Joseph Smith: All right, here we are. We're at the NOAA Library on Pivers Island. April 20, [2017], about 10:15 in the morning, prepared to interview Dr. Dave Engel, Dr. Doug Wolfe about their work with radionuclides at the lab. Here with us doing most of the interviewing is Dr. Bud Cross, former director, also Dr. Don Hoss, former director, and Dr. Doug Vaughan. I'm Joe Smith. I will turn the questioning over to Bud.

Ford Cross: What we'd like to do first is to kind of get an overview of – the laboratory has a very unusual radiobiological history, particularly for a federal agency. What was done here in the '50s and '60s was very unique work, and it served a real purpose within the federal government on radiobiological issues that came up after the Second World War. We'd like to summarize that because, one, the funding was a very special relationship that we had going, particularly back in those days when agencies didn't work together nearly as much as they do now. So I'd like to start with Doug and ask him just to give a little bit about his education, his background, and just to say a couple minutes' worth of what the funding and the relationship consisted of in the '50s and '60s with the radiobiological program.

Doug Wolfe: Well, you keep throwing new things into the loop. Background? I grew up in Ohio, like Dave did, and went to Ohio State University, thinking of becoming an MD [medical doctor], but I grew up in Ohio collecting snakes and bugs and shells and a whole lot of other things, so I was a naturalist before I had any aspirations of becoming an MD. I majored in zoology as my MD pre-med major. While I was there, I met Dr. Milton Trautman, who was mentioned yesterday. [laughter] My major advisor said, "Whatever you do, don't get married because you have to do some important biological research." I was offered a number of teaching assistantships in the zoology department. I got married after my third year of zoology major when I expected to go into med school. I decided not to do that because I decided I really wasn't suited to become an MD. But instead, I went to the medical school and majored in biochemistry and got a master's degree and a PhD, working on, of all things, lipids, which well-suited me for doing a postdoc at Aberystwyth, Wales, or the University of California in northern California, Davis, California, where other people were working on the same thing, which is keratinoid biochemistry. You're going to get a lot more thank you asked for here. [laughter] I was also interested in fish oil fatty acids, and I applied for a job at the Halifax Laboratory of the Canadian Research Board of Fisheries. But my lifelong interest in biology overtook me because, in my second year of working on lipid biochemistry in the physiological chemistry department, I signed up and took an advanced invertebrate zoology course, which was back in the zoo building, B and Z, botany, and zoology. The professor who taught that course was Dr. Carter Broad. Dr. Carter Broad had arrived at Ohio State University five years earlier from a place called Beaufort, North Carolina, at the Duke Marine Lab, where he apparently had some difficulties and moved to Ohio State. I took the first course of his invertebrate zoology series one year and the second course the next year. Both years, he took his class on a field trip to Beaufort, North Carolina. In 1962, I had the good fortune of walking into a little building that was over there – I think I'm pointing the right direction – called the Radiobiological Laboratory. We got a tour of this facility as it stood in 1962. I met Dr. Ted Rice. Dr. Ted Rice said – or actually, Carter Broad said something to the general effect of, "You might be interested in hiring some of these biology people. But I've got one chemist here too." And Ted Rice said, and I quote, "Well, we don't have many positions of any sort, except maybe for a chemist. We are always interested in having a chemist." Two years later, I, on the spur of the moment, after I had written to the Canadian

Fisheries Research Board, I wrote a letter to – I can't remember his name – the fellow who built the house over here, the Annex.

Don Hoss: [Gerry] Talbot.

DW: Talbot. I wrote a letter to Talbot –

FC: Talbot. Gerry Talbot.

