Brent Dalrymple Oral History Interview, August 28, 2013

Title
“A Distinguished Career in Geology”

Date
August 28, 2013

Location
Dalrymple residence, Corvallis, Oregon.

Summary
In the interview, Dalrymple describes his youth, his experiences at Occidental College - including meeting his wife, Sharon - and his graduate work at UC Berkeley. He then details his work at the United States Geological Survey, with specific focus paid to his research on the earth's magnetic field, investigations which contributed to the formulation of the theory of plate tectonics. Other reseach areas discussed include Dalrymple's studies of volcanic rock in the Hawaiian Islands and other Pacific islands, and lunar magnetic experiments for NASA. From there Dalrymple reviews his teaching experiences at Stanford, his tenure as dean of Oceanography at OSU, and his later research interests, including his testimony as an expert witness in two trials involving creationism. He rounds out the interview by sharing memories of OSU notables such as John Byrne, Jane Lubchenco and Paul Risser as well as his thoughts on receiving the National Medal of Science and retiring from OSU.

Interviewee
G. Brent Dalrymple

Interviewer
Chris Petersen

Website
http://scarc.library.oregonstate.edu/oh150/dalrymple/

Transcript

Chris Petersen: Okay. So, if we could begin by having you introduce yourself, give us your name, and today's date.

Brent Dalrymple: Name is Brent Dalrymple. It is the 28th of August, 2013.

CP: Okay. So, you were born in 1937?

BD: Correct.

CP: Where were you born?

BD: In Alhambra, California.

CP: Is that where you grew up?

BD: No, I grew up in Bell, which is not too far away.

CP: And what was that like? What was the environment in Bell?

BD: Bell was essentially a blue-collar, all-white town, but essentially a blue-collar town. It was fine. I had friends, and had a good time there. But to give you some idea, I graduated in a February class, which the Los Angeles school district had at that time, had two graduating classes. And my class of 63, only 7 of us had taken enough courses to go to college. Everybody else took shop. So I think that explains more about the environment than anything else I could say.

CP: So what brought your family to Bell?

BD: Well my father was a contractor; he had a business there. He was a brick contractor.

CP: Mm-hm. And your mother was a homemaker?

BD: Yes, yeah. How we ended up in Bell, I have no idea, but the business was already established there, so I spent all of my youth there—most of it, anyway—until I went to college.

CP: Did you have any siblings?

BD: [Laughs] At the time, no, at least I didn't know about them. It turns out that I did have two half-sisters that I just learned about, oh, four years ago, four or five years ago.

CP: Oh, really?

BD: And one of them died before I met her, the other—the other one, she and I have been in pretty close contact since.

CP: Wow! That must have been quite a surprise.

BD: Well, there had been—I had heard that there was a—we share a father, and I had heard that there was a half-sister somewhere. I didn't know there were two, but I didn't know her name, or where they lived, or anything else. But she knew my name through an aunt, and about four—2009, so it had been about four years ago, she Googled me. I'm fairly easy to find on Google, so. And we got in touch, and have been siblings ever since. So it was kind of exciting.

CP: Well, what were your hobbies as a child?

BD: Oh well, the usual. Baseball, football, chemistry set, and in high school I used to do some diving, some skin diving, and going to the beach and that sort of thing. Nothing particularly unusual.

CP: You mentioned a chemistry set. I'm interested in your earliest scientific interests.

BD: Oh, I liked to—I had an erector set, and I liked to build things. And a chemistry set, you know, you follow the instructions. You got something either smelly or colorful, or change from a liquid to a solid, and that was always kind of fun.

CP: Was there anybody who introduced you to this sort of thing, or did you discover it on your own?

BD: Oh, I think I more or less discovered it on my own. Most of my mother's family were in the meat market business, and my father was a contractor, had a couple of years of college, including some engineering. But mostly it was just an interest that grabbed me, you know, when I first got introduced to school.

CP: Okay, so this was in grade school?

BD: Probably more in high school. Well, in grade school, I was interested in these things too, and I don't—I can't think of any teacher who got me on to it.

CP: Not in grade school? But was there somebody in high school who was important?

BD: I did have some teachers in high school that were—kind of nudged me toward science. There was a math teacher and a physics teacher that were fairly influential, just because I liked what they were teaching.

CP: So you found that you had a natural affinity for it; you excelled?

BD: Well, I don't know that I excelled at it, but I liked it. [Laughs] That was enough to keep going. And I guess photography was another hobby I had. I used to take a lot of pictures with a big graphics—you know, 4-by-5 graphics camera using infrared film, and developed them myself. I liked that.

CP: So infrared film, was this scientific photography, or?

BD: No, no, you just get different colors. The things that are giving off infrared wavelengths, like trees and growing plants and things, all turn out, give you lots of exposure. So if you'd shoot a landscape, the trees would be white, and the shadows would be different—just a interesting artistic effect.

CP: [0:05:00] How would you characterize high school? You said it was a blue-collar town, blue-collar school.

BD: Well, yeah, it was essentially a blue-collar school where most kids were not interested in academics. The kids that were interested in academics got a rough time sometimes, unless you were a good athlete, and I was a fairly good athlete. I played basketball and tennis, and lettered in both of those sports. So that got you off the hook. You could also be a good student if you were a good athlete. If you weren't, you had some trouble, but. The high school at that time was fenced in; you couldn't leave during the day time. That'd give you another—they locked the gates [laughs] so people wouldn't wander off.

CP: Did you have other extracurricular pursuits besides the sports?

BD: Well, hunting. My father, my dad was a hunter, and so I, starting at the age of about nine or ten used to do a lot of hunting, mostly birds, ducks and geese, but also deer.

CP: Did you work?

BD: I did. I worked—I guess my first job was at J. J. Newberry in—which was a Crescent's-type store, you know, five and ten cent store—I don't know if they are still in business—as a stock boy, and worked there a couple of hours after school, and on the weekends. That was the first job I can—I think I had. And then later on, I worked for my dad and for other brick contractors. I learned to lay brick fairly young, and I had my journeyman's card by the time I was—I was sixteen, I think. And so I would work for him, or other contractors if he didn't have enough work to keep me busy at the time. I never had any trouble getting jobs during the summer.

CP: That must have been an interesting environment for a young man.

BD: It was good. Yeah, I really enjoyed it. You know, construction is something where you can spend eight hours doing it, and at the end of the day you can see what you accomplished. In my case, it was, you know, either a face brick wall, or—all he did was commercial buildings—so, you know, you could look back and see that the wall—of course we're always working with four, five, six, eight, ten other people. And you could see the wall had gone up four feet during the day, and that was very satisfying. I could have done that as a career, in fact, and not been unhappy, except it's the kind of job that you can't do when you're 65 or 70. I don't remember seeing but one old bricklayer; they just don't last that long! [Laughs] I think that's probably true a lot of construction jobs.

CP: Yeah, so you probably saw that with your own father, I would assume?

BD: Well, he was a contractor, so, you know, he worked hard, but he wasn't—he wasn't doing the same kind of physical labor after he got into the contracting business as the people on the job were doing.

CP: So you made the decision to go to college instead of pursuit of trade?

BD: I did. When I was applying for, looking at colleges, Dad asked me if I wanted to take over the contracting business, and I said, "No, I want to—I think I want to be a physicist." And he said, "Good," and he retired. [Laughs] So he didn't particularly want me to take it over, and he just closed down the business, and spent the remaining years of his life hunting and fishing.

CP: So you were initially interested in physics, then, when you went to college?

BD: Yeah, physics or engineering, right.

CP: And you chose Occidental?

BD: I did, and it was kind of an accident. I had a cousin going to Occidental College. I'd never even heard of it; I didn't know where it was, even though I had worked on some construction jobs just a couple of blocks away. But I applied to MIT and Cal Tech, and Occidental was sort of a release valve. I didn't get into Cal Tech. I did get into MIT, but that seemed an awful long ways away. So I went to Occie instead. It was the best thing that I'd ever done, but it was sort of a careless, you know, careless decision. But it was a terrific school, and I got a good education both in science and liberal arts. Then I met my wife Sharon there, too, which was a good deal.

CP: So what was your first impression of Occidental, then? What was the environment like for you?

BD: Small, friendly. You know, everything was brand new, and all of a sudden I went from a blue-collar high school to a, you know, to a really good small liberal arts college. So it was really a mind-stretching experience, I think, to get into that kind of an environment out of the town of Bell.

CP: How did you make the decision to move into geology?

BD: Well, you had to take a couple of courses in other sciences to be—for the physics major, and so I took an introductory course in geology. I had never had any geology before. I thought maybe geologists collected rocks, and that was about it. [0:09:59] And I took an introductory course from Professor Joe Birman, who was chair of the two-person department there, and just really enjoyed it, and particularly liked all of the things that were not known about the earth. And that seemed a lot more exciting than physics. I liked the out of doors. I had done some backpacking, and a lot of hunting, and geologists spend a lot of time outdoors, so. So I went in and talked to Joe Birman, and told him I was thinking about changing my major, and you know, what—could he tell me more about geology? So he explained as much as he could in a few minutes about geology and the challenges. And one of the questions I asked him were, you know, how were jobs in geology. And he said, "Well, they're just terrible. You can't get a job now for love nor money." And I said, "That doesn't sound very good." And he said, "No, that's good, because by the time you get out of graduate school it will have recovered, and there will be lots of jobs." Because jobs for geologists tended at that time, and I suppose they still do, to follow the oil company, because most geologists worked for oil companies. And at that time, the oil business was depressed. But he said, "Oh, it goes in seven- or eight-year cycles." And he said, "So this is a good time to start." [Laughs] So I switched my major.

CP: Some sage advice.

BD: Yeah, yeah. I don't know whether it was just dumb luck or what. By the time I got out of graduate school, I had a number of job offers. It was fairly easy at the time.

