

PB: Today is January 18, 2013, I'm Peter Brueggeman with the Scripps Institution of Oceanography Archives, and I'm interviewing Paul Dayton of Scripps Institution of Oceanography about his career. Paul, how did you get interested in nature, the ocean, and all that. It starts with so many people from a very early age, what led to your interests and career?

PD: My grade school years were fairly different, it's interesting. I was born just at the beginning of World War II. My father was working on a ranch and teaching school in Tucson. When the war started, he became a gold miner, deep hard rock gold-mining in the Tiger Mine in Oracle, Arizona. I grew up my first four, five years, in Oracle while he worked in the mine. After the war, the mine closed. He was in and out of work, looking for work. We moved to Oregon eventually, where he was looking for work in the logging camps. His background before World War II, before I was born, was working on ranches and as a miner in southern Arizona.

PB: You said he was teaching?

PD: My father had a college degree from Stanford, and he put himself through Stanford working in Grass Valley in gold mines in the summer and some mines in the San Pedro Valley, in Arizona. He went to Stanford during the school year. When he came home to Tucson, he found work at a small private school, a dude ranch sort of thing for rich kids from the East. This was going on when I was born, so I don't remember it very well. I remember the era when we lived in Oracle and he was a gold miner. That was about the age of four or five for me. Finally in desperation, the family moved to southern Oregon. He worked for a little while in Dunsmuir on the railroad and got fired because he wasn't in the union. Then we ended up in small logging camps around Crater Lake mostly between Trail and Umpqua, and we lived there until I was 9. We lived in crude temporary shacks and a couple summer tents and never had a flush toilet or electricity. We lived outdoors completely.

In the summers, he couldn't work because of the fire hazard in southern Oregon. The Tillamook burn in the late 1930s was a huge thing in that part of the state. Everybody was uptight about working in the summer, and he couldn't work in the winter because of the snow. Oregon in those days had a huge amount of snow that they don't have now. Crater Lake froze one year, and we had something like 10 or 15 feet of snow in the logging camp. We were snowed in for months at a time down in that part of Oregon. It was a really different era. Logging in those days was a dangerous business, and our camp had a guy killed, and several men badly hurt. Usually it was the chokers, the choke setters, that were hurt. My father had a log roll over him, but he was in deep soft mud and only got bruised and very dirty, but it still took two guys to pull him out.

John Isaacs, who was one of the Scripps Institution of Oceanography great scientists, was doing environmental safety for the state of Oregon at that time. He and I talked about it once. He had come up and visited our camp

after somebody was killed, and he remembered it, pretty much described our camp.

PB: Do you remember John Isaacs visit to the logging camp?

PD: No, not him, I was six, but I remember the event, I remember the guy, he was laid in the back of the pick up and went by in front of our house. I had been playing on the ledge above the road and looked down at him in the back of the pickup. Logs roll over people, and they're heavy.

So we lived outdoors, and we had little food when my father was not working, ... there was no welfare in those days. You could get World War II leftovers, and we got big barrels of kidney beans that they would give you, and big barrels of really bad dried milk that we used. We spent summers hiking and backpacking in the mountains to get fish. We would go in and fish out of little streams and lakes in the Wallowa wilderness areas. And I still remember the Wallawas because we went everywhere, and it was our favorite place. We would fish out one lake and go to another lake.

PB: So you were out in nature constantly?

PD: Yes, nature really is a part of me. I was having a terrible time in school. It was just very recently that I learned that I have a dyslexia learning disorder. I didn't learn to read until I was 10, well into the third grade, really. I still can't do math or languages. I had always thought I was just stupid. I just had to struggle really hard, working a lot harder to get by being stupid. Now I think well, I have an excuse, but the fact is I was still stupid. I can't do algebra or calculus or anything --

PB: That's why you collaborate on data.

