Joseph Smith: Joe Smith here. We're at the NOAA Beaufort Lab on Pivers Island, in the library. It's Tuesday, March 13, [2018], about 10:30 in the morning. Today, we'll be interviewing Drs. Don Hoss and Bud Cross, both former directors at the laboratory. Dr. Doug Vaughn's present and myself. I think Doug and I will be doing the questioning today.

Douglas Vaughn: Yeah.

JS: So we'll start the interviews. Gosh, first things first, Don, how did you end up at the laboratory, coming from the Midwest to one of the easternmost places on the East Coast?

Don Hoss: I needed a job real bad. I had graduated from the University of Missouri with a major in fisheries. But it was, of course, freshwater fisheries. I applied everywhere in the world. Most people in the States were already taking master's candidates if they could get them, and I just had a bachelor's at that time. I knew of a lab in Pensacola, and that's where my aunt and uncle live, and I applied there. Good thing I didn't get it. It turns out the guy was pretty hard to work for. But he referred me to the Beaufort lab because he knew Walter Chipman at the radiobiology lab. He referred me to Chipman. I'm almost positive that Chipman hired me because he went to the University of Missouri for his PhD back in the Depression, and he wanted to tell me about eating rabbits all the time. Not only that, but his wife was from Auxvasse, Missouri, which is about fifteen miles from Mexico, Missouri, where I was born. And not only that, the previous person that I was replacing was from Mexico, Missouri. So while I had no qualifications, when they said radiobiology lab, I said, "Well, I don't know anything about radios, but I'm a halfway good biologist, so I'll take a chance at this job." It turned out to be the best decision I ever made in my life because everybody was good to me here. I had a real, I think, good career at the radiobiology lab and, later on, various labs. The way it was formed here at Beaufort, I got to go away a couple of times. I'll tell about those later if opportunity presents itself. My memoirs are going to be *GS-5 to GS-15: Forty Forty Years* at the Beaufort laboratory. And so it was really good. Maybe Bud – before I get into too much.

DV: I had one question before we go to Bud. When did you start here?

DH: August of 1958. I didn't have a car. My dad and mom and grandmother and brother all crowded into a Chevy, and we drove down here. You couldn't get here – I could get as far as Rocky Mount. People still traveled by train a lot then. I was going to go to Rocky Mount. But then I still had to close in that two hours from Rocky Mount here. Chipman was very helpful. He told me things to expect and all. But the first six months I had to hitch a ride to the lab is the only time I lived in Morehead, but there were some secretaries and things that would pick me up every morning, and somebody would always carry me home, so it was interesting. I want to tell later how different the lab was and it became.

JS: Bud, you're another Midwesterner. I know you went to the left handed coast to school. But then how did you wind up back over here?

Ford Cross: It's all a case of serendipity. Like Don, really, has got some serendipity in his links to Mexico and Missouri and how it might have led to the lab. In my case, I was a psychology major in college and changed over. I took an embryology course by a young scientist, young

PhD, and she just really turned me on to science. At that time, it looked like the most beautiful, wonderful thing in the world, it was so logical, so I switched over to biology, and I've always been interested in the outdoors, and so when I finished my degree in biology at Mt. Union College in central Ohio, I applied to a couple of schools for master's degrees, one of which was Duke. They turned me down. I never did get on [inaudible] about that. I had a very weak science background because I had not been in science the full four years, so I ended up sending a letter to the University of Oregon. They had marine biology. I didn't know the difference back then between marine biology and oceanography, so I said, in this particular letter – this is where serendipity comes in – by chance, I used the word oceanography. I'm interested in oceanography. Well, about two weeks later, I get a letter back from Oregon State, not the University of Oregon. The letter said, "Well, your letter to Oregon showed your interest in oceanography, and so it was forwarded to us," so I get a letter back to apply to a school I didn't even apply to or didn't even write to. I wrote, and I got accepted and went out there and started four and a half years of masters and PhD work in oceanography. My master's dealt with plankton copepod distribution off the coast. Nothing had been done at that time in that area. But when I finished my master's, I knew you couldn't make a living taking a fifth leg off of copepods, a 1.2-millimeter copepod, so I changed over to radiobiology. Columbia River was dumping all kinds of radioactivity into the ocean from the Hanford plant, and so I went in with a major in biologic oceanography, and my major professor was Charlie Osterberg, who ran the radiobiological program. And we were able to use the radioactivity in the river as tracers, and there was a tremendous amount of research that was done in the Columbia River estuary and offshore. When I got ready to work, there were really only two places where I wanted to work, when I was ready to leave, and one was Frank (Loman's?) Puerto Rico nuclear center and the other one was this laboratory. I sent out two letters, one to each place. Within about ten days, I heard back from Ted saying send me an application. I'm still waiting to hear from Puerto Rico. [laughter] I finally gave up and retired.

JS: And what year was that Bud that you came here?

FC: I came here in February of 1967, so there were still two laboratories here when I came. Then we were both here for the merger into a single lab, I think, in '69 or '70. I will say for the record that, when that occurred, this is when EPA [Environmental Protection Agency] was formed, in 1970, when Nixon did so much reorganization. And we had a surprise visit one day from a bunch of EPA people who came here and went through the lab. Ted wasn't even here then. He was on travel that day. And they very seriously considered taking over this lab as an EPA laboratory. If it had not been merged into two laboratories the year before, they would have taken the radiobiological separate laboratory as their own. That was their plan. But when they got here, they found out the labs had been merged.

DH: Well, close call, in retrospect.

JS: Back to Don, Don, how about a brief summary of your work experience here at the lab, programs you worked for, and how you progressed up the GS ladder, like you said, starting at a GS-5 and ultimately winding up as lab director?