CP: So how was the curriculum in geology then at Occidental? Was it—were there a lot of field trips, like you had hoped, and field work?

BD: There was some, yeah. We spent some time doing field work, and both the professors there—one was a paleontologist stratigrapher, and the other, Joe Birman, was a hard rock geologist, you know, volcanoes, batholithic rocks. He was also a geomorphologist. He studied glacial terrain. Both of them had research projects going, and so they would often invite the few students they had—the geology department wasn't very big—to accompany them on field trips, and dig up fossils, and map glacial moraines. And anyway, it was a good education, and by the time I left Occie, I was fairly good at, you know, doing field geology.

CP: And that was an experience that cemented your interest, then?

BD: Oh yeah; yeah, it was. And I liked the—at that time, at the time I was in college, there was no theory for the Earth. I mean, everything was kind of ad hoc, and you know, try to figure out how all of these little individual pieces fit together, which no one ever did until plate tectonics came along. It was a real challenge. And so I was real fortunate, I think, in that I started geology at a time before there was a theory of the Earth, and then worked on both sides of that revolution. And it was really exciting to see that happen. But I'm getting ahead of myself here.

CP: What was social life like at Occidental for you?

BD: Oh, well, it was, you know—I belonged to a fraternity, and most kids did. And there were parties and dances, both fraternity-sponsored, and school-sponsored, and sorority-sponsored. And so the social life was good. I mean, there were always lots of things to do. College was a bit different than it is now, in that all of the women had curfew at 10 o'clock. They figured if they locked up the women, the men couldn't get in too much trouble. [Laughs] So there were curfews you had to work around for, you know, for dates and things, but they were more liberal on the weekends than during the week. Men had to wear long pants, and the women had to wear skirts, and I mean, there were dress codes, and you know, of course, that's all gone now. Much for the better, I think, but.

CP: And you met Sharon there?

BD: I did, yeah. She was actually going with one of my fraternity brothers, and a good friend. He was a year ahead, and in the junior year, he went off to—which would have made me a sophomore and Sharon a sophomore; she was in the same class—would have made him a junior. So he went to—after his junior year, he went to dental school in San Francisco. So Sharon and I were good friends, as were John and I. And I wasn't going with anyone in particular, and her boyfriend had gone off to San Francisco to dental school, so if we didn't have a date, I'd call her up, or she'd call me up, and say, "Hey, are you doing anything for this dance or this party?" Say, "Well, no, I don't have any—don't have a date," and so we'd go together. And one thing led to another, and pretty soon we ended up engaged.

CP: And you were married by the time you graduated from college?

BD: [0:14:59] We got married two weeks after graduation, in 1959.

CP: So, from there you went to Berkeley?

BD: I did. I always wanted to get a PhD, because Joe Birman said that's—you know, that was a good thing for me to do, so. A lot of things that happened to me, or decisions I made, were kind of, you know, spur-of-the-moment, accidental, not very carefully thought out, you know. Getting a PhD was one of them. It turned out to be a really good thing, but, you know, Joe Birman at Occidental said, "Well, you should go to graduate school, and get a PhD." I said okay. So I interviewed at a couple of places, Stanford and Berkeley, and I didn't get any scholarship at any place. I think my grade average when I left Occie was maybe a B+. I had A's in geology, and A's and B's in science classes, but with the

liberal arts, I didn't do so well. I wish now I had paid more attention, but. So I didn't get any scholarships or teaching assistanceships in my first year, and Stanford was too expensive, so I didn't go there.

But Sharon and I drove up to Berkeley to interview, and the graduate advisor then, who essentially made the decision of whether you got in or not, was Charles Gilbert, who is a fairly well known geologist, wrote some textbooks. And the first time you met him, he always seemed like he was kind of grumpy, but it was just kind of a mannerism. He was one of the sweetest, nicest men you could ever meet. So, Sharon and I went in to interview with Charlie Gilbert, and he said, "Let me go get your transcripts." So he went down and got my transcript, which I had submitted, you know, weeks or months before. He opened the folder and looked at it and said, "Oh, this is good. This is bad. This is good. This is bad." [Laughs] And then he hemmed and hawed for a while, and he said, "Well, I guess I'll let you in." But he said, "But I have to let you know that at Berkeley, we expect our graduate students to get straight A's." He said, "That doesn't mean I did, but I expect you to." [Laughs] And I knew immediately that I liked that man.

So, ended up going to Berkeley. Sharon was an education major. She got a teaching job at the Orinda School District, and we were off and running.

CP: So you mentioned you didn't have any financial support at the beginning. Did she kind of keep you guys afloat?

BD: Well, she made enough for us to live on. I mean, we rented an apartment out in Lafayette, and you know, we could buy food, and that's about all—and we had two cars, because I had to get into Berkeley and she had to get to work. We had two old cars. And so we had to buy gas, but we used to save up for a month to get enough to go to a movie, and if we wanted to have popcorn, then that was two months.

We used to go to the local Safeway and they had a section on a shelf there where the labels had come off the cans. I don't think labels come off the cans anymore, but then they used to come off the cans. So they always had this shelf full of cans that didn't have any labels, and they had them divided as best they could into fruit and vegetables. And sometimes they were wrong. You know, you would get fruit out of one section when it should have been vegetables. So I remember one, at the end of one month, we had a can of peaches for dessert and pears for dinner, or vice-versa. But we were fine.

CP: Well, how did your graduate school experience with people—I mean, was there a teaching component to it? Obviously there was a research component.

BD: Well, we had a—every student at Berkeley used to, regardless of your background, had to take a field course. They wanted to find out if you could map, you know, and if not, then this course would improve that, or they might make you take another one. So all of the graduate students, which were, I don't remember now, 12 or 14 from all over the country, had to take this field course. Chuck Meyer was the instructor. He was a mining geologist, a really good field mapper.

And I did well on that course, and I guess impressed him enough that I was recommended to teach the summer field course, along with three other instructors. One was a professor at Santa Barbara, and then two, Garniss Curtis and Jack Everndon, were professors at UC Berkeley, and I was the only graduate student. And that was six weeks of—six or eight, I guess six weeks of paid teaching assistanceships. And then the year after that, I got a teaching assistanceship teaching, helping to teach beginning geology. And then the following two years I got NSF fellowships.

So after the first year, I had financial support, and then we were in really good financial shape, because Sharon had a teaching job. And so by the time I got out of graduate school, we had enough for a down payment on a house, and a down payment on a new car.

CP: [0:20:03] How about the research side of things? How did that evolve at Berkeley?

BD: Well, it came out of that summer field camp teaching experience, because Garniss Curtis and Jack Everndon had been working with John Reynolds in the Physics Department on isotope dating techniques, which at the time were brand new, and in particular potassium-argon. John was using it for meteorites, and they had obvious applications to geology. So somehow, John Reynolds and Jack Evernden and Garniss Curtis hooked up together, and for a while we used the equipment in John's lab in the Physics Department, and then we'd build our own at Berkeley in the Geology Department.

But in the evening, the students were off working on their maps, and the four of us who were instructing the course would typically spend our time together, maybe going over student papers, but more likely drinking a beer and just having a conversation. We had our own tent. And so Garniss and Jack were talking about these new dating schemes, and I thought, well, that sounds like it might be a good thing to know. And so I asked them if maybe I could work with them a little bit and learn this technique. And they said sure.

So upon returning after that summer field camp, I started learning how to hook two pieces of glass together, and all about ultrahigh vacuum, and mass spectrometers, and isotopes. So that's really how my research grew out of that. Prior to that, because of Joe Birman's influence, I was interested in geomorphology—you know, glaciers and landforms, and so my thesis turned out to be trying to date some of the land forms in the Sierra Nevada, because a lot of them have lava flows on them, and to determine something about the uplift history of the Sierra.

CP: You eventually published a book on this technique.

BD: Yeah, it was new enough then that there wasn't any really good textbook about it, so when I was at the U.S. Geological Survey I published a book called Potassium-Argon Dating with a colleague.

CP: How was that received?

BD: Great. I mean, the reason we published it for—the reason we wrote it was because there were a lot of—we were constantly getting pestered by other geologists wanting to know, how does this work? So it wasn't a how-to book; it was sort of a how does this work book for other geologists. [Coughs] And you know, it was fine. I think in the end it probably sold four or five thousand copies, something like that, which for an esoteric little book like that is pretty good.

CP: You were at Berkeley in the early years of the Vietnam War. Was that—later on, of course, it became sort of a central area for protests against it. Was there anything going on at the time that you remember?

BD: I think it was the Korean War. I got out in '63.

CP: Okay.

BD: When did the Vietnam War start? Wasn't that in the '70s?

CP: Oh, that was the late '50s, early '60s.

BD: Okay. No, Berkeley was not a hotbed of protest until 1964, when Mario Savio hit the scene, and I was gone. I left in '63, so I missed all of that. Berkeley was just kind of a weird place. We lived in Lafayette for two years, and then we moved into Berkeley, where I could walk into school and sold a car, and Sharrie would drive out to Orinda to teach. And I used to walk down Telegraph Avenue every day, and back. And it was a real treat just because so many weird things going on in coffee shops, and people dressed in strange costumes. But there wasn't any rabble-rousing at the time, really. That happened the year after.

CP: Tell me about the NSA fellowship. You received that in 1961.

BD: Yes. It was the last two years, so that would have been '61–'62, and then '62–'63. Well, you applied for them, and you either got it or you didn't. They were fairly easy to get at the time. I don't know; I think a good many of the students there at Berkeley had them. And it was a few thousand dollars, which at that time was quite a bit of money. I think Sharon's teaching salary when she started was four thousand-something. And so an extra two thousand, I think you got five hundred more for a spouse, whether working or not, and you got a few extra for kids. We didn't have any kids. So we were in really good shape. You know, we could afford to go to the movie more than once a month, and eat good food all of the time. After that we never had peaches and pears for dinner and dessert.

