



John Byrne Oral History Interviews, January 24, 2014

Title

“Building Oceanography at Oregon State and Moving Up the Ranks”

Date

January 24, 2014

Location

Center for the Humanities, Oregon State University.

Summary

In interview 2, Byrne reflects upon his first professional job with Humble Oil in Houston, Texas and his decision to accept a position at Oregon State College in 1960. From there, Byrne recounts his impressions of life in Corvallis during the 1960s as well as the early years of the Oceanography Department, led by Wayne Burt. Byrne also discusses the development of his research program, the department's first research vessels, the creation of the Hatfield Marine Science Center and campus life in the 1960s. Byrne then notes his first experience of living in Washington, D.C., where he worked for a year as a National Science Foundation program manager in oceanography. The interview continues with thoughts on the growth of the OSU oceanography department into a school, which Byrne administered as Dean and, for five years, as Director of the Hatfield Center. The session concludes with remarks on Byrne's movement into upper administration, first as Dean of Research and then as Vice President of Research and Graduate Studies.

Interviewee

John Byrne

Interviewer

Chris Petersen

Website

<http://scarc.library.oregonstate.edu/oh150/byrne/>

Transcript

Chris Petersen: All right John, as with yesterday, if you could give us your name and today's date?

John Byrne: Yeah, I'm John Byrne, President Emeritus of Oregon State University. Today is January 24th, 2014. And this is the second version, I guess, of the oral history that we started yesterday.

CP: So, today we'll talk a lot about what you describe as the ocean years at OSU and at NOAA. Before doing that, though, we'll touch on a couple of topics that we didn't get into much detail yesterday, related to SC years. The first thing I want to ask you about is the field camps in Ely, Nevada, that you were in charge of?

JB: Yeah, well I wasn't in charge of it. I was the assistant to the faculty member. This was the Geology field camp, and they set up at a ranch house just outside of Ely. Ely is a copper mining town in eastern Nevada. And I think that the University of Southern California had been doing their geology field camp there for some time. And it was about a—I guess it was about a six-weeks camp, something like that. And I agreed to go out and help a man named John Mann, who was the faculty member at the time.

We camped outside, outside the house, and each a day team of two students would go out together and map the geology of the area. And Professor Mann and I would sort of circulate among the groups, and provide whatever help we could. We did get a chance to visit the open-pit mine where they were getting the copper ore, and the smelter, and so on and so forth, so. It was an interesting summer. I think it was the summer of—it must have been the summer of '54, maybe '53. I'm not sure.

CP: Another thing I wanted to ask you about was, you mentioned in your memoir about—it took you a while to prepare the illustrations for your dissertation. I'm interested in knowing more about the process of creating those scientific illustrations. You mentioned you had an artistic—

JB: Well, in those days—of course things are different today, but in those days, we drafted our own ink on what turned out to be linen, our own illustrations for the dissertation, and so on. And there were a lot of tools that you could use to help you do that. But what I would do is I would do the illustrations, and then write the dissertation, the text for the dissertation that went with the maps, the charts, and so on and so forth. And normally, when I was actually writing, I would write during the week, and then do the illustrations if I could on the weekend, for the coming week, you know. And I had it pretty well organized.

CP: Mm-hm.

JB: So, it worked fine.

CP: We learned a little bit about Shirley's background last time. I wonder if you could tell us how you two met?

JB: Oh, boy.

CP: [Laughs]

JB: Yeah, this again is another popular story with certain people. Well, as I told you yesterday, I didn't know anybody socially, really, which meant girls. And after I had been there a semester, I moved into a very small apartment that was about three or four blocks off campus. And each morning I'd get up and drive my car early, to get a parking place close to the Hancock Foundation building, which was where Oceanography and my office was, and park there, and wait until the cafeteria opened, and then go and have breakfast. And the place I parked was about halfway between a women's dormitory—in those days, they were just women or just men—between the dormitory and the cafeteria.

And I'd sit there, I noticed this attractive young lady walking toward the cafeteria several mornings, and I thought, "Well, it would be nice to meet her." And so I, on some occasion, I pointed her out to my Geology colleagues. And one day we were having lunch in the student union, in a place called The Grill. And it was a place where you walked down a few steps, and there were booths right after the bottom of the steps, and so on. And so there were probably four to six of us in a booth, at the bottom of the steps. And she came down, and this student, I think he was an undergraduate at the time,

sitting next to me, nudged me. [0:05:00] He says, "Is that her?" referring to this young lady that I had pointed out to him. And I said, "Yeah." And then he said to me, "I dare you to meet her." [Laughs]

And so, it was a dare; what are you going to do? And so she went and got her meal, and then sat at a table on the other side of the Grill. And so I got up, and I walked up to the table, and I stopped in front of it, and said to her, "I can't think of any subtle way of meeting you. Do you mind if I try a direct approach?" And she allowed me to sit down, and an hour or two later, why, after having described all sorts of things, that was how we met. So in a sense, in the old jargon, I picked her up. [Laughs] And we will have been married 60 years next year, or at the end of this year.

CP: You guys decided to get married pretty quickly?

JB: Well, that was in 1953, in the summer, in the early part of the summer. She was out at SC to get a degree in music. She had just completed her bachelor's degree at Whittier College, and so she'd come to SC to work on a master's degree in music. And we got married Thanksgiving of the following year. So I had known her for, oh, probably 14 months, something like that. Yeah.

CP: What do you remember about your wedding?

JB: Not much. It was in a small church. I couldn't even remember the town's name; it's probably in the memoir that I wrote. But it was a small church, and very modest. My team consisted of a best man and two ushers. I went to the extent of we didn't rent suits or any of that sort of thing. I just asked these guys to wear a blue suit, and I bought neckties for them. Yeah.

CP: Well, your first job out of school was at Humble Oil and Refining in Houston.

JB: Yeah.

CP: And that began in 1957. What was the connection there? How did that happen?

JB: Well first of all, I had met the director, a man named Harold Fisk, the director of research section, while I was involved with this project in the Bahamas. And then Humble had supported me to some extent as a graduate student while I was at SC. And then there was a connection, and so when I was just about finished with my dissertation, we both arranged for me to come back to Houston to be interviewed, and so on. And I did that, and I took all of these illustrations that I had made for my dissertation. And I remember Fisk saying to me, "Well, if we don't hire you as a geologist, maybe we can hire you as a draftsman." But in any case, that's where I went to work for Humble, in the summer of 1957.

CP: And how did you find Houston?

JB: Well, it was hot. I think the thing that—and very humid. I think the thing that probably bothered us more than anything else was how flat that part of Texas is, which is, the geography was boring. And then of course, when we decided we wanted to go to the beach, we'd drive down to Galveston, and it was flat all of the way. And you look forward to Buffalo Bayou, I think it was, because there were trees, and it was just a little break in the landscape. The other disappointment was that once you got to the beach in Galveston, again, it was very, very flat, you know. And you could walk off shore a quarter of a mile before you got to any waves of any sort. And the water was tepid. You know, it was not terribly refreshing.

CP: What did you observe about race relations at that point in time?

JB: Well, this was 1957, and at that time, in the apartment that we lived in—my first office was down in downtown Houston, and so I'd take a bus down to Westheimer Boulevard to the office, the Humble Building. And at that time, African Americans, blacks, Negroes, whatever you might want to call them [0:10:00], were urged to sit at the back of the bus, and Caucasians would sit at the front of the bus. Well frequently, there were no seats in the front of the bus, but there were seats in the back, and I used to go and sit in the back. I don't know how that was received, but it was that sort of thing. When we did fieldwork, and we did a lot of fieldwork on the Mississippi Delta, we'd get to these areas where there was clearly a separation. There were restrooms for the blacks, and there restrooms for the whites. So it was sort of toward the end of that period of racial discrimination.

CP: It was more pronounced in the rural areas than in the city?

JB: Yes, I think so. Yeah.

CP: What was your job at Humble Oil? What did you do?

JB: Well, the research section was set up to do two things. It was to do special problems for the operating part of the company, and we called it company problems. And somebody who was a driller, or a geologist, on a rig would get some cores, and it was important to know what the environmental deposition of the sedimentary rocks were. And I won't go into the whole geology of oil accumulation, and so on, but it was important to know that. So they sent us the cores, and we would analyze them, and using a comparative situation with modern sediments.

We'd go out, for example, go out to a beach, and study the beach, find the subtle things that are unique to beach sands, and then look for those kinds of things in sandstones. And if you found them, and you'd say, "Yeah, these things, it really looks like a beach deposit," then you send that information back to the rig. And they could use that in their exploration, subsequent exploration for petroleum. So, we did that. And because some of my colleagues spent a lot of time with Harold Fisk on the Mississippi Delta, there was a tendency, I think, to see the similarities with things that were happening on the Mississippi Delta, which, in some cases, were not true, were not right. And one example we had—we had sections of sandstone, and then small sections of shale, and, in looking at them, one of my colleagues, who spent a lot of time in the Mississippi Delta, said, "Well, these are just like a certain type of deposit on the Mississippi Delta."