DH: Where to start? When I got here, like I say, I was a freshwater biologist. And Dr. Chipman was a shellfish person in reality, and the lab was in two pieces. One was a special shellfish investigation, which was the radiobiology lab, and the other was what we think of as a fisheries lab, a traditional fisheries lab. Well, of course, his main interest – although he had just begun to work on finfish in the radiobiology lab, his main interest was shellfish, and so he put me with Jack Price and John Baptist, who at that time were doing oyster and clam to work on the uptake of radionuclides. Like I say, all of this was brand new. I mean, you were training somebody who was really green. They said, "Well, go out and collect some clams." I said, "Right, collect some clams. Where in the devil do you find clams besides a store?" And so Gray Roberts, who was a technician and a wonderful person – Gray Roberts took me under his wing, and I learned how to do fieldwork like that, like collecting what I needed to work on. I stayed working on shellfish just as briefly as I could because I knew less about shellfish than about fish, for sure, so I managed to wiggle my way into – John Baptist was switched to being head of a fish program, uptake of radionuclides, and distribution, etc. in fish, so I spent the next few years in John's project, working on radioactive uptake in fish. That went on for a number of years. I'm not going to say how many. At that time, we'd had a review here. The AEC [Atomic Energy Commission?], who was providing half our money or more – I don't know –

FC: Pretty much, yeah.

DH: – on the contract – told Ted Rice he didn't have enough [inaudible]. We did a review, and people like me, who were reviewed – I thought we did pretty good. But the review said we didn't have enough PhDs. They'd be glad to fund us. We were doing fine. But you had better get some PhDs. Well then, three years, Ted had picked up about four excellent PhDs that knew what they were doing, one of whom was Tom Duke (sp?). I ended up in his division doing the fish work on that and progressed on in that deal. In the meantime, somewhere along there, Ted came in to me – I was the youngest person at the lab by far. A lot of people were World War II veterans at that time. I was by far the youngest, quote, biologist. Ted came in one day. I was working in the old turtle lab, which was our radio lab, and he said, "Do you want to go back to school? The Southeast has a new program that's come up." Well, I was floored, so I ran home at lunch and asked Carolyn if I wanted to go to school. She said, "You damn well better." So I did and went to NC State and got my master's there. It was paid for by the government. I got leave, and my job was waiting for me. I may come back to this later, but later on, I realized I also needed one more thing. I needed a PhD. I took a leave of absence, which the lab granted me, and theoretically could provide my job when I came back. I had to get money to go through, like a graduate student. I had to get money from somewhere, and I did, so I got my PhD. But I credit the lab, and I credit Ted for allowing that to happen. We'll come back to that. But then, over time, we evolved the radiobiology part, which was a great relief to me. I never felt comfortable around it. We got into more ecology, ecology of fish in the sea and pollution problems, like coastal power plants. And at one time – it's hard to believe now that there were plans and diagrams, and we worked on it, on offshore nuclear power plants. They were going to be floating inside a dyked in area. Went through that period and then finally got to the love of my life, which was larval fish ecology. That's where I spent the rest of my time, running a team in that, and then eventually, under – Bud had become director by then. I replaced him as division chief and eventually, out of sheer longevity, became lab director for a number of years. It was a great run. I enjoyed it very much.

FC: You're not going to tell them about the time Ted caught you with your feet up on the desk?

HD: [laughter] That was Chipman, actually.

FC: Oh, it was Chipman? Okay.

DH: If that had been Ted, I'd have been fired. I was relaxing in the new wing, our new offices. We'd been chased out of the old wooden turtle building we'd been using. We had offices in a new wing on the brick building. I was doing paperwork and reminiscing and thinking like I liked to pretend I was doing. But I did have my feet on the desk, and Chipman came by. He was a very nice man, but he did believe in work. Feet on the desk wasn't a – yeah, I never did that again. [laughter]

JS: Bud, when you arrived, you had PhD in hand, I guess. Is that right?

FC: No. I had not finished. I got to the point – I was just in a rut, and I called Ted and said, "I'm not finished, but I'd like to come here." He said, "Come on, and we'll get you started, and you can finish up here." So I stuck all my thesis stuff right under where I sat in the car, and it stayed right under me until I got here. When he told me – he says, "Go on in the room, shut the door, and finish your degree. You're no good to me until you do. Meanwhile, go around, meet the people, get some idea what you want to do." When I finished up, I picked a project, and the other folks thought it was fine, and I got started. So at the time, I had come here to work on radiobiological problems. Oh, by the way, my book – he's got his book named; I've got my book named – *Confessions of a Laboratory Director*. It's kind of like [*Fifty*] *Shades of Grey*. [laughter] But when I came here, there was a transition occurring. It was finally determined that nuclear power plants were not going to release radioactivity unless it was an accident, but they were going to release hot water in many, many cases. There was a lot of concern about disrupting the local environment around these plants. At the same time, the concept got really popular that the government was going to build nuclear power plants right offshore, all the way down the coast, all the way around, and they were going to sit offshore, these great big rock structures. The seawall is protecting them, and that's how they were going to do it. This was really serious. The marine budget at the Atomic Energy Commission got quadrupled in '73, and they wanted to start funding ecology and understand the impact that these might have in all kinds of ways, these offshore nuclear plants. And so a lot of that work transported into basically ecological work, much like ONR [Office of Naval Research] and other agencies [such as] NSF [National Science Foundation] was funding. We were able to make that transition with them here at the lab, where we went from primarily an entire radiobiological lab and went through a three or four-year transition where we moved into nonradioactive areas. I, for example, worked on stable trace metals – zinc, iron, copper, manganese, and mercury – for about seven years. Then I spent a year at the Atomic Energy Commission handling research contracts for them. My major professor was head of the environmental division up there and asked me to come up and spend a year. So then I came back to the laboratory, and our division chief was Doug Wolfe. Shortly after I got back, Doug decided to take a job in Boulder, Colorado, with another part of NOAA. And I ended up then being – in '75, I became the ecology director, which pretty much put a kibosh on research. And I was ecology director for ten years and did some summary

