CHEMICAL HERITAGE FOUNDATION

WILLIAM G. McMILLAN

Transcript of an Interview
Conducted by

James J. Bohning

in

Los Angeles, California

on

25 March 1992

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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> William G. McMillan, interview by James J. Bohning at Los Angeles, California, 25 March 1992 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0104).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

WILLIAM G. McMILLAN

1919	Born in Montebello, California, on 19 October

Education

1941	B.A., chemistry, University of California, Los Angeles
1943	M.S., chemistry, Columbia University
1945	Ph.D., chemical physics, Columbia University
1946-1947	Guggenheim Fellow, University of Chicago, Institute for Nuclear Studies

Professional Experience

1941-1944	Teaching Assistant, Columbia University
1944-1946	Research Assistant, Manhattan Project
	University of California, Los Angeles [UCLA]
1947-1951	Assistant Professor, Department of Chemistry
1951-1958	Associate Professor, Department of Chemistry
1959-1965	Chairman, Department of Chemistry
1959-1990	Professor, Department of Chemistry
1990-present	Professor Emeritus
1949	Visiting Professor, Columbia University
1951-1952	Carothers Visiting Lecturer, Harvard University
1954-1971	Senior Physicist, The RAND Corporation
1966-1968	Science Advisor to Commander, U.S. Military Assistance Command, Vietnam
1971-present	President, McMillan Science Associates, Inc.

Honors

1938	Lena De Groff Scholarship, UCLA
1938	Pi Mu Epsilon (mathematics)
1939	Paramount Pictures Scholarship, UCLA
1940	Phi Lambda Upsilon, (chemistry), UCLA
1940	Phi Beta Kappa, UCLA

1942	Sigma Xi, Columbia University
1957-1961	Alfred P. Sloan Fellow
1968	Distinguished Civilian Service Award, U.S. Department of the Army
1969	Distinguished Public Service Award, U.S. Department of Defense
1969	Knight of the National Order of Vietnam
1970	Exceptional Civilian Service Award, U.S. Department of the Air Force
1984	Exceptional Civilian Service Award, U.S. Department of the Air Force

ABSTRACT

William McMillan begins this interview with a discussion of his parents and youth in Montebello, California. The youngest of seven siblings, McMillan expressed an interest in science at an early age. He attended Montebello High School, where he was greatly influenced by his chemistry teacher, Leon Broock. After graduation, McMillan entered UCLA, receiving his B.A. in chemistry in 1941. In 1941 he attended Columbia University and there earned his M.S. in chemistry in 1943, and his Ph.D. in chemical physics in 1945. While working towards his Ph.D. degree, McMillan was employed in the Special Alloys and Materials Project, a forerunner to the Manhattan Project. While a post-doc at the University of Chicago, McMillan worked under Edward Teller. In 1947, McMillan joined the faculty of UCLA as an assistant professor of chemistry, and remains there today as Professor Emeritus. He became chairman of UCLA's chemistry department in 1959, and worked to implement more student programs and offices at the university. During his tenure at UCLA, McMillan also worked for RAND Corporation as a consultant to the U.S. military. He helped form the Group on Weapons Effects, which later became the SAGE Advisory that reported on weapons tests. McMillan also worked with the Armed Forces in Vietnam, developing concepts for artillery and military reconnaissance. After contracting hepatitis in Vietnam, McMillan researched the disease and developed a blood chemistry analysis. Some of his personal research projects have included: global warming and ozone depletion issues; atmospheric studies of Venus; and Neutrinos work. In 1971, McMillan developed his own consulting company, McMillan Science Associates. He concludes the interview with thoughts on the future of the military and defense budget, and an expository analysis of the structure of electrons.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and he has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

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INTERVIEWER: James J. Bohning

LOCATION: Los Angeles, California

DATE: 25 March 1992

BOHNING: Dr. McMillan, I know you were born on the 19th of October, in 1919, in Montebello, California, which I believe is a suburb of Los Angeles.

McMILLAN: That's right.

BOHNING: Could you tell me something about your parents and your family background?

McMILLAN: Sure. My father emigrated from Nebraska to California about 1890 or so. He was born in 1880, into a large family. He had half a dozen brothers and two or three sisters. I knew some of the brothers, but some of them had died already before I was born.

BOHNING: Why did they come to California?

McMILLAN: I guess because of the climate more than anything else, and to try to earn a better living. This was a period of no great prosperity. In any event, my father had done a number of different trades. As a boy, he used to herd the cows for the local community. After he came out here, I guess when he was seventeen or eighteen, he got a job as a roust-about in the oil fields, drilling for oil here in Los Angeles and then also in Fresno. This was at a time when they used the "standard rig," which was not rotary. It was just a tool line that was allowed to fall onto the rock repeatedly and break it, and they had to haul up the string of tools and muck out the broken rock. Then I guess after a few years of that, he acquired the trade of a carpenter and in a short while worked himself pretty high up in the construction business. He was superintendent of construction on some of the big downtown buildings—big in those days—the May Company and the Gates Hotel and a few similar large buildings.

My mother came from Texas, also of a large family. Neither of my parents had more than about three years of formal schooling, but my father particularly made good use of that. He always surprised me with things he knew that I didn't know anything about. He had a book on spherical trigonometry, for example, which he had studied on his own. He knew all kinds of simplified ways of calculating things quickly—the length of rafters and all the kinds of complicated things that enter into construction.

Then during World War I there was again a depression. He and the family lived in what is now South-Central Los Angeles, down around 52nd Street and South Park Avenue. There, with a growing family and a depression, he thought it best to move out into the country. He bought a small ranch east of Los Angeles out in Montebello, which was planted in a lemon grove. The lemons didn't pay off, except for the year of the flu during World War I, so it wasn't used much as a ranch, although we had cattle and horses, and grew a truck garden fairly extensively, but not much commercialized, only for our own consumption.

I was the youngest of seven children. The oldest child was my brother, and there were five girls in between. These were pretty hard times, I gather, although I had a wonderful childhood there in Montebello on the ranch—great freedom and animals to take care of. I think I took over the job of milking the cows when I was about six years old. It was a place where one could learn a lot about mechanics—repairing farm machinery, and all that kind of thing—so even though these were pretty tough times for my parents, I never was aware of that.

There was a local school, a grade school, about a mile away. There was no busing or anything like that. Everybody walked. In fact, in those days, people were a lot more self-reliant. There wasn't any welfare or any other kind of government assistance. In fact, people would have rejected any such help, I think, simply because it was not the thing to do. I went to grade school, Greenwood Grammar School, for seven years; I actually skipped second grade. Then I transferred to what was then Montebello High School and, there, really began to think about what kind of work I wanted to do. We had really a very good faculty. We had some excellent teachers.

There was one man who was particularly influential in my choice of career; his name was Leon T. Broock. He was a chemist and an excellent teacher, just a wonderful guy. Then there was also a fellow who taught mathematics, in particular trigonometry, by the name of George M. Schurr, after whom the new high school out there is named. He later became an administrator in the Montebello Unified School District. It's pretty autonomous.

My father was always quite civic-minded and was a member of almost every board and commission. He was always tied up in the evenings. He was a member of the Montebello Water Board, the Planning Commission, the School Board, and others over a period of twenty years. In fact, all the kids in the family received their diplomas at his hand when he was a member of the School Board. So, that was a pretty good start. I still have some of his books from the three or four years that he went to grade school in Nebraska. You'd be amazed. These are books that would be almost college level in this present era, very good textbooks.

I had a very happy childhood. I don't know how the tradition got generated, but by the time I came along in the family, there was a general tradition—we all did well in school; there wasn't any question about that. All of my sisters and brother got good marks, and that helped, of course, in getting off to a good start.

BOHNING: What about your brother? Had he gone on to college?

McMILLAN: Yes. He had gone on to UCLA, majoring in civil engineering, although he didn't finish. That was in the middle of the Depression years. A few years later I was lucky in that I had a summer job that paid enough during the summer to support me at UCLA. That is, my family provided room and board, but all the fees, bus fares, and that kind of stuff, I was able to pay myself.

BOHNING: You mentioned Mr. Broock as a high school chemistry teacher. What kind of chemistry experience did you have in high school? Was there a laboratory experience?

McMILLAN: Oh, yes. There was a laboratory as part of the course. It was not very elaborate. But the point was that Broock was an inspiring teacher, and that made all the difference. Each year at the open house, his students would put on special demonstrations: a high-voltage Tesla coil that could generate sparks a foot long, an apparatus for talking on a beam of light, etc.

BOHNING: Do you credit Schurr with your mathematics inclinations as well?

McMILLAN: Yes.

BOHNING: Had you given thought to going anywhere else besides UCLA?

McMILLAN: Yes. I'd thought of trying to get into CalTech. My brother had passed the entrance exam there, but it turned out he couldn't manage the fees, so he spent a couple of years immediately after high school in what was called the Polytechnic High School. Then he transferred to UCLA. Eventually I didn't apply to CalTech.

BOHNING: I'm not clear what years you started there at UCLA. Was it 1936 or 1937?

McMILLAN: It was 1936. Jack [John D. Roberts] and I spent five years as undergraduates. This was when the bright undergraduates would be hired as teaching assistants. There was no graduate school at UCLA in chemistry at that time. That began in 1941. It was a wonderful period because we had regular professors teaching the undergraduate courses rather than graduate teaching assistants. The better undergraduates were hired on as teaching assistants.

Moreover, any research that was being done involved the undergraduates, so there was a lot of interaction, especially for the brighter undergraduates, with the faculty—a kind of a golden age. But that changed rapidly when the graduate school came in.

BOHNING: Were there many chemistry majors?

McMILLAN: There were, I would say, maybe fifty in each year, something like that. Of course the place was growing. The total population of UCLA in those days was around eight or nine thousand.

BOHNING: What was your curriculum like? What did you start out with in chemistry?

McMILLAN: In college?

BOHNING: Yes, at UCLA.

McMILLAN: There was a general first-year course, 1A-B, the lecture part of which was taught by the senior professor in the department, who was William Conger Morgan. I've written all this stuff down for Bill [William A.] Benjamin. He threw a party for Jack Roberts, and I wrote about our early years at UCLA. I can probably get you a copy of that.

BOHNING: Oh, sure. That would be excellent.

McMILLAN: It tells about those early years. There were only about a half-dozen faculty members at that time in the chemistry department. The department was dominated by Professor Morgan, who had been brought up in the Germanic school where, of course, der Herr Professor was the only professor in the department. Besides, his chemistry was fairly ancient, so that was interesting. A lot of it was new to me, even though looking back, it was probably not a very modern course. Then the second semester of that course was qualitative analysis, involving the separation and identification of ions in solution following the approach of A. A. [Arthur Amos] Noyes. That was very good as far as establishing the concept of equilibria, various ionic processes and so on. That was taught by Francis E. Blacet, who was then a relative newcomer to the department, a very able guy. He was a photochemist.

In the sophomore year, we had quantitative analysis, which was taught by Bill [William R.] Crowell, a very rigorous course. All of these courses had laboratories associated with them.

This course involved not only the ordinary chemical type of quantitative analysis—precipitations and titrations—but also involved electro-chemistry.

Then in the junior year we began organic chemistry. Again, Professor Morgan was the teacher. That course was pretty much old hat—not old hat to the students, but it wasn't modern chemistry. At that time there were added two new faculty members: Tom [Thomas L.] Jacobs and Ted [Theodore A.] Geissman, both youngsters essentially, fresh from post-doctoral work. They brought with them quite a bit of new organic chemistry. Bill [William G.] Young had been added earlier. You may know Bill Young; he was from CalTech. He taught an advanced course in qualitative organic analysis, a very exciting course. Robbie [G. Ross] Robertson, was one of the older original group of the department. He had written a very nice little book on organic laboratory operations (1). He was just an excellent teacher, but he didn't do much research in those days. It was altogether a very good faculty.

Then later on Charles [D.] Coryell joined the faculty. He was a young Turk. That was the only exposure I had as an undergraduate to anything closely resembling statistical mechanics and quantum mechanics. When I got to Columbia University, where I went as a graduate student in the fall of 1941, all of quantum mechanics and statistical mechanics was essentially new to me. It just opened up a new kind of field that appealed to me, because it involved not only the chemical issues but also dealt with mathematics, which I was interested in.

BOHNING: I want to just make a comment about Bill Young, because Jack Roberts said, "Mac and I went over to Young's house on a Sunday to have him make more unknowns."

McMILLAN: Yes.

BOHNING: So, you two must have been a very aggressive pair?

McMILLAN: Oh, yes. We had lots of fun.

BOHNING: How would you describe UCLA's facilities at the time? They didn't have a graduate program, but with all the new people they were bringing in, it looks like they were aiming to establish that sort of thing.

McMILLAN: Oh, yes. That was very much in mind. I think it was in 1938 that these two new organikers came in. The graduate school was established in 1941, so it was in the mill at that time. Also Morgan, who had dominated the department up to that point, had had a stroke, so he was out of the picture by about 1939 and Bill Young became chairman. Bill was, of course, a very aggressive and active guy, not only in the department but also in the American Chemical Society [ACS].

BOHNING: You didn't get your real exposure to physical chemistry until very late.

McMILLAN: Well, we had a very good course in physical chemistry at UCLA. The lecture courses were given by [James] Blaine Ramsey. Ramsey was an old-line thermodynamicist, but he was an excellent teacher, I thought. Not everybody agreed with me. He had a way of suffering through each one of the derivations or problems, sort of *ab initio*. He'd bull through it with the class, and he'd expose his thought processes while he was going along. But his exams were frightful.

One of our feminine colleagues was Gabriele Hamburger. She later became a crystallographer and married one of the eminent crystallographers. I remember on one exam, which might have been the first one in this course, the problems were just utterly impossible. I think I got the highest grade, which was fifteen out of one hundred. [laughter] A miserable number.

In the p. chem laboratory we had Jimmy [James D.] McCullough, who was a fairly young guy, and Charles Coryell. At that time there wasn't sufficient equipment to do individual experiments, so we paired up. I was paired with Jack Roberts. We invented our own experiments, and we were turned loose. That was a very fine thing for us. They tolerated this kind of adventure, so we did lots of unusual things there.

BOHNING: You and Jack had a publication with Coryell (2)?

McMILLAN: Yes.

BOHNING: Which I think resulted from that experimentation?

McMILLAN: Yes. That's right.

BOHNING: It was on dithionite ions?

McMILLAN: Right. Thermodynamic properties of the dithionite ion.

BOHNING: So, that came out of that kind of experimentation.

McMILLAN: That came out of our p. chem laboratory work. In fact, in that publication appears the first discussion that I know of concerning the solvated electron.

BOHNING: I think I have it here, as a matter of fact.

McMILLAN: The solvated electron is in a footnote, I think.

BOHNING: Did you and Jack write this, or did Coryell do it?

McMILLAN: We wrote the first draft, which Coryell then enthusiastically edited. [laughter]

BOHNING: Were there a number of things like this going on? Were the faculty really publishing a number of papers with just undergraduates at that time?

McMILLAN: Oh, yes. In fact, Bill Young had a number of undergraduates working with him. Both Jack and I worked with him later on. Saul Winstein had come through under Bill Young's tutelage as a master's candidate. Then he went off to get his Ph.D. and came back to join the faculty. There was a fair amount going on, actually. It was a good atmosphere, particularly for the undergraduates, because any research that got done was done by them in conjunction with the faculty. Jack and I also had a paper with Tom Jacobs (3) on the dipole moments of acetylenic ethers, which grew out of our undergraduate physical-chemistry laboratory work.

BOHNING: Where along the line did you decide it was going to be physical chemistry instead of organic? Certainly for you there was a large exposure to organic?

McMILLAN: Yes. Well, in doing research with Bill Young, there was a lot of reaction kinetics, which is one aspect of physical chemistry. Actually, I had gone to Columbia to work with [Louis Plack] Hammett, but when I arrived at Columbia, Hammett was off on war work.

BOHNING: This was in 1941, wasn't it?

McMILLAN: In the fall of 1941, yes—before the war, but while the war in Europe was already going on. Hammett was unavailable. I guess I was really grateful for that in a sense, although I certainly could have learned a great deal from him. He was an eminent guy, a wonderful fellow whom I later came to know. At Columbia there were two people particularly, George E. Kimball and Joe [Joseph E.] Mayer, who influenced me. As I indicated earlier, this just opened up a whole new field. I didn't even have any prior awareness of quantum mechanics and statistical mechanics.

BOHNING: At UCLA you were still sort of looking at physical organic chemistry?

McMILLAN: Yes. Exactly.

BOHNING: The idea to take the fifth year—did you and Roberts agree together to do something like that?

McMILLAN: Yes, more or less. You see, these were still economically depressed times. I wanted to have a dual major in chemistry and math, but it turned out I needed one more course in math, in projective geometry, for example. I signed up for that, but then a job came along, a teaching assistant job, which conflicted in time. I guess the economic pressures were too great.

Another thing that entered was that Ramsey's physical chemistry was a senior course at that time. It had a reputation of being a killer. The year before we took it, Ramsey had about fifty students, and he gave half of them F's and D's. [laughter] He was a formidable taskmaster. Not that they didn't deserve it, but he didn't quibble about things like that. He didn't care what the administration thought.

BOHNING: These were seniors already?

McMILLAN: That's right. Anyway, I suppose partly in deference to that threat, we deferred taking the physical chemistry course and instead took some courses in physics. I remember we took Kinetic Theory of Gases from Professor Adams, one of the old timers in physics. We always had lots of other things going on. We had the research with Bill Young. We had another paper together with Tom Jacobs on acetylenic ethers (3). Jack and I had various complementary capabilities. He fancied himself a good glass blower, and I knew something about electronics, and had fiddled around with radios and such. I built a Hartley oscillator, still kind of a nifty scheme. There was a capacitor in the tank circuit that determined the frequency of the oscillator, and a cell containing the dielectric liquid constituted the capacitor we wanted to measure. This capacitor could be replaced—by switching—by a standard capacitor, which we had liberated from physics—borrowed. [laughter] Then we played this oscillator—this was in the radio frequency range—against one of the standard radio stations, and we got a beat frequency. The frequency could be tuned precisely, and switching our cell capacitor in and out with the standard capacitor until there was no change in the audible frequency indicated identity of the two capacitances, which could then be read off the calibrated standard.

[END OF TAPE, SIDE 1]

BOHNING: I noticed that you used station KFAC 1300.

McMILLAN: Yes.

BOHNING: You had indicated that in the paper. The other thing that I found curious was that at the very end you said, "Further study will be done when more time and better equipment are available." [laughter] Better equipment I could see, but the more time part I thought was interesting.

McMILLAN: Yes, well, at one critical point, what we had done was use what Jack had built, a double-walled glass cell. We wanted to build this in such a way that we could use liquids that would have been corrosive to metal electrodes. We were able to plate the outside and the inside-inside surfaces of this double-walled glass cylinder with silver. Then, to protect the silver, because it was easy to scrape off, I over-plated the silver with copper. Well, it turned out we couldn't do that without greatly increasing the stress. When the copper plating got too thick, it just crushed the whole cell. We learned a few things about materials.

BOHNING: Did you pick Columbia specifically because of Louis Hammett?

McMILLAN: Yes. Well, actually, that was with Bill Young's advice. I had also applied to [University of] Wisconsin and got accepted to both places. Yes, Louis Hammett looked like the best person I knew about who would combine the physical/organic area. When I got to Columbia, there were a bunch of placement exams. There was a laboratory course there in physical chemistry that Jake [Jacob J.] Beaver, who was a professor in physical chemistry, insisted that everybody take. Well, I didn't want to take another p. chem lab, so I took him my notebooks from the laboratory that we'd had at UCLA. He impressed me at first as a kind of gruff, no-nonsense guy, but after he read those notebooks, he became very friendly and hired me as his T.A. That was a wonderful experience, because not only was the laboratory fairly well-equipped with modern spectrometers and other instruments, but Joe Mayer was assigned as Jake Beaver's assistant in there. After getting the students started on their experiments in the early afternoon, there wasn't much to do. I'd get Joe Mayer up to the blackboard and week after week, he tutored me in statistical mechanics.

BOHNING: One-on-one?

McMILLAN: One-on-one.

BOHNING: What a great way to do it.

McMILLAN: Oh, you bet, and Joe Mayer was a marvelous teacher. I've just been lucky all the way through.

BOHNING: Jack Roberts said that you and he went across country with Blacet?

McMILLAN: Yes. The first trip across country we went to the Atlantic City meeting of the ACS [American Chemical Society]. That was a big thing. We sat up all the way on the train, miserable!

BOHNING: Was that your first ACS meeting?

McMILLAN: Yes.

BOHNING: I was going to ask earlier, when you were here at UCLA, how much exposure you may have had to other chemists besides the UCLA group.

McMILLAN: Well, there was a weekly seminar, which brought in occasional outside speakers, but it was pretty much confined to the local area. There was no lack of things to do, however. This is the account that I wrote for Bill Benjamin (4). [indicates manuscript] It describes that whole period of our undergraduate work, and Jack, of course; this was written for the celebration for Jack Roberts.

