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TAPE RECORDED INTERVIEW PROJECT

Interview of F. Sherwood Rowland

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Interviewer: Jeffrey S. Gaffney

Gaffney: OK, this is an interview with Dr. F. Sherwood Rowland, Donald Bren Research Professor of Chemistry at the University of California, Irvine, and the 1995 Nobel Laureate in Chemistry for his work on stratospheric ozone depletion by chlorofluorocarbons. Today is August 1st, 2007. We're interviewing in the Chemistry Department at the University of California, Irvine in Dr. Rowland's office. I'm Jeff Gaffney, Chair and Professor of Chemistry at the University of Arkansas, Little Rock, a member of the American Meteorological Society History Committee. So Professor Rowland, can you tell us a little bit about like where you were born and when?

Rowland: I was born in Delaware, Ohio on the 28th of June of 1927. Delaware, Ohio is the home of Ohio Wesleyan University. At that time, the population of Delaware was around 8,000; now it is around 15,000. At that time, it was -- it still is 23 miles north of Columbus, Ohio, but at that time 23 miles was a very long distance. Now it has some aspects of a bedroom community for Ohio State, so... But it was a farm community. About one quarter of the students in the high school bussed in every day, every morning and back in the evening.

Gaffney: I know your father was Chair and I think Professor of Mathematics at Ohio Wesleyan. Can you tell us a little more about your family?

Rowland: Yes, I'm the middle of three boys. The older brother, who is five and a half years older, is Sidney Rowland, which is my father's name as well, and my younger brother, Richard Rowland, is two years younger. And Sidney was, worked in... He was a freelance writer for a number of years, and then he ended up working as an analyst with companies that were sampling opinion, and he lived in, still is living in Lawrenceville in a close suburb of Princeton, New Jersey, where a lot of these, where Gallop and Robinson and companies like that

are based. My younger brother has a Ph.D. in Chemistry, and he's retired, lives in Northern California.

Gaffney: All right. What did your mother do?

Rowland: My mother was a -- first thing that I would say that it's especially in terms of the timing there, she was a graduate of the University of Chicago in 1916, and met my father when he was a graduate student in mathematics. They married in 1922, and at that point my father took a position at Union College in Schenectady, New York, which is where my older brother was born. Then later, after he'd saved up money, he went back to Chicago to work on his Ph.D. again, and he was recruited from there by the Dean at Ohio Wesleyan, and moved there in 1926, and I was born there in 1927, and he was, for 32 years was the Chair of the Math Department, retiring eventually in 1958, and then going to the Mansfield campus of Ohio State University and teaching there for another six years. In the meantime, he had spent... Well, before meeting my mother, he had been, went through Officers Candidate School in World War I and spent something like 18 months in the last parts of the war, and then (inaudible) in France until shipped back in 1919, and that's when he went back to the University of Chicago to start his math work. He never actually, he never actually completed his Ph.D., in part because the supervisor literally went mad, and this made a complication for him in that someone else had to look over the thesis which he had written, and as the story as I heard it from my father was that he had told them the -- talking with his substitute professor, the professor said, "Well, I will accept this for a Ph.D., but I'm not sure I understand quite what the problem that Professor So-and-so gave you was." And my father said, "I won't submit the thesis under those conditions then," and went off and never did finish his Ph.D. So I'd say that a certain aspect of it was, I would attribute to his Southern upbringing, that sort of Southern gentleman code of honor, that's something of that sort. He was born in Camden, Arkansas and went to, graduated from Ouachita College in the Ozark area of...

Gaffney: Yeah, I'm very familiar with that school. Yeah, Ouachita Baptist is... We get a number of our students at UAR from there into our graduate program. I know your high school education was accelerated a bit. I know that my father, that a similar kind of thing happened. It was, I guess, a trend: if you were bright and capable –

Rowland: Well, I started when I... I guess I was sort of -- I don't remember the circumstances, but at the time that people were thinking of putting people in kindergarten, they put me in first grade, and then... So I started first grade when I was five and came up to the fourth grade, and I was only in that about two or three days before they moved me to the fifth grade, and so... And that meant that I was, sort of June to September, I was on the average, say, two and a half years younger than everybody else in my class. That wasn't a question of asking whether I wanted it or not; of course, I had no objection to it, no reason one way or the other, but...

Gaffney: One of the things I read in your autobiography which caught my interest was the fact that you wrote about your high school science teacher asking you to do some weather station work. Tell us a little bit about that.

Rowland: Well, the volunteer weather stations at that time measured only a few things. They had max and min thermometers, they had a rain collection device which collected, had an area, opening that was -- I remember it was about a foot across, and down at the bottom of it went down there down to $1/10^{th}$ of that area, so that if you had a tenth of an inch of rain, you had an inch to measure, and so you would, once a day somebody would put the rain gage in and reset the two thermometers and record those, and one of the science teachers, Thomas Graham, on several occasions when I was probably 13 or 14 or maybe even 15 had me read those while he went on vacation, and so I would. That was one of the duties that I had. He just asked me to do it, and of course, this was a small town and so I'd be pleased that they would ask you, also meant that you were there at least during the time that he's on vacation. You weren't going on vacation! (laughter)

Gaffney: So did that spark interest at all in atmospheric work back then, or...?

Rowland: I'm not sure it was, say it... Maybe the numerical interest, wouldn't do anything about the long-term aspect other than that it was the beginning of something in regard to science.

Gaffney: The other thing I note is that I think something you and I probably had in common a little bit, and that was sort of being scholar-athlete in high school, college, so if you could talk a little bit about your interest in sports, and...

Rowland: The first kind of effort that I made in sports was I guess to go out for the, when I was in 9^{th} grade, which meant that I was about 12 years old, went out for basketball. The basketball coach was also the math teacher and had gone to Ohio Wesleyan, where he had majored in math under my father, and the ninth grade, I was probably something of the order of fourth team on the scrubs. I mean, they didn't cut the team down, and they allowed you to... But I think I may have gotten into one game, just long enough to fumble the ball, and that would've been my, that was my... So let's say, we just ahead -- one other thing about it, though, is that he persuaded me to take up tennis, and so I started in my sophomore year playing tennis all the time in the spring, and by my junior year I was good enough to play on the tennis team, which in Ohio was not the same as Southern California. But actually, that junior year I was unbeaten in... You have to realize that the way it was done there was there were people who were, played in positions one, two, three, four, five, and I started out being the number 5 singles player, and after I won a few times I was moved to number 3. I could never have beaten our number 1 player. That was -- there was a sharp gradation in the skills. And then I also played... But I lettered in tennis my junior and senior years. My senior year was, I was getting very interested in baseball, and I didn't play tennis as much and didn't do as well. And so my sophomore year on the basketball

team, I was, again, way back on the junior high people, and my junior year I was the sixth or seventh man, until we had about five or six games left in the basketball season, and then suddenly they moved me onto the first team of the JVs. And it was not explained to me at all, and it was only much later that I realized that they had noticed that I was six or eight inches taller than (laughter) I had been at the beginning of the year, and at that rate, by next year I was probably going to be maybe the tallest kid in the school, and I'd better be getting some time on the court. And my senior year, I was sort of the equivalent of fifth or sixth man; I was not always starting and not always on the bench, but played a fair amount, was not very good. And that was... And then, but I was 15 the year that I graduated.

Gaffney: Yeah, you were a lot younger than the other kids! (laughter)

Rowland: Yeah, I was a lot younger than the other kids. So then I went immediately into University. This was 1943, it was World War II, and so I went in to class starting around the first week in July and proceeded to go... Well, I did go at that point to, sometime there to our, the big person at Ohio Wesleyan who was coaching basketball, and I wanted to go out for the team, and he said, "No, I saw you play in high school!" (laughter) And then that particular coach was, left -- he wasn't the coach that had been there in 1941 or '42, they had had a replacement, and he left and went somewhere else, and a new man came in and saw me playing in intramurals and asked me to come out for the team. And this time -- this is the year I was 17 -- I made the team, which was really... We won three games out of 17, so that was a... There were about 200 men in the school at that point. If I follow this on, when I turned, was approaching being 18, I went into the Navy, I enlisted. At that point, the circumstances were that if -- well, other things being equal on your birthday, 18th birthday you got the notice of where to report for the Army. It was not a question of -- the only thing that was questioned is you had to pass the physical exam, and if you didn't want to do that then you had to have listed already in the Marines and the Navy. So I enlisted in the Navy. I signed up probably in May and went in just before my 18th birthday, that summer, and was in boot camp when the bombs were dropped on Japan, and I had gone in as a seaman first class -- this will get involved in the fair Navy story -- as a seaman first class to get radar training, and they came to us after Hiroshima and Nagasaki and said to all of the people... They wanted radar operators for the [picket?] boats out in the Pacific, and they didn't need them anymore, so they said to us, "We have an offer for you. If you really want to get the radar training still then we will have you do that, but you'll have to enlist in the Navy as a regular enlistment for four years, or we will allow you to be transferred to the separation center nearest your home and stationed there until discharge," which was probably going to take a year or something like that. Well, so I took the -- I did not enlist for four years -- I took the offer, and Great Lakes, which is where we were, was the place nearest my home in Ohio, Great Lakes being just north of Chicago. So now it was September, and so I went out for the Great Lakes basketball team, and survived for a while. I don't know -- I would guess that Great Lakes had something like

