like a commercially produced machine. I believe they talked to Robbie [Rochus] Vogt, who at that time was the provost, about getting some extra campus effort and a little money. And I think in substance they were told that Robbie thought this was an inappropriate activity to pursue on the campus with graduate students. For them to reengineer a working piece of equipment, when we had JPL—a well-known engineering and development laboratory and a part of the institute—was inappropriate. He would first like them

to go to JPL and if JPL no-bid the job, then he'd consider it. But he really thought that JPL ought to be involved.

Actually some of the people who had been working with your husband [the astronomer Marshall Cohen] on the famous correlator—Dave Rogstad in particular had been helping him to design and construct the correlator and was very familiar with digital computer design. These were specialized computers for doing correlation of astronomical radio astronomy measurements. Rogstad was called in along with some of the other people in our telecommunications division, which is generally called by its number—Number 33—to at least look at the possibility of building what evolved to be called the Mark II parallel computer.

So at the beginning of the AMP program, we had four branches. We had the custom VLSI [Very Large-Scale Integrated circuits]. We had microelectronic devices—really, focal plane devices. We had photonics. And we had parallel computation, which for us meant hypercubes and neural networks.

So that was the beginning. We did a lot of planning. I worked with [E.] David Hinkley, who was then in the program office at JPL that did technology. It was called Technology and Space Program Development, TSPD. John Lambe, as I said, was key in terms of helping the plan. Satish Khanna was another one of the very active people in developing the concept, and worked on the financial planning with Fred Felberg [JPL associate director].

So I had developed a plan. I went back to Lew Allen and presented what was a prospectus, essentially. And he said, “Well, you better present this to the executive committee. And we're going to have to sell the rest of NASA. Because although Burt Edelson may support this”—and eventually he did, after we presented it to him—“the Office of Space Science really doesn't have a charter within NASA to develop technology. That's the Office of Aeronautics and Space Technology that has its own associate administrator, and they need to be on board. In addition, we need to see if we are good enough that we can get sponsorship from even outside of NASA.”

The JPL management, except for Lew Allen, was not receptive to this idea. It was something that didn't exist. JPL had always been able to obtain from private industry, either as a commercial product or as a special order, all of the electronic needs. In fact, there was a culture that said, “We don't fly anything in JPL's spacecraft unless it's obsolete—has been used *so* long that there is a really complete statistical history of how this technology works. It essentially has

been worn out, beaten up, and tested in so many different ways that we're very confident that we're not going to lose a billion-dollar mission because of, as they say, for want of a nail the battle was lost." The dreaded single-point failure.

So my presentation at the executive committee did not meet with cheers. There were many questions: Why are we doing this now? We've always gotten along without it. There's no money. NASA isn't sympathetic to this. Still, we did pursue. We persevered.

As part of the planning, I worked closely with Satish Khanna when it became clear that there wasn't enough laboratory equipment. There was not a good laboratory for doing these things, especially the work on detectors that really was high-tech solid-state research. And we decided that there was some equipment that we might be able to inherit through interagency transfers from the then-decaying solar photovoltaic program, that might serve as a basis, but it clearly wasn't adequate. And we really needed a building. We needed a first-rate laboratory with clean rooms, with the various lithographic wet chemical analyses. And the capability to create semiconductor materials and semiconductor devices, essentially starting with the elements, doing the necessary formulation, then carving them, patterning them, lithographing them, and building complete devices.

So we began laying out the plans for what eventually became the Microdevices Lab. That's one of the buildings currently at JPL.

COHEN: So you did get a building built?

COLE: Yes we did. There was at least a year of planning. And then finally Lew Allen and I went back to NASA headquarters and talked to I think it was General Billie McGarvey, who was head of construction facilities for NASA, as well as the NASA deputy administrator—I think it was Dale Myers at that time.

I made the technical presentation. And essentially the deputy administrator of NASA turned to Lew Allen and said, "Lew, do we really need this?" And Lew said, "Yes, we really need this. It's important for NASA." And he said, "Done." [Laughter] I think Lew Allen carried a tremendous reputation with him when he became director of the laboratory. And he was so well known in Washington that his word was really solid platinum.

COHEN: That was a real coup that JPL got him, wasn't it?