DW: – who was the nominal director of the facility here. Guess who wrote back? Because Jerry Talbot wasn't here anymore, it was Ted Rice and Tom Duke that wrote back. They invited me here for an interview. They offered me a job. I had no relevant experience, zero, except that I had strong interests in the environment and biology. Okay. That was step one. [laughter] I ended up working here for twelve years, eleven years, and did a lot of fun things. Now, as far as the history of the radiobiological lab is concerned, I think the best history of the radiobiological lab that exists is on pages one hundred and three and one hundred and four of this lab history because it refers to documents that I no longer have any memory of. I must have found them back in 1999 somewhere and cited them here. As Bud mentioned in his preliminary comments, this Radiobiological Investigations, as it was called in 1947 or 1948 – and just a minute, I'll check my date here – '48 I think it was – began very informally with a document that I can't find anymore. The initial AEC [Atomic Energy Commission] funding came to the Fish and Wildlife Service in 1948, and it was $2,500 dollars. Oh, actually, it came in – it was agreed upon in November of '48 and came to the Fish and Wildlife Service in '49 to help support [Dr. Walter] Chipman's studies while he was at College Park, Maryland. He moved here the next year, and funds in the amount of twenty-four and a half thousand dollars were granted from the Atomic Energy Commission to the Fish and Wildlife Service to support an infant program called Radiobiological Investigations. Now, if you look up Radiobiological Investigations at Beaufort, North Carolina in the Fish and Wildlife Service in 1949 online today, you will find essentially nothing. If you look up Chipman Radiobiological Investigations, 1949, '50, '51, '52, '53, '54, '55, you will find essentially nothing. Okay? It was a very low-level program. It turns out that that initial transfer of funds from the Atomic Energy Commission came from what was done under a letter from the deputy general manager of the AEC, one Carlton Shugg. And it was a letter to Clarence Cottam, who was the director of the Fish and Wildlife Service at the time. I don't know when the funding from the AEC was transferred to the AEC's Division of Biology and Medicine, which it was by the time all of us got here. I don't know when that happened. If you look up the records online of the Division of Biology and Medicine, in those same years, you find no record of any program at Beaufort, North Carolina with the Fish and Wildlife Service. The first appearances of activities going on here were a couple of Walter Chipman's publications on radioisotope accumulation, a couple of Ted Rice's accumulation papers. Those occurred in about 19 – just a minute – '56, '57. Soon after that, Walter Chipman accepted a position to go to Monaco, to the IAEA [International Atomic Energy Agency] Laboratory. Ted Rice, who had been the second employee hired on the Radiobiological Investigations, became the director. Ted Rice really put some sparks into the program. He started advertising it. I can find no record of that, of Walter Chipman ever submitting any annual reports until his last year – annual reports to the Atomic Energy Commission. I spent a lot of time this morning looking for them. I don't think it exists. But anyway, that's the really early history of the program as it became established. But I think that there's some more important history that almost certainly happened before that, because it was Elmer Higgins, who was the director of biological research in the Fish and Wildlife Service, under Acting Director Clarence Cottam, who hired Ted Rice to come here and work in the Radiobiological Investigations under Walter Chipman, who had been hired by Clarence Cottam. Now, it turns out that Elmer Higgins and Clarence Cottam are the people who initiated the Fish and Wildlife Service's studies on DDT [Dichlorodiphenyltrichloroethane] in conjunction – well, at the Patuxent Laboratory, in effect. Elmer Higgins was Rachel Carson's supervisor. And his studies on DDT certainly sparked Rachel Carson's interest in the subject and led to the publication of *Silent Spring*. Elmer Higgins doesn't get any credit for that anywhere, except in Rachel Carson's biographies. [laughter] Rachel Carson gets an awful lot of credit as being a major scientist from the Fish and Wildlife Service, which she was not. She was an editor. And she quit the Fish and Wildlife Service when she got famous for publishing some books that Elmer Higgins encouraged her to write because she was a very good writer. I interviewed Ted Rice to do this book, the lab history, back in 1998 and '99 [*A History of the Federal Biological Laboratory at Beaufort, North Carolina 1899-1999*]. Ted told me that he had never met Walter Chipman until he got here. In fact, he got here, I think, before Walter Chipman got here or about the same time. No, I'm wrong. He got here after Walter Chipman got here. Getting confused there. But it was Elmer Higgins who hired Ted. Elmer Higgins, in some of his discussions, said DDT is a pervasive global problem. Not only that but there are other things that are going to become a pervasive global problem. Radioactive materials are going to become a pervasive global problem because they are long-lived. Radioactive fallout is a global disaster. Radioactive effluents are a global disaster. He wrote only one paper on the topic, and it was published in *Transactions of the American Fisheries Society*, and it was about the potential effects of long-lived radionuclides on stream biology. All of this goes together to lead me to believe very firmly that Elmer Higgins and Clarence Cottam were probably the people who approached the Atomic Energy Commission and said we think this is a problem. We think you ought to help support us to study it. Okay. So that's intuitive and presumptive, but I believe it's true. Well, that gets us up to – if you go through those first undocumented years, it gets us up to about 1956, '57, '58, when there was a transition from Walter Chipman to Ted Rice as directorship of the laboratory. And other people began to be hired, like Dave Engel, Tom Duke, and Don Hoss. I'm going to mention one other name if I can think of it.

FC: Clare Schelske.

Douglas Vaughan: Jimmy Willis.

DW: I'm going to mention the lady who holds the world's record for the deepest dive. Sylvia Earle was here at the laboratory in the radiobiological laboratory for a very short time, very short. [laughter] But anyway, that may be about all I want to say on the early start of the radiobiological program. Ted Rice transformed the radiobiological program into a radiobiological laboratory, became the radiobiological laboratory director when the radiobiological laboratory was split off from the biological laboratory. Ten years later or so, in 1970, those two laboratories were recombined, and Ted Rice became the director of the full installation here on Pivers Island, and the radiobiological laboratory ceased to exist as such, although the joint program between the AEC and the BCF [Bureau of Commercial Fisheries], the

– excuse me – at that time, it was NOAA – was still ongoing and went into Bud's hands, ultimately, still under Ted. There were a number of interesting aspects to that total radiobiological program. That brings us to your next topic, which is fallout, maybe?