CP: [0:25:00] Or if you did, you at least did it on purpose.

BD: Right. Exactly.

CP: So that was in support of the potassium-argon work?

BD: Well, it was for—you got those fellowships for research, yeah.

CP: How did you move into the USGS?

BD: During my last year at Berkeley there was a field trip out of the Owens Valley organized by King Huber, and it brought together, oh, I don't know, half a dozen people from the USGS, and some people from Berkeley, including myself. And it was just an informal thing; there were maybe a dozen of us, to get together and go around to look at some features in the Owens Valley, volcanic rocks and things like that. And at that time, two of the people there were—Allan Cox and Dick Dole, Richard Dole, were on this field trip. And we spent, I think, two or three days going around looking at things together. And then in the evening, Allan would often call me over, and we'd sit there with a beer in hand, and he started talking to me about magnetic reversals. And I didn't quite realize until later, but he was actually recruiting me, which he was very good at. So when I graduated, I got an offer from them. I had several other possibilities, too, but I got an offer from Dick Dole and accepted it. I remember I had an offer from Kansas State as well, and the chairman of the department there kept calling me up and asking me if I was going to take this job, and I finally said, "No, I'm not." And he said, "Well, you know, why? Could you tell me why?" And I said, "Well, there are a number of reasons." One of them was I didn't like the Midwest. I didn't tell him that, but I said, "Can you tell me how far it is to your nearest rock?" And there was this long silence. [Laughs] He finally said, "I see what you mean." [Laughs] But the job at the Geological Survey was attractive because of Allan and Dick. They were real pioneers in paleomagnetism at the time, and you know, probably two of the leading people in the field.

And the Survey, the geologic division was a research environment. It was much like a university without students. We even did have some graduate students who would come there to work periodically. And it meant we didn't have to move out of the Bay Area. We liked the Bay Area, and were quite happy to stay there, so that's how I ended up over there, was because of a field trip and when I was a—my last year of graduate school.

CP: And you settled in Menlo Park, is that correct?

BD: We originally bought a house in Menlo Park. The western regional headquarters were in Menlo Park, and we bought a house about three blocks away. And we lived there until, say, from '63 to '67, and then we bought a house in Palo Alto, where we remained for 28 years.

CP: And that's where you raised your three daughters?

BD: Yeah. We started having kids when we were in Menlo Park, and by the time the third one, Mindy, came along, we needed a larger house, because the very first house was only two bedroom, one bath. So then we moved into Palo Alto, into a three-bedroom, two-bath place.

CP: Did Sharon continue teaching?

BD: No, she didn't. She thought it was more important to raise the kids. And we didn't need the money. I mean, I was making 8,030 dollars a year to start with, and that was a pretty good salary at the time. So she stayed home to raise the kids, and as they got older, and were more independent and able to be by themselves—this would have been when they were—I don't remember when she went back—maybe ten or twelve, something like that. She started teaching part-time at a local private school. And then eventually she went back full-time.

CP: So when you started out at the USGS, you mentioned magnetic reversals and the theory of the Earth. Is this the area that you set out to study or work on initially there, or did that come later?

BD: No, I set out to work with Allan and Dick to work on reversals. In fact, we published our first paper together before—excuse me—before I got out of Berkeley. It had been known for decades that some rocks showed reversed magnetism, and what I mean by that is—a lava flow is the simplest example. As a lava flow cools and the minerals go through their Curie point, they remember, or retain a record, of what the magnetic field is like at that place, at that time. So here, for example, if a lava flow came out in the office, the magnetic field would be pointing downward to the north at about a 45 degree angle. [0:30:02] So, because we construct the magnetic field, or visualize it, as a dipole field, that's as if there

was a bar magnet in the center, so the lines of flux come out and go back down, if you know what the magnetic field is like, you can reconstruct it. From one place, you can reconstruct the entire dipole field. In other words, you can determine where the north and south poles were from just one measurement. Now, there are lots of irregularities on the field. I mean, the dipole is something that we—it's a mathematical convenience, because there's lots of non–dipole fields too, but in general, you can tell pretty well where the field was. And if you sample lots of rocks from different places, you can pin it down rather precisely. So you can measure its wobble, and you can measure whenever it changes polarity.

So it had been known for a long time, since the early 1900s, that there was such a thing as reverse magnetism in rocks, but there were two hypotheses to explain it. Number one: the Earth's magnetic field had reversed. And number two: there was some property of the rocks and minerals themselves that caused them to retain a magnetism at exactly the opposite direction. And there were theoretical reasons that that might be so, and there have even been one or two rocks found that actually did that, but it's very, very, very rare. By and large, the rocks, and particularly lava flows, retain a very robust history of the magnetic field. It's hard to get rid of; it's hard to erase, unless you heat the rock up to beyond its Curie point, and cool it in a different magnetic field. It's there practically forever.

CP: So this research program sounds like it was basically three people that were leading it? Is that correct?

BD: Yeah, the three people. We had a staff of technicians, and a shop, an instrument shop, because at that time you had to build almost anything you wanted. You couldn't buy most of this stuff. But there are two ways to tackle this problem. One was to look at the rocks and minerals, and see if you could—in normal rocks and reversed rocks—and see if you could find some characteristic in the minerals that was different from normal rocks than it was from reversed rocks. That always seemed like kind of an experiment that would lead to ambiguity, you know, because you don't know really what you're looking for, except some kind of a difference.

The better way to do it, and the one that Allan and Dick wanted to do, was to look at normal and reversed rocks as a function of time. And so they could do the magnetism, but they couldn't do the time part, so I was the time guy. And so you needed both of those things, and you couldn't have done it in the early 1900s, because you didn't have the kind of techniques that were available in 1960. They were all pretty new. So what we started doing is collecting volcanic rocks from as widely around the world as we could, mostly in the Western Hemisphere, but western U.S., Alaska, Hawaii, a few places in South America, looking at their polarity, reconstructing the field to find out where the north pole and the south pole were, and then measuring their ages.

And at that time, the accuracy of potassium-argon dating was about three percent, so you could get pretty good precision back to about four million years. And so you just measured the age of the rock, measured the magnetic polarity, and then plotted normal or reversed against time. And if they came out in distinct bands, then you knew it had to be the Earth's magnetic field. If they came out all scattered and didn't make any sense, then it either was happening so fast that we didn't have enough dating precision to separate out the bands, or it was something in the rocks. As it turned out, it came into nice discreet bands. And we were doing that work, not because we thought it was going to lead into plate tectonics, but just because we thought the Earth magnetic field reversing itself was pretty interesting.

Now, stars have been observed to reverse their polarity, and you can tell that by looking at the spectra coming from different elements in the stars. And in fact, the sun reversed its polarity in the 1950s—'53 or '54. And sun spots reverse their polarity. So it wasn't something that was totally weird. And it turns out that most of the magnetic field is trapped inside of the Earth. You never see that part. It's probably a torrential field or something goofy like that. And what we see on the Earth's surface is just a—energetically—is just a really minor part of the magnetic field. And it turns out to be very unstable, and the magnetic field really doesn't care which way the north and south pole point. It could be this way or it could be that way. And so it kind of flips back and forth. I don't—at the time I retired and lost track of this, no one had figured out exactly why this happens. [0:35:03] And I suspect they still haven't.

But at the time it was possible to construct mathematical models of simple dynamos that behaved like that, that most of the magnetic field was very stable, except the polarity itself was just unstable, and kind of flipped back and forth.

CP: So, at what point did you realize that this work was going to have a pretty big impact?

BD: Well, the first thing that happened was that Fred Vine and a couple of colleagues matched up our polarity time scale with the magnetic stripes on the sea floor. Oceanographers had been towing magnetometers behind ships for a long time, and they came up with these really weird patterns of magnetic stripes, and they called them positive and negative, but there's no such thing as a negative magnetic field. So it was normal and reversed, is really what they were looking at. They didn't have a clue what caused them, but they kept mapping these things. And there was a really nice map done off the Oregon-Washington coast that had been done by, I think it was NOAA was doing most of this stuff. And then there was another really nice map showing this, just south of Iceland at the Reykjanes Ridge, that showed this symmetrical parallel stripes, and nobody had a clue what they were.

But Fred Vine, who was a British geophysicist, and a colleague were able to match up, show that the pattern—the reversals aren't regular; they occur in irregular types. So what you get is a kind of a unique layer. You get like a quick reversal, and then a longer period, and then a longer one, and then a shorter one. So you can actually match them up. So they were able to match up these stripes on the ocean floor, on one side of the ridge, with a reversal time scale. And that was when the light bulb came on that these were—these stripes were caused by magnetic reversals. And as the sea floor —new volcanic rock came up at the ridges, and moved out and cooled, it retained a memory of the magnetic field. And so you had these symmetrical stripes, each side recording the magnetic field over the last few million years.

And then there were some people in '65 who actually came up with the theory of plate tectonics, which explained the ocean ridges, spreading ocean ridges, the deep trenches, how you move continents around. You know, continental drift was first proposed in the early 1900s too, by a guy by the name of Alfred Wegener, who was a meteorologist. And what he was doing mostly was matching the shapes of continents and saying, "Ah, these fit together. You know, maybe they've moved apart." And I remember at Berkeley that this was a big subject of debate without any conclusion, because a lot of people said, "Oh, nobody gave Wegener credit for this." Well, he did get credit for it, but the problem was at that time you had to plow continents through the mantle. You know, you had to somehow move continents through what was thought to be a stagnant mantle. And Wegener could never explain how these things moved.

And in fact, the viscosity of the mantle is so high you can't do it. You can't do it today. And so it was only when they figured out that the continents aren't plowing through the mantle, but all of these features are moving on this thick lithospheric plates that are like 100 kilometers thick, and the continents are just riding along. And it's these spreading plates that are coming up at the ocean ridges and being destroyed at the trenches that are the key to how you move continents. So Wegener had it right that continents have moved, but he could not explain how they moved, and at the time, geophysically, it was impossible. The physics just didn't work. So, I don't—at Berkeley anyway, Wegener is given credit for making these wonderful observations, but he didn't have a complete theory.