papers. But what happened during those years was a tremendous transition that we had to make to basic ecology. Dick Williams was here and just getting the computer modeling and doing a lot of work with spartina. Don began the [inaudible] fish work way back then, instituted it, and built a program there. Gordon built a seagrass program. All of these turned out to be really high-quality programs, very productive because they were built – I'll get to that in a minute – on soft money, and if you weren't productive, you didn't get your money and have a job, so people really had to work in a university setting. In fact, we were often called the federal university by our non-supporters in, at that time, the NMFS [National Marine Fisheries Service] system. Ted retired in '85, and the story of how I became lab director is pretty interesting because Dick Berry was the director of the Southeast Fisheries [Science] Center, and he says, "I'm not selecting you. I want new blood in there. I want somebody else to be director." He said, "So I'd like you to take the Charleston lab." I said, "I don't want the Charleston lab. I want Beaufort." Well, I found out that he had offered (Bob Kifer?) the Beaufort lab and me the Charleston lab. We really belonged at the opposite one. I don't know what he was trying to do. So we just held our ground, and Dick finally threw up his hands and said, "Okay." It took a year and a half to wear him out. After it got all over with, my wife says – Pat says, "Geez, I would have liked to live in Charleston." [laughter] During those years, then, the councils – we were once the Estuarine and Menhaden Research Laboratory, named to that effect, as one laboratory, and we were a national center. When then – what do you call them? – the fishery management councils were devised, then we were taken from being a national center – and Ted really had high visibility in that – we became a laboratory of the Southeast Fisheries Center. The menhaden work really took a beating, verbally, because it wasn't part of fisheries management plans. The reef fish work that Gene had started earlier turned out to be a godsend in terms of giving us priority within the center, and it allowed us to keep the menhaden work going. In fact, when I was selected director, I had lunch with Bill Fox one morning in Silver Spring, and he gave me a lecture over breakfast and what would have been a good egg omelet, telling me, "I don't want to hear anything about menhaden coming out of the Beaufort laboratory." So we kind of had to hide it, in the sense that we didn't – but we kept it going. Every year, I'd get a call from Dick or from Brad, who would say, "Let's do away with at least the Gulf sampling." And then we'd go through this argument, and he'd finally give up and would half-hearted – they didn't want to take – if I wouldn't agree, they didn't want to take the responsibility to disrupt such a dataset. So anyway, it got to be pretty good. One of the things that the lab did after that was expand into two new areas in the mid-90s, and this was sea turtles and bottlenose dolphins. Sherry [Sheryan] Epperly started – we started with Fish and Wildlife funds. And Shery began a turtle program that led to the requirements of TEDs to be in inshore waters because she showed how important the sounds were as a nursery area. Then after that, then Aleta [Hohn] transferred to the laboratory, and she began the bottlenose dolphin work, and those programs continue to this day. At a little later time, I want to say a little bit about the funding at the lab and how we survived on really unusual funding for a federal facility, but I'll quit here for now.

DH: I would like to say one thing because he reminded me of it. Back to the larval fish program origins, I want to give credit where credit's due. I modeled that team and whatever it was called, the various changes they make over time – I modeled that after Reuben Lasker's group out in La Jolla and Bill Richards. Those were two completely different people. They knew different things. Reuben Lasker built a wonderful team, but he could come into a room – and I think there's books about this, but he would search the room, and he could zero in on the most

important person in the room just like that. He was [inaudible] that person, even though he may have known you for thirty years. So he knew the political ins and outs of building strong programs, and he did. Bill Richards was never known for his geniality and things. If you were an idiot, he said, "You're an idiot." Those two combinations served me well over the years. And we built a group here with [Stanley] Warlen and [William] Hettler and [John] Govoni and [Jonathan] Hare – and I'll miss some of them, but – Allyn Powell – that stood up. They could stand up to last year's group. Bill kept a smaller group, in a sense, but he kept in touch with other parts of the NOAA labs. He didn't build himself an empire. Bill was a researcher, and he did research. But I admired both of them and the team it allowed me to build. The one other thing – in *The Wizard of Oz*, if you go see the wizard, you get this big deal. You're something if you've seen the wizard. Well, my larval fish career was made because, somehow – Bud was involved in this – we talked the AEC into funding me for a year at a lab in Oban, Scotland. And that was because they had better equipment than we do to do pressure work on the effects of pressure on larval fish as they went through reactors. By golly, it went through, against one guy, who almost died of a heart attack over it on that committee. He was not in favor.

FC: He quit handling our contracts. Yeah.

DH: Yeah. I got there; the main thing is John Blaxter, who is probably as good as ever been in larval fish, was at that lab. Of course, I knew that. I'd been to a meeting there, and I wanted to work with him. He wasn't the guy I worked with on power plants, but he was a larval fish genius, so all I had to do the rest of my life to get accreditation was say, "Well, I had essentially a postdoc under John Blaxter." People would salivate, and it just really helped my career a lot. And the lab deserves credit for doing things like that. That was unheard of at the time. We did it again for people. But that was a real highlight.

JS: Well, let's stay with Don. You touched on your love of larval fish. How about some of the major contributions that you're most proud of? I'm thinking in terms of, toward the end of your career, that SABRE [South Atlantic Bight Recruitment Experiment] project that just brought in all kinds of collaboration, and it was a major, major project.

