Molly Graham: [00:00] This begins an oral history interview with Usha Varanasi for the NOAA Heritage Oral History Project. Today's date is April 19, 2023. The interviewer is Molly Graham. It's a remote interview with Dr. Varanasi in Seattle, Washington. I'm in Scarborough, Maine. We left off last time with a really critical period in your life. You finished your Ph.D. and got married to Rao. Remind me what happened first.

Usha Varanasi: [00:28] I got married first, and [my] Ph.D. was three years later. I think I talked to you about – how we got married. I went back to India to convince my parents. I came back in November 1964. He (Rao) had finished his qualifying exams. He was now a Ph.D. candidate. I was already a year behind him, and now I was six more months behind in my studies toward my Ph.D. He was worried that if he finished in six or eight months and found a job elsewhere that I would again have to move, which would be detrimental to my career. So he decided to look for a job. He got a job in the Boeing company in July 1965 due to a number of fortunate circumstances in addition to the fact he had a great recommendation from his thesis advisor. We got married in September 1965 when we were both graduate students. He had taken a leave of absence or permission to put in fewer hours from his program advisor. Then, about two years later, I told him my research was completed, and my committee said I should start writing my thesis; I would get it done in six months. Then he got really concerned. He didn't want to finish after me. He started working at night, and I helped him type everything because by then, I had finished typing my thesis. In those days, there was just a manual typewriter, and we didn't have money to ask anybody else to do [it], and we did not have a Dictaphone. Nowadays, I can talk and get it all transcribed on my computer. Both our theses had a lot of equations. He was studying viscoelasticity ['Wave Propagation and Dynamic Stress Analysis of a viscoelastic cylinder" by Suryanarayana Rao Varanasi, University of Washington 1968), and I was looking at solvent kinetics [‘A Study of The Effect of Medium on Positive Ion Ground and Transition States’ by Usha Suryam Varanasi, 1968]. In typing these theses, one had to keep changing those little symbols with different figures because [of] Greek numerals and alphabets and all. It was very tedious typing for these theses. But I did both; I typed mine, and then I offered to type his because he was working full-time. When it was all finished, we paid somebody to clean it up and make it presentable. We did all the first drafts of it. Then, in December 1967, I left the university because I had finished my degree. I think he perhaps finished around the same time because, in June 1968, we both walked together for the commencement ceremony. We wanted to walk together, but we got our degrees from two different colleges. The College of Engineering let me walk with him (Rao) even though I had different colors on my hood and cap for the College of Arts and Sciences.

Then, what happens is that Boeing lost a big contract. At that time, in the Seattle area, there were only two major employers: the University of Washington and Boeing. Boeing was much bigger than the University of Washington for full-time jobs because there were a lot of students. When Boeing lost that big – I think it was a contract for a supersonic plane – a big contract, so that they had to start laying off people. With each Boeing job, there were several associated jobs. All the associated jobs were also going down. Finding jobs [in] those days was challenging because you had no computer to see where the vacancies were. You had to go to – the university campus where in the student building there were jobs posted on some billboard, or to look at the newspaper in the wanted [section] where there were vacancy announcements. Well, I didn't have any luck, and not just me, all my classmates – hardly anybody coming out of chemistry. So, some moved; those that had the ability to move moved, but several of them actually quit going into chemistry jobs. One of them became a very well-known wedding and graduation photographer. I kept in touch with him for a long time. I did not have the option of going and looking for jobs out of town because I didn't want to. [Rao] gave up two years of his Ph.D. program to support me. Now, it was my turn not to rush around. I found some vacancy announcements that I called, like a paper and pulp company, Weyerhaeuser, etc. I would make a phone call because the only thing you could do is call on the telephone. The minute they heard an accent or a woman's voice, the secretaries/receptionists themselves just would tell me this is not a job for a woman, or a girl – girl was the word used. They did not see me, so I don't know whether there was any racial or minority issue. But I have a feeling that my name also told them this is not a run-of-the-mill white woman. But anyway, even a white girl was not getting a job. So, after I tried for about three months or four months in earnest, we both talked about it, and we decided we needed to go home to visit both our families, whom we had not seen [in] many years. This is now 1968. So [in] 1968, we decided it was time for us to go home. Rao could take time off from work, and we wanted to go to India in the winter. So, '68-'69 winter is what we decided. I had now six months before we went to India and no job