And once plate tectonics was formulated, it was pretty obvious how all of this stuff, you know, fit together. And it also became true that if you moved one part of one plate, then everything all over the Earth has to move, because you now have these plates on a sphere. You can't move one without affecting everything else. So there were a lot of mysteries, that when I was an undergraduate student, nobody knew why the Appalachians were there, for example. It's this big mountain range, you know. How did it get there? Well, it turns out it's where two continents clapped together four hundred and some odd million years ago. And they banged together just like India's banging into southern Europe, and causing—or Asia, and causing the, you know, causing the Himalayas to rise up. [0:39:59]

So all of this stuff that we used to have all of these ad hoc, what now look like silly explanations for it, you know, became pretty simple in concept. You know, the details are still complex, but all of a sudden the Earth had a theory, and we could explain almost anything. We knew why volcanoes are where they are, and are different types. We knew why mountain ranges are where they are. And when I was a student, the ocean basins were going to be the source of great mineral wealth, because they were the oldest features on Earth. And as soon as plate tectonics came along, it was pretty obvious they were the youngest features on Earth, not the oldest, and there wasn't going to be much mineral wealth in the oceans at all, which is true.

So it was an exciting time to live through this, but we had no idea that the magnetic reversal time scale was going to be important, in that sense.

CP: Well, you had a long career at the USGS, mostly as a research geologist. This is maybe too broad of a question, but I'm interested in just getting a sense of how your work evolved over time.

BD: Well, I would—

CP: There has to have been a lot of different areas in your research.

BD: Yeah, most of the research I did wasn't very exciting. It was kind of helping other geologists, you know, do their thing, which I never found very interesting. But the other big experiment I was involved in over a number of years was tracing the evolution of the Hawaiian Emperor volcanic and seamount chain. You know, that's a long chain of volcanoes that starts at the young end of Hawaii. It goes out through Midway, and well past Midway it takes a bend, and the seamount chain goes up and disappears in the Aleutian Trench. And there's one island beyond Midway, Kure, and after that they're all underneath the sea. And what this represents is essentially some kind of a melting spot in the Earth's mantle that's been feeding volcanoes for over 60 million years, 60 to 80 million years.

The current outlet's on the Big Island of Hawaii. There are a couple of active volcanoes there. There's a new one popping up on the south flank of the Big Island called Loihi, still under the sea. And as the Pacific Plate continues to move in a north-northeastern direction, the volcanoes get cut off from their source, and a new one pops up, so you've got this chain being continually formed. And as they move away, volcanic rock is no longer being supplied to the islands, they sink, just because these are huge masses of rock on the ocean crust, and they sink. And eventually they sink far enough so that the volcanoes, the volcano top erodes. Coral starts to, you know, become the top of the volcano, and then the sinking becomes fast enough and enough that the corals can't keep up, and they end up way below the sea. And this bend in the—where the Hawaiian chain becomes the Emperor Chain represents a major change in plate motion 42, 43 million years ago.

So we started—Tuzo Wilson—the age of the main Hawaiian Islands was known for quite a while. Tuzo Wilson was the one that popularized the ideas of these things moving over some kind of a, he called it, a hot spot. But we wanted to find out: are these really all Hawaiian volcanoes, all of the way out? I mean, do they resemble Hawaiian volcanoes in composition, for example, and rock type? And what's the age progression? You know, did they all come up at once, or once you get out of the main Hawaiian Islands there, they get older and older to the north and west? So we followed, sampled these volcanoes out as far as there were volcanic rock left, so Nihoa and Necker. And then beyond that we had to dredge, do some dredging with ships, to get rocks off the top, which is kind of an imprecise way to sample, but it's what you do if that's all you can do.

And then we were able to put in a successful proposal for a deep sea drilling leg, Leg 55, to drill on the Emperor seamounts. This was not with Allan and Dick. They had both left the Survey by then. Most of this work I did with a couple of colleagues, Dale Jackson and David Clegg. And we spent over, probably, ten years tracing the history of these volcanoes out into the Emperor Chain. And they have now been sampled probably about halfway up the Emperor Chain, really good samples that you can measure their age. And they all do resemble Hawaiian volcanoes. It looks like they all came from the same source, and the age progresses regularly all the way out, just in a straight line.

CP: I would imagine there were some adventures along the way, as you were doing all of this? [0:44:59]

BD: Yeah, there were a few. Excuse me; can you excuse me for just a minute? Well, Dick Dole was involved in the first part of this, and he and I actually got together with some Fish and Wildlife people aboard a Coast Guard buoy tender. All of the islands, once you get past Kauai, all of the islands are wildlife refuges, and they're overseen by the Fish and Wildlife Service. So we got together with a couple of Fish and Wildlife people, and went out to the islands that had volcanic rocks left on them, and spent a couple of weeks camping on these islands. I mean, there's nothing out there. There's just a big rock. I think Nihoa is 40 acres and Necker is seven acres, or something like that. But drilling for magnetic, you know, magnetic samples to measure the polarity of the field, and getting samples for dating.

And those were really interesting. I mean, camping on literally a desert tropical island is quite an experience. For one thing, you know, the birds have never, rarely, seen humans and they're just not afraid. So we'd stand around the campfire at night, and all of a sudden a petrel or something would wham into your chest [laughs], going 20 or 30 miles an hour. And when we first got there, the Fish and Wildlife people had some nice little dome tents, you know, that you could pop up, and two people in a tent. And they were nice and red, and by the time we left after a few days, they were just white, just totally white, just from all of the bird droppings. So we were constantly working around, you know, working around all of these birds. And frigate birds, you'd walk by them and they'd be sitting in their nest about two feet high, because

there's no trees or anything, there's just brush, and they're snapping at you. And had to be careful not to step on eggs, and so that was fairly interesting.

And then they dropped us off on French Frigate Shoals, which at the time was the master Loran A base for the Pacific. It hasn't been occupied for a while, but the Coast Guard operated the Loran stations. They dropped us off there, and we flew back in a Grumman Goose, courtesy of the Coast Guard. But one of the people on the flight back was a young Coast Guard seaman who'd spent a year out there. Now, after six months, they used to get a two- or three-week break, and he didn't take his break. We were talking to him, and said, "I could take that break. I knew if I did I would never go back." [Laughs] Because there's nothing on French Frigate Shoals except a—I think it's a 2200-foot coral runway that was put there during the Second World War, and a little bit of sand alongside of it, and that's it. You know, you're stuck there. So that was kind of amusing.

And then the other interesting thing that happened to us, when we were drilling, or when we were doing the dredge, we were doing some seismic surveying, preparatory for drilling. To get a deep sea drilling leg, you had to do site surveys before. And that required another oceanographic voyage to make sure. For one thing, they didn't want to have an accident. They needed enough sediment that you could actually spud in the drill rig, and put in a recovery cone. So they needed to know that before they would send a ship out to drill, and they also needed to know that there wasn't any possibility of hitting oil deposits because these ships at the time didn't have any risers. They had no way to put down a blow-out preventer, or anything else. They religiously stayed away from any site that might have oil or gas.

So we were drilling on seamounts, that wasn't a problem, but whether there would be enough sediment to get the drilling started was a different story. So we were doing a seismic survey using sparkers in the RVSB Lee, which was a USGS oceanographic vessel at the time. And we left A deck in fairly decent weather, at least as decent as you get up in that part of the world. And we got out a day or two, and a storm came up. It just kept getting stronger, and stronger and stronger. Finally, by the time we realized this was going to be a biggie, we couldn't get the gear in, so we lost the—lost the sparker ladders. We lost the magnetometer; everything we were towing got essentially sheared off by the prop.

And so there were these enormous waves. They had an estimated height—this comes from the bridge, not me—estimated height of 70 feet maximum, and a significant wave height of 35 to 40 feet. Of course, these 70 feet—70 foot swells are very long wavelength, you know, so you see them coming at you like a mountain, and then you rise up, you know, over the top of them. Usually the top 20 foot or something might break. [0:49:59] But we spent essentially two and a half, three days, just going in a circle, just, you know, facing into the wind. And I spent a lot of time up on the bridge just because I was a sailor, my whole family has sailed for a long time, and so just watching the sea in this state of nastiness was really interesting. It wasn't a hurricane, but it was Force 11, just one step below.

And we kept sending in weather reports. The radio man kept sending in weather reports, and we'd get back a weather report of what the weather was going to be like in six hours, and send in another weather report of what it was actually like. [Laughs] The radio man finally figured out that we were just getting our own weather reports coming back, because we were the only ship in the North Atlantic sending them in. [Laughs]

So, anyway, there was a big hydrophone reel on the back of the ship, and we weren't using the hydrophone reel, but the thing weighed tons, and it—just in the ship rolling back and forth so much, it, the apparatus that supported it—I mean, this was a huge reel, it was like eight feet in diameter and ten feet across, and the apparatus supporting it got all fractured. We were afraid it was going to break off and go into the steering department, or something. It didn't, but we ended up back in Midway, where the Navy kindly spent two or three days welding it back together for us, so we could get back to Honolulu. Yeah, you had those kind of adventures. Whenever you go to sea, you get something like that.

CP: You also have worked on the history of the moon?

BD: Did some work on the moon. Yeah, that was a gas. At the time, NASA—at the time they were planning the lunar landings, NASA wanted to get as much science done as possible, whether it made any sense or not. So, Dick Dole put in a proposal to do magnetic measurements on the moon. I mean, the moon doesn't have much of a magnetic field, and the rocks there are only weakly magnetic. So he developed a device you could actually measure the magnetic direction in volcanic rocks inside of a vacuum, which was no mean trick. And the idea was to measure all of the rocks that came back, or at least anything that was, say, this big or larger, inside the lunar receiving laboratory, before anything else was

done with them, when they were first being described. So that was an experiment that went on when the first rocks were returned on Apollo 11 and 12.