BOHNING: It looks excellent.

McMILLAN: I had some trouble with Bill Benjamin's editor, who wanted to change all the little subtleties.

BOHNING: I hope you won out.

McMILLAN: Oh, yes. I wouldn't let them change a damn thing. [laughter]

BOHNING: That's good. That'll be an excellent background for those UCLA years.

McMILLAN: I've started something that might interest you. I have had so many wonderful friends. Every now and then one of them dies off, and I haven't told him or her the things that I felt. So, I started a little campaign of doing that, writing a few people to tell them my appreciation of their actions. I now have a notebook full of such letters.

BOHNING: Yes. If you'd care to place anything like that in our file on you, it would be certainly appreciated, because it is those interactions with individuals that have such a great influence on one's life.

McMILLAN: Well, let's see if there's anything else of interest here. I can go through and give these some titles, or for that matter, there's no reason you can't have these.

BOHNING: Well, as I said, we can leave it up to you as to what you care to share with us.

McMILLAN: I wrote something for Joe Mayer on his retirement (5). I wrote an obituary for Blaine Ramsey (6). There was a dedication of the x-ray laboratory at UCLA to Jimmy McCullough after he retired, and I wrote an account of that. There are quite a few things of that sort, also letters, a letter to Edward Teller, and one to General [William C.] Westmoreland.

BOHNING: Yes, I want to talk to you about Teller. Whatever you feel free to copy and send to us will be fine. Was Mayer young at the time he was at Columbia?

McMILLAN: Yes, he was about thirty-five, I would guess. In 1941 he had been brought to Columbia by Harold [C.] Urey. Harold had been at Johns Hopkins when both Joe and Maria [Maria Goeppert-Mayer] were there. Then, when Harold was hired at Columbia, he brought Joe (and Maria, of course) along. Joe was editor of the *Journal of Chemical Physics* and taught mainly statistical mechanics and that p. chem lab that I mentioned. Maria had a kind of *ad hoc* appointment in physics. Columbia was a pretty lively place in those days, because [Enrico] Fermi was there, and Edward Teller in physics, as well as [Isidor I.] Rabi and quite a few other people. Willis Lamb was there, of the Lamb-Rutherford shift. He was the guy on my Ph.D. oral committee who asked me the only question I couldn't answer. [laughter]

BOHNING: How did you find your math background from UCLA when you got to Columbia?

McMILLAN: Very good. I had taken a lot of math at UCLA, not just the ordinary calculus courses, but what was called "analysis" in those days. There was a man who was then chairman of the math department here, his name was William Weyburn; he later became provost at one of the eastern colleges. I've forgotten which one. He taught a course that used the textbook by [Edouard] Goursat (7), a French mathematician, and went into all kinds of multivariables: Jacobians, Wronskians, the whole bit. That was very good. Then at Columbia there was a course taught in physics by Professor [Harold W.] Webb, on mathematical physics.

I don't know whether Bruno [H.] Zimm is on your list to interview, but he ought to be.

BOHNING: Yes, we've talked to him (8).

McMILLAN: Have you?

BOHNING: Yes.

McMILLAN: Well, he and Paul Doty and I were working for Joe Mayer simultaneously. Paul was often off in left field; he was interested in high polymers and biochemical molecules, but Bruno and I were genuine chemical physicists. We took not only Webb's course, but also a course in mathematical physics, which was given by Edward Teller. In this course of Webb's—it was a course essentially in Fourier analyses of all kinds, with very extensive problems—Bruno and I, out of the ten or so students, were the only two who finished the course. The physicists couldn't hack it. [laughter]

BOHNING: You had a teaching assistantship, too, at the same time?

McMILLAN: Yes. I was a teaching assistant in the phys. chem lab.

BOHNING: Physical chemistry?

McMILLAN: Yes, under Professor Beaver.

BOHNING: That's right. You said that was after you showed him your notebooks.

McMILLAN: Jake and I became very good friends. He was a wonderful guy. He taught, also, a course in Phase Rule.

BOHNING: Of course, the war started. Pearl Harbor was the first year that you were there.

McMILLAN: That's right.

BOHNING: What kind of an effect did that have?

McMILLAN: The draft was already in motion. I had a student deferment. Well, let me start more at the beginning. The fall semester had started in September 1941, and one of my courses was from Harold Urey in molecular spectroscopy. He had a very highly organized way of teaching. All of his notes were carefully written out in a notebook; he'd derive these hairy equations, with a kind of formality that intimidated the students not to ask questions. At any rate, he disappeared along about October, and the course was taken over by Edward Teller. The lecture room had a big demonstration bench in front. Edward came in, climbed up and sat on the bench, saying, "Now what'll we talk about?" The contrast was fantastic. Edward was a very inspiring guy. I came to know him well.

The immediate effect of the war was that the issue of the draft was always foremost. At one point, I went down to Marine headquarters in New York to volunteer. Of course, I took all their exams, but Bill [Willard F.] Libby and Walter [H.] Stockmayer both were at Columbia and really became annoyed with my wanting to go off and become a flyboy instead of working on scientific things. Somehow or other they prevented the Marines from accepting me as a volunteer.

At any rate I was assigned to some work in what was beginning to be the Special Alloys and Materials Project, SAM Project, which was the forerunner of the Manhattan Project. This explained what had happened to Harold Urey—he had been pre-empted by negotiations with the Brits to form this joint program, so he was tied up a lot. During that Christmas recess I built a thermal-diffusion column. It ran up about four stories in Havemeyer Hall. Then, periodically, I worked part-time on some of the special schemes for the separation of uranium isotopes, although as a student I was never told what we were doing. One of these involved the fractional solubility of uranyl salts partitioned between water and ether—I often got involved in that kind of thing.

Then after I had my final doctoral oral exam—this was now 1944—the project had grown enormously. I took a job under Bill Libby, determining some of the adsorption characteristics of the UF_6 process gas.

BOHNING: So, you were doing these projects, essentially doing some work on the Manhattan Project as a student, but at the same time you were doing a Ph.D. thesis?

McMILLAN: Yes.

BOHNING: When did you make the selection with Mayer to work on that?

McMILLAN: Oh, that was early on. My first year at Columbia was devoted to courses in thermodynamics, quantum mechanics, statistical mechanics and getting over the qualifying exams, but already at that point I had decided that I wanted to work with Joe Mayer, and we decided on a problem.

BOHNING: This was the rather thick document that appeared in *Journal of Chemical Physics* (9).

McMILLAN: Yes.

BOHNING: I've forgotten the year now.

McMILLAN: It was 1945.

BOHNING: It was on multi-component systems, published in 1945?

McMILLAN: Yes.

BOHNING: How did you and he interact in developing all of this? Were you on your own or did he give you advice?

McMILLAN: Pretty much on my own. You see, he was at Columbia only on Mondays at this time. He was working on internal ballistics down at Aberdeen Proving Ground for the rest of the week. He would show up on Monday, at which time he had to interact with all three of us—Bruno and Paul and myself. Otherwise we were just on our own. That was a good thing though, to generate self-reliance. [laughter]

BOHNING: Did the three of you work pretty much together?

McMILLAN: We were in the same laboratory.

BOHNING: Paul was off doing different things, I guess.

McMILLAN: Yes, well, at that time he was working on electron affinities of the halogens, which was another one of Joe Mayer's interests. These were sort of continuations of things that he had started. Bruno was working on the enthalpy of vaporization of some of the alkali-halide salts. I also had an experimental part to my research, which didn't get published. It had to do with the two-component system, water and trimethylamine, in which I'd been looking at the critical point.

BOHNING: All right. Because I was going to comment that yours was certainly more theoretical than the other two would have been in that sense; you were off into statistical mechanics at this point.

McMILLAN: Yes. One of the interesting things there was that a critical piece of that development was made by the notorious Klaus Fuchs.

BOHNING: Yes.

McMILLAN: Fuchs passed through New York, after I had finished that part of my thesis. I had been really dazzled by the things he had done in inverting a Jacobian, and Joe had an evening party for him. This was another thing about Columbia—they had these frequent gatherings where with any bigwig coming through, they would throw some kind of a cocktail party or something, and the graduate students would get invited. I met Fuchs there, and I asked him about this. “How did you happen to do this?” He said, “Oh, I was on a two-week vacation and tossed it off.” [laughter] He was a fantastic mathematician.

BOHNING: But he didn’t stay at Columbia very long?

McMILLAN: No, he wasn’t at Columbia. He was just passing through, on his way to Los Alamos, where he did his mischief.

BOHNING: I was struck by one comment in your dissertation. You said, “It will be necessary to adopt a system of symbols that are sufficiently complex in nature to make the resulting equations appear relatively simple.”

McMILLAN: Oh, yes. In fact, that has become a point of philosophy with me. I didn’t realize that that was in there. This is a book that I’ve been writing for years. [indicates manuscript] Incidentally, when I got kicked out of my office when I retired at UCLA, all this stuff, books and boxes and boxes, came from over there. I don’t have access to anything anymore. I don’t know where anything is.

BOHNING: I understand that feeling.

McMILLAN: “As for generally seeking to describe the interrelations of physical quantities, it is often better to keep the equations few and simple, at the expense of a somewhat greater sophistication in the symbol.”

BOHNING: Yes.

McMILLAN: Nature is essentially complex, but we have a choice as to where to put the complexity. Here the complexity is in the symbol, but the equation is simple. That allows us to think about it, instead of having to look at these separate things that are also dependent upon some arbitrary breakout into an artificial coordinate system. It’s much better to do it that way. I didn’t realize that that was in there.

BOHNING: Where does this part come from?

McMILLAN: This is a set of lecture notes that will go into a book eventually.

BOHNING: All right.

McMILLAN: But it's the way to do vectors and the way to do analytic geometry, incidentally.

BOHNING: Very interesting. So, you were already establishing that concept very early on when you were working on this?

McMILLAN: Yes. There's another thing in there that's sort of interesting. When we talk of osmotic pressure, and this was a thing that Joe pointed out—I probably can't find it now, but it says if the world were full of evanescent particles that filled all space and all our solutions and everything else, then even in the case of an ideal gas, where we're measuring the pressure, that would be an *osmotic* pressure, because of the presence of these other particles.

Now, let me get off on a different subject that's related to this. I'm kind of rambling.

BOHNING: That's fine. Excellent.

McMILLAN: Have you ever heard of the Gravitational Theory of [George-Louis] Lesage [Jr.]? It turns out that almost everybody discovers this for himself, and at first it sounds like a great discovery. It's just the kind of thing that a physical chemist would think of. Suppose we have a gas of hard sphere particles, which therefore have only contact interactions, no long-range interactions. We now ask, "What is the force between two particles?" We're going to measure the force, and this will illustrate the difference between the actual potential and the potential of average force.

I have two particles that I'm going to focus on, and I imagine I have some kind of a gauge where I can measure the force between these, holding them fixed. Then, over the time average, we have all kinds of collisions with other particles, but we don't get symmetry of collisions because the particles coming from this direction hit this one and not that one. Therefore, the two spheres are driven together. Now, when we look at the forbidden volume where any particle coming in that cone is going to collide with the other particle, then we see that this is something where this area or the angle goes like $1/r^2$? Right?

BOHNING: Yes.

McMILLAN: Simply because we've excluded that forbidden area. So, the solid angle goes like $1/r^2$.

[END OF TAPE, SIDE 2]

McMILLAN: When we now calculate the rate of momentum transfer, which is the force, we find that the force between these two particles goes like an inverse square. Now, we can imagine a theory of gravity, in which we have all these little particles running around that we can't see; matter is almost transparent to them with their tiny cross section. I call these particles "newtons." [laughter] So, one can invent in this way a kind of theory of gravitation. Now it turns out that there's a great difficulty with this, because we'd like to connect it up to the equivalence principle that says that the mass, which appears as the generator or the source of the gravitational field, is the same as the inertial mass. Okay? Now, conceivably, that would happen if we had this sea of newtons moving in all directions, and a particle intersects more of these on the front side than the back side. The only trouble is, that's a velocity-dependent thing, not acceleration. [laughter] Anyway, this theory goes back to about 1750, I guess, to a fellow by the name of Lesage. I didn't know about his earlier work at the time I re-invented his theory.

BOHNING: The value of history again.

McMILLAN: Yes, right.

BOHNING: You list being officially involved with the Manhattan Project from 1944 to 1946.

McMILLAN: Yes.

BOHNING: So you completed this work then. Your thesis was out of the way?

McMILLAN: Yes.

BOHNING: You got your degree in 1945. Can you tell me more about that period? The war was actually coming to an end.

McMILLAN: Well, we didn't know that.

BOHNING: That's true. In 1944 it wouldn't have been obvious.

McMILLAN: Right. There was general feeling on the part of the Columbia administration that the war was going to end, so at the end of 1944—oh, I should preface this. The Columbia part of the Manhattan Project was carried on in several buildings on the main campus—mainly in Pupin Hall, which was the physics building, but also in Schermerhorn Hall, which was where some of the engineering was going on. Columbia decided to get out of that business at the end of 1944, and Union Carbide took over as manager. There was a big garage building—formerly the Nash Garage building—up Broadway at 135th Street. So, Columbia "sold us up the river" to Union Carbide. [laughter]

Then things happened rapidly. In May of 1945 the European war came to an end, and in August the first atomic bombs were dropped in Japan. Again, this was an exciting period, because those associated with that project—Harold Urey had been there, and Ray [H.] Crist and Bill Libby, and Edward Teller was coming through periodically.

BOHNING: How aware were you? You said that as a graduate student you were told, "Just do this."

McMILLAN: Yes.

BOHNING: How did your awareness of what was really going on develop?

McMILLAN: Well, when we'd think about what they were telling us to do, we'd figure it out. In one case, we had a set of instructions—sort of do this, do that, do the other thing—in this business of looking for the separation factor in the preferential solubility. The instructions were wrong. If we had followed them, they would have gotten results they couldn't interpret, because instead of adding up to 100 percent, the things added up to 90 percent. They just made a goof, but it was easy to figure out.

Later on (i.e., in early 1944) we would have regular research meetings in which such senior people as Bill Libby would explain what we were trying to do.

BOHNING: So did you move up the river then?

McMILLAN: Oh, yes. I went up there. In fact, I stayed on until the last dog was hung, writing the history of that part of the Manhattan Project.

BOHNING: Was that classified information?

McMILLAN: Yes. A two-volume history, which Bill Libby always kept in his safe and moved around with him. He still had it when he died. It's still classified.

BOHNING: That was something that came up in a conversation that I had with somebody else recently, and that is how much of that time period is still classified. We're in an age where lots of things are becoming public, but it seems that a lot of that still is not.

McMILLAN: Well, you know, there's a problem. The government hasn't solved the problem of classification. Right now, I'm interested in a study that was done for the Army back in 1970. I was a member of the Army Science Advisory Board, and we had a summer study trying to look twenty years ahead for what kind of equipment the Army would need and get ready. There was a group in the Army, a big group, maybe a hundred people, doing work on this, and they wrote a very fine report in which they looked at something like three hundred countries. They had quite a mix of capabilities. This was a military operation looking at the national objectives of the country and what their problems are—what are the potential conflicts—and then describing these in terms of climate, terrain, political considerations, the whole smear. Well, that would be very useful to something I'm doing now for the B-2 problem, but that stuff is still classified "secret." What's worse, if we're going to get something declassified, we've got to go to the originator. But there aren't any originators left. That agency is long gone, all dispersed, so it's just on hold. There's no way of breaking that out. I think that's what's happening with a lot of stuff.

There are some real lessons to be learned, though. Those guys, Bill Libby and company, really had courage because they designed that whole K-25 diffusion plant before they had any diffusion barrier. In other words, they took a gamble that they were going to be able to develop that barrier. In fact, about three months before it was due, we had three different kinds of barriers, all of which would work.

BOHNING: How soon did you receive your security clearance? As a student you didn't have one, I'm sure.

McMILLAN: No. I don't know what the system was in those days. I think maybe Nancy [my wife] would know; she's got records on all that stuff. I involved her in typing up that two thousand-page history.

BOHNING: Well, it's nice to know it exists, even if no one may ever see it.

McMILLAN: Well, there's a lesson there in that business of doing the research simultaneously with the plant design. You know, today it takes ten years to get any weapon system in operation, but, by God, we had stuff coming the top end of that K-25 plant in about three years.

BOHNING: Yes, it is a remarkable achievement.

Edward Teller. You interacted with Teller, and I know after the war there were a number of papers that came out with you and Teller (10).

McMILLAN: Yes.

BOHNING: Where did you first meet Teller?

McMILLAN: In that course that I told you about where he took over for Harold Urey. Teller has an absolutely unique physical understanding that is really hard to come by. I credit him with teaching me to think, but I'm still amazed at the things he does. For example, when I got to Chicago after the war, we started to work on adsorption, among other things that I had been interested in. Also, Edward had been interested in that. He is the "T" of the BET theory, the Teller, so this was a natural thing. We began to look at the conformation of a surface that has piles of adsorbed molecules on it. That's the basis for one of these papers. Edward always seemed to be terribly short of time, traveling back and forth between Los Alamos, New York, and Chicago, and on one of these occasions I went into see him, because we had in mind making a Fourier analysis of the adsorbate surface. He said, "Now, the entropy will go something like the logarithm of the amplitude." I didn't understand that at all. [laughter] About three weeks later when I saw him, I had it figured out and I had a derivation that came out exactly that way. He said, "Where did you get this?" I said, "Isn't that just what you said?" [laughter]

Another of the things that we did during that year was to look at the production of mesons, which in those days were called mesotrons. The idea was that we have a nucleus with a bunch of nucleons in it; these have zero point energy, so they're buzzing around in all directions in the nuclear envelope. If we sock this with an incoming particle, we may get some benefit from the zero-point motion. If one of the nucleons is moving toward the incident particle, then we get a greater collision energy than we bargained for because of the zero point energy. Luis [W.] Alvarez seized on that idea and produced mesons with the new Berkeley cyclotron. He wouldn't have tried that experiment, he said, if that prediction hadn't been available.

BOHNING: As the war ended and your thesis was in hand, had you been giving any thought as to what you wanted to do?

McMILLAN: Oh, yes. In fact, I had carried on a conversation with Bill Young. I wanted to go back to UCLA. Bill was frequently in the East for various ACS things. When he came to New York I would see him. He encouraged my post-doc arrangement with Edward Teller for a year.

BOHNING: That's when you went to Chicago.

McMILLAN: Yes. Then after that I had this job at UCLA waiting for me.

BOHNING: Why did you want to come back to UCLA?

McMILLAN: Oh, my family and roots were here—and the climate.

BOHNING: Family was still here?

McMILLAN: Yes. I had been to a number of universities in the East. I knew what Columbia was like, and Chicago. In Chicago we lived on the South Side, across the tracks, so to speak. The weather was just miserable. I remember walking across the mall one morning and I slipped on the ice and nearly broke an arm. It was a miserable climate.

BOHNING: What did you do when you first got back to UCLA?

McMILLAN: The very first thing was to teach a summer-session course in organic chemistry. [laughter]

BOHNING: With all that good statistical stuff?

McMILLAN: All that good stuff. Apparently UCLA hadn't found anyone to teach their summer session organic, and Bill offered me that job. The pay in those days was pretty low. I think I started at thirty-eight hundred dollars a year, and this was an extra three hundred dollars for teaching in the summer, so it was not a trivial amount. This was my first experience handling a class of my own, quite a learning experience.

BOHNING: You were probably getting GIs coming back then, too?

McMILLAN: Not quite then, but very soon after. We had the best students, the best graduate students I'd ever seen, because they had been out, and they were mature. They knew what they wanted. They were a gung-ho bunch, just great.

BOHNING: In the summary that you sent me, you had essentially delineated your activities from the period going back to UCLA and after into three different areas (11). I thought that what I might do is try to follow through on what you call the academic science first, look at some changes that occurred along the way, and then come back, because I am quite interested in your government/military activities, as well. I'm not sure if we can really separate that, because obviously they were going on at the same time.

In 1954 you had a paper on the nuclear stability curve (12), which you've commented on. I don't have a copy of that.

McMILLAN: Oh, you don't? Well, I can give you a copy.

BOHNING: Unfortunately what we're finding out is that libraries put journals that far back into remote storage. It's hard to find some of these.

McMILLAN: I can get you a copy of that. It's a very short paper. In fact, I probably can dig it out right now. Let me see, I've also written that up in more extensive detail for my course in quantum mechanics. Let's see if I have a copy of that. Disorganized. It's quite accidental that I was even able to find this. [indicates paper] This describes the essence of it, where there are protons that are charged and neutrons that are not. If we ignore the charge interaction, then the potential well would have this common form. But what happens is that because the protons repel one another, their potential is higher, and of course that extends outside the nucleus. That has to be compensated by lowering the floor of the neutron potential well, because the overall density of nucleons in the nucleus is pretty uniform, the nucleon density. Then we have this measure of the maximum momentum to which the neutron well is filled, and only this for the protons. So we get more neutrons than protons, and that's the basis of this idea.