DH: It did. The lab, historically, under Ted and Bud, had connections with academia. That wasn't always the case with other fisheries laboratories. Woods Hole probably does. But some of them had no connection. That was good because it brought in expertise and ideas. Also, we were, a lot of us, adjunct professors at NC State, so we could have graduate students. There's nothing like a graduate student to keep you on your toes. Theoretically, you're supposed to be smarter, and they can ask some serious questions about why are you doing this and what are you doing? Those were great programs. It allowed us to get into some serious things that led to SABRE, which was everybody knew that most of our commercially important fish spawn offshore – a lot of them – but they had no concept of what kind of things happened before they got here, and we knew they used the nursery area. So we developed a very good program looking at trying to look at offshore transport of larval fish and what would happen if you built a jetty at Oregon Inlet and diverted the water, because there's only about four inlets in the North Carolina sounds, so if you're cutting off one of the important ones, what would happen? What would happen if you did oil exploration offshore and had an oil spill, etc. I thought that was a real highlight. Then the culmination of that, just before we left, is they came down with NOAA

money to do a southeast larval fish recruitment type thing, SABRE. I probably didn't get the letters right. But anyway, the acronym was SABRE, and that was done by Larry Crowder. That was a combination of universities from Duke University, NC State, the University of North Carolina, the University of Miami, and several of our other NOAA laboratories at the time. It was a big, multipurpose thing that lasted for some number of years, five or six. It was funded for more than one year, which NOAA has a problem with. They want you to do everything and get it over with and do something else. But they funded that one a decent amount of time and put out a lot of papers and, I think, a lot of very good papers and resolved some issues, at least, with offshore recruitment.

JS: Bud, I'll reflect over to you. How about – tell us about some of your major contributions, things you're most proud of science-wise that you accomplished here at the lab in your tenure?

FC: Well, one of the things I'm most proud about is to have been part of the laboratory when we made this major transition into general ecology and the versatility that that afforded us in terms of meeting needs of the agency. I did a couple of papers with Dick Barber in the early '70s on mercury, one on a deep-sea fish and one on bluefish, that got into the literature. Then I went on with my career. Around 2007, seven years after I'd retired, I got to talking to Dave Evans about that work. We said [as] a joke, "Let's repeat it," so we went up, and we were able to get fish from the Division of Marine Fisheries from off Oregon Inlet, where we had gotten them forty years earlier, and Dave analyzed them for mercury. The fish were lower by almost half, very statistically significant, no overlap of the data at all. We published that. We couldn't figure out what was going on. We pulled in Dick Barber, worked for two years, during which time, unfortunately, Dave passed away. But we published that paper. That paper is the first one to show the decrease of mercury in fish along the East Coast. We were able to show decreases in air and in water and in fallout to the point that we could relate it to decreases from the Clean Air and the Clean Power Act in both Canada and the United States of cleaning up the mercury in the air was really the thing because nobody knew until the mid-'90s that mercury was transported through the air, so everybody thought that the numbers out there in fish were background levels. Well, when they began to drop by half over a forty-year period, you know those weren't [inaudible] back in the '70s, so we went back. And the freshwater fish people had already been publishing since the mid-'90s about decreases in the Great Lakes, and nobody reads each other's research from different venues, and so Dave finally came onto that. We put the paper together. We have now one on blue marlin that is showing the same. We have a hundred and eighty-six blue marlin samples with two time periods that we're currently working on. Other than that, the main things that I feel best about were the programs that I participated in and helped manage, such as the Gulf of Mexico program that we did cooperatively with AOML [Atlantic Oceanographic and Meteorological Laboratory], the C-CAP [Coastal Change Analysis Program] land change use program that started here at the lab and, within just a few years, went from research to application, and it's now being used at the Charleston Ecology Center on the old naval base. Somebody help me. I have to skip through this. Oh, the seagrass mapping stuff turned out to be extremely valuable for the state managing the seagrass resources with the scallop fishery. It solved a lot of issues where they didn't really – it's hard to know where the seagrass beds are. Once we mapped them and gave that to the state, they were able to do a much more efficient job of protecting seagrass beds.

DH: You're almost into special activities, which is a little different than what you're saying but –

FD: That's true.

DH: – you authorized that. I'm talking about – Bud and I were both on the oil spill response –

JS: Sure. That's where I was going to go next, so.

DH: Okay. Well, I'll let Bud start that one off.

JS: So along with all those major contributions and major programs, there's what, for lack of better terms, I'd say, all of a sudden, a forest fire or a brushfire would spring up. For example, oil spills. The red tide came out of nowhere in the late 80s. Then some trivial stuff but kind of interesting was the centennial lab celebration here. Maybe you can go back and forth and discuss some of those things that happened during your careers here.

FC: When you're the administrator or manager, things can get boring, answering the phone, before email, signing forms in triplicate. So you like to have some – it's like a mistress. It's something to do on the side for fun, to get involved in some of these activities. Oil spill was one that I could get involved with, and so did Don. We were part of the NOAA oil spill response team. We took and lived through training in Santa Barbara for a week, years ago, late '70s. We participated in the –

DH: Ixtoc in the Gulf.

FC: Ixtoc oil spill, the well blow out in the Gulf, and the Amoco Cadiz in Brittany, France, so we both spent time dealing with both of those. The red tide sprung up. And this is what I said the versatility in the laboratory is that, when the red time came, we had people who could deal with it. They all weren't just seagrass people, or they all weren't just larval fish people. We had, for example, Pat Tester, who picked that up and run it. It got her a permanent job, and she made a career out of it. But we were able to work with the state. We all even got letters from the governor for the assistance we provided the state during that time. Again, the versatility is what has really, really made the lab. Getting involved in those activities, which I did with red tide, and then when the big turtle mortality occurred up at Hatteras because of the flounder fishery, I got involved in that as a side issue in addition to being lab director. Then the laboratory as a whole – Don has testified; he'll talk about interactions and potential effects of nuclear power plants, and so we've always had a versatile staff that could respond to a wide range of special activities. I enjoyed being part of that, as I was in no position to carry on any long-term research programs.

DH: The other thing I wanted to mention. As Bud mentioned, it's kind of like having a mistress, jokingly. Well, there's also the downside. You can get in a lot of trouble by having a mistress. [laughter]

FC: I agree with that.

