THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

E. BRIGHT WILSON, JR.

Transcript of an Interview
Conducted by

Raymond C. Ferguson

at

Harvard University, Cambridge, Massachusetts

on

17 and 18 November 1986

With Subsequent Corrections and Additions

E. Bright Wilson
JH
3/15/96

CKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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

E. BRIGHT WILSON, JR.

1908	Born in Gallatin, Tennessee on 18 December

Education

1930	B.S., chemistry, Princeton University
1931	M.A., Princeton University
1933	Ph.D., physical chemistry, California Institute of Technology

Professional Experience

1933-1934	Research Fellow, California Institute of Technology
	Harvard University
1934-1936	Junior Fellow, Society of Fellows
1936-1939	Assistant Professor
1939-1946	Associate Professor
1946-1979	Professor
1947-1979	Theodore William Richards Professor of Chemistry
1979-	Professor Emeritus
1942-1944	Research Director, Underwater Explosives Research Laboratory, Woods Hole
1944-1946	Chief, Division 2, National Defense Research Committee
1952-1953	Weapons System Evaluation Group, Department of Defense

Honors

1937	American Chemical Society Award in Pure Chemistry
1942	Honorary M.A., Harvard University
1948	Medal for Merit, United States Government
1949-1950	Guggenheim Fellow
1962	Debye Award in Physical Chemistry, American Chemical Society
1966	Alumni Distinguished Service Award, California Institute of Technology
1966	James Flack Norris Award in Teaching of Chemistry, Northeast Section, American Chemical Society
1969	G. N. Lewis Award, California Section, American Chemical Society
1970-1971	Guggenheim Fellow
1972	Pauling Award, Oregon and Puget Sound Section, American Chemical Society
1973	Rumford Medal, American Academy of Arts and Sciences
1975	D. honoris causa, Free University of Brussels

1976	Honorary D.Sc. degree, Dickinson College
1976	Dr. chemistry, University of Bologna
1976	National Medal of Science
1976	Antonio Feltrinelli Award, Rome, Accademia Nazionale dei Lincei
1977	Monie A. Ferst Award, Sigma Xi
1978	Pittsburgh Spectroscopy Award
1978	T. W. Richards Medal, Northeast Section, American Chemical Society
1978	Robert A. Welch Award
1978	Earl A. Plyler Award, American Physical Society
1979	Honorary D.Sc. degree, Columbia University
1979	Willard Gibbs Award
1979	Lippincott Medal
1981	Honorary D.Sc. degree, Princeton University
1983	Honorary D.Sc. degree, Clarkson College
1983	Honorary D.Sc. degree, Harvard Universit

ABSTRACT

E. Bright Wilson, Jr. begins the interview with a description of his parents, childhood, and early education. He then discusses his undergraduate and graduate studies at Princeton University, where he was inspired by the intellectual atmosphere and affable faculty. After reviewing the curriculum, his senior thesis on quantum mechanics, and his experience at Tuxedo Park, he recalls his years at the California Institute of Technology, where he began work with vibration and group theory. Next, he describes his work at Harvard, focusing on advances in spectroscopy, and his government research at Woods Hole and in Washington, D.C. Wilson concludes with a brief profile of his family and a few remarks on his publications.

INTERVIEWER

Raymond C. Ferguson obtained his degrees in chemistry from Iowa State University (B.S., M.S.) and Harvard University (Ph.D.). He worked in research divisions of the Organic Chemicals, Elastomer Chemicals, and Central Research Departments of Du Pont, principally in molecular spectroscopy, organic structure analysis, and polymer characterization. Currently he is affiliated with CONDUX, Inc., a consulting association of ex-Du Pont professional

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INTERVIEWER: **Raymond C. Ferguson**

LOCATION: **Harvard University, Cambridge, Massachusetts**

DATE: **17 November 1986**

FERGUSON: Professor Wilson, we have background biographical information about you already, and I rely heavily on [George B.] Kistiakowsky's appreciation in the "Festschrift" issue of the Journal of Physical Chemistry (1). I want to fill in some of the details, particularly about your family. Your father was a lawyer from Gallatin, Tennessee, is that right?

WILSON: He was not born in Gallatin, but he did start out there. He was a lawyer and practiced eventually in New York.

FERGUSON: In New York City?

WILSON: Yes.

FERGUSON: That was before you were born?

WILSON: While I was young.

FERGUSON: Where did he get his law degree?

WILSON: He studied with an uncle in Tennessee who was a judge. In those days you sort of served an apprenticeship.

FERGUSON: Did your mother have formal education beyond high school?

WILSON: No, not really. She was brought up in Gallatin, Tennessee. She went to the local schools, but not beyond high school.

FERGUSON: When you were growing up, your father was practicing corporate law?

WILSON: Yes, in New York City. We lived in New York for a few years. My father and mother and my older sister lived there first. My sister and I were born in Tennessee because my mother's brothers and her mother lived in Gallatin. In particular, one of her brothers was a doctor. So she went back to Tennessee for the births of my older sister, Sue, and me.

FERGUSON: Was your father active in politics after he left Tennessee?

WILSON: I don't think so. He certainly was not active in any formal sense after he left Tennessee. He did have a political career in Tennessee before he was married, and before they moved to New York. In fact, he was a member of the state legislature, I think for two terms. The second term, he was the Speaker of the House of the legislature, which is a queer feature, because he was in succession for becoming governor, if the governor was to die. I don't remember which level he was, but he was too young to be governor. There is a restriction on age, or there was in Tennessee. Fortunately, the necessary number of people didn't die and leave the dilemma! [laughter]

FERGUSON: How old was he when he married your mother?

WILSON: He must have been around twenty-five, but I'm not sure.

FERGUSON: Well, he was a very young man for a legislator.

WILSON: Oh yes, he was very young.

FERGUSON: Where did you receive your elementary education?

WILSON: Grammar school was in Yonkers, New York, Public School Number 3. We moved to Yonkers when I was about four or five years old. When it was time to go to school, I went to the public school in the appropriate district. This school went through the nine grades and had one year of high school for some of the children. I went from there to the Riverdale Country School, in New York City, on the edge of the border between New York and Yonkers.

FERGUSON: Was this a boarding school?

WILSON: It did have some boarded students, but I was not one. It was a private school. That was for one year only. Then my parents sent me to Lawrenceville School in New Jersey, very near Princeton. I had two years there. So my four years of high school were at public school in Yonkers, next Riverdale, and then two years at Lawrenceville, where I did board. I went to Princeton from there.

FERGUSON: How did your interest in science start?

WILSON: It started very early. In my early memories, I was playing with electric trains, not very scientifically, but fooling around with that sort of thing taught me a lot about electricity. Chemistry sets were the next lead-in. My parents had absolutely no scientific education, either of them. I owe them a great deal in that regard, because they very much encouraged me to read about science and provided books.

FERGUSON: What kind of community was Yonkers when you lived there?

WILSON: It was a typical suburban commuter place. It was very nice, actually, at that time. We were on the edge of a mostly forested area. You know, city blocks, but still, I remember deer running through the yard once or twice. Of course, now it's totally built up. It's terrible to go by there and I avoid it like the plague. I went once where the old house was and is, and it's all just jammed together houses now. I think of all the space I had as a child. It was a really lovely place. My father commuted about an hour every day to downtown Manhattan on a train.

FERGUSON: Did your classmates engage in the same kind of things you did?

WILSON: Yes, I'd say so. We were in a nice, fairly well-to-do neighborhood. The public school had all kinds of people in it. It was a typical public school. It had children from a wider range than where I lived.

FERGUSON: Did you get encouragement in science from some of your teachers?

WILSON: There was one teacher who was pretty encouraging. I don't particularly remember any of the others. They weren't outstanding. It was a good school. There weren't any frills.

There wasn't anything fancy about it.

FERGUSON: Did you have chemistry or physics and laboratories?

WILSON: No. Nothing. There was no science in the curriculum at all.

FERGUSON: How about the other schools?

WILSON: At Riverdale they had some very good teachers, and at least one of them, in particular, was quite encouraging. They had a science club which this teacher sponsored. So I had full, official encouragement at Riverdale. Lawrenceville had a good chemistry teacher who was mainly interested in getting us through the college boards. His interest in chemistry seemed to be rather modest. Just the year that I went there, they got a very young Harvard graduate as a physics teacher. He was immediately made fun of by the boys.

FERGUSON: Do you remember his name?

WILSON: I should because his father was well known, but I can't remember now.

FERGUSON: How did you come to go to Princeton?

WILSON: My father had people he knew, friends practicing law, one of whom went to Princeton. They sort of influenced the family to send me there.

FERGUSON: Was Lawrenceville School viewed as a preparatory place for getting into Princeton?

WILSON: Very much so. In those days, practically ninety or ninety-five percent of the class moved up to Princeton. It was certainly a very major portion of the class. Gradually, of course, with the changes in the admissions policies, no college would take ninety percent of the class anymore. The headmaster at the time was Mr. [Mather Almon] Abbott. To call him "Mr." sounds funny to me. He was known as "Bot" by the students. He was from Yale and tried hard to get some of them to go to Yale. I disliked him intensely.

FERGUSON: Because of the boarding school atmosphere?

WILSON: Well, there was the boarding school atmosphere. I didn't like the boys for the most part, although most of them went to Princeton where I knew them again. There were all kinds in that crowd. But Abbott was totally non-intellectual, which was, in my opinion, typical of a private school headmaster. I once heard that there was a gathering of headmasters at Princeton. The Lawrenceville man and the Riverdale man were there, so I thought I'd go and shake hands with them. I almost couldn't find them in the crowd--they were all so much alike! These were backslapping, healthy-looking, athletic characters. At any rate, I didn't like Abbott. I didn't like Lawrenceville. It didn't help me intellectually.

FERGUSON: Well, let's go on to Princeton, then. Did you like Princeton?

WILSON: Oh, Princeton was just a revelation to me. I went there and I found other students who were interested in things. I don't care what the heck it was they were interested in, but they were interested in something intellectually. I thought Princeton was just marvelous. I enjoyed it enormously.

FERGUSON: Did you go to Princeton anticipating chemistry or science?