prospects. I told my thesis adviser. He tried, but there was nothing. I, at that time, decided it was better not to keep getting depressed because there were no jobs, and I could learn many things. Two things I learned: one, cooking because I had never really learned to keep a household. I mean, I learned from my classmates at Caltech how to keep an American household, but I had not learned much about Indian cooking. My husband is really very tolerant about whatever food is available. But then I thought, "Okay, I am going to learn." I learned more about cooking. I'm actually a pretty good cook now. There was a library very close by. That's one of the things I so appreciated – I don't know about other cities, but I think the United States, especially Seattle, which is the reading capital of the world, has so many libraries. And I mean, public library, free books, you can check out, or you can go sit there for hours, and nobody tells you to move. It was a neighborhood library, and that's where I first discovered flowers and plants and horticulture in glossy magazines. It wasn't that much about the decoration of the house and all, but I was just fascinated. Then, I was fascinated by how quickly plants can be propagated and multiplied. I had no idea that you could buy one African Violet and then multiply it with just the leaves. I learned all the different ways of propagating. Our apartment had a little patio so I could put miniature roses. Very interestingly, our next-door neighbor was a photographer, so he would come in the morning – a young man – and photograph my miniature roses or anything I was growing because there would be some dew on it. It was so good. He would always give me copies of the prints of the photos. I don't know whether he published them in a magazine. But it was very good to get to know different kinds of people. He was a photographer, I was learning to be a horticulturist, and we both knew the importance of light. Anyway, I was actually coping. There are two ways of going through a dip in your career or in your life. I mean, it's easy to say about life's challenges, but career challenges are less serious if you have some means to survive. In my case, my husband had a job so we could get by. And for a while, it looked like I may never get a job after doing my Ph.D. But as I was learning horticulture, I thought maybe, like my classmate, I could do different things than Chemistry. Perhaps if I did not get a job with the fisheries, I would have perhaps ended up doing something with plants and maybe working in a nursery and then learning and applying whatever knowledge I had in the sciences. So, I enjoyed this phase. In November 1968, we went to India, but I won't talk about all the interactions and experiences I had as a young married woman because there are a lot of interesting stories. We spent three months in India. My husband took an extended leave because this was a time we really wanted to meet both families and also did sightseeing. We had never traveled much as kids. This is an interesting anecdote I must share. Because my mother gave me all types of lovely saris, I was wearing them in Delhi, Agra, and Jaipur, near all those historical monuments. There was a group of photographers who came and asked my husband if I could model for them in front of one of the big, beautiful golden palace doors. They didn't ask me, thinking I was an Indian woman because I was wearing a traditional Indian sari. They were from Czechoslovakia, and they were doing a traveling to India piece. Of all the people there, they thought I looked very Indian. I have these amazing photographs taken with these hugedoors. I ended up in Czechoslovakia on the front page of a magazine; it was a travel magazine. What I'm saying is traveling is so – it just so opens you up, and the experience so enriches you. That is one thing about me. I am an optimistic person who feels delighted about new experiences. I didn't worry about, "God, what am I going to do without a job?" during my travels. [Partly] because I was married, and I had a little bit of an Indian way of looking at things. I did not feel like I had to bring money either, and he wouldn't treat me with respect – I just let it happen. Anyway, we came back on March 14, 196[9]. This date is very important, so I remember that. We come back a little before March 14th. On March 14th, I got a call from my advisor saying there was a postdoctoral-type position available in a fisheries place. One of his students, Ed Gruger, who got an M.S. under Professor Schubert, was working there in this fisheries place in the Montlake building. That means my advisor was continuously calling people, trying [to] find me a job. He said, "Usha, they want an interview right away. You got to go this afternoon." March 14th – I must have come back on the 8th or something. So, on March 14th, I got these directions. My husband dropped me there because we had only one car. I don't have a very good GPS [global positioning system] in my brain, so I think I would have gotten lost. He dropped me there. My advisor had told me, " I have made sure that it is a research position and not a technician position. You just go there." So, I met this biochemist. This building, at that time, was in US Fish and Wildlife. This was before NOAA was created because this is 1969. NOAA was created in 1970.

MG: [14:42] '70. And was it Fish and Wildlife, or was it the Bureau of Commercial Fisheries?