COLE: Yes, it definitely was. He is a person of great vision, as well as deep technical knowledge. I think he tremendously surprised all of the managers at JPL, especially in the first few meetings when he reviewed a program. People at JPL talking at the upper levels of management would generally show bulletized graphs with very little technical content—mostly budget and scheduling content. And at almost all of these meetings, Lew Allen would ask a deep technical question—so deep that you'd think he was reading the current technical literature. This often caused great fear and trembling on the part of people who had been out of touch with their discipline, often for quite some time. But Allen was, and is, very deeply and intimately interested in technology. So it was a great pleasure to work with him throughout the period when he was director, because he was so extremely knowledgeable. And also, because I think he had a very clear strategic vision as to where things were going on a national and even an international scale and how JPL could be positioned to strategically leverage what are still relatively modest resources compared to the amount of electronics and advanced technology work going on nationally.

COHEN: When did your interest in students at the campus begin, or was this always going on?

COLE: Well, it was 1983. That was when Fred Shair was running the Summer Undergraduate Research Fellowship program—SURF—which was invented by Fred and Hal Zirin in 1978. And it was Fred's conception that Caltech was a small enough place and the Caltech spirit should allow undergraduates to begin research . . . [tape ends]

Begin Tape 3, Side 2

COLE: In 1978, Fred Shair and Hal Zirin conceived of a way that undergraduate research could be reinstated at Caltech. Now, I say "reinstated" because, beginning about 1920, Professor Ernest Swift for most of the twenties gave undergraduates in chemistry an opportunity to work with him in his research. So it really was Ernest Swift who inaugurated undergraduate research. But then during the thirties, and especially after the Second World War, the tradition faded away.

Fred, because he's an avid tennis player and had a chance to repeatedly lose to Ernest Swift [laughter], heard about this. I think it was one of the things probably that led to his reinvigoration of this idea. And together with Zirin in 1978 he started a program with eighteen students and, I think, seventeen faculty who acted as research advisors.

The program certainly didn't have all the features that it now has. But the idea was that students would have a chance to consult with faculty members to decide on a project. It was a collaborative enterprise. Of course, the faculty member would essentially be the expert and would act as a guide. But still the student should have an active role in choosing the research project. Then, for ten weeks during the summer, the student would work under the direction of the faculty member and would be paid for doing research. At the end of the summer, the student would write up a report on work that was performed. And eventually—I'm not sure when it actually started—it was decided that in addition, when the fall term began, there should be a time when the students gave a report orally. They should present short seminars on their work.

COHEN: Where did the money come from?

COLE: The money came out of individual research monies of the faculty. There was no infrastructure and no support other than the research grants of the professors. So it took real dedication and commitment, because this was hard-earned research monies that were going to be spent. So I think that was one of the things that has kept the SURF program close to the basic research intent with which it started out—that this is real research. It is not simply doing set pieces, where you read how the experiment is to be done in your lab book, and then you go into the lab and do it, and maybe you get the results that the book says, and maybe not. In this case, it would be a chance for the students to go into the laboratory where there are no set problems. In fact, one of the challenges is to even frame the question.

In about 1983, in talks with Fred, the idea arose that there were a number of undergraduate students in engineering who probably would like to work at JPL. JPL represents much more the environment that engineering students would encounter if they went out into industry or a large government laboratory—less of the academic atmosphere. And engineering, of course, is a very popular option.

So Fred brought this up in one of our conversations. And I went to talk to Lew Allen about it. And Lew thought it was a great idea and decided that we could probably arrange to do this at JPL. We would take steps so that JPL didn't charge all of the overhead on this. And we would pay full SURF salary, plus any support charges that were required. We could pay the students out of our own direct money, coming from either NASA or one of the other government agencies. So we began, I think, with about half a dozen students in 1983.

COHEN: Now, did a professor on campus have to sponsor them?

COLE: No. In this case, we decided that there were people of sufficient stature at JPL. They essentially were thought of as part of the Caltech research community. The decision was that I would read over the announcements. We still do it slightly differently at JPL than at campus. At JPL, we put out a call to the JPL professional staff, saying that it's now time—it's December—it's time to write announcements of opportunity, or AOs. AOs are the NASA acronym for announcements of research opportunity. NSF [National Science Foundation] does it slightly differently. So we're used to seeing announcements of opportunity. The idea is that the JPL staff, since they don't have day-to-day contact through classes with the undergraduates, needed to be able to make public their areas of expertise and what areas they would be interested in advising a student on over the summer. So they write up one- or two-page announcements of opportunity. We send those to the SURF office at campus. And there they were initially bound in a loose-leaf notebook—now they're put on the World Wide Web and various bulletin boards.