WILSON: I would guess that I started being interested in science when I was six years old. That continued intensely. I had a strong deviation for a while when I was in high school. I got interested in different kinds of engineering. Electrical engineering made sense, but also construction, which was mechanical and civil engineering--bridges and things. My father got me a job just before I went to Princeton, working on a project digging one of the subway lines in New York. I worked in the office with a contractor and kept the time sheets. It wasn't really an engineering experience for me but, fortunately, it showed me that I didn't belong in that world. [laughter] That is all too clear. I finally decided just before going to Princeton that I would postpone engineering. I started with the basic science courses. Of course, I never went into engineering, but got into chemistry right away, and then physics. As an undergraduate, I always took chemistry, physics, and math each year. I also took fluff, whatever was necessary in the way of other requirements, for distribution.

FERGUSON: Your major and your bachelor's degree were in chemistry?

WILSON: Yes.

FERGUSON: Were there minor subjects?

WILSON: I took math and physics in equal amounts.

FERGUSON: What was the chemistry curriculum like?

WILSON: I enjoyed it, but there was one aspect of it that I really sincerely regretted later on. That was something that was quite universal in those days. They certainly had the same thing at Harvard when I got up there. It was an excessive laboratory demand on the students in analytical chemistry and the rote learning of the qual scheme. I can start out now in the qualitative analysis scheme, you know. Mercury, lead, and...

FERGUSON: The chloride precipitation. I remember that, too.

WILSON: That's right. Hot water dissolves one of the chlorides [magnesium].

FERGUSON: That was qualitative analytical chemistry?

WILSON: Yes. Then we had quantitative analysis. Hours and hours and hours of it. I was terrible in that.

When I was a freshman, I was an eager beaver, there was no question about that. I went to all the colloquia. I didn't understand a damn thing, but I'd go to organic chemistry, physical chemistry, and physics colloquia. Physics, of course, was just having a terrific revolution, with the development of quantum mechanics going on full blast. I went to these colloquia, but I didn't understand anything.

There's one thing I should add about Lawrenceville. I did found the science club at Lawrenceville, which is not exactly going to be on a bronze plaque [laughter], but it's still going, I think. Also, somebody knew Professor [N. Howell] Furman, the analytical chemist, and arranged for him to come down and talk to us. Lawrenceville was very close. He gave a nice talk that interested me. When I was a sophomore at Princeton, I went and asked him if there was any way I could do research. I didn't know how to weigh anything. I'd never heard of a buret. I certainly wasn't prepared to do research in any sense of the word. But he was a great man, in my book. He thought about it awhile and said, "O.K., fine." He gave me a problem. The problem he'd been working on was the use of a tungsten wire electrode, together with the usual other electrodes, as an oxidation-reduction end point indicator. You plot the voltage change at the endpoint of the oxidation.

FERGUSON: Did you use calomel electrodes?

WILSON: Calomel electrodes and reference electrodes. He showed me how to run the titration and write the readings down, which I did. One day, I went to talk to him and said, "You know, what I don't understand is this elaborate business of a potentiometer and a galvanometer to get this voltage. Why don't you just put the tungsten and platinum wire" (we had a platinum electrode) "and run it through the galvanometer? What's the use of having the potentiometer in there?" He very patiently explained to me about reversible electrodes--that they shouldn't draw a finite current, and all the rest of the rationale for it. Nonetheless, we put the tungsten wire and platinum wire into the solutions and used the galvanometer for endpoint purposes. It worked just fine if the galvanometer was sufficiently sensitive. Furman talked to a friend who knew about patents. I think they actually sent in an application, but quickly got discouraged. It is difficult when you know nothing about patents. People string out the time it takes to get a patent deliberately. We couldn't understand those things. We got objections. We got things back from the patent office, about prior art and so on. Still, this led to a paper. It was my first research paper for the American Chemical Society (2).

FERGUSON: Did that allow you to skip some of the lab courses?

WILSON: Unfortunately not. I started to tell you about that, because we started on analytical chemistry. The freshman year, I had Hugh Taylor. Actually, that year and for a few years later they had a separate freshman course with a small number of students in it. Hugh Taylor was the chairman of the department, and a big man, but he gave the special course. I didn't learn much chemistry in that course, but it was very important for me, because Hugh Taylor made things so exciting that you just couldn't resist being taken by it.

FERGUSON: But Hugh Taylor was a physical chemist.

WILSON: He was a physical chemist.

FERGUSON: Was he teaching a general chemistry course?

WILSON: Well, this was a small, special course. They had a regular course, with fifty or a hundred students in it. This one just had about a dozen.

FERGUSON: Do you mean you didn't learn any descriptive chemistry?

WILSON: I didn't learn any chemistry in that course! Well, I'm exaggerating, of course. [laughter] What would happen is this. We'd assemble in the classroom and Hugh would arrive. First he would get out his pipe and place it in the chalk tray. Then he would open a book, which was Mellor's little book--well, a rather fat book (3).

He would open this book and say, "Let's see now, we were talking about sodium chloride, I believe." But he never got much more beyond that in the topic he was supposed to be teaching for the day, because he would start telling us about the research results they got yesterday, or last night, and how exciting they were. As I say, I exaggerate a bit. He was a very exciting teacher.

FERGUSON: Were there other professors that influenced you?

WILSON: There were others who were also important. In the spring of my freshman year Peter Debye came to America to lecture. He gave a lecture to the Electrochemical Society in Philadelphia. Many of the chemists at Princeton went down to hear him, and they took me along. There was Furman and Taylor and [Charles P.] Charlie Smyth.

We went in Furman's car. Furman was a very conservative, cautious fellow. He was a fine man, but he carried two spare tires in his car, which I thought was strange. [laughter] They took me down there and I heard Debye talk. That was a very nice occasion, because it helped me get to know more members of the faculty. I didn't take courses with all of them.

But going back,(we're never going to get beyond that analytical chemistry if we don't get moving!), Furman taught the quantitative analysis, and one of the climax experiments was the complete analysis of orthoclase feldspar. It was terrible because getting it into some kind of solution was quite an operation. I did very poorly. My results were very bad. Before the grades were announced, Furman said, "You know, I'm sorry about this but I just can't give you more than a B." It wasn't an A, but I was so delighted, because I thought I'd flunked the course! How many times I had dumped my unknown on the desktop, picked it up with a sponge, and squeezed it back into the test tube! That's a slight exaggeration, but not much, to show my inability as an analyst.

FERGUSON: Did you do other undergraduate research?

[END OF TAPE, SIDE 1]

WILSON: Yes, I went from Furman's direction to work with Charles Smyth. There are a couple of papers with my name and Smyth's on them (4). I worked in his laboratory.

By the way, he's ninety-something and going strong right now. He's not doing any science as far as I know. His brother just died at age ninety. That's Harry Smyth of the Smyth Report (5).

I kept on doing experimental work, first with Smyth for a couple of years and then with Bill [William T.] Richards. But it was pretty clear that I wasn't all that great in the laboratory. Well, it wasn't all that bad. I did all right in the Princeton laboratory, except in the quantitative analysis course. Incidentally, I don't consider myself a pure theoretician. I think that what I do best, perhaps, is think of experiments and then get somebody else to do them.

FERGUSON: Did you like organic chemistry, or ever consider it?

WILSON: No. I didn't like organic chemistry much. It seemed to me that it wasn't a very good course. We'd set up a still every time we went into the laboratory. I'd figure out what we'd distilled later, when I had to write up the experiment. I didn't get much out of that. Really it was a fairly limited course.

FERGUSON: Do you have any recollection of the text that was used?

WILSON: No, I have no idea.

FERGUSON: What about the physical chemistry course?

WILSON: The physical chemistry course was given by D. Jefferson Webb.

FERGUSON: When did Hugh Taylor write his big treatise on physical chemistry (6)?

WILSON: It must have been about that time, because I have a copy of it, and I bought it when I was there. It probably came out in my junior year or thereabouts. But Webb was teaching the physical chemistry course. I took that in the second or third year.

FERGUSON: Taylor's book was not used as the teaching text?

WILSON: No.

FERGUSON: Or as a reference text?

WILSON: As a reference, I guess. It was very, very greatly detailed. Two volumes. There was a student in the graduate school (I stayed a year in graduate school) who flunked the preliminary exams one time after another. He had obviously studied out of that big two-volume set. It was just inappropriate, because you can't really learn in detail that much material with any sense of what you're trying to do.

FERGUSON: You wrote a senior thesis on quantum mechanics?

WILSON: We had, in fact, a junior thesis and a senior thesis requirement. The junior thesis wasn't much (7a). It was paraphrasing Debye's paper. I think it was on the frequency dependence. At any rate it was on the dipole moment. It wasn't original at all. It wasn't supposed to be. For my senior thesis, I worked with [Howard P.] Robertson in the physics department (7b).

FERGUSON: You never published any of this?

WILSON: The senior thesis appeared only as a Princeton thesis, but it really was a little book on quantum mechanics. I have it around somewhere. Robertson was very helpful indeed.

FERGUSON: What was his background?

WILSON: I don't know where he picked up his knowledge. I was a freshman in 1926. I graduated from Princeton in 1930. I was doing this thesis in 1929-1930. Ed [Edward U.] Condon came to Princeton just before that, perhaps when I was a junior. I took his course in mechanics. He was busy at that time writing Condon and Morse (8). Phil [Philip M.] Morse was there with him as a postdoctoral fellow. They were writing their book on mechanics, which was really the first American textbook of its kind.

FERGUSON: Condon was on the faculty at Princeton at that time?

WILSON: Yes. He left after one year, went to Iowa, then came back again the next year and stayed at Princeton for a while.

FERGUSON: Was the course he taught classical mechanics?

WILSON: He taught classical mechanics my junior year. Two years later, while I was there, he gave a course out of [Paul A. M.] Dirac's book which had just appeared (9). I attended that, but it was rather much for me. But working on this senior thesis, I had close help from Robertson. I put out a thesis which certainly helped me very much later on in dealing with my book with Pauling (10). I don't mean that there's anything original in the thesis at all, but it helped me understand the subject.

FERGUSON: Would there be a copy of that in the Princeton library, or would yours be the only copy?

WILSON: I don't know. I have it around here somewhere. I dug it out a few years back when somebody asked me about it. It was a physics study similar to your oral history program, conducted by the American Physical Society (11). One of them talked to me about the quantum mechanics.

FERGUSON: What was your master's degree work and thesis?

WILSON: I didn't have any master's thesis. I worked with William T. Richards, the son of T. W. [Theodore William] Richards at Harvard. He was a very Harvard fellow and a very nice guy. He had a particular friend named George Kistiakowsky. That's how I first met George. Richards was a fine fellow and he had ideas. He had been working on the velocity of sound. I did something with him as a first-year graduate student. There's a paper that was sort of scraped up off the floor (12). We didn't accomplish much there.