DH: I remember my testimony. I had to give testimony at an adjudicatory hearing about the power plant in Southport, the nuclear plant. We wanted them to have a cooling tower because they were sucking up everything in sight. We went through this hearing. Lo and behold, the opposition was my major professor, B.J Copeland, saying it would be okay. Here I was, I was kind of the – he was a big Texan. He was an awesome person (inaudible). Here little old me is testifying against my major professor. That was one of the downsides, I thought. But we came through all right, and they agreed to put in the cooling tower but never did it because they sort of blocked their way out of it by – our problem was too many coastal power plants. One we knew wouldn't kill all the [inaudible]. But you could see them in every place. At that time, they could see them every place, so we were a big factor in slowing down and killing coastal nuclear plants. If you built your own lake, we weren't quite as concerned. Anyway, that was quite an experience. But are we on odd bits or what?

DV: We're on mistresses, yeah.

DH: I think another one that I was really pleased with was my appointment to the Polish Sorting Center committee. I got into that because I had complained that a lot of money was going to the Polish Sorting Center, and it was going to sort Gulf samples from regular programs that stopped and sampled every so many miles in the Gulf of Mexico. Well, the reason I complained was I wanted some of that money, of course, because we were always on hard times, so I complained. Well, Bill Richards, my friend, said, "Okay, you be on the committee." He was on the committee. Bill wasn't so much for some of that – I mean, he was for it strongly, but he didn't like going to all these meetings and things, so he said, "Okay, I'll put you on it. You're now the southeast representative." I thought, "Oh my God, what have I done?" Turned out to be one of the best events of my life. I did work with them closely for over ten years, I think, and every other year went to Poland and had people here training. It was a very good program, and we found out they were starting larval fish samples for us. They had something like thirty-five women in Szczecin, and they did a good job. We brought them over here to train them. They took them out to Seattle, who was involved in it. Miami was involved in it, and Pascagoula. That was just a wonderful program, and I met so many nice people during that period of time. I really appreciated Bill's doing that for me. He stayed active, believe me, but he didn't have to go to committee meetings.

FC: One of the ways that the ecology work justified itself. We took tremendous criticism from NMFS front office. Then, when the Southeast Center was formed, [inaudible] our work was of value to no one. And Don was one of the first ones to do it, where scientists went and teamed up with federal managers or state managers to deal with an issue. I remember one that came along shortly after Don and Randy Cheek had worked together on the Brunswick Power Plant was they had an impoundment issue down in South Carolina, and Gordon was assigned to it. Dick called me up and asked me how they were going to do it. I said, "Well, Gordon was putting together a paper to show what the effects of the impoundment would do." And he says, "Well, have you been down there to sample?" I said, "We don't need to because we know how things work together. You don't need it. We know what's in the water." He says, "Are you sure you just don't want to take one sample?" I said, "No." We'll tell you right now before the sample comes up; it's how the animals relate to each other. We're not going to learn anything on a couple of

trips to South Carolina. That turned out to be very successful. The lab went on to – we did a special report for the marine mining part of NOAA on the impact effects of – potential effects of manganese nodule mining. We did one on the effect of copper and mercury releases on a mine in Alaska that actually got filed into the brief that the federal government – and the mine was stopped, as far as what the effects were on salmon. We did the same thing with copper issue from copper – they wanted to build copper tubings in power plants, and the discharge tubes would be made of copper, naturally be toxic, and [Bill] Sunda went up there and, in an afternoon, convinced them that they'd kill every last endangered species of sturgeon that were up there, shortnose sturgeon, and [inaudible] did not even kill one. So it was using the expertise here to go into habitat issues and team up with managers that finally won us support after well over a decade of just fighting against those people who didn't understand what we were doing. We would get into these wrangles about basic research and this kind of research, and, folks, there's only one kind of research.

DH: Good research.

FC: Yeah, and it worked. It worked. Getting funding from these other agencies forced the scientists to be productive, so when Jud Kenworthy spent nine hours on a witness stand being grilled by an oil company because of one their tankers ran aground and ruined the seagrass bed in the Keys, he had the reputation, the publications, the presentations, and respect that his testimony really meant something. They won that case, so it comes down to those issues. It's your expert against their expert. And either you build your own expertise, or you go out and hire another hired gun, and then the two hired guns fight it out, and that doesn't work. So what we did, I think, turned out to be very helpful for the agency, and the fact that we had so much outside funding required high productivity to build the reputations.

DH: I want to mention again – I failed to do it when I mentioned SABRE, but you mentioned experts, and one of the experts we had – we did fisheries things as well as some of the things that we've been off on. Dean Ahrenholz, who was in the menhaden program, had a terrific line and a terrific grasp of the menhaden fishery, for sure, and he was on – I forgot to mention this, but the main fish for SABRE was menhaden and, of all the things, I'd forget that, but the main fish was menhaden. It was pulling to get Dean to go to a meeting. He liked to do these things right here. And I got him to go to one meeting in Chapel Hill. I forget what I bribed him with. But the academics really knew about Dean, and they knew how good he was. He was a real key card in that program in a resource background on menhaden fisheries, and I failed to mention that at the first round.

FC: I want to make one point real quick to support what Don said, is that having fisheries research here at the laboratory was a tremendous help in terms of the relevance of the ecology work that was being done. [inaudible] both the reef fish and menhaden.

DH: In 1958, the winter of, I collected larval fish. I needed larval fish. I collected larval fish with Nick's group and – or maybe Nick wasn't and not [inaudible] but the fellow who went to Pascagoula – anyway, we collected – I learned how to collect larval fish off the old railroad bridge into Beaufort, which I thought was an exciting experience. We did it later from the wooden Pivers Island bridge. But they taught me what to do, when to go, and how to do it.

JS: That was probably, historically, the best year class of menhaden on record, 1958. Thank you, Don.

DV: '58 year class was the year class, but there may have been a bigger one in the early '50s.

DH: That was an experience to learn, too.

