Tony Delany: This is Tony Delany with Ed Martell and Nancy Gauss. I am going to be interviewing Ed Martell, and Nancy is going to be here keeping us on the right track. It is Tuesday, the 7th of June, 1988. We are in Ed Martell's office. I have known Ed for, I think, about twenty years. Ed is in fact, one of my academic relatives. He is my uncle. Is it? You are something like that. You are Jim Arnold's brother, academically.

Ed Martell: Well, I am a close associate of Jim Arnold at the University of Chicago. Jim was a postdoc while I was a relatively older graduate student of Libby's.

TD: You both worked for Libby and then later on, oh, fifteen years later, then I was a graduate student of Jim's. So, I know Ed personally for quite a long time. Even before that, I have known of him. What we want to talk about is mostly the history of NCAR. But what I am going to do is I am going to ask Ed a few questions first about his professional background. Or maybe we could just read it through, but I think it might be better this way. Ed is a nuclear person, atmospheric chemistry, radioactive material, nuclear science in general. You had a degree at West Point, was it Ed?

EM: Yes.

TD: Just before the war.

EM: I graduated from West Point in the class of 1942. I was an officer in the Corps of Engineers for the next eight years in combat in the Pacific. Then subsequently, when I came back from the Second World War, I had an opportunity as a lieutenant colonel to go to the University of Chicago to do advanced studies, and ultimately get a Ph.D. in nuclear chemistry and radiochemistry.

TD: So, you got a Ph.D. in 1950.

EM: 1950, yes.

TD: Yes. Then you continued in the U.S. Army in the engineers.

EM: Well, you have an obligation to serve in the Department of Defense if you get advanced training. So, for the four years after I received my degree, I worked for the Department of Defense in what they called the Armed Forces Special Weapons Project.

TD: Special weapons were nuclear weapons.

EM: Nuclear weapons. So, for four years, I was involved in the testing and the determination of the radiation effects and fallout effects of nuclear explosions. Three series of tests in Nevada and one Castle hydrogen bomb test series in the Pacific.

TD: At that time, did you have any dealings, you must have done, with the scientific community as well as the military?

EM: Oh, yes. In the determination of the effects of nuclear weapons, we had laboratory groups from universities as well as from the various federal agencies of the Army, Air Force, and Navy. At the end of this period, I was one of only several that were highly qualified on the effects of nuclear explosions. But the Army didn't have a career program to utilize a Ph.D. scientist. I was to be sent to Saudi Arabia to teach engineer tactics and techniques to –

TD: How to build bridges and things.

EM: – to Saudi Arabian non-commissioned and junior-commissioned officers. So, I said, "Goodbye." I sent in my resignation and I got into scientific work on a full-time basis.

TD: But that was at Air Force Cambridge Research Center.

EM: That was in [19]54. No. In 1954, when I resigned, Libby, who I'd gotten my Ph.D. under was the new scientific member of the Atomic Energy Commission. When he found I was available, he pressed me to come to Chicago for a two-year period. Well, to carry on in a global strontium-90 fallout study that had been initiated within less than a year before that. He had three graduate students that had needed some help that were just finishing up. He had a postdoc coming from Germany to do tritium research.

TD: Who was that, Dieter?

EM: (Fred Baughman?).

TD: Oh, Fred Baughman.

EM: So, I went to the university for two years and got back into experimental work and became involved in the Atomic Energy Commission study on global radioactive fallout at remote distances from the test sites. So, at the end of this period, I came to the conclusion that one of the important neglected areas of global fallout studies was that of the atmospheric aspects, the transport and redistribution of bomb debris in the upper and lower atmosphere, and residence times of radioactive aerosols.

TD: This is when you met Chris Junge, the brothers.

EM: This is when I decided to go to the Cambridge Research Center. For six years, I had a group studying the atmospheric aspects of fallout. In my first year there, I became closely acquainted with Chris Junge, who was doing outstanding work in natural aerosols in the atmosphere. So, we teamed up for a five-year very productive association, which was especially great benefit to me. Now, at the end of that period, the Air Force Cambridge Research Center was becoming more and more a laboratory focused on Air Force interests and requirements, and not on good basic atmospheric science.

TD: Chris was invited here to…

EM: Yes. Chris Junge was invited to come to the newly forming NCAR. He was invited in – it might have been late [19]60s. Yes, it was in the [19]60s. In 1960, he was invited and pressed to come here as the first scientific director of this atmospheric research division.

TD: The whole scientific research division?

EM: Yes, for scientific use.

TD: Which would be like MIS or something?

EM: Yes. Like the Laboratory of Atmospheric Science. But Junge was a gentleman of the old European School.

TD: He still is. [laughter]

EM: [laughter] He decided that he couldn't take his family out here in the primitive Boulder area and elected to take a position as a director of a research institute at the University of Mines.

TD: Oh, that is the Otto Hahn [inaudible] radiation chemistry?

EM: No, this is the University of Mines.

TD: They had a (Professor Shelly?). Yes.

EM: The Max Planck Institute in Mines is a separate research facility, which Junge later became a division director of. But at the time Junge was here for several visits and being pressed to come to NCAR, Junge was enough impressed with this place. He strongly recommended to me that this is an alternative because I was planning to leave the Air Force Cambridge Research Lab and looking for an alternative. Meanwhile, he gave a strong recommendation for my appointment to Walt Roberts and Phil Thompson. So, I think it was in April of 1961, I came for a visit and gave a seminar on atmospheric radar of all things. [laughter]

Nancy Gauss: [laughter]

EM: I was invited to take an appointment as a program scientist in atmospheric radioactivity and geochemistry.

TD: How many programs were there?

EM: I don't know.

TD: They are ten or something like that?

EM: Ultimately, by the 1962 or three, there were somewhere between eight and ten programs.

TD: I see, yes.

EM: One in ozone, one in cloud physics, one in atmospheric chemistry with Jim Lodge, one in aerosol science. I can't remember them all, but about eight or ten programs. When I was offered this appointment in April of [19]61, I asked, "When are you going to have experimental laboratory facilities?" They said, "Well, not before the summer of [19]62." I said, "Well, I have an experimental laboratory where I am now at the Air Force Cambridge Research Lab. So, I accept and will join NCAR in the summer of 1962." That was suitable to both of us.