20,000 people there, and although I was a moderately good basketball player, I was not in the top 15 of those 20,000. The people that I remember that were on that team included two people that most people my age recognize their names immediately: Bud Grant, who was the football coach at Minnesota, also played for the Minneapolis Lakers, which is before they became the Los Angeles Lakers later, so Grant was a first line professional basketball player, and Ara Parseghian, who was the football coach at Notre Dame, was a first line athlete but was a very fast guard. Well, those are the people that did survive, and so I was back as a seaman in the Great Lakes. And then they opened, the Navy opened a separation center at Toledo, Ohio which was closer to my home, and so they shipped me to Toledo. Well, Toledo probably had 400 sailors, and I was [fairly?], whereas I had been an [also ran?] at Great Lakes. I was not an [also ran?] at Toledo, and in fact was selected at the end of... We had one professional basketball player, but he was a lieutenant, and he got discharged a couple games into the season, and I ended up being the most valuable player on the team. We won 36 games and lost eight, and that was basically my job for about six months, and then they closed Toledo Separation Center and I was shipped back to Great Lakes, and then to Northwestern for about two months where they were closing, we were closing down a Navy V5 as an aircraft pilot training that was being run on the Northwestern campus, and then I was back at Great Lakes until I was discharged. I think it is worth going into the discharge story just a little bit, because it was... I had run into, at Great Lakes I ran into a person that had been a fraternity brother at Ohio Wesleyan, and he asked me what I was doing, and in addition to being named the Most Valuable Player on the basketball team I was promoted to Petty Officer Third Class and was given choice of did I want to be a specialist in entertainment or in athletics, Spec E or Spec A, and I took Spec A. And then what that meant was that the Navy sort of would send you, wherever you were being stationed you would be attached to the welfare and recreation group. And so during the day at Toledo, my job was handing out books in the library, but there was a librarian that was there all the time, so that was really a very modest job. They had put me back in welfare and recreation, and he asked me what I was doing in Great Lakes -- this was probably late, near the end of July -- I was, told him I was running a bowling alley, which is in fact what I was doing, but it was a bowling alley that was only open five hours a day and almost never had anyone there because they had no pin boys and people didn't want to bowl -- there was a bowling alley down a football field away that had pin boys, so everybody was going there, and we really would occasionally have somebody come in, and so that was my job. And I asked him what he was doing, and he said, "Well, I'm typing discharge lists." And so, and then he said, "Would you like to get discharged?" I said, "Well, I still haven't accumulated the points for -- it'll be another couple of months, another month or two." He said, "Sometimes I make mistakes," and then we discussed where I might be sent on this mistake, and he sent me, said, "Well, I have a place on Long Island or place in Florida or one in California." "Well, I never been in California," and about a week later the discharge list came out and I was being sent to San Pedro, which is about 20 miles up the coast from where I am now.

Gaffney: (laughter) I want to check this thing. I think that we're going to stop it now and make sure I can flip this over so I don't lose the rest of this.

END OF TAPE 1, SIDE 1

Interview of F. Sherwood Rowland

TAPE 2, SIDE 1

Gaffney: OK, so we finished the first half hour of Tape 1, and we were talking about San Pedro.

Rowland: So when I was discharged in San Pedro, then I hitchhiked home to Ohio, and that was... So that was a matter of going to, first to -- well, actually, not very far from here. There's a pavilion that was a dancehall pavilion, and I came to someplace down in this area while I was... It only took about three days to get discharged, but you were allowed to leave the base, and then I went up to Hollywood, then hitchhiked up to Santa Barbara where there was a festival with a parade, and I remember staying at the, guess it's a Marine Corps base at Port [Winamy?], then headed up to Yosemite and eventually to Yellowstone, and when I left Yellowstone I had all my things with me and didn't have any plan. I was going to do what, decided if the people were leaving I'd leave, if they were going around, I would stay. And turned out they were leaving, and so I left, but at that time and before that, going across Nevada, I mean, wearing a uniform people would stop for you, but Nevada in August of 1946 when no automobiles had been manufactured since 1941, let's say it wasn't heavy traffic. I did stay up all one night after I'd hitchhiked out of Reno, late in the actually in the evening, which didn't make any sense but none of it was sensible anyway. I was picked up about 8:00 in the morning by the first car that had passed in seven or eight hours, and they drove, and I was with them for about four hours, and then they were leaving, going in a different direction, and at that point I did take a bus finally in Nevada for the last 100 or 200 miles into Salt Lake City, then went up to Yellowstone and the people [at the end of it?], people that were leaving dropped me off near Rollins, Wyoming, and then I was picked up by a convertible with four Ohio College girls in it. They picked me up to drive the car, and I'd never driven a car, but I rode with them all the way to Chicago. Eventually, they were all from the Cleveland area, and eventually I was in the Cleveland area two summers as a, working as a camp counselor, and one of the other camp counselor's older sister had been one of those, that group that had picked me up because they had talked, they had mentioned it at some point, having picked up a sailor to do the driving and couldn't do any of that. But in any event...

Gaffney: So you made it back to Ohio Wesleyan finally! (laughter)

Rowland: So I eventually got back to Ohio Wesleyan and decided that -- my credit hours were such that I only needed one more year, but I wasn't, my viewpoint after a year in the Navy was that it didn't make sense for me to rush through college, and so I stayed for two years, and that leads to the kind of -- they say, "What did you major in?" Well, I majored in chemistry, physics, and mathematics, which to the present day sounds enormous because most of the time you're talking about a university that has a much higher requirement of classes. I

did have five years of credit, but in order to be qualified with a major, all you had to have was a number of hours that wasn't that hard for any of the three areas that I had the classes in. And I also, well, I was elected as the senior class president at Ohio Wesleyan, and I had -- while I was at the first stage, during the war, I was the editor of the, sports editor of the newspaper, but that meant the conflict of interest that one has covering away games when you're the only person there, so that is getting an automobile to travel to 50 or 70 miles was a big difficulty just for the local basketball team, let alone for fans. Well anyway, that was...

Gaffney: So I've got one question for you: Your dad was math professor, you majored in physics, chemistry, and math, why chemistry? To go onto graduate school?

Rowland: The chemistry department, well, the math department... I'll put it this way: While I was there, my father wasn't there. I mean, he didn't come back from -- he went into the service in September of '41, called up for before Pearl Harbor, and got back about March of '46. That was while I was still in the Navy, and the people who were there during the war were stronger in chemistry than they were in physics and in math, so I was taking a little bit more chemistry and kept that as a major, and then went off to University of Chicago. It's absolutely true that I knew nothing about the University of Chicago except that my parents had gone there, then said it was the best university in the world, and so there was no point in –

Gaffney: Arguing, right? (laughter)

Rowland: -- arguing about it. It's also true that one of the circumstances for a student or for a person that went into the service, that you got credit towards paying, for paying your costs of college is the GI Bill. Well, the GI Bill for me furnished 27 months of support for being in a university, and that would pay for your books, it would pay your tuition, and the stipend. I don't remember what the stipend was but it was comfortable for me, at least. Well, it didn't make any sense for me to use that at Ohio Wesleyan, 'cause one of the characteristics there was if your father was on the faculty then you got free tuition, and so free tuition and living at home, there was no sense in taking that. And so when I was applying to graduate school, I had three years –

Gaffney: Of support.

Rowland: -- of support that I already knew about. I did apply for a fellowship where, which was not a major fellowship. Did apply for a major fellowship that was open to descendants of World War II veterans, so I got in on the basis of my father's service in World War I, and when I got to Chicago and found that everybody else was being paid as TAs, (laughter) I'd never heard about this. So I really knew the name of no faculty member, only that my parents had gone there and thought this was really an excellent university, and they were correct.

Gaffney: Yeah, I'll say! Basically, you've got Enrico Fermi and Harold Urey are in Physics and Chemistry as already got Nobel Prizes, and you had Maria Goeppert Mayer, who was I was very familiar with 'cause she actually was honored at Argo National Lab where I used to work, Henry Taube, and, of course, your thesis advisor, Willard Libby, all go on to get Nobel Prizes. I know when you walked in, in your autobiography you said that you were basically assigned Libby. Can you just tell us about that and...?

Rowland: They felt -- I'll give you what I think were the approximate figures. The number of people that had come in in that year to be the first year graduate students in chemistry must have been at least 60 or 70, and 'cause it was a very large class, part of it being that there were a lot of people who went into World War II as not necessarily very serious and they came out of it as very serious, and so that they would say that what we'll do is give everybody a chance, and keep the ones that really are good enough. And I think, again, the figures that were never published, but my impression was that 25 or 30 -- I think it was maybe 25 of those 60 or 70 -- were allowed to come back the second year, and there were... Or maybe there were four or five on the cusp, and I think they came back, too. So only about something less than half the class survived the first year, and there was a point when I went in to see Jimmy Parsons, who was the -- Professor James Parsons was the person who was in charge of the graduate students, and he had -- this is not the story you were thinking about, but he... I went in to see, at the beginning of the winter quarter, to see whether or not I could get an assistantship, and he said, "No, we gave all those out and so there aren't any." Then he said, "What was your grade... By the way, what was your grade in Professor Urey's course?" And I said, "A." "What was your grade in Whalen's course in Organic...?" And I said, "A." And he asked, "What was your grade in Taube's course in Organic...?" And I said, "A." (laughter) And he said, "Come in tomorrow!" And he had a half assistantship available for the rest of the... And in fact, the duties that involved -- at one point, what I was doing was assisting a man named Tuffy Young, T.F. Young, who was in charge of a laboratory for the graduate students, and I was grading lab reports in the course that I was taking because obviously they had already assigned all of the TA people that were... And, well, in addition to, actually to Urey and Fermi and Maria Mayer and Henry Taube and Bill Libby, my Chemical Physics course, which -- I took Physical Chemistry for two quarters from Urey, and then had Edward Teller for Chemical Physics in the winter, in the spring quarter of my first year. But it was also -- the scuttlebutt was basically "Go to any lecture that Fermi gives on anything," because he was a great teacher as well as an outstanding scientist. And so I did go to Fermi's course, and I still have the -- somewhere right over here, almost in arm's reach, but I don't... At the end of that course, the three physicists -- they were graduate students that probably were TA'ing it -- I didn't sign up for it, I just went to it -- they produced a book, *Fermi's Notes on Nuclear Physics*, which I bought one that had came out there, but it spread rapidly enough that copies of this University of Chicago privately published by the three graduate students

became something that, they redid it for national circulation because some of them had showed up at Berkeley and other places.

Gaffney: So you got Libby as an advisor, and...

