



Peter Wyllie, 1986

PETER JOHN WYLLIE
(b. 1930)

INTERVIEWED BY
SHIRLEY K. COHEN

March 19 and 29, April 2 and 9, 2002

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California



Subject area

Geology and planetary sciences; physics

Abstract

An interview in four sessions, March and April 2002, with Peter John Wyllie, professor of geology, emeritus, in the Division of Geological and Planetary Sciences. Dr. Wyllie received his undergraduate and graduate education at the University of St Andrews, in Scotland (BSc, geology and physics, 1952; BSc, geology, 1955, honors; PhD 1958). Joined Caltech faculty 1983 as chair of the Division of Geological and Planetary Sciences, remaining chairman until 1987. Served as divisional academic officer 1994-1999 and became professor emeritus in 1999.

In this interview, he recalls growing up in England, his grammar school education, national service, undergraduate career at St Andrews, and participation

in two British Greenland Expeditions (1950 and 1952-1954). Graduate work with Harald Drever at St Andrews, continuing under O. F. Tuttle at Penn State. Postdoc at Leeds University, U.K., 1959-1961. Returned to Penn State in 1961 as associate professor of geochemistry. Moved to the University of Chicago in 1965, remaining for eighteen years and chairing the Department of Geophysical Sciences before coming to Caltech.

He discusses his division chairmanship, teaching the introductory geology course, and the evolution of the division's curriculum, particularly under chairman Edward M. Stolper. Recalls his involvement with various geological societies and receipt of several awards, including Wollaston Medal (1982) and Roebling Medal, Mineralogical Society of America (2001). Discusses his work on the National Academy of Sciences' first national survey of earth sciences. Discusses high-pressure experimental petrology at Penn State, Chicago, and Caltech with various graduate students and postdocs, including Gus Koster van Groos, David Watkinson, John K. Robertson, Wu-Liang Huang, and Who-jeer Lee. Comments on the current state of the division as it moves into an era of global change and collaboration with environmental engineering.

Administrative information

Access

The interview is unrestricted.

Copyright

Copyright has been assigned to the California Institute of Technology © 2004, 2017. All requests for permission to publish or quote from the transcript must be submitted in writing to the University Archivist and Head, Special Collections.

Preferred citation

Wyllie, Peter John. Interview by Shirley K. Cohen. Pasadena, California, March 19 and 29, April 2 and 9, 2002. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Wyllie_P

Contact information

Archives, California Institute of Technology
Mail Code B215-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu

Graphics and content © 2017 California Institute of Technology.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH PETER JOHN WYLLIE

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

Copyright © 2004, 2017 by the California Institute of Technology

TABLE OF CONTENTS

INTERVIEW WITH PETER JOHN WYLLIE

Session 1

1-10

Family roots in London; father's work for Shell Oil, Venezuela. Grammar school in London; Harkness residential scholarship to University of St Andrews, 1948; signs up for geology course; university career preceded by less than 18 months' national service with Air Training Corps. Boxing since age seven or eight; bout at Queensbury Club, London, 1945, age fifteen; RAF Scotland heavyweight boxing champion 1949.

10-26

Life at St Andrews: plays rugby, captains boxing team; president, Athletic Union; Miller Prize for best graduate in science, 1952. Golf at St Andrews. Prof. H. Drever recruits him to geology. Member British West Greenland Expedition with Drever, summer 1950; breaks neck in rugby match, second year. BSc in physics and geology, 1952. British North Greenland Expedition 1952-54. First-class honors degree, geology, 1955; three-year PhD program supervised by Drever. Marries Romy 1956; joins O. F. Tuttle's high-pressure group in experimental petrology, Penn State, and continues work on PhD. PhD in geology, St Andrews, 1958, in absentia. Looks for postdoc in U.K.

27-36

Research fellow in chemistry, Leeds University, 1959; builds high-pressure lab; becomes lecturer in geology. 1960, International Geological Congress, Copenhagen, and International Union of Geodesy and Geophysics meeting in Helsinki. Lack of excitement in Britain re research. In Copenhagen, meets friends from Penn State; returns to Penn State 1961 as assoc. prof. of geochemistry, having declined offer of asst. professorship at Caltech. Moves to University of Chicago, 1965; neighborhood and children's schooling; master of the college, assoc. dean of Division of Physical Sciences, chair of Dept. of Geophysical Sciences. Publishes *Ultramafic and Related Rocks* (1967), *The Dynamic Earth* (1971), *The Way the Earth Works* (1976). Quantrell Teaching Award for excellence in undergraduate teaching, 1979.

Session 2

37-48

Declines invitation of department chairmanship, MIT, 1980. Comes to Caltech as chair of Division of Geological and Planetary Sciences, 1983; division's reputation for dissension. Chairmanship and interaction with provost R. E. Vogt. Feels unappreciated; resigns division chair, 1987; replaced by G. Wasserburg. Foreign associate, U.S. National Academy of Sciences, 1981; other foreign academy memberships. Wasserburg's tenure as division chair, conflict with J. Kirschvink, and 1989 resignation.

49-59

Research on magmas and volatile components under high-pressure conditions, with two high-pressure presses he brought from Chicago to Caltech. Teaches introductory geology course. Curriculum revision; the importance of earth sciences education. Works to have earth sciences included in “menu” curriculum; undergraduates’ increased interest in geology as a result.

59-66

Works with H. Daily, director of public affairs, to send graduate students into Pasadena schools to stimulate interest in science. Joins Caltech’s Committee on Secondary School Relations, 1990; involvement with YESS [Young Engineers and Science Students]; work of J. Bower; involvement with CAPSI (Caltech Pre-college Science Initiative). Academic officer for division 1994-1999; E. M. Stolper as chair; reorganization of division’s undergraduate curriculum into comprehensive course sequences. Chairman, Student Affairs Review Committee, 1994.

Session 3

67-78

Romy’s role as chairman’s wife. General British lack of incentive to pursue research. Involvement in administration of various geological societies. President of International Mineralogical Association. Controversy in IMA when decision to meet in Beijing in 1990 is upheld after Tiananmen Square uprising, 1989; U.S. government’s banning of foreign nationals from scientific meetings in the U.S.

78-90

Vice president, 1991, president, 1995, International Union of Geodesy and Geophysics. Involvement with UNESCO 1999 World Conference on Science. 1988-1993, chair of National Research Council Committee on the Status and Research Objectives in the Solid Earth Sciences; committee conducts first national survey of earth sciences, resulting in 1993 report *Solid-Earth Sciences and Society*. Concern with earth systems science.

Session 4

91-102

His graduate students and postdocs: G. Koster van Groos on liquid immiscibility; D. H. Watkinson on limestone-assimilation hypothesis; carbonatite magmas ; J. K. Robertson on water-undersaturated experiments; W.-L. Huang (1970s) and W.-J. Lee (1990s) on the effect of carbon dioxide on the mantle. Papers with A. Boettcher (later Montana). Others. Awarded Roebling Medal, Mineralogical Society of America, 2001; Leopold von Buch Medal, German Geological Society, 2001; Wollaston Medal, Geological Society of London, 1982; Abraham-Gottlob-Werner Medaille, German Mineralogical Society, 1987.

102-111

Geology division’s current direction; disappearance of old guard; new faculty, blending of

seismologists into division; planetary sciences faculty; work on global change; collaboration with environmental engineering; lack of women faculty. Romy's contribution to Caltech through Caltech Architectural Tours; her book, *Caltech's Architectural Heritage: From Spanish Tile to Modern Stone*; made honorary Caltech alumna, 1996.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Peter J. Wyllie
Pasadena, California

by Shirley K. Cohen

Session 1	March 19, 2002
Session 2	March 29, 2002
Session 3	April 2, 2002
Session 4	April 9, 2002

Begin Tape 1, Side 1

COHEN: Good morning, Dr. Wyllie. It's good of you to come and do this interview with us.

WYLLIE: Good morning.

COHEN: Maybe we could start by having you tell us a little bit about your family—your father, your mother, what they did, and where they lived.

WYLLIE: They were both born in London, England. I was never quite sure what my father did before they got married. I know that he went off to Venezuela with Shell Oil Company as a camp boss. All I remember is that he said he used to run the peons, whatever that meant. Since then I've learned a little bit about oil companies and drilling. There's a lot of labor. The jungle was pretty rough in those days. I guess he just got the people to work, and made them work, and rounded them up. I know he spent a lot of time on a horse and a mule. This was before he got married, in his early twenties. How he joined Shell, and why he joined Shell, I don't know. School-leaving age in London, if you weren't in a private school, was about fifteen. He and my mother both left school at that age. What he did until his early twenties, I really don't know. I know he went off to the jungle and came back and got married. They were married a year before I was born. I was born in 1930, so he must have been in the jungle from '27 to '28, something like that.

COHEN: He must have had a lot of stories to tell about being in the jungle.

WYLLIE: Yes. We grew up with the phrase, “When I was in Venezuela,...” and then out came a story. One of our playthings was the skin of a great big boa constrictor—a thirty-foot-long skin that we used to stuff with newspapers, and sew up, and play with. When he came back, I guess he had some money saved up. I don’t know what he did at first. Well, he was running a banjo group or something.

COHEN: So he was a musician of some sort.

WYLLIE: Not after I came into cognizance. But apparently when they were in Venezuela they played music. Oh, he was also going to write adventure articles for magazines, because he had a couple published. So he came back and thought he was going to be a musician and writer.

COHEN: That’s interesting. So one was educated even if one left school at fifteen in England at that time.

WYLLIE: I don’t know. My mother and father, by the time I knew what was going on, were both pretty well educated, but they were self-educated. I guess they didn’t learn much in school. They learned enough to learn. My mother was an avid reader. She read books all the time. She got me to go to the public library despite the fact that I had plenty of books at school. Mother was a waitress when they got married—a waitress at some London hotel, I don’t know where. He was born in the Cockney country, and she was born in the East End of London, so they are both Londoners. My grandfather came from Scotland, I gather. He died before I was born. Oddly enough, he was a draper’s assistant in a small shop at St Andrews, which is where I went to university. My grandmother told me that, when she learned I was going there—I had never heard that before. So my parents were from simple, humble beginnings. My father had a sense of adventure and went off to the jungle and came back and got married. He tried independent work for a while, I guess—he thought he was going to write—and then he went back to Shell. I don’t know what he did there during the 1930s.

COHEN: Back to where?

WYLLIE: He went back to work for Shell Oil Company, presumably after he got married and found he needed money to live. His jungle money ran out, and I was born.

COHEN: That was in London—or in England.

WYLLIE: That was in England, yes. He went back to the Shell Oil Company headquarters in London during the Second World War. His job was cipher clerk. He was called up for army service. He was all packed up and ready to go—it must have been 1939, I guess—and then he was called back on the reserve list; he was needed for the war effort. Shell Oil Company headquarters in London needed experienced employees dealing with codes, sending the oil tankers around the world, keeping the fuel lines going. So he was saved—we had a father. He could easily have gone and we could have lost him in the war. So many kids did lose their parents.

COHEN: Sure. So you went to school in London, then.

WYLLIE: I went to school in London. In grammar school, I played all sports, was captain of several school teams, captain of the school—head boy—and I did very well in the national examinations. At that time in England, we had the dreaded eleven-plus. Do you know about that?

COHEN: Well, I know you took an examination that decided the course of the rest of your life.

WYLLIE: Yes, that's right, and my recollection is that about thirty percent of the kids passed it at the age of eleven. Of those who went on to a grammar school, some stayed on after the age of fifteen to high school—the sixth form, two years in high school—and about five percent went on to universities.

COHEN: What happened to the seventy percent who did not go to grammar school?

WYLLIE: They went to an ordinary school, where everybody left at about age fifteen.

COHEN: So that would have been just regular education, nothing special. But they did take the thirty percent and send them to grammar school.

WYLLIE: Yes.

COHEN: This was not what we'd call private school.

WYLLIE: No, no. There was a small percentage apart from this, the toffs, whose parents could afford to send their children to private schools—called public schools in England—where they spoke proper, upper-class English. The eleven-plus exam and grammar schools were part of the state school system. The people who went to private schools didn't have to take the eleven-plus. I knew nothing about universities, and nobody I went to school with did, either. The only thing I knew about universities as a teenager was the rowing competition, when the light blues raced the dark blues each year and everyone had a favorite. That race introduced the names of Cambridge and Oxford universities. I was about fifteen when we took the matriculation examination, a school-leaving certificate covering eight academic subjects. The headmaster told me, "Wyllie," he said, "you need to stay on in the sixth form." So I stayed on in high school. He said, "Wyllie, you need to go to a university." I told my parents, and they were pleased, but didn't know anything about it. [Laughter]

COHEN: Now, this supposedly didn't cost you any money. I mean, you just had to be chosen to be there.

WYLLIE: Cost could be covered in three ways: competitive scholarships, grants based on means tests, and work during vacations. I got a county grant for living expenses. I don't remember what it was, but it paid something. I also won a residential scholarship from the university. The headmaster wanted me to go to Cambridge, to his old college in Cambridge, so I went there for examinations and an interview. I was offered an exhibition. He said, "Wyllie, that's not good enough."

COHEN: What does "exhibition" mean?

WYLLIE: You competed for open scholarships and exhibitions. An open scholarship provided £100 a year and an exhibition provided £40 a year. It was less honorific.

COHEN: I see.

WYLLIE: It was competitive. It meant you were accepted and could go with honor and some support.

COHEN: Now, all this was by examination, I gather.

WYLLIE: Yes. You go to the university and you sit for an examination that involves writing for several days. You also have recommendations from the headmaster. When I got an exhibition, he said that what he really wanted in the school were plaques on the wall of students who had won open scholarships at different universities. He wanted a collector's list of open scholarships.

COHEN: He thought you were a likely candidate.

WYLLIE: Yes. He said, "You know, we had a line of scholars at the University of St Andrews before the war. I think maybe you should go and sit for one of those, and perhaps we can start that again."

COHEN: So you had your choice of where to go to take the exam?

WYLLIE: I had no idea where to go. After winning the Cambridge exhibition, he told me to go to St Andrews to sit for a scholarship there. So I said OK, and I went, and I got one. They awarded three Harkness residential scholarships each year—open scholarships.

COHEN: Would it have been more prestigious to go to Cambridge?

WYLLIE: In Britain, yes, Cambridge is the center of the universe. Cambridge University, with some justification, thought that they were the top place in the country.

COHEN: I think they still do. [Laughter]

WYLLIE: I know a bit more about Cambridge now, but I'm glad I went to St Andrews, because I think I had more opportunities there. At Cambridge I think I would have had to choose a major on arrival. For example, I would have chosen physics, I would have gone through physics, and I would never have discovered geology. In the Scottish system, you were admitted in either arts or sciences, and you had to take four science subjects before choosing a major. Physics, chemistry, and math were the basics, and I took geology as an extra subject. At Cambridge I don't think I would have done that. I would probably have taken physics, math, and applied math and gone straight through.

COHEN: I see. So that really worked out. The geology was because of your father's encouragement, I gather.

WYLLIE: Yes. My father had met petroleum geologists in the jungle environment and after the jungle environment. When everybody went off to relax, he played water polo; he boxed out there, too. I think he probably got into fights with people. He said that he found the geologists were the most compatible people he knew out there in Venezuela. He said, "If you have a chance, why don't you try geology?" So that's why I signed up for geology as my fourth science subject.

COHEN: One doesn't get much of an idea of geology in high school or before then.

WYLLIE: No, not at all—and in Britain none whatsoever. It was very channeled at that time. At the grammar school at the age of fifteen, in the fifth form, you decided whether you wanted to go into arts or science, and then you concentrated. So for two years in high school, I just took physics, chemistry, and pure and applied math. I had to take Latin, too, because the headmaster had me lined up with Cambridge as a target and Latin was required of all students entering Cambridge. It's much looser now. They're adopting some of the—what I think are much too free—aspects of American education. Now I think they have to take some general courses as well. We didn't in those days.

COHEN: Was all of this paid for by the government?

WYLLIE: Once I got one of the three Harkness residential scholarships, which paid for my full board and residence in a university residence hall for three years, I applied for a supplemental Middlesex County grant. I lived in the county of Middlesex, and that was a means-tested grant. I had passed all the required examinations. The means test was such that it was clear that my parents couldn't pay for me. So I had a grant, which was subsistence, just sufficient to live on, supplemented by jobs during Christmas and summer vacations. The grant didn't pay for cigarettes, which I smoked at that time. [Laughter] So I was independent of home from the time I was eighteen in 1948. Oh, before I went to university, I chose to do my national service, which nearly lost me my scholarship. They said at first, "No, you have to take the scholarship this year." Fortunately, an influential professor on the committee persuaded them that I should be permitted to serve my country first.

COHEN: You could have chosen when to do your national service?

WYLLIE: Yes. Students were permitted to postpone it until after university, and if you kept university going long enough, you hoped either national service would stop or you would get excused.

COHEN: Why did you go beforehand?

WYLLIE: I hesitate to say it was because of a sense of adventure, but that's what it really was. I wanted to see the world. I wanted to see something. Also, I had been in the Air Training Corps all through high school through the war. That was like—what is it like in this country?

COHEN: ROTC.

WYLLIE: Yes, the same kind of thing, except ROTC is officer's training in college and this was just cadets aged sixteen to eighteen years.

COHEN: Oh, OK, like the National Guard or something like that.

WYLLIE: No, the National Guard is for grownups ready for armed duty. This was more like a military cadet version of the Boy Scouts, run after school on school premises. We did a lot of marching drills, rifle-range shooting, and got to fly in Royal Air Force planes. So I had a sense of warmth for the Royal Air Force, and I just felt I wanted to see things. I'd always felt a bit sorry that I missed the war. [Laughter] I thought I wanted to fight in the war.

COHEN: You were of an age where you could see only the glamour and not the Purple Heart.

WYLLIE: Yes. I actually heard about the nasty side during the winters on the North Greenland expedition, from people who had fought in the war. I said how sorry I was that I hadn't been able to fight, and they said, "Oh, don't be sorry."

COHEN: How long did your national service last?

WYLLIE: Only eighteen months. It should have been two years, but what happened? I got a delayed call-up, because I was on a trip with the Air Training Corps, which they generously considered to be air force work, and then an early release so that I could catch the beginning of the university academic year, so I did less than eighteen months. I applied to go to Germany, to the Far East, to Malaysia, to all overseas postings that were available. I took my ten weeks, my basic training, and I trained in signals, becoming what is now called an air traffic controller. Then they posted me to Leuchars Airport, which is ten miles from St Andrews. So after all the training, I spent the whole time at the airport right near St Andrews, telling pilots when and how to take off and land. I was aircraftsman first class. Going into town for the weekends, I learned about the backside of St Andrews, the other side.

COHEN: So you finished this basic training and your service, and you didn't have to think about that again. Then you started at St Andrews. I gather this was a good experience.

WYLLIE: Oh, yes, yes—a wonderful place.

COHEN: When did you start your boxing career?

WYLLIE: My father started my brother and me boxing when we were seven or eight. We both had boxing gloves and practiced with him and in school. I joined a team at age eleven, and I have always boxed. He had boxed. He was in the Territorial Army at some stage, when I was a baby. The Territorial Army was the equivalent of the National Guard. He had two activities: One was the swimming team—he was on a London swimming team—and the other was boxing.

COHEN: He was very athletic.

WYLLIE: Yes.

COHEN: It sounds like he had a big influence on you.

WYLLIE: Yes. He was a very good dad.

COHEN: Do you want to say anything about this boxing career of yours?

WYLLIE: We didn't have much competitive boxing until age fifteen or sixteen. I don't think we had interschool boxing. We had a lot of intramural boxing, so I boxed all through school. Oh yes, I was actually with a boxing club in Twickenham, where I lived, and a couple of former pros trained us. So I guess I must have had quite a few fights, too—I've forgotten. But there was one I do remember, around 1945. I went to the Queensbury Club in London as one of the subsidiary fights of the main program. I don't remember who the real fighters were, but there was a little newspaper clipping which said "Young Giants Fight at the Queensbury Club." I was fifteen.

COHEN: So you were big. [Laughter]

WYLLIE: We were two young almost heavyweights, yes. I was very athletic, not because I had much in the way of skills but I was determined and big and strong for my age. I just used to rush at things. My brother says I play tennis like a flying elephant, for example.

COHEN: Your brother did all this stuff, too?

WYLLIE: Yes. Although he was a bit smaller, he compensated with more coordinated skills. The poor fellow has been confined to a wheelchair for the last fifteen years with multiple sclerosis.

COHEN: Was he also academically inclined?

WYLLIE: He went through high school, but he wasn't one of the five percent who went on to university. He finished three years in high school, did his national service, and carved out a career in singing and modeling for advertisements and organizing Mozart workshops and musical medieval banquets for conventions at hotels.

Oh, then in the air force I also had more boxing. A good way to get off various duties was to be on a sports team, so I was on the boxing team in Scotland. That was in 1949, the year I became Royal Air Force Scotland heavyweight boxing champion.

COHEN: That's good. I haven't interviewed anybody else who's been a boxing champion.

[Laughter]

WYLLIE: Then I went to St Andrews and I continued my boxing there—rugby and boxing were my only two sports. In high school I played everything. It's not like American high school, where you have to concentrate on one sport; you could dabble in everything. At St Andrews, I was in 1st XV rugby team, captain of the boxing team—Boxing Blue—and president of the university Athletic Union. I also did well academically, earning the Miller Prize in 1952 for best graduate in the faculty of science.

COHEN: Your schools were much smaller, though.

WYLLIE: Yes. Our grammar school, covering the ages of eleven to eighteen, with a drop-off at age sixteen, had only 600 students.

COHEN: That's very small. But they only had academic subjects.

WYLLIE: Yes. I never even learned how to chisel wood, which my son did very well at the Lab

School in Chicago [University of Chicago Laboratory Schools]. He became a very skilled wood craftsman. I have never even known how to use a plane properly.

COHEN: So you were at St Andrews. What do you remember when you think back on that time?

WYLLIE: I remember some dreadful chemistry classes, some interesting physics classes, and some fascinating geology classes. I remember playing rugby and boxing. I remember learning how to play golf, going out for eighteen holes before breakfast and getting around in a couple of hours. You'd just go out and you'd whip around the course.

COHEN: In the rain, too.

WYLLIE: In the rain—in any conditions, yes. Since the town owns the courses, and the students were residents of the town, for £1 we could play on any one of the courses anytime, except for summer. So you couldn't afford not to play.