TD: Yes.

NG: So, NCAR just had offices then prior to that time?

EM: At that time, NCAR had a nucleus staff in the in the old Armory building, just north of Macky auditorium. It was only in the spring of 1962 that PSRB1, well, a temporary building on the East campus was built.

NG: That is the current RL1.

EM: Yes. PSRB1 and 2 were the two buildings occupied by NCAR in 1962 and 1963. I think it was something like four years later in [19]66 that the Mesa building was completed and we moved up here.

TD: So, there was not too much experimental research going on at the beginning?

EM: Well, certainly, there was no experimental research here until the fall of [19]62. When I came in the summer of [19]62, Arnold Bainbridge joined me very shortly, and my group and Pat Squire's group and several other groups, Jan Rosinkski's group, (Hans Duche's?) group, all were beginning to do some experimental work, but it took us most of the fall to get our laboratory facilities installed. So, we had bare laboratories, unfurnished, and marginally equipped in the summer of [19]62. I would say it was the six months to a year later before we had experimental programs.

TD: So, who were the people who were working with you then?

EM: At that time, Arnold Bainbridge who was an…

TD: An engineer, was he not?

EM: Arnold Bainbridge had worked with Hans Seuss, and before that he had worked with Athol Rafter in Australia.

TD: Oh, I saw Hans two weeks ago

EM: Excuse me, in New Zealand. Athol Rafter in New Zealand. So, Arnold had considerable experience in carbon-14 measurements with Rafter in New Zealand, and then later in tritium and carbon-14 measurements work he did with Hans Seuss at University of California.

TD: I saw Hans Seuss three weeks ago. I gave a seminar there, and he was there. He is still doing all right.

EM: Hans is surprisingly active for his age. See, I think he's seven or eight years my senior.

TD: Oh, is he? He is that old? I thought he was older than that. I do not know.

EM: I'm seventy and he's about seventy-seven or seventy-eight, if I recall correctly. In any case, Arnold Bainbridge and I started a limited experimental group. I was working on some fallout studies using fallout radioisotopes that emitted beta radiation. So, I had low-level beta systems and Bainbridge set up carbon-14 and tritium counting systems. Then subsequently, about a year and a half, two years later, we hired Julian Shedlovsky to join us, because Julian had knowledge of x-ray and gamma ray spectroscopy techniques that complemented those that we already had. Our goal was to develop a group that was competent in quantitative, sensitive measurements of all types of radiation that you find in natural environments and in radioactive fallout products as well.

TD: Oh, in fallout, yes.

EM: You see, the object of the program I had at that time was to use radioisotopes of natural and anthropogenic origin as tracers to study features in the atmospheric circulation, to study residence times of aerosols in various levels of the upper and lower atmosphere. In these experiments, we use both accidental tracers provided by bomb debris. We use natural radioactivity, such as cosmic rays that produce radioisotopes in the upper atmosphere, or radon as decay products emanating from soil surfaces. We also had an opportunity to use unique tracers that gave us exceptional opportunities to follow debris from known point sources in the atmosphere.

TD: You mean specific bomb?

EM: Yes.

TD: They spiked quite a few of those. I remember tungsten spiked one back in [19]50s.

EM: Well, the first spiked experiment was when I was at the Cambridge Research Center. The Atomic Energy Commission had plans to conduct two high-altitude, rocket-borne thermal nuclear explosions over Johnson Island.

TD: [laughter] Yes.

EM: These shots were code named teak and orange. They were both about one megaton in yield. So, I wrote a letter to the Air Force and also to the Atomic Energy Commission with a copy to Commissioner Libby, pointing out that if we could put a unique radioactive tag in one or both of these thermal nuclear explosions, then we could follow the upper atmosphere redistribution and residence time and fate of debris injected into the upper stratosphere. One of these shots was to inject bomb debris from the thermal explosion in the upper stratosphere. The second one would reach the lower mesosphere. Well, one of my colleagues and I recommended two specific tracers and Los Alamos assigned two diagnostic radiochemists to review this proposal. They pointed out that indium-115, which we had recommended, could be made by multiple nuclear reactions. Therefore, it was not a good tracer. They would not be able to get a quantitative yield. But rhodium-102, which we recommended, could be produced by a single unique reaction, and therefore they could get a quantitative yield estimate, and it would provide a good tracer. So, rhodium-102 was used as a tracer in one of these two high-altitude explosions. We did have a little static. We had one or two AEC scientists who said, "Putting this experiment in one device and not the other would interfere with yield and determinations and some of the diagnostics." But it turned out that these arguments were rather thin and specious. So, the first unique tracer experiment was the tagging of orange shot over Johnson Island.

TD: What date was that?

EM: I'd have to look it up.

TD: But it was when you were here at NCAR?

EM: No. That shot was conducted in about – I think it was 1959. We followed this, well, with Air Force high-altitude aircraft sampling systems, and also with balloon sampling systems. There was no detection of rhodium-102 for one and a half years after the explosion. The first observations were made in the Arctic lower stratosphere in the late winter and early spring, two years after the shot was fired. It turns out the debris in the upper stratosphere, it mixes into both hemispheres, but it mixes down over the pole in the polar vortex. It took two winters for effective transport from the upper stratosphere, a site near the stratopause, to reach the lower stratosphere and be transported equatorwards and downward into the troposphere. Estimates made on the basis of the first three years of measurements indicated that residence times for bomb debris injected in the upper stratosphere were of the order of ten years. It was because of this long residence time that we came to the tentative conclusion that it would be extremely interesting to study the source and the source distribution in the upper stratosphere near the – so, while I was still at the Air Force Cambridge Research Center, we got extra funding so that we could contract for developing a cryogenic rocket sampler to collect air and particulates in the upper stratosphere. The point was, if the residence time was so long, the particles were so finely divided that almost the only feasible way to collect this finely divided bomb debris was to collect the whole air sample. So, we chose a cryogenic sampler that would collect airing constituent of the atmosphere regardless of size.