Rowland: The story that I was... We went into, he called me in just to discuss what I was going to take, and he looked at my transcript and said, "I can see that you made straight A's as an undergraduate; we're here to see if you're any damn good," (laughter) which is interesting!

Gaffney: Yeah! Did you think about any other possible advisors besides Libby?

Rowland: No. Carbon-14 dating was intriguing, and Libby had a lot of... There were probably five of us from that class that followed him: Dick Wolfgang, and Thompson [Dhara?], and Howard [Hernig?], and Andrew [Settle?]. Andy may have been a little off in terms of maybe (inaudible) a quarter one way or the other, but there were... And everyone had a different problem, and Libby had stored up, when you went in to talk to him he pulled out a sheet of paper that he'd decided that this would be the right way for you, but he'd stored up these ideas during World War II, and now, and carbon-14 was the first one, and the understanding of the [Zulard Chalmer's?] reaction was one of those, but each of us had a different problem.

Gaffney: So I know you continued to play athletics, and you actually played semi-pro baseball?

Rowland: One of the things which, when I arrived in Chicago, I [debated?], first thing I had to do was to find a place to stay, and so for the first month or thereabouts I stayed with my grandmother and the youngest -- this is with my mother's brother and his wife, the Drake family that lived in Chicago. The Drake family included Frank Drake, who was the eldest son of that family, and this is Frank Drake of SETI, the Search for Extra-Terrestrial Intelligence, so later on when Frank started talking about Project [OSMA?], we had vacationed frequently in the summer during the time period of, let's say, when -- around the time that I was 10 or 12 and Frank was two or three years younger, we would, my father would go into the Service during the summer for two weeks at Fort Knox in Kentucky, and those two weeks we would go to Chicago, and my mother and my younger, and the three boys would spend... This is her chance to see her mother. Of my grandparents, I only ever saw one of 'em. Both of the male grandparents were dead before I was born, and the mother, and then the other grandmother died when I was still one or two. So I'd lived in, I'd lived with the Drakes in Chicago for a few weeks, and then went to, found a place to move in on the campus in Chicago, and one of the other people that was rooming in the same place was Thomas (inaudible) [Dhara?], and Tom and I became buddies all throughout graduate school, and he ended up working for (inaudible) Libby, too. And well... So the question...

Gaffney: Basically, I was just asking you, you know, about -- I know you played semi-pro ball, so I was trying to figure out how that started.

Rowland: So now we're in the September, beginning of something like that, of 1948. I started hanging around the gym playing basketball, and somebody pointed out to me that I was probably eligible to play on the basketball team, and so I checked into it and found out that their eligibility rules were that you could play any sport until you had won four letters in it, as long as you were enrolled in the University, and that had its background, the fact that the Hutchins approach, President Hutchins of the University of Chicago had created the Hutchins College in the late '30s, and that recruited people after their sophomore years in high school at a fixed curriculum of 16 courses for four years, and everybody took the same 16 courses. There was one examination in June of each year on each of the... For a year you would have four courses, of which -- and then it would be one exam, one six-hour exam at the end of the year for each of those four courses, and that was the only grade you got. Furthermore, during the, let's say, the fall and the winter quarter, they didn't even give exams, but the exams were written by people that didn't, weren't involved with the lectures, and so there was a very loose kind of grading and a very strict grading in the sense of testing at the end of the year. But at the end of your -- if you did that, then having come in as a sophomore in high school, that athletically they let them play for two years for the University High School, and then two years at the University. You would be sort of the equivalent athletically of sophomores in college somewhere else, and so... And then you had people coming in with strange transcripts from other universities and all of this, and so they just made the rule that four years, four letters in a sport was enough. So I went out for the basketball team and made it, and had reasonable season, started out very well but I ended up spraining both ankles and so there was... My high points for the year were the first games, before I sprained either of 'em. But so at this point, so I'm, well, playing basketball -- basketball practice is in the afternoon -- but talking to, sort of as we got into (inaudible) February or so on, they start talking about there would be a spring baseball trip, and I said I played very little baseball, I played a lot of softball. And my basketball buddies were saying, "You should come out for the team because there aren't 19 guys in school that can play baseball!" (laughter) So when the time came for, they started practicing, well, I came out for the team and made the Southern trip between the winter and spring quarter, and let's say I went down as one of the people that went on that trip, as one of the possible candidates for the team, I had a good Southern trip, and ended up coming back I had hit 400, and was installed at first base, and basically I stayed there for three years. I'd run out of basketball eligibility there, but the rest of the time, my second, third, and fourth years I was playing in the city leagues for independent basketball teams, and actually, my best year in basketball wasn't -- best year in basketball period was my third year at Chicago. Our team won the City Championship and I was Most Valuable Player on that team. Then we went down to Springfield in the State Tournament and won one and then lost, and so... But that was better than any of my seasons in college. OK, so baseball was something that I came back from the spring trip as the

cleanup hitter, and so I played three years there. In the spring of 1950 -- this is at the end of my second year of playing baseball, then I played baseball for Chicago and I hit about 350 or something like this -- I went home to Ohio, and then when I was there I got a telegram from the baseball coach at Chicago, who had sent a letter in answer to an advertisement in Sports Illustrated -- not Sports Illustrated, Sporting News, which was a baseball paper that came out there. There was an advertisement in it asking for a left-handed pitcher, which I was not, but the coach had said, "We have an outstanding left-hand pitcher, man named [Gene Morowitz?], on the University of Chicago team." And then he had two other paragraphs, "I had a second baseman and a terrific fielder, good hitter, man named [Jim Geocaras?], and a first baseman who is especially, who is a high average hitter and a very good target as a first baseman." And now we switch away to people in [Rutting?], semi-pro baseball team in [Oshima?], Ontario. They had, in 1949, they had recruited a pitcher and a catcher from the United States and had done fairly well, so in 1950 they had decided to pick up more American college students, and they had brought in two shortstops, both of whom were supposedly good fielders, good hitters. But they had two of 'em, and so they put the tall one at first base, and so there came to a critical game against Peterborough, the Oshima's prime opponent, and there had been two high throws that the tall shortstop had [missed?], and they went back to looking through these letters, and a good defensive fielder was just what they were looking at, looking for, so they went to the coach and he sent me a telegram, says report to this place in Ontario, so I drove up to Ontario instead of going back to Chicago. And I didn't know any of that background. So I got there one day...

END OF TAPE 2, SIDE 1

Interview of F. Sherwood Rowland

TAPE 2, SIDE 2

Gaffney: OK, you'd just been telling me about... Go ahead.

Rowland: So they put me into -- I hadn't played for two or three weeks, and anyhow, now they had taken the tall shortstop and put him at third base. And so somewhere along about the fourth or fifth inning, the person hit a, the batter hit a sharp ground ball on down the third base line, and the third baseman came up with the ball and made a throw that was down toward home plate and in the dirt, and so I came off the bag, scooped it out of the dirt and got the runner as he's going by. Well, that sort of ended it right there. Instead of having somebody (inaudible) throws that were struggling, this was a sure error that had been turned into an out, and then I had, the first eight games I got hit in each one, so that sort of took care of this for the, took care of it for the first three weeks or so. And because the arrangement was that they paid a certain amount of money and furnished housing and then found the job for you, and the job they found for me was at General Motors in Oshowa, which is, this was the leading General Motors plant, and working on the truck [work?]. And working on the truck line was... It wasn't really work, in it was work in a certain sense because they had said, "You got to find a place for this person," and what they found was it was interesting in this sense about the dyes that stamped the truck cab and the chassie didn't quite match, and they were supposed to be on each side of the chassie. There were three places where you would bolt the cab onto the chassie, and then it was slightly out of trim, and so the holes didn't quite match, and so they gave you a file and you would file each of these holes so that they would match. So you had, each truck cab that went by, you had to file these three holes down a little bit. Now, the first day your arms are just dead. By the fourth or fifth day it's not too bad, and by the seventh or eighth day I was finishing getting the holes all lined up for ten cabs, and then climbing in the last cab and riding it up front. And then, so finally they said, "You really can't go further back up the line than here, and so that you..." You'd doze until the noise of the place where they were putting it together... And so I said to myself, "I don't really need the money for doing this. I mean, I've got my housing, I've got this, I've got the money that I saved up from" -- 'cause I didn't spend all the money I had on GI Bill anyway, and I didn't come up here for the employment, I came up here just to play baseball, and so I quit. I told them that I was quitting, and they didn't understand, they being the people running the baseball team. They didn't understand that I didn't care about that amount of money. They thought I was going to leave. But now, on the baseball field, we had a game in which one of the local players was going to go into third base, and the manager was coaching third base, and the manager watched the signals, and the player was the oldest fellow on the team and he was probably 30 or something like this. When he slid he caught his (inaudible) and broke his leg, and the next morning, or the next day, the newspaper in Oshawa really just, the sports section really rode up and down on the manager, who was a volunteer.