BOHNING: All right.

McMILLAN: What comes out of that is an equation relating the number N of neutrons to the number Z of protons, which is exactly what we get from the empirical evaluations.

BOHNING: In a more general sense, when you got back to UCLA, what were you thinking of in terms of the projects or the research you were going to pursue? Was it an outgrowth of your World War II activity?

McMILLAN: Yes. One of them was—namely adsorption. To understand that, let me tell you a little bit about UF_6. This has a sublimation temperature of 60° C at a pressure of 1 atmosphere, so we have to keep the temperature higher than 60° and the pressure lower than 1 atmosphere in the process to avoid condensation. But we're sort of running close to the condensation point anyway, with saturation, and therefore we can get adsorption, physical adsorption. Worse than that, since the conduits, the barrier and so on, have metal in them, we get reaction. The UF_6 turns into UF_4 or some other one of a whole series of odd-ball compounds, $UF_{4½}$, for example. So it was important to know how much of the process gas was going to be tied up. That was one thing.

Another thing was, in evaluating the characteristics of the barrier, remember that this is a thin membrane, but it has a high surface area, so we need to know or have some measure of the pore size. One way to get at that is to measure the surface area, and the way to do that is to use the BET theory. Now, in the course of this we had found that there were certain substances that really latched onto the measuring gas molecule, e.g., nitrogen, and there were indications that this adsorption was strong enough that we might be able to observe the thermodynamics of a two-dimensional condensation. That was one of the things that I was interested in. I carried that through in a fairly complete way with Billy [B.] Fisher, who was one of my earliest graduate students (13). That was a continuation of some of the Manhattan Project interest.

At that time, I was also part-time at The RAND Corporation. That was my other foot in the world of military affairs.

BOHNING: How did you make that connection? Did that again come out of the Manhattan Project?

McMILLAN: Yes. I suppose part of it was driven by patriotism, but I'm trying to think what the origin of it was. There was a fellow here at UCLA by the name of Dave [David T.] Griggs. He was a geophysicist, a very eminent one. He had been largely responsible for the formation of The RAND Corporation. RAND had started out as a section of Douglas Aircraft. The guy who was the motivating force behind that from Douglas was Frank Collbaum, who had been the test pilot of the DC-2, going back a ways. So after the war, when RAND was formed, Dave Griggs became the first director of the physics department there. He and I had become quite close friends, just in the university. He was always camped on my doorstep asking questions about thermodynamics. There was a guy at Berkeley who was kind of a competitor, and Dave wanted to understand what he had done. We found some errors in his thermodynamics, and that just delighted Dave, so we were good friends. [laughter]

Then I got to know a lot of the people there at RAND. I forget just the exact sequence, but anyway, I became a consultant there first in 1949. (Nancy became a secretary there in 1950, largely through the aegis of Edward Teller, who knew the management and was a frequent visitor at RAND. She had been secretary to Edward and Enrico Fermi during our year in Chicago.) Now here's something you haven't seen, which is a much more detailed account of activities in that area. Unfortunately this has not been updated for years, because it's a big job. This is academic stuff, and there is a list of defense research reports up through 1975. For example, it will tell you something about my RAND work: you can just read the titles of those reports, but they're all chemical physics, as you see. Some had important consequences. For example, one item enabled the reduction in size of thermonuclear weapons by quite a bit.

BOHNING: This is just for the record—we're looking at one entitled, "A Reevaluation of the Equation of State of Lithium Hydride (U)." (14)

McMILLAN: Yes, that was the one I was just mentioning. Right.

BOHNING: I just want to connect your statement with the one we were looking at.

McMILLAN: Yes. Lithium hydride, of course, is one of the substances used in thermonuclear weapons. Lots of the details are classified. You can see the Secret Restricted Data [SRD] stamps. These others are Unclassified [U], and so on. We had contracts with both Livermore and Los Alamos at The RAND Corporation to do this work.

[END OF TAPE, SIDE 3]

BOHNING: Is it possible to get a copy of this from you?

McMILLAN: Yes, I think so. There's nothing in here that's classified. I mean, nothing written here.

BOHNING: Right. These are just titles.

McMILLAN: This item is dated December 1961 (15). We resumed atmospheric testing in 1962. Well, this document was used by [President John Fitzgerald] Kennedy in his speech that gave the arguments for that. That was facilitated because many of the people in the Pentagon were from Livermore and we'd worked with them for years, so they were in a position to provide these important findings to the President and his staff. Harold Brown was DDR&E [Director of Defense Research & Engineering] at that time. He later became Secretary of the Air Force and still later Secretary of Defense under [President James Earl] Carter.

BOHNING: I notice the name Latter shows up, A. [Albert] L. Latter.

McMILLAN: Yes, Al Latter was one of my colleagues at RAND and later was head of the physics department there. His brother is Dick [Richard] Latter, who also appears in these. They're very good physicists.

BOHNING: In a general sense, I'm interested in how you managed your position at UCLA, and I want to get to that. You went through the ranks at UCLA and became department chairman and everything else, yet it seems that a lot of your activity was still essentially in a classified area of The RAND Corporation.

McMILLAN: Yes.

BOHNING: How did you balance those two things?

McMILLAN: I just juggled them, I guess. I didn't make any conscious attempt at balancing. I guess I don't know how to say "no" to a bunch of things. I kept getting asked to do things; for example, this is a group on weapons effects. Going back to about 1960, there was a study made by a group headed by Bill [William B.] Shockley—you may remember, he was a solid state physicist—on the potential vulnerability of solid-state devices to ionizing radiation. There is a certain kind of a solid-state switch that depends on conductivity, or lack thereof, and if it's off (no conduction) and we expose it to ionizing radiation, then it becomes conducting. Thus the logic of a computer can be changed by ionizing radiation.

Well, nobody paid any attention to that at that time, although they should have. The subject came up again about three years later in a big study called Project Forecast, run by General Bernard A. Schriever, who was then head of the Air Force Systems Command, a very bright guy. He was trying to look ahead, fifteen years or so, at the problems of the Air Force. Well, this issue came up, and by that time there were some new experimental data about the effect of ionizing radiation on some of the logic solid state devices. It happened that this came just at a time after the design of the Minuteman II guidance system had been completed and was almost ready to go into production. We had to stop that. Benny asked me to form a committee to look at the problem. It later got broadened so we looked at the Polaris stuff too, so I got to know all the Navy guys, Admiral Levering Smith and company. They were in a better position because they hadn't committed the redesign yet.

But those were the days when we could things done rapidly. I went into the Pentagon with our results and briefed General [Curtis E.] LeMay and the Secretary of the Air Force, [Eugene M.] Zuckert. Major General Sam Donnelly (USAF), whom I had known before in another connection, and I went around the Pentagon, spent all day there, and we got thirty million dollars signed off in one day. [laughter]

BOHNING: Wow!

McMILLAN: It was a big flap.

BOHNING: Well, the Cold War was still certainly a big item at that time.

McMILLAN: That's right. Yes, on that subject, these are reports of those meetings. All of them Top Secret. There were some other things. "Proposal for the Declaration of the Geophysical World Intervals in the Fishbowl Test Series," (16). We got that done and as a consequence a lot of those things were declared open. One of the RAND reports was quite interesting: "Aurora from the Teak Shot," (17). The Teak Shot was a multimegaton shot—fairly high in the atmosphere—that produced aurora at the north and south conjugate points, conjugate to where it was shot off. The reason was that nuclear explosions generate large numbers of beta emitters. The emitted electrons spiral around the geomagnetic field and go down and bang into the atmosphere and make an aurora. Well, we looked at that and also the change; the aurora migrates with time because the fireball rises. That got me interested in some of the geophysical effects. I have a whole lot of things in the geophysical area to tell you about if we have time.

BOHNING: Yes.

McMILLAN: Many of these reports are interconnected. The one we were just discussing is related to the problem of the vulnerability of missiles to radiation, which was what that committee was about. Let's see, this was the *ad hoc* Committee on Radiation Effects (18). Back in 1961, starting here [indicates page], it became evident from intelligence reports that the Soviet Union was going to resume testing. They finally fired off that sixty-three-megaton device. The idea was to get ready on our side to resume testing. You see, that moratorium had never had any official standing, just a gentlemen's agreement; but the Soviets weren't gentlemen, so Harold Brown asked me to form the *ad hoc* Group on Weapons Effects, which later came under the aegis of the Defense Atomic Support Agency as the SAGE [Scientific Advisory Group on Effects]. In other words, we were the SAGE Advisory. That committee essentially kibitzed on the design of all of the 1962 series of effects tests, the Nevada atmospheric tests and the Pacific tests, out in that time frame. All of these are reports that I wrote as chairman of that group (19).

BOHNING: Were you there for those tests then?

McMILLAN: Well, some of them. I think I'm probably the only guy alive now who has seen two failures in one day. [laughter] Two duds.

BOHNING: What was your reaction when you saw your first above-ground test? How early was that?

McMILLAN: That was, I guess, 1962, in the Pacific at Johnson Island. Pretty impressive.

BOHNING: Well, obviously there were lots of pictures, but to see the real thing must have been something.

McMILLAN: Yes. Those were pretty exciting days. A lot of interaction with the military.

BOHNING: Yes, I wanted to talk about that. Maybe we should do that now while we're on the subject. That was one area you had delineated as what you called tactical military technology.

McMILLAN: Yes.

BOHNING: You're listing, for example, back in 1966, the concept of the Fragmacord Antipersonnel Mine (20). That, in some respects, seems far afield. What was your involvement in that? Was it theoretical first?

McMILLAN: Well, to understand it, we have to go back a bit. I noticed over the years, in all of these committees, there were always the same people. In other words, although we had thousands of academic personnel involved in World War II—Radar Project, VT fuze, chemical warfare and all that—most of these birds dropped everything and went back to their ivory towers after the war. It seemed to me that that was a real loss to the military. The military should have the best scientific advice, so I talked to Harold Brown about that. Oh, I should tell you another problem. There's no way of getting new blood in the advisory business without some special arrangements, because the new blood doesn't have the necessary clearances, and they don't know much of military relevance that's worth asking their advice about. So, the thought here was to educate a few of these youngsters so that they would become available.

If you're asked to form a committee to do something or other, it's always on a rushed time scale. You're not going to select a bunch of amateurs. You're going to look for people who know what they're doing, have been that route, and can give a quick turn-around answer. The idea here was to develop what I called the Defense Science Seminar, which would take some of these young people, everybody under thirty-five, and pump them up so they would know what some of the issues and problems are. I ran this for three summers during the month of August, here at UCLA, on a classified basis. The participants in each case consisted of about forty-five people, roughly one-third coming from academia, another third officers from the military, and a third from the government military laboratories. We had a good mix in the group. These guys lived and worked together. We really worked them pretty hard. If you're interested in that, I can give you a typical brochure.

BOHNING: Oh, sure, yes.

McMILLAN: This report of the third of those seminars, in August 1966, gives the attendees and the speakers (21).

BOHNING: Excellent.

McMILLAN: We also had field trips to places like Picatinny [Arsenal] and Cheyenne Mountain—places like that where a demonstration would lighten it up. Many of the "students" who were involved with this went on to become managers in the military laboratories. John [D.] Baldeschwieler was one of them. He became Deputy Director of OSTP [Office of Science and Technology Policy]. He's over at Caltech now. Fred [Frederick M.] Hawthorne is another one.

BOHNING: Those are familiar names.

I noticed in that introduction there that you were also involved with General Westmoreland as well.

McMILLAN: Yes. Now, that was really an outgrowth of this. I had known Johnny [John S.] Foster from Livermore [Lawrence Livermore National Laboratory] days. I didn't mention that when Livermore was getting started in 1952, they didn't have any theoretical group, so a number of us from the RAND physics department moved up there for the summer and constituted the theoretical group until they got going. Johnny Foster was there at the time. Then later on he followed Harold Brown—first as director of Livermore, and then to the Pentagon as DDR&E. In talking with Westmoreland in 1966, he decided that Westmoreland needed a scientific person at his elbow, and he persuaded me to take that job. As it happened, I was traveling around with a group visiting the Defense Intelligence field sites, and when we got to Saigon [Ho Chi Minh City] I just stayed there. Westy hired me on the spot, practically, and I lived with him in his villa, had breakfast and dinner with him every day. It was a very interesting life. Wonderful guy. That's how that happened.

Now, you were asking about things like Fragmacord. There were all kinds of urgent military problems in Vietnam. One of the problems was that of infiltrators. Many of these problems had not been addressed in any way by the scientific community. So I conceived this idea of having a linear mine, which we could lay along the trail when the infiltrators come in. The idea is to have our scouts out so we know when this infiltration group is coming in. We put an ambush for them on one side of the trail, and on the opposite side put this linear Fragmacord in the drainage ditch. When the line of infiltrators are all properly oriented, then we start the ambush fire. The infiltrators dive for the ditch, and then we fire off the Fragmacord. It was designed to stop infiltration.

The trouble was that the Fragmacord was all used up in demonstrations. We had about a thousand of the twenty-five-foot segments built. They arrived in Vietnam and everybody was so enthusiastic about them that they fired them all in demonstrations. [laughter] They had to reorder them.

Another time and another kind of project, while we're on the subject—before I went to Vietnam I had read a book called *Jambo* (22), which means "hello" in Swahili, written by a guy who wanted to study herds of African animals without disturbing them. He wanted to get an overview from the air. He tried this with an airplane, but that always frightened the animals whenever he got close, so he decided to become a balloonist. He tells of the problems of getting a license and all that. He points out that in a balloon there is no relative wind motion, and therefore it is as silent as a church. He could hear children playing in a schoolyard on the ground from a mile altitude. I thought, "My God, what a wonderful kind of Intelligence collection system that would be."

We had found in Vietnam that under the jungle canopy, the Viet Cong [VC] had all kinds of potentially noisy activities going on. For example, they had a volleyball court. I wrote to a guy in ARPA [Advanced Research Project Agency] and got back a dumb letter, but I kept trying. When I got to Vietnam it happened that there was a very energetic guy by the name of Dick [Richard] Cesaro in ARPA, willing to take some risks. Anyway, he had a contractor produce a blimp that was equipped with acoustic listeners. I had the idea that what we should do with this powered blimp was to fly her upwind on a dark night and then shut her down and let her float majestically over the Viet Cong areas, war zones C and D and so on, with big acoustic ears listening to locate the enemy activity. We never had a problem in Vietnam with defeating the enemy. The problem was bringing them into battle. It was often difficult to find the rascals.

So, this blimp finally arrived, but it had been designed in a very hurried fashion. I went out to the airfield at Bear Cat to watch the demonstration. A typical day at that time of the year was cloudy in the morning, then the sun would come out. The blimp was painted black so it wouldn't show at night, but as part of the camouflage it had letters on the side, "CHIEU HOI," which means, "Come rally to the cause." Anyway, it had an electric motor equipped with a stepping switch for determining the speed of the motor. The ground crew had gotten it inflated with helium, and they were trying to run it up and down the runway. It was clear that it was just marginally under control.

The balloon had a remotely controlled pop valve for releasing some of the helium, but the guys who were running it hadn't thought it through. Imagine what happened now as the sun came out. The black blimp got hot, and expanded as much as it could. The skin was plastic film, not rubber or elastic, so it just expanded to fill the volume. Then it began to rise. Well, think now, the pressure inside may have been, say, 1.01 atmospheres, and the controllers decided they'd better bleed some of the helium off. Well, they did, but they didn't bleed off enough to reduce the pressure below atmospheric so the volume stayed the same, but the weight declined, and she shot up to ten thousand feet.

Well, by this time the blimp had become a traffic hazard to all of the aircraft, so the operators sent up a helicopter gunship to shoot it down. It turns out you can't shoot down a balloon like that. You riddle it full of holes, but it takes time to deflate. Well, it finally settled down, landing in a swampy area called the Rung Sat, the "forest of thieves," a swampy region.

Well, they didn't want all the classified collection and acoustic gear to get lost, so the controllers went down there with a clipper and they clipped off all that gear, relieving the blimp of about three hundred pounds of weight. Then, of course, the obvious thing happened—it took off again. It was last seen headed over Cambodia proclaiming "CHIEU HOI." [laughter]

BOHNING: Maybe we should talk a little bit more about your military activities. I was looking at the list that you had supplied me. You wrote, "In 1970, developed concept for a SAM-defense suppression weapon (DELILAH)" (23).

McMILLAN: Yes. You see, we had things known as homing anti-radiation missiles—HARM (Standard ARM that homed on the radiation signal from enemy air-defense radars). The problem with these missiles is that when they are launched from aircraft, they have a distinctive signature. The aircraft has to go into a rising motion and the missile is then lofted. On the radar one would see the missile separating and then the radar would shut down. After the shutdown, the missile doesn't have anything to home on. I wanted to solve that problem because the enemy's surface-to-air missiles [SAMs] were shooting down our aircraft. My approach was to use a rugged missile that we could implant in the ground. This is like one of those ground-implanted sensors that we used eventually. What we would do is fly over this area where we know the SAMs are, within a kilometer or so, and plant this missile—DELILAH. So there DELILAH sits and waits, with its listening device and its intelligence, and it's listening for the radar to come up. When the radar comes up, DELILAH sees it and goes, "Whshh!", getting to the radar and destroying it before it can shut down. There's no aircraft in the air, no warning at all. That's DELILAH—a SAM killer.

[END OF TAPE, SIDE 4]

BOHNING: Well, you have that list there, too. Was there anything beyond that?

McMILLAN: Well, a lot of this is after Vietnam, of course.

BOHNING: Right.

McMILLAN: During Vietnam I had about three hundred different projects there.

BOHNING: How long did you stay with Westmoreland then?

McMILLAN: I stayed until he left and afterward. He left in June of 1968, and I stayed on until early 1969 with General Creighton Abrams. I still feel that I left too early, because there was so much left to do.

Let me tell you some other projects in Vietnam. The place is pretty well covered with jungle. One of the techniques of the Army is to use helicopters to put down a patrol or insert a group. But the natural clearings or landing zones [LZs] were all well known to the enemy, and they frequently had them mined. The standard technique was to have artillery prep that fires artillery all around the landing zone. While that's going on the enemy takes to his holes, and after the artillery barrage is lifted, the first aircraft comes in and delivers its load of troops. By the time the second one comes in, the enemies have crawled out of their holes and gotten hold of the switches on the mines implanted in the LZs, and the second aircraft and its troops get blasted out of existence. So the question arose, "Is it possible to create a new (unmined) landing zone?" Well, the marines tried lowering troops with chainsaws to cut down the trees. That takes all day, and it makes a lot of noise and clearly is a loser.

I thought, the right way to do it is to use a big bomb, blast a hole, and blow the trees down. Well, we had lots of arguments in my office as to whether that would work or not. There was a young Air Force colonel who was visiting my office for a month. I used to have many of these exchange visitors from the Pentagon. He wanted a problem, so I said, “Go over to Seventh Air Force and get the biggest frapping bomb they’ve got, and let’s run an experiment.” He did, and ran a very beautiful experiment. It took only two weeks. The ground forces seemed extremely interested in this, so they gave him lots of support. We conducted this up in the II Corps area, lowered the bomb through the canopy and set it upright, just above ground level. The colonel had his team make a map of the trees in all directions—where they were, their sizes, and so on. Then we set off this three-thousand-pound bomb, which was the biggest standard bomb available. Thirty seconds after it went off, one of my guys landed a helicopter in the cleared zone, showing that it works—we have lots of pictures.

Really, that was something that they wanted developed, so two of the other guys, another Air Force colonel and the Army helicopter pilot—Bill [William] Dennin was his name—took that project back to the U.S., and we got funding through the Air Force. I had very good support from the military for these things. These two officers spent the next six months developing a ten-thousand-pound bomb and the ability to drop it. They were very wise, wiser than I might have been. They developed the capability to use not only the Army’s crane helicopter, the big heavy-lift helicopter, but also the C-130, which is an Air Force airplane, to do the bomb delivery.

Now we get into roles and missions, a touchy subject. Anyway, the Marines requested that we create some landing zones for them in four places, and we did that using the Army’s Crane helicopter. One of the bombs lit on a hillside, so it wasn’t suitable for an LZ, but the other three worked liked like a charm. When word of that got around to the Air Force, they said, “No more of that. We’re the guys who drop the bombs.” So after that we used the C-130. That operation was known as Combat Trap, and over the course of the war we dropped about three hundred of those. They weren’t aimed at anybody. They were aimed at clearing the jungle landing zones. That was one of my three hundred or so projects.

Another one occurred early on, when Harold Agnew, who was director of the Los Alamos Scientific Laboratory [LASL] said to me, “I’ve got all these eight thousand guys here who can do wonders. What do you need?” I said, “Well, location of enemy artillery is an important issue.” Let me explain where that comes from. By this time (1967) we had the so-called McNamara Barrier going in, or planned to go in. If this is Vietnam, this is the DMZ [demilitarized zone], which divides North and South. Here is Khe Sanh. Over here is Tchepone in Laos. This area up to about halfway in from the South China Sea is flat. At this latitude South Vietnam is something like 50 kilometers wide. The idea was to build a conventional barrier across here, consisting of a fence and all that kind of stuff.

