Joseph Smith: – here, October 4, 2017, at the library at the Beaufort lab on Pivers Island. We're here to interview Dr. Bill Sunda, and present here are Drs. Sue Huntsman, Don Hoss, Doug Wolfe, Doug Vaughan, and myself, Joe Smith. We'll proceed with the interview. Dr. Don Hoss will be doing the initial questioning.

Don Hoss: Thanks, Joe. I want to say that I think the interview is with Bill and Sue because they were a team for a very long time, so feel free to ask questions to either one of them. I've got some – Bud [Ford Cross], who is not here, initiated this by sending me an email telling me that you were going to be in town, so that's how it came about. I'm really sorry Bud can't be here because he has a close relationship with the group. First of all, I'd like for both of you to just give a brief history of your educational background, and then we'll move on to lab-related things. Bill?

Bill Sunda: Okay. So I did my undergraduate degree at Lehigh University, starting out initially in chemical engineering. After one year, I asked myself,
"Why do I want to be a chemical engineer?" I couldn't come up with any ready answer, so I left that program. I wanted to be a scientist. In fact, I had gotten fascinated, starting in high school, with diving. I got fascinated with diving and the marine environment but got fascinated with the marine environment from scuba diving. I also liked chemistry because chemistry, you could basically figure out stuff using it. It was very rigorous, a certain set of laws and ideas, and you could derive things. I liked the discipline of chemistry but didn't go into the chemistry department. I was afraid of language. I was a very bad language student in high school. If you were going to be in the chemistry department, you were in the arts and science school. However, you could go into a program called fundamental science in the engineering program and essentially take the same courses and get the same education, but you didn't have to take a language, so that's what I did. It was me and this other guy. I can't remember his name – rode around on a motorcycle, long hair. Those were the two people in fundamental science. Graduated from Lehigh with a degree in fundamental science, but really it was a chemistry degree, in 1968, and I got a fellowship to MIT [Massachusetts Institute of Technology]. Applied to three schools. Scripps didn't offer me a position. University of Washington – "Yeah, you can come, but you're going to have to pay your own way." And MIT, the brand-new program – and it was a joint program between MIT and Woods Hole Oceanographic Institution, and they offered me a full fellowship. Well, that was pretty obvious what my choice was going to be. Went up there and went there in 1968, summer school. We were the founding class of that joint program. One thing about that joint program is that there were an awful lot of scientists up there who really weren't interested in being teachers, professors. They thought these students would just get in the way of the research. Now, they know differently.

DH: Was that at MIT or Woods Hole?

BS: This is at Woods Hole. MIT was used to having students, right, as a university. Woods Hole was never a university. They would have students that would come in from other universities. People from MIT, for example, did their work down at Woods Hole. People from Harvard, same thing, and various other schools. So initially, they actually ended up throwing out a lot of students that were pretty good because they didn't really know what they were doing. They knew one thing; they wanted to be better than Scripps, but they had no idea how to do that.

So I managed to survive my first few years and started out in chemistry and then took a course at Harvard by – it was a fellow by the name of Werner Stumm, who published the first book on aquatic chemistry, which was applying the rigors of chemistry, in terms of kinetics, thermodynamics, to what was before wastewater treatment. It was very applied. He applied that to aquatic chemistry, and I got fascinated with that. Also, he was very interested in the whole idea of the relationship between the chemistry of nutrients and the chemistry of toxic metals and their uptake by organisms, their availability, their bioavailability. So he piqued my interest, and I wrote a thesis proposal. That was back in the days where, actually, they gave you a fellowship, you came up with your own project, and then you found somebody whose lab you could go and do the project in. It doesn't happen that way anymore. Now, basically, you show up, and they try and get you on to somebody's project as fast as they can in order to, basically, cover the cost of your education, your room and board, and such. So I was really fortunate that I was able to actually come up with my own project. I knew the chemistry. The project was the relationship between the chemical speciation of metals and their uptake and their toxicity in phytoplankton. Well, I was a chemist. I knew a lot about chemistry, and nothing about growing phytoplankton, so went to a guy by the name of Bob Guillard, who had the most expensive culture collection in the whole world. And this guy knew how to grow algae. He actually was the father of the mariculture industry. Before he came along, they couldn't grow individual cultures of phytoplankton and feed them to oyster spat or clam larvae. He did that. Anyway, I asked Bob, "This is my project. Would you take me on?" And he said, "Sure." Before that, I talked around – "What do you think about this guy Bob Guillard?" And most people said he was a nice guy. This one guy, Dave Menzel (sp?) – I said, "What do you think about Bob Guillard?" And Dave says, "A son of a bitch." [laughter] It turns out that Menzel was a son of a bitch, and Bob, bless his heart, just died about a year ago. I ended up writing a memoriam for him. He was a sweetheart. He took on all sorts of folks, took me under his wing and taught me how to write, taught me how to think, and I'm very grateful to him. As far as a job, coming here, it was really sort of strange. It was 1974, so I walk into the lab, and Bob says, "Bud Cross called from Beaufort about a job." I said, "Who's Bud Cross?"

DH: Where's Beaufort?

BS: "What job? And where the hell's Beaufort?" [laughter] It turned out that the job came from the fact that Bud Cross, at the time, was working for the AEC [Atomic Energy Commission] as a rotator, and he was basically going around to all these AEC – it was AEC then, wasn't it – or was it something else?

Douglas Vaughan: It was already Department of Energy.

BS: Okay.

DV: ERDA [Energy Research and Development Administration]. It was ERDA.

BS: ERDA. The Energy Department went through several different names. It was always the same thing, but – and one of his contractors was Vaughan Bowen. Bowen was on my committee early on, and he had a habit of – you'd go talk to him, and he'd have this pen knife that he would pick at. Everybody was terrified. All the students were terrified of Vaughan Bowen. All the nontenured scientists were afraid of Vaughan Bowen. I was afraid of Vaughan Bowen. I got him removed from my committee. But he was responsible for actually getting me this job. So I called up Bud and find out there's this job. He said, "Well, are you interested?" I said, "Well, I don't know. I don't know. I'm not going to take a job someplace place I've never been to, a lab I've never seen. Is there any way you can do a job interview?" They figured out how to finagle me to come down there as, I guess, some sort of contractor or something like that, I guess as a site visitor. Anyway, they figured out how to get me down, to pay for my visit down here for a job interview. I had a job interview with you and Bud over at Cox's restaurant.

Don Hoss: You is Doug Wolfe.

BS: You is – I'm sorry – Doug Wolfe, yes, Doug. We had a party over at your old house when it was still the old house.