And so he quit, and next thing you know, when I had told them that I was quitting General Motors as of Saturday, as of quitting at the end of the week, they came back to me and said, "We found another job for you, (laughter) at the [Duplate?] company. You don't have to go in to do anything but pick up your check. It's a different job, different kind of job, and we're making you the manager of the baseball team." (laughter) And so this new job, I actually did a little bit of work there, but then they said, "Don't bother coming in, just come and pick up your paycheck," and so that's what I did for the... But the key thing -- we had really quite a good team, and we were the class of our league, but they also had a semi-pro tournament that is connected to the semi-pro baseball tournament in the United States, but the semi-pro tournament at that time, the transportation being what it was, was just the Ontario teams. There were three leagues, but it was playing for the semi-pro championship of Canada, [quote?], because none of the teams from Alberta or Saskatchewan were involved in it. And in that particular, we went for a... It was a double elimination tournament, we were playing in London, Ontario, and we, when we got there we were scheduled to play against a team from Windsor, and so we managed to -- well, we had the scores that they'd played a game the night before, read the score sheet and noticed that the other team, that Windsor had won but the other team had stolen a lot, several bases. And so our lead-off man, as I remember, the lead-off man walked, stole second, the next batter got a single, scored that one, he stole second, and I was the next batter and I got a single, and he scored, and we won 2-1, and we had only three hits, something like that. And then the next game, we were losing 7-2 and rallied for... This is as an athlete -- we had a big rally seventh or eighth inning, and I got on. It ended up that we were, by now it was 7-7 and there was one out. Our pitcher was coming up and was not a good hitter, and I was on third base, and we had another runner on second, and the pitcher swung and missed the first, I believe... There was one pitch... Well, the first pitch was a ball, and being on third base I had tried [just testing?] the lead-off, and so we came to... The first pitch was a ball, and then I called time, called the pitcher, we talked, and said, "I'm coming in on the pitch after the first strike," and the next pitch was a ball, and then the next pitch was a strike, and so (inaudible) after that. I had a terrific lead, and from my point of view I had home stolen (laughter), but of course it was a suicide squeeze, and the pitch was high. The catcher saw me looming and missed the ball, and they scored it as a pass ball! (laughter)

Gaffney: Your only chance of stealing home! (laughter)

Rowland: They scored it a pass ball! The runner on second was caught trying to go home on the same play, but we won 8-7, and then that night we won 6-5, and then our management said, "Now, we've got, we're the only unbeatable team left," said to the people in London, "We have volunteered to play out of order," and so the team that we won, had beaten 8-7, we said, (inaudible) said, "Now we're in charge, you'll have to come to our place," and they came to our place and we won 9-1. So it's absolutely true that we won the Canadian Semi-Pro Championship that year. It's also true that I hit 500 in the four games of the tournament. So if

you want to say, where do I feel about that... Probably that particular play was the highlight of my baseball career.

Gaffney: I'm kind of glad you didn't go into the majors as a pro because we would've lost a great chemist! (laughter)

Rowland: The best that I was -- one other way of putting it was that we did, they did pick a Canadian All-Star Team that summer, it was Ontario All-Star, and we played the Toronto Maple Leafs that then was a Triple A professional team, and we were rained out in that game winning 2-1, where I had been up twice and had one single (inaudible), and I was selected for the All-Star team, was batting clean up for them and got a single to bring in the batter. But the person that was ahead of me in the batting order was a defenseman on I think the Montreal Canadiens hockey team. He had hit, the first inning he hit a ball and it went about 400 feet. He hit a double barrier out there that kept it in the park and ended up on third, and then I got a scratch single that brought him in. (laughter) So it was the kind of thing where you could see perfectly well that this is something, that I was playing at a level that was just right for me, because I got a lot of ground ball hits that would have turned (inaudible). So the better the team you're playing against, the less likely that -- you'd start being out on some of those and your average would shrink, so... And then they asked me to come back the next summer in March to be when they started, or in April when the season started, and I said I couldn't, I was in graduate school, and came and went back [at?] 51. But they had a professional person who had pitched for Jersey City and then [put a kink in?] his arm, and he went there at the beginning of the season, and I joined the team and played it. Then the following year -- this is now 1952 -- Tom [Sugahara?] and I both got our Ph.Ds, we both got married, and we took a trip together, which took us to, eventually we went to Montreal and then stopped at -- he was going to a post-doctoral at MIT, and then he ended up after that post-doc going to Clark University in Massachusetts, and I was going to an instructorship at Princeton, so we traveled together, Tom and (inaudible) and Joann and myself. But we stopped in Oshawa, and they tried to persuade me to break off this trip (laughter), and I said, "No, I can't do that." So that was the end, that was the last time I played baseball.

Gaffney: So you mentioned your wife and marrying in '52. I guess they always say there's a great woman behind every great man. You want to tell us a little bit about your wife?

Rowland: Yeah, Joann is a graduate of the Hutchins College. Now, she was... I found a letter that said, was basically "We're awarding you a full tuition scholarship for four years at the University of Chicago," and if you worked out the dates on it, she was 14. So how did I meet her? I guess she was -- we're essentially the same age, she's three months older -- she was, after you graduated from the college, took one year in School of Education, decided that wasn't for her, and they hired her as a person who would recruit students for the college. So

she started doing that when she was about 19 or 20. When I met her, she was working, had a job at which she then very quickly changed to, was in a management training class for Time Magazine, Time Magazine having their circulation department in Chicago, their editorial department in New York. So she was in training for that. But the way -- we met at different times, so I have to explain that. There was a time when a fraternity brother, not a fraternity brother, well... At that time, the fraternities were not in strong position and they took in roomers, and so there were people living in the fraternity house, there were members and non-members, and I was with one of them when we went to the Tropical Hut, which is at that time a very popular place, and there was a long line, and my companion looked in and said, "I know those girls, we'll go sit with them." So we went in and sat with, barged in and sat down, and he was, my companion, was certainly brash, maybe obnoxious! (laughter) And Joann remembers that dinner but not me! (laughter) So then we move ahead to the first time she remembers seeing me was she was going to talk to her boyfriend, and he was going to be refereeing an intramural football game, and they were, this was, the team that we had of graduate students, I was the last one coming, and so they were waiting for me. The boyfriend was going to referee, so she remembers me coming up there, and I refereed basketball with the same person, a man named Dick Boone, and he... I was interested in, I was a pen-pal... This is very complicated! (laughter) We were, the first baseball trip that we took we were scheduled to play against Eastern Kentucky and we were rained out, and so I ended up playing bridge in the afternoon with an attractive coed and a couple of other people, so we're talking to people there, and so we've moved, we traveled from Richmond, Kentucky, where Eastern is located, to Barrie, where we were going to play the next day. And then we immediately tried to get a car, we got -- one of the people had a car, and we went back to Eastern Kentucky, where we had made commitments to come back. So that was the occasion that I met her, met this young woman, and then we wrote letters back and forth afterwards. And so then I was going to, at one point I was going to go to, down to visit her, and by that time she had graduated and was now teaching in New Albany, Indiana, which is just across the river from Louisville, and the other football, basketball referee, Dick Boone, parents lived in Louisville, and I'm being cheap to ask him, "Is there a place you know where I might stay," and he says, "Oh, I will arrange that you can stay with [our?] parents." And then over the period -- and then he said... And now, before that time came up, he'd said, "And I'm going to go down, too," and then the day before he says, "I'm going to bring my girl with me," and so the girl was Joan, and then Dick left the trip for Chicago to Louisville, so we talked a lot. And I gave my fraternity pin to Betty Jane that weekend, and then two weeks later I happened to run into Joan at Steinway's Drug Store on the campus, and asked her how Dick was, and she had just broken up with him! (laughter) So I asked her if she wanted to go out, and that sort of... And I never saw Betty Jane again! (laughter) And saw Joan very regularly from then. And then when we moved to [Prieston?] she had a job there, Prieston being a small town with lots of graduate students and lots of graduate student wives. Jobs were hard to come by and she

had a job at Gallop and Robinson, but then after Ingrid was born, then she never did work again.

Gaffney: So I met Ingrid, and I know we have a mutual interest in history and thing, and I know you also have a son Jeffrey which, of course, is my name which is another interesting coincidence. Can you tell me a little bit about your children?

Rowland: They are... Ingrid is the elder. She was born in 1953. I finished my Ph.D. and went to Princeton in 1952, and Ingrid was born the following August. We had moved out to Brookhaven for the summer, but then she came back to Princeton to have Ingrid, and I had stayed out working at Brookhaven, and got a phone call, that she called and said, "I'm having, the baby is coming and you should come." (laughter) And I said, "I'm going to miss doing an experiment," and she said, "So am I!" (laughter) So I drove off. Ingrid was born at noon the next day, and then two years later -- Jeff is not quite two years younger -- and two years later... We spent 1953, '4, '5, and '9, the first three, they were summers from Princeton, '59 was summer from Kansas, so we spent four summers at Brookhaven. But the next year, Joan was -- we were leaving or getting ready for Brookhaven, going to the summer, the pediatrician in Princeton said, "If you feel labor pains before the Holland Tunnel, come back. If not, here's the name of a physician that you can have in Huntington, Long Island," and she did not feel labor pains on the way, so she stopped off to see Dr. Warden, and saw him, and then sort of ten days later or a week, a week later, then she did feel labor pains, and so Jeff was born in Huntington, Long Island. And then that was in '55, and then in the summer of '56 we moved to Kansas, and September of that year Jeff came down with polio, and he was one of two people in Kansas in that year to get polio. We had -- this is just at the time that the vaccines were coming in, and the assumption was made that you would have, first year you're protected by the immunity from your parent, and so Ingrid started her program, had two of them, two shots, in Princeton and... Jeff was, had one shot, and then did not get, and was waiting, coming up for the next one when he came down with polio. What that did was he was paralyzed from the waist down, but most particularly in his right leg, and in the end the right leg below the knee never, did not come back. The left leg did. So his first braces braced both legs, but later it braced, he had to have a brace for the right leg, and that was, started from age... Well, the braces were needed from age 15 months on. Subsequently, he had a very bad automobile accident in which he was ejected through the side window and got a brain, was hit such that he lost an ear, temporarily lost an ear. They found it and put it back on, but three [years later?]. But in any event, so his circumstance has always been the circumstance of a person who first had a leg brace 'til he was 23, and then, after the automobile accident he never really got to... Well, when we've sometimes traveled on an airplane with him, he could, if you got on he'd get out of the wheelchair and could walk down if he had support, but he doesn't have the strength and...

END OF TAPE 2, SIDE 2

Interview of F. Sherwood Rowland

TAPE 3, SIDE 1

Gaffney: OK, you were telling me a little bit about your son and his wife.