PD: That's exactly why I collaborate, absolutely. I've always thought that the sword would fall at this establishment and they would discover my secret about being so inept, so I am always hustling trying to stay a little bit ahead of it. It started back there when I had a bad time in the Drew school near the town of Umpqua. But otherwise, it was a dream life for a little kid, imagine being 6-7 years old wandering in woods watching loggers fall trees with old cross cut saws, watching the huge trees fall and the buckers buck them up with hand saws and axes. Chain saws came to our camp the next year but the loggers would not use them at first. But for me maybe the trucks were the best. I sat on my little ledge above a road cut and watched some of the most wonderful trucks go slowly by with logs or lumber. There were old chain drive Whites and Macks, Reos, Diamond T's, Stirlings, Auto Car and even an ancient half track truck used in the woods for awhile. I could recognize them from their sounds. This is an important part of Americana and these trucks built our country and now they are all but forgotten except in the memories of old farts like me and at the Campo Truck Museum!

Eventually the camps closed, and we moved to some real towns, Cherry Grove and Forest Grove, in northern Oregon where my father got work in a mill. We lived there for a few years through 1956, at which point we packed up our old truck and our car and drove to Tucson, went back home where my mother was able to get a job teaching first grade in Tucson. We started going to the Gulf of California over Christmas break when we lived in Oregon. We would drive down to the Gulf of California in Mexico, to San Carlos Bay near Guaymas.

PB: This happened when you were living in Oregon before you moved to Tucson?

PD: Yes, we would drive all the way to San Carlos Bay from Forest Grove and spend ten days camping on the beach in San Carlos. I have some pictures of San Carlos Bay before anybody was there.

I went to high school in Tucson. The tradition continued through high school and college in which we would drive down to the Gulf of California in Mexico, to Guaymas, to San Carlos Bay in the early 1950s, 1952 or 1953, and spend Christmas vacation there. Driving, if you are unemployed, wasn't that big a deal because gas was so cheap.

PB: So that was really your first extensive interaction with the marine environment?

PD: It was my first interaction, and it was through the early 1950s, I was snorkeling and trying to kill fish.

PB: When you are on these trips from Oregon, you were snorkeling? Using what mask and fins?

PD: They had these funny old masks with ping-pong balls in the snorkels, they were held in. I don't remember using fins in the beginning. Real divers weren't using those silly masks, they were swimming pool snorkels, but that's what I had.

PB: And the mask, it was an inexpensive swimming pool mask?

PD: Yes, it had to be inexpensive for us to have it. Everything I saw seemed so scary because it was so different underwater than in the woods. I remember the first conch I saw, a Strombus, I thought my God, it's going to reach out and bite me. I was afraid to pick it up. It was all really exciting and different. There were a lot of Strombus there then; I wonder if there are any now?

PB: Did you use a Hawaiian sling spear?

PD: No, there were spear guns on the market from Italy, and they were real spear guns and quite powerful.

PB: So you got a spear gun early in your snorkeling?

PD: We got one fairly early.

PB: Because you were producing food, so it was worthwhile?

PD: Yeah, well -- I'm ashamed of all the things I killed in those days. I killed some wonderful old fish. We ate them, but it doesn't make me feel any better. Then when we moved to Tucson, at that point I really got into it in a big way. That's when I built the scuba tank and got more into the diving.

PB: And you moved to Tucson in 1956?

PD: 1956, yes. I had the scuba tank in 1956. There's a picture of me with that, and it was fun. I had some smart friends in high school that helped me with the construction of it. There was an article in an old magazine on how to make a scuba tank and scuba gear regulators and things. That's what I got really into the ocean, and I just dreamed of being a marine biologist.

PB: So what set your direction then?

PD: In high school -- Skin Diver Magazine came out about that time, and I was aware of what was going on in Southern California at Scripps Institution of Oceanography. Conrad Limbaugh, especially Connie, was written up a lot. James Stewart was written up some, but it was mostly Wheeler North and Connie Limbaugh I read about. I was aware of people diving for abalones; ... getting food from the ocean was something I was aware of. So in 1957, I came to Scripps Institution of Oceanography while in high school.

PB: Your parents drove you?

PD: No, I came by myself on a Greyhound bus. My mother was friends with Barbara Tuthill, the first wife of Carr Tuthill, the then-director of the Scripps Aquarium. They lived in Pacific Beach, and my mother set it up that I could come visit them and Carr would bring me into work. I came to Scripps, and I was here about a week. I remember it really, really clearly, because it was such a big deal for a high school kid just fresh out of the logging camps and things.