JS: Well, we've been on the serious side. All those decades, I personally find scientists sometimes kind of quirky and often, when you get a bunch of them in a room, difficult to work with or come to a consensus, but there had to be some funny times too and funny experiences along the line. Do you care to share any of those?

DH: The ringleader of the group that did things that related to fun was Bud Cross. [laughter] And the problem is a lot of the stuff could cause a libel problem in the present day. But it was a good time. We had people that appreciated humor at the right time. I won't go into – I'll let Bud, who, like I say, was the ringleader – some of it I really can't tell, but there were a lot of things that we did that made the place a better place to work.

JS: There were professional practical jokers here that it was –

DH: Professional practical jokers.

JS: – almost a dangerous place to work.

DH: Just like, all of a sudden, your phone didn't work because it was plugged with the thing and –

FC: Vaseline. [laughter]

DH: I'd like to get back to something on that, but I won't right now. But one thing I will mention, just as an example that I don't think will harm anybody, we did about ten or more years of work in Everglades National Park. A lot of that time, if we were working in Florida Bay, we were working out of Flamingo. We were working out of Flamingo in the summer, which nobody does. Nobody stays there. They close down most of the shops. There's a reason for that. You breathe mosquitos. Well, we were down there. We went off to get dinner out of the park. We came back, and somebody had left a room ajar or something. This person looked up at the ceiling, and it was black – black with mosquitos. It would scare a normal person. Well, one of our colleagues had the idea the way to get them out was to smoke them out, so he wadded up newspaper into a torch and created a big smoke. We'd forgotten that about a month before that, the Park Service actually had gotten new fire equipment and smoke alarms. Well, we closed the whole place down, and they came, the park rangers. I think they actually enjoyed it. They never said anything about it, to my knowledge. They didn't come back to the lab and say you got to get rid of that crap.

FC: I only had to cover for Gordon from time to time.

DH: Yeah. Well, anyway, that was, in retrospect, very funny. The person, who will remain nameless, hears about it from me quite often.

JS: I think Allyn Powell was on those Flamingo trips.

DH: He was on all of them.

JS: I remember one of his mosquito stories was that if you left the windows down in your truck, you could get most of the mosquitos that invaded your truck out by rolling down the windows, but there was always some mosquitos kind of tucked up by the windshield, at which point the only way to get them out was the Flamingo flush. And I said, "What's that, Alan? " And he said, "You get on a long, straight road, on one of those back roads. You get the vehicle up to seventy miles an hour, and you open both doors." He said, "That'll flush the rest of the residual mosquitos out of the cab."

DH: Let me tell one more Everglades one. Early on, I was doing the larval fish [inaudible] at the first [inaudible], which was actually taken over completely by Allyn later on. But at first, I was doing it. I put samples way up into freshwater too, because I wanted to know how far up some of these larval fish went, so we had a station way up Shark River, so you had to get to it; it was a long trip, and we had a NOAA Corpsman, Jose Rivera, and they usually ran the boats for us because they were good at that. Stan Warlen and I were on about a fifteen-footer or something going up there. We got all the way up to the river. And we're okay because you're out in the middle of it, and it's still fairly wide and, going in the boat, you got wind. Well, the motor broke down, and we drifted right into the grass and mangroves. I had never seen mosquitos come out like that, just making us black. And Jose, who was – he was a corps officer from Puerto Rico, and he knew something about mosquitos – didn't have a shirt on. His back was black. I was marveling at that. I turned around to tell Stan, "Look at Jose.: Stan had an aerosol thing in his ear with spray coming out his nose; he was so covered. I thought we were going to die. But luckily, obviously, Jose got the engine to run again. But that wasn't a fun moment at the time, but it's fun in retrospect.

FC: That wouldn't have been fun at the time.

DH: I don't think Stan would mind me mentioning his name because he admits it, I think. [laughter] Anything else you want?

FC: I have to mention – I have to tell a little story on Ted Rice. In 1972, we went to a meeting in Seattle. It was a radioactivity-type international meeting. While we were there, a couple of my friends from Oregon State and Ted and I went out to Ivar's Salmon House, very well known. We sat down, and they served us the smoked salmon they cooked over the wood, alder smoke. Ted took one bite of it and said, "Oh wow, that's almost as good as mullet." (laughter) Well, you don't say that to boys that grew up on the West Coast. Bob (Holton?) and Norm [inaudible], my friends there, just burnt, just burnt, so – "We'll get him." So three years later, Dave Evans is getting ready to fly out, finished his degree, back to Corvallis. Right after that, a whole bunch of us from the lab, including Ted, were going out to Corvallis for a radioecology national meeting.

In fact, it's the last one that was ever held. This was '75. It was three years after the incident in Seattle. I went down to Tony, and I got Tony to donate a two-pound pack of mullet to us frozen. I gave it to Dave, and he took it out on the plane with him, which also was the same flight that his gerbil got loose and ran all over the plane and into the cockpit. So we come out a couple of weeks later, and there was this great big salmon barbecue that they put on, and so everybody's in line. Word got around what was going to happen. They had a little grill down there. And that mullet was cooking down there. Everybody was standing around Ted waiting for him. He goes through there. He says, "All right, let me have some of that salmon." And they reached down, got up, and they plopped that charcoaled mullet on his plate. You should have seen his face. [laughter]

DV: Tastes like mullet.

FC: So we said, "We move slow, but we move." We got him.

DH: How are we doing on time?

JS: So we're doing okay with time. Any topics we missed? Anything you guys would like to cover?

DH: One thing I missed because my memory's – I'm old – when I came to this laboratory, we worked in an old turtle rearing lab. They had just finished tearing – well, say three years before the old two-story, beautiful laboratory was torn down. They had the T part of the brick building built but had not finished the part that then makes the T, the stem of the T. We were waiting to move in there. As I mentioned, it's where I put my feet on the desk. But at this laboratory at the time, there were probably four or five women in traditional roles. There were mostly master's biologists. There was one Afro-American working. There were no computers or anything that resembled one. We had calculating machines, so if you were doing a T-test and you were putting in squares of something, and you made one mistake, you had to start over. It might have been an hour's worth of work you were trying to cram into this. I remember one of those had a carriage that would get stuck. You had to kind of help it along to go back and forth.