Rowland: Yeah, so he met... Jeff was, the automobile accident paralyzed the other side, not permanently, but it was a blow to the base of the brain, which turns out to be -- he was in a coma for eight days, and needed to... He was in, well, a rehab center for five months, and then came out to live with us, and at that point he'd had very weak stomach muscles, and so he really, you had to have him in a wheelchair and have a belt around him to hold him in. So his recovery from that, he lived with us for four years, and after a little while, that is a few months, when he recovered somewhat -- I should go back and just say that he had been a student at UCSD as an undergraduate and had... But he was 23 at the time of the accident, and when we checked on the insurance, first thing we found was that although I was still carrying him as a dependent, he wasn't covered anymore because it only went 'til you were 23. And so then, checked about the possibility that as a student, whether he was covered, and found out he hadn't enrolled that quarter. (laughter) And then found out that under California rules, he was more than 21 so he's on his own, he had no possessions, he had no nothing of value; he was covered completely by the Medicaid, (inaudible) California [level on it?], and that included everything that was in connection with it. But at any rate, after a while he went... Then he went back and finished his undergraduate at UCI and got an MBA, and has been a businessman in the, let's say, modest success. He met Christie well after, several years after that, and so he met her, when he met her he was already in a wheelchair, and they have two daughters that are now 16 and 12, and they live in [down South?]. Ingrid, as you know, has a Ph.D. in Greek and Classical Archaeology, is a reasonably well known art critic writing for the New York Review of Books, and is, at the present time, a Professor at the University of Notre Dame, the South [Bend?] University but in their architecture division in Rome, so she lives in Italy and has been a very successful academic.

Gaffney: I know the time I met her, I was very impressed with her, and I know, just hearing about your son (inaudible) sense [there's?] drive to maintain and get through all that says a lot about... I guess, you know, so I'm also interested... Now, you've worked, your initial work on your thesis and basically your early career was pretty much in radiochemistry and hot atom chemistry, and I think a lot of the work in Brookhaven, a lot of the atmospheric community I don't think really knows a lot about that work, so I wonder if you want to talk a little bit about that.

Rowland: Yeah, certainly, it ties in eventually. Of course, Libby had done carbon-14 dating. His first graduate student, Ernie Anderson, had been assigned the testing of carbon-14 theory by doing carbon-14 analysis on trees from the (inaudible) Southern Hemisphere, which demonstrated that the same, that carbon-14 was

equilibrated through the two hemispheres, and the cosmic ray intensity is a lot more intense per square foot in the polar regions, but that didn't make, doesn't make... Carbon-14 carbon dioxide has enough time mixing back and forth to be homogenously distributed in the troposphere. Well, that's something that -- that was in the air when you're a graduate student. Hot atom chemistry is looking at the characteristics of individual radioactive atoms that have been just produced in a nuclear reaction and which are not thermally equilibrated with their surroundings. They can be thermally equilibrated by providing something for them to collide with, like argon, something of that sort. And it's partly, I mean... One of the first things... Well, Libby, after carbon-14, had a list of other experiments, but one of them involved cosmic rays, and say that the next most likely atom, radioactive atom that you would have that would be worth looking into was trillium, with a 12.3 year half life, and he had a graduate student that was after, couple of years after me, Sheldon [Kaufman?], who later on was at (inaudible), so you probably knew Sheldon.

Gaffney: Yeah, I met him coming in.

Rowland: His thesis was done on the natural trillium, and what related to that -- now, this'll be a story, also, that... The first summer that we went [booking?], I was going to be a visitor there. They had the possibility of having some support, but not the first year. They made a, I mean... It was... I'm not so sure. It was either $6 a day or $3 a day, it's not clear (laughter), to go out to Brookhaven, and nominally to work with some of the people who were at Brookhaven, Andy Anderson and Al Wolf. But before that came around, I'd gone out to Brookhaven. Dick Wolfgang, who got his Ph.D. with... Dick and I started at the same time. He finished a year earlier because he was threatened with the draft in 1951, and so Libby took, said, "Do you go to Brookhaven?" And so when I went to Brookhaven, Dick and I talked, and he was recently married, and so we, I invited them to come to a dance at the graduate college at Princeton, and we talked about some kind of research that might be done, including, I think, at the intermission. (laughter) In any event, following that now that Wolfgang and I were talking with each other, decided that it would be something that we would want to work on was measuring the trillium production by cosmic rays, and so we sort of agreed that we would do that, and then Dick... And Dick started looking around at Brookhaven and found that a man named Ed Fireman was interested in doing that, too, and then the more they talked, the more they found out that they were not going to be collaborators. That just wasn't going to work. So where it ended up was that I was going out there, instead of working in the chemistry department I would be working in the physics department with Ed Fireman. And so the... We [went on?] to have a target, a cosmotron target to shoot high energy protons into. The cosmotron target was nine inches long with a cylinder that was about, oh, an inch and a half, maybe two inches, an inch and a half, probably, and would be filled with water so we could measure the trillium production [for?] oxygen, and then later put in ammonium hydroxide and do nitrogen. And eventually, what we did was basically we actually, in the end, did the experiment, but along the way,

what one needed to do was to have a technique for measuring... Well, my contribution to this was -- it was going to be a water target, and my contribution as a hot atom chemist is you can't just look at the water, you have to look at the hydrogen, as well. So when we looked at the hydrogen, we found that there was, 10% of the trillium was actually in the hydrogen. But that is a very small amount of hydrogen, lots of water, and the T to H ratio in the water needed a very sensitive detection scheme, and that detection scheme was a cloud chamber with alcohol diffusing through it, giving you cloud where... When you watch for -- it was a visual detection of trillium, which is that there's a magnetic field involved, so the trillium makes a tiny circle, maybe while the cosmic rays are all shooting through it like this, and you just watch for those, that... And in the end, this was being done by Don Schwartzer, who was a technician, and by me, and by Ed, and Ray Davis made us a [trilliated?] water sample to calibrate it with. Our efficiency was about 2/3rds, which (laughter) you'd sort of like it a little better if it were 100%, but in any event, while we were doing that we got hold of a sample of atmospheric hydrogen from the (inaudible) products, place in Buffalo, the liquid oxygen, liquid air, and proceeded to measure the trillium content. It was about 5 x 10^-15 as the ratio of T to H. Then we heard that people at Columbia, whom Wally Broker was low man on the totem pole at that time, that they had a sample of hydrogen, as well, and so we got it, they sent us some of that, we got that and measured that, and they, their sample was from 1952 and ours was from 1949, and they didn't agree, and so we wrote it up. And at that time, Physical Review was published out of, was edited out of physics department in Brookhaven; Sam [Goldsmith?] was the editor. And so Ed Fireman just walked it down the hall, and they looked at it and came back and said, "If you withdraw the paper, then that's fine. If you insist on trying to publish it, it will be classified." And the reason being that this is the summer of 1953. Nobody has exposure to the hydrogen bomb yet, and this would give some information about that. Maybe some people were working on it, and again, this was all aural to Ed and back to me, and that if you believe both of 'em, you would think that someone had put a lot of trillium into the atmosphere between 1949 and 1952, and that there'd been an accident at Hampford at which someone had put (laughter) a lot of trillium into the atmosphere. So that publication -- because the Columbia people only put up their one number, theirs was published in 1954, and because we had two numbers, ours was published in 1961! (laughter) And meanwhile, Ed Fireman and I went on and measured the cross-section of trillium production from oxygen and then from nitrogen, and published that in Physical Review. But then I went back to -- at one point, having fooled around with some other, with Fireman, we looked at the, thought about we really ought to do trillium in a way where you've got a lot of it. And I went down to -- talking to Dick Wolfgang -- and I went to... This is 1953. The housing allowance wasn't, there wasn't any housing allowance. Joan and I were living as guests at the Wolfgang's. We had a room in their house, and so Dick and I were talking all the time, and when I came down from the physics department saying, "Really, they looked at the trillium, T plus water gives HT and it gives HTO, and this, maybe trillium would be a good thing to look at," and he pulls out a sheet of paper of Libby-like ideas, and it says "Lithium 6 and helium

3." And so we thought, well, maybe we should do that experiment, and this was late in the summer, in January of '54, between semesters at Princeton. Joan and I and Ingrid went out to Brookhaven for two weeks and worked between semesters, and so Dick and I worked together on trying to alter sonicate benzene, I think it was, and water. At any rate, we were thinking about some way of producing trillium, and it didn't work. And at the end of that period of time, I got a mortar and pestle and put lithium carbonate and glucose in, ground it up, and irradiated it, and then took it back to Princeton with me. And so that spring, I went around the organic group there and looking for somebody that could help me with the sugar, and then found a post-doctoral named Nigel [Turten?]. There was a sugar chemist named Eugene [Poxin?], but so I looked around and there was this British post-doctoral, Nigel [Turten?], and I asked him about it and he immediately had a number of suggestions about exactly what could be done, and yes, he could do that. It wasn't until we were much, much further into this that I happened to look at Nigel's Ph.D. thesis, which was on the measuring the carbon-14 distribution in glucose! (laughter) So he did all of the separations, I did all of the counting, and found that first that the glucose was radioactive and about 12% of the trillium had ended up substituting as glucose, and then when Nigel started doing (inaudible), it was, he was still working on it. The timing was the first that simply that there was trillium that was actually glucose, and that, the first time I counted it, it was counting trillium out of a solid, which since it has no range -- but then I had gotten a counter that you could fill with it and put a proportional counter-gas in. And I showed up at Brookhaven in June, I guess, of '54 and showed Dick the results that crystallized to constant activity. I know when we first showed it to anyone, we had the measurements that weren't in a gas counter, and the gas counter would give much greater precision, but it was very clear you re-crystallize to constant activity and so on, and we submitted a paper to the American Chemical, not a paper but an abstract to the American Chemical Society, I think summer of '54. Well, that ended up, when we wrote a paper on it, was the lead article in Science, and that then led to... There was [this important?] conference in the summer of '54, and Dan Miller from the AEC came to me and said they would like to support our work, and so I went back to the Chair at Princeton, Robert [Pease?], and said that the AEC had approached me and said they would like to have a contract with me. And his response was "You're too young." That was, so that was the fall of 1954, and that ended it. Well, that ended it for that time period. I was not a great enthusiast for the Princeton Chemistry Department, even before that, and so although I was associated with Princeton, my activities were, generally speaking, (inaudible) Brookhaven. At least, my allocation of money from Princeton in support of research was about $30 a month, but I knew that if I went to... Meanwhile, Brookhaven had an open stockroom, so if I needed anything I could always get it at Brookhaven, and we did some experiments at Brookhaven, radiations there. I remember carrying a, we had a radiated... Well, it was probably a lithium target. At any rate, we carried it in a liquid nitrogen [dower?] on the Long Island Railroad and then the Pennsylvania Railroad back to Princeton to do the analysis on it. It was eradiated lithium chloride, which we then... We [tried?] to have a lot of trillium in who knows what form and