He was a physical chemist. He did something special for me. It was fun. I don't know that it really had a big effect on my career. Alfred T. Loomis was a Wall Street investment person of some kind, and a very wealthy man. He set up a laboratory in Tuxedo Park, New York. He had a house there, and he bought a second house and converted half of it into a laboratory. It had a concrete floor, so he made it a laboratory. The rest of the house he used as living quarters for visiting scientists who worked in the laboratory. It was a private, personal operation, financed with his own money. It was rather fabulous. I went up there one summer, maybe two summers, with Bill Richards, who had an entre and decided to work there. One of the people who was there was [Robert W.] Wood, the famous optical physicist who made gratings. He had this fabulous collection of stories that he loved to tell and that he repeated eventually, but they were wonderful stories.

There were some other people there. There was an astronomer from Yale [Ernest W. Brown] who was busy making calculations on planetary motion and interactions. There was a man from Bell Labs who was sort of the originator of the quartz piezoelectric crystal clock. Loomis got interested in clocks and he ordered three of the very best pendulum clocks (Riefler clocks) in the world and set them up on special concrete piers in the basement. He had this guy from Bell Labs who provided three crystal clocks. They had a radio to pick up time signals from all over the world and they kept records. The astronomer from Yale [Brown] analyzed all of this information. For example, he got the effect of the gravity of the earth's tides, pushing the clocks up and down daily. You could see this effect.

FERGUSON: It was measurable?

WILSON: Yes. They had two or three years of data and could put Fourier series on it and pull all these things out. I don't think anything very profound came out of it.

FERGUSON: Was this before the radio frequency standards were broadcast from the National Bureau of Standards?

WILSON: That's right. Well, I think so. At any rate, the idea of the piezo clock was new with this Bell Labs man, or not much earlier.

Another fellow who was there was an Englishman who once wrote a book on soap bubbles (13). He was more famous than that. His name is Sir Charles Boys.

FERGUSON: You mentioned Wood and Kistiakowsky.

WILSON: Kistiakowsky went to Tuxedo Park himself later.

FERGUSON: Kistiakowsky mentioned that Wood apparently missed the Raman effect (1). That was a story I'd not heard.

WILSON: I can tell you about the Raman effect, because I was there when that happened. The Raman effect was discovered by [C. V.] Raman and was published by him in Nature (14). A copy of Nature arrived in Tuxedo Park and Wood saw it. He immediately went into the laboratory, probably within minutes, but anyway within hours. He began a typical Wood improvisation. He took a quartz crystal, a bucket and a rubber tube. He put water in the bucket and he put dye in it to filter out one of the lines of a mercury lamp, and set up the mercury lamp and the quartz crystal (it was a crystal--I don't know if it was quartz or not). He made a solid-state Raman, verified the Raman effect within a day or two, and dashed off a note of verification (15). You know, clothespin holding the pail--everything like that is typical of Wood. Improvisation. It worked.

FERGUSON: Did your publications with Smyth originate from work there?

WILSON: No. That was still back at Princeton.

FERGUSON: What in particular did you do at Tuxedo Park?

WILSON: Well, of course, I accomplished absolutely nothing. Richards was trying to do something on supercooled water and supercooled liquids. Frankly, I was incapable of contributing to that. I fooled around. I don't remember the difficulty, but I got into some kind of a permanent difficulty. Richards was not able to get me out of it. He couldn't fix it either. That was a failure. But Wood's stories stick with me.

FERGUSON: I was coming back to Princeton. You mentioned once that you had close relations with the Princeton faculty and the fact that you sought them out.

WILSON: I wasted a lot of their time! They gave it freely.

FERGUSON: Physics as well as chemistry, or other subjects also?

WILSON: Not in physics, except for my thesis advisor. I had a freshman course in physics, a big course with all the students. The man who gave it, in my memory, was an old man. Of course, he was probably a good many years younger than I am today. At any rate, he just couldn't understand and couldn't accept quantum theory. His name was [William F.] Magie. I went to him about research at the same time I went to Furman, and he turned me down very properly. He explained that in physics you couldn't really do these things until you had a lot more training.

I had close relationships with Condon and with my thesis advisor. I also did with Harry Smyth, who was a professor of physics. I took a special course on spectroscopy that he worked up just for me and for a fellow student, Bill [William M.] Preston, who became a professor at Harvard later on. He's retired now. He and I went to Smyth. We went because another professor had a [spectroscopy] course in the catalog, but it said to go see the professor before you signed up for the course. He was out of town that year. Somebody told me, "It's too bad, because he hasn't given that course in five years and he'd love to have somebody take it." Instead, Harry Smyth fixed up something special for us.

FERGUSON: How large was the Princeton undergraduate college then?

WILSON: I think my class had about six hundred students in it, if I remember correctly.

FERGUSON: How many would you have had in chemistry?

WILSON: Oh, it was very small in chemistry. There were about six or eight concentrators. Physics was even smaller. I think there were five concentrators, or something like that. They both stayed small for a long time.

FERGUSON: Speaking of spectroscopy, what was the state of the art when you took spectroscopy at Princeton?

WILSON: Well, it was purely experimental. Actually, it wasn't all that much spectroscopy, as a matter of fact. The other course that we missed was spectroscopy. For Smyth's course we put together a little homemade mass spectrometer. I can now speak learnedly to my colleagues who use them about detectors and beams and so on. They have the same troubles we had. The surfaces get corroded and charges build up on them and deflect the beam so you can't focus it.

FERGUSON: How did this operate?

WILSON: I don't remember the details. You made the ions and you accelerated them in the field. We had a magnetic deflection. I think that part of it was sort of a little box with a field.

FERGUSON: How did it detect?

WILSON: I don't remember. Actually, we didn't do anything else. We didn't do spectroscopy. Henry Norris Russell was a big astronomer of the day, and he gave the spectroscopy course, but he was abroad that year. Smyth put together a different course.

FERGUSON: Russell's spectroscopy course would have been atomic spectroscopy, I guess.

WILSON: It was atomic spectroscopy. In those days, every university had a physics department and had a big Rowland circle twenty-one-foot grating in a big special room. Everybody had one of those things. Now you wouldn't find one in the United States as a whole. I think physicists tend to go in fads, or waves, much more so than physical chemists. One thing at a time is fashionable in physics.

FERGUSON: Were there any of your fellow students at Princeton that you particularly remember, or would like to comment about?

WILSON: One person was quite a bit of help to me as an undergraduate. He was George [E.] Kimball, who eventually became a professor of chemistry at Columbia.

FERGUSON: He wrote the book <u>Quantum Chemistry</u> (16)?

WILSON: That's right, with Henry Eyring. He became a student of Eyring's. When I knew him, I was an undergraduate and he was two years ahead of me. I knew him as a graduate student, too. He was a very smart fellow, very learned. I have a story about him that I like. It seems he spent a lot of time in the chemistry laboratory. He knew a lot of chemists. People had trouble finding certain professors at a certain hour. They discovered that the physical chemists were assembling in a room in the laboratory, where they were learning quantum mechanics from

George Kimball. He was a first-year graduate student at that point. He hadn't written the book yet with Eyring, but he was lecturing as a student to the faculty. He was not only very clever, very learned, and very intelligent, but he was a very good lecturer, too. I learned a lot by asking him questions. Bill Preston was another outstanding person. But, in the little groups of about five in chemistry and physics, there weren't all that many famous people. They were mostly not outstanding.

FERGUSON: The people you've mentioned are impressive. How did you come to go to Caltech?

WILSON: That was very simple. First of all, Richards urged me to leave and go to Harvard. Well, I didn't know anything about Harvard. I didn't know anything about Caltech. I'd heard of Pauling. I talked to Taylor. He said, "You go to Caltech and work for Pauling." So I did.

FERGUSON: How was this arranged? Did you just apply formally, or did Taylor call Pauling?

WILSON: I'm sure he called or wrote Pauling.

FERGUSON: Was it established that you were going to work for Pauling?

WILSON: That's right. Which isn't a good idea in general, I'd say. We don't let our students do that, as you know. [laughter] They can't sign up until they've had about a year of finding out who is here and is right for them.

FERGUSON: How was your Princeton education financed? By the family?

WILSON: Up until the stock market crashed, my father was (on paper) a wealthy man. He paid for my Princeton education. When 1929 came, he lost everything and had a very hard time. When I went out to Caltech, I had a teaching fellowship. In those days you could live on a teaching fellowship and go to school. It has become difficult to do because the price of tuition has gone up so much more than the rate of pay for the job. I don't think people can do that anymore. I'd say most colleagues my age lived on teaching fellowships and got their advanced degrees that way.

[END OF TAPE, SIDE 2]

[**FERGUSON**: Question on the history of quantum chemistry.]

WILSON: I talked about some of this in an after-dinner talk (17).

FERGUSON: Where did you give this talk?

WILSON: In Florida, at one of those annual quantum chemistry meetings.

FERGUSON: Who was the organizer?

WILSON: [Per-Olov] Löwdin.

FERGUSON: How was the transition from Princeton to Caltech?

WILSON: That worked very nicely. I arrived during the summer. I put up at what they called the Athenaeum, which was a very elegant building. It serves as the faculty club for Caltech, for the Mount Wilson Observatory and for the Huntington Library. A lot of people go to lunch there. They also have rooms. I had a room at first, but I couldn't afford it. Secondly, I didn't like it much. They had a big sleeping porch on the third floor. It was open on one side. They had twenty beds and a shower room. A lot of graduate students lived there. I moved in and lived there the whole time I was at Caltech.

FERGUSON: Was this your first experience with a dormitory type of accommodation?

WILSON: Yes. I guess in that sense it was. When I went to Lawrenceville, I had a room to myself there. That was pleasant. I liked that.

I met some people in Pasadena. I met a fellow who was at the Huntington Library for the summer. He was the type that made friends quickly. He introduced me to a number of Pasadena girls. They became good friends, they and their ultimate husbands. I had a very good time socially in Pasadena, that had nothing to do with Caltech.

FERGUSON: Was this different from Princeton?

WILSON: There weren't any girls at Princeton.

FERGUSON: Not even town girls?

WILSON: There were very few of them.

FERGUSON: Where did you live in Princeton?

WILSON: As an undergraduate I was in one of the dormitories. As a graduate student I lived right at the graduate college that's out near the golf course. It was quite a distance.

FERGUSON: Did you belong to one of the notorious eating clubs?

WILSON: I did as an undergraduate.

FERGUSON: Or maybe they weren't considered notorious in those days!