FC: Well, at this point, I'd like to switch over. There were basically three main foci that I think about of that program. One is laboratory experiments to understand how animals metabolize and turned over different radionuclides, fallout studies, and radiation effects. The facility was designed here mainly to accomplish those three goals. So maybe at this point, Dave could talk a little bit about his background and the radio effects building, what it consists of, what kind of studies were done, what role did that play in terms of the federal government assessing the potential effects of radiation on organisms, etc., which this lab seemed to be the national leader in.

Dave Engel: So you want me to start back? As Doug said, I grew up in Ohio – and Bud did. I hate to admit this, but I was not a stellar student, and so forth. But my interest in biology started when I was in high school. And I fooled around with it, didn't do much, anything serious, but I did have enough interest that I, on graduation from high school, went to Ohio Wesleyan University, which is right outside of Columbus, where Ohio State is. And my interaction with Ohio State was driving an ambulance from –

M: Yeah, we were there at the same time. [laughter]

DE: Yes, I was driving an ambulance from Delaware, Ohio, to the hospital in Columbus.

DW: University Hospital?

DE: Yeah. Oh, yeah. Down the middle of Main Street, siren going. I survived. Anyway, while I was at Ohio Wesleyan, I was a pre-med. They had a major called pre-medicine, and I went along right in there. I had a few interactions with people that were not too good as far as the pre-medicine went. But I had a lot of time spent in zoology. I ended up with really a major in zoology and pre-medicine. While I was at Ohio Wesleyan, my sister enrolled at Duke University Nursing School. I guess it was the first part of my senior year at Ohio Wesleyan that my roommate and I took off during the spring break and drove down to Durham to see my sister. When we left Delaware, Ohio, the slush was about ankle deep. It was horrible. Got down to Durham, and the Duke gardens were in bloom and so forth. I decided at that point I – my reason for doing things is not academic – I just want you to know that. It was really pretty in Durham, so I thought, "Okay, I think I'll apply to the zoology department at Duke," which I did. Much to my amazement, I got accepted, and I got an assistantship at Duke. I went down with the idea of doing invertebrate zoology. While I was there, there was a course that was offered by a young PhD, Professor Donald J. Fluke. It was called radiobiology. I thought, "Well, that sounds good," so I signed up for it. I got hooked, and so forth. I managed to keep my head above water in the zoo department and ended up doing my master's thesis under Donald Fluke. This is a rather strange admission, but anyway, we ended up writing a paper in 1962 – it was published in 1962 – on the effects of high-energy gamma and electron radiation on *Artemia* eggs, which seems a little bit strange, I know. But the beauty of *Artemia* eggs was that you could pump them down under high vacuum and dry them to virtually nothing, and they would survive, and they would hatch. So I was able to irradiate these eggs at different states of hydration with the two sources of radiation. One was a cobalt source, a little cobalt source that Don Fluke had. The other was the electron accelerator, which was a rather awe-inspiring little toy to play with. But the outcome of the whole thing was that it turns out that that particular paper was probably the most important piece of scientific information I ever produced in my life and got me to go to a number of places right up until 1979, because of that single set of experiments, which basically showed that the main source of damage to these eggs was the decay of free radicals, and this work was – that was our result, but we didn't have any way to prove it. It so happens, a couple of years later, a friend of Don Fluke's repeated the study and was able to show that, indeed, using electron spin resonance spectroscopy, he was able to show that the decay of free radicals was indeed what was causing the damage, so it was – that is about it there. I went and, while I was working with Don Fluke, I also was a teaching assistant at the Duke Marine Lab in a course called Radiobiology for High School Teachers. Under today's regulations and so forth, people would absolutely shudder because high school teachers were allowed to have – I'm trying to think – I can't remember the exact number of millicuries of radioactivity they could have, but they could go and get radioactivity and do experiments in their classrooms to –

M: Millicuries?

DE: Yes.

M: Wow. [laughter]

DE: Yes. I mean, seriously, and a couple of millicuries, I mean it was amazing, looking back on it. And anyway, I did that for two summers, in '59 and '60. During those times was when I met Ted Rice because he had some stuff he wanted to irradiate in Don Fluke's little cobalt source, which was in a tube about so big, and so I irradiated stuff for Ted and so forth. Then the most important thing that happened in 1960 was I met my wife, and we were married. So here I was. I finished my master's degree. I was working as a lab assistant with Fluke. That's where I was. I was looking for a job, something that would support my wife and I. I was carrying two flasks of hot algal – algae – anyway, I can't think of the term – anyway, Petri dishes full of nutrient material down the hall in the zoo department there. Someone said, "Hey, there's a guy down here who wants to talk to you. His name's Ted Rice." Of course, not realizing that I was about to make a major error in diplomacy, I hollered down the hall, "Tell him to wait a minute because I got all this hot material here. I'll be there in a few minutes." I got down there and talked to Ted. At the time, it didn't seem to have made any difference. But I will remark that I heard about that yell down the hall up until the time Ted retired. [laughter] Boy, were you lucky. I should have just turned around and left. Anyway, at that point, 1961, I was hired by Ted. I came down here in June to the then radiobiological, I guess, program, investigations, or whatever. Basically, this consisted, when I came in, of our friend over here, Don Hoss, John Baptist, Jack Price.