COHEN: It sounds as though you did a lot of sports.

WYLLIE: Yes, until the second year, when I broke my neck playing rugby.

COHEN: That didn't mean you neglected your academic subjects?

WYLLIE: No, no. The first year I got the class medals in physics and geology, but I was only third in chemistry, which was really annoying.

COHEN: Then you went off to Greenland.

WYLLIE: Well, what happened was that during my first year, the professor of geology, Harald Drever, decided he would like me to become a geologist. I was in the line for physics. It was understood that I was going to do physics, but Professor Drever decided he'd like me to do geology. I've learned since then that he had a habit of— He's one of those people who saw things in black and white. He either liked a student's work or he didn't. If he felt a student was

good and would work, he would do anything for them. Students who weren't very good, he just didn't bother with them. Anyway, he decided I was a good student and would be a successful geologist, so he asked me if I would care to join him on the British West Greenland Expedition, which was going off in the summer after my first year.

COHEN: I see. So that was like a summer job.

WYLLIE: Not quite. No, no, because I was expected to pay my way. [Laughter]

COHEN: Ah, OK—an internship or something.

WYLLIE: Expeditions aren't jobs. Well, some of them are now, but they weren't then. This was 1950, and there hadn't been an expedition to Greenland since the Second World War. It's a British tradition to have polar expeditions, so there used to be lots of them. In the 1920s and 1930s, they were very active in Greenland. Drever had been to the west coast several times. He managed to get permission to go again in the summer of 1950, and this, I think, was the first one permitted by Denmark since the Second World War. It was going to be a small group of six people: three geologists from St Andrews and three Scottish climbers. He thought it would be a good experience for me to go along.

COHEN: It must have been appealing to you.

WYLLIE: Oh, it was a terrific thought, yes. I think I was only supposed to pay a modest amount for food or fare on the ship. I said, "Well, I'd love to go, but I've got to work this summer. I have to get a summer job." He said, "All right. We'll take you anyway." So he took me along as a guest—I have no idea what the financial arrangements were. We went to Copenhagen first. Then we had a ship that went across—it's nearly across the Atlantic, and around the corner into the Davis Strait. I think it took us four days on that ship to get across to Greenland and just around its southern tip. Then we had to unload and get in a smaller ship to go up the coast to the settlements. The ship was taking Danish colonial people in and out, you see. It wasn't a pleasure boat; it was taking people into Denmark's colony and all the settlements. Then we changed into a smaller ship and went up to more settlements. Finally, we got into a much

smaller ship and went up to—

COHEN: So it was a real adventure.

WYLLIE: Yes. Then we were dropped off at a little village called Igdlorssuit on an island, which was one where Drever had worked before. The locals were called Greenlanders then, even in 1950, but they were really a group of polar Eskimos who wore sealskin pants. If you think of the traditional picture of Eskimos, that was how the whole village looked. They had sealskin kayaks, they went out and caught fur seals and cod—we ate well on cod—but they also had a rowboat with an outboard motor on it.

COHEN: Just a little bit of civilization.

WYLLIE: Apparently Professor Drever had left it for them before the war. From the village, we went off on this little rowboat with an outboard motor. I was terrified—big waves, little boat. We went off to the mainland; and four of us man-hauled—pulled—sledges up the glacier, over the edge of the ice cap and down to another glacier that hadn't been visited before. We did some pseudoscience there. One of the climbers was a surveyor. Another time I was dropped on an island. We had three months out there all together.

COHEN: Would you have had daylight almost all the time at that time?

WYLLIE: Oh, yes, yes—daylight and mosquitoes twenty-four hours a day, terrible mosquitoes.

COHEN: It must have been cold if you were working on polar ice.

WYLLIE: Not very cold in summer—not like Chicago cold—but damp. It got terribly wet. Our sleeping bags got soaking wet when we went up over the ice cap—a lot of fog, a lot of mist. By the time we got back from that trip, my sleeping bag was soaking. I was really worried that I was going to get pneumonia. Another time, I was left on a little island with one Greenlander and his kayak. I was supposed to survey some rocks there, which I did. I didn't know a thing, but I collected rocks and I did my best. This Greenlander went off and caught cod every day. That

fresh cod was my food supply. Then one day three Greenlanders came by in their kayaks with fresh seal. I can still see this. They filleted the seal on a big rock. They opened it up and gave me a piece of the raw liver—hot, warm, raw liver—a special treat. [Laughter]

COHEN: I'd certainly get A hypervitaminosis from eating too much of that. I know whales do when they have too much vitamin A.

WYLLIE: We had whale blubber on the way up the coast, too. At one of the villages they had hauled up a whale.

COHEN: It sounds awful, but I guess if you're hungry, you eat it.

WYLLIE: I ate it because we were supposed to, and it was considered a delicacy—mukluk, I think they called it. It was quite nice. I've never sought it since then. [Laughter] So that was a great adventure for me, yes. Compared with Captain Scott's Antarctic tragedy, the adventure was nothing, but it was a summer expedition, it was out in the field, and I saw rocks. That's when I knew I wanted to be a geologist.

COHEN: I see. So you came back to school at the university.

WYLLIE: Back at school. Then at the first rugby match of the season—I think it was against a police team, it might have been a navy team—I got this broken neck, which was misdiagnosed as a contusion. I was X-rayed—contusion, abrasion, bruises.

COHEN: I read about this, and it strikes me that you are very fortunate to be here, because you could have done something to a nerve, or the main spinal column, or something bad. You must have a very strong neck. [Laughter] It stayed in place even though it was broken.

WYLLIE: Well, at that time they didn't know what it was. They just thought I had hurt my neck badly. I don't know if you've watched rugby football.

COHEN: Yes. It's rough.

WYLLIE: Yes. I was underneath a loose scrum, and I sat up. They had drifted off.

COHEN: A loose what?

WYLLIE: A loose scrum, a scrimmage—it's a big pile of people, and I was underneath. They drifted off me as the ball went away, I got up, and then they came back over me and jerked my head forward. So I spent the rest of the game lying down on the sideline. They whipped me off to the hospital after the game and X-rayed it; then they sent me home. The university doctor looked at the X rays and at my neck and said it was a contusion or something like that.

COHEN: It sounds a little like incompetence. [Laughter]

WYLLIE: Yes. The student union tried to get me to sue him, but I wouldn't get involved in that. So, just rest it, he said—no sports for a while. Then in January the Scottish University boxing championships were coming up, so I went back to the doctor and said, "Can I start training now?" He whipped me around and said OK. So I trained hard for ten days and went to the championships. I didn't win. I got close though.

COHEN: You can blame that on your broken neck. [Laughter]

WYLLIE: When I felt tingling in my arms and legs, I went back to the doctor, and he sent me to a specialist. The tingling in my arms and legs was enough to really scare me. Fortunately I went to see the doctor again. The main nerve things come out between the vertebrae. One vertebra was able to rotate and another one was cracked. The specialist—I still have a copy of the letter—wrote, "This boy is in danger and needs an operation right away." I went to hospital, where they reported "fracture dislocation of vertebrae C5 and C6." After the operation, I was lying on my back in a plaster cast for six weeks, I think. Then they put me in a walking cast, the kind you don't see anymore, called a madonna cast because it has a vague similarity to a nun's clothing. It sits on your hips and is a solid tube all the way up, holding the head in a fixed position so that the bone can heal. There's a cutout here, and a cutout at the top, and a cutout for your arms.

COHEN: For your face and the top of your head. It sounds terrible.

WYLLIE: I must have been wearing that for three months, walking around in that for three months.

COHEN: So it was a matter of lying down or standing up. You couldn't sit.

WYLLIE: I could sit, but I couldn't bend or look down. In my study, I used to lie on my stomach on the bed with my head over the edge, and I'd read books on the floor. For the second-year exams, they got a pulpit for me from a church so that I could write on an elevated, near-vertical platform.

COHEN: Well, I guess what you did, Peter, is prove that you're tough. [Laughter]

WYLLIE: Well, I figure my whole life has been a bonus since then.

COHEN: Have you had any trouble from this since?

WYLLIE: No, no, just some loss of neck flexibility. They took bone from my hip and fused the two vertebrae together. The only trouble I have now is looking at the backseat of the car when I'm driving. The surgeon did advise me to give up contact sports.

COHEN: So even though they didn't diagnose this to start with, they did a good job eventually.

WYLLIE: Not the people who initially served me. Eventually, when they sent me to the surgeon, the surgeon spotted it. So you're right, the university doctor was incompetent, because the surgeon looked at the X-ray and saw it immediately.

COHEN: So then did you go to Pennsylvania State University during your university career?

WYLLIE: That was much later, in 1956. This is what happened: After the first year was the expedition; the second year, broken neck, the year in plaster with exams; then came the third year; and I got my bachelor's degree in 1952. During that third year, the British North

Greenland Expedition was being formed, and Harald Drever thought this would be a great opportunity for me.

COHEN: Now that you knew some geology. [Laughter]

WYLLIE: I still didn't know enough geology. [Laughter] He was an Arctic enthusiast, and he thought he might turn me into an Arctic enthusiast. I thought, "Oh, boy; that would be terrific." 1950 was barely forty years since Captain Scott died, and all schoolboys in England grew up with Captain Scott of the Antarctic as a great hero. To do something like Captain Scott had done—not dying, hopefully. [Laughter] There was this spirit of polar exploration within a certain segment of the young population that was pretty strong. The thought of going on this big expedition was really thrilling for me. He set me up for an interview with the organizing committee. He didn't know the leader, but he knew a couple of the other people on the committee. One was a famous geologist, L. R. Wager, who had been on a 1933 Everest expedition and had published classic research on rocks in East Greenland. So I went down to Oxford to be interviewed and got the job of assistant geologist. There were two geologists: senior geologist and assistant geologist.

COHEN: Was this before you had your degree?

WYLLIE: This was prior to my degree, yes, but they knew my degree was about to be completed. I was going to get my degree in 1952—not my honors degree, but the bachelor's degree in physics and geology. The expedition was '52 to '54.

COHEN: You have to do an extra year to do an honors degree.

WYLLIE: In Scotland, but not in Britain. In Britain, I think it was only three years. In Scotland it's three plus one, with the fourth year being approximately equivalent to a master's degree here.

COHEN: Is that because you take more courses in Scotland?

WYLLIE: It's because you have a broader base at the beginning, including one extra subject.

Then in the second and third year you take, again, another extra subject that I don't think was required at Cambridge. I have my BSc degree in physics and geology, but I took enough math courses that I could have graduated in math if I had chosen to do so. I could have had that label. So you take more courses—science courses, not social sciences.

COHEN: Did you do any social science, any history of any kind?

WYLLIE: No, but I never felt deprived or inadequately educated. I still remember the dates I learned in the fourth and fifth form in high school, much to the annoyance of my wife, who took a degree in history and English.

COHEN: Well, you have a good memory. [Laughter]

WYLLIE: Yes, for some things.

COHEN: So you went off on the British North Greenland Expedition.

WYLLIE: For two years, yes, 1952 to 1954.

COHEN: Were you paid for that, or did you have to pay?

WYLLIE: No, I didn't have to pay; it was all covered. Those in the armed services received their normal salaries. The scientists were paid in some way commensurate with their salary levels. Many had families at home. As a student with no earned income, I banked a very modest stipend and received free room and board, of the Arctic variety.

COHEN: Was this sponsored by the government or by private funds?

WYLLIE: The government—essentially the armed forces—sponsored it, with support from various industries in cash or supplies, with the strongest support coming from Shell Oil Company. It was organized by a commander in the Royal Navy, a founding member of the 1946 Naval Mountaineering Club—name of [Commander C. J. W.] Simpson. They really wanted

every chance to go climbing, and polar exploration shares the same almost spiritual stimulation.

COHEN: They were looking for a steeper mountain that was harder to climb?

WYLLIE: Any mountain that was difficult to climb—the Himalayas, Greenland, anywhere. Have you met mountaineers? They are fanatics, right? So these mountaineers wanted to extend their climbing into polar regions. In 1949, Jim Simpson had the idea that they could sell Arctic warfare and naval and national prestige to the Board of the Admiralty. Through his enormous enthusiasm and energy, Simpson got this major expedition organized with the support of the armed services and the relevant scientific organizations. Then he had to run around and find scientists. With the aid of an organizing committee and members of the scientific community, they started recruiting. Harald Drever nominated me, I was interviewed in Oxford, and I got in as the youngest member, at age twenty-two.

COHEN: For the science.

WYLLIE: For the science, yes. Of about twenty-five members, about half were scientists and about half were service people, mostly with experience on ice—the climbers—but including radio people and mechanical people. Weasels were going over the ice sheet—mechanical vehicles.

COHEN: I see. So they wanted to go, and they found a mission.

WYLLIE: Yes. Before they knew it, they had this great big expedition on their hands, because the effort had to be fully justified once the armed services gave them expensive support. It was the first major British expedition since the Second World War. Winston Churchill and Queen Elizabeth sponsored it as patrons, and afterwards, in 1954, we received the Polar Medal from the Queen. It was an interesting mix of what was going to become the future modern expedition, with air support, and the old-style expedition, where you walk and travel with dog teams. The geologists, the glaciologists, and the surveyors within our part of Northeast Greenland, we were doing it the old-fashioned way. We crossed the Atlantic in a Norwegian sealing vessel, unloaded all the supplies at a fjord in East Greenland, and then all supplies and personnel were airlifted by

Royal Air Force flying boats, which flew us north, dropped us on a lake within the inland ice, and then went away. However, it was old-style in the sense that, once we were dropped, we were marooned for a year. I was an assistant geologist the first year and the only geologist the second year. My main job was caring for and driving a dog team—a stupendous experience—and learning to do regional geological mapping in a complex country. This was before helicopters lifted teams in and out, as in future polar expeditions.

COHEN: You had radio contact?

WYLLIE: We had radio contact at the base hut, because they kept in touch with naval headquarters in England. So there was a big radio. They dropped loads and loads of forty-five-gallon diesel drums. We built a prefabricated base hut from scratch, and it was in communication. The diesel drums gave us heat, and they also fueled the generators to power our base hut during the two winter periods of three months of darkness—twenty-five men essentially locked in together.

COHEN: So you had two years there, and you had enough adventure then?

WYLLIE: Yes, I've had enough of long-duration adventure. No, that's not quite true. I was ready to go again on an upcoming Antarctic expedition, but then I got engaged after one year and married after two years. After I married, I decided I was not going to go away on long trips, and by then my mentor, Harald Drever, had me set on another path.

COHEN: So you came back.

WYLLIE: Then I came back to St Andrews and did my honors year and got my first-class honors degree in geology in 1955. I started the PhD program in geology, and after one year in the PhD program, in which all the residence requirements were satisfied, Harald Drever sent me off to the States.

COHEN: You never had trouble with your neck or anything in Greenland?

WYLLIE: No. In fact, they had been rather hesitant to take me for that reason. They wrote to the surgeon who did the operation, and he said I should be fine, and I was. No, I've never had any trouble, fortunately.

COHEN: So you finished this year.

WYLLIE: Then I started my PhD, which took three years, and I had a university scholarship. Actually, I nearly joined Shell Oil Company then. The Greenland expedition was supported by the armed services, but major financial support came from Shell Oil Company. They provided the seismologists and the seismometers. They were measuring the thickness of the ice sheet.

COHEN: They were looking for oil, I suppose.

WYLLIE: No, no. They were not looking for oil, no.

COHEN: There is no oil in Greenland?

WYLLIE: Not that I know of. One certainly would not look for it beneath the ice sheet. The techniques used for looking for structures for oil were the same techniques used for measuring the thickness of the ice, so Shell Oil provided that. They also provided a series of aerial photographs of the whole region, which were very convenient from the beginning for geological reconnaissance. They came to St Andrews recruiting, and I was offered a four-year scholarship for a PhD with Shell—assuming that I'd join them, not necessarily forever but for a committed time afterwards. I was nearly ready to do that, and again Drever said, "Oh, Wyllie, you don't want to do that." [Laughter]

COHEN: He really was sitting on your shoulder all these years as your guardian angel.

WYLLIE: Yes. It was a very small department, a small university, with only 1,000 undergraduates.

COHEN: Like Caltech.

WYLLIE: Yes, exactly, except that it was a full-range university, and the majority was certainly in arts and humanities. There was pre-med. People who wanted to be a doctor had to go off to Dundee or Birmingham or somewhere else to complete their medical degree. There was one university doctor and a small-town hospital. For my neck, they had sent me to a specialist in Perth, which is miles away.

So I chose not to take the Shell Oil scholarship; otherwise I might have been in Venezuela, where Dad had been. [Laughter] I took the university scholarship. The PhD was strictly three years. It didn't go on forever, like it does in America these days. You had a scholarship for three years, and if you didn't get it finished, tough luck. If something went wrong, that was it. However, to qualify, you only had to stay at St Andrews, in residence, one of those three years. Then you could go off and do your research anywhere else, with permission of the supervisor. My supervisor, Harald Drever, decided I should learn this new high-pressure research that was becoming concentrated at Pennsylvania State University at that time. One of his former students, William MacKenzie, who had been to Cambridge and was at the Geophysical Laboratory [of the Carnegie Institution] in Washington, knew Frank [Orville F.] Tuttle, who had gone from the Geophysical Laboratory to Penn State to develop the high-pressure research program. Willie MacKenzie had been a member of the 1950 expedition [to West Greenland], a kind of big brother in the field.

COHEN: High-pressure program?

WYLLIE: High-pressure experiments which reproduced conditions inside the Earth—high pressures and high temperatures. If you can measure the reactions that actually occur under conditions down below, it becomes possible to calibrate the processes. If you're looking at a volcano, you've got the lavas coming out, and on the basis of the properties of the lava—the temperature, the pressure, the composition, minerals, and so on—you can deduce things that may be happening down below. You can develop hypotheses. You can go into the laboratory and reproduce conditions ten, twenty, thirty, a hundred miles down. Then you know what happens. It's like a calibration of the geological processes in terms of depths, temperatures, and materials present.

COHEN: So Frank Tuttle was developing this approach at Penn State?

WYLLIE: Yes. There was quite a group developing there. That was one of the centers of this research at that time. It became known as experimental petrology. Drever was a bit of a rebel. The rocks in Greenland that he took me to work with were the kind of rock that— I'm wondering how much detail to go into.

COHEN: Well, this is interesting.

WYLLIE: Yes. OK. At that time, the king of igneous petrology—igneous petrology has to do with magmas, lavas, and granites, that kind of stuff—was Norman L. Bowen.

COHEN: He was an Englishman?

WYLLIE: No, no, he was American—a great man, the leading igneous petrologist of the first half of the century. The black lavas, like those from Hawaii, are called basalts. Bowen believed that basalts were the parents of all other magmas and that no basalt had more than a certain amount of MgO [magnesium oxide] in it, which was represented by the mineral olivine. You couldn't have molten lava with more than a certain percentage of dissolved olivine. Well, there were some rocks in Scotland, and on this island on the west coast of Greenland, called picrite basalts, which have lots of olivine in them. Drever decided very early on that these had once been molten, which was against the gospel. Bowen's gospel really was gospel; nearly everybody believed that. Drever was convinced that he had evidence that these rocks had once been molten. I spent the two years in Greenland. I was originally expected to write a thesis on that research, but when I came back he said that the senior geologist had already written a thesis on this after his one year in the field, so why should I do the same. He said, "There's nothing there worthwhile. It's just regional geology. Do something useful. Do something creative." [Laughter] So he sent me to a tiny little island south of [the Isle of] Skye to study some rocks, some small versions of these picrite basalts. I won't go into any more detail, but I collected those, I studied them at St Andrews for one year, and I had all the material I needed to go somewhere else to continue studying them. I had done all the field work and, looking ahead to the thesis, I concluded, not unnaturally—he was my sponsor—that these too had in fact been completely molten when they

came in. So here were magmas, potential lavas, that had a lot more magnesium and a lot more dissolved olivine than the dogma of the time said. That was eventually published in the *Transactions of the Royal Society of Edinburgh*, and nobody ever read it. [Laughter] However, he thought that if I could go and learn to do these high-pressure experiments, I could prove experimentally that these magmas could exist at reasonable pressures.

COHEN: And he believed that.

WYLLIE: He believed that, yes. That was the motivation for me going, for him directing me to do this kind of research—quite different from polar geological exploration.

COHEN: And you were always ready to go someplace.

WYLLIE: Yes. [Laughter] Romy [Mrs. Wyllie] was in her last year at St Andrews when I came back from Greenland. That should have been my last year there. We met and we got engaged. She had a cousin who had worked as a translator in New York and then married an American diplomat. Thus inspired, Romy had always wanted to go to America. So we decided we might as well get married and go together. We spent a year apart while she was in London learning to be a secretary—postgraduate work as a secretary—and I was doing my first year of a PhD. I was also an instructor; Drever had previously arranged for me to be an instructor in St Andrews while I was doing my PhD. So we got married and had a weekend honeymoon before spending two weeks in St Andrews grading examinations, and then we went off to the States.

COHEN: Was this yours and Romy's first trip to the United States?

WYLLIE: Yes. I had been to Canada on a six-week tour after high school, when I was late being called up. The Air Training Corps in England had an exchange system with Canada, in which twelve cadets from England traded with twelve cadets from Canada. The summer after high school, before I joined up with the Royal Air Force, I toured Canada with twelve British cadets.

COHEN: I was thinking you never missed an opportunity to travel someplace.

WYLLIE: I never used to. I've had enough now. [Laughter] So I'd seen Canada and I'd seen baseball.

COHEN: Where in Canada were you?

WYLLIE: Oh, gosh, I can't remember. We traveled all the time, mainly in the eastern half. We didn't go across the Great Plains, but all kinds of places, moving around all the time. We had a cruise through Lakes Superior and Huron, we visited London in Ontario, other places, and Montreal—I remember we had one night off in Montreal. That was the only free night I remember. I discovered a dance hall and practiced my schoolboy French. [Laughter]

So, after one year of my PhD research, we got married and came to the United States in 1956. Romy worked as a secretary to the dean of summer sessions, I remember. I had a full-time job as a research assistant with Frank Tuttle, which was all arranged by Drever. I had a full-time job working in his lab, but I was also permitted to study my thesis rocks with a microscope. I wrote up my thesis in the evenings through two years, and Romy typed it.

COHEN: I know what a little town Penn State is in, in the middle of nowhere, although it's a big school with a lot of students.