TD: So, you would just take a core.

EM: Yes. The conception for this project was very good, but the contractor that tried to meet the specifications and requirements was under a rather difficult deadline. Had a year to produce and test. The rocket-borne cryogenic air sampler, well, they produced and fired two in attempt to sample the rhodium source in the upper stratosphere. Well, we found that the sampler capability was totally inadequate. The cryogen was lost before it reached sampling altitude. It didn't meet specifications. So, when I first came to NCAR, I said, "Look, knowledge of the composition of the upper stratosphere well above balloon ceiling is important." The cryogenic sampler properly developed and tested at ground level is capable of giving us larger samples, samples of the order of one to ten moles of air collected over a known altitude interval in the upper stratosphere and even lower mesosphere. So, when I came to NCAR, I came with NCAR's blessing to obtain NASA's support to develop and test and apply rocket-borne cryogenic air.

TD: Now, this was for inert materials, non-reactive materials.

EM: It didn't matter whether it's radioactive or inactive.

TD: No, no, I do not mean radio. I mean chemically reactive. I mean, you could not.

EM: This, oh, of course. If you have dissociated –

TD: Might be a free radical or something.

EM: – constituent, you'll probably get some recombination of free radicals on surfaces and reactions in the system. But this would not affect the application of the sampler for measuring stable –

TD: Yes. Chemically stable.

EM: – chemically stable trace gases, and the noble gases and the carbon and hydrogen compounds and their isotopic composition. It opened up some very important possibilities.

TD: Total halogen and things like that. It never really worked though, did it?

EM: Oh, the cryogenic samplers worked perfectly.

TD: Did it?

EM: Yes, it did. When I came to NCAR, I formed a design development team for the cryogenic sampler made up of two people from the National Bureau of Standards' cryogenics lab. One of these was (Jess Horde?). Another was Dr. Robert Jacobs who was a member of the staff there when I recruited his services and interest, but who became a private consultant when the work was in progress. Well, in the first attempt to design the cryogenic sampler, we found that there was no adequate basic data for condensation of very low-pressure air on a liquid hydrogen cool surface, or a liquid neon cool surface. So, in order to be on firm ground in the design of the heat exchanger, we supported experimental work by Jess Horde at National Bureau Standards for over a year in the cryogenics lab. Subsequently, when we had this data, we developed a system that would be capable of sampling at supersonic velocities, and it involved special aerodynamics for a split nose cone that could be deployed when necessary. When the split nose cone was deployed, we had an open orifice about five and a half inches in diameter, which received air at supersonic velocities and carried this into a heat exchanger with thirty-two layers of stainless-steel coils, which were held at liquid hydrogen temperature in the first experiment. In the first experiment, the system was a little bit dangerous and less than ideal because liquid hydrogen is very difficult and dangerous to work with.

TD: [laughter]

EM: The first experiment was a complete success, although we had a quite heavy payload. We were fortunate in the atmospheric conditions and in the fact that we deployed and consumed all of the cryogen. So, we obtained a good sample, and we had some preliminary results for the composition of the upper stratosphere.

TD: The idea there was that you were looking for some fractionation?

EM: Well, no. We were looking for the concentration and isotopic composition of water vapor, molecular hydrogen, methane, and several other trace gases. You're right, we also were looking for the noble gases to determine whether there was any change in the isotopic composition or relative concentration of the…

TD: Yes. Krypton, argon ratios. Things like that.

EM: Yes, neon, krypton, argon. When this first experiment was conducted, we hadn't developed a system that was free of degassing heat exchanger surface materials. We hadn't developed the analytical techniques to measure some of the trace gases. So, with the success of that first experiment and preliminary results, we found a few important things. For example, we found that water vapor increased with altitude due to the combustion of methane and hydrogen as you go from the lower stratosphere to the upper stratosphere. So, new insights on the atmospheric hydrogen cycle were the first important finding. We then went back to the drawing board and redesigned the system to employ liquid neon. We also developed a heat exchanger that could be degassed at five-hundred degrees centigrade, and then subjected to cryogenic temperatures, the temperature of liquid neon. This involved some rather tricky difficult design problems, which were solved at NCAR by contractors we went to.

TD: So, that was really the first research program that you were working on.

EM: Yes. It was a very challenging research program, and it helped us. During the course of developing this, we had a better capability in the NCAR machine shop and design room for working with thin wall stainless steel and producing electropolished coils in the development of this heat exchanger.

TD: Is that when Russ White came to NCAR?

EM: No, Russ White was one of the people involved but Marv Hewitt in the machine shop did some outstanding work. We had several other people in the design group that did some very important work. Rich Lueb, who was the chief technician in our own laboratory later was honored for his work in the laboratory field.

TD: Who was in that group then? When did Leroy and those people come and Dieter and all the rest of that?

EM: Leroy, let's see. In [19]62, when Arnold Bainbridge and I got started, Rich Lube was the first of our laboratory technicians to come. At the time, he was a graduate student at CU, so he worked halftime while he finished up his work for his degree. So, Rich became a full-time technician in our laboratory about a year and a half later. Then I needed an assisting radiochemist. Let's see. I'm trying to remember the details. We hired the Leroy Heidt and Walt Pollock as laboratory assistants because they were chemists that had some experience with radioactivity measurements.

TD: It was with Rocky Flats.

EM: Yes. They worked at Rocky Flats. It was amusing, we advertised for BS or MS Chemist with some experimental research experience, preferably with some knowledge of radioactivity measurements.

TD: [laughter]

EM: Put these announcements on the chemistry and physics department bulletin boards at CU. About a week later, we received applications from almost every member of the radiochemistry group from Rocky Flats. [laughter]

NG: [laughter]

EM: We hired one then and another within six months, or a year later. But Dieter Ehhalt was a young German scientist who was interested in working with our group. Arnold Bainbridge and I arranged to have him come for a one-year appointment.

TD: Oops. That is my fault.