UV: [14:47] Oh, yeah. Bureau of Commercial Fisheries. But wasn't it in Fish and Wildlife? I don't know. It was the Bureau of Commercial Fisheries. Dr. Lee Alverson was, I think, the head of that whole unit there. That building [was] built in 1931. It's, I think, about to be torn down. Let me not talk about that. So, I went there. My advisor had just given them a small part of my resume. I didn't even take a piece of paper with my resume or anything. My advisor had told Ed what it was, and Ed had told this gentleman, Dr. Donald Malins, who was a biochemist. He was working in a small lab called Pioneer Research Lab. And that was headed by Mr. Maurice E. Stansby, who I think got the Presidential Medal for his work on omega-three fatty acids; he did quite a lot of work on it. Anyway, I go there. They were looking at me like this strange being. I'm wearing a sari. I'm looking at this place, smelling of fish oil, because they were doing a lot of work with fish oil. Anyway, they talked to me about this job. What happened was there was a one-year research grant that came from the Office of Naval Research [ONR]. Actually, the primary grant was with Dr. Ken Norris, who was a well-known marine mammal scientist. He had moved at that time to Hawaii. He was in Honolulu heading up the Makkapu Center for Oceanic Research. They were studying sound transmission in cetaceans, especially studying the role of the hump in the shape of a melon on the head of porpoises in echolocation. They knew that it was used in sound transmission, but they did not know the how and why of this phenomenon. They gave a small sub-grant to Don Malins to just analyze the lipid or fat that was right in this melon, which Ken had dubbed as an acoustic tissue. They just wanted to hire a chemist who worked with natural products. They had already offered this job to somebody on the East Coast, but at the last minute, he declined to move to Seattle. I don't know anything about natural chemistry. I mean, my degree was in physical organic chemistry. But they had lost three months in this negotiation. They were telling me about these porpoises, and I was sitting there smiling but thinking, “What the heck is a porpoise?” Because I [had] taken no biology during my graduate studies. But somewhere in me was the survival instinct [that] said, "Don't ask. Don't show your ignorance. Just smile." Obviously not consciously, but I decided – although I'm very communicative]; I would have easily asked, but I wanted that job. By that time, it had been fifteen months since my Ph.D. I said to myself, “ I have a degree in chemistry. I should be able to analyze chemicals.” The discipline and training I was given during my Ph.D. taught me how to solve problems. You ask why, what, and how, and you solve a problem. You start with what, and you start with how. Then you ask the fundamental question of why, which many people don't ask; they just do what and how, and then it gets finished. Anyway, I thought, "Okay, this sounds very interesting." They said, "When can you start?" I said, "Tomorrow." They said, "Okay." There was no such thing as going to personnel or filling out any forms except basic information. They took me to walk around the lab and introduced me to Mr. Stansby. He was a very nice man. He was quite impressed with the strange-looking creature who had a Ph.D. in chemistry and came with a very strong recommendation. That is how I joined BCF as a visiting scientist (as I only had a green card) on March 14, 1969. I went home, and I had this encyclopedia that I ended up buying because those encyclopedia salespeople are so persuasive. I looked up porpoise. And then I find, “Oh, that is a dolphin. Oh, that's Flipper. Oh, that's in that TV show. Gotcha." I at least knew what the animal we were talking [about] because, during the job interview, I was thinking, “What the heck [are they] doing with a porcupine in a fisheries lab?" The only word I know – P-O-R – porcupine was the only animal I knew. Anyway, I kept quiet and got my job. Then, the enormity of the situation overwhelmed me for a while. I did not know anything about how to extract chemicals from a tissue. I did not know how. So, another thing is I have this habit of learning on the job or just learning new things. I mean, I love learning. So I said, “Okay, I am going to start.” My boss knew I did not know, but they were in a bind, just like me. He gave me some basic biochemistry – like [a] children's book, a very basic biochemistry set of five or seven books. I started reading about cells and lipids and triglycerides, etc. Because I was a young, nice-looking strange creature. People in the Pioneer lab were very interested in helping me because I was very sincere in asking for help. I had no chip on my shoulder of any kind; I was just happy to have landed this job. I told people, "I have not done any marine chemistry. I need to learn." There were two or three young technicians, and they were very helpful. I had this little desk, which I was sharing with another technician in a lab, which is now forbidden because of safety issues. I was sitting in a lab with all kinds of chemistry stuff. That's how I started. But I knew that there was a way of asking questions about a problem. So, the first problem was, what the heck is this tissue composed of? What is this tissue that's practically dripping oil at room temperature, at twenty-four to twenty-six degrees? Because in the lab it’s a little hot. You could practically hold the head (melon) tissue, and it will drip oil. I mean, it was that oily, right? The second thing I learned is how to extract lipids out of the tissue using organic solvents, which I'm used to, so I know what to do, and then once the oil was extracted, its composition needed to be analyzed. In the beginning, I was alone, but then Don put one of the chemists who was working on several projects as an assistant, so he started working halftime with me. He was not happy to be working with somebody who knew less than him. But I started to ask myself specific questions, read about porpoises, read