PB: You were still in high school in 1957.

PD: I would have been a sophomore or junior.

PB: So here you are at this major ocean research institution for a week. What did you do when you were on campus?

PD: Well, my eyes were huge. The first thing I did was find the old Scripps library. It was a wonderful building with glass floors so it was lit from above. On the top floor was this big reading room that was well

lit, and they had lots of materials to read. There was a whole shelf of materials for the diving committee, and the divers at Scripps, their logs and stuff. There were some diving magazines, and there were books. By that time I am pretty sure they were teaching a scuba diving course, and the materials were set up for the divers and for the course. I think it was Connie's course, and I think people were working with Wheeler. It was a big deal, it was new. They did it very professionally.

I think they might have even had their own dive logs up there. They had accident reports, so every time somebody had a close call it was written up. It was all on that shelf. I was so terribly excited and just read everything there. Folks came and went, some smiled and nobody hassled a high school kid from Arizona.

PB: So it was an information center or something.

PD: It was where everything was stored. I imagine when they taught the diving course, they may have taught it in that room, for all I know. But I was there in the summer and they weren't teaching anything. That was huge for me. I sat there for a full day if not more, and I read everything on those shelves. People were so damned nice. It is so ironic that I managed a career at the very place that was so inspirational to me for all the right reasons.

I walked the hallways, and to me it was like walking in the Baseball Hall of Fame or something. Every door had names, and there were important people, and the people were really busy doing important things, and they were all really nice to a wide-eyed kid.

My most notable memory of walking around was stumbling in and finding Ed Brinton. The cool thing is that Ed Brinton's office was where John McGowan sits now in the Ritter addition right below my office. I was just wandering around. He saw me standing around and invited me in. He was just the sweetest guy.

PB: He talked to you?

PD: Oh, hell yeah, he brought me in and showed me all the euphausiids he was picking, and I'm sitting there looking into his microscope. I confessed to him that I really wanted to be a marine biologist, and he was just the nicest man. You couldn't find a sweeter person to run into than Ed. He was very young in 1957. At that point -- you know, I'm very liberal -- I was very anxious not to be drafted. I was registering as a conscientious objector to war when I turned 18. I wasn't 18 at that point but this was a big issue for me then. Scripps seemed very military at the time; there was all sorts of Navy gear and things around there.

So I asked Ed, is it okay if I'm a conscientious objector to war and I want to be an oceanographer? I couldn't have asked a better person, because Ed was a Quaker. I asked the perfect person for that.

PB: Because there was a lot of Navy and military funding at Scripps?

PD: Oh, Scripps was full of it. Later, of course, I came to respect it very much, but here I was as an insecure little idealist wanting to be an oceanographer and marine biologist. Ed was just so wonderful.

PB: Do you recall what Ed said?

PD: Oh, yeah. He said "Oh yes, that's fine. I'm a Quaker." I was probably in there for an hour and a half or so. I met Art Flechsig who was working on fish as a graduate student at that point.

PB: Who else do you recall meeting?

PD: Well, I met a lot of people, but I don't remember the names. I do remember meeting John McGowan. The Tuthills had a party, and I think they did it for me, but what it was, was a chance to drink beer and eat abalones. The Tuthill's refrigerator had all of these abalones stuck to the walls of the refrigerator; you just stick them on a wall.

So the deal was that the people would bring their own beer and eat their abalone. At that party, a young John McGowan showed up. He was a diver, and it was a big deal to me. I pumped him about diving. He had already published on the squid and things. He tolerated me!

PB: La Jolla Submarine Canyon.

PD: Yes, so he was right into that. My recollection of it he was still McGowan, but he was very nice to me and treated me kindly.

PB: Does John remember the interaction with you?

PD: No, he remembers Carr Tuthill, and he remembers that they would occasionally have these abalone gatherings. I have pumped him, because I remember him fairly well because John was shorter than I am. Everybody else at Scripps was huge, in my mind anyway, and here was this burly guy with a black beard.

PB: Yes, John was quite a figure.