And then I put in one to do thermoluminescent measurements on the moon, which was—really wasn't of much use, but it was a chance to get involved in the program. They had, I think, four other principle investigators doing that. They had other people doing magnetism, but no one else measuring magnetism in the lunar receiving laboratory. And so it was a 24-hour operation, and so it wasn't just Dick; it was myself, and Sherman Gromme, and a couple of technicians. We'd rotate down there two weeks on, two weeks off, making measurements on these rocks as they first came in.

So I was in the lunar receiving lab when the first box of rocks from the moon was opened, and that was really exciting, just being around watching all of the stuff. But it was sort of—it was another one of life's goofy experiences, you know. They were afraid of moon bugs. I don't know if they were really afraid of them, but they didn't want the public to get concerned. So the lunar receiving laboratory was this, was essentially this biological lockdown facility, that, you know, had filters. And when you went inside you had to take off all of your clothes and you showered, and you had to put on white suits and white caps, and you worked in there in all of that. Then when you got done for the day, you had to shower out, and you know, throw all of the clothes that you used in there. And they got disposed of; they got burned up. And you could go through this showering ultraviolet procedure, and then get your own clothes, and go out—you couldn't have a wedding ring, watch, anything like that. Of course, all of this would have prevented bacteria; it wouldn't have done anything for viruses. If there had been a moon virus, we'd all be dead. But it was kind of funny.

One day, another one of our technicians, Ed Mankin, and I were just in the process of showering out. So we'd disposed of our lunar receiving lab white duds, we'd gone through the shower, and we were in the next room where they had towels. And after that, you went through the ultraviolet, and then you got your own clothes. Well, when we had just gotten out of the shower in the little towel room, which was nothing but a few bins of towels and a couple of benches to sit on, somebody spotted a crack in a toilet. [0:55:00] And the alarm sounded, and all of the doors lock electronically, everything. All of the individual doors inside and outside, they all lock electronically. So here are Ed and I, you know, totally nude [laughs], nothing to read [laughs], and trapped in this little room for probably an hour and a half, two hours. I don't remember how long it was. It seemed like practically forever. No way to get any clothes. You could wrap a towel around you; that was about it. So that was kind of interesting.

CP: A bonding experience.

BD: Yeah, well [laughs], I guess. I had another—one of the other funny experiences didn't have anything to do with me, but they'd had these principle investigator meetings down in Houston before rocks came back, where they were getting all of the PIs together and letting them talk to each other, and then they would explain the latest plans for where they were going to go, and how they were going to sample rocks, and what kind of samples they were going to get. You know, there's soil and there's rocks, and all of this kind of stuff. And the chief scientist for NASA then was a physicist by the name of P. R. Bell, and he was—the return lunar samples came back in a box that looks like an aluminum suitcase, like a Halliburton aluminum suitcase, except they were specially made, but that was the way they looked.

And he was explaining that these were going to be made out of 60-61 T6 aluminum, which is a very common aluminum alloy that's—and someone in the audience would jump up and say, "Oh, you can't do that, because it contains manganese, and I'm interested in manganese isotopes." And then he explained about the O-ring, Viton O-ring seal that was going to be a part to maintain vacuum inside. And someone else was interested in organic analysis, and he was disturbed because Viton's an organic compound. And then they were going to have an indium seal on the inside of that, or the outside of that, I don't remember which order they were in, and whoever was interested in indium and some of those metals, you know, they were all upset. And finally P. R. Bell said, "For God's sakes, people, we've got to make it out of something!" [Laughs]

CP: Well, in 1969 you began an affiliation with Stanford University. It looks like it lasted for a while.

BD: Yeah, I would teach a class over there periodically. We had Stanford graduate students who would come over to work in the lab, and a few of them did theses there because we had equipment there that Stanford didn't have. And they were only a mile away, so. So, it was—you know, it was a loose affiliation. Whenever I taught a class there, they gave

you a nice title, like visiting professor or something, but that was about it, that was essentially the extent of it. I knew most of the faculty there, when you spend time over there, and we had things to talk about.

CP: In 1975 you became a fellow of the American Geophysical Union, which I gather is a big honor?

BD: Yeah, the AGU elects—they can elect up to one tenth of one percent of the membership each year. So as the membership grows, they elect more and more. But that, at the time I think it was about 30,000, so they were electing maybe 30 a year, 25 or 30 a year.

CP: So that was a big milestone for you in your career?

BD: Yeah, it was pretty nice. I mean, it's a way to realize that what you're doing is, you know, someone else noticed. [Laughs] Unfortunately, there's no money, or a house in Hawaii, or gold coins or anything that come with it.

CP: In the early '80s, your title changed with USGS, from research geologist to assistant chief geologist. Were you assuming more of an administrative role at that point?

BD: That was entirely an administrative role. It was a three-year job, and it was a funny position because I didn't have authority over very many people. You know, they were all—the organization, the survey was not like that. I had about an eight or nine million dollar budget, but it was mostly for rent [laughs] in buildings that we occupied. But I was the contact with the chief geologist, and the liaison with the state geologist, and it was kind of interesting but it was a job I did care for all that much.

CP: Was there just an opening that you—was it intended to be an interim position, or—?

BD: No, these jobs were usually railroad jobs, and what happened to me happened to other people through the years. But the chief geologist would call up, and he'd say, "Brent, I'd really like for you to apply for this job." And I said, "Dallas, I don't want to do that. I'm having fun doing research, and I'm not good at administration. I don't want to do that." And he'd say, "Well, okay." A week went by and another call from the chief geologist, Dallas Beck. [1:00:01] And he said, "I'd really like you to apply for this job." And I said, "Dallas, I really don't want to do this." And I gave him three or four reasons why I shouldn't have to do it. And he said, "Well, okay."

The third time he called up, he said, "I really want you to apply for this job!" [Laughs] And I thought, "Oh, crap!" [Laughs] So, I did. In all good conscience, I hadn't really put a lot back into the Survey. I was doing research, and was doing what I was paid to do, but most of the administrative jobs in the Survey were rotational in one way or another. And, you know, if you had half a brain in your head, they expected you to do something. So I guess that was my turn to do something, but I didn't find it a very satisfying job at all.

CP: So it was sort of part of the natural arc of a long career at the USGS?

BD: Yeah, pretty much. Yeah.

CP: Well, around the same time, you got involved in a creationism trial, the first of a couple, apparently: Arkansas creationism trial, McLean versus Arkansas. Do you want to talk about how that happened?

BD: Well that happened because of another creationism trial in California, Seagraves versus State of California. And the Seagraves was a fundamentalist family who didn't think that their child should have to learn evolution in school, and they were suing the state because of the wording in the California science curriculum standards that they didn't think should be there. That is, if you're going to study biology, you have to—you know, evolution's part of that. So, I got a call from the Deputy Attorney General of California asking if I would be one of their expert witnesses on radiometric dating, which the creationists don't like, and the age of the earth, which they like even less. So I agreed to do that, and thought it might be kind of interesting. Well, that trial essentially fizzled. It boiled down to a quibble over some wording in the curriculum framework that was resolved in a minor way. The Seagraves essentially didn't get what they wanted, but the trial—none of the expert witnesses were able to—testified, because they just didn't get that far.

But that meant I was on the witness list for this trial. So when the Arkansas trial came up, I got a call from the ACLU, and wanting to interview me to see if I might be suitable to be one of their witnesses. So one of their people came out and we spent a couple of hours talking, and they finally asked if I would be a witness. There were four science witnesses in that trial. It was Steve Gould, and Harold Morrowitz, who was an open system thermodynamicist from, at the time, Princeton. Francisco Ayala, who was then at Davis. He's now at UC Irvine, I think, and he's a geneticist. And myself, as a geologist. So that was a neat experience.

I had my own lawyer, a lawyer by the name—his last name was Wolfe, a bright young guy from Skadden, Arps. Now, Skadden, Arps was a big law firm in New York who contributed a bunch of legal and secretarial talent to the ACLU causes. And there were other law firms that did the same thing, but Skadden, Arps put a bunch into that just as pro bono sort of thing. So I had this lawyer who had actually read quite a bit of the literature when he came out, and we spent quite a bit of time over a period of several months, and formulating some questions and answers.

So I had my bag packed, ready to go. When this trial started, the judge had said—federal judges have an enormous amount of power over their own courtroom. And this judge—and of course, it was a constitutional issue, so there was no jury. So the judge said, "This trial will last two weeks. And if it looks like it's slowing down, we'll work as long as it takes, but it's not going to last over two weeks." So I was told to have my bags packed, and when it looked like it was going to be within a day or two of my testimony, I was to hop on a plane and get back there.

So, I was an assistant chief geologist at the time. So I was sitting behind my big walnut desk one afternoon, and got this call from Little Rock saying, "You'd better get back here. It could be, you know, fairly soon." So I hopped on a—hopped in my little car. [1:05:00] I had a government car, and I had to drive home and get my bag. And on the way home, I was in a 35 mile an hour zone, not a lot of traffic, and the throttle stuck. So I started going faster, and faster, and faster. [Laughs] And my first thought was, maybe God doesn't want me to testify! [Laughs] I turned off the ignition and coasted into a service station, and someone came and picked me up.

Getting to Arkansas, I had to—oh, by this time, I had a script that the lawyer had prepared for me. And it was actually a script; had all of the questions and all of the answers, because they didn't want me to ramble about an answer. What they wanted was a concise answer to that particular question, and then they'd go to the next step. So, I was to read and study this script. I didn't have it in front of me when I was testifying, but I knew what the questions were going to be, and knew exactly what—you know, what kind of answer they wanted and how far they wanted to go. And so I was waiting to catch a connecting flight; it was in the Dallas Airport, waiting to catch a connecting flight to Little Rock, and I was kind of sitting there studying my script. And I looked over at the next guy, guy sitting next to me, he had a script that looked almost exactly like mine!