Well, we had a micropaleontologist with us, and the micropaleontologist took samples of the shales primarily, because there were very small fossils, foraminifera, that would actually indicate, in modern counterparts, where they lived, deep water, shallow water, and so on. And so anyway, we had this all figured out; this was shallow water deposit. And the micropaleontologist took his samples then went back to his lab and analyzed the samples and came back to us a week or so later, and said, "Well, your shallow water deposit occurred at a depth of 5,000–6,000 feet," based on the fossils that were in the shale, you see? So we weren't always right.

We were learning. We were learning. The second kind of thing we did was the studies of modern sediments, and that was, again, as I pointed out to you, to discover what the subtle differences were between deposits, so you could apply it to the ancient situation.

CP: You mentioned the Mississippi Delta. That was an opportunity to be exposed to the Cajun culture.

JB: Yeah, yeah, and I enjoyed it. I think the Mississippi Delta in those days was the most interesting flat country in the world. And it's very complex. And the problem is that as you control the sediments in the Mississippi River, which was distributing sediments to the delta in the past, and as you shut that distribution off [0:15:00], the delta begins to disappear. And I don't know what the estimate is, 40 square miles a year, or something like that. And the interesting thing is that it's not just the delta that's disappearing, it's that whole Cajun culture that's disappearing as well.

CP: Well, during this period of time, you also became a father. Your daughter Donna was born in 1958, and then two years later you had twin daughters.

JB: That's right.

CP: That surely made a big impact on you?

JB: Well, it did. You know, it kept you busy. Twins keep you busy. You feed them both at the same time, whether they're both hungry or not. You change them both at the same time, whether they both need to be changed or not, you know. Yeah, they took up any available time, there's no question about that. And then when the twins were, gosh, I don't know how old they were, a year or less than a year, we came to Corvallis.

CP: Was Shirley managing these three children by herself, or did you have any help?

JB: Pretty much. Well, she had some help, I think. She had some help with the house. A black woman named Zollie, that would come in and help with the kids, and so on. Yeah, yeah, it worked fine. And Houston was equipped pretty well. We

had a diaper service, and it was excellent. Diapers came back folded, and so on. And this of course was in the days before you had throwaway diapers. So they had to be laundered at least once a week. And when we moved to Corvallis, there was no diaper service. It was a change in lifestyle.

CP: Yeah. What was your first contact with Oregon State College?

JB: Well, when I went to work for Humble, when I went to work for the oil company, I told them that I wanted to work from three to five years, and then I wanted to find a teaching position, because I really wanted to teach. So at three years I started looking around. And one of my colleagues, one of my SC graduate student colleagues, said, "You know, they just started a new Oceanography department at Oregon State, and I submitted your name as a possible geologist in Oceanography." And in those days, oceanography was divided into four categories, physical, chemical, biological and geological. And so if they were going to have a full Oceanography department, you've got to have somebody from each of those areas.

Well, they were looking for a geologist. Wayne Burt had started the program in 19—actually, 1954, but it became a department in 1959. So in 1960, he was looking to begin to add staff, and so on. And they invited me to come. He said, "Well, you've been nominated. Are you interested?" Sure, I'm interested. I was looking for a teaching position. So I sent them my resume and so on, and they invited me for a job. And in fact, as I recall, Wayne said, "You don't even have to come. We just want you on our staff."

Well, I wasn't going to take the job without coming, without going to see what was there. So I managed to get here, and in those days there was a commercial airline flight from Portland to Corvallis, and so I flew down here, and Wayne picked me up. My first reaction, first recollection, was Marys Peak. It was a clear day; it was just like today. It was a beautiful day. And I could see Marys Peak, and it was very attractive. Wayne was driving a little blue Simca and picked me up. We got in the Simca, and it smelled moldy to me. And that told me something about the climate here, you see.

Well anyway, we got here and I spent several days, and there of course was nothing here. Oceanography had its office in the Food Technology building, second floor, back of the Food Technology building. Because I was a geologist, there was a graduate, geology graduate student, Vern Kulm, who subsequently became a faculty member in Oceanography. Vern was my tour guide, and took me down to the Geology Department, introduced me down there, and so on. And the only thing that was here at that time in Oceanography was promise. There was an opportunity, and it was an opportunity to build, and so on. [0:20:03] Well, Wayne offered me a job before I left, before I left Oregon State campus. And so I thought about it on the way back. And my job at Humble had evolved. I had been there three years; I was kind of a senior guy in the section we were in on sedimentation, and so on. And it was a hard choice. And so I told Wayne, "Well, thank you very much, but I think I'll stay with Humble." So I turned the job down.

About six weeks later, I got a letter from Wayne, and he's saying, "We've checked with your major professor. We've checked all around. We can't find anybody that we want more than we want you. Tell us what it will take to get you to Corvallis." Well, you know what? After I turned them down, within days, I realized I had made a mistake. You know, this is what I wanted to do, and here I turned down this thing, simply, maybe because it was nothing there, nothing here at Oregon State. But anyway, he says, "You tell us what it will take for you to come. We're offering you a position as an associate professor." The salary didn't make much difference; it was about what I was getting at Humble. It was around \$9,000 or maybe a little bit more. And in those days, that was—you know, that was reasonable in those days. And I would have come for less. I knew I had made a mistake. You don't often get two shots at this type of thing. And so I came out here. I started here sometime around July 1st, in 1960.

CP: So, how would you characterize Oregon State at that point?

JB: It was small. It was Oregon State College. Corvallis was small. I don't remember what the population was, but no one could tell me if it counted the students or not. They didn't really know. But it didn't make much difference. It was a small town; it was a nice little college. We didn't know at that point—well, we didn't know at that point what we were really getting into, in terms of life support and those kinds of things. Although \$9,000 seemed fine, it created a little bit of financial pressure for a family of, at that time, five. So anyway, I knew I was coming to Corvallis.

The next job was to tell Harold Fisk that I was leaving, because I had told him that I had turned the job down the first time. Fisk, you know, I have to count him as one of my major mentors, but he was volatile. And I waited until a Friday afternoon, figuring there was no point in ruining everybody's day, you know. I went in there at 5 o'clock, and I told him that I was going to come to Oregon State. Well, I think Fisk had actually been a graduate of the University of Oregon. And so his first reaction was, "Well, that institution will never amount to anything," referring to Oregon State. Well, I didn't care. I was coming for Oceanography, to teach. And so he hit the ceiling. And okay, so we lived close enough—by this time, my office was out on Buffalo Speedway, which in those days was out in the country. It's part of downtown Houston now. And so we lived oh, maybe a mile or so from the lab. So I walked home, and by the time I got home, Shirley greeted me, and she said, "Doctor Fisk wants you to call immediately." So okay, I called Fisk immediately. And he said, "I want you to come and see me tomorrow morning," which was Saturday morning. And, "Don't do anything rash." Well you know, what am I doing rash? I've already done it.

When I had told him, he called up the chief geologist of the company, and the chief geologist said, "Is Byrne—has he been working on" this particular project that we had? And Fisk said, "Yes." And he says, "Get his resignation immediately." So, essentially, I was without a job from 5 o'clock or thereabouts on, on Friday afternoon. [0:25:05] So Saturday morning, I went in to see him, and he had assembled his team, his deputy, and also the person that I reported to in the sediments section. And Fisk said to them, "John has told me he is going to leave us and take a teaching job at Oregon State College, in Oceanography." And their reaction was one of shock, or at least it looked like they were shocked. And Ray Wood, who was his deputy, said, "I think he's crazy."

And then Fisk jumped in. Fisk had been at LSU, Louisiana State University, for a long time, and had in fact become the Chairman of the Geology Department. And so when Ray Wood made this statement, "I think he's crazy," Fisk said, "Well, I don't think he is." He said, "Some of the best years of my life were teaching," and so on. So, and I had a very close relationship with Fisk. We worked together on papers and this sort of thing. And he said, "Well, we'll have a coffee reception Monday morning, and we'll tell everybody that John is leaving, to leave on good terms," and this kind of thing. And he did. He gave me a pair of agate cufflinks, you know, which I still have. But the years at Humble were good years for me. They were good people, they worked hard. I suppose my attitude toward the petroleum industry is shaped by that. I don't think they're a bunch of bandits, I think that—anyway.

CP: So you came to Corvallis, and you started to get settled in?

JB: We came to Corvallis. I met my father in Dallas, and we drove out again, a second time, across the country. And we got here on a Saturday morning, stopped, got a place in a motel, had lunch probably in Wagner's, which was in the center of downtown. It's no longer there. And then looked for a real estate place. And we did, we found one, and we were informed that it wasn't the "Will-uh-met" River, it was the "Willamette, dammit."