all about Ken Norris's work, and tried to understand what they wanted to know, and that this is a very strange place for fat deposition. Then, I learned one very important lesson from a young biologist with whom I ate lunch. He said in life, in any life, there is nothing frivolous. We may not understand a phenomenon, but in any organism, everything is there for a purpose and function. If we don't know it, it's not because there is no purpose or function. We just don't know it. I thought, "There must be a purpose for this oil." And then, once I started to read about the oil – and the oil was very smelly. It was also very sticky and would stick everywhere. When I went home, I had to take a shower and wash my hair. It was that smelly. Then, I started reading about the chemistry of that oil. A little bit was known. It turns out it contained a very short-chain acid. All lipids are made of triglycerides which generally contain three long-chain fatty acids., So, these fatty acids in marine lipids, in the whole marine food chain, are generally twelve to twenty-four carbon long, and the twelve carbon is a very small percentage, but the fatty acids sixteen and eighteen carbon long (e.g., palmitic and stearic acids) – much larger and all the way to twenty-four are present in marine lipids. But there is nothing below C_{12} in any marine food chain. I looked at all the things that porpoise eat – fish, some whales eat krill, and so on. The thing is, all of our fat comes from what we eat. So, what is happening here in porpoise melon tissue with short C_5 acids in triglycerides? Then, I started learning about isovaleric acid. Then I learned about how to separate long-chain triglycerides from short-chain triglycerides using thin-layer chromatography and then high-pressure liquid chromatography, which uses the polarity of different compounds to separate them. I learned from the analytical chemists how to do that. I did all the thin-layer chromatography work with the assistance of one undergraduate student. The thing is, because I was not yet a US citizen, I could not be directly employed by the Bureau of Commercial Fisheries. I had to be employed first by the Oceanic Institute of Hawaii. I was Norris's employee, which had a very good side benefit of visiting Hawaii many times, which was wonderful. He said, "No, no, you can send me the reports." I said, "No, no, I'll come and give you a report in person," because it was such a beautiful place to go to. It reminded me of home. Then, after a few years, Dr. Norris decided he did not want to keep asking ONR for more funding for our part of it. However, we were getting very interesting results about the structure and function of the specialized lipids, and we convinced ONR to continue our project with some funding. However, this funding could not be given to another federal agency. So, it had to be passed through Seattle University (SU), where I got appointed as an assistant research professor in the chemistry department with my credentials. Incidentally, Don Malins got his undergraduate degree from SU, so it was a logical place for us to receive ONR funding. Then, over the years, I got promoted to associate and a full professor. I could have students work in my lab, but I didn't have to teach. I was a research faculty member at Seattle University, but my funding was coming from the Office of Naval Research. It was a strange arrangement, but [it was a great experience for me to interact with the Chemistry faculty and meet many Jesuit priests who were wonderful teachers in other departments]. I started getting really good students who felt our research was interesting because there is always interest in megafauna, especially for young people.

Once we identified [that] porpoise acoustic tissues (fatty melon and jaw) contained triglycerides with isovaleric acid, I started to gain some basic knowledge that this acid is very toxic to mammals – very, very toxic. There is a disease in a newborn human – when the mother is not able to catabolize or break down isoleucine, one of the amino acids. They have a malfunction of that pathway, and it stops at isovaleric acid instead of all the way down to acetyl CoA. This phenomenon is called an inborn error of leucine metabolism in some pregnant women. Their urine starts to smell of isovaleric acid because leucine catabolism stops at the isovaleric acid level, and then it doesn't have the capacity to break it down further, as would happen normally. If a child is born, the child's urine also smells of isovaleric acid, and usually, the child dies or is quite often stillborn. We were looking at that pathway. Because my boss was a biochemist, he knew those processes. I was reading about this. Then, in Jamaica, there is this fruit – not breadfruit, but ackee – a fruit that, if you eat unripe fruit, impairs your leucine metabolism, and you get temporary toxicity due to isovaleric acid which gets into your blood, and you start vomiting and things like that. There were these couple of links from the infant mammalian studies or human-based studies that isovaleric acid showing that if it goes in the blood, the animal will die, or at least get toxic effects. Our question was how porpoises incorporate this acid into triglycerides only in the acoustic tissues? There are two questions now. Once we figured out what it was, it turned out a large proportion of the triglycerides in the acoustic tissues are all short-chain and very branched, and even the long-chain ones – no longer than fifteen are largely saturated and branched and hence very stable and resistant to oxidation. These triglycerides are not found in its blubber. The blubber lipids reflect what cetaceans like porpoises are feeding on. I don't want to go into more detail about our analysis with mass spectrometry, etc. Our research got published in *Nature* journal; I mean, our work was getting published in *Science* and *Nature* because this was a very interesting topic. Everybody's interested in marine mammals. And this is sort of fundamental biology or biochemistry. I'm so glad I took