dissolved it in ethyl alcohol and looked to see where it went. There was some acetone formation, and it was something that was tantalizing but we never followed it up because we found that we could do the kind of experiment, so I had... [Wilfred Hoff?] was a graduate student and an Army officer who did trillium in some species, did it in [elanine?] to look at the substitution of T for H at an asymmetric carbon (inaudible). So all of that started out, but the contract with AEC, I checked with... In the third year that I was there, I decided that I would look for a job in the Midwest, and wrote to each of the Big Ten universities, and found out that's not the way to get a job! (laughter) So then Ralph Adams, who was an analytical chemist, he got his Ph.D. at Princeton -- he was a New Jersey person, not really a Princeton type -- he was a flyboy, flew in the Philippines during World War II, and Ralph was... Then he stayed on as an instructor, so we were instructors at the same time. Joan and Jenny were pregnant at the same time, pushed babies together in the baby buggies. And then he was looking into a job at the University of Kansas, and so I knew when he went that he wasn't interested, and when he came back things had changed. He said that if they made an offer, he was going to take it, and they made an offer and he took it. So that was the fall of '55, and he had been there two months or something like this, and a physical chemist named Robert Taft dropped dead. And as it turns out, there's a relation. One of the first people I hired here was an organic chemist named Robert Taft, the son of the one at the University of Kansas. There's an interesting comment on -- when I mentioned to my older brother, who by that time was actually living in Lawrenceville, near Princeton, when I mentioned to him that I was interested in a position that had been left by the death of Robert Taft, my brother knew who he was, not because he was a chemist but because he had published histories of the Old West. So that, I would say this is interesting (inaudible) because Robert Taft, Sr. was, from the standpoint of the chemistry department at Kansas, was somebody who was occupying the position and not doing [much?], teaching chemistry but he wasn't doing chemical research or very much of it. But his -- he'd started out being curious about the chemistry of the photographic process, and then about...

END OF TAPE 3, SIDE 1

Interview of F. Sherwood Rowland

TAPE 3, SIDE 2

Gaffney: OK, so you were talking about the photographic process and his interest...

Rowland: Yes, then he became interested in the daguerreotypes in the Old West, and then he started lecturing about the Old West and about Dodge City and the various trails, and suddenly was in demand as a lecturer. And the chemists weren't happy about the fact he wasn't doing chemistry, the historians were unhappy about the fact that he was doing history and was very successful at it and getting a lot of acclaim and didn't have historical training.

Gaffney: And your brother had heard about him! (laughter)

Rowland: And my brother knew about him from that point of view. So it just says, it emphasizes how compartmentalized our universities are that somebody -- it would be very easy to argue that he was serving a very good educational purpose, but it wasn't the narrow educational purpose of the two groups that were impinged upon.

Gaffney: So you joined the University of Kansas.

Rowland: So I went to KU, and I arrived there on June 1st, 1956, and that's the date that my AEC contract started, and that contract ran for 38 years, which is remarkable, long-lived support, where through essentially all of this to the very end... You knew that you were going to be supported the next year, you were pretty sure you were going to be supported for the next three or four, so you could commit to a post-doctoral and not worry about it... And when I say it ran out at the end, it ran out because most of our money was then coming from NASA, but actually I think I... I didn't write a very good proposal. One other criticism was that they said, the reviewer said they couldn't possibly do all of those things, and I hadn't mentioned that I had just gotten a private grant of $1,000,000. (laughter) So I wasn't worried about how I was going to employ people.

Gaffney: So you did a lot of very successful work with trillium and tracers, and I know that's led to developments in nuclear medicine, all sorts of places. I know you applied these to photochemistry, and I know then you got interested in photochemistry. So what kind of led you to changing fields?

Rowland: One of the great influences -- if you were asked who were the people that influenced me most in career choices and so on, I would say Libby, Libby, and Libby, and he had indicated that it's a good idea to be exploring new fields every seven to ten years, and I had taken that to heart. And so the [recall?] trillium was going very well in the late '60s, late '50s, and then since we were doing good radio

gas chromatography that we could apply that to some photochemistry. For instance, instead of getting trillium atoms with 192,000 electron volts from neutrons on helium-3, you could get them with three or four electron volts by photolizing the ultraviolet [photolysive?] TBR and could measure... And with that we're able, for instance, to measure that the activation energy for the substitution process, trillium plus CH4 to give CH3T+H was around one and a half electron volts. If you had 1.8 electron volts, you got a little bit of -- you got a lot of HD, a little bit of CH3T. If you went up to three electron volts energy, then you got a lot more HT, no, sorry, CH3T. And this put us into the photolysis business, but photolysis with radiochemistry, try doing tracers on it. I won't go into the complicated details of one experiment that we did that involved the use of radioactive ketene as a source for radioactive ethylene, and we did, oh, eight or ten different types of experiments, and I don't think any of them worked out the way we expected or the way that you would've expected from the literature. One quick illustration is if we made C14CH2 and then CO, that is ketene with the C14 in the CH2 group, and photolyzed it, a small percentage came out as carbon-14 monoxide, but the carbon-14 had gotten into the other position, and that implied that there was an intermediate in which the carbons were equivalent. Well, that was not part of the literature at the time that we were doing that. We also had the experiment that, in that same thing, if you had CH2 then reacts with CO to give a product out of that. The surprising thing is that if you had two trilliums on that molecule, so CT2CO, that you got T2. That was totally unexpected, and it was even more unexpected in that we didn't know we had CT2 to begin with 'til we started examining. A question was we had diluted the (cough), purchased the (inaudible) hydride that had tritium in it, but the amount of T2 was 100 times as much what you would calculate, but then you found out that they had, that New England Nuclear had diluted it with non-active material after their synthesis by a factor of ten, and we'd done that, too. So that explained it, that was a tracer that showed up. And that says more other things, that if you've gotten far enough out away from, let's say, the well understood things, then you start getting strange signals and you ought to be thinking about, you ought to follow those up. Sometimes they're false signals that can mislead you, but the full explanation often makes things more difficult but more interesting, and so in that sense Libby, if I were just thinking of the Libby suggestion about putting in photolysis to the nuclear -- I should say the period of time that I was in Kansas, '56-'64, we were doing some things which I would say were geophysics, that is the chemical state of chlorine, radioactive chlorine produced in a chlorinated mineral, or the measurement of the amount of uranium in a mineral by activation analysis producing an isotope of xenon. So just out of commentary, there was a time in 1962 when my family and I had been on sabbatical in Germany, and we were going to finish the summer, finish -- we were moving to England for the summer, but we stopped at a meeting in Utrecht that was essentially a geophysical atmospheric related time, and about that time I was actually on the review, the board, the review board of Journal of Geophysical Research as one of the editorial board, so I had been... There was a more senior editor based at the University of Kansas, knew what I was doing, wanted someone with some radioactive to be on

the editorial board, so it might seem a little strange to find me there for some of the geophysicists, but it said that my interests included that.

Gaffney: So '64, you get the offer to come to, or '65 or so, to come to a brand new school, right?

Rowland: Right, I came here in '64, and there were... But '63, '64, I think [there before?] I was a Chancellor, a Vice Chancellor, one Dean of the Graduate School, and that was it. And then '64, '65 there were about 20 of us, each of whom was a department chair and whose job was to have a functioning department in '65, and that meant for me hiring, well, having an initial goal to hire five additional chemists, and that was augmented -- when the engineering got off to a slow start, each of the schools and physical sciences were given another position to fill, so my main job that year was to recruit six new chemists. The [trial?] that I made that didn't work was to try to persuade George Porter to come from England. He was then at Sheffield, and I thought it was clear that Porter would not remain at Sheffield forever, and there was an opening at Cambridge, and I thought, well, if Porter didn't get the Cambridge job then he might be tempted to move to the United States. He didn't get the Cambridge job, but he became the professor job at the Royal Institution, which was a perfect position for George 'cause that included Christmas lectures and other aspects that went with it. It went back to, oh, 150, 200 years of history behind it, so he was an ideal candidate for that.

Gaffney: So about this time now, obviously you continue some of the work on radiochemistry, and you're doing photochemistry, so this leads you to atmospheric chemistry. You've been to some of the geophysical things. When did you first get the idea about the course, now famous, so what happens to chlorofluorocarbons when you release them in the atmosphere?

Rowland: Oh, the starting point for this is when in the year 1970 -- this was my sixth year as the department chair and I decided that that was enough -- and so ask the dean of the graduate school for a grant of money to send me to an IAEA meeting in Salzburg on the environmental applications of radioactivity, and so I went to that and listened to it and had some ideas. And after the conference was over -- I had been at the IAEA for six months in 1969. I went back there, I think, I believe I was on a ticket that required I need to stay 17 days total, so at least I had another weekend to go, so I went back to Vienna on a trip from Salzburg, where the IAEA meeting is held, to Vienna. I shared a compartment with a person from the Department of Energy. He found out that I was -- by that time I had been supported by DOE for 14 years, and in conversation I waxed enthusiastic about carbon-14, about Libby's experiments, and so on. And so he found that I had my 14 years of NEC experience, and from that standpoint was an ideal candidate for inviting, for an invitation to the newly created workshops on chemistry, meteorology workshops. There was one coming up in a couple of months, but the next year I was invited to the one that was in 1972, and at that meeting I heard discussion of the work being done by Jim Lublock, who had invented the gas

chromatogram, invented the electron capture detector, and basically for trichlorofluoromethane, that detector sort of increased the sensitivity by somewhere between 10,000 and 1,000,000. It changed CFCs from something that was undetectable to being easily detectable, and that he found it everywhere, on a cruise from England to South America, to South America and on to Antarctica. Well, that was February of '72. I remember discussing with a couple of people [Dieter A. Holmes?] at the meeting, well, that was an interesting aspect, and [letting it go about it?], but then the following year when I was writing my proposal for the Atomic Energy Commission, I included that as a one-page add-on to investigate what would happen to the CFCs. The ozone was not mentioned. It was simply -- sort of, you think about it afterward, it was applying the cradle to the grave technique for a chemical compound at a time when that wasn't anything you do. Certainly, Lovelock had said there's no possible damage from that, but it might be useful as a tracer, and...