CP: [Laughs]

JB: And that this little town west of Corvallis was not "Philomáth," it was "Philómáth," and all that sort of thing. And they showed me three houses. The first house they showed me was a very nice house on 12th Street, about a half a block from Garfield School. And we saw it, and my father said, "This is the one you should take. Take it, take it." I said, "Well, I want to see the other two." We looked at them, and there was no real comparison. And so I said to the real estate agent, "We'd like to rent this house." And what I told him was we wanted to rent for a year or so, until we could get settled, and then buy a house. Well, it turned out the house was really for sale, but the owner agreed we'd lease it for a year, so we were committed for a year to that house. And he agreed that if we decided to buy it, the lease would account for 85 percent or so of the down payment. And so it was an ideal situation. We couldn't have made the down payment without that, you know. But anyway, it was fun. We lived in that house for 21 years.

CP: What did your dad think of your decision to—?

JB: Say it again?

CP: What did your dad think of your decision to leave Humble Oil and come to OSC?

JB: Oh, I think my folks realized I wanted to teach, and that's a worthy profession, and so they felt good about it. It all worked; it was all, it was kind of like it was programmed. It worked fine. And good experience at Humble, and certainly a good experience at Oregon State.

CP: So, I want to ask you about a couple of people from that time period. The first is Wayne Burt, if you can tell me a little bit more about him as a person?

JB: Wayne. Wayne—interesting guy. He had grown up in Oregon. He went through the war, became a meteorologist during World War II, then subsequent to the war went to Scripps, got his PhD at Scripps Oceanographic Institution. [0:30:00] Then I think his first job was at Hopkins—not Hopkins—Johns Hopkins Marine Laboratory. But his heart was in the Pacific Northwest. So he came back to the University of Washington and spent a year or so there, simply working up data that they had already accumulated and had never published on. And Wayne was the kind of guy who was—he was a very emotional person, charging hard all the time.

There were certain things he liked to do, certain things he didn't like to do. He didn't like to make public presentations. He didn't like to stand up and give speeches. But once you achieve something, use that as the base for the next step. And you could see that. He went to university—apparently had a master's degree in mathematics from the University of Oregon. So he went back down there and said he'd like to start an Oceanography program at the university. Well, they weren't interested. So then he came up here, and he encountered Francois Gilfillan, who was the Dean of Science at the time, and Gilfillan—and Wayne, you know, he had managed through his colleagues from Scripps to get the Office of Naval Research, which at that time was the funding agency for research, to give him a small grant, so that he could study the estuaries along the Oregon coast.

And he told Gilfillan that he could do this if he had a position; he needed a position at a university to continue to do that. Gilfillan says, "Fine. If you can get the money, we'll give you a position in the Department of General Science." That's all Wayne needed. And he continued to get bigger and bigger grants. I don't know whether he was teaching anything or not, at the time. Bought a small, little fiberglass boat that he used in the estuaries, little boat called the *Bell-Boy*, and that thing was around for forever and ever. But Wayne kept pushing. And again, through colleagues that he had known—you know, oceanography was not a big community at that time, and so you knew most of the oceanographers in the country.

Well, there were two guys at ONR that Wayne knew, Gordon Lew, I think, and Art Maxwell. And he got an oral commitment from them, not in writing, but an oral commitment that said, "If Oregon State will recognize formally the Oceanography program," create a department, in other words, "we'll give you some money to operate and to get a boat." And what they had in mind at the time was a 65-foot boat, really. But this was an example of Wayne. He stretched that 65 feet into 80 feet, and that was the *Acona*. And went to Gilfillan and A. L. Strand, who was President, convinced them that, yeah, he could, with this ONR commitment, which was oral, he could do it. And they bought it, took it to the board of higher education, they approved it. 1959, the Department of Oceanography was created. And I think it was 1961 the *Acona* was launched, you know. And it was a deal, and both sides kept their part of the deal, and that was the start of Oceanography at Oregon State.

CP: Did you have much contact with Gilfillan?

JB: Some, not much. Not much. Francois Gilfillan was a renaissance man. He collected ancient silver; he spoke one of the Indian dialects, and so on. You know, I think he was a pharmacist by training. But he had the right attitude. And in fact, as you learn about Oregon State University, particularly in the early days, there was a lot of flexibility, a lot of flexibility in human relations, and so on. Linus Pauling. Linus Pauling comes to Oregon State without a high school degree. Yeah, we'll accept it; if you can make it, fine. [0:35:01] And that seemed to be the attitude. Faculty—yeah, okay, we'll give you a chance. And he did. And if the chance didn't work, okay, good-bye. But a lot of universities wouldn't have done that. So it's to Oregon State's credit, and people would say, well, it was not quite a university in those days, and it wasn't. It was an agricultural college with some other stuff.

One of the other stuff was a new Department of Oceanography. When we got here in the summer of 1960, oceanography was a hot ticket. I mean, you went downtown and, well, I had just moved to town and needed telephone service, and so, "Well, are you at the college?" "Yeah, I work at the college. I work in the new Oceanography Department." "Oh, really?" You know, oceanography was second only to space travel! It was hot stuff.

CP: So was it just you and Burt, or were there other faculty?

JB: No, no. In 1959, Wayne convinced Herb Frolander, who was on the faculty at the University of Washington, to come down. He also had a graduate student named Bruce McAlister, who was working on a PhD, and he got him to come down also. They had enough money that he could hire a secretary, and a technician, a guy named Bruce Wyatt, and that was it. The summer he hired me, he also hired Bill Percy and June Pattillo, who was a physical oceanographer at Scripps. And she had a colleague that she lived with, Beth Strong, who was not a scientist, but was, oh, I think, proficient in financial matters and that sort of thing. And so that summer, I arrive before Percy and Pattillo and Strong. But they came that summer also. And so that was the beginning of developing the faculty in Oceanography.

CP: And what were the responsibilities of your first position?

JB: Well, the position was half teaching, half research. And so, I was expected to teach two quarters, and to do research two quarters, the research part being supported by the Office of Naval Research. The teaching was two-fold—one, this was what I wanted to do, and I taught winter and spring quarters, and I did that all the time I was in Oceanography. The winter quarter was one course for oceanographers who were geology specialists, so the marine geologists, in a sense. Spring quarter, I taught the second course for them, I taught the undergraduate freshman survey course, which I'll tell you about in a minute, and I taught a course in, "Geological Oceanography" for all of the oceanography students.

And the requirement at that time in Oceanography was that you take, I think two, maybe three courses in physical oceanography, and then a course in biological, a course in chemical, a course in geological oceanography. And I taught the geological oceanography one. The undergraduate course was a course that was taught at the junior level. It could have been taught at the freshman level. It was taught at the junior level because the department got more credit at an upper division than it did at a lower division. Wayne somehow had worked that out. Well, Herb Frolander taught that course, and he taught it winter and spring, or fall and winter. And so, I asked Herb, "Can I sit in your course?" And so I did, and took lots of notes, and so on. And so the course that I offered subsequent to Herb was essentially Herb's course, with the Byrne twist on it, so. And that evolved over the years.

And I enjoyed it. We would get in those days roughly 100 students in the class, whereas the graduate courses were—the geologic course, I might have six students. The geological oceanography course, the one for all of our oceanographers [0:40:01], as our enrollment went up, I would get maybe 30 students in that class. But I enjoyed it. And I signed up for an honors colloquium, taught that with the chairman of the General Science Department, a guy named Don Humphrey, and for the geology students, both in geology and in oceanography, we did a luncheon seminar on terrestrial deposits. Not the stuff I was teaching in Oceanography, but because I had come from Humble, and had worked with Fisk, who was an expert on the Mississippi River and the Mississippi Delta, I was the logical guy to talk about river deposits, and that sort of thing.

CP: Was your research based mostly based out of Newport?

JB: No. No. Until we had a ship, it was coastal stuff. It was trying to find out where the sediments offshore were coming from, so that was in the estuaries. When we had the ship, yeah, then we went off the shore and started looking at the continental shelf.

CP: What was Newport like back then?

JB: Well, it was different. Newport was much smaller. It had the old Abbey Hotel. It still had, instead of a road along the bay, a boardwalk. And so, as it is today, basically, it was the bay front, and, oh, the other part of town. I can't think of it right now. I'm having a senior moment.

CP: Nye Beach?

JB: Nye Beach. Nye Beach. And Nye Beach, not much there, but there was a big swimming pool at Nye Beach—no longer there. And of course, Nye Beach had been the attraction in the early 1900s, and so on. People would take the train—once there was a train—take the train across to Toledo, and then a stagecoach from Toledo to Newport. Yeah.

CP: So the *Acona* was a game-changer for the department, obviously?

JB: Oh, yeah. Yeah. And it was a terrible ship.

CP: [Laughs]

JB: It rolled, you know. The only time I've ever been seasick was on the *Acona*. It rolled from the vertical, on one occasion—they have a thing that measured it—60 degrees from the vertical. Guys were popping out—they were below deck, they were popping out like mad, you know. I wasn't on it at that time. But it rolled. It was like one of these dolls that you push and it rolls back, one of those things. And they did everything to stop the roll, but they couldn't do it. It was the way it was built.