PD: Yes, and he came with a bunch of his abalones that he'd plucked. Anyway, this was a huge big deal. I met Ralph Lewin, and he had me over to his house in La Jolla Shores from Scripps. He used to have a guestbook, and I signed it twice in the 1950s, also when I came over as a freshman in college. I don't remember much about it except that Ralph and I found my name in his book, and that reminded me of that and what was going on.

This was all in high school, and then in college I got more interested in archaeology because I was doing archaeology with Emil Haury and James Louis Giddings. I eventually became disillusioned with archaeologists, not those two, and I went back to my marine biology dream.

PB: So you switched majors to biology? At the university in Arizona?

PD: University of Arizona in Tucson. I switched majors from social sciences to zoology in my junior year.

PB: Did you aim to do marine biology work with a zoology major, or were you just interested in zoology?

PD: Well, I was going to the Gulf of California to dive at least twice a month all through high school and college. I was addicted. Travel was cheap, gasoline was cheap, and so it was easy to do that.

PB: Were you going by yourself or with friends?

PD: Sometimes but rarely I drove down by myself, but often with my brother or one other person. The family would go too when they could. We spent a lot of time at Cholla Bay and San Carlos Bay. San Carlos is a longer drive, and you could have a lot more hassles with the police, visas and things, so it was easier to go to Rocky Point. We spent a lot of time camping at Cholla Bay through the late 1950s and early 1960s

PB: Marine biology was not an undergraduate pursuit back then, so you knew with a zoology degree, that you could probably pursue marine biology?

PD: Yes, well, there was an invertebrate course taught by a physiologist, Pickens. He was a nice guy. I went to the Gulf of California during the course almost weekly and came back with stuff. The beach there at San Carlos Bay was full of Amphioxus, for example. I knew how to get neat stuff.

PB: The professor liked to see organisms you brought back?

PD: He was happy to have all of these things, yes. He was a really good guy, as were others at U of A. Those were good years with wonderful mentors - both professors and students. I think I learned more ecology from classmate Bob Bezy than I did from any other single person until I was in graduate school, and he was a student my age that knew everything about desert ecology that there was to know. Well, Wally Heath was another Lowe student who took me under his wing as well as Bill Heed.

PB: To clarify a timeline for the record, after the University of Arizona, Tucson, then you went to where?

PD: When I was in my senior year, I was very involved and concerned with civil rights things. I wanted to go march in Selma. I also applied to graduate schools, and I just barely had a B average, I was really a bad student, and I didn't think I'd get in, but then I got into several. I was admitted to Stanford with the idea that I would then winter over in the Antarctic at McMurdo Station working as a research technician for Donald Eugene 'Curly' Wohlschlag.

PB: So you started at Stanford as a grad student?

PD: Yes, in 1963 I got admitted to Stanford as a graduate student to go to the Antarctic, and I decided Selma didn't rate with that. That begins my Antarctic period which was covered in another oral history in the Scripps Institution of Oceanography Archives.

PB: When did you switch to the University of Washington?

PD: It didn't take me too long to figure out that Stanford wasn't for me. I would have been stuck with Curly Wohlschlag, and he wasn't my type of ecologist at all. I didn't really give the Hopkins Marine Station of Stanford University a fair shake, but I knew what they were doing because I spent the summer with a whole bunch of students. I wasn't interested in that sort of marine biology.

When I went off to the Antarctic, I wrote the other schools that had accepted me and said, dishonestly, that each one of them was my first choice, but that I just couldn't pass up this opportunity to go to the Antarctic, but I'll be back.

PB: You were shading the truth there.

PD: Oh, I was dishonest. I just didn't want to tell them 'no' because they were all nice. One of them was the University of Washington, and another one was the University of Hawaii. I had chosen to go to Washington because I wanted to work at Friday Harbor. If they had closed the gate when I didn't come and said well, that's fine, Paul, you can reapply when you get back, I would have been SOL, but they didn't, so they were sort of forced to accept me in December of 1964. I was lucky, as I should never have been admitted to a good graduate program given my grades and lack of ability to do even elementary math. But they let me come!

I had a really old Studebaker that my parents had given me, and I drove that up there that winter of December 1964, to UW.