WYLLIE: Yes. At that time there were 30,000 students. I used to know the population of the village, maybe 5,000 at that time. It's geographically in the center of Pennsylvania. They literally drew diagonals across the state, and that's how they located the town of State College.

COHEN: Did you do any traveling when you were there?

WYLLIE: I went to various national conferences. We were there for three years and didn't do any serious traveling until our last year, except for doing meetings, or driving down to Washington DC every so often to fill up our liquor cabinet without Pennsylvania tax. Oh! We did have one trip—we took two weeks off to drive down to Florida in 1957. But the last summer, in 1958, we spent six weeks in a car with a tent, driving from Penn State across the north to Washington [state] and down into California. We saw a great deal of the country. Our first child was conceived on that trip.

COHEN: [Laughter] Then you came back and finished up, and you went back to—

WYLLIE: My PhD thesis was submitted to St Andrews while I was still at Penn State. A committee examined it without me. I graduated in absentia PhD in 1958 and came back to Britain in 1959. Oh, I was appointed assistant professor at Penn State once I got the PhD. They wanted us to stay, but we said no, we had come here for three years on a Fulbright—I got a Fulbright travel grant—so we had to come home. They said they could bypass that problem and arrange visas. We were very sorry to leave. We made a lot of very good friends and had a great time there, but we felt that Britain was home, so we were going home.

COHEN: I guess one didn't go back and forth so easily. When you were in the United States, you didn't think about quick trips back to England.

WYLLIE: Our honeymoon was on the *Queen Mary*, from Southampton to New York. We were on the *Queen Elizabeth* going back to England. These were not quick trips and, anyway, we could not afford transatlantic fares.

COHEN: So then you were at St Andrews only a short while before you came back?

WYLLIE: We didn't go back to St Andrews, we went back to Leeds University. I got an appointment at Leeds University. Romy is from the coast in Yorkshire, and Leeds is in Yorkshire. While I was a PhD candidate, I had been told to apply for a lecturer position at Bristol. In those days it wasn't easy to apply.

COHEN: Let me turn this tape over. [**Tape ends**]

Begin Tape 1, Side 2

COHEN: Did you ever apply for these jobs?

WYLLIE: Oh, yes.

COHEN: You make it sound like it was terribly easy, which I have the feeling it wasn't.

WYLLIE: No, it wasn't, at least not from the States at that time, anyway, in '58. It was very difficult for a young person to know what to apply for or where to go. I forget who told me—Drever probably, or maybe MacKenzie, a former student of his, said, "There's a position in Bristol. Why don't you apply for that?" So I wrote. I don't remember ever seeing an advertisement, but I wrote to the professor at Bristol and sent my credentials, and so on. I must have had some letters of recommendation. What I do remember is that he wrote me a very nice letter eventually, saying, "Dear Wyllie, thank you for"—blah, blah, blah, blah—"I have to tell you regretfully that I can't appoint you." He said, "You're the best candidate for the position, but I need somebody who's going to be able to teach crystallography, so I have to appoint a crystallographer." So I didn't get to Bristol, which I was looking forward to, because at that time it had some people who were connected with Greenland—expeditions and that kind of stuff. So that would have been very good.

I have no recollection of how I learned that there was a Professor [Ernest Gordon] Cox in the Department of Chemistry at Leeds University who wanted to start some high-pressure work. He had a reader in chemistry who was doing some high-pressure work. I went there as a research fellow in chemistry to build a high-pressure lab. Within a very short time, the professor of geology had asked me if I was interested in a position in geology and I said, "Yes, please." So, after one year as a research fellow, I became a lecturer in geology. That's when I started teaching, but I continued doing research with the Chemistry Department equipment I had set up.

COHEN: You weren't there very long.

WYLLIE: Only two years. What happened was that in 1960 there was an International Geological Congress in Copenhagen, followed by the International Union of Geodesy and Geophysics in Helsinki. They meet every four years in different parts of the world. The work I had done in the States was of sufficient interest that somebody—I can't remember who—gave me £200 to go and read a paper in Copenhagen. I took Romy. I guess we left our son with my mother in London. We went on a boat from Newcastle to Copenhagen. We met all of our old friends from Penn State and other places in America. We had a wonderful time. We came to life. Things were very quiet in Leeds. We had a small baby. Andrew had Down's syndrome, and nobody seemed to know about babysitting, although we did have some mature neighbors

who helped us out. There wasn't much social life. Nobody seemed to do anything. People in the department— It was a very good department; they had a lot of good geologists, strong international connections, and an African research institute, which I was going to be involved in, because my experiments were leading to those rocks in Africa. But people didn't want to talk often about research. The enthusiasm for research, which was sort of oozing out everywhere at Penn State, was regarded almost with distrust at Leeds. People looked at me very strangely. [Laughter] Why was I talking all about that? The sense of competition somehow—

COHEN: Wasn't there?

WYLLIE: No. At Penn State, everybody was competing to do good research and tell everybody else about it. I got the sense in Britain that you kept your research secrets to yourself. It was a very strange feeling. At morning tea and afternoon tea, everybody wanted to talk about cricket, or soccer, or the latest play downtown. Nobody wanted to talk about research.

COHEN: Was that because they didn't want anybody else to know what they were doing?

WYLLIE: I don't know. It seems very strange now. I can't remember what it was, but there was the sense that there wasn't any excitement in research. Yet I made several lifelong research friends there. The salary was very meager, too. I remember the professor in geology saying to me, "Would you like this job? We're going to appoint a lecturer. You'll have to go through the routine applications process." I said, "Of course," and he said, "I don't see any hurdles. I'm sure you'll get it." Then he told me what he would offer me, which was £200 above the basic rate, which I think was £1,000 per annum. I went through the university interview with people who had no idea what I had been doing. He was on the committee, but he didn't say anything. They read my research list and so on. There were a couple of people who were pretty snippety about this, and said, "This doesn't mean anything to me," about my list of publications, which I was tremendously proud of. At the end they said, "Well, you've got the job, if you'd like it." I said, "Thank you very much." The registrar pulled out a chart and he said, "Your age is thirty. Your salary will be so and so," and it was £200 less than what the professor had offered me. I thought, "Never mind, it's a job." [Laughter] I mentioned to him afterward that the salary was a bit lower than he had indicated, and he said, "Oh, I'm sorry. We'll give you some ups in a year

or two. We'll get you bumped up."

Then we went to Copenhagen, and everybody from Penn State was saying, "Why don't you come back? You should come back." They told me that I was going to get a letter of invitation, a formal letter to go back. Our car broke down, and I had to replace the engine, and then I got the letter from my professor at Penn State, Frank Tuttle. He had just gotten Parkinson's disease. He had become dean while I was gone, and while he was still dean, he would like to make me an offer to go back as an associate professor.

COHEN: So it sounded very good.

WYLLIE: Yes, it did. So we decided to go. A week later I got a letter from [Lee A.] DuBridge [Caltech president 1946-1969] offering me an appointment as assistant professor at Caltech, and I said, "No, I'm sorry, I'm already—." Some people I knew here were quite indignant. They said, "Just turn back the Penn State offer. You don't have to stick to it."

COHEN: Well, I suppose you really knew all those people [at Penn State] because you had been there.

WYLLIE: Yes, but I knew some people at Caltech, too—not as well, of course.

COHEN: Had they been at these meetings also?

WYLLIE: Yes.

COHEN: Who were some of these early people you met?

WYLLIE: Sam [Samuel] Epstein, Hugh Taylor, Lee [Leon T.] Silver, Gerry [Gerald J.] Wasserburg, Al [Albert E.] Engel, Bob [Robert N.] Clayton, who had married the provost's daughter and then moved to Penn State. Those are the ones I remember without having to dig into my memory. Hugh Taylor came to Penn State for one year while I was there, immediately after his PhD, but then he went back to join the faculty at Caltech.

COHEN: So you packed up and said goodbye.

WYLLIE: And got on that boat again. We sailed on great liners three times, in steerage class.

COHEN: You were probably carrying stuff with you.

WYLLIE: Yes, yes. The first time we went, in 1956, we went with one cabin trunk. We went back to England three years later with nine cabin trunks. This time, in 1961, we shipped out furniture.

COHEN: So now, had Romy seen—

WYLLIE: She was at Penn State for the full three years.

COHEN: Right. So she knew where she was going.

WYLLIE: Yes.

COHEN: Was it indeed much livelier than being in Leeds?

WYLLIE: Oh, yes, yes. It was a sad departure, though. My father was beginning to get a bit sick. He was having blackouts. It was early senility, or maybe it was Alzheimer's disease—I don't know. In 1961, who knew what these things were? He was not in very good shape. Romy's mother had recently been diagnosed with breast cancer. So it was difficult leaving. Romy puts it this way: She says, "We had decided that we could clearly do much more for ourselves and for our children in the States than we could in England."

COHEN: So you came in—

WYLLIE: 1961. I came as an associate professor of geochemistry. They called me a geochemist. That's because the department was geochemistry—the high-pressure department at Penn State. I was there for four years, '61 to '65. The guy who invited me there, Frank Tuttle, got Parkinson's disease; the Parkinson's disease got worse and he retired. We hired a new dean [of the College

of Mineral Industries], a very famous geologist from Caltech, Dick [Richard H.] Jahns. Do you know Dick Jahns, from Caltech geology many, many years ago? He must have left in '61 or '62 or so to become dean at Penn State. [Jahns left Caltech for Penn State in 1960, to chair the Division of Earth Sciences. He became dean of the College of Mineral Industries there in 1962—ed.]

COHEN: We came in '69, so, no.

WYLLIE: OK. He's well remembered by the geologists. Then I received an offer to go to the University of Chicago.

COHEN: After four years at Penn State.

WYLLIE: After three, I guess it was; yes, in '64. We weren't very keen, because there we were in secure rural America. The University of Chicago had a terrible reputation in the 1950s for its safety, or lack of safety. At that time, the ghetto had swamped the whole university.

COHEN: It had come up to Hyde Park.

WYLLIE: Yes, right through Hyde Park. So we said, "No, no, we couldn't possibly go there. It would be too dangerous." But they invited us to visit in November of 1964. I remember that Romy was pregnant. So we went by train and looked and were very impressed, because Penn State somehow had never seemed like a university to us, after Britain. It was like a big factory, with crowds of people rushing around. Chicago, with its quadrangles and pseudo-Gothic ambience, was just like a university again.

COHEN: With plenty of ivy on the walls, yes.

WYLLIE: Yes, yes. It was a very good department. We went on a Sunday afternoon just to have tea with one of our friends, and their two little daughters, who were about four and five, or maybe younger, were out on the sidewalk with their tricycles, rolling up and down. I thought, "What's all this about the big, bad city?" It didn't seem bad. But I remember that was a

significant aspect. We realized that you could go out on the street; you could walk on the street—because from Penn State you had the impression that it wasn't safe to get out of the house. There were so many stories of muggings.

COHEN: I know so many stories here about what the Pasadena schools are like, from people who have never been in them. [Laughter]

WYLLIE: So we accepted.

COHEN: This was like getting back to a city like London. You were going back to a big city.

WYLLIE: Yes, and we really enjoyed the big city. We had eighteen years there. It is a tremendous city. We loved it. Our living was great. We had a townhouse. When we went there, we said to— Do you know [the geochemist] Julian Goldsmith, by any chance?

COHEN: I've met him.

WYLLIE: He was chairman [of the Department of Geophysical Sciences] at the time. He gave us the offer. He said, "We've got just the place for you to live. They've just knocked down a slum, and they're building a set of townhouses here for only \$50,000 each. They'd be perfect for you." I knew what the salary was going to be. I said, "We couldn't possibly afford a house for \$50,000, Julian." He said, "Don't worry, Peter. We'll see that you don't starve." [Laughter] He had such a different attitude from Penn State, where the budget was governed every year by the legislature.

COHEN: Sure. You were in a private school, where people do what they want to do.

WYLLIE: So we bought a house before it was built, before we got there. We got there, and the house still wasn't built, so we all went off for six weeks to Romy's cousin, who had retired to Delray Beach, in Florida. We came back and the house still wasn't built, so we lived with some former friends from St Andrews who were in Hinsdale, which is a western suburb of Chicago. We stayed with them for a month. We had three children by then. Eventually—in November,

just in time for Thanksgiving—the company put us into another house that they had just finished. We invited students for Thanksgiving dinner only to discover that the builders had put the gas valve in backwards. They fixed it Thanksgiving morning. We sat there for two months, I think, before our house was finally ready.

COHEN: So you finally moved into this row house.

WYLLIE: Yes, on a very cold, snowy day in January 1966.

COHEN: It was very nice, I gather. Could you walk to the lake?

WYLLIE: We could walk to the lake in fifteen minutes. It took eleven minutes, door to door, to walk from home to the department.

COHEN: So you felt quite safe and you didn't have any problems with safety? You had to watch it?

WYLLIE: We had to watch it, yes. Romy had her handbag snatched once. Thirteen-year-old kids from the neighborhood school entered our house at four o'clock in the afternoon. I was down in the basement. Oh, that was another time. Twice. [Laughter] Once, I was down in the basement and I came up and I saw these two little black boys picking up our radio. I roared them out of the house and I chased after them, but they split. Another time, somebody came in and ripped off all the jewelry upstairs and left in a hurry without taking the TV that they had stacked ready to go.

The kids went to school. Andrew, the Down's syndrome one, went to the city school one block away. Then, for high school, which was quite a long way away, he had to go by bus. He survived it very well. Then he was accepted to Truman College, a city college, which I thought was amazing. The dean of admissions said, "Well, our philosophy is that if a person can improve his lot in life by attending this college, then he can be admitted, and mental retardation is no handicap." Andrew would benefit from the social interaction.

COHEN: He must have been quite bright for a Down's syndrome child.

WYLLIE: Yes. His IQ was 66, but he was high functioning. He learned to go from Hyde Park to the north side by public transportation every day. He took the bus into the area west of Hyde Park that none of us would walk in, from the bus up onto the train, and then right across town to the north side.

COHEN: I think he did exceptionally well, then.

WYLLIE: Yes. I think he was there for—one year, certainly. I think he was there for a full two years. So, how did I get off on that track?

COHEN: We were talking about safety.

WYLLIE: Oh, yes. He managed to survive the whole city system. Our other two children went to the University of Chicago Laboratory Schools, which was great, because it was an excellent private school, and faculty got fifty-percent tuition remission. They grew up streetwise, and I'm very glad they did. I'm sure they were hassled once in a while going to and from school. They had to walk only four blocks. They certainly had no serious concerns about safety. When they started driving the car, and they went driving off at night to late-night parties and coming back, we were worried, but they never seemed to care. They both joined the Chicago Children's Choir, which is a multiracial group, and made many friends both in local rehearsals, Chicago concerts, and lengthy national concert bus tours.

COHEN: And you pursued your geology?

WYLLIE: I did the same stuff. I set up another high-pressure lab there and continued the same kind of experimental petrology.

COHEN: I see you moved into administrative positions there. You became the chair of the department [of Geophysical Sciences]. Is that right?

WYLLIE: Yes, I finally became chair in 1979. They talked me into being master of the college one time, but one year of that was enough. I asked to be excused. Master of the college is

nominally responsible for all the— Chicago has a system where they have a college within the university. The college was administratively separate. Not everybody taught in the college.

COHEN: It's sort of the Cambridge system, isn't it, where there's a college and then there's a department or something?

WYLLIE: Not the same, no. The Cambridge colleges are built around residential units. At the University of Chicago, the college comprises essentially all of the undergraduate instruction. They had residences within the college, but they weren't really part of the college structure. They also made me associate dean of the Division of Physical Sciences [1972-73], which included the graduate programs, but it meant being nominally responsible for the undergraduate teaching of the physical sciences with no significant control. As far as I could see, there was little chance of doing anything useful. Anyway, I decided it was not for me. I never wanted to do just administrative work anyway. I got talked into doing it, so I did it for a year, and then I said, "Look, I really don't want to do this. I want to finish my textbook." So I was let off.

COHEN: What book were you writing?

WYLLIE: That was my third book, *The Way the Earth Works*, published in 1976. It was the first introductory textbook dealing with the revolution of plate tectonics. Although it was designed for non-science majors, it was widely read by professionals wanting to catch up on the revolution. My first book was *Ultramafic and Related Rocks*, 1967, with contributions from thirty-three authors that I organized into a comprehensive volume. Reviewers noted that this textbook set a new style and standard for books of collected articles. My second textbook, *The Dynamic Earth*, 1971, was one of the first advanced textbooks to capture the revolution of plate tectonics and to develop multidisciplinary approaches to earth processes. At that time I was teaching the introductory class in geology and was awarded the Quantrell Teaching Award for excellence in undergraduate teaching in 1979 for the course that was based on my 1976 book.

I guess they figured I might be a good person to be master of the college, because I was obviously devoted to this introductory class that few people wanted to teach. Then I think I said no to the chairmanship once, and then my time had come. I was old enough.

COHEN: Obviously your career was developing very well.

WYLLIE: Yes, we all seemed to be happy.

COHEN: So I think maybe this is a good place to stop.

WYLLIE: Yes. **[Tape ends]**

PETER J. WYLLIE**SESSION 2****March 29, 2002****Begin Tape 2, Side 1**

COHEN: I'd like to start out by asking you what made you leave Chicago, where you were obviously very happy, and succumb to Southern California.

WYLLIE: Well, you're right. We were very happy there. We were expecting to spend the rest of our lives there. I diverted invitations to move. However, we had an invitation to go visit MIT, with the view to being the next chairman there. It must have been about '79 or '80. We weren't interested, but they said, "Well, come and have a look-see," so we did. Romy and I went together, and we spent the usual week looking around and checking things out, and I guess they were checking us out at the same time. Although it was tempting in a way, after eighteen years in Chicago, what killed it was the fact that at that time mortgage rates were up at twenty-two percent. I didn't know anything about refinancing, and housing simply wasn't desirable. We couldn't find anything that we could possibly afford that would be as attractive to us as the house in Chicago. Chicago was great, because we could walk to work. At MIT we would have had to live quite a way out, and we decided we didn't want that kind of commuter life. However, we were attracted enough that our assumed resolve, or expectation, that we would stay in Chicago for the rest of our lives was weakened.

The next year, 1981, I had a call from Caltech. I had been nominated as the next chairman [of Caltech's Division of Geological and Planetary Sciences]. You know the way it's done here: The whole faculty gets together and works things out, and they decided they'd like me to consider it. I said, "No, we have just said no to MIT, so we know we're not interested in moving." They said, "Well, come out anyway," so I came out on my own, first, for a few days.

Let's see. We had Bob [Robert P.] Sharp [as division chairman] for years and years, who made the division famous. There was an interlude when Clarence Allen and Gene [Eugene] Shoemaker had short terms, and Barclay [Kamb] became chairman in 1972. He had two terms, and he'd had enough by then. [Laughter] So they were looking for somebody new. Actually,

before Barclay became chairman, in about 1970, Lee Silver came to visit me in Chicago and asked if I was interested in being division chairman at Caltech. I said, “No, no, I’ve been turning down administrative positions. I don’t want to do that.” One reason for coming to America was to avoid becoming a U.K. professor, which was then the only way to advance but which also required being head of department. I was doing my research and was very happy, and that’s what I wanted. It turns out it might have been a serious invitation, because the provost at Chicago phoned me up a couple of weeks later and said he’d had a call from the provost at Caltech inquiring about me and said, “Please don’t do anything rash.” I said, “It’s all right. I’ve already said, ‘No, I’m not interested.’” Then, after Barclay’s session, they came back to me. So I came and looked around, and I went looking at houses without Romy.

COHEN: What time of year was this? They usually bring you in February from Chicago to Southern California—that’s often the ploy. [Laughter]

WYLLIE: Yes. I think it was early spring. The weather was nice, but that’s true eleven months of the year, isn’t it?

COHEN: Right.

WYLLIE: Mortgage rates had gone down all of a sudden. I saw some very nice housing that I liked. The place is very attractive scientifically, of course, and I knew a lot of the people here pretty well. I knew the place had a reputation for internal strife between certain kings and emperors within the faculty. [Laughter] Apparently everybody except for one person welcomed me. One person told me he voted against me. I won’t mention his name.

COHEN: OK. You can, and then seal it. [Laughter]

WYLLIE: No, no. [Laughter] He was the one who was given the task of taking me to dinner—well, the pleasure of taking me to dinner—at the Athenaeum and explaining what the division needed, what they were expecting from a chairman, and so we exchanged views. I went back to Romy and said, “You know, it’s not so bad after all. We should think about it again.”

COHEN: So you were not overwhelmed by the administrative part of the position here?

WYLLIE: No, no, I know what had happened: In 1979, I had finally accepted the position as department chairman at Chicago, after turning it down a couple of times. I was already chairman at Chicago, so I realized that I wasn't going to escape administration. I seem to have spent the early part of my life being chairman or president or director or captain of the team, and I had wanted to stay away from it. I got involved in research, and I had a few excursions with administrative work at Penn State—they made me chairman for a year, I think. Then in Chicago, as you know, I was master of the college and quit that after a year. I didn't want that. I was too happy teaching, doing research, and writing. However, I finally accepted the chairmanship at Chicago. I had been chairman already for two years, out of a three-year term.

COHEN: I see, and you were realizing it hadn't been so bad.

WYLLIE: No, it wasn't that—it was that I wasn't going to be able to escape it. It was my time in life to take on senior responsibilities. That was the reason. So Romy said, "OK, I'll come out and have a look," so we came out together. It must have been in the spring. We looked around and talked. We went house hunting again—it seemed to have been an important part of whether or not we were coming.

COHEN: Well, I think that's true.

WYLLIE: Jane Caughey, our agent and the wife of engineering professor Tom [Thomas K.] Caughey, took us around. She had seen from the first visit the kinds of things I thought we would like. [Laughter] We visited a house in Kinneloa Canyon, which is where we live now, and Romy said, "Well, if we can have this house, let's come."

COHEN: She really liked the house.

WYLLIE: Yes. I said, "Oh, that's ridiculous."

COHEN: It's not walking distance. [Laughter]

WYLLIE: No, it certainly wasn't walking distance, but it's only a fifteen-minute drive downhill, no freeway, very nice. We agreed that we would accept the position, but not when they wanted us in the fall, because our son had his last year in high school and we didn't want to take him out. We said, "Yes, OK. If we can have one more year in Chicago, we'll take it on."

COHEN: So what made you take it was that you liked the house? [Laughter] Or that Romy would be happy?