Don Wolfe: On the screened-in porch.

BS: The screened-in porch.

DW: Which isn't there anymore.

BS: Yeah, and after looking – oh, I asked Doug and Bud – I said, "I'd like to do this, this and this." I was fascinated with the black water up there, Newport [River], which has this organic matter in it, and it's called humic compounds, which complex copper and regulate its availability, so I was interested in working with that stuff. I said, "I'd like to do this, this, and this. Can I do this?" They said yes. I said, "Okay, I'm coming." Drove down here in May of – actually, it took me another six months to get my degree, longer than – well, it's not unusual, so I was supposed to come here in the fall, and I ended up coming here in May. This is the fall of '74; ended up coming here in May of '75. Packed up my car and drove from Woods Hole down here and went through an entire spring in two days because when I left up there, the leaves hadn't come out yet. By the time I got down here, and I was staying in a garage apartment over in Beaufort, the guy whose property it was had a garden with tomatoes. I was shocked. By the time, the end of May, it was like ninety degrees down here. That's how I ended up down here. The first five years were fabulous. I didn't realize just how good I had it – we all had it that good because all we had to do – we had that AEC contract, which you had helped get it. I just lost the name of one of the most important people in this lab.

DH: Couldn't be TR Rice?

Sue Huntsman: Ted Rice?

BS: TR, yes. Ted Rice.

SH: How could you forget?

DW: That fellow in the picture right over there.

BS: That guy.

JS: Speaking to the picture.

DW: Who, by the way, was known widely for his ability to culture phytoplankton and feed them to specific shellfish in the 1950s.

BS: I will tell you – I have a story about Bob and Ted that the only reason I know is because Bob told it to me. But Bob was on a ferry going from – I think he was going from Milford, the Milford lab, which actually was a main place for growing phytoplankton, in fact, and doing mariculture. Initially, they wanted to grow these phytoplankton to feed them to oysters and clams so they could raise these things up to a certain size and put them out. Before that, you really couldn't do that. He just happened to be on the – Ted Rice and Bob Guillard just happened to be on the same ferry going across the sound over to Long Island. Ted was talking about how he had given up his career for his people. And I think he did. His job here was to break people in here and nurture them. Most people think of – we have all sorts of ideas, all of us – okay, we knew Ted – about Ted. But he really was devoted to this lab, and he was devoted to his staff.

DV: Yeah. Ted brought me here.

DH: You're absolutely right about that. Before we get back on that, though, I want to have Sue at least tell how she got here –

SH: How I got here.

DH: – because I think we may have got you from Duke marine lab.

SH: Yes. Education, if you want that, I have one thing in common with Bill. I wanted to be a chemistry major at Cornell. Found that, as a New York state citizen, I could get into the biochemistry department and be in the Ag [agriculture] school, which was a state school, so I wouldn't have to pay tuition. So I graduated from Cornell in 1963 and had met Gene at that point. I had dreams of going to Scripps or Yale or someplace with a really good marine program. And Gene was already – he was a year ahead of me, and he was already going to Iowa State. He said, "Well, pick Iowa State. It's halfway between both oceans. How can you lose?" [laughter] So I ended up at the last place I ever wanted to be, but I was married to the same guy for the last fifty-four years, so I guess it's okay. But anyway, at Iowa State, I was a phycology major, and I studied extracellular product from a diatom that only grew in the wintertime in Iowa, on the rocks, so that was not the most pleasant environment for doing your thesis. After that, we had postdocs at University of Miami, in Florida, for a year. And then I came up to the Duke lab on a postdoctoral fellowship with John Costlow, was doing that for two years. And then Dick Barber came to Duke. He was looking for people to help him out. The CUEA [Coastal Upwelling Ecosystems Analysis] program was just – the upwelling program was just getting started, so I signed up with him and stayed at Duke until 1980, I believe. Then the CUEA program, the upwelling program, ended. I was sort of looking for something to do. Bill and I had collaborated on a chapter for a book that Ian Morris was the editor of. Once we had a little bit of finagling, and he got me a seven-hundred-hour appointment was the first one. Then I was out of work for a little while. Then he worked through Francois Morel and got –

DW: At MIT.

SH: – at MIT, yeah – got me as a contractor for MIT, so my checks came from MIT, and I did –

BS: Was that five years (inaudible)?

SH: Yeah, it was quite a long time. Then I was on call here at the lab. I had two of those MIT appointments. They weren't quite back-to-back. It was an on-call in between, and then another on-call, but that's how I met Bill was through working on that chapter and –

BS: Yeah, from '81 on, until you retired, you were here continuously. But how you were funded was something else.

SH: It affected my retirement salary, I'll tell you that.

BS: There was a hiring freeze when we first picked her up.

DH: Well, this is Don Hoss again. I think we ought to identify ourselves, or else this could become pretty loose in ten, fifteen years. I think you started to get into it, but from '58 until sometimes after you came, this lab was really, really unique. Chipman started it, and Ted perfected it on being able to do research within the National Marine Fisheries Service. But quite frankly, they didn't have a clue sometimes on what we were doing. It was really good research on a lot of things. Some related to fisheries, some didn't closely relate. But you alluded to it, and I think it's an important point. I think Ted – we all had opinions of Ted, but Ted was a terrific director. And the thing – like he got me back in school for my advanced degrees. He did things for people that some people have no clue of. And while he might have been occasionally a little difficult to work with, it was all meant to be good, I think. I'm glad you brought that up because that's something we ought to get into the record. But now – oh, go ahead.

BS: Just a little aside – actually, not even aside – there's a rumor I had heard. You probably will know about this. He basically – he did play hardball. When he wanted something, okay, he played hardball. And I had heard that, in order to get his way with the National Marine Fisheries Service, in order to do the research that he wanted done at this laboratory, he threatened the National Marine Fisheries Service that he was going to pick the lab up, lock, stock and barrel, and move it to, I think, AEC. And they caved. There was nothing behind it, but this is the kind of guy he was.

DH: I don't know that specific instance, but I know he did certainly things like that. When he had the first – [Walter] Chipman stayed away from reviews by not increasing our budget. Ted wasn't going to have that, so he immediately had a review, and we had it in the old director's residence, which is gone. And Ted – let's see – I don't believe we had any PhDs on the staff – but anyway, he had masters and bachelors and good people, but they didn't have the certificate.

DW: I guess you're right. For that first review, no one – Tom may have just come.

DH: I can't remember.

DW: Tom Duke may have just come.