that job and did not say, "Oh, I don't think I know anything about this area of chemistry. I am eternally thankful to my inner being [inner self], who said, "This is interesting. You can do it. You are trained. You don't need to know a specific subject; you know the process and go with it." That changed my life. This was another very critical opportunity in my life getting this job – like meeting my husband on the first day on the Caltech campus. These are pivotal points in my career and in my life, right? So, once we knew WHAT it was, this specific triglyceride (triacylglycerol with short-branched chain acids), the question of HOW became very important. How in the world is it producing it –? Before we ask WHY, we need to find out how these unique compounds were found only in acoustic tissue and nowhere else in the bodies of porpoises and whales. just having here and not anywhere else. This was before the 1972 Marine Mammal Protection Act. When an aquarium had an animal just about to die, we got fresh acoustic tissue that was used to show that an error in isoleucine catabolism allowed the isovaleric acid to get incorporated into triglyceride since this was occurring *in situ*; it was not going through the liver and blood so toxicity was not an issue. This work was published in highly respected journals such as *Biochemistry* because, for my young career, just publishing in *Nature* and *Science* is what you call a publicity type thing, so your colleagues don't take you seriously. They want to see if you really have the chops, so to speak, to be able to publish in respectable or very thorough journals like *Biochemistry* and *Journal of Lipid Chemistry*. I knew that I needed to publish – I couldn't be just flying off and giving talks about cute porpoises. I needed to show I knew the science behind the phenomenon, especially because I am not a marine biochemist. It was a very good time for me intellectually. But during that period, in 1970, I contracted – or I perhaps was always prone to it, but I got very sick. It turned out to be rheumatoid arthritis. It was so severe that I was hospitalized because my hands and feet used to get swollen and very painful. We did not know the seriousness of this disease. In those times, there were no rheumatologists in group health hospitals and clinics; they were all oncologists who just treated RA – because it is another autoimmune disease. So, they didn't look at rheumatology as a field in itself. And even though I was a young woman, they just gave me very strong medication because they felt the disease must be controlled immediately, or I would get really deformed and I would become bedridden. The reason I'm telling you is that there is a physical aspect of life that suddenly became extremely painful. I couldn't go to the lab to do the experiments. We had to rethink our path. I think in my ICES paper – you read that – my husband and I had to think about whether I should quit. But I had so many really well-wishers. I had an excellent husband who, at a young age, supported me through all these crises. I had good advice that if I give up something now, next time when I have another flare-up, I will have to give something more, and I will slowly become wheelchair-bound. That's something that would totally ruin me. I think I would practically shrivel up. But the good thing was that my lab – we have not yet become NOAA. This is late '69 or early '70. It was a research lab, like any university research lab. The agency thought we were doing something weird – not just me, but the whole Pioneer Research Lab was doing chemistry that nobody cared about in fisheries, although fish oil and fish protein research had more impact on humanity. We were kind of sidelined, which actually worked to my advantage because nobody was expecting a timely stock assessment or going on a fisheries vessel or anything. I could start thought experiments and read and review papers when I could not go to the lab. During that period, I decided – and my boss agreed – to take a little break to think [about] what we need to do next. Once they started giving me very strong medication, I could go and do some work. I had one or two wonderful young students who were very excited about this work, so they kept helping me. 'I went over heavy ground with a light foot' and survived this period. It lasted several years. But productivity didn't decline. Once we answered WHAT and HOW, then we needed to address the WHY of this phenomenon. That, of course, [was] Norris's first question – why? At that time, we found that the jawbones of porpoises were hollow. I think in most echolocating marine mammal cetaceans – toothed marine mammals – jaw bones are hollow and filled with the same oil, same lipid. So, it appeared that these unique lipids were involved in sound transmission going from the blowhole through the melon protrusion and then echo returning through the jaw bone lipids and going through the ear. Then, just as luck would have it, We were trying to figure out how to measure sound velocity because I could measure the density-specific gravity of oil, and if I could measure sound velocity, you could also measure frequency/compressibility. These three aspects are related through a simple equation.: sound velocity – it depends on the density of the medium or the specific gravity of the medium. It depends on the compressibility of the medium. So, compressibility, velocity, and specific gravity are connected. I could measure specific gravity in the lab. There was an applied physics lab at the University of Washington. But this is how science is done, and such work made me into a bonafide scientist. From a student, I became a scientist because quite a lot of it I was left to do it myself. I was left to learn and ask questions. It is an important part of my career. The thing is, there was an applied physics lab at the University of Washington where they were doing some work using submarines and underwater sonar because of the war – whatever they were using the submarines for – the sound underwater. They had those