But this plan was widely advertised in the news media and, of course, the North Vietnamese knew what was going on. They saw this as inhibiting their activity up here. They had a couple of mean divisions, like the 324-B, here. They’d run South across the DMZ, zap the villages there, and run back, like playing wood tag. They saw the barrier as a threat to their activities, and they moved artillery down into this area to prevent its construction. They had about a hundred pieces of 152-caliber artillery—a fine artillery piece with good range. This was in the spring of 1967. I had a number of projects going on up here at Con Thien—a number of scientific-type experiments—and the North Vietnamese Army [NVA] bombarded this whole area with about a thousand rounds a day. Very noisy. All my guys were under cover, so I didn't lose anybody, but I think I had more people up there from my shop than the Marines did.

At any rate, it became clear that there was no way that we could put a construction battalion out in the open—for building the fence—so the question arose, "How do you suppress this artillery?" There was a big meeting in Westy's office, and the head of the Seventh Air Force, General Spike Momyer, was asked to bomb them out of existence, which he agreed to try to do. But again, the analysis never caught up with the problem; the problem was that these artillery pieces were all revetted. Here's the artillery piece here in the center. The radius of the revetment berm is about 5 meters, and the berm is a couple of meters high. That means we've got to get the bomb inside to have any appreciable effect.

Moreover, although we had had VT fuzes, proximity fuzes, in World War II, we didn't have them in Vietnam. That was another thing. I got that fixed when I got back. The standard CEP that the Air Force claimed for bomb delivery was about 200 meters but actually was closer to 1000 meters. The way to deliver the bombs was with the aircraft coming in at a shallow dive angle. Depending upon the skill and perseverance of the pilot and the intensity of the defenses, he may pull out pretty high up, and the accuracy is 20 or 30 miles, so he doesn't get anything inside—right? But if we just make this calculation, say that the CEP is 200 meters, that means that in a radius of 200 meters, half of the bombs fall inside and half outside. The radius of this revetment is 5 meters, so that the probability is $(5/200)^2$ times ½, because of the CEP. That's a pretty small number. That's $(1/40)^2 \times ½$ or 1 in 3200. So one bomb in thirty-two hundred could be expected to land inside the revetment.

BOHNING: Right.

McMILLAN: But there's a worse factor. These revetments were constructed so that they had three times as many as they needed. The Air Force takes the photography, but then it takes overnight to get the results of the photography, and overnight the enemy shifts the cannons all around, so now we have to put in one-third as the probability that a given revetment is occupied. Not we've got 1 in 10,000. I talked to Spike about this. I said, "Spike, you can't do what you promised to do." His response was, "Well, what can I do? I've got to try."

Over the next month in "Operation Neutralize" the Seventh Air Force expended a hundred-million pounds of ordnance. Now, a standard five-hundred-pound bomb costs about a dollar a pound. So we are talking about a one hundred million-dollar operation. One hundred million pounds is about two hundred thousand five-hundred-pound bombs with a probability of 10^{-4} of hitting a target. At the end, the Bomb Damage Assessment showed that four of these hundred guns had definitely been killed, with a possible ten additional damaged—just about what you'd expect from the odds we calculated.

That left the problem of going after the artillery in some other way, one of which was to locate it acoustically and to respond with counter-battery artillery five. Now, there was an acoustic locator system which happened to have been developed by the acoustic group at UCLA during World War II, a very good bunch of acousticians led by Professors Leo P. Delsasso and Robert Leonard, both of whom are dead now. Anyway, this acoustic locator, adopted by the Army during WWII, was called the GR-8. It consisted of a line of Helmholtz resonator microphone pots designed for low frequencies. These Helmholtz resonators are like an Erlenmeyer flask. They have a hot wire inside. When this pressure wave comes along, it causes a tiny subtle motion of the air in and out, so the interaction with that hot filament changes the current flow, and a measurement of the current flow indicates the time of arrival.

Now, this GR-8 array stretches out over a few kilometers. I explained all this to Harold Agnew, and said, "I don't see any natural length in this problem." We know the wavelength of the acoustic wave; that's pretty big, but that's not relevant. What matters is the speed of sound multiplied by the rise time that we can measure. That's a fraction of a meter. I went on to say, "Why don't you guys build me a microphone array and the necessary computerized electronics on a one-meter baseline, not 10 kilometers?" "Okay." They went and did it—but not exactly 1 meter; it came out 5 feet!

That was the source of what became the LASL Acoustic Locator System, a beautiful system completely radio linked. It didn't have any hot wire. One of the problems with the old GR-8 was the interconnecting wires lying on the ground; these were always torn up by incoming artillery, and had to be rewired frequently. Also, the GR-8 had batteries to run the hot filaments. The LASL system had none of that. All of this was remote. There were four microphones on each square array about five feet in diameter. Each one of these microphones receives a signal at a different time. The differences in times provide the direction to the sound source. Several of these arrays, scattered around and radio linked, provide an intersection of the direction lines that yield the source location. That was one of our great successes. But the sequel is a sad, sad story. I don't know if you want all of the details.

BOHNING: Oh, yes.

McMILLAN: I tried to get this experimental LASL system into proper Army channels. See, nothing gets done as long as it remains outside channels. We could get anything done on a one-of-a-kind basis, but to get anything in production and get the Army to adopt it was like pulling teeth. I tried to find someone on the Army staff who would take this on and pursue it. I finally found a young lieutenant colonel who was really enthusiastic about it. He said, "I'm due to go to Vietnam and I'm going to pull from that end. I thought, "Well, we've got it really wired." He went out to Vietnam, and on the third day he was there he got killed in a helicopter crash, so we were back to square one.

There were other problems. I was supposed to have a blank check for funding such developments. I didn't have any budget of my own, but funding was supposed to come from

ARPA. This initial development of the LASL System at Los Alamos cost about fifty thousand dollars. We had, I think, maybe three or four prototype sets made. Then Harold Agnew wanted to be repaid. I went to Johnny Foster. I said, “Hey, we promised Harold that 50K.” He said, “For Christ’s sake, they’ve got a two hundred million-dollar budget, let them take it out of their hide!” Well, you can see how that was received. I was very annoyed and of course I never got anything else out of Los Alamos.

Anyway, we eventually got the Army to produce the LASL System. I’d made a demonstration to General Abrams of quite a number of technical items for which MACSA had sponsored development, and when I demonstrated the LASL System he said, “My God, we could use a thousand of those.” The reason was (you never heard about this) that practically every night there were something like thirty “attacks by fire.” These attacks would be against either military installations or villages. The Communists would come with mortars and rockets, set up, get everything ready, fire them and take off before we could do anything about it. Now, in those attacks there was an average of six shells launched, maybe by mortar or rocket, and an average of 6.2 people killed, so we can make an equivalence—one shell, one life.

When our forces invaded Cambodia and President [Richard Milhous] Nixon came on TV to justify that, he pointed to the enormous piles of mortar shells that had been captured—eighty thousand of them—but there was no connection made to their meaning in terms of lives saved. The next time I was in Washington, I went to see General Al [Alexander M.] Haig, who was [Henry A.] Kissinger’s assistant in the White House. I said, “Why don’t you guys put these captured munitions in human terms so that people can understand it? You’ve got eighty thousand mortar shells captured; that’s eighty thousand lives saved.” But you know, no connection. Anyway, General Abrams had been really impressed with our demonstration and said, “We could use a thousand of them.” So I transmitted that statement to the other end of my chain in the Pentagon, who was Leonard Sullivan. It got translated from a thousand into twenty-five. When the Army got around to it, they made five of them!

By that time, I’d rotated out of Vietnam, but I tried to keep in touch with what was going on. USARV [U.S. Army, Republic of Vietnam] was the Army element there. They were asked, “Where do you want these things?” They answered back, “What are they? We don’t even want them.” That was absolutely frustrating. If I had been there, I would have been on top of it.

With respect to another development, I used to think that the last stronghold of slave labor were the graduate students. Not quite true. It’s the artillery. Every time an artillery battery moves, they’ve got to dig in and fill fifty thousand sandbags, which takes quite a while. That always annoyed me. “Here we’ve got these slaves out there doing that. This is supposed to be a nation that knows how to mechanize things. Why don’t we get a ditcher-sandbagger that will cut a trench that the troops can take refuge in and at the same time deliver the spoil to an altitude high enough so we can put a bag under it to fill the sandbag? I had a young fellow in my office by the name of Jim Keats. I said, “Jim, go back and buy two ditchers and send them out.” Well, we can’t just do that. It’s got to go through channels. There was an organization called the Limited War Lab. It was pretty limited. Anyway, this request went out to them, and by God, eventually, about a year later, there arrived at the Seventh Aerial Port two ditcher-sandbaggers. One of my guys—Colonel Ed Cesar—was there and he came flying into the MACSA office, yelling, "Bring your cameras!" [laughter] Anyway, we finally got these ditcher-sandbaggers; the contribution of the Limited War Lab was to weld on a step to hold the bag, and to put a big brass plaque on the side that said, "Limited War Lab."

Anyway, these came out to Vietnam under Army auspices. Ed Cesar was a very energetic Army colonel who was also my field liaison with the Marines. He was very close to Marine General Tommy Tompkins, who was the commanding general over the whole northern part of I-Corps. This was just at the time when Khe Sanh was heating up. You remember the Siege of Khe Sanh? The Marines didn't have any excavating machinery there. They needed a ditcher-sandbagger to dig communication trenches and to fill sandbags. I never asked how Ed Cesar did these things, but he had ways of getting things done. He arranged for a C-124 airplane out of Tokyo to transport one of the ditcher-sandbaggers to Khe Sanh. I don't know how the hell he did it.

To Bill Marroletti, my guy stationed at USARV Headquarters in Long Binh, I said, "Let's clear our baffles with USARV." He went to see Brigadier General Bob Tabor, who was the chief of staff at USARV, saying, "We want to take one of these ditcher-sandbaggers up to the Marines at Khe Sanh." "No way," said Bob Tabor. He gave him a big argument. It turned out that Marroletti had already cleared it with Commanding General Bruce Palmer, so we got permission to send this Army machine to the Marines. The other machine was kept in a pool so that when an artillery battery moved, we could ship the thing out to them. You see the kind of practical problems?

BOHNING: Yes.

McMILLAN: These are only a very few of the three hundred or so such programs.

BOHNING: You had quite a staff working with you.

McMILLAN: Yes, I had, at any one time, about twenty-five people.

BOHNING: In general then, the military was really on your side in what you were doing?

McMILLAN: Oh, yes—at least for the most part.

BOHNING: You had tremendous support from them?

McMILLAN: Well, I lived with Westmoreland, which gave me a lot of additional clout. I never used it, but I think everybody was afraid; by God, if they didn't cooperate, they'd get clobbered.

Sometimes we got into trouble. I had one guy who was an Intelligence expert. There was an area up in I-Corps called A Shau Valley. The damn North Vietnamese moved in there. We had special-forces camps in there. They overran them. The bastards even brought in a gasoline pipeline, all the way from North Vietnam into A Shau Valley! We had previously built three airfields in there. MACSA had a project to worry about possible applications of dirty tricks the enemy could do to exploit these air fields. My Intelligence specialist, Billy J. Mills, was an Air Force officer. He had a lot of old pals throughout the Intelligence community, and he induced some of his Air Force friends to run a bunch of photographic missions over A Shau. As a result, we had thousands of feet of eight-inch film. We knew where every bloody thing was in that place—including the many anti-aircraft batteries.

It turned out there was an unfortunate thing, and to this day I feel bad about it. I was on a visit back to the States when MACV [Military Assistance Command, Vietnam] planned an A Shau Valley offensive, so I didn't know about it in advance. The First Cavalry Division moved in there, right in the teeth of all the enemy anti-aircraft guns sitting there, and they had twenty-eight helicopters shot down the first day, just a slaughter. I said to my Intelligence man, Billy Mills, "My God, you'd better get up there with that film." He went up to talk to General Tolman, who was the commanding general of the First Cavalry up there. Tolman was ready to hang him. "Where was this information when I needed it?" Well, lack of communication.

But there's a funny thing with the military; there's a fatal flaw in some of the ways that they operate. The commanding general will designate the objective—"You go take that"—but he won't give them instructions on how to do it. He leaves it up to that guy to ask for anything he needs. The guy goes in blind. I mean, no Intelligence, nothing. He's afraid to ask for it. You know, "What do you mean, boy? Well, your predecessor didn't need all that stuff." It's a mess. That ought to be fixed.

Well, maybe enough about that. Let me finish off one thing. Most of the time, whether it's in academic work or in the kind of contract stuff we do here at McMillan Science Associates for industry and government clients, we write a report, it may be a glowing little gem. We send it off into the blue, and rarely hear anything back. It was different in Vietnam. There were all kinds of problems that were useful, that would save lives, actually save lives on both sides. Once it was decided to attack one of those problems, there were all these resources. Like with Combat Trap, we spent two weeks. When the project got back to the States, it took them six months to do the first one. We got immediate feedback— it worked, it didn't work— or thanks for your help. Very different. Very satisfying. A lot more personal satisfaction than the things we do back in the States.

[END OF TAPE, SIDE 5]

McMILLAN: All that work for the military—of course, in Vietnam that was a full-time job—but otherwise it's been pretty much half-time. We don't get any credit for that in the academic circles, yet it was this kind of work over the years, not just by me but by a lot of people, that caused the demise of the Soviet Union and made it so all the rest of these academics could live in their ivory towers. We don't get any credit for that.

BOHNING: Well, I thought about that in looking at your publication list, which is shorter than most academics would have. It's obvious that your time and energies were often spent on things that would never see the light of day.

McMILLAN: Right.

BOHNING: But in many respects they were probably equal to anything published in the scientific journals. How did you feel about that?

McMILLAN: Well, I made the decision to live with it. It's something of a sacrifice if your main goal is to achieve outstanding academic recognition. But that's the way things are.

BOHNING: Well, you did receive recognition from some quarters in the military, at least.

McMILLAN: Yes, and with those and a dollar bill you can probably buy a beer. [laughter]

BOHNING: What kind of a person was Westmoreland? You commented generally about him before.

McMILLAN: Well, he's very bright. He is, of course, very dedicated. He's a real gentleman of the old school. He paid a lot of attention to physical fitness. He spent a great deal of time visiting with the troops out in the field. I used to go around occasionally with him on some visits to the units.

I was sort of appalled when I first arrived in Vietnam. I was treated very well, in the sense that at any meeting I sat at the head table. We had a Weekly Intelligence Estimate Update, the WIEU. It met at 7:00 a.m. Saturday morning and ran most of the day. In the headquarters there was a U-shaped table. There was the briefer sitting on the side, and charts and blackboards were up front. Westy sat at the head, and then lesser ranks down the table. I had what was called the “assimilated rank” of lieutenant general. So I was usually sitting here. There’d be General Spike Momyer of the Seventh Air Force here, and the USARV general over here, and so on down the line.

When I first got out there I was bothered that there was almost no discussion. It was a very different kind of atmosphere (from a university). The briefers would give their briefings, and Westy would comment, or the various staff members would make their proposals, and Westy would approve or disapprove or comment, but with almost no discussion. So after I had been there about three weeks and hoped that I was getting to know Westy, I broached the subject. I had just pointed out the difference from the climate you’d find in the university. He said, “Well, I’m worried about that, too. When Bill DePuy was here, there used to be a lot of discussion. Let me see what I can do about that.” That’s the kind of receptivity that he had, which I really admired.

Well, the next week he gave them a little speech. “I want to tell you about my overall strategy, and then I want your comments. Don’t sit there like a bunch of lumps on a log.” He went through like a schoolmaster. He reviewed the history, reviewed what the problems were and what his intentions were and so on, beautiful! From that point on there was a fair amount of discussion.

I never felt inhibited, although, in any case, I would not try to put myself forward, because I take a back seat to the military. Let them do their thing. If there was something scientific that came up that I knew something special about, then I would talk about it. For example, there was this McNamara Barrier that was just going to be installed; that is, the air delivered sensors were about to be dropped. The infiltration trails were over here in Laos, the Ho Chi Minh Trail. The plan was to drop these sensors over here.

Okay. This was under discussion. This was just when Khe Sanh was beginning to undergo siege. There were four enemy divisions around Khe Sanh that were going to try to gobble it up. So, at the end of the session, Westy would always go around the table ask, “Have you got any comments?” Well, at that invitation I said, “I would like to suggest that because of the siege at Khe Sanh, let’s pick up that sensor operation (called Niagara) intended for Laos, and plant it around Khe Sanh where the action is, and use that to target all those investing troops.” Well, Spike Momyer was sitting next to me, and he said, “I got it.” He was writing a TWX [teletypewriter exchange] to do that to the NKP [Nakhom Phenom], so that was a case where, I think, the scientific advice was good, and it really worked. That stopped that siege pretty cold. We could target those guys pretty easily.

In another case, in the WIEU briefing, there was a young Army captain, by the name of John Stewart (who has since become a general). The country was divided into four corps areas, and up here in II-Corps, just south of I-Corps, was the Tri-Border Area; that is where Laos, Cambodia and Vietnam come together. Captain Stewart had given an Intelligence brief on the build-up of enemy forces over here. It looked like there were thirteen battalions of the enemy sitting across the border there. We had four battalions on this side, but nobody remarked on that disparity.

Again, at the end of the sessions, Westy went around the table, and when it came my turn, I said, "Hey, could we call back Captain Stewart and have him review that situation again? That looks like it's an unhealthy ratio." Well, Stewart was sitting on the edge of his chair wanting to do that anyway. He came back and repeated his story and Westy turned to the COC, the Combat Operations Center officer, and said, "Let's move the 173rd Brigade up there, and get going on it right now." That night at dinner, he said, "We didn't move the 173rd up there any too soon. They were attacked this afternoon."

I have one more story like that. After the big Tet attacks, Westy had told the J2 Intelligence staff officer that he'd like an analysis made of Tet, in terms of the potential for a coup. You remember that there had been repeated coups there. The Intelligence officer, Major General Phil Davidson said, "Yes, sir." He was a two-star. So they did it. They made their analysis and they presented it two weeks later. This was now two or three weeks after Tet. But it didn't contain anything resembling a coup.

Anyway, there was some discussion, and Westy said, "Well, that's fine, but I wanted to see what this looked like in terms of a potential coup." Then there was this thing that always irritated me, the subordinate officers, "Yes, sir." No discussion. So I stuck my neck out, saying, "Well, isn't it so that in former coups, these were always concentrated on Saigon, whereas in this case, every province capital was attacked?" Westy says, "Yes." Well, now I'm in for a penny and in for a pound. So I say, "Well, isn't it so that generally each coup relied on some disaffected element of Vietnamese military to support what they were doing? And that didn't happen?" Westy says, "Yeah?" [laughter]

By this time I was in deeply, questioning the idea of the commanding general and I didn't know how to get out of it, so I made a joke. Westy was saying, each time, "Yes, but I still want it analyzed." [laughter] So I said, "Well, I'm always fearful that in the Intelligence business we may run into a situation that was illustrated in the cartoon Charlie Brown. Little Lucy is looking at the ground, and Charlie comes up and Lucy says, 'See all those bugs there?' 'Yes,' says Charlie. They Lucy says, 'You see that big black bug there? That's the queen.' Charlie Brown looks at it, throws up his hands, and says, 'Ah, that's a jelly bean.' He walks away in disgust. Lucy looks at it, and says, 'Now how did a jelly bean get to be queen?'" Well, everybody laughed and it kind of relieved the tension. But the J-2 for some reason was offended, and he said, "Well, I don't know about that, but it doesn't sound very respectful." Or something like that. Westy says, "I understand it perfectly. Bill means that we mustn't become enamored with our theory." Now there is a <u>big man</u>! Well, he saved my ass. I later went up and apologized to Phil. "I'm sorry if I embarrassed you." He says, "Oh, I don't know what got into me." [laughter] But those are little vignettes about General Westmoreland.

BOHNING: Very good ones.

McMILLAN: He's a wonderful friend. There was nobody, I think, who could have done a better job out there, given the political handicaps. If you want to read about that, there's a good book called *Strategy for Defeat*, written by Admiral U.S. Grant Sharp (24). President [Lyndon Baines] Johnnson bragged that we couldn't even bomb an outhouse without his permission. That's a crazy way to run a war; we can't run a war like that. I wrote a little blurb on that. I had to give a talk to the symposium of the military R&D people. It was held at West Point on one of my trips back. I had a graph. I have all these things here somewhere. I have a viewgraph that shows the bombing campaign. Here's the intensity, as a function of time, and it goes like this. First we bomb the POL [petroleum, oils, and lubricants], and they then put all their POL in fifty-five-gallon drums and scatter them along the highway where they need them. Then we bomb the electric power and stop and see if they've had enough. Then, if they haven't, we resume and next hit the railroad yards. If we want to have an effect on something, we've got to have a shock wave. It's very clear to anybody who knows anything about thermodynamics. [laughter]

BOHNING: Were the students at UCLA aware of what you were doing? Were there any problems?