DV: Were they Marchants or Monroes?

DH: We had two at the end, two kinds. In fact, in the best days, we had Monroes with printers that would print out the results. The scientific equipment was all mechanical. There was no modern [inaudible] that recorded data underwater. We were still stringing water balls offshore. If the first one screwed up, the ten below it were screwed up. We had to haul it all out and start over. Everything like that was mechanical. The secretaries were on typewriters. I'll put in a plug. Margaret Rose – you had to have two typewriters because she'd get the first one so hot it would jam up and had to go over to the cold one. She was good. That woman hated computers the rest of her life. She never accepted computers. But it was just we had, in the whole system, even after the radiobiology two-story building was built, in that building, there was a telephone on the second floor in the hall, a telephone on the first floor, and of course a telephone up in the office or two. Division chiefs had a phone, so there was a total of about five phones. That was the same for the distribution throughout the fisheries lab. Most of the people now have two

phones, at least, and computers and e-mail, and the communication system's different – not better, I said, different. In my day, you waited for a letter to come, which took a couple of days. You fooled around answering the letter. Probably the problem was gone by the time you answered the letter. [laughter]

DV: That was a good thing.

FC: And that gave you time to do something else.

DH: You could do research.

FC: Now, the answer comes right back.

JS: Instantaneous.

DH: Research may be rarer now. I don't know. But anyway, it was quite a different thing. But now I forgot the question you asked me.

JS: I'm curious about the area. You came in the '50s.

DH: Oh my god.

JS: And Bud, you came in the '60s. Maybe some comments about Carteret County, which is a unique place to me on the East Coast and in eastern North Carolina, and maybe just some comments about – was it a culture shock to you back then going to the county?

DH: Not to me, because I was from Missouri.

JS: That's true.

DH: But I could tell you a lot about the county. It may have been a shock for you.

FC: Well, it certainly was culture shock to Pat.

DH: A lot of women suffered.

FC: Oh, yeah. We had a Sears mail-order store in Morehead, a Belk mail-order store, a small store in Morehead. We had a Montgomery Ward mail-order store in Beaufort. And that was it. You come off the bridge, you turned right, and the next stoplight was Havelock when we came.

DH: When you came.

FC: When I came. That's right. It was different when you came.

DH: I'm going to tell it different.

FC: All the businesses – there were no big – like Walmart – no big corporations here, all small businesses. They all shut down on Wednesday afternoon, every one of them. They opened Friday morning until noon – I mean Saturday morning until noon, including the banks. Yeah, this place just shut down on Wednesdays. The schools were a bit backward. The kids had no air conditioning. The people were a little bit resentful, in a sense. I know an ex-employee of the lab once, without kidding, called me damn Yankee. I'd only been here about a couple of months. I was told I was the one with the accent. But I'd never lived in a community that had as caring and as good people as what we've experienced here. I didn't plan to stay very long, but [inaudible] a great place to raise a family back in those days. And it still is, to a large extent. I do want to mention one thing that, in terms of a cultural change, has happened during our time and yours too, to an extent, that when we came here, we had virtually no women working, very few. When you got on a plane to go somewhere, it was all men, businessmen, on the plane. You went to a scientific meeting, almost exclusively men. In four decades, that has changed tremendously. I mean, we didn't even have women's restrooms here for field[work]. When women started going to the field, Don had to get up some – we had to do some switching around and give them where they could have a shower. That's how one-sided we started out. I think that's a real plus for the laboratory, and we've had some fantastic female employees over the years who've gone on to have good careers from here too.

DH: That's something that I will say – I'm not overly awarding to government in general. The government has done a wonderful job on being equal-rights type of people. We actually – and I want to say one thing for Ted – Ted hired, when I was – you were here too, I think, but Ted hired a PhD scientist, a woman. Unfortunately, she didn't stay long, because her husband was a Marine and, when he left, she left. But she was a fisheries person, very intelligent PhD, and we got along great. The other thing is that the Beaufort lab, to its credit, had one of the first women scientists in [Samuel Frederick] Hildebrand and [Louella] Cable. Louise was hired back before – certainly before World War II. [inaudible] Anyway, old-timey. She was an illustrator and scientist, and so we have a plus on our side when we start getting [inaudible].

FC: She was a colleague of Rachel Carson, wasn't she? Worked with Rachel?

DH: No, she didn't work with Rachel, no.

FC: She never did, okay.

DH: So we've got a good record there. There was something else I wanted to add, and I've forgotten it, so you're lucky. What were you talking about when I jumped in?

FC: Women.

DH: Well, anyway, and we have had a good record on women since then. But the whole government program directed that. On other minorities, not so good, but not for not trying. We appointed people, special people, to recruit minorities. Before I retired, which is already twelve, fifteen years ago, I did a chart, and the number of Afro-Americans had varied between one and four over my whole career of forty-four years. Sometimes maybe a couple more, but it has been very, very hard to recruit, and it wasn't because we didn't try on that. It's just hard.

FC: And I think part of that is that Ted Rice started this university relationship, starting with North Carolina State. Then it went out to University of Virginia. Jud and Mark came from there. Evans came from Oregon State. And really, but it started with North Carolina State. That program that Ted started allowed people to come here, work on their master's, their PhD – (Ken Tenner?) was another one – work on their master's or PhD and get a degree and add tremendously to our overall program. Through that, we were able to have two African American PhDs get their degree under us. One of them also got his master's with us. We had several others here that went on into science, Black females, who – there's one working at Rite-Aid right now that, when she heard the lab was to be closed, she wrote three letters to Walter Jones over that period. That's how much she cared about the start that she got here – ended up in pharmacy, but it all started here, and she gives full credit to the lab for that, so we haven't had a lot of non-whites here – but had success.