CP: How long before the *Yaquina* came was that?

JB: Oh gosh, the *Yaquina*. When we got the *Acona*, I think it was around '61. The *Yaquina* must have come '63 or '64. The *Yaquina* was a converted World War II vessel. It was called an FS, which is freighter small. It was used as a supply ship in World War II. *Pueblo* was a converted FS. And the FS, that ship, which was about 180 feet long, was a very popular ship to convert to a research vessel. And so Rhode Island had one, we had one, Texas A & M had one, Scripps had one, which they had all converted to research vessels. And our conversion was a good conversion. You find that the oceanographers of the period we had the *Yaquina* loved the *Yaquina*. And when it was time to get a new ship, they weren't sure they wanted a new ship. You know? It was an old ship by then; it was necessary to replace it.

CP: So what path did your research take then?

JB: Well, as graduate—graduate students are the ones who do the research, and so as we were acquiring graduate students, we essentially looked at the sediments, primarily, off of the Oregon coast, continental shelf, continental slope. There is a major canyon that flows, a submarine canyon, off of the mouth of the Columbia River. That, there was a fan at the bottom of that. So the graduate students were studying the sediments, and so on. [0:45:00] We had done the studies of the estuaries to see which rivers were contributing which sediments, and so on. So, it was mostly sedimentation that we were focusing on. In those days, things were not very sophisticated, and there was no satellite locations; there was no global positioning system, or anything like that, and so it was pretty crude by today's standards, but it was basic, you know, and that was what we were doing.

Most of the research was descriptive. It was: what's out there? How did it get there? Has it changed? We just got one thing that we discovered, for example, was, several of us wrote a paper on what we call continental accretion, that the rocks that made up one of the banks on the continental shelf had actually been formed a certain geological period before, in water much deeper. And we could do that by looking at the fossils, the live animals down deep, and the fossils in the rocks that made up the thing. And we had said, "Well, we've had continental accretion here of so many miles of rock, since a certain period of time." So we were doing that sort of research as well.

CP: Did you ever experience particularly bad weather, or anything particularly frightening when you were out at sea?

JB: No, I never did, but others did. After we had the *Yaquina*, the *Yaquina* was operating down off Mexico, and it was in an area where there was not good communication. It was at a time where we didn't have the kind of coverage we have with satellites, and so on, and there was a hurricane, called a typhoon in the Pacific, I guess. And Howard Lindsay was the captain of the *Yaquina*. Didn't know where the hurricane was, just knew it was somewhere there, and they made a right turn when they should have made a left turn, and got caught right in the eye of the hurricane. And that scared people, particularly when they lost the electricity on the ship, you know, and all of the lights went out. Frightening, yeah.

CP: Aside from Burt, who were some of your other close colleagues at the time?

JB: Well, the early oceanographers, the ones I call the pioneers, Bill Percy certainly came at the same time I did. Larry Small came the year later. Herb Curl came at the same time Larry did, and they both focused on phytoplankton, floating plants. Let's see, and Drew Carey came. He was more interested in the benthic animals, the animals that lived on the bottom. Physical oceanographers, June Pattillo was the key. She had a couple of graduate students, one of whom, Bob Smith, became a faculty member at Oregon State. Ricardo Pytkowitz came as a chemist. Kilho Park came as a chemist. We hired Vern Kulm, we hired a man named Jerry Fowler as geologists. And so the whole staff was kind of—the pioneers are the ones that came in those early days, and stayed for their whole careers. That's my identification of a pioneer. They

built the department. And yeah, Small, Percy, me, chemists, Pytkowitz until he passed on, June Pattillo certainly until she passed on, yeah.

CP: Was there any sort of shared social life? Was there a hangout—?

JB: Oh, the social life for the oceanographers, it was a small family. And so the Burts would have parties at their house, or, they had a little farm out on Peavy Road, we'd all go out there. And so, yeah, it was a tight group, a tight community. Following a PhD oral or something like that, the graduate students would head downtown. I suppose the Peacock was a spot that's still there. [0:50:02] But there were others that have closed up since then, and so on. But yeah, there was a regional social life. And of course, being at a university, all sorts of cultural things that were going on. And you certainly quickly discovered that you couldn't keep up with all of the opportunities: classic movie series, chamber music series, symphony; you know, it was just on, and on, and on. And you were busy with your family, too.

CP: Another recreational pursuit that you were involved in was that you got your pilot's license.

JB: I got a pilot's license, yeah. And then Shirley was going to get one of these co-pilot things from, you know, the spouse—if something happens to the husband, the spouse can land the airplane, sort of thing. She got her license in a shorter period of time than I did. But yeah, and we used it, and I'm glad we survived. But one of the things that we did was we used the airplane that we could rent out at Corvallis Airport; we used it for field trips. And we had one of the geology crowd, we split them up. Some of them flew out with me to the coast, and some of them drove, and then we switched them up the coast, and we would see the coastal land forms on the ground, and also from the air. It was kind of neat.

CP: Was this something you had wanted to do for a while?

JB: Yeah, I probably always wanted to fly, you know, so I got it out of my system.

CP: The family did a lot of skiing too?

JB: Yes, after we had come back from Washington. We haven't gotten to the Washington scene yet, but when the kids got big enough to ski, that took all of the extra money that we had, and the extra time that we had on weekends, you know. Yeah.

CP: What do you remember about the Columbus Day storm in 1962?

JB: Columbus Day storm, the oceanographers were at that time located in what is now Whittaker Hall called the Physics Chemistry, in those days. And as I recall—there's still a parking lot just to the east of Whittaker. I had driven to campus, had my car parked there, and it was a station wagon, it was a Chevy station wagon. And Columbus Day storm hit, and stuff was falling down all over campus. Looked out the window, down on the parking lot, and my car was covered with a tree. You could barely see it, just the back end of the station wagon, and there was this huge tree on top. But turns out, it made one dent in it. It was one of these things where the branches sort of straddled the car.

So, we managed to get out, and people started going home, you know? And our house, why, we lost electricity and that sort of thing, but it wasn't too long. The patio furniture flying across the lawn, that sort of thing. We survived, but not everybody did. It blew down barns, old barns, and things like this. It blew down a lot of trees. It made a big difference on campus. There were some nice, big trees on the quadrangle, at Memorial Union, and so they went down. It was destructive.

CP: Well, the department certainly began to expand throughout the 1960s, and one of the real milestones was the Hatfield Marine Science Center in 1964.

JB: Yeah, that was an interesting episode in the evolution here. We in Oceanography had been looking for a place to tie up the *Acona*. Until we had that, we paid money to the city of Newport, the Port of Newport, and tied it up at the municipal dock. But Wayne was eager to get a special facility. Had a National Science Foundation site panel come out [0:55:02], and you know, as I told you, you give Wayne an inch and he wanted a foot, you give him a foot and he wanted a yard, and so on. Well, we got the site panel here, and they proposed to them two things. They proposed to them a facility at the coast to operate the ship, and also a building on campus.

And a panel came down; we had a nice visit in Newport. They finally decided: no, you can't have the ship facility, but you can have the building on campus. And that is what is now Burt 1, Burt Hall 1. And so we got that, and about that time, shortly after being turned down by NSF, Hatfield was Governor, and he called, or somehow communicated with Roy Llewellyn. Roy Llewellyn was the Chancellor of the state system of higher education. And Roy Llewellyn sent a message out to, at that case, both Jim Jensen, who was the President of Oregon State, then whoever the president was at the University of Oregon, "Is there anything that you folks could be doing in Lincoln County to stimulate the economy?"

Now, what happened was there was a federal organization called the Area Redevelopment Administration. And ARA, as it was referred to, was set up to help depressed areas recover. Well, Georgia Pacific had a major plant in Toledo, which is in Lincoln County, and at that time, for whatever reason, economic, they had laid off roughly half of their employees. So, it immediately made Lincoln County the most depressed area, the most depressed county, in Oregon. Hatfield gets this indication that maybe there's some money to help Lincoln County, and sends the message out: well, sure, why don't we build a marine science center? Which, we could employ people, and we'll use it, with an aquarium and a museum, to attract tourists to boost the economy.

And that was the idea behind what was called the Marine Science Center. The center was located on a—I don't want to call it a spit; that gives you the wrong impression. But this area of sand on the south side of Yaquina Bay, from which ferries used to operate before the bridges were built. And that was the point that was picked, and a proposal was put together; I think it took them a few weeks. You know, it was a campus proposal. Wayne was involved, and Tom Scott, head of Fish and Wildlife, might have been involved. Fred Burgess was in engineering, he was involved.