And so I turned to him, and I said, "I'll bet I know what you're reading." And he kind of went like this, and suspicious, you know, because he didn't know who I was. I introduced myself, it turned out it was Francisco Ayala, one of the other witnesses. We just happened to be sitting next to each other. We hadn't met at the time. But so, yeah, that was a really interesting experience, and the judge ruled that that was—that the Arkansas law was unconstitutional.

CP: So was your testimony—did it go according to plan, in the sense that it was, you were basically reciting the script?

BD: It did, yeah.

CP: There was no cross-examination?

BD: Well, there was, but—I felt really sorry for the Arkansas people. The ACLU had, I don't know how many people they had involved in this, but you know, between the lawyers, and the clerks, and everybody else, there must have been 50—I'm just taking a guess—50 people. I don't know; maybe more. And the Arkansas Attorney General, he didn't handle the case himself; he gave it to a deputy attorney general who got pretty much chewed up. And so there was him and a couple of assistants, and they were just outmanned, because when they would go to ask you questions, they didn't even know enough about the science to ask a sensible question. And a lot of it went, you know, went like that. And when they did ask a sensible question, it was pretty easy to answer, because they were being advised by the creationists, and the creationists have a lot of literature, these so-called scientific creationists. They've published a lot of stuff over the years. So all of us

who were involved in this as scientific witnesses had read all of this creationism stuff, and we pretty much knew what their attacks were and how to answer. So the cross-examination wasn't really a problem at all.

CP: There was a Supreme Court case that came after that.

BD: That was in the Louisiana case. The Arkansas case never got out of the district court, and so, of course, it didn't apply nationwide, although judges could use it if they wanted. But at the same time, right after Arkansas passed their law, Louisiana passed a law that was almost identical. It was taken from the same script. And so this same team that did the Arkansas law was next to go to the Louisiana trial, when that happened. In fact, I and the others had already been deposed for the Louisiana trial.

And one of the funny things in that disposition was that the lawyer for the State of Louisiana asked me, he says, "You know, I'm not very familiar with science and scientific terms. Maybe you can help me if I get tangled up?" [Laughs] And the ACLU lawyer jumped in, and he says, "No," he says, "He's supposed to answer your questions, not help you ask them." [Laughs] And so when he got all tangled up, all I could do was say, "I'm sorry; your question doesn't make any sense. I don't know what you're trying to ask." But we'd already been deposed for the Louisiana trial, but after all of the depositions were in, the judge in Louisiana, the district judge there, said that the case was so clear that he didn't need a trial, so it was a summary judgment. And that's the one that went to the Supreme Court, and the Supreme Court upheld it.

CP: So there was never any further involvement from you, as far as that went?

BD: No, from then on it was—no, that was the only legal involvement, were those three trials. But you know, from then on I was being invited to talk about the subject, and, you know, writing a few papers about it. [1:10:03] And I spent over 20 years doing that kind of stuff as kind of an interesting sideline. It was fun, because I met a lot of people I wouldn't have met. In the first place, I met a lot of creationists, who were really pretty interesting people. You know, some of them were a little goofy, but they were pretty interesting in their own right. And then I met a lot of people that I wouldn't have met, you know, like Francisco Ayala is just a wonderful, wonderful guy and great scientist. And he's in genetics and biology, and I probably never would have met him. I might have at the Academy; he's an Academy member, but I wasn't at the time. So it was a good experience. I don't regret doing it, and spending that time, and it was a public service.

CP: Were there ever any threats?

BD: No.

CP: No.

BD: No, I never had any. The majority—all the creationists I've ever had any contact with are really pretty kind, gentle people. They're not the sort of people that, you know, get mad at you. In fact, when you talk with them, their replies are always very calm. They never seem to get mad. There were a few exceptions to that, but mostly they were fairly kind, Christian people. They just have a different view of the science than the rest of us do.

CP: In 1991, you published the book *The Age of the Earth*?

BD: Yeah, and that came out of my involvement with the creationism, and it's because there wasn't any good book on the age of the Earth. The age of the Earth has been known, essentially, since the 1950s. Patterson, at Cal Tech, who was a research professor there, you know, figured out how to do that using meteorites. And so he came up with 4.54 billion years, and the number hasn't changed significantly since, even though there's a lot more evidence now than just the few meteorites he used. But there wasn't a really good book that explained how we know the age of the Earth. The last comprehensive one had been published in the '30s, and of course it was totally out of date. That was before any of the isotope evidence was available.

So I started out to write a little, kind of a small book, that would explain it to someone who was just interested in science, but didn't necessarily have a background. I got so interested in the subject that it turned out to be rather a fat thing that only—would only interest people who are really interested in the age of the Earth. And that was the first one I published, was called *The Age of the Earth*. And I had a contract with Stanford for a smaller book that would leave out all of the math, and you know, explain things in a simpler way. And I never did—I didn't get that finished; I'd worked on it. But

I finished it up just as soon as I retired from OSU. And then that one came out in, was it '80-something, or, excuse me, 2004, and that was called *Ancient Earth, Ancient Skies*. It was a little bit of a thinner volume.

CP: Your reference to Stanford University Press reminds me that the potassium-argon dating book was published with W. H. Freeman.

BD: Mm-hm.

CP: Which I have an affinity with because of my connection with Linus Pauling, who was reporting to that.

BD: Oh, yeah.

CP: And sort of the founding of that company.

BD: Well, Bill Freeman, yeah. He's the one that recruiting Pauling for—and Bill, I knew—when we published this book with W. H. Freeman, Bill Freeman didn't own the company anymore. He lost it in a divorce settlement. But I knew Bill from other reasons, and he told me once that he liked to maintain a line of good quality science books, but he didn't make any money on them. Books like ours, they wouldn't make any money one. But he hoped every year or so to find a Linus Pauling's *General Chemistry*, which had made—at that time selling, you know, 50 or 100 thousand copies a year. And that's where they made their—that's where they made their money. And they had another one, Gilluly, Waters and Woodford, I think, was a geology textbook that sold, you know, hundreds of thousands of copies over the years. So yeah, Pauling was one of Bill Freeman's good moneymakers. But at the time I wrote that first book, Marv and I wrote that first book for W. H. Freeman, he didn't own them anymore. They were still in San Francisco.

CP: Yeah. From the letters, he seems like a real charismatic fellow.

BD: Who?

CP: Freeman.

BD: Oh yeah, yeah, he was. He was really a book lover too. [1:15:01] He regretted the fact that book companies were being taken over by lawyers and accountants, and were no longer being run by people who just published books because they love books. And that's one of the reasons the prices of books have gone up so much.

CP: Did you ever meet Pauling?

BD: Never met Pauling, no. I knew his son-in-law, Barclay Kamb, who was a professor at Cal Tech.

CP: Yeah, a geologist of some renown.

BD: Yep, yep.

CP: Well, in 1992 and 1993, you were elected to the triple-A-S and also the National Academy of Sciences. Do you have any interesting memories of that experience, of those experiences?

BD: Well, yeah. The American Academy of Arts and Sciences was an organization I didn't know too much about, but it was kind of a nice honor. That was in '92. And I like to work in the lab myself a lot, and I would typically get there at 6:00 in the morning, 6:30, grab a cup of coffee and a donut on the way, and you know, get an early start and work twelve hours. I really enjoyed that. So in April, on Monday morning, I was in the lab really early, and it was about—it was I guess maybe 7:00 or 8:00. I got a call from the president of Occidental College saying that the trustees had thought I should be awarded an honorary degree, which was really cool. They don't give many to alums. And so I said, "Thank you very much, and let me know the details of when."

And the very next day, Tuesday, I got a call from a colleague, Warren Hamilton, who was USGS in Denver, calling from the Academy meeting in Washington, D.C., the National Academy of Sciences, telling me I had been elected to the National Academy of Sciences. So that's pretty cool. And so the following weekend, our three daughters, and Sharon and I were out on our sailboat. We had a 30-foot sailboat for over twenty years. So we were out sailing in the bay, and

I happened to mention it. I said, "You know, I had these two really good things happen earlier this week. Good things are supposed to come in threes, I wonder what's next." And our daughter Robin didn't miss a beat. She said, "Dad, buy a lottery ticket." [Laughs]

CP: [Laughs] Did you take her advice?

BD: No, I didn't. [Laughs]

CP: [Laughs] Well, in 1994, you came to Oregon State.

BD: I did, yeah.

CP: Why the change?

BD: Well, I had worked for the Geological Survey, at that point, 31 years, and I had—I could retire with a pretty decent retirement. And after a while, you lose your sense of humor working for one organization, and I thought I'd like to try something different. So I started looking around for other jobs. And when you're a member of the National Academy of Sciences you have job opportunities that you don't have when you're not a member, because universities like to collect people that. So I figured, well, it's worth a shot. So I applied several places, and got several job offers, but I decided to come to Oregon State. I had never been a dean before. I thought, well, maybe I'd make a halfway decent dean. It would be worth giving it a try, anyway.

CP: Was that the attraction then?

BD: Well, the attraction was just to do something different, and to come to an academic environment full-time, which was kind of interesting, had lots of students, and lots of faculty. It was somewhat of a different place than the U.S. Geological Survey. And it was just sort of a change of scene. I wasn't quite ready to hang it up entirely. And of the interviews and job possibilities I had, this one—I like the area; I like Corvallis. And neither Sharon nor I had ever lived in a small town before, and it was kind of appealing, with this downtown that's alive. And we had always been around a university town, and so we thought this might be a nice place to live. And it was on the Pacific Coast, near enough to the ocean where we could go over and look at it once in a while.