WILSON: They were notorious, when you think about it. I really objected very strongly, eventually. Somebody misguidedly gave a prize to the club with the best standing--a scholarly prize of some kind. Of course, any club that got that prize was immediately socially dead! Our club had some smart people in it and we got the prize. It was not high on the social ladder. It wasn't Ivy.

FERGUSON: Let's go back to your course work at Caltech.

WILSON: I took two courses worth mentioning. One was Pauling's lectures on quantum mechanics. I minored in mathematics and the second was a course in [Edmund T.] Whittaker and [George N.] Watson's book, an excellent analysis (18). That's as far as I got in mathematics.

Also, [Richard C.] Tolman was there. I got to know him very well. I was very fond of Tolman. He wrote this big book on statistical mechanics (19). It was a lovely book. He also did various astrophysical things that I didn't know about. I used to spend hours talking to him. He says something in the preface to his book to thank the discussion groups, and said he had lengthy discussions. I guess he sort of used me as a person to try his ideas on. I admired Tolman. I admired his political views. He was a very liberal person, but sensible.

FERGUSON: Did he continue on the faculty of Caltech through his career?

WILSON: Yes. He died a couple of years after the war. I encountered him again during the war, when I was in wartime research. He was sort of my contact point with the top people. He was one of the board of trustees, so to speak, at that time.

FERGUSON: What projects were you on?

WILSON: I was at Woods Hole originally, but I became chief of Division 2 of the National Defense Research Committee. He was on the board of that committee.

FERGUSON: We'll come back to that. As I understand, you took notes from Pauling's course on quantum chemistry.

WILSON: I guess I took notes. I don't remember about the notes.

FERGUSON: Then how did you and he come to write the book?

WILSON: I spent two years as a graduate student at Caltech, and the second year I was the grader in his course. I graded the problems that he handed out. That way I had a closer connection. The next year, the postdoctoral year after I got my degree, was when we wrote the book.

FERGUSON: Pauling got this from his time in Europe?

WILSON: Pauling was one of these lucky people that were in Europe at the time of the introduction of quantum mechanics. There were a lot of people who were.

FERGUSON: You have mentioned John Slater. What particular approach did you use? There was the [Erwin] Schrödinger approach, and the...

WILSON: [Werner] Heisenberg approach and the...

FERGUSON: Dirac approach. Which one?

WILSON: We used the Schrödinger equation. That's what most people used at first. Of course, now they're just so interchangeable. They're all the same thing, just different ways of expressing it. But at first that was not known.

FERGUSON: Did you get into group theory at this time?

WILSON: Well, that was sad, I'm sorry to say. I was at Caltech at the time, probably in my second year as a graduate student. I didn't know anything about molecular vibrations or molecular spectra, but there was a colloquium on it. I think Tolman was giving it. I got interested in the problem of vibration of polyatomic molecules, about which really practically nothing was known. [David M.] Dennison had a couple of papers on CO_2 (20). There were several older papers on CO_2. I read a paper by Kimball and Eyring on group theory for valence bond calculations of electronic energies (21). Frankly, no great genius was required to see that the molecular vibration problem was exactly the same. The group theory that they used treated electronic energy levels of symmetrical molecules. It was just a five-second step from there to applying it to the vibrations of molecules, which I did. I got very excited. It's marvelous; it's so simple.

Of course I wrote up a paper. Then I went around and talked to several people who did molecular spectra. None of them knew anything about this method. So I sent in the paper and a quick letter.

In the meantime, I discovered that Eugene Wigner had already published a paper putting group theory onto the vibrational problem, in Göttinger Nachrichten, which is a little obscure (22). The thing is, I'd gone to Wigner when I was still at Princeton as a graduate student, to see if I could work for him, on any problem he could propose. I think he did propose a problem which later became a famous problem. The fellow who became president of the National Academy of Sciences, Frederick Seitz, did his thesis on it. I fooled around a little bit with it, for a short length of time, and decided I didn't want to go on with that.

But to go back to Caltech, where I saw this paper by Wigner, I had never talked to Wigner about the vibrational problem when I was at Princeton. I kept on applying group theory to other problems, but had to withdraw the paper that I'd sent in because it was all done. I guess my contribution was largely spreading the gospel, getting people to realize how simple group theory was to use and what you could do with it.

FERGUSON: What was the state of molecular spectroscopy at that time?

WILSON: For polyatomic molecules it was very near zero, but not quite. The first paper on polyatomic molecules came out in 1914. It was by a European, on carbon dioxide (23). It was very good, but this was long before quantum mechanics.

Then came the people at Michigan. There were several experimentalists there. They had beautiful equipment--the best infrared grating spectrometers in the world at that time. They were working on very simple molecules, triatomic and so on. Dennison was the theoretician, and a very good man too. He had written about normal coordinates. Normal coordinates are considered a classical idea, but the key thing was the normal mode description of the coordinates.

There certainly wasn't much else going on at that time. Now, in Europe there was more--there was Teller.

FERGUSON: Edward Teller?

WILSON: Yes. Edward Teller, [L.] Tisza and another fellow. They were working on the vibration theory and group theory, too, at about that time. But I wasn't up to date. I didn't see their work for a long time.

FERGUSON: You still take some exception to Edward Teller's political philosophy?

WILSON: I sure do.

FERGUSON: Well, back to the science. Be sure this is clear now. Vibrational spectroscopy, up to that point, was still classical mechanics and experimentally...

WILSON: ...was pretty limited. By the time I got on the group theory, the idea of going from classical normal coordinates to the application of quantum mechanics was known. So my contribution there was not original. I rediscovered the use of group theory for the problem.

FERGUSON: Your first paper on that subject was the "Calculation of Vibrational Isotope Effect in Polyatomic Molecules by a Perturbation Method" (24)?

WILSON: Yes. That didn't have much to do with group theory.

FERGUSON: Then you did benzene?

WILSON: That's right. Benzene was a straightforward application. Wigner had published the principles there, although actually his example was methane. But just the Europeans were using it. I don't know exactly when they started publishing. But benzene was a straightforward application. Of course, it's a beautiful model to apply things to. Furthermore, the literature was completely screwy to that point. [Donald H.] Andrews at Johns Hopkins had it all wrong. He had triply degenerate states. Group theory showed that the publications were wrong, but this was a purely theoretical paper (25). I later attempted to assign frequencies and I made a mistake there too. The original benzene paper is just an example of an application of group theory to vibrations in symmetric molecules.

FERGUSON: You did some early additional work with J. B. Howard. Was that here at Harvard, or was he out there?

WILSON: He was a Harvard undergraduate and went to Caltech. He didn't like it very much, so he came back to Harvard. I barely knew him at Caltech, but we worked together here closely. He was a student of [John H.] Van Vleck, here at Harvard. He and I worked together, particularly on the separation of the rotation from vibration of polyatomic molecules (26).

FERGUSON: Did he take his degree here at the physics department?

WILSON: He got his degree here, that's right.

FERGUSON: Going back to Caltech, who were the other faculty members that particularly influenced you?

WILSON: I mentioned Tolman; he's the most important. I really saw more of Tolman than I did of Pauling. But of course, when I worked with Pauling on the book in my first postdoctoral year, we worked very closely.

FERGUSON: Did you stay more or less continuously during those three years, including summers?

WILSON: I came back home one summer.

FERGUSON: This is out of order, but I forgot to ask you earlier whether you had brothers and sisters?

WILSON: I had two sisters. My younger sister lives in Washington. My older sister died some years ago.

Going back to Caltech, you asked about people. I mentioned Tolman. [Richard M.] Badger is somebody I saw a fair amount of, because he was doing infrared of molecules.

FERGUSON: Kistiakowsky indicated that they had to give you some sort of experimental project so you could get your Ph.D. there.

WILSON: It's a lesson I learned which was very important. Pauling suggested that there was a problem concerning nitroso compounds, and you'll notice in the bibliography, there's one paper there (27). Pauling thought these were colored because there was a triplet state involved. He wanted me to measure the magnetic susceptibility. You see, you could distinguish triplets this way. I hadn't done badly at Princeton in experimental research. I didn't feel that I was clumsy, particularly, or incompetent in the laboratory. In fact, I felt fairly confident about it. But I was a total loss on this simple-minded conventional balance scheme with a magnet.

FERGUSON: Did they call it the Gouy balance?

WILSON: Yes. One got an analytical balance [set above a magnet gap] and hung the sample tube from the balance pan. The damn thing was always pulling over to the side, rubbing against the poles of the magnet. I really got terribly discouraged; I did what I've seen many a graduate student do. You have to catch them quick and stop them. They just sort of slow down. They don't show up every day, or they come in late. I was in that state. It was very, very bad. Finally, it didn't take much arguing, but Pauling put me on a theoretical job and took me off the balance. I did finally get it. The compounds were diamagnetic, not in a triplet state. That was a real hassle.

FERGUSON: What was your thesis at Caltech?

WILSON: It was on calculation of the ground state of the lithium atom. It was nothing. Frankly, you should never give a Ph.D. student a thing like that. It was a big disappointment.

FERGUSON: But you had other publications in progress?

WILSON: At that time, I think I was struggling to do the experiment first and then calculating. We went on to try to calculate beryllium. That was a much bigger job, and Pauling provided me with a fellow student as a calculator for me.

FERGUSON: Were you using a mechanical calculator of some kind?

WILSON: I don't know if it had a motor on it. They eventually became motorized.

FERGUSON: Speaking of that, whatever happened to A. Sprague Coolidge's mechanical calculator which you had here?

WILSON: I think it was junked long ago.

FERGUSON: The Smithsonian Institution is interested in collecting historical things like that. Maybe later we ought to go through a possible list.

WILSON: It was around for a long time.

FERGUSON: To us at least, that mechanical calculator would be interesting. All right, we've been meaning to push on to Harvard, but we are hardly there yet. How did you get your appointment with Harvard?

WILSON: I got a telegram one day saying that I'd been elected to the Society of Fellows at Harvard. Well, I'd never heard of the Society of Fellows. This was my second year at Caltech, my last graduate year. Pauling was away. He had offered me a job for the postgraduate year, and I had accepted. I got the telegram from Harvard, and I wired back saying, "Thank you very much. I appreciate the honor, but I have agreed to take a position [at Caltech] for that year." When Pauling got back, he was mad, because, I'm sure, he had a number of students in need. I'm sure he felt that I had refused a good job. He could have taken that money and handed it to one of the other students! At any rate, he said I shouldn't have done it. I shouldn't have. He explained what the Society of Fellows was. He knew about it, but I hadn't heard of it.