McMILLAN: I never had any problem with them. During that 1970 invasion of Cambodia, there was a student strike. Some of the faculty went along with that. One of the things that I became thoroughly disenchanted with was scientists; they can act like fools, just like ordinary people. I can tell you all about that. I conducted my classes without any interruption, and there was no demonstration or anything.

During that period, a girl from one of my classes came in and said, "I'd like the assignment." I said, "Fine. Why aren't you coming to class?" She said, "Well, I'm participating in the student strike." I said, "Why?" She goes into this. I had a map of Vietnam on my desk. I said, "Let me tell you about that Cambodian incursion. These bases just over the border are North Vietnamese bases 701, 2, 3 and 4." These were well known from Intelligence. "What the North Vietnamese do is come across the border and zap a village or some military base, and then run back across the border. It's like playing wood tag. They have a sanctuary. It's not we who have enlarged the war. Those are North Vietnamese troops in Cambodia, where they have no business being. All we did was to go over and clean them out." Well, we talked for a few minutes, and pretty soon she was in tears. The next day she was in class.

I did have a group of students contact me who wanted to stage a counter-demonstration to the student strikes, so I gave them some ammunition for that.

BOHNING: What about some of your faculty colleagues? You commented generally about some of them. Was there any objection on their part to what you were doing?

McMILLAN: Oh, yes. Not about what I was doing, but the fact that I wasn't full time at the university. Of course, I was very scrupulous, and I was on half-time pay during much of this period, even when I was chairman.

BOHNING: So the administration had no problem with it. They were supportive of what you were doing?

McMILLAN: Yes, in fact they were even supportive of the Defense Science Seminar. Franklin Murphy was chancellor at this time. I ran into him at a cocktail party and told him about the Defense Science Seminar. I said it seemed to be appropriate for a university that was involved in teaching. We could raise up another generation of people knowledgeable in military problems. He said, "Young man, you may get drummed out of the lodge for wanting to do some teaching." [laughter] No, I didn't have any problem with anybody in the administration.

BOHNING: It might have been different if you had been at Columbia at that time.

McMILLAN: Yes, that could be, although the Columbia fracas happened quite a bit later. I'm talking about 1963-1966, and the Columbia business was 1968.

BOHNING: Yes. Well, let's follow up on something you just said before, since you said you wanted to talk about ozone and global warming. Before we forget it, why don't we talk about that?

McMILLAN: Yes. Let's look at the ozone thing first. Look at the reasons that are advanced and apply our own chemical knowledge to these things. Now, fluorocarbons are being banned, and that's a big displacement. Not only does it take out of the economy a very useful thing, but it ruptures all the manufacturing. All that has to be replaced. Now, where's the ozone hole discovered? At the South Pole. It's well known that the atmospheric communication between the northern and southern hemisphere is very slow. These CFCs are released in the Northern hemisphere, where the industrialization is, yet they're blamed for that in the South. Now, in effect, it's the chlorine—not necessarily the molecules, but chlorine. How much chlorine is carried into the atmosphere by waterspouts, little tornadoes on the ocean, or spewed out of volcanoes? That's a factor five-hundred times that from chlorofluorocarbons, integrated over all time. The amount spewed out by Mount Pinatubo dwarfs that, so how can they say that the chlorine comes mainly from the chlorofluorocarbons? That's point number one.

The second point is, how does ozone form in the first place? Well, we've got O_2 plus a photon to give two oxygens, and then we have interaction with a third body to give ozone. So, the fundamental step involves solar radiation. But at the Poles the solar radiation is dim, of course, because it comes in at a glancing angle. So, it may not be a question of chewing up the ozone, but rather of making it in the first place, and nobody addresses these issues. It seems to me just insane to formulate a national policy, particularly one that costs big bucks and disruption of industry, on the basis of this kind of poor science. I've heard Sherry [F. Sherwood] Rowland talk about this. I asked him, "How do you know the chlorine comes from chlorofluorocarbons?" He gave a waffling answer. How does the amount of chlorine compare with what's naturally produced? He doesn't know, but he cites numbers like concentrations of 10^{-12} or something.

Now, if we look at the global warming, that's in the same kind of a boat. The thing that people ignore there is that when the temperature rises slightly, the number of clouds and cloud area increases. That greatly increases the albedo, the reflectivity of the planet, so we get a compensating effect. In other words, natural processes are not balanced on a knife edge. They never could be what they are if they were. There are all kinds of buffer systems that interact.

BOHNING: But it makes very good press, doesn't it?

McMILLAN: Well, yes, I suppose. Have you ever heard of a guy named Peter Beckmann?

BOHNING: No, I haven't.

McMILLAN: Then I have to introduce you to this. I don't know how your time is going. I have lots of time.

BOHNING: I do, too.

McMILLAN: And I'm having fun.

BOHNING: All right. We'll keep going.

McMILLAN: These are two things. This is *Accuracy in Media*, by a guy by the name of Reed Irvine (25). I don't know if you're familiar with him.

BOHNING: No.

McMILLAN: This is a little sheet put out by Peter Beckmann, who is a retired electrical engineer at the University of Colorado (26). Both of these happen to discuss this global warming thing. In this one, Beckmann quotes a statement put out by a whole group scientists, saying in effect that the data just doesn't support what's being said. Here's a list of people who signed that. They're all pretty prestigious people.

Let's take a break, and while we're doing that, I'll get these copied for you.

[END OF TAPE, SIDE 6]

McMILLAN: Much of this is unpublished work that I just haven't had time to write up. That's one of the problems. I became concerned about this. Do you remember some years ago, I guess around 1983. There was a thing that looked like an atmospheric nuclear explosion in the South Atlantic area. Remember that?

BOHNING: Yes.

McMILLAN: In the Intelligence world, that's known as Event 747. Anyway, I began to think about natural background, if you're going to worry about test detection. There was an earlier case in point. I perhaps ought to mention that before going to Vietnam, and in fact during Vietnam, I spent about a third of my military interaction in the Intelligence field. Back in the 1950s, the only detection means we had for atmospheric nuclear explosions was acoustic, where we detect a long, low frequency acoustic wave. At that time there was an event that was not understood, which was associated with the middle of Africa. It was, I say obviously now, not a nuclear explosion, but it was likely a big meteorite. Now, if we take that kind of information to the President and he proclaims that South Africans fired off a nuclear explosion, it makes us look like a bunch of fools.

Anyway, I was concerned to look to see whether there are other phenomena that can be misinterpreted. The way the Vela satellite works is to detect the optical output of an explosion. A nuclear explosion is characterized by a two-pulse trace, because what happens at first is that all this radiation comes out. This dissociates the nitrogen, which recombines as NO and NO_2, which then quenches the light. But then, as it expands, the light comes through again. Okay? There is the double pulse, so there are some criteria that have been applied.

Anyway, I began to look at another phenomenon, which was a natural geophysical thing, known as earthquake light. You may never have heard of it, but for earthquakes that happen at night and are sufficiently big, like magnitude six or bigger, we frequently get not only light generation that seems to be associated with mountain peaks, even at ground level, but also aurora. So I went to ARPA, who own the Vela satellites, and got them to support this study.

We can explain the earthquake aurora in the following way. We've got, in the F layer as well as in the van Allen belts, electrons of number density about a million per cubic centimeter. Now, these electrons are thermalized. They're not just sitting there, because if they have any velocity at all, they're spiraling around the geomagnetic field lines. But what happens is, the field lines are far apart in the equatorial region, but they converge near the Poles. If an electron starts out with a velocity at a pitch angle with the geomagnetic field line, this determines the spiral around the field line; as the electron moves toward the geomagnetic poles, the pitch gradually gets less steep until the electron gets to the mirror point, and then it turns around. So these electrons are spiraling around the magnetic field lines and oscillating back and forth between the mirror points. The mirror points are generally above the atmosphere, because if they weren't, the electrons would intersect the atmosphere and get gobbled up.

Now, if we change the electric field even by a small amount, we can change the position of the mirror points—the depth of the mirror points—in the atmosphere, quite drastically. We can see this in the following way. We have at the surface of the Earth an electric potential gradient of about 100 volts per meter, perhaps 130 volts per meter. It is bigger on mountain prominences and accounts for the luminosity there. If we were to change the field at the F layer, by as much as, say, a few volts per meter—we are talking here about electron path distances of a few thousand kilometers (10^6 meters)—a few volts per meter changes the potential energy by a few million volts. That changes the pitch so that these electrons now come down into the atmosphere and produce an aurora in the usual way.

There was a dentist in Japan who decided that he wanted to get pictures of this aurora. He rigged up a camera that was triggered by the earthquake. He got some very nice pictures. I have a picture of these.

So that's one area of geophysical interest. Another one that I haven't published is the atmosphere of Venus. In this, just the barest facts. The Venus atmosphere has a pressure of about a hundred bars at the surface. It's at least 97 percent CO_2, with very minor amounts of other things. There have been a number of measurements of the scattering of solar radiation from Venus. There is Venus circling the sun over there. Looking at it from the earth, we get, at various angles, the scattering of sunlight.

It turns out that there is a Glory Effect. Another name for it is the Brocken Bow, named after the Brocken Peak in the Harz Mountains of Germany. It's quite an interesting thing. If you're standing on a peak and have the sun at your back and there's a cloud deck below where your shadow is cast, then the sun, being reflected back in a very narrow angle from the cloud, shows a halo around the head of your shadow. It makes me wonder whether this is the source of a lot of those religious pictures that show halos around the heads of religious leaders. That very narrow scatter is known as a Glory Effect. It's a very personalized thing, because if there is a guy standing next to you, his shadow is also next to yours, but when you look at his shadow, it doesn't have a halo. It's that narrow an angle.

Now, the back-scatter from Venus has this Glory Effect. For some reason the physicists conclude that the scattering centers have got to be spherical. That's just wrong. If we ask any airline pilot whether he has ever seen the Glory Effect, and go through this with him and find out at what altitude the clouds were, they were clearly frozen, so we can get the Glory Effect from crystal scattering also. That'll prove to be important in what I'm telling you.

Now, in fitting this back scatter, there's a guy at NASA Ames by the name of Jim Pollard who has flown many high-altitude measurements on the Venus scattering and obtained various parametric fits in which there are two parameters to get the best fit. One is the particle size, which settled on one micron; the other is the refractive index of the scattering particles, which turned out to be 1.44. The fit is pretty good.

The group, mainly sponsored by NASA, had a big convocation to determine what the clouds of Venus are. They never got a sample of anything for some dumb reason, but they were going to do this theoretically. They had a list of some thirteen candidate materials—all kinds of crazy things that no chemist would ever propose, such as mercuric oxide. Anyway, they concluded that the best fit was droplets of sulfuric acid—85 percent concentration. I always tell these guys that's too high because it's really half acid. [laughter]

I asked this guy Pollard, "Doesn't it worry you that the refractive index that you have determined is exactly the refractive index of solid CO_2?" "No." [laughter] But here's the way it goes. If we plot the pressure, the logarithm of the pressure, against the temperature in the atmosphere, then the ground has a high temperature, high pressure, so it's up here. Here's the ground. As we rise in altitude, the pressure gets lower and lower and the temperature gets lower. We get a curve that looks like this, and then it turns around just like it does in the Earth's atmosphere when we get up to the ionosphere. The temperature at the ground is 740°K.

Now, we can also see how this curve comes about. First of all, it's hard to measure the temperature in the atmosphere with any kind of a probe, because the reentry body is coming in at 10 kilometers per second. We can't just hang a thermometer out the window because the stagnation temperature is very high. So how do we measure it? Well, what they do—and this has been done mainly by the Soviets—is let the reentry body slow down below mach one and then pop a drogue chute. They can measure the temperature from the slowly falling reentry body.

The trouble is that that gets them below the cloud layer, so they don't have any information at all about the temperature of the clouds. The cloud layer occurs between about 55 and 60 kilometers altitude. Thus the error bars in the temperature are very large for that reason. If we plot on the same graph the sublimation pressure-temperature curve—of course, for dry ice—it narrowly misses this curve. If we believe the Soviets and take their statement that the atmosphere is practically isentropic, and calculate with the isentropic equation of state, then we actually get a crossing. This suggests that the clouds of Venus are really dry ice. This is kind of an amusing thought then, because as soon as these particles of solid CO_2 form, they will settle down, evaporate, and we get a continual turnover. The question is, "Where did all the CO_2 come from?" There's more carbon in CO_2 in the atmosphere of Venus than there is carbon on earth.

Did you ever hear of CO_2 wells? There used to be, and I hope there still are, wells out here in Death Valley that were drilled in search of oil, but what they found was CO_2. The reason is that various carbonates get too low and too hot and they decompose. That's where a lot of the dry ice used to come from, and maybe still does, I don't know. One of the professors at UCLA whom I haven't mentioned, Hosmer Stone, used to consult with those people in using high-surface-area carbon to absorb out any hydrocarbons, which would otherwise taint the ice cream.

BOHNING: That's interesting.

McMILLAN: Now, I went to NASA with this. There was a chief atmospheric scientist there by the name of Flood, and I had my little slide show ready to show him. I started with this and he said, "Wait a minute." He got up and closed the door. He said, "I'll tell you. You won't get a dime for this [project]. You know why? It's because there are all these thirty or so academic guys who have a lock on this, and they're not about to let this kind of an idea disrupt what they're doing." Well, that's the measure of the objectivity of our science.

Let me tell you one other geophysical thing. It has to do with thunderstorms. I think I may be the only person in the world who understands how thunderstorms work. There was a fellow by the name of [Everly J.] Workman at the University of New Mexico, a meteorologist who made some very interesting experiments. He took a plastic ice tray filled with a solution of a given salt, put electrodes in the two ends, and then progressively froze it from one end, measuring the potential with an electrometer as the freezing developed. The freezing is progressing now. Now, what happens is that one of the two types of ions gets preferentially incorporated in the ice lattice, and the other is preferentially excluded, so the liquid acquires one sign of charge and the ice the other. Workman measured potential differences up to 300 volts, but this happens only while the freezing process is going on. Why? Because when it stops, the little residual conductivity slowly discharges the charge buildup.

Now let's see what's happening in the case of the thunderstorm. We've had a few here in the last few days. In fact, there are some more expected tonight. We have the droplets formed in the clouds, and there's a certain amount of salt in the air. If you don't believe it, look at the way cars rust down near the beach. So the droplets constitute a salt solution. These particles may be several microns in size. There's a very interesting and curious phenomenon that happens, that as we lower the temperature with the sun sinking in the west, the amount of radiation received is declining, and the temperature is decreasing. As that happens—temperature going down—we don't get any phase transition, we don't get crystallization until we get down to about –40°C.

Now, you can understand from chemical physics what the reasons for that are. The surface tension of the droplet controls the orientation of the molecules in the surface, which is different from what the crystal prefers. So there's a big entropy effect that has to be overcome, and we do that by lowering the temperature. When freezing finally nucleates, then, of course, the temperature jumps up to zero, because we now have a two-phase system. But there's a kind of funny coincidence: the fusion process releases 80 calories per gram, and it takes 40 to get back up to 0°C, so in other words, only half freezes. Now we have a droplet with a little frozen chunk in the middle that has preferentially excluded one of the ions. So the frozen core has one sign of charge and the surrounding liquid has the opposite charge.

But another thing happens when this partial freezing occurs and heat is released. Namely, the surrounding air, which used to be in the vicinity of –40°C, suddenly is a lot warmer because of all the heat released from the droplets, so the air parcels start to rise. But the half-frozen droplets have inertia, and the rising air scrapes off the liquid carrying one sign of charge and carries it aloft much farther than the frozen center. Therefore, it's just like a [Robert J.] Van de Graaff machine. We've got a mechanism for separating the charges, and this mechanism then explains both the rising thunderhead, which happens because of the release of the energy of fusion, and the resulting lightning discharges. I think this satisfies all of the observations, but each one of these little items is a key input.

There are a couple of other geophysical things that I think maybe I've listed in there that are published. One is the decoupling of underground explosions (27). That came about because Edward Teller was sure there were mechanisms that would offer concealment of nuclear explosions. Sure enough, there is if it's done right. We've since done some experiments—not only with HE, but also in a Louisiana salt mine—that show that we get big decoupling factors like three hundred. In other words, we could make a hundred-kiloton explosion look like a one-kiloton—whose seismic signal would not even be seen outside the country. That was at one time a big political issue.

BOHNING: That was during the time of the nuclear-test ban treaties?

McMILLAN: Yes.

BOHNING: I think you were somehow involved in that.

McMILLAN: Yes. I was working through RAND at the time. I was in on the design of the Cowboy Experiments, which were conducted in the Louisiana Salt Dome, a big sphere, measuring the decoupling from high explosive shots. The essence of that problem is that if we have an explosion that is very closely tamped, and the explosion generates sufficiently high pressures that it causes physical displacement of the surrounding material—essentially it just pushes it out—finally it comes to a point where the medium goes elastic. At that point the physical motion of the material itself stops except for a vibration, and it launches a seismic wave. Now, if we take out that center region to which motion was imparted—that's like removing a big hammer—we get a much smaller blow on this acoustic shell.

Another thing, not geophysical in this case, has to do with the missing neutrinos. Do you know that story?

BOHNING: Oh, the ones they're looking for underground?

McMILLAN: No. The solar neutrinos. Do you know about them?

BOHNING: No.

McMILLAN: The problem is this. The initial experiment done by Ray Davis showed that there were not nearly enough neutrinos, if one understands nuclear physics, to account for the rate of energy radiated by the sun. That doesn't surprise me a bit, and I'll tell you why. More than that, we've now got a new detector on line, a Gallium detector in the Soviet Union, which shows the same thing. The reason that the second detector was important was that the Davis experiment could detect only those neutrinos that are quite energetic and that come from rare reactions in the sun. We have to make lots of extrapolations and assumptions about that reaction, whereas we'd like to have information directly on the main energy-production reaction, which is the combination of two protons to produce a deuteron.

Now, let me tell you how a star works. Assuming that we start out, after the big bang, with a lot of material around, there are nonuniformities. These nonuniformities tend to be exaggerated by gravity. There is a local density fluctuation that tends to grab other things. Then, if there is any angular momentum in the big bunch as it contracts due to gravitation, then we get a fair amount of angular momentum, which accounts for the angular momentum of the planets and the rotation of the sun.

Now let's look at the history of this. Imagine we go back in time and we're looking at the reaction rate in the sun for the energy producing reaction. Now, that's a bimolecular reaction. Again, it's a chemical physics problem, a bi-molecular reaction, so that the rate ought to go like this: reaction rate $\sim n^2 e^{-\Delta E^{\ddagger}/kT}$, where n is the number density of protons, $\Delta E^{\ddagger}$ is the activation energy, and T is the absolute temperature.

[END OF TAPE, SIDE 7]

McMILLAN: Now initially, prior to star formation, the number density is too low to give any nuclear reaction. Initially any energy generated has to come from gravitational contraction. The way gravity works is that collisions between these particles slow down their orbital velocities around the common center and convert the kinetic energy into heat. Eventually that slowing down leads to contraction, so the nuclear reaction is initially zero, but then as the density increases, the temperature finally begins to rise. As it rises the reaction goes faster, because the temperature is increasing so we get a nice reaction that then goes bubbling along.

Now we can actually put in the numbers using the gravitational constant and the mass of the sun, the size of the sun, and so on. Just converting the gravitational energy into temperature, using the heat capacity, we get a temperature of 2 kilovolts, which is what we need to get the nuclear reaction started. Now, however, here is the sun and the reaction is really going on only in the central core here, the very hottest part. Eventually what's happening overall is we get four protons getting converted into helium, plus a number of neutrinos. There are electrons, also, that come in here. Don't worry about that. The point is that the products occupy a lesser volume than the reactants, and moreover are not burnable at that temperature. So we have this furnace going on, and the "ashes" (i.e., helium) are collected in the middle. Before we can get new fuel in we've got to take those ashes out, so it waits for some kind of radial circulation to bring new fuel in. This is the part that the astrophysicists don't pay any attention to, apparently. They have that core sitting there cooking forever, but they don't say how it gets rid of the reaction products and brings in new fuel.

Now, what I claim is the reason for the missing neutrinos is that the sun has gone out! That ought to make headlines. But let me persuade you that it could happen. First of all, I've told you why it happens—because the ashes haven't been removed. But it can happen without our noticing in the following way: in the sun, given its great diameter, it takes about a million years for a photon to diffuse out. When we look at the outside temperature and calculate the rate of energy radiation, that's a reflection of what happened a million years ago in the interior of the sun and not necessarily what's going on today. So the sun could go out and we wouldn't even know it. [laughter]

This phenomenon of vertical circulation is highly nonlinear, like many of the hydrodynamic aspects—how do I say it? It's an instability. Now, let me talk about something more familiar—the atmosphere of the earth. We know that if we have a perfectly isentropic atmosphere, it would have a certain vertical temperature gradient. We could take a parcel of air from down here and put it up there, and it would still be in equilibrium because it expands as it cools, and reaches a region that has the same properties. If we want vertical circulation, then what we need is a situation where we take this parcel at low altitude and elevate it so that it expands—it's still hotter and thus less dense than the environment—so will continue to rise. It's an automatic circulation. Otherwise, we elevate it and if it's colder than its surroundings, it's denser, and subsides. We have that latter situation here in the Los Angeles Basin all the time.