DH: I could remember if I took time, but I don't want to do that now. But anyway, he didn't have – I'll say right now, he didn't have any PhDs. We presented, I think, good research and the review committee saw that, but we hadn't been to Oz, and we didn't have the certificate from the Wizard of Oz that said we could do research, which is justifiable. I mean, no problem with it. They approved the stuff, but they evidently mentioned he should jack it up a notch. I think, within two years, we might have had five – Duke, (Shelsky?), Williams. Soon there was more than that.

Interviewer: Angelovic.

DH: Angelovic. There was more than that then within five years. I went back to school because I could see the writing on the wall, [inaudible] and I. So anyway, I think he was one hell of a director. A lot of that thinking is in retrospect when I can see the whole picture, but he certainly did a lot for me. Now, then, back to our game plan, you started to describe your career at the lab and how we had it good at the start. I think we did alright over time.

BS: More than all right. Really, more than all right.

DH: If you want to go on now and maybe describe some of your range of projects that you had and were involved in.

DW: I have a question that relates to that.

DH: This is Doug Wolfe.

DW: You both mentioned Francois Morel. Where was he in 1975?

BS: That's an interesting story. It's only peripheral. Actually, it is related to this lab because he and I had parallel careers and worked together for a number of years. My project was the relationship between cupric ion activity and the toxicity of copper in phytoplankton, basically, looking at the relationship between the chemical speciation of copper or its chemistry and its uptake and biological effects – biological effects mainly mean it was poisoning, toxic – copper [inaudible] toxic [inaudible]. So that was my thesis. What came out of that was an idea that the free copper ion concentration controlled the toxicity and controlled the uptake at high levels and this stuff that was bound up by, well, the humic materials up there in the Newport, or that we now know that, essentially, all the copper that's out there in the environment is bound up by organics, and [inaudible] available. Anyway, this guy Francois Morel had gotten (infected?) with the same question from Werner Stumm, but indirectly via Jim Morgan, who was Stumm's most famous student and was the co-author of this book, *Aquatic Chemistry*. So after doing a postdoc with Morgan, Francois Morel gets hired at MIT as an assistant professor. He hears that some guy is down there at Woods Hole working on the same stuff, so he calls me up and invites me up to give a seminar. I give my talk. Afterward, in his office, he shoves this proposal in front of me. I read it, and it sounded like the introduction to my thesis. I had just done what he had just gotten funded to do. So anyway, that was the beginning of – oh, and then, immediately, I got – since he obviously knew what my thesis was about and, quite frankly, nobody else on my committee did – they didn't understand the chemistry – this was all fairly high-level coordination. It's physical chemistry of metals, and these guys didn't understand that stuff, not really. So I got him on my committee. He was very instrumental in honing my thesis from basically something that might not have passed muster to something that got past the committee. Then we kept close contact. Then around 1981, this was, I think you were involved in this – Tom O'Connor, Kilho Park, called me up. We had already been – basically, we'd been working on a grant from AEC for five years. This is why I was talking about how good things were. It was a block grant to the lab. Everybody wrote a little piece. And we submitted. But it was almost like a rubber stamp. I mean, all we had to do was good work, and you could write anything. As long as you published something good, we were going to get the thing back again. And things don't work that way, not normally. We lost that. And I forget what the – and there was a bunch of (overlapping dialogue; inaudible).

DW: It did have to be related to theme that the –

BS: Of course, but you didn't have to do exactly the same thing.

DW: – that the laboratory and ERDA, the Department of Energy, were sponsoring. But you guys were right on, so you didn't have that problem.

BS: (inaudible) we didn't necessarily do what we said we were going to do. You might find out, after getting into it, that, in fact, this was really the wrong question to be asking. We ought to be looking at this over here because it's more central, so – but there was a kind of freedom that you don't see these days. I thought this was what science was all about, had no idea that I had sort of stumbled into this –

SH: Nirvana.

BS: – nirvana. Yeah, that's the term I was – anyway, after about six years of this block funding – you would always get it, and you had a lot of freedom – we lost it. There was some intrigue. (overlapping dialogue; inaudible).

DW: I think 1980 was the last year.

DH: Which year?

BS: '80.

DW: 1980. Fiscal year '80.

BS: And a fellow by the name of Tom O'Connor – you know him well – he heard that we had been doing all this research on the relationship between metal speciation – oh, and the reason why (overlapping dialogue; inaudible).

DW: I think Kilho Park, also.

BS: And Kilho. That's right. It was Tom and Kilho.

DW: Tom was working for Kilho in something called the Ocean Dumping Program.

BS: That's right. Actually, maybe I should get back as to why they even hired me here. Basically, this lab had built up an expertise in terms of the ecology of certain radionuclides, basically bomb fallout radionuclides. It turned out that most of these things are metals. There was manganese-54, zinc-65, cobalt-60, etc. Once they stopped the bomb testing, there was all this expertise here in dealing with these metals. It turns out that most of these metals are also toxic. They're heavy-metal pollutants. So I think it switched over. It didn't really take too much to go from following copper-60 – or not copper – zinc-65 when we're looking at zinc as a toxic metal. They realized that it was not just important to know what the concentration of these things was, but there was this issue of speciation. They knew that this [inaudible] thing, so they hired me, and then we were doing all this really good, pretty fundamental speciation work. This stuff is still highly cited. And Kilho Park and Tom O'Connor had money to deal with a bunch of dredge spoil out of New York Harbor that was being dumped in New York Bight. And a lot of this stuff was heavy metals. And they needed somebody to come up with the information on what do we need to know to protect the toxicity of this stuff, and whether it's going to end up in our shellfish or the fisheries organisms, so they came down, and they said we have this money. Are you interested in getting some of it? And I said, "Of course. Where do I have to sign?" And then they said, "Do you know anybody else who might like some of this money?" This sort of thing doesn't happen anymore. It really doesn't. I said, "Well, I know this guy up at MIT, and he's doing similar work to ours, and he's doing really good stuff." It turned out that we ended up going up to Francois' lab. We all met together, Kilho and Tom. Francois got up at 4:00 in the morning and wrote an outline, something I couldn't do. The outline turned out that Tom and Kilho looked at it and said, yeah, this looks good. That was that. We got funding for the next five years. Francois has always been grateful to me because that was the seed funding for his early work. This was the first grant he got. It was solid money. I think we each got something like $150,000 a year. Doesn't sound like much, but it was good money back then. That's what Sue was hired on. That's why Sue ended up as an employee for MIT. She was hired on that money. Because I was working with Francois, basically, they just shipped the money, set up whatever your salary was. I don't remember.