velocimeters that I could use. I had to extract a large amount of tissue to get oil to measure sound velocity, but the thing is, if you take the whole melon and take all the oil, it gives me information, but it's not granular enough. It did not explain why this very toxic liquid was there. Just around that time, because I was working with the applied physics lab, Henry Feldman, who was my colleague and had co-authored a couple of papers with us. He said, "Usha, there is a physicist [at] Yale University, in Mason Laboratory, applied physics lab. They [are] able to measure the compressibility of very small droplets." It's some kind of machine; I won't tell you more about it. "But if you can measure the specific gravity of a very small droplet and then go to this lab and measure the compressibility of the same small droplet –then you can calculate the sound velocity of these small droplets." I spent several weeks in the summer of 1974 at Yale University in the laboratory of Robert Apfel. It was the hardest work, and I had to manage my R.A., but we were able to map the entire melon and jaw tissues – we matched each droplet from tissues and located them in a map. I hand-carried oil samples in many vials. I was taking medicines, like 3200 mg of Advil or whatever those were. I spent fourteen days in Robert's laboratory using his little device to measure all the oil. You see, it is, I would say, perseverance, tenacity, and curiosity – those things that propelled me. Otherwise, it was a very boring job extracting hundreds of samples and labeling them. I couldn't take my student because there was no such money like that. I came back with a map of sound velocity. It started to become clear that there was a lipid lens with different sound velocity profiles to focus the sound, like a beam coming out of the porpoise head impinging on the object and returning echo received by the lipid in two jawbones to the inner ear. The reason for doing it is not to lose any sound energy when the sound is made in that blowhole with the two nostrils, and it needs to go out to hit the object because the visibility is poor – they can't see very much in turbid water, but with this echolocation, they could distinguish whether the fish is fresh or dead. I mean, all those experiments [made] famous by Norris. Our results showed how the lipid lens is used to focus sound energy during foraging or navigation. It took [in total] seven years of my career. I sometimes worked only part-time because there was not enough money. But when I worked part-time, the next opportunity presented itself.; I got paid seventy-five percent because the grant money was going downhill, and we knew we were going to be done. But during that twenty-five percent free time, I worked with some of the chemists there, learning about petroleum because the porpoise oil had some hydrocarbons – waxes – long-chain saturated hydrocarbons. When they talked about hydrocarbons in petroleum, my ears perked up, and I went and attended their meetings because I wanted to learn. This is another learning while I was there; instead of resting for twenty-five percent of unpaid time. I was curious [about] what was going on. And that is the time the Alaska pipeline was happening. During this period, the Montlake facility became a part of a newly formed NOAA. The whole Bureau of Commercial Fisheries became the National Marine Fisheries Service. We just kind of got [transferred]. I was not employed, but I was a visiting scientist. I still was not a US citizen because I had an immigrant visa, and we wanted to go home. So, although I was eligible in 1970 to apply for citizenship, we tried a couple of times to go back home to see if we could work there, but getting two professional jobs for a young couple like us was not feasible in India. – it was very clear to my husband that only he would get the job. With my health, my family will coddle me. I also knew nobody would give me a job. Just like in 1964, potential employers will say, "Have children before you come back." Just like they said, "Get married before you come back," We decided we had to stay in Seattle. That is when, in 1973, I applied for US citizenship. From 1970 to 1973, I worked as a visiting scientist/research professor at Seattle University. But I started to work a little bit on this hydrocarbon question, and what attracted me [to] this whole thing – see, these are all biologists. Except for Don Malins, there were no biochemists. I am a chemist, but I was becoming a biochemist. I was a curious chemist. I attended these meetings at Montlake lab, and they were talking about oil spills in other parts of the world, like Norway. There was this big oil spill in Norway. They were reporting that fish sampled near spilled oil did not have detectable levels of hydrocarbons – they didn't see petroleum-related hydrocarbons (i.e., polynuclear Aromatic Hydrocarbons -PAHs) in the tissues of fish. The question was, are fish not able to absorb large molecules like PAHs? Or are they taking up PAHs and removing (excreting) very quickly? Those are the only two questions they were asking. Everybody liked the idea [that] these were too big molecules; they just can't absorb. The other idea – that they are very quickly excreting them – was less popular – because people believed that fish did not have the ability to metabolize or convert hydrocarbons into water-soluble compounds as we do. The reason they did not believe it was that they had taken the liver enzymes from the fish, and they had done studies in the flask to show that the model PAHs, benzo(a)pyrene, was not getting metabolized by fish liver enzymes. People who were doing this work at the University of Washington and other places were all studying rats and mice. They treated the fish liver the same way as rats and mice, and they found there was practically very little metabolism. I was sitting there and listening to all these findings. And then thinking about maybe we could approach the question differently – like, we can put bottom fish onto sediment spiked with radioactive