They put together a proposal, and the Area Redevelopment Administration said, "Yeah, we'll give you 700,000," I think it was \$700,000, "to build this facility." Well, it included the original Marine Science Building, and a building and dock to operate the ship. And so Wayne got what he wanted for the ship. The ARA, and I can't substantiate this, but I'm told, or have been told, that it ended up building two buildings. It built the Marine Science Center in Oregon, and it built a mining museum in Butte, Montana. And then ARA disappeared. Whatever the federal action that was necessary to eliminate a department, it was eliminated, and that was it.

Well, the Marine Science Center was built, and this was around '64 or '65. We discovered that, or we reasoned that, if you could keep people at the Marine Science Center until 2:00 in the afternoon, they couldn't reasonably escape from Oregon that day, and that was more money for the Oregon economy. So all these Californians coming up the coast, once we could get them to stay at the Marine Science—so we started doing lectures and movies, and all sorts of stuff, to keep them at the Marine Science Center. Yeah.

CP: Another milestone from this period of time was the *Cayuse*, the RV *Cayuse*? [1:00:01]

JB: The *Cayuse* was built, I guess it was about 1967. It was christened in 1968, and it was about the size of the *Acona*, at 80 feet. It was a small coastal vessel, much better designed than the *Acona* was. Chris Jensen, Jim Jensen's wife, was scheduled to christen it. I had just been made department chairman, and so Shirley actually broke the bottle on the bow of the *Cayuse*.

CP: Did it replace the *Acona*, or were there three ships?

JB: Well, no. The *Acona*—when we got the *Yaquina*, it was with the understanding that somebody else would get the *Acona*. And I think the *Acona* went up to the University of Alaska. And the University of Alaska stretched it; they put it in a shipyard and made it a bigger vessel. And I don't know how it served them. I think everybody at Oregon State who had operated on the *Acona* was delighted to see it go, particularly to see it replaced with the *Yaquina*, which was a fine research vessel.

CP: What are your memories of President Jensen?

JB: I don't have too many of President Jensen. I arrived during A. L. Strand's tenure. Things were very formal, at that point. There was a reception once a year at the MU, and you were expected to show up for that reception. It was an evening reception. The festivities included a receiving line in which you shook hands with President Strand and his wife Molly, and then you would have a cup of punch and a cookie. But you had better be there, because if you weren't, they

would—it was noted. So that was formal. Commencement under Strand, every faculty member was expected to attend, and you lined up in order of seniority in front of what was then the library. And being junior, you were at the end of the line, where the old-timers were at the front of the line. But that was Strand.

Strand was proper, and Molly was proper. On the receiving line, the women were expected to wear gloves and a hat. It was different. Jim Jensen came in—Jim and Chris, and they were much more relaxed. Jim Jensen could be very formal, but Chris was very sociable. They started what was called a holiday party, and while they were here, that continued. That was discontinued under the MacVicars. It started with a Halloween party, and then it disappeared. When Shirley and I were involved, we started the holiday party again, and until Ballot Measure 5 hit, why, we had the party. But anyway, Jim Jensen was very professional.

And the occasion I remember most was a racial incident we had with a football player named Freddie Milton, and Dean Andrews, who was the football coach at that time. And it was during the Centennial celebration, Centennial Lecture from Oregon State's 100th year. And Linus Pauling had been invited back to make a talk. And in those days, the only big facility we had for that sort of thing was Gill Coliseum. And so they had the platform set up on the basketball floor, and everybody sat up in the—where you watch basketball from. And the black students—and I don't remember how many there were, there weren't very many; there were less than 100 at Oregon State—they protested. They marched, and they demanded a microphone.

And I remember sitting up there watching, and there was a young co-ed next to me who was absolutely shocked that this was happening at her university, her college. And I said to her, "Don't worry. This guy's a professional." [1:05:01] And Jim Jensen gave them the opportunity to make their statement, if they would then leave Gill Coliseum, which they were delighted to do. And that resolved it for that particular incident. There were some subsequent things.

Anyway, the race problems have plagued the university for a long time, and probably continue to. During my watch, we had the same problem. During Paul Risser's watch, there was the same kind of problem. It only takes one or two individuals to create a racial incident, and it's very unfortunate. But Jim Jensen handled it very well, very professionally. And that's how I remember him, as a very professional president. And of course, he was here at the beginning of a very tough time, which was the student unrest caused by the Vietnamese War. And he left during that period, and Roy Young came in as Acting President.

It wasn't over, because it was on Roy Young's watch that the students were killed at Kent State. And that caused a rupture nationwide. So that was a difficult time. It was a difficult time; I think that generally, the public appreciated what the presidents of the universities did to hold things together during that time, because it was a threatening time for the stability of our education. And it changed—the student unrest at that time changed the nature of student life at universities, nationally. An example, before that—I can't give you an exact date—but before that period, there were little hedges along the sidewalks at the MU quadrangle. Students did not walk on the grass. You know, this was sort of a carryover from what had existed prior to World War II. Subsequent to this, those hedges were gone. Students were throwing Frisbees on the lawn, and, I mean, gosh, this was unheard of before that. But students assumed greater responsibility subsequent to the Vietnamese thing.

CP: Do you remember much in the way of demonstrations on this campus, during Vietnam or Kent State?

JB: Yeah. I remember, you know, there were problems at universities everywhere. There was a bomb threat at the University of Oregon. And Oregon State had always been a very conservative institution. I guess it still is. But there were—I call them students, they may not have been students even, who came to Oregon State, didn't belong here, but they came here to foment demonstrations. And I remember there was one occasion down at the MU ballroom where these guys came in and tried to rabble-rouse, and so on. And the football team took positions around the outside of the auditorium, and they were not going to have any nonsense from anybody outside. And they made sure these guys understood that. We didn't have the demonstrations. Yeah.

CP: You took a sabbatical in 1966, at the NSF?

JB: That's not quite accurate. I was scheduled for a sabbatical, and it was '66 and '67, and what happened at that point was—first of all, let me back up. The National Science Foundation had a habit of taking people from universities for a year,

we called them rotators. They would come in for a year, learn how the NSF operated, be active in making grants, and so on, then go back to their university. And my colleague, my counterpart at the University of Washington, a guy named Joe Creager, Joe did this, then he went back to Washington. So at the time, I'm looking for an institution at which I can go to have a sabbatical leave. And at that time, I was thinking about writing a textbook on oceanography, and I was thinking about the University of New Hampshire for a number of reasons. [1:10:00]

But Joe called me up, and he said—and I knew where he was, and what he was doing—he said, "Would you consider coming back to Washington, and serving as a year as the program manager in Oceanography?" And I said, "Yeah, I'd consider it." And so I went back to Washington. They interviewed me; I could see the fit, and so on. And so rather than take a sabbatical, I was put on leave without pay, and went back to NSF, and for 14 months was at NSF doing their thing, to make grants to universities in oceanography, and so on. It was a great experience for me, because it acquainted me with the entire academic oceanographic community. And so I got to know them; they got to know me.

And then subsequently, there were some other things that happened at that time. They had a change in the dean's position at Oregon State, Dean of Science. A man named John Ward became Dean. He had done a rotatorship at NSF, and I got to know him while I was at NSF. And so when I came back, Wayne had had some medical problems, and he resigned as chairman of the Oceanography department. And, long story short, I became the chairman, his successor as the chairman of Oceanography. And I don't think that would have happened—it might have happened, but I don't think it would have happened, certainly, if I hadn't spent the year at NSF. It put me on a different plane as far as Oceanography was concerned.

CP: It was also your first major administrative position?

JB: Yeah, yeah.

CP: Did you find that you enjoyed it? Did you at that time start to envision administration as being something you might pursue?

JB: Well, I had always thought about it, you know. I thought about, wouldn't it be nice to be a university president someday, and so on.

CP: So you were considering that as far back as then?

JB: I was considering it as far back as being a graduate student at SC. In fact, while I was with John Mann out in Ely, Nevada, we got all of the reports from the students and John and I took them into Ely, rented a motel room, and graded the reports. And at dinner there that night, he said, "Okay Byrne, what are your aspirations? What do you want to do?" And that's when I told him. I said, "Well, I think I would like to be a college president someday. And I would hope that I would receive an honorary degree somewhere." And he said, "Why?" about that. And I said, "Well, that would indicate that I did something worthwhile that other people recognized." It happened.

So, yeah, I was thinking about being a university president. And yes, the NSF thing I suppose you could think of as administrative, but I didn't have any responsibility for managing people, just for managing and distributing money for scientific research. It wasn't until I came back to being department chairman of Oceanography that I had really a full spectrum of administrative experience.

CP: How was family life in Washington, D.C.?

JB: It was good. We managed to rent a very large apartment just outside the Beltway, and we had the whole family in the apartment. The girls were, the twins were in first grade, Donna was in third way, and they were picked up by a bus at the apartment, and so on. The apartments had—it was a complex of three major buildings. Each building had its own tennis courts and its own swimming pool. And so it was almost like living in a country club, you know?