FERGUSON: Had he probably recommended you for this?

WILSON: I don't think he had originally, but he might have. I think it was Bill Richards who did it. His uncle was the head of the Society of Fellows, the major-domo.

FERGUSON: Could you reverse your decision, or did you have to wait until the following year?

WILSON: They re-elected me the next year. The Society had just been founded, and I was elected to the first year, but didn't accept. But they elected me the next year. Richards said, "Well, you know there are people on the board that think, 'You turned it down, you shouldn't get elected,' but my uncle didn't feel that way!" [laughter] That's what happened, lucky for me.

FERGUSON: What did you do while you were a junior fellow?

WILSON: I worked on a lot of things. It was a very productive time from the scientific side, particularly with Howard. We worked on the separation of rotation and vibration. The idea is that you can write an effective Hamiltonian that treats it as a rigid body, but the parameters change for vibrational motion. I guess that Hamiltonian is still useful. Howard and I worked on ethane, for which there is a problem of internal rotation. We did quite a bit of work together. I think we published about three papers (26, 28). That was a very good time for me.

FERGUSON: The duties of the Society of Fellows were very nominal?

WILSON: There weren't any. I went to see [Lawrence J.] Henderson when I arrived. After the preliminaries, I said, "Well now, what am I to do?" Oh, he was outraged! "You're not supposed to do anything." No duties. I mean, it's true. Absolutely free agents. You could work with somebody if you wanted to, or you could go talk to people, or take courses if you wanted.

FERGUSON: This could be disastrous for some people, not having pressure.

WILSON: Yes. Not everybody has done well. Most people have, I must say. There's a tremendous list of Nobel Prize laureates who were members of the Society of Fellows.

[END OF TAPE, SIDE 3]

FERGUSON: Did you get into any experimental work during this period as a member of the Society of Fellows?

WILSON: I did either that year or the next year. I tried to set up a long-path near-infrared spectrometer. In the basement, I had a long pipe about a hundred feet long. I had some help from Kistiakowsky, I'm sure. When I was here, I had two people who were very, very helpful to me. One is Kistiakowsky, and I did try some experimental work, which was a failure. The other was Van Vleck, who was most helpful on mathematical theory.

FERGUSON: Was there any experimental infrared work going on here then?

WILSON: No, there wasn't. Only spectroscopy, I guess, if you draw a distinction--fluorescence and visible spectroscopy.

FERGUSON: Were you in the Society of Fellows for three years?

WILSON: I only stayed two years. It was a three-year appointment, but Harvard offered me a job. In my second year our book had come out (10), and I gave a lecture course from it while I was in the Society. The chemistry department offered me an assistant professorship and I resigned from the Society.

FERGUSON: Did you begin teaching physical chemistry then?

WILSON: Right away. I gave that quantum mechanics course before I was appointed to the faculty. The first year I was a faculty member, I took over the physical chemistry course from my predecessor, who had been promoted. I had about a hundred students in that class.

FERGUSON: What was the general content of the physical chemistry you taught then?

WILSON: Thermodynamics was the center of my emphasis.

FERGUSON: Did you develop your own notes or use someone's text?

WILSON: I pretty much developed my own notes. I had a text, but there weren't many texts available. I wasn't really very happy with any of them.

FERGUSON: Jud [A. Judson] Wells was your first graduate student?

WILSON: Yes. Jack Howard was in a special category. He was not a graduate student of mine, of course, because I was in the Society of Fellows at the time we worked together. Part of his thesis came out of the work we did. Then Wells came along as the first real graduate student.

FERGUSON: And you built an infrared spectrometer at that time?

WILSON: Yes. George Kistiakowsky gave me an assistant, a postdoctoral fellow, Harold Gershinowitz. He later became head of the Shell Development Company. George paid for him, but he became a postdoctoral with me. We worked together, but Gershinowitz was crucial. We built a semi-automatic infrared spectrometer. It was the queerest thing you ever saw (29)!

FERGUSON: Would you describe it? I've heard tales about it and never saw it.

WILSON: [laughter] We had one machine piece built for us, and then there was a typewriter carriage with some ratchets. It was a motor driven thing that took a crank. The crank had strings attached to make things happen. First of all were the two cells, one on top of the other. They moved up and down in this mechanical contrivance. They'd move up to put the blank cell in and move down to put the filled cell in the path. The strings pulled the typewriter carriage so that the photographic paper moved a notch at a time. The machinery rotated the prism by a definite amount each cycle. You'd get it going and then it ran itself. It put the blank and the zero on photographic paper. You had to measure the photographic paper afterwards, but you could do so at your leisure. It also had interchangeable prisms, so if you used a potassium bromide prism you went a little further into the infrared than with traditional rock salt.

FERGUSON: Sodium chloride was the regular prism?

WILSON: Yes.

FERGUSON: How long would it take to run a spectrum?

WILSON: About a week! It's something you could do on an early Perkin-Elmer spectrometer in about half an hour or less. It would take five days on ours.

FERGUSON: What was the source?

WILSON: Globar.

FERGUSON: And it stayed stable that long?

WILSON: We had zeroes every cycle, and the blank. On a bad day it would drift all over the place. We had an optical amplifier directly on the thermocouple system. The galvanometer was then amplified by a second galvanometer onto a grid of slots in a brass plate. As the light from the galvanometer swung across this thing, you got light going through the slots or not. You could get enormous amplification, up to Brownian motion with no problem at all. There were noise limitations due to the bouncing around and the drifts.

FERGUSON: This was mounted in the basement?

WILSON: There was a big pier in the subbasement. It was in a box, because otherwise the ground water would come in. The box came up to the basement floor from the subbasement. The concrete pier was inside. We sat on that pier and did our best on the noise and vibration.

FERGUSON: Are any parts of this spectrometer still in existence?

WILSON: I don't think so.

FERGUSON: Was it an advance in the state of the art at the time?

WILSON: Yes, I would say it was. At Michigan, which was <u>the</u> place then, they used a graduate student to write down the deflections of the galvanometer. Peak, zero, blank, and write it in his notebook. Of course, we had to measure just exactly those same quantities off the photographic paper, but we could do it at our own speed.

FERGUSON: When did the first commercial infrared spectrometers arrive?

WILSON: Pretty early.

FERGUSON: Did you get one here in the chemistry department?

WILSON: This department got the first one from Baird Associates. We had nothing to do with that. It was the organic chemists. That was certainly a number of years later. At Perkin-Elmer, I do know, it was during the war. I heard stories about that.

FERGUSON: Did you really get much more into the experimental side of infrared and vibrational spectroscopy?

WILSON: Well, yes. We obtained spectra of molecules and did other things.

FERGUSON: Yes, I forgot the intensity measurements.

WILSON: We did intensity measurements with Jud Wells (30). People still use that method. It's not a good method, but I guess it's the best we have. It's difficult to get good results with it, but we did. That's an example of what I meant earlier.

FERGUSON: Why don't we jump ahead momentarily, because I should really ask you what you feel your most significant contributions were.

WILSON: I think one of them that has had a lot of effect was the polyatomic molecule work. All these things are very simple when you look back. I wish I could give you a sophisticated answer.

FERGUSON: You sound like Peter Debye, explaining to us how simple his theories of the polymer physics were. And not being believed! Please continue.

WILSON: This work with Howard on the polyatomic molecules has really been used a great deal since. Nowadays, of course, they have modern versions which are much more sophisticated, but people still mostly use this model. Now, on the group theory, I think I had an influence, despite the fact that I didn't discover it. I was sort of a salesman for the technique. I certainly had a great deal of pleasure applying it myself to a large number of molecules. We did a number of molecules on the infrared spectrometer. We'd get people like Bryce Crawford and Jack Linnet who did molecules themselves. Of course, I had a marvelous group during my first years as an assistant professor.

FERGUSON: Was Bryce Crawford a postdoctoral?

WILSON: Yes, he got a [National Research Council] fellowship, the only one they had in those days. He had done work for Paul Cross in graduate school. The paper for which I've had the most demand for reprints was the F-G method (31).

FERGUSON: Would you describe that briefly?

WILSON: The F was the force constant matrix, a quadratic force field. G is the way of expressing the kinetic energy. I think the matrix method is very well known to mathematicians. It was not well known in this application. I wrote a first paper on it. It got abstracted in Chemical Abstracts as follows: "MATH" (32). That is the entire abstract! [laughter] I wasn't the only one who suffered from that, because Chemical Abstracts had nobody to do the theoretical papers. Many of the abstracts were like that. Later, a few years ago, a journal [Current Contents] began little articles every month on highly quoted papers. They asked me to write about the F-G method, as it's quoted so often (33). I wrote that I didn't get quite that kind of reply when the paper came out! [laughter] But that was generally, I'd say, the most quoted paper, of course partly because we included it in the book on molecular vibrations (34).

FERGUSON: Now, microwave spectroscopy, of course, was another area.

WILSON: I worked on vibrational spectra for a while, and then the war came along. I worked on it a little bit longer, and after the war we got the book [Molecular Vibrations] out. Then I moved, almost immediately after the war, to microwave spectroscopy. It had been done during the war by MIT and the radiation lab. Now the Stark effect spectrometer was again, just like my tungsten-platinum invention, a product of ignorance. I'm a great believer in products of ignorance. But it was really so, because it seemed perfectly obvious to do this, to modulate the source. It is perfectly obvious; other people would come on it a week later. But I didn't understand a thing about it. I didn't know, for example, that the main source of noise was 1/f [1 over frequency] noise. What the Stark effect modulation does is to push up the frequency, so there's less noise.

FERGUSON: Yes.

WILSON: I didn't know about that. It seemed the obvious thing to try, and I had Dick [Richard H.] Hughes put together the machinery and we tried it. The first time we turned it on, it was great! We put ammonia in the system. Now ammonia was the only molecule you could really do before, and it required a very long path. With Stark modulation, the signal just blossomed. The signal-to-noise ratio went up severalfold, practically infinite on these three lines. It was a very exciting moment.

I will boast a little bit. We made this thing available to others. We had just come out of the war, and everybody was so secret in those days, because they'd all been trained to be secret. We tried to fight that and did. The people around here built their own systems immediately. Of course, other people had thought of it at the same time. A fellow at Westinghouse in Pittsburgh actually went to their patent department for a patent. We applied for a patent, but we waited until the last minute before we applied. They had preceded us by nearly a year in applying. We got the patent, in the end, because we had reduced it to practice. They never made one.

FERGUSON: Did you ever get anything out of that patent?