WYLLIE: I told Romy that since this was in spring and we weren't coming for a year and a quarter— I said, "You know, there's no way we can get this house." She said, "Well, ask, ask. There's no harm in asking." She had heard about Caltech's generosity in this kind of thing. What happened in the end was that Caltech bought the house for us, and we bought it from Caltech when we sold our house in Chicago. So they bought it and held it, which was very nice. Also, in looking at MIT, we had become conscious of the fact that winters in Chicago ten years, twenty years down the road would not be too pleasant. A lot of our older friends—a little bit older than us—had already suffered broken legs by slipping on the ice. I shoveled snow every day through the winter outside the house. We said, "Well, do we really want to spend our retirement with Chicago's winters?" That was definitely a factor. We decided no. Weather was a factor. It hadn't been, for eighteen years—I mean, shoveling snow was exercise—but looking ahead, the absence of snow shoveling was attractive. The general environment in both Caltech and the division, the long-term view of winters, and attractive housing that we could afford—housing that was an improvement on the housing we had in Chicago, instead of stepping down to less desirable housing, which is what we would have had to do at MIT.

COHEN: You mentioned that there was dissension in the division. That didn't bother you?

WYLLIE: No, it didn't. It was a joke throughout the geological community in the United States, the way some faculty members in our division used to fight with each other in private and in public.

COHEN: Was this because they were in different fields? Or were they in the same field?

WYLLIE: No, I think they were usually in closely related fields. There was competition and rivalry.

COHEN: I haven't heard this from anybody, so I'm happy to hear more about it.

WYLLIE: I don't think I know enough of the details to speak about it with any confidence of accuracy. I'll tell you one incident, without giving names. I was told, after being here for one year, that one faculty member had his car dented by the car door of another faculty member. They knew because of the color of the paint, or something like that. I mentioned this to the wife, saying, "Oh, I hear that 'Charlie Brown' dented your husband's car." She said, "Oh, he's still talking about that? That was twenty years ago."

COHEN: OK. [Laughter]

WYLLIE: That was a trivial thing, but it related to their scientific competition, or scientific jealousies, or whatever they were.

COHEN: So you think it was the basic nature of these people, not particularly the science.

WYLLIE: Yes. After I came to Caltech, a lot of people said, "Oh, boy, I'm going to be interested to see how you survive with that lot—see how you handle that lot." One of my old friends from as far away as England said, "I'd like to be a fly on the wall when you preside over faculty meetings." When Art Montana [emeritus professor, Department of Earth & Space Sciences, UCLA] presented me for the Roebling Medal last year, he published a sentence indicating perceptions of Chicago and Caltech: "Peter served as chairman at Chicago and then, always a glutton for punishment, moved on to accept the chairmanship at Caltech—akin to jumping into the La Brea Tar Pits twice—head first."

COHEN: So when you came here, you already knew about the reputation of this department, which is stellar, certainly.

WYLLIE: Oh, yes, it's been the leading, number-one department in earth sciences and planetary

sciences through decades, thirty years at least.

COHEN: Was it mainly Bob Sharp's vision that took them into this—?

WYLLIE: Yes. I understand that it was his vision, his decision to start certain programs and bring certain people in. Soon after 1953, the field of isotope geochemistry was transplanted from Chicago to Caltech by the appointments of Harrison Brown, Sam Epstein, Heinz Lowenstam, Clair Patterson, and Gerald Wasserburg. As I say, Bob offered me a position in 1960. I have sometimes wondered how my life would have been if I had been here from then on instead of— So in a sense, it was like coming home to Caltech, because I had been invited twice before.

COHEN: Well, you obviously knew everybody here.

WYLLIE: I knew the people in my field. I didn't know the planetary scientists too well, but I knew a lot of people here very well.

COHEN: OK. So you did move, then, in—

WYLLIE: June of '83. I came in as division chairman. I found the faculty to be warm and welcoming, and I experienced none of the rumored battles, but I did not survive the first five-year term. With Barclay Kamb as my model, I came with the expectation of being chairman for five years and then being routinely renewed for another five years. I figured, "OK. I'm coming in at the age of fifty-three. In ten years I'll be sixty-three. That will take care of my administrative responsibilities. Then I can go back to teaching and research again." Also, I always said that the only attractive thing about being chairman of a department is to bring in new people—to make it grow, to bring something worthwhile, bring something novel.

COHEN: You saw that as your main mission—to bring in good new people?

WYLLIE: I didn't see that as a mission at Caltech, in particular, but that was my feeling—that that's the only worthwhile aspect of being chairman of a department or division. All the rest is—

COHEN: Another responsibility that we haven't really touched on is, once you take a position like that, then you are part of the major administration of Caltech, to decide major things that aren't particularly the division's special things.

WYLLIE: Yes, but that's not something I would choose to do for a career.

COHEN: How did you find that aspect?

WYLLIE: The relationship between the administration and the chairmen? I thought that was a wonderful system. The system at Chicago was quite different, because it's four times as big, in terms of number of faculty.

COHEN: Well, there isn't a level of deans here.

WYLLIE: That's precisely it. As a chairman in Chicago, I was responsible to the dean of Physical Sciences, and that was it. I knew the president; we went to all their parties and so on. However, administratively I reported to the dean. So I never administered with chairmen in other departments outside the physical sciences. In fact, I didn't operate very much with the chairmen within the physical sciences, either. The dean didn't have regular meetings of the chairmen to discuss things. It was usually one person at a time.

COHEN: So the dean was really the powerful person there.

WYLLIE: The dean was the person who controlled maybe five departments, yes.

COHEN: But here it was very different.

WYLLIE: Here, as you know, the chairmen meet with the president and the provost once a month.

COHEN: Yes, and of course you have to, as a whole, decide things like who's going to be in the humanities division. So that would have been very different. How did you find that?

WYLLIE: It was really quite refreshing. However, I wasn't happy, because the lid was on appointments. Robbie [Rochus E.] Vogt told me that—well, we all agreed to it. He was the new provost.

COHEN: I have to ask you how you found that, too [laughter], speaking of controversial people.

WYLLIE: Robbie was a very, very interesting person. I admired him very much. I still admire him, and I still like him, but he was tough. It just wasn't in my character to argue vigorously in front of other people. I'd be embarrassed. [Laughter] The president, the provost, and five other chairmen are there at IAC [Institute Administrative Council] meetings. If I had a serious issue, I'd tackle it by writing him a memo. I remember after one memo, he said, "Oh, that's just like a Talmudic argument. You persuaded me with this." I thought I was doing very well, that I was getting things done, but apparently after three years the division faculty didn't think I was visible enough, that I wasn't generating enough things.

COHEN: You mean, your own division decided you weren't—

WYLLIE: Well, I don't know exactly what happened. Robbie called me in one day in summer 1986, and said he wanted me to know that he had met with the faculty to discuss my chairmanship. He reported that the faculty had concluded that although there were no serious problems, they thought at this stage that they would not want to renew my appointment for a second term after 1988. He was very sweet, charming, and sympathetic, and felt that he should inform me of this progress report.

COHEN: Did he say why they felt that way?

WYLLIE: No. I never quite understood why. However, I was quite shaken by this, and I said to Robbie something like, "Well, if there's a perception that I'm not doing the job adequately, that means I'm failing, and therefore I shouldn't be in it. So why wait until the five years are up? I'll keep working, but will step aside any time you find a replacement for me." I added, "I hope you'll do it quickly."

COHEN: Maybe it was the way you operated, being quiet at the meetings?

WYLLIE: I know that the faculty wasn't happy that we weren't getting any new appointments. We had meetings of the IAC when Robbie, like all new provosts, came in and looked at the overall faculty distribution through the past twenty years, and the next ten years, and so on. His main conclusion was that engineering was low. Engineering faculty had progressively declined relative to all of the other divisions. I think biology was down a bit, but our division was up. So until somebody left, we shouldn't get any new appointments. Now, as you know, the faculty here doesn't like to be told what they can and cannot have. Our division certainly wanted some additional appointments, and I couldn't do anything about it—at least, I thought I couldn't, because Robbie's analysis was backed up by the IAC. There was overall approval of his analysis. First, the total faculty should not increase above 275, I think it was, because then we'd lose our quality—I mean Caltech's quality of life. Secondly, engineering had to be built up before other divisions got any breaks.

COHEN: Did that in truth happen? Did other departments get appointments because they were aggressive behind the scenes?

WYLLIE: No, I don't think so, not during the time I was on the IAC. But I'm sure that our faculty didn't like my acceptance of this decision. They probably thought I should have been in there thumping on the provost's desk, saying that we must have certain appointments.

COHEN: I see, so that was the style they wanted.

WYLLIE: That may have been part of it. At any rate, I did not decide to stay on to try and prove myself to the division faculty—I really thought I'd been doing fairly well. So I announced it to the faculty as calmly as I could, at our first monthly faculty meeting of 1986-87. I said, "I understand that you—" Well, I summarized what Robbie had said. "So I concluded that I should step aside as soon as you'd like to appoint somebody else, but please do it quickly. You will appreciate that this is difficult for me."

COHEN: OK. So you stepped down. You didn't feel terribly bad about it?

WYLLIE: I did. I did. I felt very badly about it, because, as I said to Robbie, “I’ve never failed before, and apparently this is a failure.” I tried not to let people know that I felt badly. I tried not to feel badly, but I did, and I still do.

COHEN: It’s a matter of style, I think.

WYLLIE: Well, it’s certainly true that I’m not a table thumper. My personal style is indicated by the brass plaque given to me at the closing ceremony of the 1999 General Assembly of the International Union of Geodesy and Geophysics, with the engraved citation: “To Peter J. Wyllie. In appreciation and gratitude for his dedicated, inspiring, integrating and gentle leadership as President of IUGG in the period 1995-1999.”

COHEN: So you went back to being a professor again?

WYLLIE: Yes, but not for a while. The replacement procedure dragged on until January or February 1987. I was somewhat comforted after my public offer to step aside when some of my friends in the faculty reported that the division had not called Robbie to a meeting but that it was Robbie who had organized a special meeting to discuss my chairmanship, and quite a lot of faculty could not get to it. Some of the faculty who were there told me that they were quite upset about this meeting, and some people who weren’t there expressed similar opinions. They said, “The chairman’s job is the worst job ever. There aren’t many people who want to do it, and you were doing OK.” But I guess I was doing things too quietly.

I also received some hints that my induced voluntary resignation may have been part of some grander Caltech scheme. Also, I will add that many years later, at a party in our home, Robbie told me something that seemed to indicate that there was more to it than my administrative style. He said, “You know Pete, one day I’m going to tell you something about this, and I’m going to apologize to you.” But a history should not deal with speculation, so I’ll leave it there.

COHEN: Who was the next chairman?

WYLLIE: After what seemed an interminable interval, Gerry Wasserburg was appointed

chairman in early 1987. I'm sure that the invitation had been made much earlier, but I expect that he set up some very tough conditions for the administration before accepting the chairmanship. [Laughter]

COHEN: Did they give him any new appointments?

WYLLIE: Yes, there must have been some. I don't know whether they gave him the appointments as part of the deal or not, but there were some new appointments. We had Geoff [Geoffrey A.] Blake very soon, I think. Then, after a couple of years, success with a female cosmochemist, Ewine van Dishoeck, who unfortunately returned to Holland soon after. There was some talk of a new center for cosmochemistry. I can't remember; I've blanked out.

COHEN: So you went back to being a professor again?

WYLLIE: Yes.

COHEN: And you had a named professorship?

WYLLIE: No, no. I had one in Chicago. I never got one here. The standard for named professorships at Caltech was very high; independent recognition of my research by election to seven national science academies did not qualify. I was elected foreign associate of the U.S. National Academy of Sciences in 1981, Fellow of the Royal Society in 1984, and subsequently I was elected foreign member of national academies in Russia, China, two in India, and of Academia Europea.

COHEN: So then you became just a professor in the division. Gerry took over. How did the division find that, then?

WYLLIE: Well, he reorganized many aspects of division procedures. As you know, or maybe you don't know, he resigned after two years. He was involved in a conflict with the faculty about some policy, and he said, "I resign."

COHEN: I gather he was very angry.

WYLLIE: He remained very much under control, as far as I could see. I think the crux of the matter was that one of our faculty members—can I be as open as I like?

COHEN: Yes.

WYLLIE: Joe [Joseph L.] Kirschvink had a proposal to study the human brain, because he had done some tremendous work on magnetite crystals in the brains of salmon, fish, and even pigeons, I think—all to do with the Earth's magnetic field and the creatures' migratory systems. Bacteria, too—the bacteria stuff was very exciting. He had written a proposal to do something on the human brain—I don't know the details—and Gerry was chairman and refused to sign it. Joe was not one to sit quietly. He erupted and said, "You can't do this. My academic freedom is involved." I remember we had a meeting of the division faculty to discuss this, and everybody agreed this just was not acceptable. I think that was the turning point—the emotions within the division got going. To strike at academic freedom by saying, "No, I will not sign this proposal," I think that's what— I don't know what happened. I was in my withdrawal stage at that time. [Laughter]

COHEN: So you went back into research full time at that point, because the chairmanship must have taken up your time.

WYLLIE: Yes, the chairmanship takes time and mental energy. For me, the mental energy was the most demanding. Some administrators seem to be able to switch it on and off. They say, "All right. I'll do administration in the morning and research in the afternoon." I couldn't do that—not very effectively—although it turns out that my rate of publication wasn't reduced during the span of years I was chairman.

COHEN: Did you have a group at this time?

WYLLIE: I never had a big group. I only had two grants; two overlapping NSF [National Science Foundation] grants was my load. Well, for a while at Caltech I had three. I used to have one

postdoc and a couple of students. I think here at Caltech I ended up generally with two postdocs and one student.

COHEN: That sounds like astronomy. I don't know if that's true anymore, but that was common at the time, unlike biology and chemistry.

WYLLIE: Well, some of the people in our division have very large groups. Gerry has a huge group now. I think people in geophysics and planetary sciences manage to get bigger grants and more people.

COHEN: So how big a group you have is really determined by how big a grant you have.

WYLLIE: Oh, yes, yes. It's definitely a matter of money raised.

COHEN: What research were you doing at this time? Do you want to talk about that a little bit?

WYLLIE: Yes. It is conveniently summarized in the division Web site, something like the experimental petrology of magmas and volatile components at pressures reproducing conditions to depths of 150 kilometers in the Earth, with applications to granites, subduction, and andesites, kimberlites, carbonatites, and some associated ore deposits. I told you earlier that I started off in geology because I wanted to see the world and solve big mountainous problems. I got sidetracked by being sent to Penn State to learn this new high-pressure petrology. Once I got going in that, there always seemed to be more and more to do. It was more satisfying than field geology. The trouble with geology is that you can go into the field, look at an outcrop, and six geologists can look at it and have six different interpretations—not for all rocks, but for some particular kinds of rocks. Whereas you can go into the laboratory and you can do some experiments. You've got some data that are going to last forever. They may be improved by new measuring techniques later on, but you've got data that will last forever, and they can provide the definitive answer to a particular problem. That had a lot of appeal to me. I started off, way back at Penn State, on the problem of granites. Making granites is an integral part of making continents. My early work was predominantly concerned with this process. My plan overall has always been to find a problem, a good geological problem, and do experiments with

the rocks themselves. If you study a simple model system with just two or three components, let's say, MgO, SiO₂, and H₂O, representing a simplified version of a rock, you can apply the phase rule and see exactly what's happening everywhere. If you do a complex rock system with fifteen chemical components, you have a slice through a series of phase assemblage reactions that cannot be interpreted in a rigorous way. However, if you take the model system, the simple system, where you can see what's happening, that helps you to interpret the big system. Then you go into the Earth's interior, where the process of interest started, and you take what material is assumed to be down there, and you determine in high-pressure apparatus exactly what reactions occur, with results permitting you to link from the depth to the surface. I never got strongly involved in basalts, which are lavas, the second most abundant rocks after granites.

COHEN: The stuff that comes up from the lavas is the basalts?

WYLLIE: Basalt is the name for the most abundant lava, yes. There are two major earth-forming processes. The first stage involves melting basalt lava from the Earth's deep interior that rises to the surface and forms the crust beneath oceans. The second stage is making granites and continents. Granites may be derived from basalts when they've been hydrated and turned into amphibolite, which is then part of the continent—a short-cut description—and we did many experiments melting amphibolite at high pressures.

COHEN: Could you get an age on this from what you were doing?

WYLLIE: No. I wasn't ever involved with ages, just with process and the origins of rocks—conditions of depth, temperature, and composition, specifically the effect of the volatile components, like water, carbon dioxide, fluorine, and chlorine.

COHEN: Did you duplicate all this high-pressure equipment here at Caltech?

WYLLIE: I brought it from Chicago. They let me bring my two big high-pressure presses, because the NSF had paid for them. The dean and some faculty at Chicago seemed very annoyed with me for leaving. I had already started my second term as chairman at Chicago. I said to the dean, "Well, I've got to leave after one year, next year, but I'd be happy to carry on as

chairman and do my very best for Chicago.” But, no, I was immediately removed [laughter], and the department wanted to hang onto my high-pressure apparatus, the presses.

COHEN: Was anybody else using them?

WYLLIE: No, but they thought that maybe they’d appoint another person in this field. When the dean heard about it, he was mad. He said, “We don’t work that way. Of course you may take it with you.”

COHEN: So that was OK. It was not Chicago’s stuff; it was the NSF’s stuff.

WYLLIE: Yes. Well, it came from the NSF, but it belongs to the university. The university has a right to keep it.

COHEN: I see. They had to give you permission to do that.

WYLLIE: Oh, yes, they had to give permission. There was a formal written agreement between the universities.

COHEN: So then you brought the stuff here. Is it still in use?

WYLLIE: Yes. Two years ago I handed it over to the next generation, which is Paul Asimow. Just yesterday I discovered that the oldest one of the presses is being torn down because it has become too bent with age.

COHEN: You’re not using it anymore? You said you’re just doing writing now.

WYLLIE: Yes. My lab closed in 1999 and I’m writing a textbook.

COHEN: OK. So we have taken you now to leaving the chairmanship, and you became a professor. What courses did you teach?

WYLLIE: I never really fitted into the teaching program here, which I always felt unhappy about,

because there were already too many experts in my area to do the basic courses in igneous petrology. I shared in a few graduate courses. I taught a course in phase equilibria.

COHEN: I see. Who here does things closest to you?

WYLLIE: Ed [Edward M.] Stolper, Lee Silver, Hugh Taylor, Arden Albee, George Rossman. All of the courses that I would normally slot into were occupied, so I took on the introductory class, Ge 1, which had been one of my favorite classes in Chicago. In Chicago I taught the big non-science-major class, with 350 students, and also the much smaller foundation class for our majors in earth sciences. It was very different here. All the students were well-prepared science majors, and they included those who would become our majors.

COHEN: That would be for geologists. Was there nothing else in general for the rest of the students?

WYLLIE: No, although Ge 1 was taken as an elective by majors in others subjects. Apparently, some years earlier, there had been a common core that included one course in astronomy, one in geology, and one in biology, but then that was cancelled. I don't know the details. Everybody had to take one of those three, and a lot of them took geology. I know that Bob Sharp used to teach a big introductory class. I've met many people who have said they were in Bob Sharp's class and really loved it. I don't know how many students they had—probably 100 or 150.

COHEN: I see. So this was a big course for non-geology majors, and all students had to take one of these three?

WYLLIE: I don't know whether they chose one or whether they had to take all of them. I would guess they had to choose one, but I don't know. It was long before my time, and I don't know why it was abolished. Apparently the absence of such courses was responsible for a growing feeling that Caltech students were coming in too specialized and going out without having the opportunity to see other things. Biology had been pressing for some time, I think.

COHEN: OK. Now let's go back to your teaching. You said you were developing—

WYLLIE: I taught the foundation course, Ge 1, for the division, and various efforts with graduate classes. However, we had too many people in the area teaching too few students. The basic courses I used to teach at Chicago simply weren't viable for me here.

COHEN: Was that because people were more specialized early on, or more advanced?

WYLLIE: No, no. It was because too many people already taught those courses, you see. People who had been teaching for years were not going to step aside to let somebody else come in. Why should they?

COHEN: I see. The teaching load is one or two courses.

WYLLIE: I taught two courses in an academic year. That was pretty standard for us.

COHEN: That's not a very heavy load.

WYLLIE: No, no.

COHEN: Was that true of all the people?

WYLLIE: Most people. A few people taught three.

COHEN: Was that because you didn't have too many graduate students or geology students and you were top-heavy in faculty?

WYLLIE: Yes, but we had good coverage of the field—selective, not complete coverage. We needed more faculty to cover everything. In the research field we had very good coverage of the areas in which we were hottest, in which our reputation had been developed. I don't think Caltech has ever designed faculties on the basis of the number of students that were in any division, as far as I know. Engineering was complaining about the fact that they had too many students for their faculty, and that was perfectly true.

COHEN: That's an interesting comment that you're making—that Caltech does not organize its

course offerings or professors around how many students they have.

WYLLIE: I've always assumed that was true. I've never heard anything different. That's in contrast with a state university, where, when you start losing students, you're likely to lose a few faculty slots. The way I understood it was that Caltech has never had any faculty slots. You decide you need a faculty member in a broad area or a specialized area. If you get permission from the provost to appoint a search committee, the search committee goes and looks for a person in that area. It may or may not have to do with teaching, but teaching is never the primary concern. The concern is that this is an area that will benefit from a good researcher.

COHEN: I have the impression that you're very interested in teaching.

WYLLIE: Oh, yes, that is true.

COHEN: So then, how did it evolve into this more general course that you started to teach?

WYLLIE: Oh, not a more general course here at Caltech. No. My course was Ge 1, which then became Ge 11, but it's still the foundation course designed for our majors. It became Ge 11 when we had the new menu course, Ge 1, which I will get to. I was teaching the foundation course in geology. Caltech's common core committee had for some time— **[Tape ends]**

Begin Tape 2, Side 2

COHEN: Let's proceed with your teaching.