What's happening is, the fuel density is declining because it's being burnt up. The temperature may rise for a while, and the reaction rate stays high because the temperature is rising, but eventually, if we run out of fuel, there's nothing we can do about it. The reaction comes to a halt. There are no more photons being produced, but gravitational contraction will continue to raise the temperature.

During that contraction process, what I believe happens is that whereas this had reached a stable configuration—in other words, no vertical circulation—the analog that I was just talking about is where we elevate a parcel and it subsides—but with a continued contraction and the change in the temperature distribution, it finally goes unstable. Then there is vertical circulation that brings in more fuel, and the reaction starts up again. So I envisage this temperature going like this [indicates] and then it sort of tapers off and goes down here. Then, maybe at some later time, it starts up again, but this time delay is not uniform. It's not a periodic thing, because it's an instability that's triggered by this behavior. I say, "Sure the neutrinos are missing; that's because the sun's gone out." But when I talk to the astrophysicists about this, I get nowhere. They don't have any answer, except that it's not the "standard model." [laughter]

BOHNING: Does that come back to your earlier comment about academic scientists?

McMILLAN: Yes. This has application, incidentally, also to what I call the autogenesis of smog (28). Not to make a point about the creation of the smog by automobiles, but it goes this way. People talk about temperature inversion as though that's a rare event. What they don't know is that every night, in every place, every land area on earth, there is an inversion. It happens because the land is a good radiator and the air isn't, so the air that's close to the land is communicating with it thermally, and then it gets cold. We might start out at noon with a temperature high near the ground and falling off with altitude. But at night, the temperature at a higher altitude is not affected because the air is a lousy radiator, but down near the ground the air gets colder.

Now, the sun comes up and warms the land, and it causes the foot of the temperature curve to move out; but if there are absorbers or dust in the atmosphere, another thing happens—and it doesn't take very much dust, because the air has very low heat capacity. Pretty soon a kind of temperature bulge develops. Putting smog in the air creates more smog, because the smog is held close to the ground. You see, air is cleansed on the one hand by washing the particulates out, and on the other by having the winds blow this junk over to Arizona, where, given time, it gets chewed up by the atmosphere. The cure for this, insofar as the automobiles produce the smog, is to have everybody get up late. [laughter] It turns out that on weekends, like Sundays when everybody sleeps late, the air tends to be a lot cleaner. Well, those are some of the geophysical problems I have been concerned with.

I want to give you this summary of MSA [McMillan Science Associates] activities. Since 1971, I've had this little business. We've done quite a few things that don't appear in the summary, but that gives you a feeling of it.

BOHNING: Excellent, because I wanted to ask you how McMillan Science Associates came about. We might as well discuss that now.

McMILLAN: Sure. That happened in 1970. The physics department at RAND got very disenchanted with RAND management. The physics department was essentially self-supporting with outside contracts. RAND management skimmed off the overhead, so there was no funding for recruiting nor any of the standard things that we wanted to do in running a business. The physics department decided to form its own corporation, which it did; this came to be called RDA. It has now been going on for twenty years.

I didn't want to go with RDA for a variety of reasons. First, the RAND physics department had been mainly concerned with strategic nuclear matters. I saw that this field was pretty well covered, but having just gotten back from Vietnam, I knew that the tactical arena badly needed some help, so I thought, "Here's maybe a place to contribute." Anyway, I started up this business and have been wrestling with that ever since.

BOHNING: Is most of your work still government related?

McMILLAN: Well, it's getting away from that, for the reason that the government has become very hard to work with. They've become nearly impossible. We have auditors and inspectors swarming over the place all the time, taking up everybody's time—totally useless from my point of view. We're a small outfit. We're not spending millions of dollars, yet the government, I'm sure, spends millions of dollars auditing us—and they have never come up with a single criticism of our activities or costs.

BOHNING: Do you have other people here working with you?

McMILLAN: Well, we're down pretty low now: there are only about three or four of us, for the most part. We were up to about twenty at one point, but that was when we had some business.

BOHNING: Have you always been at this location?

McMILLAN: Yes, essentially, but it's getting so that it's more and more difficult to stay afloat, so we're planning to move out.

BOHNING: What about the RAND physicists? How has their group done?

McMILLAN: They've done very well. They recently sold out to Logicon and made a pot of money.

BOHNING: Are there any other small groups involved with government projects?

McMILLAN: Oh, yes. There are quite a few small groups around. What we do mainly is operations analysis for a variety of things. That's just a kind of analytical careful thinking, the sort of thing that I described earlier on the bombing of the artillery sites. I think there's room for that in industry. We've got a small contract with one of the big corporations. I don't want to give you too much detail, but this corporation has a fleet of aircraft that handles a lot of their travel. The question comes, "Do they have enough aircraft? Do they need more or what?" So, here's a little analysis of what happened in October and November of last year, which gives the number of days in which they had zero requests for charter flights, and up to seven requests, which is the spectrum. That spectrum is shown here with a bar graph. We can fit it with a theory, which was developed from scratch and uses only the average number, the N bar. Here is the probability. Then, when we fold that in with the availability of the aircraft, taking into account about 13 percent maintenance, we can calculate how many of their requests could be fulfilled. Here it is—the theory gives ninety-three, and the actual was ninety-five. Pretty close. This tells them that they can't get rid of one of those aircraft. It's kind of amusing that we can do that with fair precision.

BOHNING: Yes.

McMILLAN: We've also done quite a bit of work with industries that themselves have military contracts, for example with Hughes. They developed a wonderful radar called the TPQ-37 (FIREFINDER) for backtracking shells. Somebody's shooting artillery at us and we want to know where the origin is. With that, they can see a one hundred fifty-five-millimeter shell at a distance of 30 kilometers and tell us where it came from. That's pretty damn good. One of the things that we did was to point out that it's even easier to see large missiles (29). We can see them at a much greater distance, so if we have SCUDS coming at us, we can locate where their origin is. That always involves quite a bit of mathematical detail, and we have to know something about the missile flight characteristics.

BOHNING: There's some controversy now about the Patriot effectiveness, whether it really was as effective as claimed. Do you have any opinions on it?

McMILLAN: I haven't looked at the data.

BOHNING: Let me go back. There are some other things I wanted to ask you about in some of your earlier scientific papers. I think we've covered a good deal of this, but you did a paper in

1957, “Determination of Rate-Constant Ratio in Competitive Consecutive Second Order Reactions,” (30) a standard physical chemistry kind of thing.

McMILLAN: Yes.

BOHNING: You had an acknowledgment to Donald J. Cram for it.

McMILLAN: Yes, he came to me with that problem.

BOHNING: All right.

McMILLAN: I worked it out then. A funny thing happened sometime later. I got a very irate letter from some guy in Sweden saying that he’d done all this before and published in some obscure Swedish journal. I was sorry about that.

BOHNING: Jack Roberts had a sabbatical at Caltech. I don’t have the right time frame. One of the reasons he took that sabbatical at Caltech is that he wanted to be close to you.

McMILLAN: My goodness.

BOHNING: Because you had gotten him into molecular orbital theory.

McMILLAN: Yes. He describes that in his autobiography. Have you seen that ACS volume (31)?

BOHNING: Yes.

McMILLAN: This was in 1951, when I was teaching for a year at Harvard and he was at MIT. The way he tells his side of the story is that he was going to teach a course in physical organic chemistry. He had promised to do this using molecular orbital theory, figuring that he would read [Michael J. S.] Dewar or whoever and get up on it, but he didn’t know anything about it, so he came to me with the typical “Roberts’ approach”: “You’ve got to teach me this stuff.” I spent quite a bit of time with him showing him what the quantum mechanics was about and how to use group theory and so on. Then he really seized the ball and ran with it, and did some very nice work.

BOHNING: Yes.

McMILLAN: But most of that interaction happened back east. I don't understand the Caltech connection.

BOHNING: All right. Well, they may or may not have been totally related, but he was commenting about your close friendship and being close to you because you were at UCLA.

McMILLAN: We haven't really interacted a whole heck of a lot. We're both very busy.

BOHNING: You also had your name on a paper with Saul Winstein (32).

McMILLAN: Yes.

BOHNING: Shades of your work with Louis Hammett?

McMILLAN: Well, except that this was a quantum mechanical calculation, my contribution to it. Saul had a Ph.D. student who had done the quantum mechanical calculation that I kibitzed on.

BOHNING: You became chairman in 1959.

McMILLAN: Yes.

BOHNING: You were commenting earlier that much of that was really part-time. How would you characterize the state of the department when you became chairman?

McMILLAN: Well, we have to go back in time a little bit. Bill Young had been chairman until I guess about 1948; then he moved over into the central administration where he was responsible for the Campus Development Program. He was succeeded by Francis Blacet as chairman about

1953, and in turn, by Blaine Ramsey about 1956, when Francis became dean of physical sciences. At that point Francis came and twisted my arm to become chairman.

Of course the department had, by this time, a good Ph.D. program going. By 1959 the Ph.D. program had been in operation about twenty years, but it was a sort of one-horse operation. That is, there was only one departmental secretary. If we wanted to get anything typed, we had to take it over to Central Steno across campus. There was a guy in charge of the laboratory who was an utter incompetent. He was almost never there and was totally useless. There was a woman in the business office who really did his job, a very competent woman. That was about it. We didn't have any kind of reproduction facilities. There was a machine shop, but no electronics shop, no computer facilities, no instrumentation support, et cetera. It was in a rather primitive state that needed to be brought into modern practices, so I took the job with the proviso that we recruit an individual, namely Dr. E. Russell Hardwick, who had been a Ph.D. student of mine and was then teaching at Stanford, to come and be my vice-chair. That was the first time there was a vice chairman, so there were two of us interacting there and we turned things around. I figured that if we got Bill Libby to come join us, that would be something of a coup.

BOHNING: Of course, you had some long-standing connections with him, too. That sure helped.

McMILLAN: Right. We were very good friends. We were able to get quite a bit of support from the administration. We had to fight for everything, but I got secretaries—like one secretary for three faculty members—scattered throughout the building. We reorganized the business office. I got a new laboratory director who was a crackerjack. He just retired, unfortunately.

Russ counted up one time and said that we had fired fifty-seven people. [laughter] I don't know what kind of an index that is, but when I left the chairmanship after six years, boy, the department was a humming organization. We had an electronics shop. We had an instrumentation shop, lots of instruments to work with. We had, of course, the machine shop. We had our own liquid air/liquid nitrogen production facility, due to the help Bill Libby gave us. We had our own reproduction and printing facility at that point. It just made life a lot simpler, so we could spend time on research. The faculty didn't have to worry about all these details.

BOHNING: What about the graduate program? You said it was well established, twenty years old at that time. Did it grow any? Did this bring in more graduate students?

McMILLAN: Yes. It has continued to grow and, of course, what really grew was the post-doctoral aspect. But there was another thing. I mentioned the golden years, so to speak, that we had when we were undergraduates, Jack and I. What I did was try to re-establish that. I put in an Office of Undergraduate Advising, as well as an Office of Graduate Advising. Those two still exist and there are all kinds of things going on over there, for example, a rogues picture gallery of the students. At one point, when I was teaching one of the p. chem courses and summer was coming up, I had a particularly good class of able students, many of whom would have liked to do research. I went around and put the arm on a lot of my colleagues for a little bit of research money. Not that they gave it to me, but they promised to hire students in the summer.

One of the students involved in that was a fellow by the name of Joncich, who later joined the NSF. He was the guy who established the NSF undergraduate research program patterned on the UCLA model. So there were long-range effects.

BOHNING: Yes.

[END OF TAPE, SIDE 8]

McMILLAN: I think that made quite a difference in the atmosphere for the undergraduates, because when they came to UCLA, thirty thousand students, they didn't have any "home." There was no focal point. The department was there, but it was just a bunch of buildings. But with the undergraduate office and the advisory system and records keeping so that we know who's who, it makes quite a difference.

BOHNING: There are some other things you've listed here under your academic side. We've talked about some of these. What about "On the Mechanisms of Exploding Wires" (33)?

McMILLAN: Yes. Charlie [Charles P.] Nash did that. He's now professor of chemistry at Davis [University of California].

BOHNING: I'm wondering how some of these ideas developed and led to a problem.

McMILLAN: Yes. Let's see. During that period, which was in the late 1950s, there was a big argument—again, a geophysical problem—about the moon craters. There was a bunch of misguided people who maintained that these were volcanic in origin. I knew very well that they were impact craters. Again, there's an interesting story—it interests me, anyway. There is a meteorite of some size that is in orbit, and at this distance from the sun, what that means is that it's moving at 30 kilometers per second, thereabouts.

Here's the kind of thing that I would learn from Edward Teller. HE [high explosive] has an energy density of about 1 kilocalorie per gram or 4 kilojoules per gram. Okay? Now, let's ask the question, "What speed V does an object need in order to have that kind of kinetic energy density, $V^2/2$?" If I express the energy in ergs, which I tend to think in terms of because I'm of the CGS generation, that's 4×10^{10} ergs/gram and V now is in centimeters per second. But if I put V in kilometers per second—a kilometer is 10^5 centimeters—then the 10^{10} goes away. I have now V in kilometers per second, and it's the square root of 8, which is 2.8, so I say something moving with a (relative) velocity of 2.8 kilometers per second has a kinetic energy density equivalent to TNT.

Now, the kinetic energy goes up like the square of the velocity. That's pretty potent, but maybe it's moving in the same direction as the moon, so the relative velocity is not that high. Then we have to consider the escape velocity. The escape velocity from the earth is 11.2 kilometers per second. It's still pretty high compared to the number we just calculated of 2.8 km/sec.

Now, the next question is, how is this kinetic energy communicated? How does it get converted into internal energy and cause an explosion upon impact? That's where shock waves come in. Well, shock waves I know about from nuclear weapons design. What happens is that the meteorite runs into the surface and a shock waves develops which moves both ways, through the target and the missile. There's a theorem about strong shocks, that half of the shock energy goes into residual mass motion, and the other half goes into heat.

So what happens, then, is that this missile bumps into the surface. The shock wave races back through it and tends to reduce the forward motion to some extent. Only half of the energy of the shock wave is left as residual kinetic energy of the material, which tends to reduce its forward motion, but the other half goes into heat. The missile doesn't penetrate very far because it is now heated to an enormous temperature and is flying apart in all directions, so we have a very shallow crater formed. Moreover, when the dust settles, the crater has a big bump in the middle because that's where the material was elastically compressed and it springs back up when the pressure is released. This is exactly what the craters in the moon look like.

Now, I've wondered—there are still meteorites flying around; this process ought to create a lot of light. If we just sit and watch, we ought to see these flashes on the moon, but I've never been able to find anybody who's seen one.

That's sort of the beginning of my interest in explosions. Then this has another curious feature, and had been done years ago by astronomers who were interested in explosions. If we look at the current in an exploding wire as a function of time, we've got a big initial pulse. Then the current declines and then later comes alive again. So in between is what is called the dark pause.

This is written up. I don't know if you have a copy of that paper there with Charlie Nash (33), do you?

BOHNING: I don't know if I do or not, let me see what I've got here.

McMILLAN: We can pull this out in the *Physics of Fluids*.

BOHNING: All right. I have a note that somebody gave me here, saying our library doesn't have it.

McMILLAN: Oh. Let me see if I have a copy of it here.

BOHNING: It's surprising that the Penn library does not receive it.

McMILLAN: Do you have copies of those decoupling publications?

BOHNING: I think I do. I'm pretty sure I do.

McMILLAN: Oh, my. I don't have copies of all these things. They're in boxes somewhere. It's a damn nuisance not to have access to that stuff.

BOHNING: I can get a copy. It's just a matter of sending for it.

McMILLAN: One of these days, I'm going to get dug out so I have things available. Well, sorry.

BOHNING: That's all right. We can get a copy of that later.

McMILLAN: You'll see that it involves, essentially, the Debye theory of conductivity. What's happening is that initially, the wire is a good conductor, which causes this large current pulse. Then the temperature heats up to a point where the vibrations of the atoms are such that they cause a great scattering of the conduction electrons and a consequent great increase in resistance. Then as the material expands, it goes over into essentially a gaseous type of conduction, and the arc strikes again because there are now a lot of free electrons. We can control the rate of expansion by putting gas around the wire that impedes the expansion. We studied the expansion as a function of the density of gases around the wire. We pretty well understood that.

There was a program that the Air Force was running back at the Cambridge Research Center at the time. There was a symposium on exploding wires and we made a presentation.

BOHNING: Something with hepatitis?

McMILLAN: Yes. I'd just gotten back from Vietnam when I was stricken with one of the milder forms of hepatitis that was rife out there, a peculiar disease because little is understood about it. There are no drugs or anything to take for it; the only prescription is to go to bed. Well, I was in bed, and not only that, committed to bed for God knows how long, because the doctors didn't know the length of time. Fortunately, periodically, I was having various blood analyses made, which is the way to keep track of hepatitis. There is an analysis of the blood called SGOT [serum glutamic-oxaloacetic transaminase] that's a measure of the destruction of the liver cells. So, having nothing else to do, I decided I'd develop a theory of hepatitis. I don't think I have that paper available. I never did publish it—partly because in that Defense Science Seminar there was a guy from the Army Research Laboratory who's a hepatitis specialist. I sent him my paper before publishing and he criticized it to death.

Nevertheless, it's a strain of hepatitis and a common one that I found out later had been studied quite a bit in Vietnam. It had a six-week period. The Army doctors in Vietnam split a group of four hundred into two groups, one of which they put out at hard labor, digging ditches and whatnot. The other group they kept in bed. There wasn't any difference at all in each group's progress, but I didn't know that at the time. At any rate, I set up a little theory of the following sort, where someone gets a bug and it's a growing infection that grows by the normal scheme, but it also induces the production of antibodies, so that the bug is multiplying, but then the antibody begins to take over. Ultimately, if the patient survives, the antibodies knock the disease down, but the antibodies continue to multiply until the bug is all gone.

BOHNING: All right.

McMILLAN: Now, I had the blood chemistry analyses, and I could fit that curve right on. Then it occurred to me later that this is also the situation for an ops analysis of a guerrilla incursion, where the guerrillas come in and they attack the government. The government produces the antibody troops. Actually, this also explains, I should mention, the post-disease immunity that we wind up with. Here it is. So, I've got lots of things going. I thought that sometime I'd publish that in one of these ops analysis journals with these two versions.

BOHNING: "Collective-Coordinate Treatment of Electrolytes" (34) was another one of your papers.

McMILLAN: Yes. The guy who worked with me on that was Ken [B.] Eisenthal, who has really done very well at Columbia now with his picosecond laser chemistry studies. What this did was take a development by [David J.] Bohm and [David] Pines, in which they were dealing with collective coordinates. They weren't working with solutions, but rather were working with a gas of protons or positive ions in what's now called jellium, with a background of uniform electron charge. What one is interested in is getting the interaction energy essentially of the ions. For that we made a colossal blunder, which turned out not to matter, because of clean living, I guess.

Let me point out in the following note—I don't know if we want to discuss it, but the most complicated thing we could analyze with our Fourier method was this kind of a potential function. This was for charges of unlike signs. We don't have to worry about the ones with like signs because they stay out of each other's hair. But here this has the defect— which was a simplification for the mathematical treatments—that it would allow unlike ions to get as close together as they might. The thing that saved us was that we used a cut-off at small wavelengths, so that we cut all of those out. Our calculations of the mean ion activity coefficients extend up to much higher concentration values than those we get from the Debye theory. It worked out very nicely. It's better to be lucky than to be smart. [laughter]

BOHNING: I have a number of things here yet. Alternative explanation of the periodic system of elements?

McMILLAN: Yes.

BOHNING: There's no paper published on that one, is there?

McMILLAN: No. I haven't published that one.

BOHNING: I was curious about that.

McMILLAN: Let me tell you about that, because it's kind of amusing. I'm going to have to clean off this board, I guess.

BOHNING: Would you mind if I took a picture along the way here while you're working at the board?

McMILLAN: Quite all right.

The idea behind that item is to use the statistical model of the atom and to see what one can learn from that about the chemistry. I have since written this up as a preprint and will give you a copy.

The preprint does three things. First, it shows how to write the grand partition function for interacting particles as though they were independent, thus allowing an easy summation process that ends with showing that the occupation number for a given quantum state is the same as for truly independent particles: $\nu = 1 \big/ \left(e^{\beta(\varepsilon-\mu} \pm 1 \right)$.