DH: Joyce Smith.

DE: Who?

DH: Joyce.

DE: Rice.

DH: No, Joyce Clark Smith.

DE: Joyce Clark – who was the other guy?

DH: Jimmy Wheatley.

DE: Jimmy Wheatley was here, yes.

DH: (Parish?)? Rees?

ENGEL: Rees. Yeah, George Rees and Edna Davis and Marianne Murdoch. There was the radiation – I was hired to do radiation biology. I got here, and they had this old 100-KV Picker X-Ray machine. I was used to doing very precise measurements of dose rate and so forth and so on. they didn't have anything that you could measure precisely with. Plus, the fact that 100 KV was a soft enough x-ray that, if you're dealing with marine organisms other than a fish, you're going to lose a lot of your penetration to the radiation, just simply because of the shell or the carapace or whatever. As a result, Edna and I worked together very closely, and we finally decided the thing to do here is to try to find out what effects we could show of the x-ray on fish. Well, we tried to do it. I thought, "Well, heck, you know what we'll do is we'll just look at the blood. After all, this is one of the things that in human and mammalian studies is crucial is the effects of radiation on the production of red blood cells and the destruction of white blood cells. We could do that in fish." Well, it turned out nobody had any idea what the hematology of fish was. The closest thing we could come up with was a book on hematology of chickens. We used that information to do a study on fish, trying to make determinations as to the hematology. So our first publication turned out to be the hematology of fish. Of course, the surprising thing to me was, instead of looking through the microscope and seeing these nice little round things like mammalian blood cells, these things are shaped like a football, and it had a nucleus, and that was the fish red cell. Plus, the fact that radiation didn't seem to make any difference. But with the white blood cells, there was one or two types that, right off the top of my head, I can't think of the names that were affected. But here again, we weren't too sure that we were looking at the same cell all the time because we didn't know anything about how they were being generated and what was immature and which was mature, and so forth and so on. Anyway, we went ahead and did that. I guess prior to my coming here, Edna had tried to do some radiation on survival of fish, but the x-ray machine would just burn the side of the fish, so you had a fish with a badly affected right side, but the left side was fine. Anyway, we were sort of stuck there. And I kept – politicking, I guess, is the best word – politicking for a cobalt source. We needed cobalt because it was a hard enough x-ray, hard enough gamma-ray that you could really do some precise measurements of dosimetry, and you could do, possibly, some good work. The first cobalt source we got was a ten-curie Picker unit that was designed for taking x-rays of steel beams and so forth and the welds on beams and so forth when they're building big bridges and whatnot. But to use this thing, we had the source that was sitting in a little two-wheeled wagon inside of a big chunk of lead, and then this long tube that you turned a crank and the radiation source went down the tube to the end. That's how you x-rayed the beam. But if you do that in the lab, you basically kill yourself. So at that time, John White came here. It was John White and Edna Davis who'd tried the first real experiment with it. They tried to do a low-level experiment with juvenile flounder. And they built, in a garage behind Claude Guthrie's house, a sort of a shielded area with concrete block and lead bricks. They did some experiments out there, and they were not of any – well, they showed something, but I'm not exactly sure. I don't have the papers to say much about it.

FC: Dave, wasn't there – I mean, there was a specific radiation effects laboratory built then.

DE: Oh, yeah, I know. But –

DW: It comes after this.

DE: It comes after that. The radiobiological laboratory was added on in, what, 1964, wasn't it?

DW: It was finished in 1964, and it was –

DE: It was finished in 1964.

DW: It was under construction, I think, in 1963.

DE: Yeah, '63, '64. I remember, when they were driving the pilings for that thing, I was sitting at my desk, and the thing would go boom, and the little calculator I had on my desk would jump up when they hit the hardpan. Anyway, when they built the main laboratory building, they also constructed a radiation building. That's what it was called, the radiation building. That building was constructed in such a way that we could do long-term, low-level experiments in a shielded, truly shielded environment. The room was, I think, about fifteen by fifteen, and the walls were thirty-six inches thick, of high-density reinforced concrete. The ceiling was ten inches of concrete, which, as it turned out, was not exactly the best thing. They should have had thirty-six inches up there too. But the room was supplied with seawater. There were some seawater tables in there. The way it was designed, there was a passageway that went down the one side, a little hallway. You went down this hallway, and there was a metal door that had some lead on the outside, inside. That door then opened into the room. So you had to go down a hall and around there to get to where the source was. The source was controlled in that hallway. The container that held the source was in the room so that you could crank the source outstanding in the hall, and you wouldn't be exposed to the radiation from the cobalt source. There were a number of experiments that were done in that room. I did one with juvenile crabs and their growth rate and so forth and survival. The one thing that came out of that was a little bit against your intuitive thought was that the ones that were exposed to low levels of radiation actually grew faster than the ones that were controls, so a couple of people didn't like that too much. But that's what happened. Also, John Baptist did a clam study using those saltwater tables. He used juvenile clams, and he irradiated those. But the real thing that was kept being pushed at us was you got to try to do this, so it's environmentally meaningful. In other words, is it going to make a difference on a population? Well, we tried I don't know how many different combinations of things, these little micro-environments. We used brine shrimp, where we'd get the brine shrimp going in these tanks, in these cylinders, with algae, and then we'd cover them so that the brine shrimp would eat the algae and then the algae would go down and, in other words, it was a rather stable environment. Then we put them in concentric circles around the source at different distances away, and so we got various dose rates as you move out. And unfortunately, nothing ever came of them. There was a lot of work to do it, but we weren't very successful at all in those efforts. But I kept pushing the idea of getting a big cobalt source. Finally – let's see here – it is actually written in here somewhere. It was 1963; we got a 1,500-curie cobalt-60 source that put out fifty-thousand rads per hour. That was used for quite a while. About that time is when Joe Angelovic joined the –