WILSON: I think a few thousand dollars altogether. We gave it to the Research Corporation, who eventually arranged to take it over. They sold licenses to Hewlett-Packard and to another place. But, of course, there was an exemption for government use. I think the Hewlett-Packard spectrometer was sold almost exclusively to government agencies, at first. By the time they got around to having anything paying royalties, the patent had run out. So that was not a financial bonanza.

FERGUSON: Microwave spectroscopy never really took off in the way that NMR or infrared did.

WILSON: I've often wondered if that isn't partly my fault, because with you fellows as students, I had a real monopoly situation. If you look at the people who did microwave spectroscopy, so many of them have been here. I did have a particularly special position, and that's not good.

FERGUSON: Well, it was tough competition, though. There was Charlie [Charles H.] Townes.

WILSON: Oh, yes. No question about it.

FERGUSON: Similar work was going on at MIT.

WILSON: [M. Woodrow P.] Strandberg was at MIT. First, they were physicists. In this kind of business, physicists are a lot smarter than chemists, let's face it. Except they know too much sometimes. Like Townes, because he knew you had to use the superheterodyne type of detection, beating the frequency down. But it's not so. We showed it, but we showed it out of ignorance. Look, I don't want to claim that Charlie Townes is number one in this game, or was. Strandberg was a very capable guy, and there are many others. But they didn't get into polyatomic molecules. They worked with diatomic molecules.

FERGUSON: That's right. You then pushed into the internal rotational problem.

WILSON: I was pushed.

FERGUSON: I failed with nitromethane. I couldn't solve that one. [William D.] Gwinn's group beat me.

WILSON: Well, that's too bad. I'd say I was pushed into the internal rotation problem. Of course, I'd been involved with George earlier with a lot of vibrational molecules which we'd studied, and the ethane problem. I worked on several papers on the theory of statistical weights and so on. So I was very interested in internal rotation in early days. When it became possible to do this by microwave, I was ready. There, I must say, you fellows pushed me pretty hard.

FERGUSON: [laughter] I thought you pushed us.

WILSON: It was sort of both ways, but [Ralph W.] Kilb and, of course, [Dudley R.] Herschbach were right in there with me. Other people who worked on it weren't quite ready to push it as far as, for example, Kilb insisted on doing.

FERGUSON: Well, you'd sort of run out of rigid rotors by the mid-1950s. [laughter]

WILSON: No, that's not quite true. There are always rigid rotors.

FERGUSON: Let me jump ahead. You wrote one very good paper on group theory application to NMR spectra of symmetrical molecules (35). Then you did nothing more in NMR, as far as I can recall.

WILSON: You said earlier that microwave hasn't played the role that NMR has. Of course it's nowhere near playing the role that NMR does. You know there are certain obvious reasons for that. First, NMR works on the liquid state, while microwave is mostly limited to the gas phase. It's a little hard to say what should have been done, but we could have done better.

FERGUSON: I was never able to sell gas phase microwave spectroscopy in Du Pont and I don't think anybody had much success in industry with it.

WILSON: That's true.

FERGUSON: It's almost the same as with gas phase electron diffraction.

WILSON: First, the limitation of the gas phase is a very extreme limitation. Secondly, the dipole moment requirement is somewhat of a limitation. It doesn't have the universal applicability that NMR has. Still it's a marvelous tool, and I still love it, quite frankly. I wish I could go on and do more with it.

FERGUSON: In your later career you got into reaction kinetics via double resonance experiments?

WILSON: We did some relaxation times, but there we were really scooped by Takeshi Oka. He did a much better job and he beat us to it for a lot of molecules. So that was a fairly disappointing performance.

FERGUSON: What prompted you to write the book, An Introduction to Scientific Research (36)?

WILSON: I guess my wartime experience, where we goofed up some things, unfortunately, in the war situation. I won't go into the details of that, but that's the use of statistics. We made a mistake in one of our projects, which showed me my ignorance of statistics. So I started learning something about statistics.

FERGUSON: I did quite a bit of reading on operations research. I think there was a book titled Operations Research (37).

WILSON: George Kimball wrote something on that.

FERGUSON: It probably was Kimball's book, then. Your book was based in part on that sort of thing?

WILSON: Yes. I'm still a member of the Operations Research Society of America.

FERGUSON: You wrote your book when you were at Oxford?

WILSON: Fortunately, I had a sabbatical at the right moment after the war, in 1950. I had collected a bunch of ideas I'd written on cards. I'd carry them in my pocket. I'd pull out an index card, and ask somebody, "Well, what's your favorite scientific joke?" or "What do you think about scientific methods? Is there any special thing that you might have used?" Everybody had a special thing that they believed in, a special trick, or a special theorem or something. I collected all of these cards. (I found them the other day.) Those were the basis of the book which I put together in Oxford. I pretty well did it that year. I had a lot of fun writing that book.

[END OF TAPE, SIDE 4]

INTERVIEWEE: **E. Bright Wilson, Jr.**

INTERVIEWER: **Raymond C. Ferguson**

LOCATION: **Harvard University, Cambridge, Massachusetts**

DATE: **18 November 1986**

FERGUSON: I reviewed the tapes which we made yesterday. You mentioned informally that you weren't particularly happy about your World War II government work, but I think we ought to cover that a little bit anyway. How did you get involved in that project at Woods Hole?

WILSON: That came about one day after the fall of France. Of course, there was considerable agitation around. Dr. [James Bryant] Conant, the president of Harvard at the time, was one of the people who started the National Science Research Council, which was a government agency but under the Office of the President. It was devoted to utilizing university research capabilities for wartime research. This, of course, was before we were in the war, but right after the fall of France. This was announced in the paper. Kistiakowsky knew about it and said, "We've got to go over there and talk to Conant and see what we can do." We did that. Conant said, "Since you're chemists, why don't you work on explosives?" I had no experience whatsoever with explosives. Kistiakowsky's experience was multiple, but accidental, in this laboratory! [laughter] But we agreed to do that. I got a few people together, and set up a small committee to look at explosives. Ultimately John Kirkwood was the leading person. A little later we got John Von Neuman. He was terrific and, of course, very, very important. One of the things he wanted to do was to repeat the old experiments of [Ernst] Mach for whom the "Mach 1," "Mach 2," etc., terminology is named. Mach had done the experiments with smoked glass plates and electric sparks. This made a shock wave extending out from the spark and blew the soot away in patterns. Mach discovered that the interaction of two finite shock waves gave a special effect. We went over to Radcliffe College and permanently borrowed their ancient Wimshurst electrostatic machine.

FERGUSON: Did it have a continuous belt that generated...

WILSON: You cranked it at the end of a glass disk, or something like that. At any rate, it made static electricity. We just repeated those old experiments of Mach in the laboratory, plus additional ones suggested by Von Neuman. This turned out to be very important.

FERGUSON: Did you do this yourself or did you have students?

WILSON: That particular thing was being done by Bill [William D.] Kennedy, who later became director of research at Tennessee Eastman, and accidentally died three years ago. I don't want to go into too much detail on that. Next they set up an explosives division in this organization. At first Kistiakowsky was vice chairman, and then he was head for a while. One of the things they wanted to do was to set up an underwater test laboratory. They asked me to do that. I went around looking for a place to do that and by accident ran into some people I knew in New York, and ended up setting up the laboratory inside the Woods Hole Oceanographic Institution. It started with Bob [Robert H.] Cole. He was a student of Van Vleck's, who went on to Brown University. A fellow named [John P.] Slifko was assistant under Cole. We ended up at the end of the war with about a hundred people. I think about twenty-five or thirty of them were Ph.D. scientists. We studied physics of explosives, explosions, and power differences in chemical mixtures. One of our accomplishments was to persuade the navy to change their explosives from TNT to a more powerful new kind, the making of which was done by other people in the same National Defense Council under Kistiakowsky's group. Testing the safety was done in their laboratory in Pittsburgh. The power was still at Woods Hole, with both Mayer and Moore. We also worked on fuses and a variety of other things. We were ultimately set up to measure pressure waves from explosions in air and in water.

FERGUSON: What kind of devices did you use to measure the pressures?

WILSON: They were piezoelectric gages. We owe a great, great deal to the British, I should say, because we never found a single earlier report on any of these subjects. We never got our hands on any. I don't think there were any in our military establishment. The British had an elaborate classified exchange of information. There were lots of interesting reports, one of which I worked on between the wars. One of these was using piezoelectric gages, but they were great big things. They didn't have any amplifiers in those days--vacuum tubes were just coming in. At any rate, that's what we did. We certainly worked hard. We did learn how to measure these pressures. It was difficult to get it right the first time.

FERGUSON: I think this was your first exposure to vacuum tubes and electronics except for perhaps your radio at home?

WILSON: I'm sure I'd seen a radio as a boy. I guess we didn't have much. I certainly didn't do anything with electronics, if I remember, before that. I didn't do much in the war. Bob Cole was in charge of electronics for the water work and Kennedy was in charge of the whole thing for the air blast. George Frankel from Columbia was the electronics man for the air group.

FERGUSON: I brought this up because I recalled that you said when I was working for you that after the war you had an impulse to smash every vacuum tube you ever found. Were they that unreliable? Did you have a lot of problems with them?

WILSON: [laughter] Yes, I'd say we had a lot of problems. I don't know, millions, billions, trillions--I don't know how many vacuum tubes were made. We made requisitions by the hundreds. We'd dump them overboard, break them, throw them away and get the next ones. Vacuum tubes were being produced at fantastic rates during the war, for military purposes. I don't really have anything else much to say. They were awfully good people there.

FERGUSON: Did you live at Woods Hole during this time?

WILSON: I moved down with my family. I was first in charge of organizing this laboratory, and the director of it. It was a sublaboratory; we were inside the Woods Hole Oceanographic Institute. We called ourselves the Underwater Explosives Research Laboratory. We did have our own contracts through the Institute. Of course the main job was desperation--getting people. There was the radar group at MIT, the Los Alamos project, and, of course, many others, gobbling up every scientifically trained person in the country that they could get their hands on.

FERGUSON: Was Don [Donald F.] Hornig in that group?

WILSON: Yes, Don Hornig had started his graduate work with me, and had started on his thesis. He was in the thermocouple business. He moved to Woods Hole when the rest of us moved. George Kistiakowsky was my boss in the organization. He was the division chief of this explosives work. He was transferred to Los Alamos, and he went on a manhunt and grabbed Don Hornig. They went out there, and Don was very important to him at Los Alamos.

FERGUSON: Then you went on to another job?