Gaffney: Yeah, I recall the Department of Energy was going to release large amounts of (laughter) these tracers, if I recall, from some of those workshops!

Rowland: Some of those substances, yeah. So...

Gaffney: So I remember, I think, the first time I actually met you, if I recall, was at the University of California, Riverside, and not too long after that, those meetings in your proposal, and if I recall it was, I think, you and [Lauren Molina?] discussed your work about the CFCs, and Professor Jim Pitts' research group at the [statewide?] Air Pollution Research Center. So I don't know if you recall that visit...?

Rowland: I went to Riverside two or three times there. I think it was probably... Jim and I and two other people had spoken at an ACS meeting in fall of '74. I do remember going to Riverside -- I guess it would've been '76, because that trip I was accompanied by a photographer from People Magazine, and aside from the fact that they did use a picture, he had followed me around for a day and a half and took something like 700 or 800 pictures, of which they used five, as I got some other copies later on. But we had dinner with the Riverside people, and there was, suddenly there was a recognition of another person at the restaurant, and they called to him, and he was a photographer familiar to Riverside people, familiar for one of the papers who was called One Shot Benson! (laughter) He would take one picture and that was it, and here I was being accompanied by a person who was shooting a whole roll every couple of hours! Now, that's no longer unusual. Now, because the digital photography and all of the capacity, it's not unusual for somebody to shoot 30 or 50 shots just waiting for the right... But anyhow, I was at Riverside at that time, but I don't have an accurate record. I may could be, could reconstruct them.

Gaffney: Yeah, I just... It's something that I recall, and I just remember talking to you. In fact, I remember you telling me something about, you were talking about

other interest in environmental things, and you had talked to me about doing mercury measurements in tuna fish or something if I recall. (laughter)

Rowland: We did! That was the first thing, that first we felt going to the meeting in Salzburg on environmental applications and radioactivity was saying (inaudible) analysis for mercury is as simple a technique as you can imagine. The sensitivity, the amount of mercury in typical fish is in the order of part per million, and the sensitivity is easily to part per trillion.

Gaffney: Now, see, as I recall, after you'd come and lectured a little bit about this, I think it was just before the science paper actually went out –

Rowland: The science paper on mercury.

Gaffney: No, on the CFC work. It was actually -- 'cause I was in school up 'til '75.

Rowland: Well, the CFC work was published in '74.

Gaffney: '74, right, so this was I think either early '74 or...

Rowland: No, there were no talks.

Gaffney: No, this was just a private thing, that you'd come to visit our group. So anyway, I don't remember, but anyway, I do recall Barbara (inaudible) and myself having lunch with you as graduate students and talking to you about tuna fish, and as I recall we were eating tuna fish at the time! (laughter)

Rowland: Oh, I'm sure that it was not the spring of '74 'cause I was living in Europe.

Gaffney: It may have been '73, I don't know.

Rowland: No, it couldn't have been '73 'cause we hadn't found it out –

Gaffney: Maybe it was in '75.

Rowland: '75 would work out.

Gaffney: So one of these days I'll have to check my numbers. But anyway, it was -- I just recall it was such a pleasure to meet you then and talk to you. OK, so the paper in '74 comes out in Science –

Rowland: Nature.

Gaffney: -- Nature, pardon me, and the CFC theory leads to a significant amount of controversy. Can you talk about that and the impact it had on you and your family?

Rowland: Well, let me put it... One aspect of it: If you examine the list of my collaborators from 1975 on and look at the post-docs that are involved, they turn out to be Japanese or German or something. There were no Americans. No American applied for the post-doc with me, from then until about 1988, except somebody in the research group staying on or something of that sort, nor were there invitations to speak at chemistry departments until about 1988, and then that's after the Montreal protocol and after Dupont said no, that they weren't going to continue manufacturing. But because I had -- the AEC then became DOE, because I had that kind of support, I had a steady stream of very outstanding post-docs coming from the University of Tokyo. I should say in terms of -- that in the near scientific vicinity, for the period of, say, 1974-1988, the near scientific vicinity, the closer the people got to it, the more they realized that this was not a hoax or a flimsily constructed hypothesis, that there was a lot of science behind it, and the further you got away from it, the easier it to have people say, "This is ridiculous, they're heavier than air, they can't even make it up." And one of the interesting things is that almost none of those arguments have ever disappeared. That is, if you put in "chlorofluorocarbon ozone hoax" you get a lot of responses.

Gaffney: Yeah, I require one, I remember one book that I was asked to look at, and I think contacted you about the... Was it *The Holes in the Ozone Scare* or something by a couple of fellows named Maduro and Schauerhammer or something like that? (laughter)

Rowland: Right. Maduro and Schauerhammer were... Maduro wrote, and I think still does, for 21st Century Science and Technology, which isn't quite as... Well, in 1975 that title implied the future. It doesn't imply the future anymore. But this is a Lyndon LaRouche political aspect, and one of the characteristics -- well, one example is the Nobel Prize speeches by Melena and Goodson and myself were picketed by the Schuller Institute, which was an adjunct of the Lyndon LaRouche people, and handing out pamphlets about it, Nobel spinning in his grave and analogies to Liscenco and biology under the Soviet Union.

Gaffney: In my recollection, I think there was a number of us that had looked at what you'd done and really realized just how significant it was, and to me one of the things that happened was a real focus on stratospheric chemistry, which had not really happened before.

Rowland: I have some comments about that that are observations about atmospheric science that probably would apply to, let's say, up to 1950 or thereabouts. The full elaboration of the chemistry of the atmosphere really was broken open by things like the electron capture detector, but I remember as a graduate student discussions of the radiation chemistry of ethane and the question of -- the interpretation of that is breaking a carbon-hydrogen bond, but the measurements did, were... Once the measurements moved from about sensitivity of a part and 10^4 to a part and 10^6 or 7, one realized that soon after you started the radiolysis ethylene was present, but ethylene couldn't build up because hydrogen atoms

were present and kept knocking it off, and so it was just a question of, there of increasing the sensitivity by a factor of 100, and the mechanism, became obvious that there was a different mechanism involved. And if your sensitivity is 10^8, one part and 10^8, there are 14 [compounds?] in the atmosphere. Five of them are rare gases, noble gases, and then there are a few of others, and until you started measuring... There are two things that changed it: One is the measurement of sensitivity that you can see dozens of molecules present, and the other is the question of precision...

END OF TAPE 3 SIDE 2

Interview of F. Sherwood Rowland

TAPE 4, SIDE 1

Gaffney: OK, so you'd mentioned that it was really important to realize the importance of being able to measure things at high sensitivity and precision.

Rowland: High sensitivity and/or high precision. Methane and CO2 measurements to show that there was an increase in the concentration in the atmosphere. It was probably ten years after Dave Keeling started measuring carbon dioxide before people began to accept that carbon dioxide was increasing, because the question -- they were still sort of looking for cycles that would appear as to whatever it is that's controlling it, and, of course, they did see, right away saw a yearly cycle with the downturn in carbon dioxide with photosynthesis in the Northern Hemisphere, and so... And methane, there were scattered measurements from a variety of people prior to '78, and then around '79, '80, '81 it became clear that it was growing up, but even then the question of cyclical behavior on methane was not totally eliminated. And then, of course, what has happened in the 30 years, 29 years that we've been monitoring it is that the 1% increase a year of the 1980s has faded into no change in the last six or eight years, the concentrations. So the question of why methane is not, is no longer increasing -- of course, that's the mirror to why was it increasing before, and there were a number of suggestions that (inaudible) rice patties being, you flooded more than once a year, and the question of the number of cattle and so on, going up, doubling since 1950. Those were things which could be were plausible explanations for the increase, but establishing what amount of the increase belonged to each of those was something that was left undone because you just didn't have enough... If you're going to measure the methane given off by cows, then you have some question as to how you apply measurements made, let's say, in Southern California to cows in Uganda, and that cows are all over the world with maybe 10% in any one major country, so you need an awful lot of experiments to demonstrate that there is a categorical cow emission. I can say in a lecture half a pound of methane a day per cow, well, average (laughter), but... It wasn't until 10,000 cows had been mentioned, let alone 10,000 scattered all over the world... Well, having participated in some of this allowed me to, also to see some of the problems or some of the areas in which chemists come in built in with a different attitude than a meteorologist does, and if I sort of summarize it, from my point of view, the typical chemistry student is involved with experiments in which, to simplify it, A is converted to B, and starting at point 0 and ending six hours later with a measurement every half hour, every half hour, and you determine whether it goes as the square of A or A to B, and that by and large doesn't happen in meteorology. So the chemist learns that there is a mechanism and it has a start and it has a stop, and you'll always look for it. The meteorologist, on the other hand, can't start and stop. They always are coming in on something that's going on. It usually has, instead of having A reaction with B at A, B, C, D, and E, there are all these things