EM: I think his first visit was in [19]64.

TD: Had he worked with Junge?

EM: No, never had.

TD: No.

EM: No. I met Ehhalt when Junge and I ran the – see, Junge was secretary of the International Commission on Atmospheric Chemistry and Radioactivity. I took part in the [19]63 symposium in Utrecht, and I met Ehhalt there. Bainbridge and I were both there and discussed the possibility of his coming. So, he was delighted. He came to NCAR for a year, and I think it was beginning the summer of [19]64. Then later we got a long-term appointment for him. I forget whether it was a three- or five-year appointment, which was subsequently renewed. But he took an extended leave of absence later to go back to Germany for two years to earn his advanced teaching degree. So, Ehhalt was here for quite a few years, interrupted only by one year following his initial appointment.

TD: That is the time when NCAR was growing. There were quite a few people who came to NCAR then like Blifford and Julian Shedlovsky and quite a few others.

EM: Yes. Well, as I say, Julian Shedlovsky and Dieter Ehhalt. Julian Shedlovsky came on a regular appointment in my group about [19]64, I think. The same year that Ehhalt came on a one-year appointment. Then a year or two later, NCAR hired Irv Blifford. Now, Irv Blifford was an old friend and acquaintance of mine, and I knew him to be a very capable experimental scientist. At that time, they had a program group in the atmospheric radiation studies under (Dr. Davey?). They were badly in need of a good experimental physicist to help them with several instrumentation problems. So, at my recommendation, Blifford was brought into Davey's group to work with them developing their instrumentation for two years. Then subsequently he left that group to join mine to do atmospheric aerosol research. In our group, Blifford looked at the extensive important work that Junge had done on atmospheric aerosols. Most of Junge's work was on aerosols in surface air in the lower troposphere. Then at the Cambridge Research Center, in association with my group, he did work on the stratospheric aerosols and discovered the so-called stratospheric sulfate aerosol layer, for which he became well known. There's still some controversy about the mechanisms of formation and dissipation of that. But it was studied extensively by Junge at the Cambridge Research Center. But then Blifford said, "Well, there's a gap. Very important information is needed on aerosols in the troposphere from the lower troposphere to the stratosphere." So, using NCAR aircraft, Blifford inaugurated an extensive program on the size distribution, number distribution, and the properties of tropospheric aerosols.

TD: He worked with Julian on that as well?

EM: No, he was with my group.

TD: Oh, with you. Julian was there then?

EM: No. Blifford was independent of Julian initially, doing aerosol research. Julian came in to do neutron activation analysis and x-ray and gamma ray spectroscopy of neutron-activated products. That was a separate project of his. He was concerned with use of neutron activation in the quantitative study of atmospheric chemical constituents and aerosol constituents.

TD: Elemental, yes.

EM: Yes. So, Julian's work, quite independent of Blifford's work. Julian under my guidance proceeded to develop an ultra-clean filter material with negligible inorganic constituents. This was –

TD: Polystyrene stuff.

EM: – polystyrene microfiber filter, which we borrowed from commercial laboratory production of polystyrene filters. But then set up a clean room operation and had extremely high purity materials to start with. Did indeed produce a filter with no inorganic background to speak of, was so negligible that it could be massed for most elements by fairly short periods of aircraft sampling. We also established, in order to support this work, we went to command nuclear in

Colorado Springs.

TD: Colorado Springs.

EM: Obtained a neutron activation system that went into a small satellite laboratory under the spiral stairwell of NCAR, just opposite the NCAR main entrance. But as it turned out, the sensitivity of neutron activation with the fluxes we could obtain with this small system, fell short by two to three orders of magnitude in neutron flux compared to the fluxes we could get either at Argonne National Laboratory or Los Alamos.

TD: Oh, just the trigger down here?

EM: Yes.

TD: The USGS.

EM: Well, no.

TD: Oh, I see. Yes.

EM: But at the time so that we made only limited use of the neutron activation system here.

NG: Is that the stratosphere exemplar you are talking about?

EM: No. A neutron activation system involved a –

TD: It is a Cockcroft-Walton linear accelerator that generates neutrons. It is thermal. Well, not very well thermalized, but it does generate neutrons. It lived in that room that is under the access.

NG: Right.

EM: The laboratory under the stairwell if you are right opposite the main entrance of NCAR.

TD: When you come up from the parking lot, you walk across the roof of it. There is a lab under there.

EM: So, it was used for a neutron activation room.

NG: I see.

EM: Then later on, we used it for a drosophila laboratory, and we carried out those limited study of it.

TD: So, then pushing on a little bit, when did that group split up? Because I remember when I came here in 1970, it was quite considerably different. So, somewhere between [19]66 or

[196]7, then it split up.

EM: Well, initially we had an organization evolving.

TD: Maybe you could just say something a little bit about the organization in the late [19]60s, and how your groups fitted in.

EM: Well, refresh me. When was the infamous Joint Evaluation Committee reborn?

TD: [19]73.

NG: [19]72 and [19]73.

TD: [19]72, [19]73.

EM: Was that [19]72 and [19]73?

TD: Yes.

EM: Well, in the late [19]60s and early [19]70s, a major field program called FAPS, Fate of Air Pollution Study, a St. Louis study, was organized. It was organized primarily by Jim Lodge and Dick Cadle. Or at least it had Dick Cadle's stamp of approval. They encouraged various experimental groups in our division to participate and take part. I guess I was one of the earliest critics of that program. Then I suggested, "You don't design an expensive and elaborate field program by going out and measuring everything you know how to measure. You focus more carefully on a few important experimental objectives, and then design the measurement program through the essential measurements and the essential grid of measurements." Now, the fact that I was criticizing their approach was simply taking a shopping list and going out and measuring everything under the sun. Somehow some results were supposed to accidentally fall out of this. Good science is not done that way. Because I dared to criticize the department and division head, I was simply pushed to one side and said, "Well, Ed Martell, he's difficult and uncooperative." So, they punished me for that. Julian Shedlovsky was cooperative. At least, I wasn't aware of it, but they were aware that he was very cooperative.