And then we would manage every weekend to do something touristy, see something in the Washington, D.C. area that we couldn't see anywhere else. And that worked out fine, because being there a year, we could schedule visits and avoid crowds to the extent possible. I remember visiting the Capitol Building on a Sunday morning, and I think there were 27 people on tour or something like that, and during the summer, they run 10,000 people through the building, you know.

[1:15:00] So we were fortunate, and we kept notebooks and scrapbooks, and all that kind of stuff. So when we got back to Corvallis, the kids were all set up for being the "experts" on Washington, D.C.

CP: Well, as you mentioned, you were named Chair of the Oceanography department in January of '68. What was your vision for the department at this point?

JB: I think at that point it was—you know, it was keeping it alive. I remember telling Wayne, "We had the seven good years, and now it's time for the seven lean years," you know? And of course, I was learning the whole thing. Money was always a problem, because the Oceanography department operated on 90 percent federal money, or non-state money I should say, and ten percent state money. And the non-state money was never guaranteed. And so along about January or so, you didn't see how you could make it through to June. We always did, but it depended on grants, and contracts, and that sort of thing.

Beth Strong was the financial manipulator. In those days, things were pretty loose. She would shift money from the ONR contract to NSF, and vice-versa, and so on. We had some AEC money at the time, Atomic Energy Commission money, too. And so, I was learning all of that. And things changed. Everything tightened up; regulations increased, and so on. But for me, it was a matter of keeping Oceanography alive. And we did manage to grow. And so, when you ask me about vision, the vision wasn't very long-term. Yes, we said, Okay, we did a long-range plan. We didn't really use it; it was really more of a wish list than anything else. And so we did that, and we hired some people, and as I say, the department continued to grow.

One other major element—when Jim Jensen was president, just before he resigned, he appointed a goals commission for Oregon State University. And the goals commission was set up to identify just what we are talking about—long-range vision, long-range goals. And one of the goals, and Wayne had been pushing on this, was for Oceanography to become a separate school, separate from the rest of the School of Science. Well, Jensen left, and the goals commission finally finished its report. It was three people. It was Warren Hovland and Jim Knudsen from Chemical Engineering, and who else? Who else? I don't know—short memory. Anyway, they submitted their report, and it landed on MacVicar's desk.

And MacVicar had been chosen as the Permanent President after Jim Jensen, and Roy Young had been the Interim President. And so it said: Oceanography to become a school, with certain stipulations. That happened in 1972. And so I had been chairman of the Oceanography department from '68 on. MacVicar, being the kind of person he was, said, "Well, we don't need to do a search. You can be Dean of the School of Oceanography." I think Wayne wanted to be the first dean, but he wasn't.

And so I was the first Dean of the School of Oceanography. The only thing that changed was the reporting structure. I no longer reported to the Dean of Science; I now reported at the same level as the other deans reported on, and that was to the president, because we didn't have a provost. We had Dave Nicodemus was Dean of Faculty, and he was basically a faculty personnel officer. The university was still evolving.

CP: So how did the department get to the point where it could be classified as a school?

JB: It kept getting bigger, and bigger and bigger, and more students, more faculty, more staff, and it was based on research contracts. [1:20:03] We didn't teach significantly any more courses, but one of the things that happened in those days, and I guess maybe it still does, is on any research grant, a certain percentage of the grant are for indirect costs. Indirect costs are the kinds of things—they're real costs, but the easiest way to handle them is to say, "Well, we're just going to give you a percentage of whatever the grant is, for you to put in the library and maintain the buildings, and so on, and so forth."

Well, some institutions don't do this, but Oregon State has always taken a portion of the indirect costs, and returned it to the unit that generated the grant or the contract that provided the money. And that's called return to overhead. And Wayne—at that time, a lot of the schools, the departments, would distribute it to the faculty member who had written the proposal. Well, Wayne never did that. From day one, Wayne said all of the return to overhead is coming back to the chairman of the department. And that was a very wise move, as it turned out. That meant that as all of this research thing grew, so did return to overhead. And return to overhead was used to build boats, to buy new computers, to build buildings, big ticket items, as it were, and also to pay salaries of staff, and so on, who were running the department.

And so, Oceanography's growth was two-fold. It was, yes, the research, but it was also the return to overhead. And by the time we got close to being a school, the Oceanography department was bigger in many ways than any other department in the College of Science, the School of Science. It didn't have the same problems that the rest of the departments had. They had problems with undergraduate students, and how do you justify so many faculty, and so on. That was never a problem for Oceanography. It was operating, in a sense, like a research institute more than it was as an academic department. And so when the time came, it was the right move; it was obvious. In the department, we had no sub-units. We a lot of biologists; we had a lot of chemists, and these guys operated as de facto departments within the Department of Oceanography, and subsequently, in the School of Oceanography.

CP: How did you feel about the compromises you were having to make as your administrative load increased, and you ceased being able to teach and do research as much?

JB: Yeah, well unfortunately, that's true. And the courses dropped by the wayside. The first courses that I had to let go were the geology courses for the geologists. And it was probably because of the way I used to teach those courses. They were small, a small group, and so it was mostly discussion and dialogue. I frequently never knew where a class session was going, because we'd get involved in debates and discussions, and so on. Then the next course that disappeared was the course for all of the oceanographers. And the last one that I hung onto was the undergraduate course, which was a lecture course. But, you know, you get caught up in these things.

Seeing the evolution of oceanography nationwide, and trying to grasp the opportunities as the nation changed, began to take on a bigger part of the job. And so, the teaching was—I had done it, okay, but now where do we go? There were two things that happened. I think the most important was in the late '60s [1:25:00], I think Nixon was the President, and said we ought to have a decade of international ocean exploration. And so it turned out that the '70s was designated as the International Decade of Ocean Exploration. And a major chunk of money was given to the National Science Foundation to get some big programs going. We referred to it as IDOE. And IDOE said, "Hey, we want you guys to do big projects." That was one thing that happened.

And so we had to decide, did we want to participate in this? And so what I did—and I was either department chair or dean, I don't remember which—had a meeting of all of the faculty, and we talked about it. And I said, "If we do this, it means that our ship is going to be gone maybe for six months of the year." And so faculty decided we have to do it. If we're going to be an oceanographic institution of any repute at all, we've got to do this.

Something else happened at about the same time. A group of directors were pulled together, and I was one of them, to create a new system for operating ships. And this was a result of something that had happened at the end of the '60s. End of the '60s, oceanography was getting bigger and bigger internationally. A commission called the Stratton Commission was created. The Stratton Commission looked at the whole spectrum of oceanography, and made all sorts of recommendations. And I think that it was a very wise thing to do.

They came up with all of these recommendations, and then the task came to implement the recommendations, and I don't think any of them were implemented the way the Stratton Commission had proposed them. But what it did was it stimulated some thought and some serious discussion about a variety of things, one of which was how we operate our ships. And so from that was created a thing called University National Oceanographic Laboratory System, short term UNOLS. So UNOLS and IDOE happened on or about the same time.

UNOLS, as it ended up, said, "Okay, we've got this fleet, this academic fleet. Now we're going to operate it as a single fleet." And part of the reason for this was one summer, there were three oceanographic institution vessels in the Mediterranean at the same time, and those three institutions didn't know the other guys were there. And had they known that, they could have done some spectacular science that got missed. So anyway, the decision was, let's do UNOLS.

And UNOLS sort of standardized the fleet. It meant that your faculty might be on somebody else's ship, and their faculty might be on your ship, so. Well, in a sense, it wasn't your ship anymore, it was the fleet's ship. So that happened, and IDOE happened, and we decided we were going to participate. And that turned us from pretty much from a coastal oceanographic institution to an open ocean, blue water ocean—it put us in the big leagues. We were now in with Scripps, Woods Hole, Lamont at Columbia, Texas A&M, Rhode Island, Hawaii, and Miami. We were the oceanographic institutions, and Oregon State was one.

And as you look at what's happened subsequently, Oceanography at Oregon State, now Oregon State ranks with Scripps and Woods Hole. We're number three, and that's because of some good leadership along the way, and we've hired good people. I didn't do it, but I had a hand in getting it started.

CP: Something that happened around the time of what you're talking about, you were named the director of the Hatfield Marine Science Center.

JB: [Laughs] Yeah.

CP: How did that work out?

JB: Well, I had an assistant in Oceanography [1:29:59], a guy named Dave Zoff, and Dave liked to go down to the Marine Science Center. We had had some problems down there. When Marine Science opened, Jim Jensen, for whatever reason, made a very bad administrative decision, as it turned out. He said, "We're going to have two directors, Wayne Burt and Tom Scott." And they're both located on the Corvallis campus. Well, they had their guy, their person in Newport, and Wayne had a guy named Joel Hedgpeth, a crusty guy, you know, old biologist. And Tom Scott had Bill [snaps fingers] again, senior moment, had his guy. And they didn't get along. And so what evolved was if you were in the wing that Joel Hedgpeth was responsible for, you didn't cross the line and go into the fisheries wing, and vice-versa.