COHEN: Now, tell me something about these things. If an opportunity arose, did that come with a person? That is, did it mean that if you went there to do this, you would be working with this particular person?

COLE: Oh, yes, yes. For instance, Mike Janssen does high-energy astrophysics. He works on cosmic background radiation. If he were interested in having a SURF student, he would write up an announcement detailing the analysis of COBE data—Cosmic Background Explorer mission data—that he was interested in having a student work on. He'd outline this briefly, one page, and we'd send that to the SURF office. An astronomy major might see that. Then Mike

Janssen's phone number, mail stop, and e-mail address would be attached to this announcement. It's up to the undergraduate student to show the initiative to get in touch with Mike Janssen, spend an hour or more with Mike Janssen, understand in much more detail what it is that the project involves, possibly do some reading. And by that time, SURF had evolved so that we required a real research proposal. The student would have to learn enough about it that he or she could write a cogent one- or two-page proposal, narrowing down from the announcement of opportunity specifically what the plan for the summer is. Those proposals are then submitted to the SURF office, but they are then reviewed, because there's an administrative committee consisting of at least one faculty member from every division, plus JPL. Those faculty members read all of the many submitted proposals and look for intellectual content. And they also look at recommendations about the student. The student has to bring forward a little bit of background—obviously, what's your academic record, what courses have you taken, but also a letter of recommendation from at least one or two faculty, or recent RAs [research associates], who know them and their work habits and can say, "This is a very hardworking student." So the administrative committee puts those pieces of evidence together with the written proposal and decides which students will get to do SURF.

Now, in some cases, there is an endowment for the SURF program.

COHEN: Let's go back to the initial JPL So you proposed this and Lew Allen thought it was wonderful. The money was forthcoming.

COLE: Yes. We set up an infrastructure at JPL to be able to get the announcements collected and get them to campus and to get the students. I think we started initially with about five or six students. Currently, we run between forty and fifty students. That part of the program at JPL gets the bulk of our non-Caltech students. In the middle 1980s—I'd have to look at my notes to find the date—the faculty administrative committee decided to open the program to non-Caltech students. Partly it was because David Van Essen, who was a professor here at that time, had gotten a grant from NSF to bring minority students. The program became MURF—Minority Undergraduate Research Fellowships. It was for biology, and for him specifically, to bring in minority students from outside of Caltech to do work in biology. That suggested that we should consider opening the program to students from other universities, which we did toward the end

of the 1980s. It turns out that because JPL staff members have to prepare written announcements of opportunity that are now posted on the electronic media, when people call in or dial in over the Web to find out what the opportunities are at Caltech for SURFing, they see forty or fifty JPL written announcements. They see relatively few announcements written by Caltech faculty, because the Caltech faculty have class time and office hours to make those personal one-on-one contacts. There's a continuous venue for contacts, whereas for students at a distant university, outside of the easy driving range, there's no such intimate contact.

We do have students coming on the campus. I think also JPL has a worldwide reputation among engineering students because of its endeavors in space exploration that probably brings in a number of students who are interested really in aerospace engineering. Where there's relatively a small program in that area on campus, there's a huge program at JPL.

COHEN: I remember when for one or two years Ann Boesgaard, the astronomer, was here. I don't know if she was part of SURF, but she brought women from some of the sister colleges on the East [Coast]. She felt they should be here. Was that worked into this program then?

COLE: Well, I think the program has been co-ed from its very inception. The program now, in fact, has come full circle. Julia Kornfield is now [associate] professor of electrical engineering and she was one of the early SURF students.

COHEN: So were you in charge of this whole thing at JPL?

COLE: I ran it as an assistant, or an adjunct, to the campus program, which was run by Fred Shair and Carolyn Merkel. I think Carolyn's been associated with the program from 1978, from the very beginning, and now of course is the SURF director for campus. Fred left Caltech in 1985 or 1986 to go to Cal State Long Beach.

COHEN: Let me backtrack again. It seems to me at one time you told me a very good story about how many Indians you had working that you had gotten from Flagstaff. Is that right?

COLE: Oh, yes.

COHEN: Were they SURF students?

COLE: This was a program that was essentially a NASA program to help Native American education at Northern Arizona University, which is at Flagstaff. I think they have either the most or the second most Native American enrollment of any university in the United States—primarily because of their location, virtually in the Navajo reservation.