WILSON: Yes, at some point, they reorganized the system. We stood off as Division 2. Division 8 was the explosives. Division 2 was for the effects of the explosion. It originally started out as passive protection from bombing, but they reorganized it. I became a civil servant at that point. My official location was Washington. Woods Hole was a sublocation. Maybe it was the other way around, but the office was in Washington. My contract responsibility was Princeton and Woods Hole, primarily, and a separate operation at Duke. So I made the round trip every week from Woods Hole to Washington to Princeton to Providence, and took the boat across in the morning to Woods Hole. It was quite an operation.

FERGUSON: You continued to live in Woods Hole?

WILSON: Yes.

FERGUSON: Who was minding the store at Harvard while you were away? Were your courses taught?

WILSON: Well, actually, there weren't very many people. There wasn't much to mind. There were a few girls around, and very few boys. They threw Radcliffe and Harvard together at that point and Ralph Halfred, who had been at Harvard as a research postdoctoral fellow with George, stayed and taught both our courses. They consolidated a lot of things, but he stayed and did the teaching.

FERGUSON: While talking about your government service, can we jump ahead and talk briefly about your work with the Weapons Systems Evaluation group in Washington?

WILSON: The Korean War got me into that. I was in England when they asked me to join it. Robertson, the professor I did my senior thesis work with in quantum mechanics at Princeton, was the research director of this operations analysis group which worked for the secretary of defense and for the Joint Chiefs of Staff. They were the customers and Robertson was research director. They had a military general at the head of it, a man named General [Geoffrey] Keyes. Robertson had been there for a couple of years. It was the situation in the government sometimes where, if you can't get a successor, you stay. So you work to get your own successor to get out of there. At any rate, they recruited me for this job. You see, the only reason why I thought I could conceivably have any competence there was that I'd begun thinking about this scientific method business and statistics at Woods Hole.

FERGUSON: Actually, you wrote your book An Introduction to Scientific Research (36) before this job though, didn't you? It was issued in 1952, I believe, and it seems to me that you worked on that at Oxford.

WILSON: I worked on that at Oxford and that, of course, was earlier. That's perfectly true.

FERGUSON: I think there are one or two points in Kistiakowsky's article in this Festschrift issue where I thought the dates were not factually correct, or the sequences were not entirely correct, according to my recollections (1).

WILSON: I think that's completely true. I've found errors in there. I've put the correction somewhere.

FERGUSON: It was really your experience though, and expertise in the scientific method and operations research that...

WILSON: I had no obvious professional qualifications whatsoever as an operations analyst. I hadn't done anything. I didn't belong to that crowd.

FERGUSON: What did you think about the quality and the competence of the people you worked with in Washington?

WILSON: It was a mixed bag. First of all, we had hardly any civilians. It was pitiful. We had about five or ten civilians. We were supposed to have a lot more than that, but they hadn't recruited them. So when I went down there, the first job I had was to find some people. I did find some, and it was a heck of a job, because after I'd found them they had to be cleared, and it took forever to get them cleared. Then, the Republicans came in and they froze the situation. I'd had civil service approval to find these bodies. They'd take them on if I could find them. As soon as I'd found them, Charlie [Charles Erwin] Wilson, the secretary of defense, froze everything. I had these people giving up their jobs, and they had nothing to eat. They couldn't take the new job, but they'd given up their old one. One of those was George Raft. You might say he's quite important in the effects of the nuclear weapons, the control of nuclear weapons. He has sort of a public policy professorship at MIT.

There were a few very good people, but there were just hardly any civilians. The rest of the group was military. We had General Keyes, who was a lieutenant general, at the head.

We had an army general and a navy admiral. We had three flag-rank people. Then there were colonels galore, and a few lieutenant colonels, and that's as low down as we got. Well, we had one sergeant, and he did all the work of course! [laughter] It was rather funny. I can hear the orders. I'd talk with General Keyes in his office. Then I'd go back to my office and I'd hear him call this executive man in. Then I could hear him going to the next officer, and then the next officer, as the orders were passed down. I knew where they'd end--in the sergeant's lap!

FERGUSON: I assume you thought this multi-tiered bureaucracy was pretty inefficient.

WILSON: It was a very strange thing. You couldn't really judge. I'll pass on that one, because I think it was too special a kind of organization. I'll just say this: the thing that upset me terribly was the interservice rivalry. It was absolutely incredible and childish in the extreme. For example, the wording of a directive was important. We'd get directives from these people and they'd fight for weeks over the wording of the directive because it might give the navy an advantage or vice versa. Interservice rivalry was terrible. Of course, it still is.

FERGUSON: Before we finish I want to talk a bit about your family, and maybe this is a good time. Tell me a little bit about Emily Buckingham.

WILSON: I'll start with her father. Her father was a professor at MIT in mechanical engineering. I guess he was probably the world's greatest authority, or certainly a very important one, on gear business, gear theory--many books and tables and so on, and design of gears. He did a lot of very important consulting on gears. His wife was a fine person, but was not technically minded. Emily was working for a master's degree in physics.

FERGUSON: Did she go to MIT?

WILSON: Harvard. She was a Radcliffe student. She stayed on for a master's degree, and was teaching at Wellesley when I came back from Caltech. We met and got married. Kenneth was the first child. He's here today giving a lecture.

FERGUSON: Yes. I'll jump out of sequence a little bit. You've nurtured at least a couple of Nobel prize winners--one by parenting, and Dudley Herschbach. Anybody else?

WILSON: I think that's it.

FERGUSON: Did you get to go to the award ceremony for Kenneth?

WILSON: Yes, we did. He was very generous and sent the whole crowd, and it was quite a crowd. We had a marvelous time. They do a terrific job, you know. They really work at this thing, year after year after year. I don't see how they put up with it, but they do. The king and the queen were on deck for the occasion for nearly forty-five minutes.

FERGUSON: Were you surprised [about the prize]?

WILSON: I was not surprised. I was surprised earlier, when he got promoted to tenure, because I had misjudged the situation myself. I remember talking to Kenneth about publications.

FERGUSON: About his publications at Cornell?

WILSON: Yes, about the necessity of publishing things in order to get ahead in the academic world. Well, in his usual manner, he said nothing. Very soon thereafter I discovered that Cornell was equal to the task of recognizing his abilities without requiring a long list of publications. The publications came later, after he got tenure. I think it's probably Hans Bethe that's mostly responsible for that. So, yes, the Nobel prize wasn't a surprise, because people talked about it so much around here. Physicists would come to me and say, "Oh, he's sure to get it." I was a little bit annoyed, because time passed and he didn't get it. Those things aren't all that rational to ensure that the father isn't right in the first place. But it finally did happen.

FERGUSON: On to David. I confused you yesterday because I was asking about David and thinking about Kenneth.

WILSON: Right. David is a full professor, also at Cornell, but in the field of biochemistry. He's an active man, and he's done some nice work. Right now he's scientifically in a very good situation.

FERGUSON: And Nina?

WILSON: Well, we're still in the Ph.D. category. Nina got a degree in economics, and married a physicist. She's had a number of important jobs, but she was with the staff of the Federal Communications Commission until the Republicans came in. This was a presidential appointment, so she had to resign. Now she's in sort of a private practice as a consulting economist. I think that's what she's doing now. She was the consultant for the football league that was suing the other football league!

FERGUSON: Did Emily get you interested in canoeing and outdoor life, or vice versa?

WILSON: I always liked canoeing at Princeton, and did a lot of that. She certainly liked it, and was very enthusiastic about it. But I can't say that she started me on that.

FERGUSON: Now, Thérèse, is that pronounced right?

WILSON: My pronunciation isn't right either--never has been--but that's the best I can do. After Emily's death, I met Thérèse here. She came to this country as a postdoctoral fellow with George Kistiakowsky. She had the equivalent of a Ph.D. from the University of Brussels in physical chemistry. She won a year's trip to America and came to work with George.

[END OF TAPE, SIDE 5]

WILSON: Going back to Emily, she got her master's degree, and she taught for a year. Then Kenneth came along and she stopped working. Our best year really was in England, at the time of my sabbatical after the war. That was a very successful year. I wrote the method book. We had a really, really old house there. It was a beautiful place. We were right next to a little village. Emily got really incorporated into the life of that village. She used to play the organ for the Sunday school and go to the women's club meetings and it was a wonderful year for both of us.

FERGUSON: How about the year in Washington? Did Emily and the family enjoy that?

WILSON: Well, not too terrifically, I'd say. You know, it was all secret. I couldn't talk about what I was doing at all. We only had one car, but I didn't drive every day. It wasn't an overwhelming success.

FERGUSON: Did you and Emily talk a lot about science?

WILSON: To a certain extent, but not in detail. Yes and no. Probably too much. Kenneth obviously had absorbed it. He was demanding of my time.

FERGUSON: I remember the summer we stayed in your house when you were out working on the book with Decius and Cross (34). I ran across some of Kenneth's math papers and I was just overwhelmed with what he was doing at that time. It was rather advanced group theory of some kind.

WILSON: He was certainly very advanced and questioning, questioning, questioning, all the time. Well, going on, I did meet Thérèse at a critical point.

FERGUSON: I interrupted your comments earlier. You were in England when Emily became ill suddenly?

WILSON: That's right. In 1954 I was in England just for a trip on government business. It was very dreadful for me. First of all, I had a great difficulty in communicating, because the telephone system was so bad. There was really only one public telephone at Oxford. Of course, it was either occupied or out of order half the time. Then getting back was slow. I went over on a MATS, Military Air Transport, under our contract. As soon as I got the word that Emily was sick and it was serious, I tried to get home, but it took a few days to get on a plane. It was rough. Then she died almost immediately after I got home.

FERGUSON: It must have been very fast.

WILSON: It was very fast.

FERGUSON: Were there any indications earlier?

WILSON: Earlier she hadn't been in good health, but people had never figured out what it was. I don't know why they didn't.

FERGUSON: It was leukemia?

WILSON: Yes.

FERGUSON: Now on to Thérèse.

WILSON: We met and married pretty quickly, within a few months. Of course, I wasn't going to let her go back home. So we got married quickly. Now, as I say, she has the equivalent of a Ph.D. She was doing research with George.

FERGUSON: Did she continue with her research until the children came?

WILSON: She continued with her research at first. Then I had a sabbatical after a couple of years. We went to Geneva for the sabbatical. Ann was born in Geneva. The three children are a year apart. Ann and Paul are a year apart, plus or minus a couple of days. Steven was a year and two months later.

FERGUSON: What are these children doing?