Well, that was compounded by the guy we had down there in Oceanography, Dick Redmond, who ran the ships. Redmond was appointed as the facilities manager for the Marine Science Center. Well, he was a martinet. He was retired Navy, like [claps hands] he did it this way, you know, and that's the way we're going to operate this place. And it didn't work too well with faculty. So Dave got down there and he discovered what this mess was, and so he came to me one day, and he says, "I think we can fix it." And he says, "How about if we orchestrate it so you become the director of the Marine Science Center, one director, and Wayne and Tom Scott resign as directors, and then I, Dave Zoff, become facilities manager under you? We get rid of Redmond; he just operates the ships, doesn't have to worry about the rest of the center. Wayne doesn't." Hedgpeth had become a thorn in Wayne's side. And I said, "Okay, but who [unclear]? Who tells Hedgpeth he's no longer in charge?" Who tells Bill that he's no longer in charge?" Well, of course, I got to do that.

And so, there was a committee that helped Wayne and Tom Scott manage the thing from over here. It involved Fred Burgess, who was then Dean of Engineering, and so on. And so, we proposed this, sent the proposal to MacVicar, and Mac said, "Fine. You guys go do it." And I talked to Joel. Joel and I had a good relationship, so I could get away with it, whereas other people couldn't. And so I became the director of the Marine Science Center.

And then the job was to convince MacVicar that we needed a director of the center down there. And that took a number of years, and then I think he appointed a committee. He didn't take our word for it, but appointed a committee. I said, "Mac, you've got to have a director down there." And Lavern Weber was selected as the director, and I think Lavern was director down there for 25 years. And that's when the Marine Science Center began to come together. But it was a problem. But anyway, so I was "acting director," which meant going down at least once a week, sometimes twice a week. Dave Zoff went down every other day. And I was acting director for about five years.

CP: While you were Dean of Oceanography?

JB: While I was Dean of Oceanography, yeah. [Laughs]

CP: How did the O. H. Hinsdale Wave Research Facility come about?

JB: Well, that was another interesting one. Hinsdale, Howard Hinsdale. Howard Hinsdale was the President of Umpqua Navigation. Umpqua Navigation wasn't navigation at all; it was the outfit that built the jetties along the Oregon coast. And their style of building jetties was to get huge chunks of basalt, which they could get from a quarry on the Columbia River, and take them down, and actually not just dump them, but place them so that they interlocked. And that's what Howard Hinsdale did.

Now, Howard Hinsdale discovered that there was a proposal in the federal government, probably from the Atomic Energy Commission, to build artificial islands on the continental shelf [1:35:00], on the East Coast, and on those artificial islands, to put nuclear reactors. East Coast because the continental shelf there is shallower and broader than it is on the West

Coast. And so Hinsdale—he could see dollar signs, I guess, and he said, "Boy, if we could get those contracts for those islands on the East Coast, we'll build them any way we can. But I need an independent assessment of our way of building jetties." And I think he went to Corps of Engineers first, and they weren't interested. And he looked around, and there was no facility where adequate study could be made.

And so he knew that there was Oceanography at Oregon State; he knew that there was Engineering at Oregon State, and so he proposed that we create, we at Oregon State, create a wave tank that could do the research on his way of building jetties. And as a university, we would be clean; we would be able to document that his way was best. Well, I think that the people involved said, "We'll be happy to do this, but we're not going to promise you that the research is going to turn out in your favor." Well, Hinsdale was confident it would, and so he gave us the money to build this wave tank.

We had two guys who were competent to do it. One was Jack Nath, who had spent a year here on sabbatical; he had spent the sabbatical in Oceanography. He had come from Colorado State, and he wanted to transfer to Oregon State, and Oceanography couldn't handle him. He was an engineer. We didn't know how to evaluate an engineer. So he sent his stuff over to Engineering, they hired him immediately. I mean, he was a competent guy. And so there were two engineers, Jack Nath and Larry Slotta in Engineering. And they said, "Yeah, we can build that tank." They had never built one before, and they figured they could do it. And so they did; they built the first tank. It was 300 feet long, plus or minus a few feet. It had a wave board that made independent, or irregular waves, if you wanted it to. But you can build a model wave that was five feet high from trough to crest. And it was out in the fields. There was no building around it, no one—they got one of these little temporary buildings with the equipment in it. So they built the wave tank, tested Howard's facility, or way of building things that work. It was good.

And then of course, what happened was the Atomic Energy Commission decided, well, it wasn't such a hot idea anyway; we're not going to build those islands. But we had the wave tank. And the researchers in Engineering, there were some in Oceanography, mainly in Engineering, continued to do wave research. Navy got interested. The Navy decided we were going to make that a center of excellence, and they began to build the buildings around the wave tank and some other tanks. The National Science Foundation came in and said, "Yeah, this would be a great center for tsunami research." And so, that's how it all started. What started with Howard Hinsdale's idea that he was going to build islands on the continental shelf. [Laughs]

CP: In 1976, OSU got its hands on another boat, the *Wecoma*?

JB: Yeah. The *Wecoma*—*Yaquina* was getting old, and Woods Hole proposed that they, the National Science Foundation, that they design and build a new vessel, and they would call theirs the *Oceanus*. And National Science Foundation had enough money to build two vessels. And so there was a competition for who gets the second vessel. Well, we wanted it; Rhode Island wanted it. Rhode Island had, as I mentioned earlier, a similar ship to the *Yaquina*, a World War II vessel that was getting pretty old. And so we competed. And when Wayne resigned as department chair [1:40:01], he was made associate, I think they called them dean in those days, Associate Dean of Research for Ocean Science, or something like that. And he spent his time writing proposals, doing the same thing he had been doing all along, but he didn't have to worry about day-to-day management.

And so Wayne wrote this proposal for the ship, for what turned out to be the *Oceanus* class vessel. There were going to be two of them, and so we had to compete for it. So, we were asked to come back to the National Science Foundation and make our presentation, and I made the presentation. Rhode Island made their presentation. I was told, shouldn't have been told, but I was told subsequently by one of the panel members that listened to our presentation that ours was better, that the guys really didn't want to give up the *Yaquina*, and the guys at Rhode Island didn't want to give up the *Agassi*, whatever the name of their vessel was.

And people at NSF made a very smart move. They said, "You know, if we give the second ship to Oregon State, if we place it at Oregon State, Pell, Pell grants—Senator Pell would really be upset, and he'll put money in the budget for a ship for Rhode Island."

CP: [Laughs]

JB: Now, we surmised that, but that's what happened. So they built three of them. We got the second one; Rhode Island got the third one. And the people who cut their teeth, their oceanographic research on the *Wecoma*, swear by it. They thought it was so good, just the way the people on the *Yaquina* thought the *Yaquina* was so good.

Well, the *Wecoma* was launched, and I don't remember the year exactly. It must have been '75 or '76, in Sturgeon Bay, Wisconsin. And so we went up there for that launching. The ship was built there, came on around, and while it's coming around, I left Oceanography and went over to the research office for the university. George Keller became Acting Dean of Oceanography. And we had the celebration and so on for the *Wecoma*. And it was an interesting experience, and, you know, little things. Sturgeon Bay, Wisconsin, and seeing—I played the band in high school—seeing the band play "On, Wisconsin." Great! The Wisconsin band actually played "On, Wisconsin," and then they broke into the University of Oregon fight song, and I'm sure I was the only guy there that knew, you know, they had blown it, you know. And of course it didn't make any difference, but it wasn't the Oregon State fight song, it was the University of Oregon fight song.

CP: Well you referenced that you moved out of Oceanography and became Dean of Research in 1976. How did that happen?

JB: Roy Young had been selected to be the Chancellor at the University of Nebraska. And George Keller had said to me before that happened that he wanted more responsibilities in Oceanography; he wanted to learn more about how Oceanography operates. And so I went to MacVicar, and I knew Mac pretty well at this point. I had traveled with Mac, and in fact I had even roomed with him. You know, you volunteer for that kind of stuff, and you have a special relationship. And so I went to Mac and I said, "Why don't you put me in the research office, and on an interim basis while you do the search, put George Keller in as Interim Dean of Oceanography? And then after the search is done, I'll go back to Oceanography."

I never went back, and I was selected to be the next Dean of Research. And George was still the Interim Dean of Oceanography, and he did not get the permanent job. Ross Heath, who had previously been a faculty member in Oceanography and had left, was brought back as the next Dean of Oceanography. But I went over to the research office and, again, it was a learning thing. The value to being in a research office, to me, was it gave me an opportunity to see the entire university [1:45:01], rather than just the oceanographic and marine stuff.

CP: So what were your duties when you entered into this position?