JPL had for a number of years a program of outreach under Gil Yanow—part of our Educational Affairs Office—to go into the Navajo reservation with some JPL engineers and help with high school education. For instance, help Navajo teenagers to learn how to build short-wave radios. Communication in those days was, and in fact still is, a sometime thing on the Navajo reservation. And there weren't cell phones. And there weren't citizen band radios. So the idea was that through projectization, you could teach something about electronics, a valuable skill, and also that meant that the Navajos could have some short-wave communication to call in, let's say, medevac helicopters for medical emergencies or other kinds of emergencies.

So we had contacts with the Navajo tribal council and also peripherally with Northern Arizona University. We negotiated a NASA grant to NAU to help bring Native Americans—mostly Navajo because of the geographic location—to JPL to work. I think the program ran for three, maybe four, years.

We titled the students who came under that program as guest SURFers, primarily because that gave the Native American students a context. Here were people the same age. Since they were SURF students, they could have dorm rooms in the student houses. It made an association. By being part of this program, they could engage in the supplementary, extra programs that SURF runs. We give a course on public speaking. We give a course on “Can you do research for a living?” “How do you balance research and a family?” “What kinds of jobs can you get in industry and government labs?” So there are a number of additional programs—enriching programs—that go along with SURF. And we thought it was appropriate that the students—and especially the Native American students, who probably hadn't had the chance to get that kind of information—be allowed to participate.

COHEN: So why did it run only three or four years?

COLE: Well, partly because I believe there was some mismanagement of funds on the part of Northern Arizona University. I don't think there was anything criminal. But I think NASA was very disappointed with, shall I say, the lack of attention that the NAU administration paid to the program. Especially in terms of getting very good students and preparing them for what for them was going to be a very, very challenging experience. Because mostly these young people had never been off the reservation, or only peripherally off the reservation. They had never really had much of a laboratory experience, or certainly a research experience. So it was something that was very, very challenging for them. And you were putting them in a situation where their lack of preparation—which was no fault of their own—put them in an awkward situation. And I think that, together with probably the lack of putting really aggressive entrepreneurial people from NAU on the program, eventually led to its demise, unfortunately. I think it was a good program, but it was a program that took special skills. It had to be tailored in a different way than for Caltech undergraduates, because the Caltech undergraduates of course have been in this very competitive atmosphere. They're highly selective. They generally have superbly prepared themselves, or have had the opportunities for superb preparation. And these young people didn't have any of that.

COHEN: But SURF, of course, is blooming.

COLE: SURF continues. We're now sort of asymptotic at about 220 to 250 students. It has proved very popular nationally—so much so now that there's a national conference on undergraduate research that's held roughly during spring break every year. And the attendance now is of the order of 2,000 students.

COHEN: Now, is this because other institutions have adopted this program?

COLE: Other institutions have adopted the SURF model—or the MIT UROP model. UROP stands for Undergraduate Research Opportunities Program.

COHEN: Now, did this happen before or after the Caltech program?

COLE: It happened before. And it's a bit different, because UROP students actually get academic credit. It's more a part of the real academic program of the school. We decided early on not to have SURF for credit. First of all, you can't get paid for anything that you get academic credit for, under the rules of the institute. And second of all, we wanted the students to be able to select something unusual. If it were for credit, there would be subtle and not-so-subtle pressure—you better do a research project in your major, because it looks good on your record and it will help getting a job, or getting into graduate school. By having it not for credit, somebody who's a math major but knows somebody in biology, can think, "Gee, my friend is telling me some very interesting things about what's being developed in molecular biology these days. Maybe I can do a SURF this summer and learn what research in biology is really all about. Maybe I can apply my math skills there. But I'll find out about another area that I had not done and wasn't aware of." So I think that gives SURF an extra flexibility. And I think it's more beneficial than a for-credit program.

The administrative committee decided to limit the non-Caltech enrollment to twenty-five percent. We want it to continue to be a Caltech program. We could probably have 300 or 400 non-Caltech students. We have an enormous number of inquiries now. We now have applications from Russia and Eastern Europe—essentially worldwide applications.

COHEN: So how much of your time goes into this?

COLE: Well, SURF is a busy time during the summer. And I probably spend, during peak periods, especially when we're getting the proposals in and reviewing them during the summer, ten percent of my time. Averaged over a year, it's probably no more than ten percent of my time, working on SURF. But it's time very well spent. I enjoy it exceedingly.

COHEN: And that puts you on the campus a great deal.

COLE: Yes.

COHEN: So now what are you doing?