FC. But you used that – that room was a 10-curie source to look at sublethal effects, like reproduction and growth and things like that.

DE: Yeah. That was the idea, in those controlled environments.

FC: And with the cobalt-60, the high-level source, you looked at, essentially, LD50s of a whole suite of organisms, right –

DE: Oh, yeah.

FC: – and tried to determine an LD50 those – you came up with some kind of phylogenetical hierarchy in terms of sensitivity? Is that pretty much it?

DE: Well, yes and no. This is in retrospect now. This is not at the time because the way things were done in radiobiology was really dictated by what was done with mammals, so you had the thirty-day LD50. Well, the thirty-day LD50 was the death due to hematology. But there were different levels of damage that would go on that would eventually build-up to the mortality in humans. So it worked out in thirty days, and it was how they did it. We followed that. Just like toxicity of metals and pesticides and everything, it was ninety-six hours. It turns out that both of those completely erroneous as far as being real because it turned out that the – like, the thirty-day LD50 for a fish didn't mean a whole lot as far as hematology goes because the thirty-day effect was based on human red blood cells, which have about a ninety-day life, and they turn over. There's a turnover about every ninety days, so at thirty days, you've lost half your blood cells, and if you don't replace them, you're gone. But with a fish, we finally figured out that – and so on the literature somebody had done a study on fish and found out the lifetime of a fish blood cell was two-hundred-and-seventy days. So here we are looking at thirty-day LD50s of fish, expecting to see effects on blood cells, and it wasn't relevant.

FC: How much of this work was going on at the time around the world compared to –? Would you say that the work here was essentially pioneering with marine fish?

DE: Yeah. What we did here was new. It was different. There were people, particularly at the University of Washington. They had a program going. I think (Donaldson?) was the –

FC: On salmon, yeah.

DE: – was the leader of that. They did quite a bit of work with salmon and salmon eggs and this kind of thing. But as far as moving out from just salmon into the other phyla, there wasn't much of it going on in the United States at all. We looked at oysters. Jack Bryce did oysters. As I said, John Baptist worked with clams. I had crabs. *Nassarius obsoletus* – that's a mud snail, isn't it?

DW: Yes.

DE: And I did LD50s there. What we ended up with was that the invertebrates were very, very resistant to radiation relative to the fish. But here again, you have to control the environment because you're dealing with poikilothermic animals.

FC: Did the agency show much interest? Did you provide information from this radiation effects work to help in any evaluations?

DE: I don't really know, Bud. I really don't. Every time they would come here, they'd say, "What kind of low-level effects have you shown?" Basically, we hadn't shown any. We tried. God knows, we spent – I spent a lot of time in that catacomb out there. To do this low-level thing, then you have to know the exact biology of the organism you're working with and that type of thing. Here, I was working with things from snails to fish to crabs to all kinds of things, and each one has their own separate biology and environmental requirements so that it would have been very, very hard, with the small number of people that we had here, to do anything that was a long-term study. The nearest thing we did that would even come close to that were those pond experiments that were done by Tom Duke.

FC: Yeah, and they weren't effects work.

DE: No, they weren't effects work, but that was –

FC: There was cycling work that was – as far as the environmental –

DE: Yeah. But that would probably have been the closest [inaudible].

FC: Okay. So the radiation effects work really went from the late '50s into the early '70s. It's pretty well phased out at that point?

DE: Yeah, it started to go down. Wait a minute. I can probably tell you pretty quick when it started to go down. Well, '79, it was dead. '79, it was gone. But up through the mid-70s, we were still doing things. But I personally was shifting over to looking more at environmental variables and how that affected the animals more than the radiation.

FC: Right. We can talk about that shift in a minute. But I wanted to ask Doug what he might add in terms of also at the laboratory was research on, laboratory experiments on cycling and fallout work. And could you summarize, essentially, what went on during that time, which probably went from the 50s on?