The second thing is that it derives an equation of state, taking account of the statistical interactions—whether bosons or fermions. The third thing it shows is that within an atom, electrons of a given angular momentum occupy a spherical shell bounded by spherical surfaces both outside and inside. If this shell is narrower in radius, the larger the angular momentum. This is what explains the striking similarity of the chemistries of the rare earths: when the 4f shell begins to fill, the 4f electrons are confined to a narrow radial regime that is embedded in the atom—far from both the nucleus and the outer valence electrons. The valence electrons, which determine the chemistry, think that the 4f electrons have been added to the nucleus (so that they cancel the progressively-increasing nuclear charge) and the chemistry of successive rare earths is almost unchanged. But insofar as the valence electrons can penetrate to a small extent inside the 4f shell, they feel the increased nuclear charge, which accounts for the small regular Lanthanide contraction.

I wanted to do much more than this. There is a close analytic fit to the Thomas-Fermi Potential that was produced by a fellow by the name of Tietz many years ago. It turns out that using his analytic fit, we could integrate the radial-phase integral analytically. Then I wanted to take that and look and see not only when they get the first p, d, f, etc. electrons, but also when the subshells come in, but it's not accurate enough for that. It gives the first appearance as Fermi did. Fermi did it numerically; this gives an analytic fit, but it's not accurate enough to give the subshells.

[END OF TAPE, SIDE 9]

McMILLAN: I didn't mention in the geophysical business another project that I've had that's been revisited several times—detecting tunnels. This was a problem in Vietnam, where the VC used extensive tunnels, but also came to be a kind of major problem in the Korean DMZ, where the North Koreans had tunneled under the DMZ. There were three known tunnels, but defectors have said that there may be as many as twenty-eight. Now, whether it's still a military problem or not, I don't know, but the tunnels that were discovered were big enough that the North Koreans could march a division through in one night. To have that size unit suddenly appear in the rear of our forces might be rather embarrassing.

There have been lots of conferences, some with the Koreans, on tunnel detection. The way they're doing it in Korea is punching down deep drill holes every 2 meters, which is a hard way to find tunnels. Anyway, I had the idea of causing the tunnel to reveal its presence. One of the problems is that the medium is very nonuniform, not like the ocean at all. A tunnel presumably has characteristic vibrational modes, characteristics that say in essence, "I'm a tunnel, not a rock," because it has a long extension and a certain diameter and so on. If one could excite those vibrations and then get them imposed on some kind of a background of an electromagnetic wave, then we might be able to see the presence of the tunnel, which otherwise we couldn't see. Then if we have enough sensors around, we might even be able to get some directional feel for it. Anyway, we did a study on that, and it looks pretty promising (35).

BOHNING: It was done here?

McMILLAN: Yes.

There's one thesis I didn't tell you about and which we have not published yet, done by a fellow by the name of Doug Harris, who is a native of Haiti; actually, he's a U.S. citizen, but he was born in Haiti. A very bright kid. We looked at the following problem. I don't know if you recall the way the Einstein coefficients are usually derived. What one says is that there's a two-level system—an atom, let's say, with two levels, an upper and a lower level. We say that the population of the upper level compared with the lower—that is, the number in the upper compared with the lower—is e to the minus beta times the energy.

Then we set up the equilibrium with this case and out comes the Planck radiation and, incidentally, the Einstein coefficients that go with that. There's only one trouble: I can sort of read between the lines and see that Einstein was a bit worried about this, but he never quite addressed it directly. It has to do with the fact that in this derivation, the atom is stationary. If the atom moves, then the radiation is Doppler shifted, among other things. Also, angles and everything else changes, so it must be done relativistically. That's what Doug Harris did; he did the whole problem relativistically. It changes the definition of the radiation coefficients, but not in a way that perturbs the agreement with experiment.

BOHNING: What was it that you said way back in that first paper? "When there's more time to do more experiments?"

McMILLAN: Yes. [laughter]

BOHNING: You had a very recent paper on Stokes' Law (36).

McMILLAN: Well, yes.

BOHNING: You also had that very comprehensive one on the Virial Theorem (37).

McMILLAN: Yes. That was another graduate student, a French boy. I had been working with the Virial Theorem for a long time. It's curious that here is a theorem that has all the generality of Newton's laws but separates out the kinetic and potential energies, yet we never hear of it in the mechanics courses or anything. So we wrote a comprehensive paper on that that does it in forty-seven varieties.

BOHNING: Then the Stokes' Law was the most recent one, it looks like?

McMILLAN: Yes. I have another one in press, which is number theory (38).

BOHNING: Oh, yes, that's right. Fibonacci numbers. Is that right?

McMILLAN: Well, it's a Pell equation. Did I send you a copy of that?

BOHNING: No, it's just on the list. How did you get involved in that?

McMILLAN: In a silly way. I used to sit on an advisory board for Oak Ridge. As a consequence, they always sent me their Quarterly Reviews. There was a guy there whose name was [V. R. R.] Uppuluri, who had a little puzzle section called, "Take a Number." One of the puzzles attracted my attention because it involved a question, a sequence of numbers that had a certain odd characteristic. It's closely related to the (L)(L+1) factor that occurs along with the degeneracy 2L+1. If we make a sum of squares from 0 up to L, that is $0+1^2+2^2$ and so on, the answer is (L)(L+1)(2L+1) divided by 6. [laughter]

We can relate that to some of the quantum mechanics things. This is what attracted my attention. Starting from that end, it was a bitch of a problem, but anyway, I solved it by fooling around with it, and then later discovered that some of these numbers were the same as what are called the Mercene numbers in diophantine analysis, so I sent it off just as a casual correspondence to this guy. A fellow there by the name of [L. M.] Hively had also been interested in this problem, only he greatly generalized it.

Out of that grew this paper, and it's had a spotty history. Hively suggested that we publish it in the *Fibonnaci Journal*, and there the dadblasted editor required all kinds of changes and then ultimately rejected it and ruined the paper, to my way of thinking. It's now being published in the *Proceedings of the Tennessee Academy*. One of those things that just grew up.

BOHNING: Well, I think I've covered all of the ones I had on my list, in terms of your scientific papers. I think we've gone through the military technology pretty well. I think we did cover the nuclear technology. I don't know if there's anything else you want to add along those lines or not.

McMILLAN: Well, let me just read down this list of things I thought I would mention to you if you were interested.

I spent quite a bit of time in dealing with this shell backtracking business, looking at artillery shell trajectories. Now, these have been done largely by computer, but I developed some analytic solutions that I can put on my little hand computer and get very accurate results. It would seem to me that that would be useful in the artillery business, not in calculating *ab initio* trajectories, but in making corrections. If the forward observer says you're 100 yards over and 10 yards to the left, and you want to make the corrections, I can tell you how to do it very quickly. But I haven't been able to sell that to any of the military.

There's a bunch of stuff that I did years ago on the contribution of nuclear motion to the equation of state. Usually that's totally ignored. I don't know whether you came across this little paper on the equation of state.

BOHNING: Oh, yes. Another one I'd had here.

McMILLAN: That's a thermodynamic paper using the statistical models, just measuring the sublimation pressure (39). That's kind of tricky because of the thermal transpiration effect. Here's the one I'm thinking of here. This is adapting the Thomas-Fermi-Dirac model to predict the compressibilities (40), using a scaled volume, and a scaled compressibility. We covered the whole periodic system with that.

Then there's another thing that I was quite happy with. Here's a little paper, "On the Compressibilities of Simple Metals" (41). This uses the Virial Theorem and assumes that the electrons are free in these simple metals, so that gives the simple expression for the change in the potential energy of the function of the scale parameter. That fits pretty well. We derived the compressibility from the heat of vaporization, of all things. This is our theoretical value, and there is the experimental value, right close.

BOHNING: Yes.

McMILLAN: It's kind of fun to find that kind of a connection between a heat of vaporization and compressibility.

BOHNING: Well, it's intriguing how you've had several general themes, I suppose, yet you've really explored on a broad landscape at the same time.

McMILLAN: Yes. One of the things that is interesting is that there very few papers that are on the same subject.

BOHNING: Yes. You've not spent your life beating one subject to death, as it were.

McMILLAN: Yes. That has a disadvantage in that I then don't become known as an expert in that particular arena, but that never bothered me much. I did what I did. [laughter]

BOHNING: It sounds like you enjoyed it.

McMILLAN: Oh, yes.

BOHNING: As you were talking about it, I could tell that you really found a lot of enjoyment in what you were doing—the challenge of solving a problem.

McMILLAN: Yes. That's part of my problem. I enjoy it while I'm solving a problem, but once I get it solved then it tends to go on a back burner and not get published.

BOHNING: That's unfortunate, because there are so many of these things that we've been talking about here that haven't been published.

McMILLAN: Yes.

BOHNING: How many Ph.D. students did you have?

McMILLAN: Oh, totaling about a dozen. Not a whole lot, but about par for the people in physical chemistry.

BOHNING: That's what I was going to ask you.

McMILLAN: Oh, yes. Well, one time back in the early 1960s I made a check, and Saul Winstein had put out 5 percent of all the Ph.D. theses in the university. He was an unusual guy.

BOHNING: Oh, yes. He was.

McMILLAN: Did you know him?

BOHNING: I didn't know him, but I read his papers as a graduate student and knew of him through his work.

McMILLAN: Yes. He was a pretty intense individual.

BOHNING: There are lots of stories about him.

McMILLAN: Yes. The one I like is that some graduate student was giving a paper at one of the meetings of the ACS. Saul always had a way of pursuing the question; he'd get a question in his mind and he wouldn't let up until it was answered. He had some objection to what the kid was saying. The student answered. "Well," Saul would say, "it doesn't sound quite right to me because of such and such." It went on for three or four exchanges, and finally the kid in desperation said, "Well, Professor Winstein, I just don't know. Now argue that!" [laughter]

Saul's son, Bruce, is a nuclear physicist in Chicago.

BOHNING: I didn't realize that.

In view of all of your military and government activities over all these years, you've commented about the pluses and minuses, things that need attention. In view of recent political events—the breakup of the Soviet Union, things of that kind—what do you think the effects are going to be on the military?

McMILLAN: Well, I'm afraid that it's going to be very bad, because we've got a bunch of amateurs running the country, frankly. The reason that the Gulf War went so fantastically well was that [President Ronald Wilson] Reagan had supported the buildup of these new hi-tech systems. I was deeply involved in a lot of that for the Air Force. I ran a summer study on the air-delivered ordnance. You may not appreciate the difference this makes. For example, in Vietnam there was a key bridge known as the Thanh Hoa Bridge, near the coast of North Vietnam, where all of the infiltration traffic came down. Naturally the military wanted to knock that bridge out of action. The enemy didn't want that to happen, so they surrounded it with missiles and triple A. We lost thirty-five aircraft against that bridge, and didn't knock it down—thirty-five aircraft at about four million bucks apiece! But when we got the laser-guided bomb, the first mission dropped that bridge in the river. There are some things we cannot do without accurate ordnance, and it does a lot less damage to the countryside if we knock out what we want to knock out and leave the rest standing.

So I ran the summer study for the Air Force. Again, a frustrating damn thing, because we knew what weapon systems we wanted to promote. I took a group over to Germany to brief General Charlie [Charles A.] Gabriel, who was then the head of USAFE, the U.S. Air Force in Europe. We met in his conference room, but before I was allowed to get up and make my speech, Charlie sat there and told us all the things that he wanted, and they were exactly what we were recommending. Practically gave my speech. Then, when he was later made chief of staff, nothing seemed to happen for quite a while. You know, it takes about ten years for the system to respond.

But I was invited to sit on a committee of the Defense Science Board looking at munitions acquisition, so I had another chance at it. This was five years later. I discovered we were still buying dumb bombs. I raised hell about it, and by God we got some changes made, so maybe that made a contribution. It was largely smart munitions that made the Gulf War successful.

Now what Congress is doing is cutting off all of the new hi-tech procurement, which means in a decade the stuff we have is going to be ten years old. Not only that, but let me tell you another thing. We specialize in ops analysis here. I want to show you an economic analysis concerning the B-2 bomber. Here are the DOD [Department of Defense] budget and the Air Force budget. Now, let's say we put in a principal p, which we can think of as one billion dollars.

Now, that money goes to the direct employees and the subcontractors as salaries. By and large, at these salary levels, what we're talking about is that something like 10 percent may go into such things as capital investment, housing, and other capital things that employees buy. Ten percent of that salary goes back into the Treasury as income tax. That leaves 80 percent. That 80 percent gets spent on clothes for the kids, food, and all that stuff that goes into the economy, so that there's another echelon down here in which the same thing happens—goods and services. We compound that so that here's the analysis. We put in the principal p, and x—which is now 80 percent—moves down to the next level. So what we're going to take is, through all these echelons, x^k. That means that we get $P/(1-x)$. If x is now 8/10, that means that this is [10/2]P, and that multiplies this input by five. That turns out to be a well-known factor in economic analysis. If we put in something at the top, it gets reused, recycled, and the factor is about four or five. I won't argue which it is.

Now, the total tax return that we've got is 10 percent of that, so that means we put in one unit and get back into the Treasury half a unit, and in addition we've got high employment and we've got a B-2 aircraft.

BOHNING: All right.

McMILLAN: Now let's do it on the other side. We throw these people out of work. I don't mean just the B-2 people. I mean everybody who depends on them. We now have to support them by some kind of welfare, unemployment compensation. That's a bare subsistence level, so we don't get much in the way of return tax. All this unemployment and everything else, and we get nothing for it. Now, that seems to me so damn simple to understand. It's not a good argument unless the thing we're going to get is useful, but there are many such things. In particular, the B-2 is a hell of a useful tool, but these people in Congress make the dumbest arguments. Here comes the Air Force and they want a Minuteman III. The Congressman says, "Why do you want a Minuteman III? We gave you the Atlas and the Delta and the Minuteman I and II, and you didn't use any of them." [laughter] What do you do with people like that?

BOHNING: Hope they throw them out of office.

McMILLAN: That's right. Maybe we've got a chance now**.** Vote these guys out. They talk about a peace dividend. That's the stupidest thing in the world. What do they want? They want to buy more votes. I have a plan. If I ran the zoo, I'd fix it so that anybody on welfare is temporarily off the voting rolls, and that way there wouldn't be any connection between buying votes. It's totally unreasonable, of course, because nobody would buy that.

BOHNING: Interesting approach.

McMILLAN: All right, let me see.

[END OF TAPE, SIDE 10]

McMILLAN: Incidentally, out of this statistical model comes the Madelung Rule, the way in which the atomic electron shells are filled in sequence. It goes least $n+\iota$, then least n. That's one of the things that comes up. I'm in the middle of an article for the *Encyclopedia of Applied Physics* on capacitors. It's a joint paper with those guys, which is a kind of a pain in the neck because of trouble with the editor. He wants it short, and there's no way to make it short and still have it be good.

There's another thing that I wanted to mention. For many years I have provided a weekly question for the chemistry department calendar that announces forthcoming seminars and so on. This is a list of five hundred or so of these Chemysteries (44).

BOHNING: That's interesting. Now, what were these for?

McMILLAN: Well, they appear in the weekly calendar. Let me get it. This is the calendar for this period.

BOHNING: Oh, yes. How long have you been doing this? You've got five hundred?

McMILLAN: Ten years or so. Twelve years. Now, any of this stuff that you'd like to have, we'd be glad to send you.

BOHNING: Can I take this one along or is it your copy?

McMILLAN: I'll make a copy.

BOHNING: I would like a copy of that, also.

McMILLAN: You can have this just like it is.

BOHNING: All right.

McMILLAN: But remember, it's way out of date.

BOHNING: Right, but it's still very useful. Well, we'll just use that one as an example of these. They vary all over the place.

McMILLAN: Oh, yes.

BOHNING: Of course, some of these things, which may on the surface appear to have a simple answer, obviously do not.

McMILLAN: I try to make them so that there may be answers at several levels, so that the freshmen can get something out of them.

BOHNING: We haven't talked about any of your consulting work.

McMILLAN: No, not really.

BOHNING: And there's a long list of government committees and activities that we haven't discussed.

McMILLAN: Right.

BOHNING: Is there anything particular that stands out in your mind in those areas?

McMILLAN: Well, in the government committees, much was concerned with nuclear weapons and weapons effects, but then I was also for twenty or so years a member of the Air Force Scientific Advisory Board. I was chairman of the Weapons Advisory Group that advised both on nuclear and conventional weapons. In the course of that, I was chairman of the Divisional Advisory Group at Eglin Air Force Base, where they developed all these weapons for about ten years. So I was deeply involved in that side of things.

BOHNING: You mentioned a few of the people with whom you've interacted along the way in these areas. Are there any others?

McMILLAN: On the military side?

BOHNING: Yes.

McMILLAN: Oh, yes. I mentioned General Benny Shriever, but there was also Major General Bill Maxwell at Eglin, who later directed the Supersonic Transport Program. There was a Navy admiral, Noel Guyler was his name. He was in charge of what was called the Brown Water Navy (the Riverine Forces in Vietnam). Then later he became CINCPAC [Commander-in-Chief, Pacific Command].

I mentioned Admiral Sharp, who wrote the book. I'm continually amazed at the education these military officers have. For example, Lew Walt, who became a four-star Marine general, wrote three books. Westmoreland has written two or three books. I used to argue with [Secretary of Defense, Robert S.] McNamara occasionally.

BOHNING: Was he a tough person to deal with?

McMILLAN: Well, yes. It's hard to fathom, because he didn't have any scientific training. In a briefing one time, the guys were telling him about some radar system. About three quarters of the way through he says, "Now, what is a dB [decible]?" How do you deal with that?

I knew General [Maxwell D.] Taylor pretty well. I knew Ambassador [Henry C.] Lodge, Ambassador [Ellsworth] Bunker, all from Vietnam experience. I knew a lot of the people on the Vietnamese side, like General Thieu, who was the president, and Air Force General Ky.

BOHNING: Have you ever thought about writing about your experiences?

McMILLAN: Yes. In fact, that touches a sore spot. When I came back from Vietnam, I wanted to get all this stuff written down, because we made a good many developments that would be useful, but I could never get support for that. We spent in Vietnam about a hundred and twenty five billion dollars, and DARPA [Defense Advanced Research Projects Agency, now ARPA] alone about twenty billion in development. You'd think that they'd want to have some record of that. I never managed to get any kind of support. It's doubly frustrating because I had proposed at one point to look at the Intelligence problems of this kind of infiltration. I had made a proposal to DARPA, and they took that proposal and gave a contract to a contractor in Santa Barbara, whose president then called me up. He said, "Look, we don't know anything about this. Will you take a sub to do this?" I said, "Hell, no." That kind of thing just burns me up.

Another time I made a proposal about fortifications in the Korean DMZ, which greatly resembled the situation in Europe but on a smaller scale. The guy there called me up and said, "Will you come in and discuss this?" I went to Washington and went in to talk to this fellow. There was another contractor sitting there. He got the contract. I guess I don't know how to play the game.

BOHNING: Amazing. Well, is there anything else on your list?

McMILLAN: Well, let's see. We haven't talked about fuel air explosions. We devised a facility for a simulation of large nuclear explosions, a kiloton or so, using fuel air explosions.

There's an interesting geophysical problem, a geophysical effect that was discovered here at UCLA by Louis [B.] Slichter, who was one of the eminent geophysicists, head of the Institute of Geophysics.

Namely, in the big Chilean earthquake of 1963, he had just installed a new kind of instrument called a gravimeter that he had developed. It was able to spot the normal modes of vibration of the earth, various kinds of normal modes. There was one of these, the one of lowest frequency, which he eventually interpreted as being due to a solid core inside the liquid core of the earth, which was set in vibrational motion. Dave Griggs and I worked out the theory of that one afternoon at RAND. Also it poses an interesting question from the physical chemistry point of view, because we have an increasing temperature and an increasing pressure as we go down in the earth. We have to take into account the angle of the melting point curve of the stuff that's in the middle of the earth. What we find is that there's a crossover. That is, the temperature is high enough when you're not quite to the center that keeps things melted, but the pressure eventually causes the formation of a solid in the center, so it works out very nicely. It's all physical chemistry. In fact, I used to tell my students that everything's physical chemistry when it comes right down to it.

I once made a proposal to NASA to measure the one-way speed of light, but I ran into a woman there who didn't know which end was up, and I gave it up. She just couldn't comprehend what I was talking about.

One of the fundamental papers in ops analysis was due to [Frederick William] Lanchester. I don't know if you've ever run across Lanchester.

BOHNING: The name is familiar.

McMILLAN: It is essentially chemical kinetics. There are red forces and blue forces that kill each other off at a certain rate. If we write down the rate equations, then we get out the Lanchester results, the main one of which is "divide and conquer." That is, if we can bring to bear most of our force on half of his force, then we can really annihilate him. He shows how this was done by Wellington in meeting the Napoleonic forces, and also, a classic example is the engagement of Nelson at Trafalgar, in which he cut off pieces of the combined French/Spanish fleet and defeated them piecemeal. Very nice piece of analysis.

We adapted some of that to our analysis of the European situation, where we had developed a whole different scheme for defense. It would have been a lot cheaper than what we were doing. [laughter] Ah, well.