WYLLIE: Yes. I don't know how it came about, but the core committee was beginning to discuss whether or not it was desirable to broaden the general science education of Caltech students. The question was raised at a faculty meeting. The people in our division weren't terribly enthusiastic about it, because the thought of teaching 150 students was rather daunting. We were all invited to submit statements to the core committee—I know I showed you a couple. I wrote a thing for the chairman of some Caltech committee, explaining why I thought it was important to have general science education for our students, and why I thought earth sciences would make a

very good cause. Then they had a whole series of committee meetings. I kept on going to the succession of committees and pitching the same story. Eventually the issue got to the Faculty Board for discussion. The proposal was for a menu of courses, where there would be two, three, or four—a reasonable number of courses broader than physics, chemistry, and math would be offered—and students would have to take one of these, in addition to biology. The Biology Division argued for and got the Faculty Board to accept that theirs should be a required course, so biology became a required course in the common core. This was general education. I'm calling it general education because I gather that's what it's called elsewhere, although it's more specialized here, of course. Then there were two menu courses accepted, one in geology—earth sciences—and the other one in astronomy. Now, you saw the memos I've written at various times. Since 1991 I had been strongly advocating at every opportunity that our division should do this—for example, to our division visiting committee. At faculty meetings, when it was suggested that we should look into this and decide what to do, I was instrumental in—I think at one meeting, the chairman at the time said, “If anybody wants to look into this, get something written, and present it to the faculty, please do so.” So I got a group together in March 1993. We had several people in the division who were very, very keen on this, while most seemed to be totally disinterested—at that time, anyway. So we got material written up, summarized, and made the pitch to the division. The division thought, “Yes, this would be a good thing,” because we had very few undergraduates. Our majors were small numbers—five to ten each year.

COHEN: Of undergraduates in geology?

WYLLIE: Yes. Total undergraduate majors in geology and planetary sciences were perhaps twenty, for years and years, with probably five or six graduating each year. I may not have the numbers right, but it was on that order. There are lots of arguments for exposing students to earth sciences, and I've got them all written down, so I won't try and recover them from memory. Except that this is very important to me. Many science students, especially good ones who have been dedicated in high school, come in knowing physics, chemistry, and math, and nothing else. They may have had biology these days, but really know nothing else. They come in, they get involved in these courses, they graduate in physics, chemistry, and math, and many of them don't know what they want to do with it. This coming century, I think, we are all going

to be dominated by biology and earth sciences, because we are the earth sciences, and people are integral parts of the geological cycles. We are changing the cycles, as everybody knows, whether they accept it or not. Our total environment is going to be changed through the century by us, by what we do. People have to make decisions, whether they're in government, in business, or voting citizens. They've got to make decisions about land use, vehicles, all of these environmental problems, which are really scientific problems. They are really earth science problems. It's one thing to talk about saving the rain forests, but that's a very, very small part of this total environmental thing, which is the earth sciences. If we don't have a public that is reasonably educated in this—well, I must admit I'm a pessimist. I think things are going to get very bad by the middle of the century for *Homo sapiens*, but we might do better if we know something about the science. So I think it's very important for all educated people to know something about the land they build on, the air they breathe, and the water they drink. The earth sciences need people who are strong in physics, chemistry, and math. A lot of people go into geology because it's a relatively easy option for those not so good at math. They take science, they struggle, and they think, "Oh, God, what am I going to do?" Then they find that geology is attractive, and in many areas it's not too hard, but it requires different skills. You can do well in some aspects of geology without much quantitative basic science.

COHEN: So you say that's sort of a cop-out for some people wanting to do science?

WYLLIE: An escape hatch, rather than a cop-out. Many mathematicians and physicists would probably make lousy geologists. Some people in geology are there because it turns out to be another path when they get into trouble with the basic sciences. But earth sciences also needs really good scientifically based people, and here at Caltech we have faculty like that and we have students like that. I made the case several times that if we could just expose students—because this is what happened to me—if we could just expose our students to some of the problems in earth sciences, they will recognize that those are beautiful problems, that those are relevant problems, and they may find that they'll turn their skills to solving some of those problems. That's the kind of case I made to our division faculty, to the Faculty Board committee, and to the committees that were set up to look into menu prospects. I volunteered to do it. I said, "This is a course I'd like to teach."

COHEN: They usually go along with something like that by saying, “If you can do it, fine, go ahead.”

WYLLIE: We’ve also had two revolutions in earth sciences. In 1967 or ’68, there was a revolution in plate tectonics, which was a first step toward recognizing earth processes as part of one series of continuously interlocking cycles. Whereas it used to be petrology, paleontology, economic geology, mineralogy, a lot of separate disciplines, it became clear that earth processes were all interrelated and a multidisciplinary approach was the way to go. Then, in the nineties, we had a developmental revolution into earth systems science, where plate tectonics, which dealt with the outer part of the Earth, became thoroughly incorporated into study of the interior of the Earth, the atmosphere, and the oceans, which control our obvious immediate environment. The whole thing is a wonderful series of interlocking systems, and the phrase “earth systems science” became current. That was what I was suggesting we should teach. In fact, at the final meeting of the Caltech faculty, when this whole menu requirement was discussed, I was asked to make the case for earth sciences being one of the menu courses. I distributed the course outline of what I would teach if I were given the privilege of doing it. I don’t remember how long it took, but the Faculty Board finally approved the addition of menu courses. It must have been the year ’96-’97 when the courses started. Biology got the required course, and astronomy and earth sciences got two menu courses.

COHEN: So they could choose either one of them, earth science or astronomy?

WYLLIE: Yes.

COHEN: I see, as part of the core.

WYLLIE: Yes. But I didn’t get to teach the course. [Laughter]

COHEN: Who taught it?

WYLLIE: Dave [David J.] Stevenson. He did a fantastic job.

COHEN: I noted from your write-up that you wrote, “By the time this gets done, I’ll have so little time to develop and teach it, it wouldn’t pay for me to teach it.”

WYLLIE: I wrote that?

COHEN: Well, in one of your things here you said that.

WYLLIE: Oh. I was probably criticizing the Caltech committees for taking such a long time to make a decision. That was probably it, yes.

COHEN: This course [Earth and Environment] has been taught now, and I think you wrote somewhere here that it’s attracted a lot of people to geology.

WYLLIE: Dave Stevenson taught it for three years. I sat in on his course for the first year so that I could be sure that my foundation course didn’t overlap, so that students who came from his Ge 1 course into my Ge 11 course [Introduction to Earth and Planetary Sciences] weren’t given the same old stuff over again. He taught an incredible course, and he got the teaching prize for it. He did a very novel thing. Instead of having the course with a lab, which was going to be a difficult problem for us—

COHEN: Because there were so many students?

WYLLIE: Yes. What used to be big teaching labs in the old days had been chopped up into small research laboratories, so we didn’t have any big teaching lab spaces. Bob Sharp used to have a great long lab right opposite where I live, which was the introductory laboratory, but we just didn’t have that space anymore. Dave Stevenson had a series of ten research projects, I think: lectures for the first five weeks, trailing off for the next two or three weeks; then group research projects run by individual faculty members assisted by graduate students, involving field trips, work in labs, and so on. So they all got specialized hands-on research through the last three or four weeks of the course. It was very good.

COHEN: So that’s been very successful. Of course, he had to get these faculty members to be

willing to do this. [Laughter]

WYLLIE: Yes. [Laughter] As academic officer, I wrote the nominations for him for this teaching award. I always had a little paragraph saying, “Not only was he a superb teacher, but he also was a great manager, not only of the course but also of the ten faculty members that he managed to drag into this program.” They all liked to do it, but it’s very difficult managing ten faculty members within the confines—

COHEN: It’s like herding cats. [Laughter]

WYLLIE: Yes, yes. [Laughter]

COHEN: So this course now is just part of the menu.

WYLLIE: Yes. Ken [Kenneth A.] Farley took over from Dave. He’s taught it one year; he’s just starting his second.

COHEN: I think you mentioned that this has increased the number of geology undergraduates.

WYLLIE: Yes. Whereas we had maybe twenty undergraduates before, for years and years—that’s for one year, twenty majors—I think we’re over sixty now. It was a slow climb.

COHEN: But definitely attributable to the fact that it opened up the field.

WYLLIE: Oh, definitely. There’s no doubt at all, no doubt whatsoever that students saw the subject, they liked it, and they realized that they could do things with their physics, chemistry, biology, and math. So here we are. It’s worked.

COHEN: It is a success.

WYLLIE: That reminds me of something else. Hall Daily, then director of public affairs, was running a formal outreach program, and he asked me if I could get some graduate students to go into three high schools and one middle school to stimulate interest in science. I’m sure he had

students from other divisions as well. We had three graduate students going into schools once a week. Apparently they were very successful. The teachers loved them, and the students loved them. One of Ed Stolper's students worked with volcanoes, and she got them all to devise some wonderful experiments. The students would ask, "When is Miss Volcano coming again?" This program took Caltech into the classrooms, and then 300 Pasadena School District students attended my lecture at Caltech, which was the conclusion of the science education element of the Caltech centennial celebration.

COHEN: I think that was not a high school program. I think that was—

WYLLIE: You're right. It wasn't only high school; there was a middle school included. I always used to accept invitations to give talks on geology at schools, and when I ceased to be chairman I had more time to indulge.

In 1990 I joined Caltech's Committee on Secondary School Relations. Jim [James M.] Bower was the very dynamic chairman, and Jerry [Jerome] Pine was with us. Lee Browne, the man who used to run the summer programs, had retired. The first year I was on the committee, we decided to start this new program called YESS [Young Engineers and Science Students], which replaced Lee Browne's summer activities to some extent. I don't know how the money was raised—I did know, because I was on the committee, but I have forgotten. It was Jim who pushed. He wrote proposals for grants and got money for the program. It was different from the previous programs in the sense that it was for forty students from disadvantaged homes, homes where parents had not been to college and the students didn't even know if they were going to college.

COHEN: I see. So it was not their minority effort?

WYLLIE: It wasn't exclusively for minorities.

COHEN: Lee Browne tried.

WYLLIE: Yes, he tried. I understand that he had a lot of parents who treated this as a summer camp—wealthy parents. YESS wasn't directed to minorities per se, but obviously it connected

very largely with minority groups. The committee worked hard to develop a new academic program and find faculty to participate. We wanted to get faculty volunteers to do the teaching for the summer. There were also student associates from our undergraduates.

COHEN: Lee Browne just depended on graduate students, if I'm not mistaken.

WYLLIE: I had no contact with Lee Browne's earlier programs. Since I was on the committee, I figured my job was to make sure that all sciences were properly represented; otherwise they wouldn't have been. There was often a tendency to just cover basic physics, chemistry, and math. They brought in engineering.

COHEN: But no geology?

WYLLIE: Yes, but because I was there, I made sure that it was included. The committee met and organized, found the instructors, and worked out the curriculum.

COHEN: Was it a Saturday morning program?

WYLLIE: No, no. The students came for four or five weeks during the summer. They lived in residence halls. They had very intense classes. This is the kind of thing we discussed—how intense should the course be? After a while, it got tied in with CAPSI [Caltech Pre-College Science Initiative] on North Hill Street, because Jim Bower was involved with that, too. For several years it was independent, allied with Student Affairs.

COHEN: Did they pay tuition themselves? Who paid for it?

WYLLIE: It was all paid for. The money was raised through the grants obtained mainly through the efforts of Jim Bower. Caltech subsidized it to some extent after a while, I think. When the primary fund-raising efforts faded and the resources had been reduced, Student Affairs took it over from our committee. We were involved in an advisory capacity, but they ran it; previously the committee had run it. I don't remember the details now. I think Jim Bower didn't like some of the things imposed on the organization by Student Affairs. I think he may have left us.

COHEN: He may have gotten more involved with the CAPSI program.

WYLLIE: Yes, that may be it; he got more involved with CAPSI. Then there was a new director of minority affairs, whatever the title was. A lady came in. I had two or three graduate students from geology involved, and they organized undergraduate counselors. I heard from an undergraduate that this lady was really messing things up, and he was terribly upset. There was a geology field trip. He came in one day and said he was furious because the bus was there waiting, it was all loaded, and the director came an hour late. [Laughter] I don't know what happened. He just told me that she was really messing things up. Field trips must run on time.

COHEN: There was dissension in the ranks. [Laughter] That program didn't last very long—a couple of years?

WYLLIE: Oh, it was longer than that. At least three, when we were physically running it. Then for a couple of years it was joint—we used to meet in CAPSI, and the YESS committee would come in to meet and discuss things. Someone from Student Affairs was running it. We weren't involved in the details anymore. Then it kind of fizzled out, I don't know exactly how. I think Student Affairs, or maybe the Caltech administration, decided that the increasing amount of money that they were asked to put into it was too much.

COHEN: But you did work with it for a couple years.

WYLLIE: I was on the secondary schools committee between 1990 and 1995, and I think it was in 1994 that YESS was being taken over by Student Affairs. From 1995 to 1997, the name was changed to the Educational Outreach committee.

COHEN: Now, when did you become academic officer?

WYLLIE: When Ed Stolper became [geology division] chairman in 1994, he asked me if I would be his academic officer. Most of this activity to get us into the common core was before '94. He knew I was interested in teaching. I was academic officer for five years, 1994 to 1999.

COHEN: What did being academic officer entail?

WYLLIE: Well, nominal responsibility for all of the graduate and undergraduate students and the curriculum.

COHEN: How is that different from the chairman?

WYLLIE: As I understand it, this is the equivalent of an executive officer in other divisions. I was involved with students primarily, incoming students, arranging the review of all of the graduate applications, when the faculty sit down and say whom we should admit and whom we should not admit. The students and their courses, curriculum planning, degree policies, and student support and welfare.

COHEN: So you took this over and evidently you did fine. Did you enjoy doing it?

WYLLIE: Oh, yes, yes. Ed always seemed very pleased. I think I was more active, or more proactive—more Caltech aggressive—than during my previous Caltech administrative role.

COHEN: You learned. [Laughter]

WYLLIE: Yes.

COHEN: You did this for several years?

WYLLIE: Five years, yes. It's a five-year term.

COHEN: Did it go smoothly and you enjoyed it?

WYLLIE: Yes.

COHEN: Then you became emeritus. I see.

WYLLIE: There are several things I'm pleased with in that academic-officer period. One was

getting this menu course started, although I wasn't teaching it. Dave Stevenson, I'm sure, did a much better job for the Caltech students than I would have done.

COHEN: Well—

WYLLIE: I think so. I mean, my book for non-science majors—the style wasn't quantitative enough for Caltech students, which I learned with Ge 1. Dave is a superb teacher. He's got one of these universal brains that knows everything. He can pluck facts out of the air and spin them into an interesting story. I'm sure he did a much better job than I could have done. We got that going, and then, to prepare for the anticipated additional number of students, we revised the undergraduate curriculum.

COHEN: The whole thing?

WYLLIE: Yes. I suspect it's the same across campus, but our individual faculty members tend to teach the course that they want to teach. Our undergraduate program was very much a series of rather isolated courses. I think a state university would be appalled to see this kind of structure, because a state university has a mission to cover the field. Our undergraduate offerings attempted to cover the field, but it was a very— I refer to it as a green-stamp approach, where green stamps are stuck in various places until you've got enough, and then you graduate. Consequently, they didn't have a rounded program, and they didn't have a rounded structure when they left and went to graduate school. Although our students were very good, not all of them did well in graduate schools in other places. I had a couple come to Chicago. I always felt it was because somehow they didn't quite grasp the total field. I heard similar comments about our students when I was on the Stanford visiting committee. I'm waffling a bit. The division has graduated many first-class students.

COHEN: Well, I know what you're saying—that people come here because they have an idea of doing one thing and not the overall field, and they do their thing exclusively. Nobody says you have to do this and that.

WYLLIE: They somehow weren't mentally adjusted to stretch. I can imagine a lot of my faculty

colleagues saying, “Oh, what nonsense.” [Laughter] Never mind. In fact, they were good enough that they could stretch where needed to cover the information gaps. We could get away with it with a small number of students—small classes, two or three, or four or five students. Undergraduate classes were commonly like that, except for the intro class—the old Ge 1—which had from fifteen to thirty students. However, with the anticipation that we were going to attract more students— Ed and I had often talked before that about getting a decent undergraduate curriculum going. Ed started off by appointing an academic committee. We have four options [geophysics, geochemistry, geology, and planetary science—ed.], each of which has a representative who’s responsible for that option. They formed the academic committee with me as chairman, and we decided that we really should have a go at this.

COHEN: A coordinated course of study.

WYLLIE: There have been several aborted attempts since I’ve been here to do something along these lines, but we sat down and we worked. We talked to lots of people. Finally, after lots of revisions, we got an approved comprehensive course sequence—a series of sequences with vertical continuity and lateral connections for the whole division.

COHEN: That’s really very good. That’s almost a miracle.

WYLLIE: Yes. I was very pleased with it. It took nearly five years and a collaborative academic committee. Ed Stolper’s support persuaded the faculty.

COHEN: So you really have done something of great value as far as the teaching of geology goes here.

WYLLIE: On paper, yes. How it’s working out, I don’t know, but what the faculty finally approved in 1998-99 had everybody feeling pleased. Oh, I know what has changed: Our division has really changed structure. We’ve got maybe ten new young faculty who have come in the last eight years, because we had this huge bulge of people retiring. Faculty meetings now have quite a different character. There are these ten new people who don’t know the history, which has always been dragged out during discussions. Whenever a suggestion for change

came—of the curriculum or anything to do with graduate students—one or another of the faculty members would say, “Oh, we tried that fifteen years ago. It doesn’t work. It doesn’t work.”

[Laughter]

COHEN: So getting rid of the old guys and getting in the new ones has really opened things up?

WYLLIE: Yes, but this curriculum change was completed before many of them rolled in. I expect that by now the curriculum has been modified.

COHEN: All the people who came in the sixties have retired and they’re still here, physically here.

WYLLIE: Yes. We have thirty faculty members on our notice board. We also have twelve emeritus faculty.

COHEN: And they’re all here.

WYLLIE: Most of them, yes. [Laughter] There’s another committee I should mention, because it’s central to Caltech’s academic mission. In April 1994, Tom [Thomas E.] Everhart [Caltech president 1987-1997] appointed me as chair of SARC, the Student Affairs Review Committee. The charge was to assess the present strengths and weaknesses in Student Affairs and to advise the president how Student Affairs can be made more effective and can best serve the students and the institute. This assessment started with the recommendations of the [John E.] Bercaw committee of 1989 and Gary Lorden’s four goals of the same year. We had a great committee—Diana Barkan, John Bercaw, Paul Dimotakis, Steve Frautschi, Jean-Paul Revel—and through May and June we interviewed many, many people from all sections of the Caltech community. The report was delivered to the president in August, and presented to the Faculty Board in November 1994. I won’t attempt to recover the recommendations here, but presumably the report can be filed with my papers.

COHEN: I think this is maybe a good place to stop, because I still want to talk about that other parallel route of international stuff that you’ve been doing. **[Tape ends]**

PETER J. WYLLIE

SESSION 3

April 2, 2002

Begin Tape 3, Side 1

WYLLIE: I didn't talk about Romy. When I came as [division] chairman in June of 1983, Romy definitely came as a chairman's wife.

COHEN: What does that mean?

WYLLIE: She had built up her interior-design company in Chicago, and when we moved here she let that slide into decline. She didn't bother to build up contacts and clients to replace her abundant Chicago contacts. We thought of a chairman's wife as being a social hostess for the division.

COHEN: I see. So you thought it was a dual job, in some sense.

WYLLIE: Yes. We made efforts to have a series of dinners at our house and bring in all the faculty members in small groups. We had annual parties around the pool and that kind of thing.

COHEN: I'm sure it was much appreciated.

WYLLIE: I think so. Whether it was or whether it wasn't—I guess these days it's old-fashioned to think in those terms. However, that was 1983, and we had come from an environment where there was a lot of social activity. The University of Chicago had many social functions, and we were strongly involved in them. As chairman in Chicago we were socially active, and we thought we'd do the same thing when we came here. So we tried that and thought that Romy wouldn't go to work to build up her company again in Los Angeles.

COHEN: There is another question that maybe we should take up. You have had, of course, your full share of graduate students and postdocs through the years, and I was wondering if it was

significant.

WYLLIE: I'd like to do some homework on that first. I do have a table with all of my graduate students and postdocs, which I prepared when I got the Roebling Medal [of the Mineralogical Society of America] last year, but I haven't looked at it. Rather than do injustice to somebody, I'd rather look at it first.

COHEN: That is often something one wants to have as part of the record.

WYLLIE: That also follows and ties in with the specifics of my research and achievements, if there's any point in going into those. So far I just gave you a very broad woolly statement of my approach. If you want to talk about medals in that context, I could talk about specific research achievements and people.

COHEN: OK. Well, let's talk then about these organizations in general—how you became affiliated with them and why for some you put in a big effort and for some you did not.

WYLLIE: I did think about that, and looked up some dates, because you warned me that this was coming. So I know that my first international meeting was the International Geological Congress in Copenhagen in 1960, when I was back in Leeds. I think I told you that somebody gave me a couple of hundred pounds to go and present the results I got at Penn State.

COHEN: That's where you realized that you and Romy really would rather live in a more open society.

WYLLIE: Yes, where there was incentive. Did I talk about incentive?

COHEN: No, you did say there were things going on.

WYLLIE: OK. This is an afterthought. When people asked us why we were going back to the States, I told you the other day that we decided that we could do more for our family and children in the States than in Britain. I used to use one word, "incentive," to explain why I came

back, because we recognized already in 1960 that in the States there was an incentive to work: If you worked hard and you did well, you were promoted, you were advanced, you were recognized, and you got paid for it. In Britain at that time, that didn't seem to happen. The salary in the university was definitely an age-related salary scale. The only way you could get advancement was to get promoted to professor, which meant you were *the* professor, the chairman of the department at that time. Who wanted that kind of a job at a young age, even if the opportunity arose, which was unlikely for a youngster? I did tell you that there didn't seem to be much excitement about research, not much discussion about research, whereas in the States everybody was excited about it. So the one word, "incentive"—I used to use that to sum up why we moved.

COHEN: You spoke very enthusiastically about the feeling of fresh air when you got to the meeting with your colleagues.

WYLLIE: Research enthusiasm and a sociable life, yes. So that was my first meeting. I went immediately afterwards to the International Union of Geodesy and Geophysics, which was in Helsinki. There I met up again with Caltech people. I remember in particular Sam Epstein sitting in a restaurant, looking out of the window, and counting up to ten in Finnish as ten attractive Finnish ladies marched by. [Laughter]

COHEN: I can imagine that. [Laughter]

WYLLIE: Bob Clayton and Hugh Taylor were there, too. Those were my first two contacts with these two international organizations. I didn't pay much attention to them. Then, about 1970, I was asked to join a commission of the International Union of Geological Sciences—a new Commission on Experimental Petrology.

COHEN: This was the new field that you were very involved in.