DW: Well, maybe. I have some perspectives on the question you asked Dave also because the Atomic Energy Commission got started in the radiation effects business because of the atomic testing. A lot of that was Pacific Island atomic testing, and they were interested in marine organisms. They started a program at the University of Hawaii, which you can find online references to, interagency agreement between the University of Hawaii – and I don't remember the investigator's name – and the University of Washington, with Donaldson. Hanford Laboratories, Oak Ridge Laboratories. They were looking at marine and aquatic organisms. Our program, I'm going to suggest, came along a little more slowly [and] didn't get a lot of advance credit because it really wasn't going until '58, '59, '60. Somewhere in there, the Atomic Energy Commission was beginning to realize that, well, we had signed the nuclear test ban treaties and things like that, and some of those programs were being phased downwards beginning around that same time. And so there were major contributions made here, but they were at the tail end of the national priorities on that kind of program. That's my perspective.

DE: I think you got it.

DW: The same statement applies to fallout studies. Fallout studies were going bigtime at Oak Ridge, Hanford, and some of the major university contractors long before Claire Schelske came here and started the fallout program here at the laboratory. It wasn't until 1963 that we bought the multichannel analyzer. And Claire started ashing marine organisms and doing gamma scans of the ash. And globally, things had already gone way past their peak by that time. But we did some interesting stuff on fallout.

DH: I think the uptake and accumulation part was very relevant because it told where the fallout was going in the fish. And if you didn't eat bones, you wouldn't get [inaudible], things like that.

DE: Good point, yeah.

DW: I agree. But anyway, Claire had just gotten the multichannel analyzer a year before I got here. They had analyzed an awful lot of samples for radioactive content, and they had an awful lot of scans of a wide variety of organisms – fish, mollusks, and lots of things – before I got here. My involvement actually started because Claire was looking for some help figuring out how to quantify the radionuclides that were shown on the scans. I devised the stripping program that allowed us here at the laboratory to do that, which places like the University of Washington and Oak Ridge and Hanford had been doing for maybe a decade beforehand and had been doing using computers, I think. I can't remember the guy's name at Oak Ridge who published papers on –

DE: (Auerbach?)?

DH: Was it Jerry (sp?)? I'm trying to think of the guy.

DE: (Auerbach?)?

DW: Yeah, it was somebody who worked for (Auerbach?). Auerbach was the program manager.

DV: Yeah. I worked for Stan.

DW: Oh, okay, you did, too? [laughter] But he was a young guy, actually. He was the chemist or something.

DV: I think I know who you're – in the environmental sciences division.

DW: But anyway, and I was also a shell collector at that time and had done some shell collecting out on the Neuse River and discovered that there was a species of clam that lived over a broad range, environmentally, of salinity. I proposed to Claire that we go sample that population over the entire range of habitats that it occupied. And that started our *Rangia cuneata* study. And a number of papers came out of that effort that supplemented Don's and Claire's fallout in fish. The papers were largely published in the second and third National Symposium on Radioecology and in the IAEA symposium, Radioactive Contamination of the Marine Environment. Let's see. Claire had another one, I think. No, I guess it was in that book here. But that program lasted, essentially, from 1964 until 1972. I think the last work that was done in that arena was by Dave Jennings and me that described the ruthenium 103, 106, and the iron 55 in the *Rangia* clams that we had been sampling for three years, four years earlier. We went back and analyzed those same samples for additional radioisotopes more carefully. So there were some ten papers, or so that came out of that.

FC: As Don said, in the meantime, there had been laboratory experiments going on, looking at uptake distribution and elimination of radionuclides done by a host of people here.

DW: Yeah. Chipman actually began those studies, Chipman and Rice. Ted began those studies back in the mid-'50s and published a few papers that ended up in the first National Symposium on Radioecology, which all of us missed. Don didn't miss it, I guess.

DH: I don't think I went to it, but I was here.

DW: And a number of us participated in those – all of us, I think, participated in those kinds of efforts, exposing a variety of marine organisms under a variety of conditions, both mixtures and individual radioisotopes, mostly individual radioisotopes, so that we could get at the dynamics of accumulation and tissue location and the like. A lot of focus on zinc-65. Tom's study in the turtle ponds was on zinc-65. I don't know how the laboratory pulled that off, as far as licensing. They just put the zinc-65 out there in that flow-through open pond, I think. It ended up in the estuary.

M: Dumped it in.

M: Yeah.

DW: Don, of course, knows a lot about this experiment, which was the gold experiment in the Cape Fear, which I know nothing about, in effect, but it was another trace experiment in which isotopes were put out into the natural environment and followed.

DH: Well, lost. [laughter]

DW: Or lost. Yeah.

M: [laughter] Lost.

DW: We tried to follow them. [laughter]

FC: Well, I think what this did, this program was extremely unique, certainly in the Interior Department. And EPA [Environmental Protection Agency] was not around then. By doing this type of research, it gave the Interior Department a body of knowledge and expertise that was here that could provide information on radiation-based issues that came up over the years.