PB: To the main campus, or to the Friday Harbor?

PD: To the main campus, it was the zoology department. In the winter of 1964, that December they had huge floods. I have really strong memories of whole houses floating down the Rogue River and other things. It was really hard to get there, and it was all snowy and yucky, and I had never been to real graduate school, I was scared to death. I went off and spent a bunch of my Antarctic savings to buy a sports coat so that I could be a proper professor, look professional.

PB: With leather patches on the coat's elbows?

PD: Yes, the whole nine yards. I arrived at the chairman's office dressed all like that. This is now in the middle of the 1960s, and I was still a

baby from 1962 or 1963 and did not realize about long hair and all the social changes. Here all the UW students were hippies, and I was all dressed up. They were very nice, and tolerated me. I did my thesis up there. When I got there, I did badly in school, and I got a C in a big course, and the Dean tried to kick me out. It was a nervous time, but at the same time, I wanted to do field work, that's why I was here. So I was going out to the outer coast at the time.

PB: On the Olympic Peninsula?

PD: Yes, on the Olympic Peninsula. My ecological background from Tucson was environmental physiology, which was what people did in Tucson at that time. Eugene and Howard Odum, who had just introduced ecosystem type of biology, food webs and energy flow, that was ecology to me in 1964, so I didn't know anything else. I went out and looked for an animal to do environmental physiology, and got fascinated that first summer of 1965, with the big *Anthopleura anemone*. I was going to do environmental physiology on the anemones in the intertidal.

Then I took a course -- at Washington, I was so lucky to have the people that were there. Gordon H. Orians taught an ecology course and really changed my whole life by making me think of things in evolutionary perspective, how populations change through time. Everything was evolution, and he was very much into the theory, but it was evolutionary thinking. Then I got into competition, and there were some famous papers by Joseph H. Connell on competitive exclusion, and doing experiments. I really wanted to do experiments because I had read a paper by John R. Platt about testing hypotheses and negating hypotheses. I wanted to bring an experimental focus into ecology. I saw that most marine ecology was pretty observational, inductive, and I wanted to use the hypothetical deductive approaches championed by John Platt in a paper that had just come out.

PB: Into the intertidal... that was your focus?

PD: That was what I wanted to do, but I didn't know quite how to ask the questions until I got Orians' course and hooked up with Robert T. Paine. Connell had already done that in the 1950s, and his competition papers in 1961, and Paine's *Pisaster* sea star work that was ongoing at Washington was my foundation.

But from Orians' course I had thought that everything was driven by competition, the whole world was. At that time ecology was in two camps, dating from the early 1950s. In 1954, two books came out, one by Herbert Andrewartha and Charles Birch in Australia [*The Distribution and Abundance of Animals*], and another by David Lack in England [*The Natural Regulation of Animal Numbers*]. Andrewartha and Birch pushed the critical importance of density independent factors, for example, climate on insects. In their system, the climate does control the insects, and they had a whole book on density independent factors. David Lack worked on birds, which are very

competition oriented. He had a whole book on the world of birds being structured around competition.

Density independence doesn't fit very well into nice comfortable competition based theories, and so the theoreticians of the 1960s were built by bird people looking at systems that were very much driven by density dependent competition. This included Robert MacArthur in the 1950s and through the 1960s pushing competition-based models of evolutionary consequences of different interactions and things.

So I was introduced very suddenly from my ancient history of Arizona to modern ecology with all of these webs of social importance that people put on different processes. It was hard for me to deal with all of that -- and of course I am dyslexic, and I have trouble with the necessary math. I didn't know I was dyslexic at the time; I thought I was just stupid, but I wasn't able to follow what was going on to say the least. It was really a struggle for me to keep up with the academic forces that were flowing around me with the competition-based models. I was crawling around the intertidal aware of Paine's work on *Pisaster* as a predator alleviating competition, and Connell's work showing that competition drives the intertidal.

Joe Connell was at Friday Harbor at that point in the summers, and we would maintain his experiments. We got to know Joe pretty well at Friday Harbor in the summers. He was pushing competition all through the 1960s. He got off competition by the end of the 1960s, having basically been battered by us when he came up there. All of us FHL students were pushing him because we could see that competition wasn't going on.