WYLLIE: This is the field where I was making my young mark, yes. There had been a Commission on Petrology, which was very, very classical, and apparently it had come to be dominated by a couple of Europeans who liked very much being in charge of commissions. It

gave them a kind of a stature, a little budget, and so on. IUGS wanted to break this classical mold, so they said, “Let’s get rid of this old Commission on Petrology and have a new Commission on Experimental Petrology, and would you join it?” So I joined it. I was a member from 1970 to 1992. Somewhere in there I was chairman for four years [1976-1980].

Commissions for these international unions do various things. Some are strongly research-oriented. Some do nothing except have a chairman or a secretary go to meetings and say, “I am the chairman.” We decided that what we should try to do was to get the information, the results coming from experimental petrology, spread as far as possible through the community.

COHEN: Now, were there Caltech people among the people you were involved with?

WYLLIE: No, I was the only American. There was an American, a German, an Australian—

COHEN: I see. You were the delegate.

WYLLIE: Not a delegate for the United States. The union selected the members of the commission, and not every country was represented, but there was an attempt to have geographical coverage.

COHEN: OK. I imagine the countries where research was going on—

WYLLIE: Yes, that’s right. My three closest colleagues: One was Werner Schreyer in Germany, and I’ll be presenting him for the Roebling Medal this year; another one was Dave [David H.] Green from Tasmania, who is a very famous person now; and another was William MacKenzie from England, my former companion in West Greenland. Anyway, we had a very good, active group. Geologists in general were sort of frightened by this stuff, because it was physical chemistry and phase diagrams, and it turned off a lot of geologists.

COHEN: They’re afraid of chemistry?

WYLLIE: Yes, many of them were, but I hasten to add that this has changed, because now we have many geochemists solving geological problems. Although the results were clearly strongly

applicable and people were reaching for the results and using them, we saw an opportunity or a need—perhaps a bit of both—to generate symposia where the results could be brought to a broader public without people having to study the papers. So that's what we did. We looked for every big international geological conference, and a lot of small specialized ones, and we organized special symposia involving, let's say, experimental petrology and geophysics.

COHEN: So you didn't try to set up your own meetings, but you went to meetings already in existence and put your subject in.

WYLLIE: Yes.

COHEN: Did you have a good response to that? People came?

WYLLIE: Oh, yes, yes. People were very anxious to understand and to use these results, because they were clearly capable of changing the way people thought about many aspects of earth processes—not just geology but earth processes. So that was that one. Then what? Oh, I became president of the Mineralogical Society of America in 1977—one year as vice-president, one year as president, and quite a few years on the executive council.

COHEN: What does that group do?

WYLLIE: It's rather complicated. It's the focus for mineralogy and petrology within the United States. The society runs a journal, the *American Mineralogist*, and it runs two meetings a year, the annual meetings of the Mineralogical Society of America. One was tied in with the Geological Society [of America] meeting in the fall, and the other one was tied in with the American Geophysical Union in the spring and in December.

COHEN: So that would not be an international thing.

WYLLIE: No. It is American, but it has a strong international influence, with membership extending across the world. In fact, in recent years it's teamed up with an international meeting called the Goldschmidt Conference of Mineralogy and Geochemistry, and it cosponsors that.

COHEN: When you say “America,” does that include Canada and Mexico?

WYLLIE: Canada has its own Mineralogical Association of Canada. They have their own journal [*The Canadian Mineralogist*]. However, I think that most Canadian mineralogists will also belong to the American society. So that was a nice, simple job—one year, and the secretaries did all the work.

COHEN: Do you go to these meetings regularly?

WYLLIE: Oh, yes. Well, I did until 1993, when the brushfire came through here in Pasadena. I’d never missed one of these domestic annual meetings. I always went to all these meetings from way back. I never dreamed of missing one, but that year I stayed home, because Romy had had a minor operation and stayed in bed. The fire came through when the meeting would have been on. I would have been in Boston when that fire came through.

COHEN: Did you lose your house?

WYLLIE: No, we were one of the lucky ones. Lucky or unlucky, whichever way you look at it—lucky because we saved our possessions, unlucky because we have the same old house when everybody around us has a bigger new house. [Laughter] We were very lucky, very lucky. Having missed that meeting for the first time, I decided I could live without it.

COHEN: You haven’t bothered going back?

WYLLIE: I was getting so busy with international things by then—I had so many international trips—I thought, “Well, I can do without these.” So I didn’t go again until last year, when I got the Roebling Medal from the Mineralogical Society.

COHEN: We’ll talk about that a little afterwards. What were the international meetings that you went to?

WYLLIE: Let’s see. I was president of MSA, the Mineralogical Society of America, in the mid-

seventies. I'm trying to remember when I first got involved. My next jump involved the International Mineralogical Association, which met once every four years, and I had been going to those fairly regularly. At the meeting in Novosibirsk in 1978 I was elected second vice-president, and I had four years in that position. Then in Bulgaria, in 1982, I became the first vice-president for another four years. Then in 1986 the meeting came to the States, and I became president until 1990. Then we had a meeting in China, and this was followed by four years as past president. So I had a total of sixteen years on the executive committee of IMA.

COHEN: That took you away quite a bit—at least twice a year?

WYLLIE: That was not a lot of meetings, because— Well, I guess it was. Yes, of course. The scientific meetings were once every four years. We had symposia in other international conferences. The executive committee met at least once a year, somewhere or other. We moved about. It was a small meeting, six to eight people sitting around a table for three or four days.

COHEN: You obviously enjoyed this sort of thing.

WYLLIE: It was a mixture of enjoyment and frustration. [Laughter] I got very tired of traveling, especially in coach-class seats across oceans. [Laughter] I don't know; I just got involved; I must have enjoyed it. Well, I did say no several times to these things, but I often had my arm twisted. The IMA was simple and straightforward. That was my subject, experimental petrology. Experimental mineralogy is part of mineralogy and petrology, which we consider to be under one umbrella. There aren't many societies of petrology, which deals with rocks. They grew up within the mineralogical societies, which were formed first. Minerals make up the rocks, of course, so that's how it happened. All the mineralogical societies are now trying to emphasize that they cover minerals, petrology, and geochemistry, but they retain the name "mineral." So this was my subject, my mainline subject, and I was very pleased to be involved.

COHEN: OK. So you have your professional services—I call that your citizenship. [Laughter] Were any of those particularly rewarding or frustrating?

WYLLIE: The IMA was very smooth until the last meeting, in Beijing in 1990, where I officiated.

At the meeting at Stanford in 1986 we had—I don't know how many—two or three invitations to host the next meeting in 1990, and the assembled delegates voted to go to China, to Beijing.

COHEN: That was before the Tiananmen Square—

WYLLIE: Tiananmen Square was '89. In '86 we decided we would accept the Chinese invitation to go to Beijing. The Chinese were trying very hard to develop an international outlook, and so for various reasons we thought that was a good place to go. I can't remember what the competing countries were now. That was fine until 1989, when Tiananmen Square came along. Then, of course, lots of people were sufficiently incensed by the political aspects of that that they said, "We should not go to Beijing. We should go somewhere else." The International Geological Congress was in Washington, DC, in 1989, and the International Mineralogical Association always organized symposia at the IGC, so I was there. IMA was always involved in the IGC, which also met every four years, but on a different cycle. The Chinese representatives were there, and the mineralogists were there, and we had a meeting with them.

COHEN: This was after Tiananmen Square?

WYLLIE: This was after Tiananmen Square. They had been working for three years to get the meeting going, and they were really anxious for us not to pull out.

COHEN: Did they offer any excuse?

WYLLIE: Do you mean an excuse for Tiananmen Square?

COHEN: Yes, or a rationale or anything?

WYLLIE: No, no. This had nothing to do with that.

COHEN: Did they talk about it?

WYLLIE: I'm sure we talked about it. But it was clear—

COHEN: They weren't going to say anything.

WYLLIE: When scientists talk together, we tend to ignore those things. Actually, I was in Prague, Czechoslovakia, for the IGC in 1968 when the Russians invaded. At that time, Russian geologists were apologizing to us, but I can't remember our Chinese friends apologizing about Tiananmen Square. They were obviously embarrassed by it, but their main concern was, "Are we going to be able to hold this meeting?"

COHEN: They wanted the meeting?

WYLLIE: They wanted the meeting, yes.

COHEN: You guys were quite split about this, I gather.

WYLLIE: Well, the mineralogical community was. The council at that meeting voted eight to two to go, to continue with that plan.

COHEN: So you were quite moved by the delegation from China.

WYLLIE: We were sympathetic about the delegation from China. My own view was that we shouldn't cancel the meeting for political reasons. I wrote a letter to the people who were objecting. Shall I read this bit?

COHEN: Is there a quote you want to read from the letter?

WYLLIE: This was to the national representatives, chairmen of commissions, working groups, and other friends of IMA for use in the congress in July '89. There are a couple of introductory paragraphs. Then, "Following the recent disturbances in China, some colleagues have written or phoned urging that the locale for the Fifteenth General Meeting be changed as a protest. Others argued that we should proceed as planned in order to support Chinese mineralogists who have been working hard for two years to organize the meeting, and thus to set an example that the spirit of international science transcends other actions. The council affirmed its policy that it was

inappropriate for IMA to offer formal political protest. Some people would suggest that we should. To what extent would the registration of foreign mineralogists decrease because individuals expressed personal protest? Attempts were made to estimate how many potential registrants would stay away. Many responded that it was too early to decide. They wanted to see how things developed in China. Members of the Chinese organizing committee assured us”—ah, they did tell us something—“They assured us that things have calmed down in Beijing and that plans were proceeding smoothly. After much deliberation, the council voted eight to two in favor of holding the general meeting in Beijing as planned.” So we made that recommendation.

I have another letter. “Dear Peter, I’m writing to you today as president of the International Mineralogical Association.” Oh, this is from Germany, the German representative saying that they had voted not to send an official delegation to the IMA meeting in Beijing. Individual Germans could go, but they would not send an official delegation.

COHEN: That’s good, sitting on the fence. [Laughter]

WYLLIE: Yes. He writes here, “These decisions are certainly not against IMA. They are a signal against the present Chinese government, whose bloody pursuit of students and intellectuals, men and women who are mentally most close to us, is still in our mind in terrible and fresh pictures.” This next statement represents a concern we had from many others. “I’ve written a letter propagating my conviction that the free world should not give hands to sanction this bloody system. Therefore I am writing to you today,” etc.

There was one other thing I wrote here. When we started getting these letters coming in, saying, “We’re not going to send our official delegation,” I prepared a long, basic reply.

COHEN: So people were saying that their people will go but they don’t want to be part of an official delegation?

WYLLIE: More than that. Some people were saying that the whole thing should be moved elsewhere. For example, the past president of the Mineralogical Society of America, E-an Zen, who is of Chinese origin but has been an American all his life [E-an Zen immigrated to the United States at age eighteen and became a U.S. citizen at thirty-five—ed.], printed a letter

saying, “Furthermore, with Chinese current efforts to portray a business-as-usual situation there, the IMA meeting might well be used as proof that the international mineralogical community is accepting that depiction, despite the numerous evidence of blatant violations of human rights. Please consider your decision to go to IMA carefully.” So here’s activity within the American mineralogical community from a former president to tell people not to go as individuals. The council of the Mineralogical Society of America received this letter, and some members of the council were reluctant to publish it, feeling that scientific affairs of the society should not be politicized. So there were those who felt that this was a political thing, and those who wanted to maintain that it was science and science should carry on, and that was my view.

COHEN: The meeting went on?

WYLLIE: The meeting went on, yes. Here’s what I wrote at one time: “Others maintain that every effort must be made to maintain a separation between politics and science. It is a fact that once one starts making judgments about which countries are not politically suitable hosts and which nationalities should be denied participation by a host country, international science will soon run out of acceptable countries. We sit on a wedge on which there is no single sharp boundary obvious to everyone. Everyone has their personal idea of where politics begins to take precedence over science.” That’s consistent with the position of ICSU, the International Council of Scientific Unions, and the IUGG, the International Union of Geodesy and Geophysics, which I moved into in ’91. IUGG members always made the point that it was these unions and these scientific meetings that kept Russian and American scientists talking all through the cold war.

COHEN: Of course, for some of these groups—I don’t know if your groups did—the government itself started to look at the list of people coming in and saying, “This person can come and this person can’t come. This country can come and this country can’t come.” That has happened in some organizations.

WYLLIE: Yes, but whenever that happened, as far as I know, the meeting was moved if the government interfered significantly with the free exchange. Several times through my twelve years with IUGG we had to appeal to the statements in ICSU, which insist that this cannot happen in free scientific works. We had to get ICSU involved in writing to the government to

give people visas and things like that.

COHEN: And that certainly happened.

WYLLIE: I know it happened, yes. I remember one meeting where it did not work. ICSU got involved too late, and some people from Iran were barred from coming to America for a meeting.

COHEN: And the meeting went on anyway?

WYLLIE: Yes, because it was a big meeting, this was a small group, and it was very late. We got ICSU involved with the U.S. government, but it was too late. It does happen.

COHEN: The policy would be, if the government interferes, then you don't need that.

WYLLIE: I'm trying to think if it ever actually happened on my watch. I think it did, but I can't recall what it was. The only one I remember is when people from Iran were denied a U.S. visa. But I read about it happening in other scientific communities.

COHEN: One can't solve the world's problems. Now, how about government committees?

WYLLIE: Before getting to home again, I should comment on my most significant international effort, and that was with the International Union of Geodesy and Geophysics, which is a big one. It has seven associations dealing with seismology, volcanoes, the Earth's magnetic field from the center to the outside, fresh-water hydrology, the oceans, the atmospheres, and geodesy.

COHEN: Everything leading up to the stars maybe? [Laughter]

WYLLIE: No, it doesn't go outside the envelope of the Earth's magnetic field, although we've been trying to include in every international meeting a symposium on the geology of space science. So we're reaching out to the stars, but not grasping them.

In 1991, I was at the quadrennial meeting of IUGG in Vienna, and the election procedure had gotten fouled up in some problems. [Laughter] There was a retiring secretary-general from

Belgium who had been there for many terms, and there was a French candidate for vice-president on the slate. Then, from the floor, during the discussion, France came up with a nomination for the secretary-general. They wanted to retain Gallic command of this position of secretary-general, which is very influential. I wasn't in the council, I wasn't involved, but I heard about it afterwards. The normal voting order was changed, the floor-generated secretary-general nominee from France was elected, and therefore the vice-president nominated by France could no longer be considered, so the whole slate had to be readjusted. Romy and I had just arrived. We were asleep at ten o'clock in the hotel, and my friend [Peter] Robin Brett phoned and asked if he could put my name up for vice-president. Robin was U.S. delegate on the nomination committee, I think. They had to have nominations from the floor without duplication nationalities on the bureau, and they had to have approval from all nominees. I said, "Oh, no thanks." Romy was beginning to wake up and hear what was going on, and she said, "No, no! Not that," having not quite finished my sixteen years with the IMA. He said, "Well, you probably won't get elected." I ended up saying, "OK, put my name on it." Anyway, I did get elected. [Laughter] So that was four years as vice-president of IUGG, until the next general assembly, in Boulder, where I was elected president. The IUGG has seven associations, and they all have their own international meetings on a four-year cycle, so there are lots and lots of meetings and executive committee meetings and bureau meetings. I had a lot of meetings through those four years, from '91 to '95. I declined to be nominated for president over and over again. I said, "No! No! No!" But in the end I was told that it was time for an American president—the last one was elected in 1963—and that the NSF would surely award me a grant to govern and an extra travel fund as well.

COHEN: So you wouldn't have to go coach. [Laughter]

WYLLIE: Oh, no, I had to go coach. They're not allowed to pay for business class, but by that time I had accumulated miles and I could upgrade to business quite often for the long flights.

COHEN: OK. Now, was it with this organization that you had your encounters with UNESCO?

WYLLIE: Yes, yes.

COHEN: That was an interesting story. Do you want to tell it?

WYLLIE: Well, I'll finish up IUGG first. Four years as vice-president, then four years as president, '95 to '99, up to our general assembly in Birmingham, England—a simple homecoming for me, instead of presiding in some exotic city. Now I'm winding up. I still have four years on the executive committee, but I only have two meetings between the major conferences, and my last general assembly will be next year in Sapporo, Japan. So that will be the end of it, thank goodness. [Laughter]

This science for the twenty-first century—the 1999 World Conference on Science [for the Twenty-First Century: A New Commitment]—the initiative came from within UNESCO. However, UNESCO realized that they couldn't handle the science, so they got ICSU to be the partner to handle the scientific part of what turned out to be a very political program. The conference objectives were to analyze where the natural sciences stand today, where they're heading, what their social impact has been, and what society expects from them.

COHEN: UNESCO wanted to discuss these points?

WYLLIE: Well, this was the stated aim of this conference, which was initiated within UNESCO. Finally, it would establish what efforts needed to be made to make science advance in response to these expectations and to the challenges posed by human and social development. If that sounds like a series of muddled statements, that's the feeling we got from this from beginning to end—dealing with platitudes and muddled statements. They had a preliminary version of a draft declaration on science and the use of scientific knowledge, which was supposed to be prepared as a result of, the product of, the meeting. They had it all drafted, circulated, and written before the meeting.

COHEN: Who is “they”?

WYLLIE: Some committee within or appointed by UNESCO.

COHEN: Who are the people in UNESCO? I don't think the United States has been part of UNESCO for a while, have they?

WYLLIE: They are involved, I think, but I gather they haven't paid their dues. I don't know the degree of involvement. I was told that it was somebody in a scientific position at UNESCO—and I don't know his name, I don't remember the details—who wanted to make a splash before he left UNESCO, before he retired. He thought that what he could do was to organize this World Conference on Science that would influence governments and be his legacy to UNESCO and the world, I suppose. That's the general sense I got of what was behind it. I don't know if it's true, but that's the sense I got.

COHEN: What finally happened?

WYLLIE: Well, I'm looking at an overhead now that I prepared when I reported to the executive meeting of the International Union on Geological Sciences. First of all, we had the announcement, in '97 to '98. Here's the announcement from UNESCO, with ICSU being incorporated to make sure the science was sound. The ICSU president sent a request to all the unions and various other people asking specifically for a detailed report on how your union could contribute towards these rather vague fora, which were listed in the preliminary announcement. They had Forum I: "Science, achievements, shortcomings and challenges;" Forum II: "Science in Society;" Forum III: "Towards a new commitment," that kind of thing—very broad, empty titles, but they had no substance. The president of ICSU sent this message around, and I sent back a very detailed report, ten pages, indicating how the earth sciences from IUGG and ICSU had a lot of functioning committees that deal with earth sciences and society. I sent this long report indicating how our union, all of our seven associations, could contribute to a meeting like this. A couple of months later— Oh, the ICSU special general assembly was then; that's another series of meetings. I had to go to the ICSU general meetings as an officer in the union [IUGG]. They had a conceptual outline of the program, but it was, again, a big empty program with vague phrases. There was no earth science in it anywhere. In late summer we got in touch with UNESCO again. Then, by the fall, they sent out forms to request invitations. You had to submit a request to be invited to register for this World Conference on Science. Again, there was still this conceptual outline program, but no details, no substance whatsoever. They were supposed to have a committee. My report and all the other reports were sent to the scientific program committee, but nobody ever heard what they were doing, what the results were. All of

these nice pink pages kept on coming out with the same skeletal framework. I had an exchange then with a man called [Maurizio] Iaccarino, who was the chief operator. I've got a letter from him in here somewhere—I know he lived in Italy. We would have meetings of our executive committee to discuss these matters. I sent a long statement of concerns, and I got a nice polite letter back, a report assuring us that earth sciences would be well represented in this. In December I had a request from the president of ICSU, who was new and temporary and didn't know things very well, asking for information and advice about specific topics like energy. Well, energy comes from oil and petroleum, which is a geology subject. Geophysicists find it. So I sent him a very, very detailed response, and none of it ever got anywhere. I don't know what happened.

COHEN: Maybe that big building there in Paris is kind of a black hole. [Laughter]

WYLLIE: Yes. They finally got a detailed program out—supposedly detailed program—but it was all still conceptual. The earth science, if it was there, was hidden and integrated. That's what we were assured by Wolfgang Eder, who was a UNESCO geologist. He said, "Don't worry. The earth science is in there. It may seem to be hidden, but it is integrated." During all of this time, we were trying to find out who were the speakers, who was going to present these programs. We had made the pitch that, yes, we would send some representatives to the meeting. They only wanted one or two people from each union at the meeting. I think they had quite a few participants from the U.S. National Academy of Sciences who were not academy scientists but academy staff. The thing was clearly directed towards that kind of person and to governments—government representatives of science who were not necessarily scientists themselves. I did see a detailed program after the conference, and I've forgotten the details. It was populated by well-known authors, well-known politicians who spoke about science—names. Few names that would be recognized as active scientists by scientists, I think.

COHEN: What finally happened? Did you do anything with this?

WYLLIE: I didn't, no. I wasn't going. The dates were only just before our IUGG General Assembly in Birmingham. In the end they had a couple of what they called satellite conferences in Budapest. The Budapest Geological Society organized a water conference, so our vice-

president, who had been chairman of our hydrological association, went to that. He represented us at that. He gave a scientific talk.

COHEN: I see. So there were some peripheral kinds of things going on.

WYLLIE: Organized very late. The meeting was in June or July. In March he learned that he could go and give a talk at this. I don't know what happened in the end. They had this big document, the "Declaration on Science and the Use of Scientific Knowledge," which I suppose was approved formally by the conference participants. On the last day, they were supposed to be discussing this and everybody signing off on it, but as I say, it was essentially written before the conference. I also heard from my friend who was at the water conference that on the last day, when they were supposed to be doing all this work, most of the delegates went off on tours. Presumably it was sent to various government agencies throughout the world, but I don't know to whom.

COHEN: Headquarters put out the statement, and that was the end of it. So that was your experience with UNESCO. [Laughter] Maybe there is a reason why the U.S. doesn't pay its dues.

WYLLIE: Yes. This is not the kind of thing, I guess, that goes into this interview, but I've got some of the correspondence here. This was my report/response to the request from the president of ICSU. There are many pages of dense stuff.

COHEN: Well, that will be good stuff to go in your papers. So that was your experience with UNESCO.