benzo(a) pyrene– because I had done work with radioactive leucine, I was kind of used to thinking about, "What if we put a radiotracer on a hydrocarbon-type compound and see where does it go in the fish?" Nobody [was] doing this. People were only working with liver enzymes to conduct *in-vitro* studies. I was used to working now with live head and live tissue, so I exposed some fish to radioactive BaP in sediment and some in water. Lo and behold, the radioactivity was detected not only in the liver, [but] so much more in the gallbladder. If it's in the gallbladder, that means the liver is processing it. It's just people were studying in vitro metabolism at a higher temperature (37.5C) suitable for mammalian studies rather than what would be suitable for fish because fish are cold-water fish. My student and I did in vitro metabolism studies at various temperatures and found that twenty-five degrees was the optimum temperature for the bath in which you do the conversion study. Fish are as good as marine mammals [and] as good as terrestrial mammals – rats and mice to metabolize PAHs. Big Idea. It got published everywhere. Suddenly, people realize that fish are taking up PAHs from oil spills and rapidly converting them into water-soluble compounds, which get excreted into the bile. If you want to know [if] the fish have been exposed to oil spills, and there is no smell (tainting), all you need to do is see if the hydrocarbon metabolites are excreted into the bile. One colleague of mine (Dr. Peggy Krahn), a very bright analytical chemist, developed a method to measure fluorescence in bile because hydrocarbons when they are multi-ring, like benzo a pyrene or pyrene naphthalene, they have fluorescence could actually be quantified. This was a huge breakthrough. Because first of all, you don't need to do tissue analysis, which is slow and expensive. This is just liquid. You just excise the gallbladder and get the bile fluid. Then, separate different hydrocarbons depending on how many benzenes rings they contain and do the same thing I was doing in separating triglycerides of different polarity using chromatography. That was amazing, both the discovery of the science and metabolic pathways, where I come in and the methodology developed by Dr. Krahn that actually allows you to analyze thousands of samples of bile fluid right on the ship where this instrumentation is installed during monitoring of areas impacted by oil spill.. This was getting to be the 1980s when my papers on hydrocarbon metabolism started to get published. I am getting ahead of myself. When I became a US citizen and when Northwest and Alaska Center under Dr. Lee Alverson received funding from OCSEAP, I was hired by NOAA to study hydrocarbon uptake and metabolism. Going back to the science story, our studies on PAH uptake and metabolism and analysis of bile fluid gave us enough time to build the science. Both complete solid pathways – what happens in marine mammals can happen in fish. Marine mammals, by that time, [were] protected, so all we could do was extrapolate. But if they die and you get gallbladder, you can find PAH-related fluorescence if they were exposed to oil spills. During that time, I started to develop lots of contacts with scientists in the field of marine pollution. This work is going on, I'm going to meetings, and I've become a small-scale well-known scientist, and people – very rude – say, “You mean National Marine Fisheries has scientists like you?” [As if] they're all hacks. They're all just going and working with the fishing industry and doing numbers. “They're not really scientists. Why do you work there?” I tried to explain [it’s] because I like solutions to real problems; I don't want to just be in academic life where it's all in the books. But I knew I needed to write books because publishing papers, being recognized by your peers, being cited, being invited to give talks, talking to students who just love it – because this is all new and things like that – all of that [is] very important to be a scientist. We were sort of an oddball group, The Biochemistry team in a newly reorganized Environmental Conservation Division. Dr. Alverson established the Environmental Conservation Division because all these monies were coming from the Outer Continental Shelf Ecosystem Assessment Program called OCSEAP. That's the first evidence – a first example, I should say, of doing science before a catastrophe happened. The question was, what if that Alaska pipeline leaked? If it leaked, how would we know? How would we know contamination is happening? What would fish look like? A lot of money was put into addressing these questions – a unique situation because, most of the time, science is done after a crisis happens, and everybody wants to know how much impact there is after a disaster. But OCSEAP was established way before any crisis happened because the National Academy of Sciences had said this [is] a pristine area. Their report said, "We need to find out what will be the impact before a crisis, leak, or damage to the oil pipeline happens." It's a very radical idea. I was at the right place at the right time. I had science, I had the interest, and we had a small focused group. By now, I have one postdoctoral fellow, John Stein, who finally, over the years, became director of NWFSC after I retired, and a couple of students and technicians. Almost all of my staff were graduates of the chemistry department of the University of Washington [who] started to learn there was a place, the Montlake Lab, where you can do first-rate science and apply [it] to real-life problems. Soon, we became almost like a Bagley annex. Bagley Hall is where the chemistry department is located at the University of Washington. People started calling us Bagley Hall annex because we were hiring as there was money coming in to analyze hydrocarbons in sediment in Puget Sound and money from OCSEAP. Then, once there was concern about oil pollution, there was also concern about all these other pollutions. This was an active phase of my life as a full-fledged scientist.