SH: Yeah, it's four thousand dollars. [laughter]

BS: They just shifted the money, so [inaudible] Beaufort, it went to MIT to pay Sue's salary.

DH: But you never worked up there?

SH: At MIT? No, I never set foot on the campus –

DH: Yeah, I thought you were here.

SH: – in that respect, no. And you never worked with anybody at the lab that collaborated with anyone here at this lab?

BS: Up there? Francois was always trying to hire me away from this place.

DH: I remember.

BS: I figured, if I went up there, I would be under his thumb. I wanted to be my own man, and I could do that here. Maybe we didn't have quite the resources that MIT had, but, better or worse, okay, I stayed. I rather liked it here.

DH: I hope so.

BS: Well, you stay at a place for thirty-nine years, you must like something.

SH: But didn't you collaborate with Pete Hanson or (Randy Ferguson?) or some of the other scientists?

BS: That comes later. Pete Hanson, I collaborated with Pete because Pete had a – we wanted to look at a relationship between, essentially, the chemistry of copper binding to, well, the organic matter in the Newport River, these humic compounds, and they had similar ones up in the Neuse River, so we collected – and Pete had an atomic source spectrometer, which he could use to measure the metals. I couldn't measure metals. I knew the chemistry. So he and I collaborated. It was a nice little project we came up with.

DW: Where was Pete then?

BS: Pete was here.

DW: He was already here?

BS: He got hired in '79 or something like that. I forget who hired him, but he was hired here.

DH: Bud.

DW: Bud.

BS: Bud hired him. And he was hired here as an analytical chemist for trace metals.

SH: And he worked with Randy on the GoMEx cruises.

BS: That was another set of projects. After we got money – and you were part of GoMEx, but you were – no?

DW: No.

BS: Not scientifically. Weren't you involved in the funding?

DW: I left here in 1975.

BS: No, I knew that.

DW: And it's really interesting that you bring up the ocean dumping program and Kilho Park and Tom because that program was established in approximately 1978. It may have been going on before that.

Interviewer: Oh, the ocean dumping?

DV: It was going on, I can tell you –

DW: Yeah, it was actually initiated, I think, in response to a congressional act that was passed in 1972. NOAA had a little piece of it in the National Ocean Survey, and they were doing circulation studies out in the New York Bight in support of that program. But then NOAA decided we ought to do more, and they hired Kilho Park away from the program I was in, Outer Continental Shelf Environmental Assessment Program, and made him director of a new expanded ocean dumping program, in 1978 or '79, somewhere in that, and Kilho hired Tom O'Connor, fresh out of graduate school. Within a year after that, it was 1980.

DV: [inaudible] doorstep.

DW: And what happened in 1980?

DV: What's that?

DW: What happened in 1980 in the federal government? We elected Ronald Reagan.

DV: I blocked that out in my memory. (laughter)

SH: He sent you a birthday card.

DW: It turns out that, in this 1978-79 time period, we were under the administration that never really quite got its act together scientifically under Jimmy Carter. In 1980, NOAA was reorganized. They reviewed the ocean dumping program and said, mm, this is science. Why are we doing this? They reorganized that whole program and established something called the Ocean Assessments Division. I got transferred into it. Kilho Park got transferred into it. And Larry Swanson was made the director of it. He was a NOAA Corps officer and the head of the ocean dumping program in New York, the New York Bight program. All of a sudden, I got transferred back to Washington, DC, instead of staying with the program I was with that had been commissioned to be transferred to Seattle. I ended up back there too. We all ended up switching – I mean, Larry got canned. Larry Swanson got canned too. We got shifted into a program under Bud Ehler, the Ocean Assessments Division. But it was the Ocean Assessments Division that was providing you funds through a variety of programs. I know because I was reviewing the proposals and making recommendations on (overlapping dialogue; inaudible).

DV: This is why I was saying you were involved but not here. You were basically up in management.

DW: For eight years or nine years, I was responsible for compiling the NOAA administrator's report to Congress on ocean pollution monitoring and research. This is the first one I had anything to do with. They were actually doing these for eight years before I got to Washington, DC. But here, on page forty-eight of the 1981 to '82 report, is a mention of our projects that we were supporting. One of them is here at the Beaufort Laboratory, and it's called A Quantitative Study of Mechanisms Controlling Biological Effects of Trace Metals in the Ocean. There's another one right behind it to [inaudible] MIT, Modeling the Effects of Wastes Dumped in Deep Ocean Gyres. Those are your programs that we're talking about. You're mentioned in each of the next eight years' worth of agency administrative reports. We summarized many of your publications in our program bibliographies, from Rockville and Silver Spring, so.

Interviewer: Well, the GoMEx program in Beaufort, that started [inaudible] –

DW: But that was only part of your research.

BS: Basically, there were two big projects, and they would bring a lot of money into the lab. Actually, Francois said that you guys – I probably shouldn't say this on tape – he says, "Those guys are taking advantage of you," because we were bringing in all this money, but it was not going all to us. It was being used for Pete Hanson's salary, for example, when he basically wasn't involved in any of this stuff anymore.

DH: No, let me get in here. The GoMEx program wasn't just a chemistry program. I went to the meeting when they organized it out at Boulder. And Swanson was still in charge, and that's the first time I ran into (Hal Sanford?), and he played a big part in it.

DW: Was that done through (MESA?)?

DH: I don't know. I can't remember that.

DW: It may have been.

DH: But the program included chemistry. It included all kinds of larval fish, transport in conjunction with [inaudible] all the stuff he did.

BS: It wasn't GoMEx I was talking about. It basically is an ocean dumping thing.

DH: Okay, well, that's different.

BS: That's different. It's okay.

DH: Sure, it's okay. We just all had a [inaudible] –

BS: The GoMex thing, basically, part of it had to do with really, once you figured out that speciation was important, well, you need to go out and measure it. Otherwise, what good is it? What good is this knowledge if you can't go out and actually measure the speciation? So that was for the next five years – actually, for the first ten years of my career, that's what it was all about. We came up with these bioassays because the available chemistry at the time was not sensitive enough or selective enough in order to measure this stuff, so we came up with these bioassays, which looking back on it, this was a bit insane relative to some of the sophisticated chemistry being done now. But that's where we were.

DH: I think you mainly went on (Ortner's?) cruises on GoMEx.

BS: Yes.

DH: Hanson, fortunately, or unfortunately, went on the NMFS cruises.

BS: What was he –?

DH: Well, let me tell you. On one of these cruises, I was, of course, working on larval fish, so I had to put on hairnets and coats and things and collect larval fish out of the nets and real quickly do whatever we did with them, wave a wand over them, and that's where they were being analyzed, I believe, for some of the techniques. It was probably too contaminated to be real, but maybe not. But we were providing the larval fish for trace metal analysis on those cruises. And he made one or two of those cruises.