WYLLIE: Yes. I now understand the comment of an earlier president of IUGG, a Russian, Vladimir [Volodya] Keilis-Borok, who was still on the executive committee when I came on. He said he had spent days wandering through UNESCO. He said, "UNESCO has millions of dollars." Let me step back a bit. We, the IUGG, were trying to make the case—trying to persuade our colleague earth scientists—that we had to find some way to use the scientific knowledge and experience of all of our members and associations, and channel it in some way so

that it would be useful in helping to solve or address societal problems. That's been a constant theme, certainly since I've been there. Volodya, who's been a visitor at Caltech— [Tape ends]

Begin Tape 3, Side 2

WYLLIE: Volodya maintained that UNESCO had millions of dollars for this kind of effort, if only we could find it. That was his phrase, "If only we could find it." He said that he had spent useless days and weeks wandering through the halls of UNESCO, trying to find out where this money was, while he was president of IUGG. He said he never found it, never found the right contact.

COHEN: They put out a lot of posters with third-world-country aspects to them.

WYLLIE: I think ICSU has a tendency that way, too. So do the unions. IUGS, the geological union, has one program with UNESCO which has been very successful. It's called IGCP, International Geological Correlation Programme. It's a program that gets a sizeable chunk of UNESCO money for international geological projects that involve people in developing countries. It's aimed toward the developing countries, but it has very active participation by the developed countries; that's why it's successful. IUGG has lots of programs, lots of commissions, where the work is directed towards developing countries—workshops on water, workshops on earthquakes, workshops on volcanic hazards—things like that.

COHEN: So where do we go from here?

WYLLIE: I had a thought running around in my head about IUGG along these same lines. We had a big program that came out of our union. It was called Earthquakes and Megacities. In my four years as president, we had a committee going on megacities. We were trying to get all of the associations to develop a program devoted to the problems of megacities. The one on earthquakes got going, and the volcanologists did well with selected volcanoes and cities, but the megacity committee did not manage to get the seven associations coordinated.

COHEN: Well, earthquakes may be more important at the moment. OK. How about some of

these medals?

WYLLIE: Well, maybe before that, I should talk about this National Academy [of Sciences] committee report, because this also has to do with the earth sciences and society.

COHEN: Actually, I did want to ask you something having to do with your international and national organizations. The National Research Council and the NAS have many committees and boards. Have you been involved?

WYLLIE: I've never been elected onto any of the formal NAS committees because I was a foreign associate, and that's not permissible. I could join any of the NRC committees, and I've served on a few.

COHEN: Right. You don't have to be a member of the academy if you have an NRC committee. So foreign associates are not on the boards?

WYLLIE: Foreign associates can be on the NRC boards, but very few foreign associates are U.S. residents. They can't be members of NAS committees. Apart from being a member of various NRC committees through the years, which are all a blur now—I can't remember them—my main job, my main contribution, was being appointed chairman of the Committee on the Status and Research Objectives in the Solid-Earth Sciences: A Critical Assessment.

COHEN: What year was that?

WYLLIE: 1988 to 1993, when this report was published. 1988 is when Frank Press [then president of the NAS—ed.] called me and asked me if I'd be chairman of a committee. It was the first national survey of earth sciences as a discipline. When I came to Caltech, I heard a lot about the Field committee, which had been a [decadal] committee of astronomers led by [George B.] Field of Caltech.

COHEN: George Field was at Harvard.

WYLLIE: Oh. I heard his name so often, I thought for sure he must be at Caltech. [Laughter] Several other disciplines had done these national surveys, where they set up a list of priorities and hoped to leverage more money out of the government as a result. Earth sciences had never had one, and earth scientists in Washington, who were sensitive to these things, were getting more and more concerned about the fact that astronomy, for example—

COHEN: Well, they are the prime example. They were very successful.

WYLLIE: They would put in a list of ten telescopes, or ten big instruments, priority from one to ten, and they'd always get the top few. Then after a few years they'd put in another list of priorities and they'd get more. So finally it was decided that earth sciences could have such a survey, a review looking for a critical assessment of priorities. One reason it was possible was because they got a grant from the [W. M.] Keck Foundation. I don't know the details. I just know that Frank Press called me and asked if I would take this on, and I said OK. We had a big committee. Altogether, I think we had about 125 scientists involved in subcommittees and in working groups. We got all the material together, edited, and published in 1993. This is the book that was published: *Solid-Earth Sciences and Society*. It was very favorably reviewed by Caltech alumnus Gordon Eaton, then director of the Lamont-Doherty Earth Observatory of Columbia University and about to become director of the U.S. Geological Survey. He wrote, "...for the first time in a highly comprehensive and remarkably ambitious way is a road map for the future of our science that should and will guide significant decisions concerning strategic planning for and funding in support of research, as well as long needed and overdue revisions of Earth-science curricula across the nation." [*Eos*, 74:41, Oct. 12, 1993]

COHEN: It's a good-sized book. I'd say it's eight by eleven inches and certainly three-quarters of an inch thick. [Laughter]

WYLLIE: With 346 pages and, because we had Keck Foundation money, they were able to put a nice fancy cover on. I get it out just to enjoy this cover. It was called *Solid-Earth Sciences and Society* for these reasons. I think originally they wanted "Earth Sciences," but Washington jargon requires that the oceans and the atmosphere have to be separated from earth sciences because they have their own domains, their own territories, and their own funding agencies. In a

disciplinary sense, atmospheres and oceans are two separate sciences, and geology and earth sciences is another one. After plate tectonics, as I have said before, we had reached the stage where these things were all interrelated, and a multidisciplinary approach was the way to go. However, we had to put “Solid” in front because Washington required it. It should have been called *Earth Sciences and Society*. One of our top priorities in there was water. [Laughter]

Our first discussions involved what the approach should be. Half of the group preferred very, very strongly that we should deal simply with the science—like the astronomers, get a list of priorities, pure, simple science. The other view was that earth sciences were now so much involved with society. As I said the other day, society is changing earth sciences, and society is really a part of the Earth cycles, so the whole thing is integrated, and we couldn't really separate the science from society. Finally we all agreed that we would deal with “earth sciences and society,” so that is where the “Society” came in. That was implicit in the charge, but it wasn't specified, and it wasn't in any titles. We spent three years gathering information and arguing about priorities, and finally we got this thing written. It was supposed to go to governments, to deans' and presidents' offices, and they were supposed to use it. It was also supposed to change education in the earth sciences—modify it. What it did was, it caught the tide of earth systems science, which I mentioned to you before. We had had the revolution, in '67 to '68, with plate tectonics and then progressively we moved towards this earth systems science, which actually came in through NASA. In 1988, they had their first big report using this phrase, “earth systems science,” but it was concerned essentially with the oceans and the atmospheres and the skin of the Earth. The geologists refer to it as kind of a skin-disease approach to earth systems. [Laughter] We took it right down to the Earth's core. The processes involving the atmospheres and the oceans go right down inside the Earth and right to the core, where energy exchange occurs. So our major conclusion here—the number-one top priority—was that there should be a major effort to steer everything into the framework of earth systems science, “everything” being research programs and education. Out of this framework of earth systems science, we can find many different directions which could be used to help solve, or at least ameliorate, societal problems. We had a nice, neat separate little pamphlet with the executive report and chapter one, which is an overview chapter that was distributed widely. I know it was translated into Chinese, for example. I think it was used by research agencies in other countries and by our National Science Foundation. NASA and NOAA [National Oceanic and Atmospheric Administration]

already had their own earth-system skin-disease thing.

COHEN: Was that good for funding? Was it as successful as astronomy has been?

WYLLIE: No, not in the same way as astronomy, because we couldn't set up ten big facilities in a row, you see.

COHEN: You couldn't prioritize?

WYLLIE: We did set up a system of priorities, but it's not easily counted in terms of so many million dollars for this and so many million dollars for that. I think we did a very good thing here in setting up a framework, which tried to indicate that we don't have pure science and applied science, that there is a continuum between them. We all know there's a continuum between them, but nevertheless you tend to fund either pure or applied science and think in those terms. We set out a matrix in which we had one side of the matrix as scientific areas in a fairly conventional sense, from the Earth's surface to its center, except that we were dealing with earth systems science—instead of taking disciplines, we took processes. Then, along the top of the matrix, we had a series of objectives: A, understand the processes; B, sustain sufficient resources of water, minerals, and fuels; C, mitigate geological hazards, and those were earthquakes, volcanoes, and landslides; and D, minimize global and environmental change—assess, mitigate, and remediate. Within the matrix, you could come in through any one of these things and find a way right through from the applied to the pure or from the pure to the applied. We selected eight priority themes, each with a top priority research opportunity, and two high priority opportunities.

COHEN: It looks like that was a huge effort.

WYLLIE: It was, yes. That's why I didn't get my own textbook revised. [Laughter]

COHEN: Is the academy continuing to do these kinds of things, or was that a one-shot thing?

WYLLIE: This was an academy committee that was appointed. Its job was finished when the

report was out.

COHEN: Oh, so it's not like astronomy, which does this kind of thing every ten years.

WYLLIE: Presumably sometime there will be another committee set up to review the priorities. NSF has its own series of five-year plans—NSF and earth science—and we've had back and forth between them.

COHEN: I can see it took a big effort on your part, and a lot of time, I would think.

WYLLIE: Yes, yes. I'm proud of it. It may not have had much effect in this country, but I know it was influential in Scotland, as well as being partly translated into Chinese. I know a seismologist at ETH Zurich [Swiss Federal Institute of Technology] who told me that he went to the university president's office and the volume was sitting on the president's desk. I think it had influence outside of this country perhaps more than in this country. Although the recommendations that we made for education are now happening, I think they would have happened anyway. For a chairman or a dean to go to the president and say, "Look, this is recommended by the National Academy. We ought to have this kind of program."

COHEN: So you feel that there was certainly some value.

WYLLIE: Yes. I don't kid myself that this is what made these changes, but the changes that we recommended are happening. There's one other quote in the book's prologue that I'd like to mention. "This process-oriented, integrated global approach should be incorporated into revised earth science curricula in universities and schools. There are also educational opportunities in redefined engineering and geology." We picked up on that as one of the research things later on—that there are opportunities between geology and engineering that have not been utilized or even recognized. In the old system of engineering, there was one simple course on geologic engineering. I taught this course at Penn State without knowing any engineering—but I gave the students rocks, their foundations. Well, here at Caltech now we have a lot of collaboration and connection between environmental engineering and our division. So it's happening here.

COHEN: We'll have another session when you'll talk about the students and the medals. [**Tape ends**]

PETER J. WYLLIE

SESSION 4

April 9, 2002

Begin Tape 4, Side 1

COHEN: Good morning, Professor Wyllie. We were going to talk today about—

WYLLIE: The research. That's a tall order. I'm not averse to talking about the research, but it's not—

COHEN: Well, maybe you have talked about that enough. Perhaps you'll just talk about people and such.

WYLLIE: I made a list for another purpose, for the acknowledgment of the receipt of the Roebling Medal last year. I discovered that I have twenty-two PhD students who ended up as coauthors with me. Some of them didn't finish their degree, but twenty-two finished up as coauthors. Of those, eleven are in academic positions, five went into various kinds of government research positions, like the U.S. Geological Survey, and three are in industry—one is in oil, one is an environmental engineer.

COHEN: They've all stayed as geologists in one way or another.

WYLLIE: There was one who was a coauthor, Gil [Gilbert] W. Franz, who didn't— Oh, I guess he did stay in geology. I lost track of him in 1965. The last I heard was that he went off and was dealing with aggregates, building stones, and a business involving stones. So that's geology, yes. But I lost him. He disappeared completely. I think they all did some pretty useful research, but few of them became—what shall I say—became famous, in the sense that they were appointed to MIT or Caltech or a leading research university. They're all in good universities. Many of them have become department chairman and so on and have played significant roles in national committees and things like that. I'm looking at the list in front of me, thinking, "Gosh. Shall I talk about research that they've done?" There's just too much in 300 articles.

COHEN: Maybe you could talk about a few of them who particularly stand out in your memory.

WYLLIE: Well, Gus Koster van Groos, who came from Holland to Penn State and then followed me to Chicago—he was the one who discovered liquid immiscibility between silicate melts and carbonate-rich melts under high pressure and the effect of water. These experiments changed, or rather they consolidated, the way of thinking about the origin of some very, very peculiar carbonate-rich magmas or melts. So that was a start. We've picked that up again in the last fifteen years with fancier equipment and different pressures. He got people thinking along those lines to explain these peculiar rocks. New applications have been found in the last fifteen years, so we dove back in and consolidated the whole range of experiments.

COHEN: Now, where is this man?

WYLLIE: He became chairman at the University of Illinois, Chicago Circle campus.

COHEN: That's a major school, certainly.

WYLLIE: Yes. He is a very good experimentalist. He developed a beautiful lab there. Research, as you know, is a lot of hard work, involving some skill at putting things together, and then there's intuition, these flashes that some people have. The hard work involves making things, especially when you're in this business of experimental petrology. Gus Koster van Groos had what we refer to as a very good pair of hands. He could construct a difficult apparatus and make it work.

In those early days at Penn State, overlapping to Chicago, Dave [David H.] Watkinson, who was a professor at Carleton University in Ottawa for many years—I think he retired recently—did some very nice experiments which also changed the way we think about certain kinds of rocks. If I get into too much detail, this technical detail, just stop me.

COHEN: Right. But that statement alone makes him important. He changed the way we think.

WYLLIE: Well, it's not quite as high-flown as it sounds. There are two broad classes of igneous rocks: There are those that are rich in silica and those that are low in silica. The low-silica ones

are the basalts, the lavas that come up from Hawaii, the ones that come from the Earth's interior, and they are the first step in making a crust. The high-silica ones are the granites, which are recycled—progressive melting until finally you've got lots of silica. The basalts and the granites are the two most abundant kinds of igneous rocks. They are end members in a series of rocks with mainline variation from low silica to high silica. Within that mainline variation, you have lateral variations with relatively higher and lower alkalis.

COHEN: So this man worked on the interfaces.

WYLLIE: No, no. I'm providing background, the petrological background, because what he did doesn't make sense unless that's there. Now, along the relatively high alkali side of this main trend, there's a whole series of wonderful rocks called alkalic rocks, which have great names and some interesting minerals. How these are formed had been debated for years. In 1910, a man named Reginald Daly, from Harvard, set up what was called the limestone-assimilation hypothesis. When I was an undergraduate, in 1948 to 1952, this was still debated; this was a big seminar topic. Daly maintained that the mainline rocks from basalt to granite in certain environments assimilated limestone—they dissolved the limestone. This removed silica from the magma, allowing the alkalis to make different minerals, and this is how all of these fascinating alkalic rocks were made. That was, as I say, a seminar topic for fifty-five years, and it was still being strongly advocated in the 1950s. What Dave Watkinson did was produce some experiments designed to find out the effect of dissolving calcite in these silicate melts. It wasn't done before 1950, because the apparatus wasn't available. The results showed that the limestone-assimilation hypothesis couldn't work; you had to have some other mechanism. He did this research in the early 1960s, and I wrote a review in 1974 that involved that and several other experimental studies with carbon dioxide, which I think effectively killed the limestone-assimilation hypothesis.

COHEN: That sounds quite important.

WYLLIE: It wasn't a great new idea, but it demonstrated that Daly's idea simply wouldn't work. That was related to another whole series of experiments we did which involved carbon dioxide. I think I told you that at my first International Geological Congress, in Copenhagen, I had two

papers. One was called “Melting in the Earth’s Crust,” which at that time was novel because it involved the new experiments made possible by Frank Tuttle’s new high-pressure apparatus, and that was one line I followed all my life [(with O. F. Tuttle) *Proc. XXI Int. Geol. Congress*, Part 18, 227-35 (1960)]. The other one was the discovery that these peculiar carbonate-rich rocks called carbonatite could be magmatic [Wyllie, P. J., & Tuttle, O. F., “Experimental verification for the magmatic origin of carbonatites,” *Proc. XXI Int. Geol. Congress*, Part 13, 310-18 (1960)]. Again, that is a long story. In the late 1880s, some Norwegians discovered what looked like limestone; but they had an igneous, volcanic kind of attitude in Norway and Sweden, and they said that these rocks were magmatic.

COHEN: What’s the word?

WYLLIE: “Magmatic,” from magma. “Magma” is the name for the melt that comes out of the lava inside the Earth.

COHEN: Does that have anything to do with magnetism?

WYLLIE: No. Editors often get that mixed up. Once in a while, you find a paper that refers to magnets in certain magmas. “Magma” is one of those lousy words that geologists invented to sound scientific in the 1800s, I guess.

COHEN: OK.

WYLLIE: So they claimed that these limestones were igneous, and that they were placed in the crust as magmas and flowed out as lavas. Everybody said, “Impossible,” because calcite melts at 1300°C, and these things came in at 700 or 800°C. A very famous petrologist, Norman Bowen, looked at these in 1920 and said, “No. The heat relationships are wrong. The stuff melts at 1300°C. This could never be magmatic.” Then the limestone-assimilation hypothesis ran parallel to this, in the sense that it claimed that some of these carbonate-rich rocks were in fact melted limestones incorporated into magmas, and they separated off, and that’s what made them molten. Again, all the theoreticians and experimentalists said, “Impossible.” After the Second World War, little limestone hills were being discovered in Africa that were enriched in valuable

trace elements like niobium and radioactive elements. That was a big search. They became very important ore deposits. There was obviously something different about them. Papers from 1950 said that these surely must be of igneous origin—but again, all the experimental data indicated they couldn't be magmatic. So there were discussions about whether these were great blocks of limestone that were incorporated into big magma chambers and contaminated by elements from the magmas and so on. There was a big push to prospect for these carbonatites and a resurgence of interest in how the hell are they formed. There were several different hypotheses, which I won't bore you with, but I'm afraid that must be precisely what I'm doing. [Laughter]

COHEN: That's OK.

WYLLIE: One of the things Frank Tuttle, my sponsor at Penn State, said when I got there—this is written up in one of my first award acceptances—he said, “Pete, when you get a chance, see if you can melt calcite.” So I tried it with our new high-pressure apparatus with some water in a sealed gold capsule, and I found I could partially melt calcite at a moderate temperature, which led to a series of experiments demonstrating the conditions under which you can have essentially a carbonate-rich melt at moderate temperatures with other ingredients dissolved in it, but it's going to precipitate calcite, which will sink and make the rock. It demonstrated that magmatic carbonatites were possible. That was my other paper at Copenhagen in 1960, and that set me off on another line of experiments, which related to this Gus Koster van Groos's liquid immiscibility. You can take the silicate melt, the carbonate melt, and through a range of compositions they are immiscible. So a silicate melt with some dissolved limestone, for example, when it cools will split off the immiscible carbonate liquid, and there's a separate magma.

COHEN: So these ideas were really very mainstream problems and ideas.

WYLLIE: But the carbonatites are very rare rocks. As I said at one stage, all of my experiments have been focused on or inspired by a geological problem. There are two ways of doing these experiments. I hope I don't sound too racist. [Laughter] Germans have a way of doing things, and a lot of Germans, once this experimental work got going, they said, “OK, I'm going to do this system.” You'd set up a triangle or a quadrangle, you'd put points all the way through, and

you'd run everything, and then you would have a complete phase diagram. It's very thorough, very great, but you can do an awful lot of runs that don't get you anywhere. It's not just Germans. [Laughter] I feel terrible saying this. When I was growing up, we were told that the Germans were very thorough. During the war, of course, they were very thorough.

COHEN: They proved that over and over again.

WYLLIE: In science in school, we got the same kind of message: You have to learn how to read German, because they did the detailed, thorough experiments. Then, as I say, they got going on experimental work. I guess I must have learned from Tuttle—although he did a lot of very comprehensive stuff, too—instead of saying, “I am going to study this system,” my approach was to say, “Here's a geological problem. Where within this vast system can we select the right compositions to get a handle on what's going on?” That was the approach I used.

COHEN: That's a different approach.

WYLLIE: Yes. They were all focused on a geological problem. So although these silly experiments involving melting calcite were very limited in scope, they were focused on— It wasn't exactly a mainline geological problem; not many people bothered about carbonatites. I mean, they are very rare rocks, but they became significant because they had these concentrations of ore deposits and rare-earth elements, like niobium and thorium. There seemed to be a successive wave of things that they were valuable for. We did prove the existence of carbonatite magmas after about seventy years of denial and debate. Gus Koster van Groos demonstrated in a rigorous way that liquid immiscibility existed between some silicates and some carbonates. Dave Watkinson did some experiments. He followed through a complex system and showed that limestone assimilation wouldn't work, so the alkalic rocks had to be generated another way. They all added pieces onto this general problem.

Chris [Christopher M.] Scarfe was a student who went to Alberta and set up a big experimental lab at the University of Alberta. He unfortunately died—hit by a car while jogging, which seemed such a waste—I guess twenty years after he graduated. John Robertson did some very nice experiments on the crustal stuff. I started off in granites and granites with different volatiles, like fluorine and chlorine and so on. He moved into what we call the vapor-absent

region. All these new experiments had been done with an excess of water. You pumped water in under high pressures, superheated, which imparted the pressure and made sure there was enough water to make things happen. That's true in some geological environments, but in most geological environments there isn't enough water for that kind of condition. You have water dissolved in a silicate melt with no free vapor. So John developed the first set of water-undersaturated experiments—using water, but without the excess. Then John fulfilled his ROTC commitment to the army, and taught at [the US Military Academy at] West Point for years and years. He wasn't teaching experimental petrology, but he had a high-pressure lab. I don't know what he was doing. It was a secret. He was doing high-pressure work in the army. He was an officer. He retired early and went to Texas. He now plays golf all the time.

Russ [Russell B.] Merrill did some very nice experiments. I won't go into those. By 1970 in Chicago we had moved into another set of problems. Russ went off and did research administration. He was working for the Deep Sea Drilling Project for some time. I don't quite know what he did, but he was an editor and administrator, things like that.