TD: Good fellow, Julian. Yes.

EM: So, they split my group into two programs. This was really a slap in the wrist to me. They set up a separate aerosol research program under Shedlovsky and put Blifford under him.

NG: Did this happen as a result of the JEC report or did this happen prior?

TD: No, this is before.

EM: No, this happened before. This happened before. Well, I suggest that if FAPS were the only cause of JEC report, your suggestion is correct. That if they listened to my criticism, taken them seriously, the FAPS study could have been streamlined and properly designed. But I think the JEC report was due to not just one, but a number of marginal programs. NCAR was going in too many directions with efforts of various sizes and qualities. But that was certainly one of the major problems, and one of the programs that was most sharply criticized. This criticism came back in a not too surprising way. About two years after I criticized this program in orally, and then in writing, and I say I was punished variously by having my program split. They did me a favor because I had a smaller group with a better focus. But what happened thereafter is FAPS developed into a fairly substantial program. I forget what it was. They submitted a program to NSF with a price tag of $2 million more.

TD: It was quite large.

EM: For a two-year program. The atmospheric science division at NSF said, "Well, we can't simply endorse. This is an expensive program. We can't endorse such a program without some outside review." So, they sent the FAPS proposal to at least four atmospheric scientists in universities. In the course of the next several weeks, I received phone calls from two of the reviewers, and they asked me questions about this program. I said, "Now, wait a minute. I haven't reviewed this program." "What? You're a member of the NCAR staff?" I said, "Well, I criticized this program in its early stages, so they weren't interested in criticism. They called me uncooperative and left me out of it. I have not seen the proposal in its draft stage or in its final stage and my comments were not invited."

NG: Was there any mechanism at that time to review proposals in any way besides that?

EM: This proposal, unfortunately, was reviewed only in the little family of Jim Lodge, Dick Cadle.

TD: Will Kellogg.

EM: I'm not sure to what extent Will Kellogg was. Will Kellogg put a rubber stamp on it, and Will Kellogg is the man who ignored my critical comments on it. But, you see, the project was not reviewed, the program was not reviewed by Dieter Ehhalt or me or Blifford or several others who would have some background and experience that could – in fact, I think several people with much more experience in field work were left out. So, the university reviewers were just shocked that the internal review was so superficial. So, FAPS was not recommended to what extent the JEC report was an outcome of the embarrassment of this project, and several other projects, and the fact that we were going and the – I don't know. I simply think that FAPS was an unfortunate program that could have been avoided if we had adequate and thorough internal review of programs at that time.

TD: Well, FAPS being a project certainly when the JEC report came in, I am not too sure – I read the JEC report several times. I am not too sure that it, in fact, was specifically especially critical of FAPS. It was critical of the management style within NCAR, which would allow such a thing.

EM: No.

TD: But then what happened is the NCAR management then turned around and said, "It is all the atmospheric chemist's fault."

EM: [laughter]

TD: We all got hit on the head.

EM: Well, and of course, there were some other embarrassments before that.

TD: Oh, yes.

EM: There was a famous or infamous (Picardi?) effect study.

TD: That was the thing that Walt Roberts wanted Dick Cadle to do, was it not?

EM: Well, Walter was always interested in solar-terrestrial interactions, or extraterrestrial-terrestrial interactions. So, at one time when he read an article about the Picardi effect which implied that extraterrestrial interactions were involved in some type of radiation interaction, which influenced physical and chemical constants in the atmosphere and on the earth.

TD: It was not specifically the atmosphere.

EM: No, it wasn't specifically. It influenced physical constants.

TD: I still do not understand how it was supposed to have done that.

EM: Well, nobody understands it. [laughter] Now, it turned out that the Picardi effect book is a very highly speculative book. Picardi's experimental work was marginal science and not good quantitative experimental science. Anyway, the Picardi experiments were repeated in Jim Lodge's group under Dr. William Fischer and two full-time assistants. This experimental work was carrying on for about five years. They were simply repeating non-quantitative experimental work so that the results were of such a nature that you couldn't assess the magnitude of the error from real variations. It was unpublishable experimental data. But this project survived at NCAR due to inadequate internal review for about five years before it was canceled.

TD: Yes. That probably was the bad thing, was the structure of NCAR. It is like a small company that had grown without getting its act together properly.

EM: Well, I think it was a combination of the Picardi effect experiment, badly conceived, and FAPS, which led to the demise of Jim Lodge's group at the end of – Jim Lodge had the largest experimental group in NCAR, but he was directly involved in two of the most unfortunate programs.

TD: But when the JEC report came out, all sorts of other groups got into trouble.

EM: Yes. The main objective, I think the JEC report was pointing out that NCAR's director and division directors were too outwardly focused, and were not. There wasn't enough critical review of NCAR's research programs and activities internally. But as a result of this critical report, while NCAR was required to respond to it, NSF reacted by cutting the NCAR's budget.

TD: Yes, that is right.

EM: So, you had a heavy budget cut added to this criticism. The only response in the face of the budget cut was to trim the staff.

TD: Was lay off people. Yes.

EM: Now, it's easier to save money by firing a few experimental scientists, Ph.Ds. because you eliminate the cost of laboratory assistants and laboratories and materials and so forth. If you instead eliminated a Ph.D. position in meteorology or in modeling, you lose one scientist. So, the brunt of the staff cuts to meet this NSF budget cut came by cutting out large chunks. I think nearly half of the atmospheric chemistry capability was wiped out.

TD: No, probably more than that. Yes.

EM: What happened?

TD: I think that chemistry, I always think of it in terms of FAPS, but it drew the wrath upon itself. [laughter] There were others of us who were not really associated with that, but also felt it. For instance, well, like, the people who remained were mostly associated with aerosols. But even we suffered a fair amount. Then there was the reorganization. Do you want to talk about that?

EM: Well, I'm a little rusty.

TD: We went round and round and round with the big eight. Remember that?

EM: Well, we went through so many phases of reorganization.

TD: We reorganized for about two years, did we not?