After that first summer, when I took these courses, I just started crawling around the intertidal looking to see if I could see any of this competition that everybody was saying was there. I couldn't see it, so I tried to figure out what was going on. There was Paine and a couple of his students looking at predators, and I couldn't convince myself that those predators (Paine's starfish and John Emlen's whelks) were doing what I was seeing.

So finally, just sort of crawling around looking at things and wondering what is really going on, I realized that the limpets were really big players. I incorporated the limpets into the predator stuff that people were already talking about and suddenly my perceptions of nature made sense.

PB: Are you speaking specifically of the large owl limpets or all the various types of limpets, volcano and --

PD: All the limpets that were in the intertidal, and honestly, I lumped them, I knew that they were different, very different species and that they foraged very differently because eventually I started painting them and looking at their home ranges and their foraging biology. But in the

beginning, and I had to stay with it, I could only do so much, and I separated out limpets as a guild.

Joe Connell, to his great credit, was a wonderful mentor for me during his summers there. He took me by my hand to show me how to make cages. He told me what sort of stainless steel to buy and things, but then he took me into the shop at Friday Harbor with a lathe and a big hunk of plastic and helped me make washers and help me figure out the right stainless steel screws.

PB: Cages that you would install in the intertidal? Exclusion cages?

PD: Yes, cages for the intertidal, exclusion cages, yes.

PB: The exclusion cages were to exclude what?

PD: The whelk predators and the limpets. The limpets are not predators, just serious disturbance agents. I used dog dishes to manipulate limpets. I cut the bottom out of the plastic bowls and tacked them down. My memory of setting that stuff up was that conceptually getting the cages was fairly easy and the plastic rims excluded the limpets, but drilling the holes to screw them in was hard. I had a hand drill, and I finally bought another one that I could push on my chest, so I had something that was going on my chest, and I could lean on my chest and drill away. I had these expensive drill bits that kept wearing out. I would spend a whole tide drilling one hole.

PB: Because it was hand powered?

PD: It was hand powered, and the rocks were variably hard. Then Paine came to my rescue, he was quite helpful. He got some money for me to get a small gas powered generator that I would crank up, and it was a little Homelite thing like I had in the Antarctic, so I knew how to use them and I was comfortable with the equipment. Now I had the money to buy a generator. I mounted it on a backpack and bought a couple electrical drills. I would still go through those expensive diamond drill bits fairly fast, but now with that generator roaring on my back, and an electric drill trying to electrocute myself I was in business-- I knew about not getting electrocuted, and I wore boots and I was careful, but it was more risky.

Later in my graduate career, a nice Brazilian guy named Peterson I think, came and watched me doing this and went home to Brazil and did electrocute himself. So walking around with a generator on your back in the intertidal isn't maybe the smartest thing I ever did.

PB: And you were drilling holes in which to glue in anchors?

PD: Yeah, we had these little plastic expanding things, and you would hammer them into the hole, then you would put your stainless steel screw and plastic washer that holding the cage in place. They worked great.

PB: It would withstand the intertidal waves?

PD: Oh, yes, because Joe helped me get the right mesh of the stainless steel. We had to pay for it because it was heavy stainless steel screen. I had to special order it.

PB: And the mesh holes were pretty small, due to limpet size?

PD: They were large enough that hopefully the flow was not impinged. I had controls with a wall or two taken out so I could control that. Actually, I did learn that the desiccation issue was quite different, so there are nuances.

PB: How large an area would a cage cover?

PD: The cages were maybe six - ten inches on a side at the most.

PB: The technical aspects of doing that work in the intertidal are interesting.

PD: You had to have a lot of them, and I duplicated levels, so there was a stratified design at different levels. Then I had habitats exposed and not exposed to the water motion.

PB: From the front of the rock, the back, the side?

PD: Yes, so it was a hell of a lot of cages and dog dishes and things that I had around the state of Washington. Meanwhile, I was asking questions, comparing the types of predation from these things. I was keeping track of the natural stuff again, especially the real importance of desiccation that I came to realize. I used to think that it was all temperature because in Arizona that was what we used to measure because in the desert, temperature is important.