COLE: Well, now I continue as chief technologist. We're continuing to develop the AMP program, which has now become the CSMT [Center for Space Microelectronic Technology]. And within the last year, I guess the whole idea of centers of excellence has finally caught on. There are now plans in the works for, at least, ten centers of excellence to be formed. The microelectronics center would be one of them. And I am currently running a search program for the leader of a center of excellence on in-situ solar system exploration and experimentation on the planets.

COHEN: Putting somebody up there. How's it going to be?

COLE: Well, we've done essentially all of the flybys of all of the major bodies in the solar system, and quite a few of the minor bodies. So we've been there, done that. We're now in the era of orbiters. We had the Magellan orbiter doing radar mapping of Venus. We've got Galileo in orbit around Jupiter. We'll have the Mars Global Surveyor around Mars by summer of 1997. And we'll be launching Cassini to be an orbiter around Saturn. So we're well into the orbiter phase. The next logical step is to land with a robotic survey instrument package.

COHEN: Now, this is completely unmanned?

COLE: Our role traditionally for JPL has always been the unmanned exploration of the solar system and the universe beyond. And I think we have the charter from NASA to explore any other solar systems that we find. We now, I believe, know about more than nine planets outside of the solar system. And that, of course, is another area of intense development at JPL. There really are two new centers of excellence. One is in interferometry, under Mike Shao. And that's to build and eventually fly an interferometric telescope with resolving power sufficient to actually image an earthlike planet around another star.

In addition, we have this in-situ center of excellence, where we're trying to develop in-house an expertise in building instruments that are miniature chemical laboratories and miniature geochemical, geophysical laboratories.

COHEN: I think your interest always goes back to the chemistry.

COLE: [Laughter] Yes. We're enjoined by the management to do more than just instruments, because traditionally we know a lot about flying by or going into orbits. But now we have to think about how we land on a planet safely, whether there's an atmosphere or no atmosphere. Eventually, we'd like to bring samples back. And that means thinking about ways to get back off of bodies. If it's a very light body, like an asteroid, it's relatively easy. But a heavy, large body like Mars requires that you have another rocket there. It's expensive to bring fuel all the way from Earth. So we have to think about how you produce a propellant locally from indigenous materials. How do you launch when there's no Cape Canaveral, get into orbit, possibly transfer samples, and bring them back to Earth?

COHEN: Do you wish to say something about your interaction with faculty at Caltech and scientists at JPL?

COLE: I guess that's been one of the areas that I've tried to concentrate on. I think when I came to JPL there were probably on the order of a dozen collaborations—mostly in the area of planetary sciences, because of course it started really with Bruce Murray, Peter Goldreich, Andy Ingersoll. But there were relatively few, if any, in other areas—chemistry, engineering.

The last tally we did, I think in 1993, we totaled up 150 collaborative enterprises between members of the JPL staff and something like 120 of the 270 faculty members. So there were nearly half of the Caltech faculty. And these range all the way from biology, some in economics, a lot in engineering, and a lot associated with the Microdevices Laboratory and the CSMT. A number of the young faculty members we take pride in, I think, would not be here had it not been for the presence of this superb laboratory, because the facilities at the Microdevices Lab really are facilities on a national scale. The only other comparable laboratory, really, is the Cornell Microfabrication Facility. So this is a facility that's good on a national scale and it exceeds, really, anything that's available on campus.

COHEN: So there are professors here that are doing—

COLE: Kerry Vahala, Dave Rutledge, Demetri Psaltis. A number of people, of course, in the neural network field—Harry Atwater and Alex Scherer, to name people off the top of my head.

COHEN: Who are doing their research in . . . ?

COLE: Well, they're doing a significant fraction of their research in that facility and usually collaborating in some way with one of the staff members.

COHEN: Now, are there any problems, because graduate students can't do work there if there's clearance necessary?

COLE: The barrier right now is financial rather than clearance. There are certain restrictions about foreign nationals. But usually we can clear those up, provided that the person is always working essentially in the presence of a JPL staff person—even foreign nationals. Which means they can't go up there at three o'clock in the morning when there's nobody there. But for other people, there really is no security restriction. I think the amount of classified work going on at JPL is probably less than one percent of our total cash flow at present. And anyone who has a faculty or a student card can essentially gain admission to JPL. So people can come up freely and come in and work.