And I liked the people. I mean, I thought the people were really something special. Everybody was friendly; everybody was really impressive. And of course, oceanography at OSU is something special, too. It was the kind of high-power research environment I'd spent my career in, so it was nice being around that. And so it was a fairly easy decision. So we sold our house, and threw away a bunch of stuff, and moved north with 15,000 pounds of stuff in a moving van. [1:20:01] [Laughs] Mostly books.

CP: What was your sense of the university when you arrived?

BD: Well, if you haven't spent a lot of time in the university, it can be a confusing place. So it took a while to get everybody sorted out, and figure out what was what, and how to do the job effectively. But I liked it from the very start, just as I say, because the people I ended up working with were, were—they were nice, they were helpful, they were encouraging, and you know, a lot of them became really good friends. I mean, some of our best friends still are people who we met, who were deans. Deans socialize a lot with each other, because if you move up, you're in a different circle, and if you move down, you're in a different circle, and so the deans end up kind of being isolated. You know, you always have about twenty percent of your faculty kind of mad at you [laughs], and that makes parties awkward! [Laughs] So we still have some very good friends who are former deans at OSU.

CP: And you mentioned the college was in really good shape when you arrived, the College Oceanographic and Atmospheric Sciences.

BD: It's a different place. It's not quite so different from oceanographic institutions, but it's a different place from any—from most departments or colleges at most universities, in that it's supported, you know, very much by grants and contracts. When I was there, only six percent of the money came from the university. The rest of it came through grants and contracts, and the overhead from that. So, it's a self-sufficient—it was pretty much self-sufficient. Now, from the

university, we got all kinds of support: buildings, heat, lights, access to the internet, that sort of thing. But in terms of just raw cash to hire faculty and things like that, that was always a struggle. But it was because of the faculty that that worked.

And it's an unusual culture. For example, at the Geological Survey and at Stanford and other places, you will find emeritus professors hanging around, occupying—or, non-emeritus professors hanging around, occupying positions into their 70s. At Oceanography at Oregon State, that tends not to happen, because as soon as—they're all on contract, nine-month contracts, which means if they can't bring in their share of their salary from grants, then the university has to pay for it anyway. Well, that responsibility falls to the dean. Somehow you have to find the money to pay; you can't not pay them. And so at that point, the faculty, when they get to the point where they're not able to bring in grants and contracts anymore, which tends to happen to people in their 50s, they know that if they stay they're going to be a hardship on their colleagues, and they don't want to do that. So it is very common to see people retire in their late 50s.

CP: Hm, interesting.

BD: Very few hang around longer than that, and the ones that do are the ones that are still bringing in money. And the ones that aren't, you know, they leave. They retire, because they can collect their retirement by then. So it's an interesting culture. And I didn't start that; I mean, that started years and years ago. But it's what has allowed Oceanography to, you know, to continue to grow.

CP: What were your priorities as an administrator when you took the job? How did those evolve?

BD: Not [laughs] do any more damage than possible! Oh, I think the job of a dean is not to get in the way of the faculty, particularly in Oceanography's case. You know, those people knew what they were doing. They knew what their research was. They knew their program managers. They knew how to get money, you know. Who was I to stand in their way? And there are no departments in oceanography, either. The way things would get decided, like who was going to teach each course, the faculty in, say, biological oceanography would get together in a room with cups of coffee, and they would hash it out themselves. And they would decide who was going to teach a course. Some would want a little extra time, because they had some cruises or something. That was fine, but they knew that they would have to take on a little extra burden next time. And they all—they worked it out amicably, so I never had to get involved in assigning teaching duties, or anything like that. They just assigned them themselves, and all I had to do was kind of look over the schedule, and make sure everything was covered. But it always was. I mean, they just did that all by themselves. So my job was to, you know, to keep the bureaucracy off their backs, and maybe dig up some money. [1:25:01] I did get some money for a new wing on Oceanography out of Congress. And I had a lot of fun working with the university administration too. They were a good group.

CP: Were you able to build up the research funding base as well?

BD: Well, the research funding base tends to grow, anyway. We were able to build it up in some areas, again, working with faculty. We were able to get grants from, oh, NASA and ONR for labs, computer labs in particular. They still do have a pretty big supercomputer set up right in Oceanography. And getting students access to that. It was important, but it just built up sort of naturally. You know, the job I had was, when we did hire people, to make sure we were hiring really good people, who could survive in that atmosphere.

And the interesting thing, in the seven years I was dean, we never lost anybody. I hired people from Woods Hole, and you know, lots of other big-name places, but we never lost anyone. We had one faculty member who was struggling with grants, and decided on his own to go back to Taiwan. But we never had anyone hire anyone away from us. And it wasn't because of the super-high salaries, but it turns out that it's a good group of people to work with. It's got a, you now, easily a critical mass. If you're in Oceanography, it's one of the better places in the country that you can be. And people like Corvallis. It's a good place to raise kids, if you're raising a family. And you know, if you're a woman, you want to walk around downtown at night, you're probably not going to get in any trouble. So I thought that was really interesting, too. And I suspect this is true in other departments, that Corvallis is just a really nice place to live.

CP: Were you able to pursue any research while you were dean?

BD: Oh, I played a little bit, but not—nothing significant. And they were projects that were ongoing. For a number of years, I had NASA grants to work on dating lunar rocks. This wasn't the thermoluminescent stuff I was talking about earlier, but. I forget when I started the project. It was maybe eight or ten years before I left the Survey, but we were looking for pure little amounts of melt rock. I was working with Graham Ryder at the Lunar Research Institute in Texas. Graham was a petrologist who was specializing in these melt rocks.

What a melt rock is, is when a big impacter hits the moon, you know, these giant, basin-forming things, it throws out a lot of debris that never melts, but it also melts a lot of the rock it hits. And some of that recrystallizes into new rocks. It cools slowly enough so it recrystallizes. And then other impacters come in and splash this stuff, you know, all over the moon's surface. And what we were trying to figure out is what the early impact history of the moon was. And there were two models for it. One is that as the moon was gathering up material from its own orbit, and material from Earth's orbit, that it started out; there were lots of impacts, and it got smaller, fewer and fewer, and smaller and smaller as time went on, until about 3.8 billion years ago, there was sort of a final cataclysm. There were lots of impacts in one big blast, and that it's been pretty mild ever since. The other model was that there weren't many impacts to begin with, but there was just this one huge bunch of impacters hit the moon 3.8 billion years ago.

So using these metal rocks, which came from these big impacts, if we can measure their ages, we can then determine when these big craters formed, when all these big impacts formed, you know, like embrium [?] and so forth. So Graham and I identified the melt rocks, and I had an argon-argon system that used a laser. We could actually work on little, little tiny mineral grains, or little tiny pieces of rock, down in the microgram range. Because these things weren't very big to begin with; you couldn't get very much of them. So we were doing this kind of dating. We did that for quite a while, wrote a number of papers on it. Turns out that we never found anything over about 3.8 billion years, so we think that probably there wasn't a huge bombardment to begin with, but we had this big cataclysm at 3.8.

Anyway, I still had some of that going on, and Bob Duncan, who was a professor of oceanography, a geologist, had a dating lab, so we were working on some of that. [1:30:06] And when I retired, I turned those grants over to him. Then I had another grant doing some modeling on the geyser geothermal field with a colleague at UCLA that I finished up while I was here. But that was actually a—

CP: Well, I have a couple of people I want to ask you about, some high-profile people associated with the college. This first one is John Byrne.

BD: Mm-hm.

CP: Your interactions with John?

BD: Well, he was President when I came. And, whereas John's a geologist, so we talked the same language, and John's just a really nice man. It'd be hard not to get along with John. And I still see him once a month for lunch. He was always supportive, and always cheerful. So I like John and his wife Shirley a lot, and Sharon and I still see him socially.

CP: How about Jane Lubchenco?

BD: Well, Jane I got to know here. She was on the faculty, and then not too long after I came, she was elected to the National Academy, which was a big deal. And I gave her—I had an extra little Academy rosette I gave her she could put on her lapel. But Jane's a real, just a remarkable talent, and she's indicative of a lot of the talent that comes to OSU, and you kind of wonder why. I mean, she and Bruce came here on a job-sharing thing, you know. OSU gets people like this. You know, what is it about this place that attracts the kind of—attracts the Jane Lubchenco's and the Bruce Menge's, in spite of the fact that the pay's not very high? And she was continually ranked by the magazines as like third, third-ranked university, which is kind of funny, because it's not based entirely on academics.

But I was always impressed by the faculty at OSU. I mean, they're really outstanding. I think they're as good as anywhere in the country that I've seen, across the board. I mean, liberal arts, chemistry, biology, whatever you care to name. They just have a really good—there's something about OSU and Corvallis that attracts people like Jane, you know, who if they haven't made it already, are going to. It's kind of a minor miracle, I think.

CP: Who else was important to you during your time as dean, as an ally or a colleague?

BD: Well, I think all of the deans at the time. As a group we worked together really well. None of the ones I worked with are still there, they've all retired since. But you know, we were always competing for funds and resources, but at the other time, on the other hand, we were supporting each other. And I remember once when the provost wasn't handing down a budget—the president wasn't handing down a budget to our liking, there were half a dozen of us got together, and under the leadership of Tim White, went back to his office, and we wrote the budget, [laughs] and sent it back in, and said, "This is better than yours." And they adopted it. Tim White is now the Chancellor of the California State University system. So, you know, Tim was great. Kay Schaeffer is terrific. Don Parker is still good friends. I see, a number of these people I still see, we get together at lunch, for lunch once a month. So the deans were important.