What I learned with my work is that the *Anthopleura elegantissima* anemone, which is covered with shells, if you take the shells off them, they are really uncomfortable. I was taking their temperatures with my Schultheis thermometer, which is a very fast reacting thermometer. You could just stick it in their mouth and in ten seconds you get it. So I was taking all their temperatures, and then for some reason I took the shells off a bunch of them because I thought that the white shells reflected the sun and this would make the dark green anemones warmer, and they all shriveled up and died. They didn't get hotter, they were much cooler. Then I realized that the wind was evaporative cooling and drying them out and killing them.

So then I got a relative humidity gadget you spin around, and I built relative humidity into all of my projects. Of course, I knew I should have been measuring the wind, but I didn't know how. But while I was

doing these experiments on the biological interactions, I was learning a lot about the physical factors.

PB: So what was growing inside these exclusion cases with the predators excluded?

PD: They would be the organisms that had settled and weren't eaten by the snails, in that case. I had to exclude the *Pisaster* starfish, and I did it the same way Paine did it, I just took them all off a large area. Every time I'd go, I take them off a reef. I think I got most of the most of the time. I just threw them in another area. So I was manipulating limpets and starfish and snails in the upper intertidal and I had a whole program down lower in the tidal zones manipulating algal competition and other plant-plant interactions.

PB: This was at several sites?

PD: Yes, I think that was the important thing that I did. I think it is probably one of the most important conclusions that nobody has seen. My two papers did get recognized for focusing on disturbance, which I did, but I did it across a gradient of wave exposure from the outer coast all the way to some of the really protected areas in the San Juan Islands. I had about six, maybe seven sites. So what you are looking at is the strength of the biological interactions across a gradient of exposures. Within each gradient, you had the exposed side and the protected side of a reef. Then it got really rather too complicated for a dyslexic to evaluate, even with my wife helping me do the statistics when I was writing. So I never really maximized the pretty sophisticated design I had somehow put together in my ignorance. I don't think I maximized the exposure stuff, that's still not recognized in the literature and it is one of my major career mistakes in my mind.

When I was writing my papers, at that point the competition people had become so powerful and so obnoxious in ecology that I was sort of picking a fight with them, and I didn't really emphasize all of those exposures. But my God, to get the exposure gradients, I was driving several thousand miles a quarter.

PB: Plus low tide is whenever it happens.

PD: Yes, the low tide is when it happens, and I still had to earn a living being a teaching assistant, so I would drive all the way back to UW for a course and sometimes get back on an early ferry go back out. I put together a list of the visits to each study site over five years, per month. It is appendix one in my UW PhD dissertation, and is entitled 'Number of visits per month to each site.' I put one in my box in the library. If you look at that list, even though I would visit maybe two study sites on a tide or whatever I could, you could see that to do that you were in the field constantly. And the drives could be exciting with slow traffic and other obstacles. One Easter morning at about 2 am they had put on an extra ferry, the old wooden Klickitat, and it was cold and I

was sitting in my little Scout when the car beside me started up and caught fire. The guy ran off and I opened his hood and shot off my fire extinguisher to no effect and I was worried about my Scout burning up, and I opened the chain gate at the back of the ferry and tried to talk the guy in front of the burning car to back up with his metal trailer and push the mother off the back of the ferry, but then crewmen showed up with those old fashioned acid-soda fire extinguishers and started hosing out the fire but the stuff squirted through the fire as they were not shooting it at the base of the fire and they hosed off my crotch as I stood on the other side of the car! The ferry jammed into the dock and they started unloading us as fast as they could and I drove off with my crotch on fire from the acid and I parked in the cold rain and ran into the water at the base of the dock barely saving my jewels from the acid!

PB: Wow, looking at your list, I see from 1965 to 1970, there are eight sites. Some sites were not visited from the beginning, but as you say overall a high number of visits at the sites over time.

PD: It is a huge amount of geography that I studied, very intensely.

PB: You would visit sites from one through four, to nine, ten times a month.