DH: But we had success in what you said. What we haven't had success at is in keeping them. And the other thing that – I believe it was Ted or it could have been you – we worked out a relationship for a while with Elizabeth State [Elizabeth City State University], and Pernell Lewis –

DV: I was thinking of Pernell [inaudible].

DH: Pernell did his PhD here with [David]Peters –

FC: And masters.

JS: Peters.

FC: It was Peters, yes.

DH: He then became – he went to South Carolina, but then he was at Elizabeth State and may still be there, but I don't know.

JS: He went to Eastern Shore.

FC: Eastern Shore.

JS: U [University of] Maryland, Eastern Shore for a while. I lost track of him.

DH: So the effort has been there. I think we can be proud of that.

FC: Yeah. This whole business, as you all know, isn't easy. It's tough times at times.

JS: Any closing comments by either you, Don, or Bud? Things we may have left out?

DH: Being older, I'll close first. But what I want to – I may die before it's over. I think some of the things that I think are almost unique at some times – although other labs may have done it

since – we had a really, really – and have, I hope – a good scientific reputation. Universities and other labs would always mention how if it was published here – fisheries, ecology, whatever – if it was published here, it was almost assuredly worth taking a look at if you're interested in that subject. The other thing I really liked – and this was reasonably unique – was the worldwide exchange we had here. We got into programs under Bud and Ted that got us being able to have like National Science Foundation – whatever they call –

FC: National Academy of Sciences.

DH: – or Academy of Science fellows and stuff. We had them from South Africa, South America, China –

FC: Ghana.

DH: – Ghana, and that got us into going other places. I worked in Japan because of an exchange with a scientist there and one of our people going there to work, so I tagged along with him. And Poland, of course, we exchanged a lot of stuff. But to me, the international part was a really good thing. And then the other thing would be the education of the staff. You could almost go back to school if you wanted to. That started with the NC State stuff, and we had our staff go to Oregon State, South Carolina –

FC: Virginia.

DH: – Virginia. I can't think where else.

FC: Yes, a lot.

DH: Anyway, and they went under good deals, where sometimes they actually got paid to go, like I was one time –

FC: Jud was, yeah.

DH: – and Jud, and then we went on the exchange stuff I mentioned, like my thirteen months in Scotland. Well, I know Jud went to Spain for a year. Doug went to Puerto Rico. You had a different kind of [inaudible].

FC: I went to AEC for a year, yeah.

DH: And so we had good – those things, I think –

DV: But that was all Ted's encouragement for enrichment – going back to school. There aren't many federal programs that had sabbaticals, and we had them, but we made our own.

DH: We had to call them something else. Like you say, Ted – we often said a lot of things about Ted because, like all humans, he had his quirks. Some of us suffered under some of those quirks. But for me, he made my career by sending me back to school [inaudible].

DV: Yeah, Ted brought me here, so I –

DH: He was a good one. Let's see. The Alaska lab you never mentioned. We inherited that.

FC: Oh, yes. We did inherit the Alaska lab in 2000, and we still have it.

DH: Yeah. Bud was in Washington. I get this note, "You want to take over the Alaska lab?" Well, want to or not –

FC: Can never say no.

DH: There was nobody there. It needed rebuilding.

FC: Oh, it was terrible.

DH: It was a political hotbed because Senator [Ted] Stevens, I believe, was the one that wanted it, and he's the one that built the bridge to nowhere.

FC: And he wanted it for the University of Alaska to use.

DH: He wanted it for that. And the guy at Alaska wanted it. As soon as we got it and got it fixed up, the guy transferred. He quit the University of Alaska and went to Mississippi. [laughter] But that was an exciting experience and caused us to spend a lot of money that we didn't have. They still got it, don't they?

FC: Yes. Yeah, they're still – *Albatross*. I just want to finish by putting on the record the situation that the ecology group was in throughout the '70s. We began to lose our AEC support when AEC and (DCF?) or NMFS were jointly funding us, and we had to replace that support that we were losing from AEC with other contracts. Some years, we would have as many as fifteen, eighteen outside contracts because we would start the year with enough base NMFS funds to pay fifty of permanent salaries, so we were actually having to go out and find money to support permanent employees every year. It didn't bother us in DC, as long as we could do it, because going through a RIF [reduction in force] and all that stuff is just long, hard – if you guys can survive fine, we'll leave you alone. Yet, they would still give us positions. John and Mark and Pat Tester, Jeff Govoni, they all came late.

DH: Hare.

FC: Jon Hare. I owe it to the laboratory – really knew that if they didn't go out and find – and the manager can't go, or the director can't go out and find all those things. It's got to come from the staff. The staff went out, worked hard, [and] found money. I remember, one year, we were about to go under. We were not going to make it. In August, Dave Engel gets a call and gets seventy thousand dumped on him from EPA and got it through the year, so we were fortunate, but it was a level of pressure that you would have at a university, I think. We had it here because

we were responsible for permanent salaries and didn't have the funds to cover it with the NOS [National Ocean Service], and then they've been covered since.

DH: That was interesting. I always kind of wanted to see a little bit of it happen because I wondered, how are you going to fire a permanent employee? You're going to have to come up with money. We knew Brad had it in a bucket down in Miami.

FC: He what?

DH: Brad always had money, you know, pulled from somewhere else.

FC: And he did from time to time.

DH: In fact, he kept a lot of our money.

FC: At the end, he was canceling [inaudible] two cruises for us to get the money. But that's our lives in a nutshell.

DH: It's been fun.

JS: It's decades, decades, and decades. Thank you for – I think we touched on most of the subjects we were going to cover and appreciate that. Don and Bud, Doug, thanks.

DV: Thank you very much, guys.

DH: And Joe.

JS: Thank you.

----------END OF INTERVIEW----------

Reviewed by Molly Graham 3/1/2022