There's another area that I haven't talked about at all in relativity where there are some quite interesting aspects. Take the structure of the electron; physicists have given up on that. It's treated as a point charge, but of course that's unrealistic because a point charge has infinite energy. On the other hand, it can't have any extension because that's not relativistically invariant. So how can the electron be treated as a point charge, which would have an infinite self energy? Ever since I returned from Vietnam in 1969, my research has been dominated by this nagging question.

My interest in this question began when I calculated the energy in the electric and magnetic fields about an electron. For example, the energy in the electrostatic field, $\mathcal{E} = e / r^2$, outside a sphere of charge e and radius r_e is:

$$E = \frac{1}{8\pi}\int_{r_e}^{\infty} \mathcal{E}^2 d\tau = \frac{1}{8\pi}\int_{r_e}^{\infty} (e / r^2)^2 4\pi r^2 dr = (e^2 / 2)\int_{r_e}^{\infty}\frac{dr}{r^2} = e^2 / 2r_e.$$

This is the same as the energy of charging a spherical shell of radius r_e:

$$\int_o^e q dq / r_e = e^2 / 2r_e.$$

But the electron also has a magnetic moment, $\mu = e\hbar / 2mc = e\lambdabar / 2$, where $\lambdabar$ is the Compton wavelength = Zr_e and Z = 137 = $1/\alpha$, the fine-structure constant. Now the magnetic field $\vec{B}$ due to a dipole $\vec{\mu}$ at distance $\vec{r}$ is: $\vec{B} = [\vec{\mu} - 3(\vec{\mu}\vec{r}_1).\vec{r}_1] / r^3$ and $B^2 = [\mu^2 + 3(\vec{\mu}\vec{r}_1)^2] / r^6$. For the magnetic field energy we need the integral from some minimum $\breve{r}$ to ∞:

$$E_{mag} = \frac{1}{8\pi}\int_{\breve{r}}^{\infty} B^2 r^2 d\Omega dr.$$

The scalar product is $\vec{\mu}\vec{r}_1 = \mu\cos\vartheta$ where ϑ is the angle between the directions of $\vec{\mu}$ and $\vec{r}$. Thus:

$$E_{mag} = \frac{\mu^2}{8\pi}\int_{\breve{r}}^{\infty}\frac{r^2 dr}{r^6}\oint d\Omega\left(1 + 3\cos^2\vartheta\right) = \mu^2\int_{\breve{r}}^{\infty}\frac{dr}{r^4} = \frac{\mu^2}{3\breve{r}^3}$$

since $\left\langle\cos^2\vartheta\right\rangle = 1/3$. If, now, we take $\breve{r} = r_e$ this gives over fifteen hundred times the mass energy mc^2 of the electron!

I ran into Edward Teller at an Air Force meeting and showed him this calculation. "Oh," he said. "You have to cut it off at the Compton wavelength $\lambdabar$." At the time, I didn't understand why. But clearly this would greatly reduce the calculated magnetic energy. The next time I saw Edward I reminded him of his injunction to "cut it off at the Compton wavelength $\lambdabar$." His puzzled response was, "Did I say that?"

Anyway, this whole issue seemed to me to be very important to both physics and chemistry, since there is only one electron. But then it occurred to me that a proper theory of the electron structure should also explain the masses of the other two charged leptons: the muon (mass $\approx$ 206.77 m_e) and the tauon (mass $\approx$ 3477.6m_e). So I have been working on this problem off and on ever since. Along the way I have found several other connected questions:

1. Can an electron interact with its own fields (i.e., electric and magnetic fields but also its quantum mechanical field ψ)?

2. What about subtraction of the potential of the electron *a la* Hartree?

3. How to explain the electron magnetic moment, $e\bar{\lambda}/2$?

4. Is the electron mass of purely electromagnetic origin?

5. Where do the various neutrinos fit in?

In trying to answer such questions, I tried to devise a model of the electron that would fulfill its well-established properties:

charge: e

classical "radius": $r_e = e^2/mc^2$

mass energy: $mc^2 = e^2/r_e = 0.510$ MeV

deBroglie wavelength: $\bar{\lambda} = Zr_e$ (Z=1/α=137.036)

magnetic moment = $e\bar{\lambda}/2$

Thompson scattering cross-section: $8\pi r_e^2/3$

Compton scattering cross-section

quantum of magnetic flux = πZe

angular momentum = $\hbar/2$

Around 1923 Goudsmid and Uhlenbeck, then graduate students at Leyden University, discovered that many puzzles in atomic spectra could be resolved if the electron were assigned an intrinsic angular momentum (or "spin") of $\hbar/2$. When they took this proposal to Lorentz, he discouraged publishing it on the basis that, for a spherical electron of radius $r_e = 2.82 \cdot 10^{-13}$ cm to have so much angular momentum, it would require an equatorial speed of

$v / c = \hbar / 2mcr_e = \lambda\!\!\!^{-} / 2r_e$~68, i.e., nearly seventy times light speed. Fortunately, they published their result anyway. But Lorentz's calculation shows that something is terribly wrong with that model of the electron.

In my electron model a charge of e (an "eon") moves with the speed of light in a planar circular track of radius $\lambda\!\!\!^{-}$. This charge has zero rest mass, and is maintained in orbit by its own EM fields: consider the positron, with its positive eon. As it moves, it generates a solenoidal magnetic field oriented along the fingers of the right hand when the thumb is directed along the velocity. If the eon strays from its circular path it experiences a force $e\frac{\vec{v}}{c} \times \vec{B}$: this force is always directed so as to restore the circular path.

The average current J is the charge e divided by the period $\tau = 2\pi\lambda\!\!\!^{-} / c$ so that $J = ec / 2\pi\lambda\!\!\!^{-}$. This generates a magnetic moment of magnitude:

$$\mu = \frac{J}{c} \cdot \text{area} = \frac{e \cdot \pi\lambda\!\!\!^{-2}}{2\pi\lambda\!\!\!^{-}} = e\lambda\!\!\!^{-} / 2, \text{ that is, just the Bohr magneton.}$$

Viewed edge-on, the total magnetic flux within the ring is $\Phi = \pi\lambda\!\!\!^{-2}\langle B\rangle = 4\pi M$ where M is the equivalent pole strength. Then $\mu = M\iota$ where ι is the pole-to-pole length of the magnetic dipole: $\mu = \frac{\lambda\!\!\!^{-2}}{4}\langle B\rangle\iota = \frac{e\lambda\!\!\!^{-}}{2}$. $\therefore \langle B\rangle = 2e / \iota\lambda\!\!\!^{-}$. $\Phi = 2e\pi\lambda\!\!\!^{-} / \iota$. If ι is taken as $2r_e$, $\Phi = \pi e\lambda\!\!\!^{-} / r_e = \pi eZ$. This is just one quantum of magnetic flux. Also, $\langle B\rangle = e / \lambda\!\!\!^{-} r_e$. Another expression for Φ is: $\Phi = \int \vec{B}\overrightarrow{dS} = \int \overrightarrow{dS}\,\text{curl}\,\vec{A} = \oint \overrightarrow{ds}\vec{A} = 2\pi\lambda\!\!\!^{-}\langle A\rangle$, so that $\langle A\rangle = eZ / 2\lambda\!\!\!^{-} = e / 2r_e$. Thus the magnetic energy, being eA, is $e^2/2r_e$, i.e., just $mc^2/2$.

A way to get the interaction energy between a magnetic dipole and an external magnetic field $\vec{B}_{ext}$ is to take the difference,

$$E_{int} = \frac{1}{8\pi}\int\left(\vec{B}_{ext} + \vec{B}_{\mu}\right)^2 d\tau - \frac{1}{8\pi}\int\left(B_{ext}^2 + B_{\mu}^2\right) d\tau = \vec{B}_{ext}\int\frac{\vec{B}_{\mu} d\tau}{4\pi} = -\vec{B}_{ext}\vec{\mu}$$

$$\therefore \vec{\mu} = -\frac{1}{4\pi}\int\vec{B}_{\mu} d\tau = -\frac{1}{4\pi}\int\vec{B}_{\mu}\overrightarrow{dS}(2r_e) = \Phi r_e / 2\pi = \pi eZr_e / 2\pi = e\lambda\!\!\!^{-} / 2.$$

The energy of interaction of the dipole moment μ with its own magnetic field $B_{\mu} = e / \lambda\!\!\!^{-} r_e$ is:

$$\mu B_{\mu} = (e\lambda\!\!\!^{-} / 2)(e / \lambda\!\!\!^{-} r_e) = e^2 / 2r_e = mc^2 / 2.$$

This suggests that half of the electron rest energy mc^2 comes from magnetic self-interaction, the other half presumably from electric self interaction. To test this, note that:

$$E_{elec} = e\partial\Phi / \partial ct = \frac{e}{c}\int\vec{B}\overrightarrow{dS} / dt = \frac{e}{c}\frac{\vec{S}}{\tau}\int d\vec{B} = \frac{J\vec{S}}{c}\vec{B} = -\vec{\mu}\vec{B} = e^2 / 2r_e.$$

Thus it appears that the mass of the electron is purely electromagnetic, split equally between magnetic and electric contributions.

Another way to see the equality of the electric and magnetic contributions in this model is to examine the Lorentz transformation properties of $\vec{\mathcal{E}}$ and $\vec{\mathcal{B}}$ to the system of (primed) coordinates moving at $c\vec{\varphi}_1$ $(\vec{\beta} = \vec{\varphi}_1)$ with the charge. This gives
and

Since for $\upsilon = c, \gamma = \infty,$ the coefficients of γ must vanish. Thus in the lab frame,
and . Therefore:

$$\mathcal{B}_\perp^2 = \vec{\varphi}_1 \cdot \vec{\mathcal{E}}_\perp \times (\vec{\varphi}_1 \times \mathcal{E}_\perp) = \vec{\varphi}_1 \cdot \left\{ \vec{\varphi}_1 . \mathcal{E}_\perp^2 - \mathcal{E}_\perp \vec{\varphi}_1 . \vec{\mathcal{E}}_\perp \right\} = \mathcal{E}_\perp^2 .$$

Also the Poynting vector $\vec{S}$ gives:

$$\tfrac{4\pi}{c} \vec{S} = \vec{\mathcal{E}}_\perp \times \vec{\mathcal{B}}_\perp = \vec{\mathcal{E}}_\perp \times (\vec{\varphi}_1 \times \vec{\mathcal{E}}_\perp) = \varphi_1 \vec{\mathcal{E}}_\perp^2$$

or $$= -\left(\vec{\varphi}_1 \times \vec{\mathcal{B}}_\perp\right) \times \vec{\mathcal{B}}_\perp = \vec{\mathcal{B}}_\perp \times \left(\vec{\varphi}_1 \times \vec{\mathcal{B}}_\perp\right) = \vec{\varphi}_1 \vec{\mathcal{B}}_\perp^2$$ so that

$$\vec{S} = \frac{c\vec{\varphi}_1}{4\pi} \left(\vec{\mathcal{E}}_\perp^2 + \vec{\mathcal{B}}_\perp^2 \right) \Big/ 2 = \vec{c} \left(\vec{\mathcal{E}}_\perp^2 + \vec{\mathcal{B}}_\perp^2 \right) \Big/ 8\pi$$

This says that the energy flux density is simply the energy density times the velocity $\vec{c}$.

One consequence of the equipartition between electric and magnetic parts is that only half of m_e is moving; the other half is stationary along with the $\vec{\mathcal{B}}$ field. Thus, the angular momentum in terms of the effective mass m is: $mc\bar{\lambda} = (m / m_e)\hbar = \hbar / 2$. Moreover, the centrifugal force $mc^2 / \bar{\lambda}$ must be counterbalanced by the magnetic part of the Lorentz force:

This model thus appears to give a number of right answers, so why have I not published it?

In the first place, it smacks of numerology. I am reminded of the old Laurel and Hardy proof that 7×13=28. It goes like this:

13	13
× 7	13
21	13
+ 7	13
28	13
	13
	13
	28

And if you are still not convinced, add them up: 3-6-9-12-15-18-21, 22, 23, 24, 25, 26, 27, 28!

Moreover, I have not found a way to formulate this problem (or the model) in rigorous quantum-mechanical terms.

In searching for some non-linearity that might explain the spectrum of charged lepton masses, I hit upon the idea of converting the well-known classical result for the momentum in an electromagnetic field into a quantum-mechanical operator. In the 4-dimensional space-time of special relativity this takes the form:

$$m_o \underline{v} = \underline{p} - q\underline{A}/c \Rightarrow \frac{\hbar}{i}\left(\Box - \frac{iq}{\hbar c}\underline{A}\right).$$

Noting that for my model the rest mass m_o=0, squaring this operator and applying it to the 4-potential $\underline{A}$ then gives:

$$\left(\Box - \frac{iq}{\hbar c}\underline{A}\right)^2 .\underline{A} = \Box^2.\underline{A} - \frac{iq}{\hbar c}(\Box A + \underline{A}\Box).\underline{A} - \left(\frac{q}{\hbar c}\right)^2 \underline{A}^2.\underline{A} = 0.$$

This resembles its classical counterpart for light with a 4-current source $\underline{j}$, which is nonlinear:

$$\Box^2 \cdot \underline{A} - \Box \cdot \Box\underline{A} = -(4\pi/c)\underline{j}.$$

Such a nonlinear current was introduced by Landau and Ginsberg in an application to superconductivity.

In applying this to my model it seemed appropriate to use a system of coordinates that most nearly reproduced the motion of a charge on a ring. This proved to be toroidal coordinates, with axis along z, and in which the position of a point in 3-D space is specified first by μ, the logarithm of the ratio of the lengths of two vectors extending from the point to the ends of the coplanar diameter a of a directrix circle about the Z axis, second by the vertex angle η between these two vectors, and finally by the (usual) azimuthal angle φ made by the axial plane through the point with the reference direction. For given φ, assignment of μ generates as η varies the two circular cross-sections of a doughnut.

A key parameter in this coordinate system is: $p_\pm^2 = \cosh\mu \pm \cos\eta$. When the substitution is made for a potential $\psi = p\_F$ the laplacian $\nabla^2\psi$ yields in toroidal coordinates the equation: $\nabla^2\psi = \left(p_-^5/a^2\right)\left\{\frac{1}{\sinh\mu}\frac{\partial}{\partial\mu}\sinh\mu\frac{\partial F}{\partial\mu} + \frac{\partial^2 F}{\partial\eta^2} + \frac{\partial^2 F}{\sinh^2\mu\,\partial\varphi^2} + \frac{1}{4}\right\}.$

By designating z=cosh μ:

$$= (p_-^5/a^2)\left\{\frac{\partial}{\partial z}\left(z^2-1\right)\frac{\partial F}{\partial z} + \frac{\partial^2 F}{\partial\eta^2} + \frac{1}{z^2-1}\frac{\partial^2 F}{\partial\varphi^2} + \frac{1}{4}\right\}.$$

If we now separate the variables in the usual way by factoring $F = M(\mu)H(\eta)\Phi(\varphi)$ we get:

$$= \left(p_-^5 / a^2\right) F \left\{ \frac{1}{M} \frac{d}{dz} \left(z^2 - 1\right) \frac{dM}{dz} + \frac{1}{H} \frac{d^2 H}{d\eta^2} + \frac{1}{z^2 - 1} \frac{1}{\Phi} \frac{d^2}{d\varphi^2} + \frac{1}{4} \right\}.$$

And if we take $H(\eta) \sim e^{in\eta}$ and $\Phi(\varphi) \sim e^{im\varphi}$, then:

$$= \left(p_-^5 / a^2\right) F \left\{ \frac{1}{M} \frac{d}{dz} \left(z^2 - 1\right) \frac{dM}{dz} - \left(n^2 - \frac{1}{4}\right) - \frac{m^2}{z^2 - 1} \right\}.$$

Finally defining n-½ = ι:

$$= \left(p_-^5 \big/ a^2\right) F \left\{ \frac{1}{M} \frac{d}{dz} \left(z^2 - 1\right) \frac{dM}{dz} - \left[\iota(\iota + 1) + \frac{m^2}{z^2 - 1} \right] \right\}.$$

Set equal to zero, the quantity in curly brackets is just the differential equation for the associated Legendre functions, but with z = cosh μ rather than cos θ.

For the electron of spin $\hbar / 2$ the z-projection should have m=½; i.e., $\Phi \sim e^{i\varphi/2}$. But this has the difficulty that the wave function is not single valued: for example, at $\varphi = 0$ this factor is 1, but at $\varphi = 2\pi$ the factor is $e^{\pi i} = -1$; this means that after a complete revolution in φ the wavefunction would cancel itself. It was to avoid this that [Wolfgang] Pauli was driven to his spinor treatment. So this seems to be a dead end.

One final point: how can such an extended model of the electron act like a point—e.g., in the hydrogen atom? The relatively small size $(\lambda\!\!\!^- / a_H = 1 / Z)$ coupled with the enormous frequency $\left(c / \lambda\!\!\!^- \sim 10^{21}\, \sec^{-1}\right)$ make the average of the electron potential very near that of a point charge, e/r.

So in its present state, the nature of the electron (and its congeners) remains an enigma. As Fermi remarked on his attempts to measure the electron-neutron interaction, "I made up for my ambitions through lack of accomplishment!" [laughter] He was a marvelous guy. As I mentioned earlier, Nancy was secretary to him and Edward Teller in Chicago. We already knew Edward pretty well, and we also got to know Fermi and his wife.

Well, there's a lot more I could tell you about.

BOHNING: Oh, I know. It's up to you. Whatever you say is fine.

McMILLAN: I don't want to wear you out. [laughter] I did mention, there's a great interaction between the courses I've taught and some of the military stuff, but also the research. For example, I wrote a chapter on electrodynamics (32), which has got some unique stuff in it.

These are the relativistic fourvectors of all kinds. They have a surprising fourth component. The fourth component of r, of course, is ict, but then the velocity is gamma times IZ, and so on. Here's the quad operator and all that. This is the whole chapter on all this kind of stuff. Also, I get into the transformation properties of the fields themselves, under the Lorentz Transformation. That's a different formulation; it gives the same results, but it's more like a chemist's formulation than some of these fancier ones. You've seen some of these papers where the guy says, "Now, let's consider the Maxwell Equation." [laughter] That's the whole setup.

Oh, there was one other thing. I had a program that I called isomorphs. This was to be a teaching device, the idea being that there are quite a few problems in chemical physics that have the same mathematical form. All we've got to do is change the identity of the meaning of the symbols, and then take over and tack all the rest of it. I wrote several of them, but this is the first one, "The Classical Linear Harmonic Oscillator." That first gives the concept, then applies it to a simple pendulum, torsion pendulum, the vibrations of a mercury manometer, the quartz fiber microbalance, the bending of a beam, the Helmholtz resonator.

This is a nifty way of determining the gamma for a gas through this apparatus. I built one of those and still have it. I don't have it here. Then molecular vibrations of all kinds, CO_2. We can get quite a few results out of this quickly, by calculating the shift in frequencies due to isotopic substitution. This is a compendium of diatomic molecules vibrating; we can derive various simple rules, the energy relation to frequency and so on. These are the isotope effects in ammonia and phosphine, and we get pretty close just by looking at those. These are the torsional effects in ethane. This is the ethane torsional energy level. In normal mode there is little treatment of that. You've got the matrices, secular equations and all that, crystal liquids, compressibilities, the whole bit. It all hangs together.

Now, the pedagogical virtue that I had in mind was, the students learn the basics of this. Then when they go ahead and apply it to all these things, they don't have to learn so much. It's always bothered me that there are about a million pages a year coming out in chemical physics. Now suppose only .10 percent of that is worth a damn; that may be, or worse than that. I don't know, but that .10 percent is a thousand-page book. Now, there's no way that the educational system has, other than to pile new, annual thousand-page books on top of the graduate students. Well, it's got to be condensed and shoved down, there's no reward in the university for that. In fact, quite the contrary. If professors do that and they don't do research, they don't get promoted. It seems to me that it's up to a few of the elders of the society to do a certain amount of that. That is what this is about.

Well, maybe enough, huh?

BOHNING: Well, I've enjoyed it very much.

McMILLAN: Well actually, I have, too.

BOHNING: I thank you for taking the time. I think we've made a dent. Maybe this would be an appropriate point to stop. But I know I'll be back in the area from time to time and after you see the transcript, if you want to add some more or sit down for another session, we can do that as well.

McMILLAN: I think we've touched most of the high points, anyway.

BOHNING: It's been very interesting.

McMILLAN: I've told you almost nothing about what's gone on in the business, but they're all technical things.

BOHNING: I'm still amazed; you've really maintained almost three separate lives.

McMILLAN: I suppose, yes.

BOHNING: Your academic world. Your military world. You have your business world here. While they're all intermixed a little bit, you really have kept, for the most part, these three separate lives going for a long period of time.

McMILLAN: Yes. Well, it's been interesting, anyway.

BOHNING: Yes. Well, thank you again, very much.

McMILLAN: Well, I appreciate your interest and your patience.

[END OF TAPE, SIDE 11]

[END OF INTERVIEW]

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