EM: Yes. For about two years. I think the experimentalists that remained were mainly those concerned with atmospheric aerosols, radioactive aerosols, my group, Blifford and a few others. Shedlovsky had gone. Jim Lodge and many of his group had gone. Eric Allen, who's a photochemist, was gone because his work was high quality, but he was eliminated because he was one of these small efforts. We were going in too many directions. So, several, quite capable scientists departed, not because they were not competent, but because they were not member of a strong group.

TD: Oh, Dieter Ehhalt, he disappeared about that time as well. I am not quite certain how it worked out, but he did.

EM: I can't remember.

TD: Well, I remember there was a time right after we had all reorganized. Blifford called all the Ph.D. scientists and said that now they had this new position, and that the only person in the whole of the chemistry division who had a senior scientist position was Ed Danielson. Everybody more or less figured out that was because Ed Danielson was a dynamic meteorologist, and neither Ed Martell or Dieter Ehhalt or Tony Delany or anybody, except Ed Danielson. I seem to remember that was a kick and a punch.

EM: Yes. Well, in the first part of the reorganization, because the strength was in aerosols, they tried to give a stronger focus to aerosols and let a few of the others go. So, this project was first put under Eric Allen. Then Eric Allen was cut belatedly to meet the budget cuts. So, he served for a short period as the chemistry department head. Then Blifford took over as the aerosol project head. Then because of a lack of support by both the people, well, above and below, the directors committee didn't give Blifford the kind of support he wanted. Blifford didn't have the, well, the personality to get the best out of the people under him. He had a very difficult personality. So, the aerosol project under Blifford was in an unfortunate state, and they decided to encourage Blifford to depart and let Ed Danielson take over.

TD: [laughter]

EM: So, now he had a situation. He had an aerosol capability and an experimental capability in aerosol science and a limited amount of other capability. He put a meteorologist in charge. Ed Danielson is a very capable meteorologist, but he also had a fairly narrow focus. He had some pet hypotheses about the role of the aerosols and dust storms in convective storm systems.

TD: About the role of aerosols in thunderstorm.

EM: Yes. So, he gave the work of the group too narrow a focus for the next two or three years and was sharply criticized by cloud physicists outside of NCAR. I don't know. I don't remember the details, but Ed Danielson left again because of inadequate support within and outside of NCAR in the atmospheric science community.

TD: There is a period from about [19]73 to about [19]76 when everything just kept changing. There were big field projects run like dust storm, and there were all sorts of other things. Some people left and some people came, but there was no coherent effort that made sense. Well, I was thinking it was going to get better, and it would get better, and then it would get worse again. It would get better and get worse again.

EM: It was during this period, also, that I changed my whole direction of research. I was called in. At the time John Firor was still the director. This is before. This was, I think, it was late 1973. John Firor called me in and said, "Ed." He said, "You've been spending a lot of time working on the problem of plutonium releases from Rocky Flats, off-site plutonium and its hazards and tobacco radioactivity." He said, "I would advise you, for your own good, to drop all of these side issues, important as they may be, and get back into the mainstream of atmospheric tracer studies." I said, "John, if you had said this to me six months ago or a year ago, I would've had to agree with you. But we have some very exciting experimental results, and I think they could be of very great importance. If I have to go somewhere else to do that work, I'll go, but I'd lose a year or two in the process." So, I told him I'd appreciate it if he discusses it in some depth with me, and then decide. So, I pointed out that we had found that tobacco leaves have a high concentration of hairs on both surfaces. The tips of these hairs had accumulations of very high concentrations of atmospheric aerosols and radon decay products. That the specific radioactivity of these trichome tips was four orders of magnitude higher than the specific activity of the bulk tobacco. That when cigarette fibers are burned, these tobacco trichomes undergo incomplete combustion to produce radioactive aerosols that are in the mainstream cigarette smoke. So, the number one candidate for lung cancer and smokers, on the basis of this work, appeared to be radioactive aerosols; alpha and beta emitters, lead-210, and its two decay products, bismuth-210 and polonium-210. These particles were extremely insoluble and could accumulate in damaged bronchial tissue. So, when you have the most common lethal cancer in man, and now in women, is lung cancer in smokers. If you can identify the agents and mechanisms of human cancers and the most common human cancers, it's an important side issue. So, John agreed and said, "If NCAR can't support a few scientists to work on important side issues, who can?"

TD: Well, this is interesting because that was the time when Francis Bretherton took over?

EM: Yes.

TD: Yes. One of the things when Francis took over, Francis asked me – and we talked about your work and importance, and I presume that he talked to many other people. But right about that time – which was also when Paul Crutzen took over the chemistry department, from that time on you have been effectively allowed to work on your own completely.

EM: Yes.

TD: Who made that decision? Was that Francis Bretherton or was it before then at Firor's time?

EM: Well, it was Firor who first gave me his blessing. Also, I think John supported me strongly with Francis that this was important work and my atmospheric science contributions in the past were of some significance. Therefore, I deserved the opportunity to continue this work.

NG: Was this unusual at NCAR at that time?

EM: It's unusual because I think at that time I was the only experimental scientist who was working completely independent of any of the NCAR program objectives. But Francis also gave me limited support because at that time, I was also appointed president of the International Commission on Atmospheric Chemistry. I had been secretary of that international commission for eight years. In those eight years, we had major symposia on four occasions and other smaller symposium in atmospheric chemistry and radioactivity. Some of the important work in radioactive tracer studies in evaluating atmospheric circulation and mixing features and residence times of aerosols and so forth were brought together in the symposium proceedings. Several symposia proceedings were published either in *JGR* special volumes, special issues, or regular issues, or in *Telos*. It's an important part of the literature of atmospheric chemistry and radioactivity. But I think because I was elected president of the International Commission of Atmospheric Chemistry for the next four years, that this helped me to continue my independent work with a smaller group at NCAR. I also got some outside support. Paul Crutzen felt that if I wanted to pursue work that was not on atmospheric trace gases, I should get most of my support elsewhere.

TD: Because that was about the time when effectively, all the aerosol work ceased. Before then the first time all the trace gas work ceased. Then three years later, or four years later, it did a flip flop, and then it was the other way around.