going on together, and so you start looking for the changes, and you come up with cycles, and there is no starting point and no end point. And so when you come to chemistry, you say, well, there isn't any starting point or end point to CFCs, and we say, well, no, there is a starting point, it's when they were put into the atmosphere. They say, "Oh yeah, but it's put in all over the world and it cannot be destroyed all over the world." I say, well, that's true, but it's in the stratosphere at altitudes above 25 kilometers, depending on the sunlight, and that is reasonably, that's something that you can reproduce in an experiment. But still, I remember hearing a meteorologist at one point I had complete respect for just say, "They, in their experience, don't have a start and a stop. They don't have a single cause." And of course, I came in with the experience that there is a single cause is that CFCs are put into the atmosphere starting at a certain time, and they go away in the stratosphere at a certain altitude. Well, really, the only real exception that I have for this is that where there's an overlap between the chemist looking for the starting point is when a volcano goes off and you put sulfur dioxide in the atmosphere, and you can follow that, and ten years later the signal that you've gotten has disappeared, and that was a starting point and that had a [particular?] starting point, and you could work in the meteorology aspects that fit into it afterward. But this does say that you can also think of some of the hydrogen bomb tests that had specific labels on them where the neutron, the intense neutron flux in the hydrogen bomb on some selected isotopes, stable isotopes, it converted them into a radioisotope that you knew when it was formed and you knew where it was formed, and you could follow it for ten years afterward. Well, that kind of experiment, it's... You couldn't have done it with CFCs until you could detect them, and detect them with reasonable precision. We have measurements now that say that the... Would it do a spiral with the DC8 or something like this and collect samples at various altitudes, and where they cross-sect, the concentration of (inaudible) 11, for instance, is the same plus or minus a half a percent from 2,000 feet to 30,000 feet, then not the concentration but the low fraction, and you just know that it's been mixed so that by now, since CFCs are hardly going into the atmosphere, if you have the up to date information you could say, something close to this, the concentration of fluorocarbon-11 is in the range of 230, 240, somewhere, parts per trillion, and you say where everywhere, everywhere in the troposphere. At any given time, so much mixing is going on now that it's rare to see a deviation of 2%.

Gaffney: Sort of a follow-up on that, one of the people that's I think helped working with you for years is Don Blake. I just thought I'd ask you to tell us a little bit about Don and what he's meant to you and your group, looking at these kind of traces.

Rowland: Don is a bear for precision, and I'd say almost as much for sensitivity, but recognize that if the greater precision that you have then the more likelihood you can find out, you get information out of an experiment, but you know, constant pushing to get more... Right at this particular time, we have an analytical regime that we use for atmospheric samples that is based on what you find in the remote

atmosphere and in what you find in cities. The things which are missing from us, from our analysis, you see, for instance, by going to a dairy farm, and at a dairy farm there are a lot of nitrogenous compounds, including reduced nitrogen compounds, and those just don't survive in the remote atmosphere, and so they disappear very soon if you move away geographically from the dairy farm, but if you stay with the dairy farm... And now we're trying to calibrate for the peaks that we have seen that we don't, that don't fit our previous pattern.

Gaffney: I think a lot of the work you've been doing in the recent years ties in, and you mentioned the regional and global scales and specific sources and things. I just wanted to get your comments on how global society is going to deal with some of the global climate change issues. The CFC issue to me is one of the great... what I want to say is basically blessings we've had of an actual global agreement and recognition of a potential problem that could cause, you know, tremendous damage and problems. Right now I see us facing the real issue with things like all the greenhouse gases, and would point out that even the CFCs, as your work, now there's (inaudible) greenhouse gases. I just wondered if you could comment a little bit about the difficulties that, you know, do you think we'll get past these difficulties (laughter) that we've had. Are there always going to be these people that are always naysayers?

Rowland: I think there will always be the naysayers. If they happen to own an oil company, it gives them a very large (inaudible) compared to other people, and so it is, it will be a hard road ahead, I think, to control carbon dioxide at... Right now at this time we see people plausibly arguing for 450 parts per million cap, which is almost impossible to prevent. We're going up about 2 parts per million per year, and we're, say, 382 or so right now. Well, 68 parts per million would be 34 years, and I think I would be reluctant to... If I were to hazard a guess, I would think we probably will hit 450 within 40 years. I just don't see the way that a generation will be enough to put the brakes on, certainly requires using all kinds of alternative energy and looking very closely at sequestration, and it's clear that some places sequestration has been made to work, but so far the ones that I hear about are oil -- not oil but natural gas drilling that comes up with a mixture of, let's say, of methane and CO2, and separates the two, which when it's methane and CO2, not methane and CO2 and air, diluted in air, separation is relatively easy to do, and then putting the CO2 back where the whole, everything started from, probably easy to do, as well, except the question of guaranteeing that's going to stay there for 500 years. That's a different question.

Gaffney: So I know you shared the environmental achievement prize, the Tyler Prize in '83, which preceded the Nobel Prize, with Dr. Molina and Harold Johnston, and then you'd also shared the prize with Dr. Crutzen, and you mention Jim Lovelock and some other people. There's other people besides Johnston and Crutzen and Lovelock that influenced you, or would you like to comment on that influence or your thoughts about some of the work on the CFCs?

Rowland: Oh, the question is... Certainly, well, the analytical aspects of looking at the atmosphere by people like Susan Solomon where putting the chemical molecules into theories and making, waiting until you found some way that the theory, beginning to match what's being observed. The high precision measurements that Jim Henderson made over Antarctica under conditions that, let's say, were daunting conditions for even making the measurement, let alone having it come out in such a definitive fashion, there've been lots of people... One of the things as a scientist, Mario and I had the luxury for almost a year of having a problem all to ourselves. Now it's very hard to find something about that particular problem that isn't being, isn't a Ph.D. thesis for someone that's just completing it! (laughter)

Gaffney: One of the things I've also noted a lot now is that your work is really highlighted in a lot of the educational textbooks, and I know that my undergraduate students and some of my graduate students, of course, and I personally have always considered you a hero (laughter) for, you know, basically [hanging?]... Did you have heroes? I know you mentioned Will Libby. I wondered if you had any other heroes?

Rowland: Any other scientific heroes?

Gaffney: Yeah, scientific heroes.

Rowland: Ferebee was clearly the... The way I would describe it is that there aren't very many people that don't have to worry about the standing of their institution as far as it affects their... Ferebee didn't have to worry. If Ferebee were there, it was one of the leading institutions in the world, period, and that was basically... True problem but somewhat less with Libby, but Ferebee was sort of your all-purpose physicist at the time, and well, I think I'll leave it at that.

Gaffney: You've been very active, also, in scientific societies, as well as the American Association of the Advancement of Science, having served as the President in '93, ACS, AGU, National Academy of Sciences. Do you have any interesting stories to tell us about some of your experiences being an officer in those groups?

Rowland: I can raise some questions... When we, meaning the AAAS, built a new building to replace the rented building that we had around 1990, this came about during the time period while I was going through the rotation of President Elect, President, Chairman of the Board, and so when I was Chair of the Board we had the opportunity of choosing the architect for that building, and for that building he employed as an associate architect a person whose specialty was environmental aspects of architecture. I would like to see, now we have -- that building was erected so that it was up around '95 or '96, so we have 12 or so years of experience. I would wonder what worked, what didn't work, and how the things that did work are being transmitted into the architectural community, because if

you look ahead 40 or 50 years, most of the major buildings of the year 2050 or so are going to be built, are not yet built, and so it will be important as you try to do something about carbon dioxide to build them with environmental requirements involved in it. The experience I had with the being (inaudible) Secretary of the National Academy of Sciences made me realize how universal the scientific education has been, that there was a time when there were 15 of us meeting as, well, essentially each representing a country, and we were meeting in a meeting which was an adjunct meeting to Davos in Switzerland, and the conclusion that I came to, listening, and then -- which I said at the time -- was that I thought that if you had recorded those conversation and then passed it through a filter that took out accents and took out the knowledge of the particular language that would be disclosed by the way, sort of translated it into a neutral language, and then were told who was there and asked to match up the speaker and the speech, that I thought you would have a great difficulty of telling whether it was the Chinese or the Turk or somebody from Europe or the United States, which, because that there was a lot of parallelism in the educational system, but seemingly very different by where... Of course, by that time most of the people that were present there had been associated in one way, either with the North American or European scientific education, but they were now spread all over the world.

Gaffney: So I sense now that we have the latest IPCC report come out that 90% of the scientists in the Silver 2000 of them across the world would be support of that statement, and so I was going to ask you one question, I guess, and that is you said you could've worked possibly with Fermi at Chicago... (laughter) Is there something, if you were to do it all over again, would you have done anything different? Would you have gone into another field besides chemistry? I guess I'm hearing not, but I'm just curious if there was anything that you ever wondered about, gee, if I'd walked down a different path what might have happened?

Rowland: I don't think that there's any different path, but I think there were enough sort of random events that influence everybody's path that it doesn't, that even if you tried to run through the same way you'd get diverted, something else would've happened, and I can imagine there was a time when my older brother and I -- I had pointed out to him a particular scientist at Brookhaven that I thought was good for a major national story, man named Nicholas [Hristopoulos?], who had developed the principle of the alternating gradient (inaudible) while he was working at an elevator company in Greece, and patented it, and when the Brookhaven people came up with the alternating gradient (inaudible), [Hristopoulos?] got on the ship or flew, went to Brookhaven, and said "I've got a patent on this." And when they examined it they finally got through -- he had invented some things on the way, different notations and so on. When they looked at it they agreed that he had. I think what happened was that they, they certainly... He didn't go back to working at the elevator company in Greece, he worked in AEC after that, and on a variety of really off the wall ideas that were worth exploring. Well, if that story had been accepted by [Collier's?] and we got as far enough to talk to a senior editor, but it didn't fly because nobody, because it

didn't have -- it's economic impact was not very strong because there was no market for dozens of these instruments, and so this was sort of an abstruse scientific story, but my brother was then operating as a freelance writer, and if that one had succeeded I think he would've bugged me about other scientific subjects. (laughter)

Gaffney: He might've ended up a consultant for the [colors?]. Before we end this interview, is there anything else you'd like to add?

Rowland: I probably will -- when I see the transcript, I will...

Gaffney: You want to (laughter) take a [shot at?]... Well, I think if that's the case, I really want to thank you for your time and answers, and this interview is now completed.

Rowland: Thank you, Jeff.

END OF INTERVIEW