DH: (inaudible) Bay at Camp Lejeune was another one where stuff was turned loose.

DW: I don't think there's any question that the laboratory here made a major contribution to that body of knowledge as a whole.

FC: Yeah. We were asked to come up with information for them on plutonium, on the dumping of nuclear subs off Cape Hatteras, and a number of issues that I just remember from the time here that they –

DE: Yeah, I remember [inaudible].

FC: – had a place they could come and, by doing these experiments, you had a scientific crew or scientific expertise both in effects cycling and fallout that could provide them the answers they needed, including commenting on the effluents from nuclear power plants, on the applications, and we did that for a number of years, so there was a real service by building this program until the radiation issue more or less decayed away.

DE: Yes. [laughter] That's true.

DW: Yes. It's really interesting because it really was a temporal phenomenon. If you look at the Atomic Energy Commission, which really had a much more focused interest in radioecology and radiobiology than did the Fisheries Service, if you look at their bibliographic efforts that described the science and programs, in 1960, the laboratory here is scarcely mentioned. But in 1975 and '80, this laboratory and its publications plays a central role in those updated bibliographies.

DH: Some of that work came up again very much when Chernobyl blew up, and the Scots and people like that started wanting to know about fallout in grasses that the sheep and things were eating. We got requests. They went ahead and did some things. But the background was there. If it was this, it was going to go there. The background work was relevant.

FC: Well, I think that it was a very unusual government program in that the Atomic Energy Commission became Energy Research and Development Administration, which became the Department of Energy over the years had a joint agreement with first Fish and Wildlife and then NOAA to jointly fund this program at the laboratory and, without the AEC support, this program never would have gone but also, without the Interior support as well. That program morphed, beginning in the ’70s, when the Atomic Energy Commission or ERDA [Energy Research and Development Administration], at the time, began to be more concerned about coastal and estuarine ecology because they got a tremendous increase in their budget. They were going to be floating nuclear power plants all up and down our coasts, and they had to know the basic ecology, once it was found that, except for accidents, that radioactivity was not going to be released, that they had contained a cycling system. It was not going to be released to the environment, and so people studying that kind of topic on uptake and accumulation of radionuclides then began to change. And this laboratory changed with it for probably another fifteen years. I think Dick Williams was real instrumental in doing that. Go ahead.

DW: Yeah, just coincidentally, that was 1970, which was when the National Environmental Policy Act [NEPA] became effective and when EPA was established, and it was recognized, suddenly, that the way the environment worked was really an important thing to know in terms of understanding effects of pollutants on the environment. And so we had to do environmental impact statements all of a sudden. You needed information to write an environmental impact statement. A lot of that information didn't exist. And NEPA required it. And our program changed in response to that –

FC: Good point.

DW: – as did the AEC support –

FC: That’s correct.

DW: – or ERDA support.

DH: I think we should mention – I don’t think you did, Doug, but when I came, it was a special shellfish investigation, and Chipman was not under Talbot, technically, and they had a lot of fights over that.

FC: Move up here, Don.

DH: Well, when I came, it was the special shellfish investigation at Beaufort in '58 and before. And it really reported to an office in Washington. We didn't report to the regional director. Talbot, of course, being the director, wanted Chipman to obey his edicts. Chipman was not inclined to do so very often. And they had some interesting conversations over that. But we didn’t report to the regional director. We reported to – (John Glude?) was one of them later on, I remember, but there was somebody before that in the Washington shellfish office, because Chipman came out of a shellfish background, and his work – in fact, everybody here did something with oysters, if he was around, including me.

FC: So it goes back to the point that we always had a champion in Washington that played a role in seeing that this work was established and continued.

DH: We had terrific freedom of bureaucracy at that point, early on.

DW: Has anyone gone back and looked in this room for the reports to the Atomic Energy Commission prior to 1960?

DH: I haven't looked since we came up with the problem. But I brought in a quarter – I was going to mention this too, Doug – when I came, we were doing quarterly reports.

DW: There were quarterly reports, yeah.

DH: And I gave those – I just brought those in a couple of months ago.

DW: You have some of them? Those would be very valuable for expanding the history of the radiobiological investigations during those early years because there aren't any formal publications that came out in those initial years, but those early reports would tell what was going on here during that first five or six or seven years. Some of those reports are actually cited in the literature, that is, in the subsequent publications of Ted and Chipman. But I've never seen them.

FC: Okay. I would have thought that they would have been cataloged, just like the AEC reports are.

DH: The laboratory reports – the laboratory, they morphed into a laboratory thing, and fisheries and radiobiology did reports. Eventually, then radiobiology separated again, and they eventually went into what you're talking about on the AEC annual report. But before that, there were reports that took these quarterly reports, and they went somewhere, I'm sure. [laughter]

[Recording paused.]