That is where I think we could stop because that is when two significant events happened in my professional life. One, I got invited by the chemistry department of the University of Washington to be an affiliate faculty. The reason was my friend, Dr. Alvin Kwiram, who was a graduate student and Teaching Assistant when I was at Caltech and taught me all about quantum chemistry, actually became the chair of the chemistry department. He said the chemistry department was too narrow; they have no outside affiliate faculty – he said, “We want industry relations, and we want federal government relations.” They looked for eligible scientists to appoint as new affiliate faculty. Then, because UW had no environmental college and no environmental chemistry department, degree, or anything, I became the *de facto* advisor for undergraduate and graduate students, but mostly undergraduate students in chemistry and biochemistry who were interested in environmental issues like pollution. Plastic was, at that time, not the burning issue, but chemical pollution, PAHs, and PCBs in marine and estuarine waters were becoming an important issue. The students had nowhere to study in these areas in the whole big university. I had lots of students coming to ask if they could work in my group or in the analytical chemistry group. Students started to come and learn in different labs. By that time, I had become the director of the Environmental Conservation Division. So, I have kind of rushed through from '80 to '87. During that period, a lot of science got done [and] Don Malins retired – or left. Yeah, he left. And suddenly, they needed to have a new director. I was the least favorite applicant in the odds when everybody was playing odds because I was an unconventional candidate, a woman of color scientist. I'm a chemist. I think what affected [me] is not that I was different because, by that time, people kind of got used to me that I was a great scientist. But they said, "She's a pretty good scientist. You give her something; it will get done. But she is a chemist. She does not know traditional fishery biology. They wanted a manager and felt I had too small a group. I just published a book. There were four applicants to that division, which was, I think, '86-'87, just before it split into Northwest and Alaska centers; it was still part of Northwest and Alaska Center. The head of that center was Bill Aaron, who was a marine mammal guy. He should really not say why you should not hire non-fisheries people. I think his review committee would have chosen one of the other candidates – but at that time, Nancy Foster had become a leader. She was the head of the protected species and habitat office. Those were combined at that time. She saw there were no women anywhere in leadership positions in NMFS. I met her because the habitat conservation office was under her. I met her in 1985 or sometime like that. She took to me, and we became pretty good friends. We became lifelong friends. But she was also a stickler for high-quality candidates. She told me I had to apply for this position. I said, "No, I don't have any chance. And I'm not interested. I'm doing science." She said two things. "One, if you don't apply, nobody will have to defend themselves against criticism that they didn't hire a woman. You have to apply just to show that you are a capable woman candidate because otherwise, they'll just say no capable woman applied." Okay, that's one thing. "Second, how long can you remain as a scientist if somebody else becomes division director who doesn't care for all those things you are doing." That hit me more than being a woman and applying, although she pushed me for that part of it. I just thought that if somebody else comes who doesn't understand, and this is just the division, it's not the center director position, just the division position. Even in the specialized division, doing biochemistry was the outlier in the outlier division. My work and my staff will be in danger. So, I applied. The minute I decided to apply, I decided all systems go, all stops removed. I'm not just going to throw my resume into – I am going to get my resume that nobody can – at least if they are biased, it will show. They can't say she's not capable. I had, by now, a lot of people knowing my work. I had already learned to talk to people in Puget Sound. At that time, there was the People for Puget Sound. a big organization looking at pollution levels and impacts, the head of the organization wanted a woman science leader to speak about it. I complied, gave public lectures, and became a member of their inaugural advisory board. I made friends outside of NMFS, especially highly regarded women leaders from academia, WA state, and NGOs. They wanted to see other women succeed – see, all this talk about women don't like other women and all that is nonsense. I mean, there may be one or two bad apples everywhere, and some people may be insecure, but most of the time, I got helped quite a lot by other women in different fields in different parts of the agency. My experience is that good people always help people who seem worthwhile. That's where we can stop because then I am now going – I was already employed by NOAA in 1975. I just associate NOAA with 1975, although it was formed in 1970. In 1975, they reorganized the Alaska Fisheries Science Center with this Environmental Conservation Division and all that when Lee Alverson was director. I think he had a bigger vision beyond traditional fisheries science, although he was very big in the fishing part of his career. He knew this was important, and he needed these people. I think we had a somewhat enlightened leader. There are lots of things about him people don't like, but that part of it, he knew. Then, by 1987, I became director of the Environmental Conservation Division, the first transition of my life from scientist to science manager – they call science enabler. I got you to that stage.

MG: [1:06:11] Yes, you've covered so much ground. When we pick up next time, I'll ask you some follow-up questions about topics we covered today.

UV: [1:06:18] Yeah. I just told you the story. Now you can ask [about] any of those things. You can look at my paper. You can look at your notes. The next one is you talk, I answer.

MG: [1:06:31] That sounds good. Thank you for giving me a great overview of that period after your Ph.D.

UV: [1:06:38] Transition. Because you can't just say you fell into it and you became – how did I transition? Yeah. Okay. Very good. Send me some dates, and we will continue.

MG: [1:07:03] I will send you some dates, and we can continue the conversation. Thank you so much.

UV: [1:07:07] Thank you for listening. Bye.

MG: [1:07:10] Bye-bye.

---END OF INTERVIEW--
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