Wuu-Liang Huang was interesting. I had two students from Taiwan. He was the first, in 1971 or '72. He got his PhD and he stayed on [at Chicago] for a couple of years as a postdoc. He did a lot of elegant experiments. His thesis was on crustal rocks, the granitic kind. Then, in the postdoc period, I put him onto carbon dioxide on a mantle system, the interior-of-the-Earth system, to find out the effect of carbon dioxide on melting below the crust. Between us we laid out the whole framework for the system $\text{CaO-MgO-SiO}_2\text{-CO}_2$. If one considers iron as proxy for the magnesium, this system amounts to ninety-five percent of the Earth's mantle. So it's a relatively simple model system: You add carbon dioxide. We persuaded ourselves that we could understand the effect of carbon dioxide on the mantle rocks and magma. We got lots of results published, but we were in a rush to get the applications out, so we published papers with a theoretical framework and applications without all of the definitive experiments. Then he went back to Taiwan and I moved on to other things, and we never got the rest of the experiments written up. These experiments got me started on the origin of kimberlites, more rare rocks that bring diamonds to the surface from depths of at least 150 km. It didn't matter too much, because those published results and the theoretical framework were sufficient for petrologists to accept our applications to the geological problems. Twenty years later, Woh-jeer Lee, my second student from Taiwan, joined me here at Caltech. I only had two PhD students at Caltech all the

time I was here. Woh-ger did his thesis on liquid immiscibility, which Gus Koster van Groos started in '65. He picked it up again in '92 or '93 and did these very, very complex phase equilibrium experiments, taking the study a lot further and with many more geological applications. Then, when he finished—he was a postdoc for a while—I said, “How about picking up all those experimental tables of Wu-Liang Huang and getting them published?” We had all the tables of experimental data from his fellow countryman from twenty years before, and Woh-ger also completed many additional experiments at a different pressure. He drew the phase diagrams from old and new data and we had a joint paper with Wu-Liang Huang from 1975, Woh-ger Lee from 1995 or '96, and we published two comprehensive papers in the year 2000. [Lee W. J., Huang W. L., & Wyllie, P. J., “Melts in the mantle modeled in the system CaO-MgO-SiO₂-CO₂ at 2.7 GPa.,” *Contrib. Mineral. Petrol.*, 138:199-213; Lee, W. J., & Wyllie, P. J., “The system CaO-MgO-SiO₂-CO₂ at 1 GPa, metasomatic wehrlites, and primary carbonatite magmas,” *Contrib. Mineral. Petrol.*, 138, 214-28.]

COHEN: What's interesting is that nobody somewhere else picked it up.

WYLLIE: There was somebody in between in Manchester and in Canada who did some more experiments on liquid immiscibility, but they went off the track a bit. One reason for coming back to the problem again with Woh-ger Lee was to see if in fact they had gotten things wrong. I said, “Well, we had better take a look at this, because it doesn't look right.” So he did, and in fact they were wrong, so he put it right again. The fact that the runs done in the mantle system between 1970 and 1975 were still not superseded by 1995 gave all three of us a lot of satisfaction.

COHEN: Why do you think this was so? Just because it was a very difficult experiment?

WYLLIE: Well, we were in competition at the time, in 1975, with people at the Geophysical Laboratory [of the Carnegie Institution of Washington] who were also working out the effect of carbon dioxide on the mantle, with a different experimental approach. That's why we were in a hurry to get our results and applications out, without going through the tedious exercise of tabulating all the experiments properly. So between us, we had laid out the field. We had gotten the results. I'm probably wrong here, but I can't think of anybody else doing experiments on

that system again, until the early 1990s when somebody at Bristol in England started doing some. Somebody in Texas did, but that was with a new generation of apparatus that could go to much higher pressures than our domain. So they started picking it up at much higher pressures, which was another reason for us to return to consolidate results in CaO-MgO-SiO₂-CO₂ for 50 to 100 km. depth in the Earth. Oh, the people in Bristol did some experiments—an extension of what we did—but again, I didn't like their results. I thought there was something wrong. Woh-ger checked them out, and indeed, they did have errors.

COHEN: Did you finish up this work here at Caltech?

WYLLIE: Yes, yes. My last four research papers in the year 2000—two of them were on these experiments with Woh-ger Lee. What Wu-Liang Huang did—this is interesting—he went back to Taiwan in the seventies. He was an associate professor, then a full professor there. He built up a high-pressure lab. Then he went on a fellowship for a year [1979-1980] to the Lunar and Planetary Institute in Houston, where they do lunar science. He got a job with Exxon in their Exxon research laboratory in Houston and stayed. He has a son, Bert, a fine Taiwanese name! Bert went to Harvard. I think he's still at Harvard and hasn't quite finished. Again, I don't know exactly what Wu-Liang did, because it was a secret, but it was experimental work involving the porosity of rocks and minerals. He did tell me, "One-percent difference in porosity is worth millions"—or was it billions?—"of dollars as to whether or not the oil will percolate through."

COHEN: So he went into commercial work. He probably had a wonderful lab.

WYLLIE: I never saw it. It's a basic research lab with Exxon at their research headquarters. I think he was director of the lab, but again, I'm not quite sure of that. [He was a member of the senior research staff—ed.] Then he took early retirement and went back to Taiwan to be a professor at the university there again, because they were trying to get people to go back to Taiwan. They don't have enough scientists to fill their departments. He's still there now. He's been back for, I guess, two or three years. I don't know whether he's going to stay there. His son is at Harvard and about to finish. His wife has a very good position at a university now in Taiwan, but I think she didn't really want to go back to Taiwan. So those are my Taiwanese friends.

Chuck [Charles R.] Stern from Chicago, he's at the University of Colorado. I think he was chairman for a while. Alan Byrnes went off into industry and then to the Kansas Geological Survey. You don't want this kind of detail, do you?

COHEN: Just any that were particularly outstanding.

WYLLIE: The recent ones are all assistant professors or associate professors.

COHEN: It sounds like they've done very well.

WYLLIE: You did mention postdocs, and most of these went on to very good academic or research careers. I should mention one of the postdocs, Art Boettcher. He changed his name to Montana. Boettcher was a German name that he never liked. When his father died, he asked his mother, "Do you mind if I change my name?" She said, "I don't care. It's not my name." So he changed it to Montana, which is where he grew up. He did his thesis on rocks in Montana, too. He was a field geologist PhD at Penn State. I moved to Chicago. He asked to come as a postdoc to do experimental work, so he did, and he did very well. He got the new high-pressure lab going. He did a tremendous amount of work. I had more papers published with him than anybody else. Then he went back to Penn State as a professor. Then he moved out to UCLA as a professor before we came to Caltech, and he became chairman there. So I pick him out because he's one of my associates who went to the best academic position at a first-rank research university. UCLA is better than all of the others, and he became chairman in geology. He retired early from academia, moved to New Mexico, and was appointed mining commissioner by the governor. He has since consulted on dam removal and on nuclear waste disposal in Japan. I've got a lot of other successful postdocs here, but this one, Sven Maaløe, went back to Norway and got a distinguished chair early on at the University of Bergen.

COHEN: They've done very well.

WYLLIE: Oh, they've all done very well, yes. I'm very proud of them all. I'm proud of the work they have done.

COHEN: So tell me about the Roebling Medal that you received in 2001.

WYLLIE: The Roebling Medal is the top medal of the Mineralogical Society of America for distinction—

COHEN: A lifetime of work.

WYLLIE: Yes. It's the old-guy medal. It should be old females, too, but I don't think they have had many females—maybe one. [Helen D. Megaw in 1989; since this interview was conducted, there was a second, Alexandra Navrotsky in 2009—ed.] It's the “mature” medal, let's put it that way. I think it's for originality in research, that kind of thing.

COHEN: Well, but that's very good.

WYLLIE: I was delighted to get it, yes. It's my field, and the society is my society.

COHEN: Where was this medal given to you?

WYLLIE: That was in Boston. The Geological Society of America has its annual meeting. The Mineralogical Society meets with them, and that's the annual award function.

COHEN: You received another medal in 2001.

WYLLIE: Yes, that was a nice surprise. That was the German Geological Society, the Leopold von Buch Medal, which is for scientific research, but it has to have an element of service to the science in some way.

COHEN: So it's a dual medal in some sense, for both research and citizenship.

WYLLIE: Yes. It is for scientific citizenship, and it's normally given to a non-German. They can, once in a while, tap a German, but it's their overseas medal.

COHEN: Now I'd like to come back to another subject—

WYLLIE: Before that, you asked me originally about the ones I'm most proud of. I think it would have to be the Wollaston Medal, the top award from the Geological Society of London, which in its announcement says it's the oldest geological society in the world. They gave me that in 1982.

COHEN: Would that have been just before or after you came here?

WYLLIE: Just before. I came in '83. That one is better, because it's geology, which embraces mineralogy and many other subjects, too. So I was very astonished and pleased and delighted to get that one. I would also like to mention the Abraham-Gottlob-Werner Medaille, given to me in 1987 by the German Mineralogical Society.

COHEN: OK. Now I'm going to come back to something, and that is your feelings about the division here.

WYLLIE: It's going in incredibly exciting directions. It's been very thrilling to see the way it's been changing in the last four or five years.

COHEN: In what way?

WYLLIE: All the old guys are retiring. We have on our notice board a picture gallery of the faculty. There must be twenty-nine or thirty paid faculty. We already have thirteen emeritus up there. Some of them are so emeritus that they're not here very much, but most of them are recent retirees. I think this year we have five more people retiring. Ed Stolper told me there were five retiring, and when he told me who they were, I said, "Gosh, there goes the division." The old division

COHEN: The old guard.

WYLLIE: Apart from the four or five who've retired in the last two or three years, one third of the old guard are disappearing within a few years. Ed Stolper has galvanized some rather sluggish faculty committees into search committees, and we are progressively replacing them. I can't

count how many new ones we've had in the last three years, but there must be at least eight, nine, maybe even ten. I still go to the faculty meetings—we have them once a month—but I speak only when spoken to. [Laughter] Well, that's because until seven or eight years ago emeritus faculty were invited to all meetings. Once a month at lunchtime we have a regular meeting, and then we have special meetings in the evening when we discuss potential appointments, promotions, five-year plans, and that kind of thing. There were a couple of emeritus faculty members who were always noisy and insistent when it came to discussing new appointments. They knew what kind of appointment we wanted; they knew who we wanted, and so on. They were really becoming a nuisance. I'm not going to mention any names.

COHEN: OK. [Laughter]

WYLLIE: They were too pushy. I think everybody else agreed that the future makeup of the division should not be dominated by retired professors. [Laughter] We moved into a position where it was agreed that emeritus faculty would no longer be invited to appointment discussions, except if there was special experience and knowledge required. So that changed. We are still invited to the regular, routine faculty meetings, and some go and some don't. It's fascinating to sit there and see eight or nine of the young new people who don't know the history of the division. What was very impressive when I came in '83, and it lasted all the way through, was that whenever anything came up for discussion, somebody would say, "Oh, we did that ten years ago. It didn't work then, it won't work now," or, "We have always done this, so we've got to continue doing this." The traditions are very, very strong. Everybody knew them. As one or two new faculty at a time were introduced to the division, they were instructed, nurtured, they accepted these traditions, and they just carried on. Now, however, we've got a whole bunch of new folk who don't know these traditions arriving almost simultaneously.

COHEN: Now, why are these people asked? Are they replacing the people who retired?

WYLLIE: They're not in the same slot. We do not consider slots. We do not work in slots.

COHEN: You never have?

WYLLIE: No. Well, not since I've been here anyway. A search has to be directed, as you know. You have to publish the description of the search and you mention the field, but most often it's understood that you're really searching for the best person. Very commonly the person we select doesn't fit the job description. We've had a series of five-year plans that started maybe a dozen years ago. When the forthcoming retirements were really looming large, we decided we had to look ahead and not wait until they came. So Chairman Ed Stolper had a five-year plan set up to identify the fields that we thought were going to be significant in the future. Our searches are done along those lines, and have been modified two or three times.

COHEN: So this is in broad areas.

WYLLIE: Usually in broad areas, yes.

COHEN: So supposedly the person who came did not replace anybody who was leaving but came in with his or her own stuff.

WYLLIE: The person is coming because the division has decided, "This is where we need to go in the future." There are no empires that have any continuation after the emperor retires.

COHEN: So the assistant professor does not come to work with a professor?

WYLLIE: Oh, no, never.

COHEN: OK. That may be true of all of Caltech, but I don't know that.

WYLLIE: I had assumed it would be true of all Caltech, but it may not be. Postdocs come to work with professors, but assistant professors come and they're given their head immediately. They are set up, and Caltech supports them very well. I was amazed when I came here at how much money was available for the division—the division chairman's discretionary money and the money we put into a new person—compared with Chicago, which was, I thought, pretty generous. There is just much more flexibility here, many more facilities available, much more financial support, and in our division—I don't know how it is in the others—spiritual support. I

think the division's rather proud of the fact that very few of its appointed assistant professors do not get tenure. We choose them very carefully and we try to nourish them. It used to be said that we tried to instill in them the spirit of the division. Whereas now the new ones, I think, are bringing in their own spirit. [Laughter] They're changing the spirit. I can think of only one of our assistant professors who didn't get tenure since I came. I think of the batch we have now, and who knows? Their style, everything about them, seems to suggest that they're with us.

COHEN: That's really quite an accolade to the department.

WYLLIE: This department has had this kind of "we're-the-best-of-the-bunch-and-we're-going-to-stay-that-way" spirit for long before I came.

COHEN: Now, it wasn't completely smooth when the seismologists had to move over there.

WYLLIE: That was a long time ago, long before my time. When I came in '83, they were thoroughly integrated, but there was a kind of separateness to them. Their budget was different.

COHEN: But they were over here already.

WYLLIE: They were over here in the new building [Seeley G. Mudd Building of Geophysics and Planetary Science, known as South Mudd—ed.].

COHEN: Although they still maintain that station over there [Kresge Building, Seismological Laboratory].

WYLLIE: But I don't think anybody resides there—certainly no faculty member. They moved over to the big building, I think it was in '75, and I came in '83.

COHEN: So you were never a part of that integration.

WYLLIE: No. When I came here, there were four options. Geophysics was one, which was essentially seismology, and the others were geochemistry, geology, and planetary science.

Geophysics was somewhat different from the others in the sense that they were also members of the Seismological Laboratory and they retained the identity of the laboratory, which was different from the academic option. There was some administrative friction. I was told in my first year that I really should take a look at the Seismology Laboratory budget, because they had their own budget, which was separate from that of the division. I asked politely to find out about it. I don't remember the details, but I had my wrist very thoroughly slapped.

COHEN: You mean, you weren't supposed to look at the budget?

WYLLIE: Right. "That has nothing to do with you! That has nothing to do with you!" They had their own budget that they carried over. As I say, I don't remember the details now, but I know that they retained a separateness that has gradually diminished, certainly in academic things. So they tended to be separate with their courses. The undergraduate structure tended to be idiosyncratic. Everybody taught his own thing, and the Seismology Lab in particular tended to resist integration. Well, now they're totally integrated in the undergraduate and the graduate curricula.

COHEN: I see, so all those differences have blended together in some way.

WYLLIE: Yes, yes. When I arrived, by '83, there wasn't any obvious friction. There was a separateness, but not friction.

COHEN: When you speak about the division, it's really very encouraging. It's refreshing and sort of wonderful.

WYLLIE: Oh, I think the appointments have been very, very good. Things are changing partly because of the subject areas. The discipline areas have shifted compared with what they were for twenty years, and we've got this collaboration and joint appointments with environmental engineering, which is good. We're dealing with global change, and because we are dealing with global change, we have many more people in atmospheres and oceans than we did before. Previously, our research on atmospheres dealt mainly with the other planets, so it was in planetary sciences. A few people within the planetary science department started turning their

attention to the terrestrial atmosphere, but not in a big way. Now we have quite a strong group in global change collaborating with environmental engineering, and they're working on a joint curriculum.

COHEN: How about the planetary people?

WYLLIE: That's one of the recent problem areas. I haven't been involved with the committee's reports since '99, but I know there has been a whole series of search committees to find new people to replace the older ones who are retiring or are about to retire from planetary sciences, and they haven't been very successful. They haven't found what they wanted. It still needs attention. I think our last visiting committee said that it needs attention. I'm sure there is still an active planetary-science search committee. We have made some very good young appointments, but I don't think we have enough new appointments dealing with the planets. That's what we need. They haven't been very successful in developing new links out to the planet programs at JPL [Jet Propulsion Laboratory]. I think that that's true.

COHEN: Well, it's interesting, because for a long time that was the focus.

WYLLIE: That was one of the great things that Bob Sharp did. He developed this new option, this new area in planetary sciences that linked up with JPL. That was a strength through the decades, through all the planetary programs.

COHEN: But now you feel the environmental stuff is taking precedence.

WYLLIE: The environmental program is growing and flourishing, and the other planetary stuff is fading to some extent. That doesn't mean it's going to fade away, because there are committees working on new appointments. After all, Arden Albee has his huge Mars program, which is still going. At last, lots of his flights to Mars are coming home with data. He's probably got more data coming in now than he can cope with. We have Don [Donald S.] Burnett, who is collecting planetary particles in the outer atmosphere. He's got a huge program. I think they're collecting particles on satellites, which are going to be dropped in the desert somewhere and brought back for detailed chemical analysis. So it's the whole study of the physical particles—not radiation

stuff, but physical particles coming in towards the Earth. Andy [Andrew P.] Ingersoll still has a large planetary program dealing with atmospheres in various places. Yuk [Ling] Yung, although a lot of his attention now is on the Earth, he still has research going on with the planets. We have connections with astronomy through Mike [Michael E.] Brown, Geoff Blake, and Shri Kulkarni. So we do have quite a few programs going on, but the future isn't as clearly defined as it was in past decades.

COHEN: Your sense of excitement is coming from this other area.

WYLLIE: Yes. I'm fascinated by the progress in global change and the links with environmental engineering. But I must emphasize that we have so many superb faculty that there is excitement everywhere. Our new generation of petrologists, geophysicists, and isotope geochemists is awe-inspiring.

COHEN: I usually ask if it's been wonderful being at Caltech. I assume that you're going to tell me it's been wonderful being at Caltech.

WYLLIE: Yes. It's been a very stimulating place to be. I'd hate to go back to a state university after this. [Laughter]

COHEN: How do you find this new administration these last few years? Certainly the direction is biology now.

WYLLIE: Since I ceased being chairman, the administration hasn't bothered me at all. [Laughter] You're talking about directions of the institute and that sort of thing.

COHEN: Right.

WYLLIE: I've left that to the people who are engaged in it. I'm happy and satisfied that they're going to make good decisions. They have been giving us the support in our division that we need. It's quite clear that biology is the science of the century, so I certainly think it's a great idea for us to do all we can in that area. Our division is actively engaged with biology, with

people in the Biology Division, to develop and enhance our program in geobiology. Some of our new appointments—

COHEN: Are in geobiology?

WYLLIE: Yes. Joe Kirschvink has been a pioneer in geobiology for decades, and now we have a young lady, Dianne Newman, who came in from Harvard Medical School with a huge team of people. They took over a large part of the sub-basement in North Mudd [Seeley W. Mudd Laboratory of the Geological Sciences]. It was made over for her. I think she came as an assistant professor with about eight people, which is a large group for a beginning geologist.

COHEN: So she's taking over the trappings of biology in a big lab. [Laughter]

WYLLIE: Yes, she's doing microbiology, geobiology. Dianne Newman has adopted the geological processes involving microbes in the biosphere, which is just one of our several geospheres.

COHEN: That sounds very exciting.

WYLLIE: She's been here for a couple of years. She's given some great talks.

COHEN: I was wondering about women professors. I know some years ago geology was criticized in that a large percentage of the graduate students weren't women. What happened in between?

WYLLIE: In earth sciences in general, as well as at Caltech, the proportion of females in graduate schools is going up fast.

COHEN: But not the faculty.

WYLLIE: Not the faculty. The faculty is much slower.

COHEN: Was that anything to grapple with, then?

WYLLIE: Oh, yes, constantly. In '86, I think it was, we were working very, very hard. I told you that we had a lid on appointments. The division faculty voted to appoint one of our own female graduates, but we didn't make it through the process. I think that this was a case of one of our influential faculty members going in through a back door to Murph [Marvin L. (Murph) Goldberger, Caltech president 1978-1987] and saying, "This is not the person to appoint just because she is female." That was the rumor we heard anyway. I think it may be true that, as good as she was, her record would not have been considered good enough for a male. About a dozen years ago, we had Ewine van Dishoeck from Holland for a while, an assistant professor who was a cosmochemist in our planetary science effort. Then she went back to Holland, so we lost her. I guess Joann Stock was our first female appointment after her. Ed Stolper exhorts all search committees and faculty to seek out excellent female researchers, and now we have Dianne Newman.

COHEN: It's interesting that you set up a big lab for her, but that's how it's done these days. OK. Is there anything else you want to say, Peter?

WYLLIE: Well, yes, I think I ought to include some comments on the contributions to Caltech by my wife, Romy, and this seems a good place to do so. David Baltimore [Caltech president 1997-2006] wrote me a letter congratulating me on my retirement and thanking me for my contributions, etc., and in my reply I wrote that whatever my research contributions may have done for Caltech's reputation, they would become insignificant compared with the historical value of Romy's book, *Caltech's Architectural Heritage: From Spanish Tile to Modern Stone* [Balcony Press, 2000].

I said earlier that when we moved to Caltech, Romy decided to keep busy as a chairman's wife and not to rebuild her Chicago interior-design company. She did continue with small commissions, eventually becoming interior designer for the Athenaeum in the mid-nineties. Romy was president of the Caltech Women's Club in 1985, and that year's board decided that the club should develop a docent group to give architectural tours of the campus. The founding members of CATS [Caltech Architectural Tours] were Romy, Missy Jennings, and Paula Samazan, joined by several others, including Roxana Anson, Bunny Gould, Linda Kamb, and Micheline Vogt. So Romy worked with the wives of four of my colleagues on the IAC, doing

research on the early campus architecture. Romy organized the research material and gathered it into a tour script; additional and replacement docents joined the group, and now there are about twenty docents. In 1988 Romy added a forty-five-minute illustrated lecture comparing Caltech architecture with historical precedents from around the world. Tours are given monthly for the general public, for any organized group that requests a special tour, for alumni on Alumni Weekend, and for trustees and other friends of Caltech, including potential donors through the Development and Alumni Relations and the Public Service Departments. By the end of 2002, CATS had taken about 8,000 people on tours of Caltech.

Before he retired as president, Tom Everhart asked Romy if she would write a book on Caltech's architecture, and she did. The result, *Caltech's Architectural Heritage: From Spanish Tile to Modern Stone*, is a handsome coffee-table book, thanks to Tom Everhart's financial support for architectural and modern photographs, and was also thoroughly researched, thanks to the efforts of CATS members and Romy's writing. My favorite review comment described it as "a work of immaculate scholarship that is highly readable." After publication of the book in 2000, and a generous review in the *Los Angeles Times*, the number of people attending tours tripled. Romy was made an honorary Caltech alumna in 1996. That makes us a complete Caltech family. **[Tape ends]**