FC: Because it's one of the neatest statements I ever heard – about 1969, we had a review here. We had a review, with AEC would come as well as the BCF or NMFS [National Marine Fisheries Service] people. And it was an assistant regional director from St. Petersburg here who, in the review, where we were beginning this transition into more general ecology work, asked Dick Williams if anything ate *Spartina*. And Dick's reply was, "Well, occasionally, I see a leaf with one bite, but I've never seen one with two." That was the answer he gave, which I thought was pretty slick, and it's on record now because I don't think it's anywhere else. [laughter]

DH: But we went into a terrific program trying to feed spartina fish.

FC: The what?

DH: That caused investigations into making stuff out of spartina, let it decay, and radioisotopes, and I was cramming it down pinfish in capsules.

FC: Oh, you were?

DH: And part of that's because a guy named (inaudible) Darnell was big into things eating stuff like that.

DW: The detritus food chain. [laughter]

DH: I couldn't think of the word. Thank you. So it was interesting.

DE: Bunch of strange things.

DW: One of my first NC [North Carolina] State graduate students, as an adjunct professor here, was Joe Ustach. He did a master's thesis on the decay of *Spartina*. We actually labeled the *Spartina* with zinc-65 and put little packets of it out in the environment, and looked at the loss of both organic and zinc-65. That's in his master's thesis, which was never published, but.

DH: I was working on that when I called up into the Army. [laughter]

DW: Well, that's what happened to Joe too. He got called up into the Army. [laughter]

FC: I think, when we're talking about doing this work and putting stuff down the drain that we had done experiments with and everything, it's good to note that, when the lab was built, the radiobiological lab and the radiation effects laboratory said all the outlets went directly into the Sound and that the radioactive waste that we dumped was taken – only had to be taken to the sea buoy and dumped. Those regulations have long changed.

DE: When the ocean dumping program was – I can't think of the words – anyway, Beaufort Laboratory was actually mentioned in the ocean dumping thing as that Beaufort had to stop dumping their waste in the ocean.

FC: Oh, really?

DE: Yeah. It's in there. Here are all these giant operations and –

FC: Little Beaufort.

DE: – Beaufort Laboratory.

DW: Yeah. Beaufort Laboratory had a license to dispose of material offshore. Clarence Roberts drove the *J-1110* – that was the name of the vessel – offshore carrying buckets and sometimes carboys of water containing dissolved nuclides. I don't know where they went for sure. I don't know whether they always went out to the side or not. I never got on one of those cruises.

DV: [inaudible]

DW: I was here at the lab when, on one occasion, someone in the public brought back a three-dram vial with a lid on it that had washed up on the beach that was very clearly identified as something that had gone on at this laboratory with radioactivity in it. They asked, "Do you know anything about this?"

FC: Holy mackerel.

DV: Holy shit.

DW: Stuff like that was supposed to go through a glass crusher. And apparently, it didn't always. Gray Roberts ran that glass crusher and incinerator. There was also an incinerator out in the –

DE: In the garage [inaudible].

DW: – in the garage.

FC: There was, in Gray's shop, there was an incinerator. I remember that. Yeah.

DE: And the famous drain on the floor.

DW: Both of those mechanisms were used in waste disposal, and I'm not sure what the criteria were for working one way or the other. We also had a minor chemical waste disposal problem here at the lab because, every so often, there were things leftover that we just –

FC: Put in the back [inaudible]

DW: – couldn't use anymore. I remember Jim Willis, and I disposed of some spent – I will call it spent – nitric acid. Let's see. Where did we dispose of that? We dug a trench, actually, a deep hole and put a lot of oyster shells in it and poured the acid into the ground back behind the – or beside the high-level radiation building. Of course, that took care of the problem because it all turned into neutral salts and just became part of the soil. But I suspect that EPA regs now would prevent that. [laughter]

DV: Yeah.

FC: I also want to mention that there was a –

DH: [inaudible] a couple other things [inaudible].

FC: – there was a special room in the laboratory that all the radionuclides were kept in –

DW: Experimental radionuclides, yeah.

FC: – a lead-lined vault. There was a hood in there, where you could go underneath, and you would cut the stuff. Over the years, the floor in there became somewhat contaminated. It's in the room where Wayne Litaker is now. But before anybody moved in there to work, we had to have the floor completely chipped out and taken to a radioactive waste disposal site and a new floor put in, so there were –

DW: Was that done on the basis of analyses or just intuition?

FC: I think it was analyzed.

DE: I went in there with a survey meter and found –

DW: A Geiger counter?

DE: – a Geiger counter and –

FC: It was everywhere.

DE: Yes, there was a couple –

DH: Bud, can we take a break?

FC: Yes. In fact, I think we're probably finished. I want to mention Don Hoss has been the new addition here that's been making comments. You'll hear more from him later, I'm sure. So does anybody have anything they want to add at this point? If not, thank you so much. It's good to get different types of experiences that's not in writing down here for perpetuity, and we'll continue on with this. But thanks, gentlemen, for all your help and your thoughts and ideas.

---END OF INTERVIEW---

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