JB: In the research office? Well, we had a couple of major grants that we used to distribute money. And mostly the effort was to help faculty member prepare proposals for research, make them aware of opportunities. And if you looked at the whole university, it was skewed in terms of research. Oceanography, Forestry, Engineering, to some extent Pharmacy, I suppose, knew how to get research grants. Folks in Liberal Arts didn't have a clue. And it's not a criticism of them; it was just not part of their experience. And so it was an opportunity to see how we could help the Liberal Arts, among other things.

Roy Young had started a program of bringing faculty interns into the research office, and I think the first one that he brought in was a man named Richard Astro, who at one point was the Chairman of the English Department. So we pulled somebody from liberal arts to help, you know, help Liberal Arts get going on research, scholarly activity. And so that was one of the things we did. The intern that I had happened to be Larry Kennecke, who came out of Education. And Larry did a number of things to, again, to advertise how to do research.

A second thing that happened about that time was that two faculty members, one in Pharmacy, one in Biochemistry, I think—no, Microbiology—Microbiology. Jim Ayers and Sandine, what was Sandine's first name? Anyway, developed, with graduate students, a microbial cheese starter. So, you dump it into milk, and it begins to form curds, which drop out. Well, what they came up with was a combination that controlled the acidity of the whey, the milk left behind, so that it could make more curds. And one of the subsidiaries of Borden Company, Borden Milk Company, Elsie the Cow, discovered this, and these guys made a lot of money on contracts of this stuff, licensing out this thing, which they had a patent on! Well, it meant that we needed, in the research office, we needed to beef up this whole patent-copyright business.

And so Ralph Shea had recently stepped down as the Chairman of Botany and Plant Pathology. Ralph Shea was brought into the research office, and so a whole new thing of technology transfer began to develop from the research office. It still goes on, but it started with Ralph Shea, and it started pretty close to my watch in the research office. So there were a lot of things going.

It was a great university, that hasn't really achieved all of the opportunities that it can achieve. And that's part of the reason for being in the research office. The research office was created by Jim Jensen. Roy Young was the first director of the office, and it was the same sort of thing. It was, you know, have a couple of big university-wide grants that you can distribute money from. It had a research council that met periodically to develop policies, and that sort of thing. It was good.

CP: You had a misunderstanding with the Experiment Station during this time?

JB: Yeah, well, not so much with them as with the people that relied on the ag research stuff. And Jack Davis was the director of the ag research enterprise. You have to, again, put it in context. Agriculture at Oregon State, this was an Oregon Agricultural College, and agriculture was, you know, the top of the pecking order. And a lot of things had evolved under that sort of a system. Well, now we had research everywhere, and all I wanted was Jack Davis, in one way or another, to communicate so that we knew what was going on in agricultural research. [1:50:02] Well, so we needed a double-reporting routine, not just to the dean of agriculture, but also to the Dean of Research of the university.

And so we set that up, and there was some misunderstanding. And one thing you discover if you are president is nobody outside hesitates in writing to the president telling him how to do his job. And the aggies are no exception to that. And so, when it became known that the director of the ag research station was going to report to the Dean of Research, you know, they barred the door. And so these guys were upset, and so on, and MacVicar was being bombarded with letters. And so we called a meeting and explained everything. "Nothing's changing, guys. It's okay. It's just that we need better communication internally." And they bought it.

CP: One of the people you worked with in this particular episode was a legendary figure, Warren Kronstad. I wonder if you—?

JB: Warren was great. I mean, you know, what a champion. But Warren, you know, you hear all sorts of things about guys who've been in production agriculture, and so on. Warren was a wheat producer, and he was very well known. He had a program where he was trying to hurry up the evolution of wheat, and he developed a program with Mexicans where they grow wheat here for a while, then you go down to Mexico and grow wheat down there, and so on. And Warren had a very successful program. And I said, "You know, Warren," this was when I first went into the research office, I said, "I'd really like to know what you're doing."

And I spent more than a half a day with him. We went out to this little farm out here, and so on, and Warren said to me, after it was over, he says, "You're the only administrator that ever spent this kind of time with my program." Bernie Briskey was the Dean of Agriculture, and Bernie had an award ceremony where they recognized the agriculturalist of the year, and so on. And the program was too long in my opinion, but anyway, they had this event, and the culmination of it was to name the agriculturalist of the year. And of course, it was Warren Kronstad. And I said to Warren afterwards, "I would have been disappointed if it wasn't you." But he was a real champion.

And Warren pointed out to me that it takes—in those days, it was using sort of traditional genetic stuff. This was not genetic engineering that's going today. But Warren would say, "Well, it takes 10 to 12 years to develop a new strain of wheat, and it takes the diseases about 10 or 12 years to make sure that you do develop a new strain of wheat." And the strain during my tenure was Stevens. The Steven variety was ultimately produced; 90-plus percent of the wheat produced in Oregon was Stevens, and that was Warren Kronstad.

CP: Another highlight from your period as Dean of Research was the Andrews Experimental Forest.

JB: That was my first. I think I had just moved into the research office. I didn't have a clue what I was supposed to do. You know, I was still learning. And Carl Stoltenberg was the Dean of Forestry. And he said, "You know, we've got this experimental forest situation out here in the Andrews Forest. It's a drainage basin; it's a complete drainage basin." Streams

flowed down into the MacKenzie River. And he said, "The National Science Foundation is coming to see whether or not that can be a national facility, and they need a commitment from Oregon State. So we need support for the director who will be out there, okay?" Well, my son and I at that point were riding dirt bikes. So we put the dirt bikes in the back of the truck, and we drove down to Andrews, and rode our motorcycles around Andrews.

And it's a neat little drainage basin, you know? And I said, "Yeah, this is pretty good. We need to put some money in here." [1:55:00] And I could talk then about Andrews Forest with some reality. I had been there; I had seen it. I had gone to the top of the thing and looked right out there [claps hands] and the Sisters are right in your face, you know? It's a beautiful spot. So I said, "Okay. We'll provide the salary and the directive." Carl was only looking for a piece of the salary. Well, I provided the whole thing, you know.

CP: [Laughs]

JB: And I don't know what it did to the research office budget, but it made a hero out of me with the foresters, you know, because I was dumb. [Laughs] It was a dumb mistake, and it blew the guy from NSF away. He said, "That's great. You've got it." And I don't know, I guess it's still going on as a part of a national program. And it's been good. They wanted a place where they could do research that stretched out over a long period of time, and to understand what happens in a forest, you need to do that, you know? So, the Andrews does that.

CP: Well, our last topic for today is the last position you had at OSU during this chapter of your time here, was initially as Interim Dean of the Graduate School, later you were named Vice President for Research and Graduate Studies.

JB: [Laughs] Yeah, that was another funny—the office, the research office and the graduate school office were right next to each other. Bob Newburg had been the Dean of the Graduate School, and he decided he was going back for a year to work for ONR, or somebody back in Washington, and so we needed an interim dean of the graduate school. And Mac looks down from the sixth floor, and he says, "Well, the offices are next to each other on the third floor. We'll let Byrne do it." Well, it wasn't that hard, really. Wendell Slabaugh, who was a faculty member in Chemistry, was Associate Dean of the Graduate school. And so, Wendell was there, and he knew the ropes, and so on.

So, I moved into the graduate dean office, and I had two offices; I had a research office and I had a graduate school office. Mac says, "We can let Byrne do this. This way, he doesn't have to hire another person." And so I go in, and Wendell—Wendell Slabaugh is a great guy, and he died much too soon. But anyway, Wendell has got some very definite opinions about some of the different graduate programs throughout the university. He's not too high on the School of Education, and he didn't bias me. He says, "They claim that their students come in with the lowest grades, and graduate with the highest grades." And he says, "And they claim that's because of their superior teaching over there." Well, he has a different idea. He just thinks that they have this grade inflation for the guys in education. That was Wendell. That probably shouldn't even be in this thing.

But, I'm in the graduates, and the people who come to see the dean of graduate school want you to wave some regulation. I'm generalizing. More often than not, they want to wave a regulation. They don't have enough time; they want you to extend the deadline, or whatever it may be. So, the answer is no. But I was a softie. I was inclined to grant their request, until one of the clerks in the office says, "You can't do that. All of the rest of these graduate students are meeting those regulations, and meeting those things. So you can't do that. That's detrimental to the rest of the graduate programs." And she was right. And so the graduate school office became my No Office. No, you can't do that. We'll help you meet your requirements, but we're not going to wave it.

So that was the No Office. The research office became the Yes Office, because these are the guys who would come in and say, "I've got this great opportunity, and I need to do this. Can you help me do that?" And so I was, "You bet. We're going to make Oregon State a better place. So, it was yes and no, and I would spend the mornings in the graduate school, when I was fresh and tough, to say no, and the afternoons in the research office, where I could easily say yes.

CP: [Laughs]

JB: Yeah. And that lasted about a year or so. Newburg never did come back. And I don't know who the next dean of the graduate school was. And I left to go to NOAA.

CP: Well, we'll take up with that next time.

JB: Yeah, ok.

CP: Thanks.