BS: Ultimately, he basically got into what was the – the program for going out and looking at metal pollution here, there, and everywhere.

DH: Yeah, the survey stuff that they did around the country.

BS: What was it called?

DH: We'll think of it before this is over.

BS: This is the [inaudible].

DW: This was Tom O'Connor's Status and Trends program.

JS: Status and Trends.

BS: Status and Trends. That was it.

DW: [inaudible]

BS: Pete was in status and trends.

DH: He was in charge – Bud put him in charge of Gulf from our lab, in charge of Gulf – [Recording paused.]

DW: – a subset of that that NMFS was heavily involved with, this laboratory and the Seattle laboratory, called the benthic surveillance project, which was actually measuring trace metals and organic contaminants – same suite of contaminants that were being measured in the mussel [inaudible] program –

DH: After something like ten years –

DW: – but in fish.

M: – Hanson and I got – [Recording paused.]

BS: We were doing – using natural bacteria and their response to copper with these buffer systems. I won't go into all the details on any of the science because nobody will understand that. But we were using this to estimate copper complexation – not even estimate, measure it – in various environments in the Gulf of Mexico, going all the way from the Mississippi River plume, which is basically at very, very high productivity and still is, to the Gulf loop, essentially the Gulf Stream coming through, the loop of it coming into the Gulf of Mexico and looking at relationships between the level of complexation and the biology, the productivity of the system, the sort of organisms that were there. The most interesting thing that came out of that wasn't these bioassays. It was the fact that, in order to do the bioassays, it was known at the time that the old ways of doing things, collecting water, contaminated the water with all these metals, mainly from the hull of the ship. Anyway, we won't talk about all the sources. What we found out, actually, the controls in those experiments were probably the most interesting thing of anything, because we found out that, once we used these clean methods that avoided contamination, including amino acid contamination – we're looking at amino acid assimilation by the bacteria – that the assimilation numbers went up by an order of magnitude and that the turnover time for amino acids, for example, in the Gulf loop, instead of being a hundred days, which is what was in the literature, was now – it was one day. There were definite relationships between the assimilation of the bacteria and the sort of ecosystem you were in, so that, actually, out here, we did some experiments off the dock, and the turnover time was like twenty minutes. This was in August when this place is cooking. Anyway, Randy and I wrote a paper together and published it. I thought it was a really fabulous piece of work. I thought it was much more interesting than the bioassays. I still think it's a great piece of work. And it's been ignored. Biologists never wanted to be that clean. It just wasn't in their nature. It wasn't the nature of somebody like Pete Hanson. I won't name any names, but there's a number of people – actually, some of them are in this county – that just never wanted to be that clean, so it was ignored. They kept, instead of – with the trace metal stuff, once they figured out that they were contaminating all these samples, what they were able to do was actually show that there were profiles of these metals, and the metals – things like a zinc profile looked like a phosphate or a nitrate profile. These metals actually were nutrients. Ultimately, what came out of that was that there was at least a third of the ocean where the ocean was iron-limited, and the iron is controlling the biology. Not only that, it's controlling the biological pump, which basically was transferring CO_2 from the atmosphere to the deep ocean. All that came from the fact that the chemist and ultimately biologists – if you were a biologist and still if you're a biologist, and you're not using clean technique, forget it. You can't get anything published. In fact, you're not even doing useful work. That never happened with the amino acid group. Recently, we published some stuff – this was after you left – it was the stuff that I did with Rance Hardison – and we showed that the levels of ammonia that actually limit things are down on an animal range. Let's see. They're ten times lower than what people actually measured. When you actually use clean techniques to measure things off the dock, you find out all the old measurements were bad. That's still the situation right now. And I just met with a guy over – Nathan Hall, who's over at IMS [Institute of Marine Sciences], and he did another comparison and showing that the standard methods that everybody uses, including Hans Paerl, with his ModMon [Modeling and Monitoring Program], but all that stuff, basically all the low stuff, is garbage. He will publish, okay? That's why I went over and met – we need to do this. But I have a feeling that that's not going to be like the trace metal thing, where people will actually take on the new techniques and make proper measurements because there's an awful lot of inertia in science. Oftentimes, it's hard to break through that. Now, I shouldn't be rambling because that [inaudible].

DH: No, no. Rambling's good. I love rambling. I'm quite good at it. I want to go back to the Oregon II cruise. But then we're going to shift a little bit. We went to a lot of trouble to try to be clean. You've been on the ships. You know how we tried to set up a clean – I don't know if you were ever on the Oregon II, but we had a, quote, clean room and a hood. We even went off in small boats, eventually, as we learned about, well, hell, everything we're getting's just flaking off the ship. We tried that. But it's extraordinarily difficult on a ship that's not set up to do that. This was set up to be a trawler, exploratory fishing vessel. We'd converted it over the years into a decent biological sampling thing. But for things like you're talking about, it was really hard. You could have me – I looked like a clown with my baker's hat on and –

BS: We all looked like clowns.

DH: – various stuff –

SH: Still do.

DH: – running around this ship, and I was probably stirring up more trace metals in the –

BS: Yeah, the hairnet.

DH: Yeah. Anyway, that's exactly the kind of – I think this is a great interview. But I want to ask – this question comes from Bud. I'm going to read it so you don't hit me. But can you explain –

BS: My arm's not that long.

DH: [laughter] Can you explain how you were able to manipulate the system for both of you to keep research funding in salary? I think you've alluded to how, quote, the system was manipulated to some things that, when we had to start using other funds than AEC, funds were – I don't think Bud – one of the things I appreciated about him was that, if larval fish money was down, we still did larval fish work. If your money took a sink, he'd find a place to make it work and through Doug and other people, but it was a good place to work because no good project was canceled just because NOAA had a three-year-only funding program. You could fund the biggest project in the world well for three years. You didn't have the answer because you knew at the start it was half a lifetime was needed to solve it. But somehow, we kept most of the programs running, but I think you've addressed part of that question, but I loved it when I saw it. Another one he had was, do you have any thoughts on the most satisfying aspects of working at the lab? Anything that comes out? You stayed a long time. It was just a good place to work, maybe, for the most part.

BS: There is no perfect place to work, ever. So yeah, I had my complaints. I would complain to Margaret, my wife. She'd just say, "Shut up. You don't know how good you have it."