WILSON: Ann first. Ann has had amazing experiences. She has been on about seven or eight expeditions to the jungles of the Amazon in Peru, studying a special kind of monkey. She's writing her thesis now at the University of Michigan in animal behavior, especially these little monkeys. She has found that they are quite special in that they often have two males and one female in the family. As I say, she's been down there, and she's accumulated a vast amount of detailed data on these behavioral qualities. She's married. I don't know what will happen. She's now writing a thesis and, I think, this spring she'll certainly get a degree. What will happen next we don't know.

FERGUSON: The next is Paul?

WILSON: Paul is a graduate student at Berkeley in biology, but in an entirely different area. He's working with frog embryos and how they grow and differentiate, and particularly how they control patterns.

FERGUSON: It's somewhat similar to Paul Doty's current interest in differentiation of collagen.

WILSON: Yes. Well, this is a great field to study, this question of why does this cell become an ear and not a big toe and so on. Paul is quite mathematically gifted. He's had a checkered career so far. He did physics as an undergraduate. His thesis was an application of general relativity. He went from there to London. He spent a year at the London School of Economics, where he got a distinction of some sort, but I don't remember what it was called. Then he worked for a while for an MIT professor, who had a big contract from South America. He worked on that, and he actually briefed these South American people on the results of the MIT work at a big meeting they had down there. Then he switched over to biology, which he had studied quite a bit of in Princeton, and he's still working on it. I think he's well launched. He's been out there for three years now. They've treated him extremely well. I'm really enormously impressed by the way this group of professors have treated Paul.

FERGUSON: Now Steven.

WILSON: Steven went to Harvard, but he didn't finish then. He dropped out from sociology. He dropped out at the end of his junior year, and set up a little company. He'd worked as a technician in the biology building, making little gadgets that were essentially analog to digital boxes with controls. He developed a little box that you can connect up to a home computer and to some apparatus, to automate the scientific apparatus. The first box he made is still running. He made several of them which were hand-wired. Then he decided, "Well, if there's a need for this thing, why not make a company?" He started this little company some years ago. I'll be careful with my remarks, but another company bought his product from him, and it's still on the market. In fact, it's now apparently switched over to Honeywell. They are marketing this thing as if it were new. Apparently it's exactly the same thing. That was his first product. He gave that to the other company for a price. Then Steven decided that he would try to get into the use of personal computers in process control work. He's got a real company, and he was able to get financing. He first got one man who financed his earlier operation, and this man is still involved in the thing. But now there's a whole group of five or six venture capitalists. They've put quite a lot of money into his company.

FERGUSON: What's the company called?

WILSON: Data Acquisitions System, Incorporated.

FERGUSON: I think I've seen their literature. I haven't bought anything from them, but I know about it. I want to skip back from your family to your scientific career. Paradoxically, the tape got very noisy at the point where you were talking about 1/f noise and Stark modulation. [laughter] So let's clarify that a little bit. I particularly missed what you had to say about observing the spectrum of ammonia and the vast signal-to-noise improvement you saw.

WILSON: Of course, the reason why it was better (I didn't know it at the time) was that 1/f noise was the limiting noise. When we were Stark modulating at a hundred kilocycles, we'd switch from practically zero frequency in the ordinary non-modulated system--just the sweep frequency--to a hundred kilocycles. That made quite a difference in the 1/f noise. As I say, I didn't know about that at that time. We were doing it because it seemed a good idea. It's a primitive notion to modulate. You throw out stuff that doesn't associate with modulation. It's a very striking improvement in the signal-to-noise, certainly a number of powers of ten.

FERGUSON: You also published a paper with [Richard H.] Hughes on an electrical secular determinant solver (38).

WILSON: Yes. Well, let's forget about that.

FERGUSON: [laughter] I didn't want to forget about that. You had it hanging on the rack, in your lab, when I was in school here. It didn't work well?

WILSON: It worked, but it's an analog. You don't use analog machines anymore. It was too much trouble to change the parameters and so on.

FERGUSON: This was a set of pots and resistors?

WILSON: Yes, it was electrical. This was copied after an earlier mechanical thing that we'd built. The mechanical thing was a sort of pendulum and springs. It was strictly analog.

FERGUSON: Did you publish on the mechanical one? I seem to recall it, but I don't think I was able to find it in the publications list.

WILSON: I don't think we did. We built one that worked pretty well, but it was a nuisance to use. That isn't worth spending time on.

FERGUSON: Are these devices still around?

WILSON: No.

FERGUSON: I also missed part of what you were saying about the infrared intensity measurement work with Jud Wells (30). Could you describe that once more?

WILSON: That was something rather simple-minded. I liked it. Jud made it work. The problem of measuring infrared intensity, as you well know, is difficult. You don't have a simple system for gases. The main difficulty is that in a typical situation you have a series of rotational transitions, spectral lines. As you shine light through the gas, in ordinary circumstances, you're measuring mostly how much space there is between the lines, rather than the lines themselves. [Typically, it is similar to looking at light through a comb.] Other people had worked on this problem. One approach was to extrapolate to zero absorption length. High resolution helped [but aggravated the comb effect]. Anyway, the scheme we had beats the problem ["by (a) eliminating the violent fluctuations in intensity with frequency by broadening the rotational lines with a non-absorbing foreign gas and (b) eliminating the error due to the intensity variation of the envelope by extrapolating the apparent integrated absorption coefficient divided by the partial pressure to zero partial pressure of the absorbing gas. These two steps permit vibrational intensities to be measured to a reasonable accuracy even with a spectrograph of low resolving power"--from the abstract of our paper.] Jud Wells tried it out, and it worked fine. It is in use still.

Nowadays, of course, it's not going to stay in use very much longer because of Fourier transform infrared spectroscopy, which is much more convenient, and cheaper. That's the only way people are going to do things. With the high resolution you can get now, you should change the system, the emphasis.

FERGUSON: You said a little bit about Bryce Crawford and we didn't get much on that. You did work together with Bryce when he came here, didn't you?

WILSON: Definitely. We had a group that consisted of Bryce and Fred Stitt--both had NRC fellowships, I believe; Jack Linnett, who had come from Oxford to work with Kistiakowsky, who passed him on to me; Harold Gerschinowitz, who eventually became the director of research at Shell Development; and various others, of course--Jud Wells, Jack Howard. It was fantastic.

FERGUSON: Do you consider your paper with Crawford to be an important one?

WILSON: The nice one with Bryce Crawford was the handling of internal rotation and vibration. The paper with Crawford I wanted to mention was "The Normal Vibrations of Molecules with Internal Torsional Motions" (39). I think that was a beautiful paper. Bryce and I wrote that in a hotel room in New York one night. We'd been thinking about it a lot, of course. I don't know who did what, but I would say that was a case in which Bryce and I both made real contributions to the method.

FERGUSON: We'll come back shortly to some specific papers, but I wanted to ask you about how many copies of the Pauling and Wilson quantum chemistry book (10) were sold. Do you recall?

WILSON: I never found out. At some earlier point we used to know the numbers.

FERGUSON: But it had a large circulation?

WILSON: It certainly had a large circulation. It's still in print. McGraw-Hill has an Oriental edition, or a Japanese version. Now Dover Publications has taken it over here. I don't know how many they're selling, because they don't send me the circulation figures.

FERGUSON: How about An Introduction to Scientific Research (36)? Was it widely distributed?

WILSON: Not quite as widely, and not in the same degree as the quantum mechanics book. That still is in print, in paperback. McGraw-Hill discontinued the hard cover edition.

FERGUSON: How about the book on molecular vibrations with Decius and Cross (34)?

WILSON: Again, not the same as quantum mechanics, because it wasn't a textbook at all. It's been quoted and cited, tremendously. Dudley [Herschbach] has some kind of a story about a time curve on the number of papers in the field and how it broke and shot up when this book came out.

FERGUSON: Was there something new in the book, or was it just putting together pretty much what had been published in papers before?

WILSON: I think most of the material in there I had published before. I had actually written the book before the war. But time really flies. Jack Decius really wrote the new, published version.

FERGUSON: Was he one of your graduate students?

WILSON: He had been a graduate student and then went to Woods Hole with us and went back to Brown. Yes, he was very, very able.

FERGUSON: How about your most cited papers?

WILSON: Most likely, undoubtedly, the books, but they don't count that. Two papers on the F-G method have been cited, apparently, a very large number of times. One journal put it in their monthly list (33).

FERGUSON: Yes. I think it's Science Citation Index. Well, we started just trying to summarize what you thought were your most significant scientific contributions. We didn't get through this systematically.

WILSON: You can catalog or you can classify things according to different kinds of importance. One of them is, did other people use it?

[END OF TAPE, SIDE 6]

WILSON: I think the F-G method is the most quoted. The Stark effect microwave spectrometer has certainly been widely used. On the other hand, I have to say that if Hughes and I hadn't done it, somebody else would have done it very soon. In fact, some people did. The Westinghouse people just didn't do anything about it. It's still what people use today, mostly, although not quite exclusively. The intensity in the infrared was important. We did a paper on intensities in the microwave, not as good as the intensities in the infrared, not as important. It was more obvious. We pushed a lot on double resonance in microwave spectroscopy, and I'm still pushing for double resonance. I think it's a great thing. The students did a lot of the theory with me. They seemed to get the most out of the microwave spectra. I remember John Bragg worked on the the nuclear quadrupole interaction. Sidney Golden worked on the dipole moment determination.

FERGUSON: Was this the paper on the Stark effect in microwave spectroscopy (40)?

WILSON: Yes, on the determination of dipole moments. What he got was the effect of the electric field on the spectrum. Well, that was important. It has been quoted and used a great deal. You know, all these things, after a while, look sort of evident. You want to say, "Well, that's obvious." But it wasn't obvious back then. What I would say is these effects, like quadrupole and electric field and centrifugal distortion and a number of other things, owe a tremendous amount to the students, who often took the lead. Internal rotation was a good example. Of course Dudley Herschbach wrote the definitive work on that, after we'd been playing around with it in various ways. It's customary nowadays for you to say how much you owe to your graduate students. I think we're all in the same boat to varying degrees. But when you look at what has happened to my former graduate students, how could one fail to be involved with tremendously good work, with students like that.

FERGUSON: Did you anticipate that Dudley Herschbach was going to win a Nobel Prize?

WILSON: No. I knew he was about the best student I'd had. He was terrific.

FERGUSON: Well, I think we have done as well as we can. It's been a delightful interview.

[END OF TAPE, SIDE 7]

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