EM: Yes. If you want to destroy the experimental capability of an organization, what you do is give it a sharp focus in one direction, and then several years later give it a sharp focus in a completely different one.

TD: [laughter] Yes.

EM: We went from a focus on aerosol research to a focus on trace gas research to the point where it's amazing that we have any experimental capability left. I guess we have some rather versatile [laughter] atmospheric chemists.

NG: Do you think that an organization cannot focus in both areas at the same time? Or is that too much to ask?

EM: Well, it takes people of different experimental capability, different…

TD: But there is more to it than that. There is also the fact that the funding and all that stuff is now much more highly, or has been for the past ten years, it has been more highly for homogeneous gas phase, atmospheric chemistry. Now, maybe that is changing again now, but that is the way it has been.

EM: Right. I think that Ralph Cicerone, our recent division director, appreciates that NCAR shouldn't have a narrow focus in trace gases. We should have a broader focus. But partly because of the era, we've lost much of the good capability we had in atmospheric aerosol studies. Furthermore, we do not have an adequate capability. With my retirement, we have no one taking the initiative to do radioactive tracer studies. Furthermore, the capability we have for stable isotope studies dissipated with the loss of, first, Howard Moore and then Dieter Ehhalt. These were the only two people who had good capabilities to study and to do good experimental work in stable isotopes in atmospheric trace gas studies. There's a little bit of work going on now, but not the same. I don't believe it is anything like that, that could have been carried on if we had a Howard Moore or Dieter Ehhalt or preferably both of them.

TD: Let me see if there were some. I think we were just running through quite nicely. In fact, that is more or less what I have got, is right through to recent times, which I have got recent times as being from 1980 on.

EM: [laughter]

TD: That is more or less when you have been working on your own. Let us see. When did Stewart Poet leave and your lab finished? Well, you have not talked about your fruit flies genetic experiment.

EM: No. Well, when I began to study, I was concerned with the off-site plutonium east of Rocky Flats. I discovered after some reading that the Atomic Energy Establishment and the radiology community have a grossly inadequate approach to the assessment of the cancer risk of alpha emitters whether natural or anthropogenic origin.

NG: What timeframe is this now?

EM: Excuse me?

NG: When were you working on this project?

EM: My first paper on the off-site contamination of Rocky Flats was published in *Health Physics* in 1982.

TD: Must have noted it down.

EM: No, 1972. [laughter]

TD: [19]72.

EM: Excuse me.

TD: Because you did that [inaudible] memories seems to be.

EM: Yes. Of course, I felt that if alpha emitters or other natural or anthropogenic radioisotopes are important in human cancer, we better find the answers before we contaminate the environment irrevocably with plutonium from nuclear reactors and from fallout. Plutonium has a 24,000-year half-life. It's here forever. In 24,000 years, you'll have half as much plutonium as you have now. [laughter] It means a thousand generations that's been in inappreciable decrease, even if you had no further production. So, after some reading and thinking, I came to the conclusion late [19]73, early [19]74, that lung cancer in smokers, the most common lethal cancer in men, could be due to alpha activity in the cigarette smoke. This suggestion had been made seriously by a Nobel Prize winner from New Zealand.

TD: (Dahl?).

EM: No. Who?

TD: Dahl?

EM: No. What's his name? It's Ernest Sir or Ernest Marsden. Marsden was a young colleague of Rutherford and studied scattering alpha particle interactions and with materials and with tissue. In the last years of his life, he wrote several speculative papers suggesting that internal alpha emitters may play an important role in cell transformation and therefore in cell mutation in cancer. But he didn't focus on any particular radionuclides. Then later in the mid-[19]60s, we had a Harvard School of Public Health group that published three or four papers pointing out that polonium-210 was present in cigarette smoke. It's an alpha emitter, polonium-210, a natural alpha emitter. It was also found at the tumor sites, at the bronchial bifurcations in smokers who died of lung cancer. So, this gave some focus to my work. The work at Harvard fell into disrepute because they had no mechanism which could explain the presence of polonium-210 in bronchial tissue. Because polonium-210 in cigarette smoke is a volatile radioisotope. It's molecularly dispersed, and therefore it's cleared. Animal experiments showed that inhaled polonium-210 is cleared from the lung. So, I came along in [19]74 and pointed out that there is an explanation for the high polonium-210 observed experimentally by the Harvard group. It's simply that when you burn trichomes at the high temperature of cigarettes, you produce insoluble lead-210 rich particles. Lead-210 is a precursor of bismuth-210 and polonium-210. So, the lead-210 particles persist in damaged tissue at the bronchial tumor sites, and the polonium-210, granddaughter, grows in.

TD: It grows in.

EM: So, here, we had a very specific explanation. At that time, in the mid-[19]70s and late [19]70s, it was considered by a number of scientists in this country and elsewhere that polonium-210 could be the primary agent of human lung cancer in smokers. Now, since then we've brought in several other factors. It turns out that indoor radon decay products are also contributing to lung cancer. In the synergistic interactions of radon decay products in smoke-filled rooms, and their retention at the tumor sites in the upper respiratory tract, results in a combination of lead-210 and its decay products, bismuth-210 and polonium-210, and radon decay products also contributing. So, now we have several alpha and beta emitters contributing. So, the case for radiation-induced lung cancer is now a strong one. For a while in the mid-[19]70s, I was the only one saying so. Now, there are several other groups in Sweden, in Austria, in North Carolina saying the same things and doing further research on this problem. So, I feel that now the published evidence is good. Although, acceptance is not as widespread [laughter] as one might like that lung cancer is indeed radiation-induced cancer. It's combination of alpha and beta interactions that lead to it. Now, I've gone further since then. I've just completed a very comprehensive review, which suggests that natural background radiation is adequate to explain most spontaneous mutations and most spontaneous tumors. This is going to be controversial. But indoor radon decay products are suddenly recognized as having tripled the background radiation dose as we conceived it five or ten years ago. See, five or ten years ago, background radiation was principally cosmic rays, gamma rays from surfaces, and internal potassium-40. Radon decay products were one percent of background, and the background was a hundred milligram per year. Now, we have international agencies and national agencies saying, "But radon decay products alone are contributing between one hundred and two hundred milligram per year for the average exposure." So, here in the United States, we have three hundred milligram per year as the average exposure to background radiation, with two-thirds of it coming from radon decay products.