DH: Well, you weren't immune to coming to me. [laughter]

BS: But I would go visit my colleagues at universities to do some research. These people were running around like chickens with their heads cut off because they were constantly having to do this, that, and the other thing. I'd look at those folks, and I'd say I couldn't do this because it's almost like a three-ring circus. At least here, you could pretty much focus on – you could never, ever do exactly what you wanted because you're always basically – somebody's giving you the money, and they have a certain interest, and it's hardly ever that the two exactly mesh. The best thing, towards the end there, so we were doing all this great process stuff for quite a while, and we got a lot of good papers out of it. All this stuff is still highly cited. But towards the end, we got into I guess it was Status and Trends, and they were interested in mapping pollution. So we were running around basically measuring [inaudible] concentrations here, there, and everywhere. We were mapping zinc concentrations. But we couldn't do any real process work. I remember telling Tom O'Connor if we measure particulate metals, we could figure out how the metals could get with particulate settling from the water down the water column and end up at the bottom, and we could formulate some interesting hypotheses. I won't put this on Tom. But somebody there said we're not interested in hypotheses. That floored me. That really did, because in science, if you're not interested in hypotheses, you really aren't interested in asking questions, and if you're not asking questions, what are you about? So I was basically, towards the end, chomping at the bit, having to go around and monitor pollution, even though I was monitoring sort of things that were interesting you couldn't really measure that well before. We had figured out chemical methods for measuring [inaudible] concentrations. I get this call. This was like 1989, summer of '89, and – no, it was the summer of '90. And I get this call from Ollie [Oliver] Zafiriou. He's somebody I knew from my graduate school days. He's at Woods Hole. He's a photochemist. He said, "How would you like to be a rotator at ONR (Office of Naval Research?), go up there and basically help us run the program for two years. You know what rotator [inaudible] you did that, sort of – maybe you didn't.

DW: Kind of.

BS: Bud did. I said, why would I want to go up to Washington and be a bureaucrat? I mentioned that to Margaret. She says, "I think you ought to reconsider this." So Ollie calls up a month later and says, "Bill, was there ever a research project you always wanted to work on or a piece of equipment you always wanted to buy?" I'm thinking – because basically, this is 1990. Two years before that, John Martin, who's now dead, but he's the father of this whole iron hypothesis that large sections of the ocean are actually being run by iron limitation – they had published a paper in *Nature* showing this, and this stuff was – but I'm looking at this, and I'm stuck basically mapping trace metal pollution up in New York Harbor and Boston Harbor and New York Bight. I'm thinking – and I said, "Well, could they fund my lab?" He says, "Sure." So what ended up happening is I went up there nine months a year, and they gave me money to pay Sue's salary –

SH: And Maria's, I think.

BS: – my salary, Maria Bondura. Essentially run the lab. I could do whatever I wanted. No strings. It's like one hand washes the other. They were good at that. That's important. You basically are asking somebody to do something for you. You do something for them, and everybody's happy. So I got ten years of funding out of that, and that was the most productive ten years of my whole career.

DW: And that started in 1990.

BS: That started in 1990. In fact, actually, January of '91. I would plan experiments. I would send the stuff down. Did we have email then? I'm not sure.

SH: No, we didn't have email. No. We'd talk on the phone here and send it to –

BS: We'd talk on the phone, or I'd put it in the mail and send it down. Then Sue would execute the experiment. She'd send me back up the data, and we'd talk on the phone. [inaudible] research for two years by remote control. Of course, it's not worse than these university professors, who basically – they're being paid to teach, and then they have to go to all these meetings, and they have to sort of do the research on the side. It wasn't any worse than that. And then they continued to fund me for years. And after that, I came back here, and people would say – oh, you might say, "What are you doing?" "I'm doing ONR research. They'd say, "Okay." That went on for about ten years. It was sort of like I was playing a shell game. You know the shell game, where's the pea? I got away with it for another ten years. Then, basically, they sort of cornered me and –

SH: But even during Status and Trends days, our research continued. We didn't just do status and trends monitoring during that period of time. Our work was cheap.

BS: We used ONR of our funding to do all that really fabulous stuff on DMS [dimethyl sulfide?] research.

SH: DMS, yeah.

BS: And DMS turns out to be – basically, it’s a giant feedback between algae, which produced DMS, and the DMS goes into the atmosphere and oxidizes sulfuric acid, makes clouds, so there’s huge feedback between these algae, making this stuff and basically clouds and planetary albedo and rainstorms and all that.

SH: And acid rain too, yeah.

DH: I've only got a couple more, but do you have one particular research that you did, like a paper or something, you two, or that you think is the peak of your career?

BS: The best?

SH: The piece de resistance.

BS: What’s that?

SH: The piece de resistance, the absolute ultimate paper.

BS: There was a nice paper that was published in ’97 in *Nature*, where we showed that light limitation and iron limitation were totally linked and that, in fact, it was [inaudible].

DH: The what light?

BS: Really, what it comes down to is, if you look at where the – all these algae are limited by iron. If you look at where all the iron is, it’s in the photosynthetic apparatus. It’s basically in the machinery of photosynthesis and all these proteins and protein complexes that are in these – that participate, and it’s almost like a ballet in converting light energy into chemical energy. It turns out that, when you start – so that iron and light have to be linked, and when plants don’t have enough light, they have to be able to make more pigments, but they can’t just do that, because it makes the – basically, what you have is a bunch of pigments, and it’s [inaudible], and it’s funneling energy down to what’s called a reaction center, which [inaudible] the light energy and doing photochemistry. You could basically collect more light by having more pigments. But then you end up with a leakage of the light energy, the captured light energy, called excitation energy, into heat, and the process would be inefficient. So basically, what it turned out is the organisms, the plants, the algae, have to make more photosystems. For that, they’d need more iron, so that light limitation and iron limitation are totally linked, that those are actually a colimitation.

DH: Where was that publication?

BS: *Nature*.

DH: *Nature*? That’s right.

DW: Bill, how similar is the DNA map for those iron pigment proteins in phytoplankton to that for hemoglobin?

BS: Go one step back. Why hemoglobin?

DW: Well, because it's an iron pigment protein. I'm just curious.

BS: Oh, okay. The various proteins that contain iron are quite different.

DW: Yeah, okay.