The real barrier is, the building cost \$19 million, and we have about \$20 million of equipment in it. And that equipment becomes obsolete about every three or four years. So maintaining this facility.... Clean rooms take tremendous maintenance. All of the advanced equipment becomes obsolete. It's costly, and somebody has to pay the bill. So there's a heavy overhead charge for using that equipment. Initially, it looked like it was going to cost about \$40,000 per graduate student per year—prorated, of course, on the amount of time they used the facility. Partly that was because there was what looked like double charging on overhead between the campus and the lab. We have gotten the campus to not charge overhead, so we're down now to only \$26,000 per year. We're hoping to persuade NASA to pick up half the overhead charges so that we can get it down to \$13,000 or \$14,000 a year. So it's a financial barrier. If the collaboration is a close one—that is, if the faculty member is actually working on a NASA grant or contract—then we can put that overhead, the operating charge (it's really not

so much overhead as specific operating charges at the lab) into the budget and take care of it up front.

It's a going program; we continue to work in that area. We're now spinning off our own companies. Within the last year, we formed Photobit, which—Who knows?—may be the new Intel. Eric Fossum is one of the people that we recruited about six years ago from Columbia.

COHEN: When you say a spin-off, has he curtailed his work with JPL and set up his own business?

COLE: Yes, he and his wife, Sabrina Kemeny, were both working at JPL, and while at JPL Eric invented a new kind of imaging device called the active pixel sensor—APS for short—that I feel will eventually replace the CCD [charge-coupled device], which is the imaging device in all camcorders and, in effect, in all electronic imaging. The visible is done with CCDs. CCDs are power hungry; they're expensive. They're expensive to make. And this invention is essentially a way that any semiconductor manufacturer who manufactures memory, the commoditized random access memory, can now make imagers.

COHEN: So they set up their own business?

COLE: Sabrina resigned from JPL two years ago and incorporated as Photobit in La Cañada. And as soon as they are ready to go public, I want to buy their stock. They have gotten exclusive license. Caltech got the title to the patent.

That's one of the interesting things. Lately NASA's view of intellectual property has changed significantly, in that when an invention is made at JPL, quite often NASA will assign title to that patent. They will not try and commercialize it themselves; they've learned that it's almost impossible for the government to commercialize anything, because if it's owned by the government it's owned by everybody, and so there really is no proprietary thing. People want a patent because it gives them a license to be a monopoly—at least for a while. So, many of the inventions are now being assigned to Caltech, and Caltech is rapidly developing a commercialization program. So Eric and his wife Sabrina now have exclusive licenses for, I think, eight patents that have to do with these active pixel sensors.

COHEN: And then what happens? Does the money come back to Caltech?

COLE: Caltech will get royalties. So Sabrina is the CEO [chief executive officer], and Eric is director of research.

COHEN: Is this the first of these companies?

COLE: Well, in the past, JPL spun off Aerojet, which became Aerojet General. That was people who left the lab early.

COHEN: Yes, but JPL and Caltech didn't benefit from that.

COLE: No. There have been other people who have gone off to set up companies. But I think this is probably the first where there was an overt [decision] to say, "Yes, this is something that everybody knows about." It wasn't done sub rosa. It wasn't done because people got angry and left. This was something that the lab management knew about, Caltech administration knew about. It was essentially a blessed activity. So we're looking forward to Photobit growing. They essentially have, I think, the key to economical picture phones. This device will allow a camcorder to run a full week on a battery charge, instead of a couple of hours. And Kodak has a contract with Photobit. AT&T has a contract with Photobit. Definitely on your watch list for your stock list.

JPL and the JPL-Caltech connection continue to be an interesting place to work—full of personalities. We didn't have a chance to get in all of the personalities.

COHEN: Is there any closing statement you'd like to make?

COLE: Since I've been here, I've been lucky enough to do what I find fascinating and fun at just about every point. So I'm looking forward to continuing with these activities and looking forward to the discoveries that some of our in-situ instruments and probes will make in the future. And perhaps branching out the lab's technical expertise in some areas.

Another recent initiative is to try and form an alliance with the Brain Research Institute at UCLA [University of California, Los Angeles], to bring some of our parallel supercomputing expertise to displaying the kind of physiological imaging information that John Mazziotta and Arthur Toga are developing over at UCLA.

COHEN: How about what's going on here?

COLE: In terms of biology, we have a collaborative enterprise with Barbara Imperiali, to take some of her research on short peptide chains and make a biological chip where you use the selectivity of the biomolecules, you attach them to a silicon chip and to transistors on the chip, so that in water solution you can read out the concentrations of ions floating around, or even other biomolecules.