TD: Most of that being when you are in your house.

EM: So, we've gone from one percent to two hundred times that. [laughter] But this is only the tip of the iceberg. We have several other groups of radioisotopes that are just as important. So, my work on lung cancer in smokers and my focus that tumors are due to hotspots and the internal distribution of alpha and beta emitters that give you – there are several factors other than the average organ dose. The factors are, the local concentration in small tissue volumes, the mitotic index because dividing cells are much more sensitive to mutagenic transformations, and the oxygen tension in tissue. All of these are factors that determine the risk.

TD: How about aging? That was another thing that you are sure is very controversial.

EM: Well, my explanation of aging in mammals is a very logical one. Once you recognize that the internal alpha and beta emitters are the important agents of spontaneous mutations and spontaneous tumors, then you could begin to see what is really going on. You see, metabolic rates are inversely proportional to lifespan. This holds very well for mammalian species. Well, it also turns out that spontaneous mutation rates are also inversely proportional to lifespan. This is both the spontaneous mutations of germ cell mutations and sematic cell mutations. Well, it's fairly simple to understand when you realize that the important alpha and beta emitters are the internal emitters that you obtained by inhalation and ingestion. The radioisotopes that we inhale with the oxygen required for metabolic processes and the radio isotopes we ingest with water and with food, particularly with energy-rich food like calcium phosphate, these are correlated. So, as you go to shorter-lived species, you've got a higher rate of intake per gram of tissue in each mammalian organism. So, the mutation rate goes hand-in-hand with the metabolic rate. So, you have a logical explanation. But it's always been evident that you can increase the lifespan of experimental animals by giving them a very lean diet. You can increase experimental animal life expectancy by as much as fifty percent by giving a good diet, but just barely enough to survive. Well, the control animals are given all that they want to eat of the same good diet. Well, it's also been shown that if you give various large doses of alpha emitters to experimental animals, you can shorten their lifespan. Well, the logical explanation of this is that it's the somatic mutations and the germ cell mutations due to the radionuclides alpha and beta emitters that are inhaled and adjusted, that are responsible for the dominant lethal mutations which cause stillbirth, sudden fetal deaths, birth defects, and which influenced the rate of development of the main causes of death in middle and old age, cancer, atherosclerosis and the other diseases of aging. So, now I can't explain in a few minutes.

TD: [laughter]

EM: I can't set forth the evidence.

TD: No. This is not really what this is about. The main thing though I just wanted to pick up for the interview is that this is your area of research and that you are doing it at NCAR and you feel that it is appropriate work to be done here.

EM: Well, I think every scientist wants to work on the most important problem that he can contribute to.

TD: I thought some worked on the ones that they get the most grants for.

NG: [laughter]

EM: Well, those are the less fortunate scientists, that's those who are circumscribed by circumstances so that they're forced to work on funded projects and are not given the freedom that we should have for some science. I think older experienced scientists who want to work on an important problem, and which requires them even to cross disciplines, should be in more cases than not, allowed to do so. Because if we're going to make any progress on complex interdisciplinary problems, we have to have competent, experienced people crossing disciplinary lines and contributing a new point of view and a fund of information, which helps to sort out the intelligence speculations you find in each discipline. We have too many specialists today and too few generalists. I think every national center and every university ought to have a limited number of senior incompetent scientists that are allowed to work on these complex problems. It's very demanding and time consuming. If you are a radiochemist and you find you want to you work on the role of radionuclides in human cancer, now you've got to study radiation biology, radiation genetics, cancer biology, cancer epidemiology. You have to [laughter] get into a number of disciplines. It's difficult. You have to learn a whole new language. You have to have dictionaries of biology and genetics [laughter] and medicine before you can do this. So, it takes a while to make the transition. It makes so many demands on your time to absorb this material that I think most people who appreciate the difficulties would not accept such a challenge. But if you have the opportunity to do such a thing, a few people should do it, a few more people than now.

NG: Do you see NCAR in the future as continuing to support independent research such as the kind you are doing?

EM: Well, I hope so. But they are stimulating more interdisciplinary research. They're concerned with atmosphere-biosphere interactions, and I think that's important. When someone wants to follow atmospheric pollutants into biological systems and their distribution and their cellular effect, I think they ought to be encouraged to do this. I don't suggest that young scientists do this. But I think that if in older scientists of ability, if their interests carry them into these important side issue areas, I think that centers like NCAR should support at least a limited number of people to do such research, and to give them the freedom to do so. Well, I was delighted several months ago to be invited to present a paper at the Gordon Conference on cancer. That's in August of this year. It's an indication that some of my work on the importance of hotspots and the internal alpha and beta emitters as tissue sites of high tumor risk, and therefore important contributing factors in human cancer being recognized. I think a few radiobiologists have been paying more attention to the fact that we cannot ignore the non-uniformities in the distribution. The fact that alpha and beta emitters are extremely effective mutagens, are independent of dose rate, down to the individual interactions of alpha and beta emitters with cells and cell nuclei and DNA. So, now I find it's been a struggle to cross disciplines [laughter] and gain enough knowledge of the important work and relevant work in each discipline to discuss it intelligently in interdisciplinary audiences. But I only wish I'd started to do this ten or twenty years earlier in life. [laughter]

TD: [laughter]

EM: But I probably would not have been given the opportunity had I done so.

TD: Well, thanks, Ed. I think that was about the right length of time as well.

NG: Yes. We are about ready to run out of tape. [laughter]

TD: Just run out of tape.

NG: Well, thank you very much.

EM: Well, we didn't talk too much about personalities, I guess. [laughter]

TD: No, no.

EM: You're off now? Excuse me.

NG: No, I will be now.

[end of transcript]