BS: So one of the nice things about iron – and I don't want to get into it like a biochemistry lecture – the whole thing is an unbelievably fascinating story because the reason why we're sitting here doing this is because we're breathing air or breathing oxygen. Without oxygen, you don't have oxygenic or oxygen-consuming respiration, which generates an awful lot of energy, so that, back when the phytoplankton – when photosynthesis evolved, and it evolved in these single-cell – they weren't plants (inaudible) bacteria, cyanobacteria, there was no oxygen. A lot of iron around because the ocean didn't have oxygen – what's called reducing, sort of like when you dig down in sediments out there. All that black stuff, that's iron sulfide. Anyway, there's lots of iron around. A lot of the primitive proteins that basically transfer electrons – everything in photosynthesis – it's chock full of iron. Why? Because it's a metal that, depending on what you bind it to, you can tune its redox potential through the whole span that you would find in all organisms, so it's extremely useful. It's very efficient. So iron ended up being basically used in the whole photosynthetic apparatus. These proteins, these systems that were put together, a lot of the stuff evolved only once. In photosynthesis, there's two photosystems, photosystem one, which is a primitive one, and it's got like twelve irons in it. These are all iron-sulfur proteins –

DW: Actually, this was the basis for my question. I was actually trying to get back, relating this to the (redoxin?) thing, where energy transport systems and energy capture systems are ancient, and I was just wondering if our hemoglobin protein structure was related to any of those protein structures in the energy capture system in photosynthesis –

BS: The answer is probably, but not exactly.

DW: Oh, yeah, of course. There have been five hundred million years of progress.

BS: Right. So the fact that iron is used to bind – in hemoglobin to bind oxygen, there's redox chemistry going on there.

DW: For sure.

BS: Iron is just very, very efficient. There's an iron protein which is replaced with a non-iron protein. It's ferredoxin and flavodoxin. Flavodoxin basically is used – it's used by organisms that are from a very low iron environment. The problem with the flavodoxin, it's twice as big, so it takes twice as much carbon, twice as much nitrogen. That's inefficiency. So if an organism has the iron, it's not going to – basically, it can sort of save on its carbon and nitrogen budget by using iron, and so iron has just been – it's such an efficient redox catalyst that it was always used in all sorts of these redox reactions, whether they're in respiration, photosynthesis, oxygen transport, a whole raft of things. It's central in biology, and yet the plants ultimately liberated oxygen and caused all the iron to go from soluble iron II to insoluble iron III. All the iron ore deposits around the world, that's all iron oxides that basically precipitated out of the ocean with the great oxygenation event, which I think actually (inaudible) this whole oxygenation thing went on for billions of years and didn't become high levels of oxygen until about six hundred million years ago. And at that point, you get an unbelievable amount of radiation in biology going from single-cell organisms to multicell organisms, ultimately to land plants, mammals, and us. But that all happened really quickly, so there's this very interesting relationship between the evolution of biology and the evolution of earth chemistry. The two are feeding back on each other. This I find absolutely fascinating. That's why I'm still in science. That's why I'm still doing this stuff. And you can actually –

DW: You told us about the ten years between –

BS: Oh, sorry. That's right.

WOLFE: – 1990 and 2000, when ONR was funneling money here. You were here, though, for –

BS: What happened afterward?

DW: – eight years after that or so. Who was providing the funding after that to finish Bud's question about how you manipulated the system to do all this great stuff?

BS: The system also manipulated me.

DW: Yeah, of course.

BS: It always does.

DW: What happened to the ONR funding? Did it dry up? Did it end?

BS: Yes. What happened was the demise of the evil empire. When I was at ONR, the Soviet Union was still the Soviet Union. They had their ballistic submarine fleet. We had ours. We were terrified of each other and distrustful of each other. We would leave no stone unturned – I say we – ONR would leave no stone unturned if they thought there was something out there that could give us a one-up on the Russkies, or they might use something to get one up on us, so they did all this fundamental research so they could understand the ocean physics, chemistry and biology well enough to be able to hide from their surveillance systems and then have our submarines hide from theirs. There's a photochemistry research initiative. I was in that in the '80s. We got money, actually with – and why was that there? That was there because ships going through a fluid, the ocean, would generate superoxide, which generated hydrogen peroxide, so every ship was giving off a wake of hydrogen peroxide. They needed to know what were the background sources of hydrogen peroxide. Nobody in that project, nobody that got funded from that stuff, including us, knew why they were doing this, the trace metal stuff, partly because ONR funded really a lot of the development of the techniques for measuring the [inaudible] trace metals. Zinc wakes – every one of those ships had zinc sacrificial anodes, which basically – and they were saying, well, there might be zinc wake. I don't think it ever panned out, but they had to look into it. They wanted the best. This really was national security stuff. Most people had no idea. I did because I was up there. And after that, in – let's see – 1998, we went from National Marine Fisheries Service, and everybody here is well aware of this, to NOS [National Ocean Service]. I saw that, and it made me very nervous at the time. I think probably subsequent events (inaudible) that I was probably right in being nervous because two things happened. The lab was basically the Center for Coastal –

DH: Estuarine Research.

SH: Coastal Estuarine [inaudible] research.

BS: The important thing was coastal. Iron limitation was not an important thing in the coast, nor was zinc or any of this other stuff. So if I was going to work on stuff they were interested in, which I was, if I wanted to get a salary, I had to give up all the trace metal stuff and ended up actually working on harmful algal blooms.

DW: On what?

BS: Harmful algal blooms. That was the stuff where we came up with some ideas on harmful algal blooms. Quite frankly, if you try, as an outsider, to come into an established community of researchers with wild ideas, you're not going to be accepted.

DH: You're not going to be popular anyway.

BS: So we came up with what I thought was pretty interesting stuff, and it's still not accepted. I've even wondered, "Why the hell did I do this?" There's an awful lot of sociality that goes on in science that maybe is as important as the intellectual stuff, the thought process, and the experimentation. Anyway, that should be about it.

SH: Yeah, except for the trying years when the ecology wing was falling down, mold in the ceiling, and we had to work out in the radiation building, which was absolutely foul.

BS: This was a sad turn of events because when we had the best funding to work on whatever we wanted, the lab was under construction.

DH: Reconstruction.

BS: Yeah, reconstruction.

SH: We weren't physically kicked out. We still kept our lab there, but everything was going on around us.

DH: Yeah, that was a tough time.

BS: It was a tough time.

DH: But it had to be done, evidently. I had a lot of problems with the way it was done –

SH: And the roof (inaudible).

DH: – and what I lost. I couldn't have saltwater where I wanted it and things like that. But again, you may have been against my saltwater plan because it affected your allergies, probably. Anyway, the endgame was, in my opinion, I agree with you now, but the switch to NOS, at the time, Fisheries had – Brad Brown had said, essentially, get out with your work. Now, whether he could have achieved his goal of cutting us, I don't know because it's very hard to do away with federal employees. But at the time, we were in good grace with Nancy Foster. Had Nancy Foster still been alive, the story would have been different. But the truth of that is too don't put your money on one horse because the poor lady died four years later.