Of course, the provost is always important, because he's your boss. And Roy Arnold was the provost over most of it, and then Tim White became provost, and they were both outstanding to work with. I mean, the kind of people you could go and talk to, and explain your situation, and they would understand it, and if they could do something to help. And then George Keller, who was the vice—I think he was—I forget what his title was at the time. I think maybe he was associate provost, vice provost for research, or something. I'm not sure he was vice president yet. But for research, and he is the one who gave out matching grants.

George was extremely important, because if you wanted to get a big chunk of money for some facility, or for research out of ONR or NSF, they often expected—especially if it was for equipment—they expected you to put something in the pot too, even if it was only five or ten percent. Well, some of the oceanography grants are pretty big. I mean, they were in millions sometimes. So you had to go to big George for 50 or 100 thousand dollars, and George was always very receptive, and I think partly because he came out of Oceanography, and he realized how important that money was, not just to that research grant, but to the survival of the college.

CP: [1:35:01] Any memories of Paul Risser?

BD: Yeah, Paul was a pretty dynamic guy. He was always—Paul's problem, he was always spinning off new ideas and expecting the deans to go execute them, without any additional funding. [Laughs] And so he's one of these—and there people like this in science, too, but Paul was one of these people that's just always treating [?] out ideas. But he doesn't want to get involved in the details, because they're difficult. So you know, when he wanted OSU to get bigger into teaching over the internet, for example, it was, "Go do it. Just go do it." "Yeah, but this is going to require A, B, C, D." He didn't want to be bothered with that: "You just go do it." So sometimes he'd give you these just impossible tasks, and you couldn't do it. So the result was he got ignored a lot, not because people didn't like Paul, or they didn't think it was a good idea. It was just, you know, he always seemed to think that you should be able to do everything all at once, and that's not the way any organization works, you know.

CP: Well, in 2001, you decided to retire.

BD: Mm-hm.

CP: It just got to be time?

BD: Well, yeah. I was about getting close to retirement anyway, and then we had a budget situation. As the salaries went up in the college, it required the college to come up with an increasing large—larger percentage to fund them. And the university didn't always cover that, so eventually over the years, the salary part, the state salary part, eats into your discretionary budget, and all of the sudden you're faced with not having anything for anything else. And it's just the way the dynamics of the budget in Oceanography happen to work. So, then you have to play catch-up. You need a chunk of funds, so you can now go into this declining, you know, this declining salary mode again, or increasing salary mode again. And we were at the point where there was just no way to solve this problem.

So I thought, well, Tim White was the provost then, so I prepared a letter of resignation. And I don't bluff; I've never done that. I just, if I decide to retire, I was going to retire. But what I was able to tell Tim, I was able to explain the situation to him, and then I could tell him, I said, "Well, you can either deal with me, and we can get this solved. I've got to have some more resources. Or you can deal with whoever the next dean's going to be, and that's going to be a lot more difficult, because you're not going to be able to hire anyone until this problem's solved, at least for the next ten years." It tended to

be like a ten year cycle, I think. And so Tim understood that. [Laughs] Then they got a big chunk of money, and I left. But I was within six months of retiring, anyway.

CP: So, now the university is sort of in this expansionary phase. Do you have any thoughts on the current direction of the university, or the current direction of Oceanography, as you've observed it from afar?

BD: I really don't. I haven't been that observant, so. I think a lot of the expansion, I suspect, has to do with economic circumstances, and this happens to universities every time there's an economic downturn. You know, what do people do if they can't get a job? Go to school, improve yourself. So if the economy continues to improve as it has recently, although slowly, I suspect that the plans for a 35,000-student campus, you know, may not happen as quickly as possible. But the country's growing, so. When I was a kid, the population was a little over a hundred million, as I remember it correctly. Now it's what, 320, 330?

CP: Over three.

BD: Yeah, it's over three.

CP: In 2003 you received notice of having received the National Medal of Science.

BD: Mm-hm.

CP: Can you tell us your experience of that notification, and then the award being—?

BD: Can I interrupt for just a minute?

CP: Sure.

BD: [Clears throat] Oh, yeah, there it is, National Medal of Science. Name on the back. I was told it's made out of pewter; it's not gold, for sure, so. But anyway, it's kind of neat.

CP: So how did you find out about this?

BD: Well, I had an acquaintance, a friend, who worked in Washington, D.C., for a long, long time, and as staff members for senators, and congressmen, and the NSF, and so forth. [1:40:07] And she was working for, I think, the NSF at the time, and she called to let me know. However, we were out of town. We were over in Bend. We have a place over in Bend, so we were over in Bend and hadn't told anyone where we were going. Our kids knew, but. So she was frantically trying to get ahold of me, because they couldn't issue the press release until they'd notified all of the seven recipients of the medal for that year. So I got back; there was this frantic message from her. [Laughs] The first thing she did was chew me out for going somewhere and not letting, you know, people know, because they called the university, and nobody knew where I was. And so as soon as they told me, then they could issue the press releases. And I don't remember when that was. It was not very long before their presentation ceremony, you know, like a couple of months, I think. But it was nice to be told by someone that I knew and liked.

And interestingly enough, it's the 2003 medal, and I was given it in 2005. And the Bush White House was a little slow, I think. I was told it was because the FBI was busy investigating terror suspects and didn't have time to investigate all of the medal recipients for 2003. [Laughs] So they were a couple of years behind. They investigate everybody they are giving the medal to, for obvious reasons. They don't want to give it to somebody that will—that will cast a bad light on the White House, so that's the reason for the delay. The other thing I found amusing was to—you know, I mean, this is the top honor you can get from the U.S., and I found it interesting to get this 40 years after I had done the work that I was cited for. They never give it to young people, right? You have to wait until you get old to get really good things like this. But the main citation was for work I had done in the 1960s, reversal work.

CP: How was the ceremony?

BD: It was wonderful. Most of the stuff surrounding presentation of the medal is handled by a foundation that's funded —I think it's funded through the NSF. But I mean, they can have parties, and dinners, and so forth that you can't do as a

government agency. So they give these grants to the national medal—I think it's called the National Medal Foundation. They do the Medal of Science and the Medal of Technology. So it was great. We got to—you could invite just a few guests. I was able to get all three of our daughters in, and our oldest daughter's husband, plus Sharon. So we had tickets to the White House to do that. And the ceremony was great. They have all of these young military people, they're called interns, military interns, and they're all junior officers in all of the services. And they have to be on call 24 hours a day and they have to be single. But they act as hosts and hostesses in the White House. So they were—you know, guided us around.

We had the complete run of the main floor, you know, where the State Dining Room and the ceremonial rooms are, and then downstairs where the theater, and library, and so forth. We could go anywhere we want, sit on the furniture. [Laughs] And then President Bush came in and met with us for, oh, probably twenty minutes or half an hour, at the start of the ceremony. We went through a rehearsal. Again, these military interns showed us where to stand, and what to do. And then after the ceremony, we went back, and Bush—and the Secretary of Commerce also was there because the technology medals go through Commerce, I guess. Met with him for another twenty minutes.

The interesting thing, I thought, was, he had arguably some of the fairly decent scientists in the country, and technologists, in this room. Included people who developed the substrate for the catalytic converter that you use in automobiles and that kind of thing. [1:45:00] And President Bush never asked anybody a question. I thought that was revealing. And then they had a reception in the National Academy Building, where they also had the finalists for the Intel Science Challenge, high school students. They had, oh, I don't know, half a dozen or two dozen, maybe it was a couple of dozen, of the finalists there at the same time. And so we got to mingle with them, and vice-versa, and that was fun. And Bruce Alberts, who was president of the Academy at the time, asked me if I would talk for just a few minutes on how I got from high school to where I was now, which I did. And that was kind of neat.

And then they had this huge banquet—it was at the Ritz Carlton—with about 400 people. And we sat at—they had a table with one of the committee members. I don't know if you know how this medal is determined, but the National Science Board, which is a governing board for the National Science Foundation, appoints a committee to go through all of the applications and nominations, and they make a recommendation at the board, and the board makes a recommendation to the President. And there are about—I was told there are about 4 to 600 nominations a year, and they carry over about 200 a year from the previous year. So your odds of getting one of these dudes are pretty slim, amidst what I would guess is probably a pretty good field. We got to sit with one of the committee members and his wife.

And his wife was an officer in the Public Health Service who was detailed to the Army, so she had on her Army uniform, dress uniform, it looked really slick, I thought. But she found out that our youngest daughter, who was in the Coast Guard and still is, was at the time a lieutenant commander. She had the same rank as this Army officer, Public Health person, and Mindy was just wearing a dress. And this woman turned to her husband and just really gave him a glare, said, "You told me I had to wear my uniform." [Laughs] So, but it was fun having all of our kids there, and we were, again, escorted into the banquet individually, the medalists, by a high school student from one of the magnet schools, science schools, in Washington, D.C., and then introduced, and the dinner was wonderful.

CP: Well, as we start to close up here, I wonder if, as you look back on your career, you could share some of things you're most proud of having achieved?

BD: Well, I think the reversal time scale was a nice piece of work. It turned out much better than we could have anticipated. I thoroughly enjoyed measuring the ages, and studying the Hawaiian volcanoes in the Hawaiian-Emperor Chain; took place over a long time and got to work with some really neat people over that. And then I had a lot of fun writing a book on the age of the Earth. And I had a lot of fun working with moon rocks. And I enjoyed being at OSU; I enjoyed being a dean there. People ask me what I taught, and I tell them I didn't teach anything. I didn't know enough to teach, so they made me dean. But I enjoyed working with everybody there at the university, the faculty and the administration.

I have absolutely no regrets. We had thought, when we moved here, that after I retired we would move back to California or someplace else, but we really like Corvallis, and we just decided to stay. Now when I drive back to a place like the Bay Area, it just drives me nuts, traffic, you know? But no regrets. And I had a good time, and I had a good career that was helped by a lot of people. But I do wish they'd give these to some of the younger people.

CP: Well, thank you, Brent. I appreciate it.

BD: Yeah, you're welcome. [1:49:34]