BS: How could we know that our horse is going to break its leg coming out of the gates almost?

DH: But from the time we got in there, I was in trouble with the people I had to deal with.

BS: It was a tragic –

DH: We didn't get along, quite frankly. Anyhow, that's the end. One last thing, do you recall any particularly humorous events that happened while you two were working together because I can. So if you don't say anything, I have a book full. [laughter]

BS: But you'll be limited by the time constraint. You only have five minutes.

DH: Exactly. Anything really? Let me just refresh your memory. I came to work – this was while you were doing GoMEx. I wasn't lab director, but I was division chief at the time. I came to work one day, and Bill had left for a cruise with [inaudible] in Miami. He left the day before. I never used the elevators, so this could have gone on for a longer time because most people didn't use the elevator. But I had something I had to carry up to the second floor. I usually would throw things on the elevator and take the steps. I opened that door. And from the base to the ceiling was full of equipment that Bill had not taken with him to go on the cruise. Do you remember?

BS: No, I don't remember.

DH: One of you ought to remember it because we were panicked because we had to get –

BS: Was it finally gotten there?

DH: Oh, yeah, so you wouldn't have remembered because it – but we had to go through a lot of –

BS: I’ve always been absent-minded.

SH: Travel orders, passports, bank.

BS: I’ve got a story on myself. But the thing is, right now, I’m seventy-two years old; now I forget things. But I have to remember that I have always been forgetful. So how do you know if you're getting Alzheimer's? I've got one. And actually, Bud would be able to tell this if he was here. I was going to a meeting in England and flying out of Kinston. This was back in the days where you needed traveler’s checks. This was like – I don’t know – ’81 or ’82, ’83. So I go into the bank to get my travelers’ checks. I had my briefcase, which I’d never taken to a bank, but I did. It had my passport in it. I got the traveler's checks. I guess I must have left the briefcase at the bank because when I got out to the Kinston airport, and I opened the trunk – nothing. No briefcase, no passport. You can imagine; I was just aghast. I knew damn good and well that, at best, it’s a three-hour round trip to go back. Needless to say, I missed the flight. I had to call up Bud. There’s another little story.

SH: Bud loves these stories.

DH: I remember the [inaudible].

BS: I was out at this meeting in Sicily.

SH: Oh, yes. This is the one I was going to remember.

BS: It was a trace metal meeting. Afterward, there was a guy I met there, [inaudible], and he said, “We’re having this meeting, small meeting, in Plymouth. We would like you to come and give a talk.” I said, “Well, I guess I can do that.”

DH: I’m there. I’m here.

BS: Afterwards, I was going to basically do a two-week trip in Italy, taking the train up the boot. I said, well, yeah, I can basically take some of that time and go over to – I think it was a half a week we did this – fly over to London, take the train down to Plymouth and then fly back to Rome. Anyway, I called up Bud from Plymouth. I told Bud what I’d done. He said, “You’re where?” because, well, you know, basically – you were a lab director – everything is supposed to be on there, as far as your itinerary, and you're not supposed to ad-lib and just pick up –

DH: That made it interesting. But I remember a few things like that that I’ve done too, so you’re not alone. You wander off somewhere. But one little quickie, You may always wonder – and I doubt if anybody in this room does – why I don’t scuba dive. This is one you won’t [inaudible], but even though I’m eighty, I have a fantastic memory for trivia. When you first came, you scuba-dived here. In fact, there weren’t too many people that did. And there wasn’t any – well, there was hardly any of the safety stuff.

BS: None.

DH: Well, I talked to you and –

DW: Oh, come on. Pete had already set up the lab's dive program.

DH: But not much.

BS: The answer is if you were diving for NOAA – I never dove for NOAA – absolutely. He was a former SEAL.

DH: But even then, it was a lot less than it is now.

BS: Yeah, a whole lot less.

DH: Anyway, I said, "Bill, would you show me how to scuba dive?" "Sure." We got the gear out. You showed me how to put it on. You probably said something, I don't know what, but there used to be about a thirty-foot hole, you could walk right off the beach –

BS: Over here.

DH: No, over there. It was on the shallow side, thank God.

DW: Oh, was it one of those dredge holes?

DH: Probably.

DW: Where they dredged the fill behind the seawall?

DH: So we went down, and I was down there. I said this isn't too bad.

BS: So you and I were diving together?

DH: Yeah. I'm sure it's so small an incident that you're not going to remember this. But anyway, one little thing you didn't tell me was you always should breathe when you got this stuff on –

BS: I must have told you that.

DH: I could have forgotten it but –

BS: You forgot it because basically that's the first rule in scuba diving.

DH: Yeah, well, it didn't stick. When I came up, I evidently didn't breathe.

BS: You were holding your breath coming up?

DH: I must have been because my facemask was full of blood. My nose was bleeding like crazy.

BS: Oh, my god.

DH: I had one hell of a headache, I'll tell you that. [laughter] I said, "Probably I'm not going to be a good diver." If you told me, I forgot.

BS: You forgot.

DH: Why would I hold my breath? I don't know, but I wasn't always –

BS: Well, when you're snorkeling, you hold your breath.

SH: You do hold your breath then.

DH: But anyway, it kind of killed my interest. Anyway, I think, Joe or anybody, you got any other thing you want to say? I think this has been a really interesting interview. So thanks for coming. Thanks for doing it.

BS: I've enjoyed it. How often do I get to spout off on people and have actually a captive audience? Now, if my wife was here, she'd be out of here.

DH: [laughter] Well, you finally [inaudible].

BS: As she says, I was probably lucky to come here because I'm not sure I would have gotten tenure if I'd gone to – I'm sort of a maverick.

DH: I noticed that.

SH: You certainly didn't want anything administrative. You avoided that like the plague.

DH: I think that's great, and you added a lot to the lab that wouldn't have been there from a lot of different directions, but you added a lot, I think.

BS: This has been a –

DH: Are we done? We have to cut it, but thanks.

BS: I give a lot of credit to Ted because he allowed a level of freedom that you often don't find in federal labs. In fact, this was one of the few federal labs I would have come to.

DH: Anymore.

SH: Yeah. And even this one now.

DH: This one had the lead, I think, in being – a lot of independence.

SH: Yes, sort of an outpost. Nobody bothered because no one knew we were here.

DH: Okay. Well, I think it's running out.
-----------------------------------END OF INTERVIEW------------------------------------
Reviewed by Molly Graham 3/3/2022