PD: Those high visit frequency sites were probably easy access sites at Friday Harbor that I could get to. Paine asked for that table for my thesis because he was impressed with how much time I was spending out there. There are some mistakes in there too, I can't explain it. I think I had people visiting my study sites while I was in the Antarctic in 1967. I put them in there which isn't really quite fair.

So, foraging theory at that time was better understood in my mind than it is now, because it was based on natural history. It went back to people in the 1930s who were working with muskrat foraging and other things. Predator behavior and foraging biology was something we thought about a lot, and we tried to figure out how to model it. We did it in terms of maximizing the energy and the time and the risk of a predator. That wasn't me, that was John Emlen and people in Paine's lab, and in Orians' group too. So we were all thinking about it, and I did too. I was trying to keep track of all of my various predators, in this case species specific, and I tagged tons of them. I watched their behavior and watched how they foraged. You can learn so very much sitting there watching animals and I spent as much time as I could snorkeling over them at high tide.

PB: At each site or just at some of the sites?

PD: not all sites. I could not snorkel at high tide at Waddah or Tatoosh Islands, for example, but at the other sites I tried.

PB: You tagged them in a certain area?

PD: Yes, in a certain area. I started off with some fast drying paint in different colors.

PB: How do you get paint to stick on a limpet shell in the intertidal?

PD: I got paint that people were using on model airplanes, and the butterfly people were tagging butterflies with it, and it would stay on for a whole summer. I learned later that Peter W. Frank, an ecologist at University of Oregon, had already done it with limpets, and I didn't know about it, so we did it too. He published it and he was correct. Some of the limpets have very distinct home ranges. They would come back to the same scar even though they foraged maybe two or three feet away on a tide. So I was looking at the foraging biology but I bloody never worked it up in my publications, and I really regret that.

In hindsight, I missed a chance to maximize the geniuses of Bob Paine and Gordon Orians, because Orians assumed that predators behaved optimally. Paine was going at it by evaluating the energy budget. In that era Paine was doing a lot of bomb calorimetry work in calories and respiration and such, which I had already done at McMurdo, so I understood that. But I might have done more to blend these two approaches because Orian's approach was so powerful.

I still think foraging biology needs to have both approaches. I didn't do a very good job of maximizing that, but I had my hands full with what I was doing. Paine had another student, Thomas M. Spight, who was working on the snails. That was being done well by Tom, so I didn't really get into the foraging biology as much as in hindsight I wish I had, especially the risk. I still think that risk and fear are really critical components of predators' behavior, so I sort of missed on that.

PB: You got all your training from the professors?

PD: Yes and no. I was made into a better scientist by interacting with the graduate students. I was really lucky in that we had two sets of graduate students that really interacted a lot, all the time. Every evening they would be up in their offices fighting and arguing about things. There were some that were just finishing when I came included Henry S. Horn, Eric R. Pianka, John Emlin, and John Stockton. These guys became quite famous ecologists in their own careers, and they would be talking to each other a lot. There was just a tremendous amount of student interaction with the students explaining things to each other and challenging each other.

PB: Were you talking about your interests, were they sharpening you?

PD: They were sharpening their own interests, but us younger guys were sitting there all ears. Then those guys went away, and we all picked it up. I had my own cohort working really hard to educate me. It was Gordon Robilliard, Bruce A. Menge, Chuck Birkeland and Richard R. Vance, and a little later Sarah A. Woodin. We talked and argued all the time. Poor

Rick Vance trying to teach me statistics, it must've been hilarious for an outside person watching, because I couldn't learn statistics, and Rick just couldn't figure out why anybody couldn't understand these wonderful things he was telling me. What a great education from the students, and then the naturalists at University of Washington especially at Friday Harbor, there were a whole bunch of professors that loved natural history, and they were out there a lot. They encouraged us students, all the time. Robert L. Fernald was one, Paul L. Illg, Alan J. Kohn, these are just wonderful people.

And George E. MacGinitie, who made his career in California and in Alaska, had retired on San Juan Island, and I made friends with him and interacted with him a lot as well. I was really lucky in that I had a huge amount of theory pushed down on me from the Gordon Orians group with my natural history supported by all these other guys. I just think somehow the gods put me in the right spot at just the right time. I don't think you would get that anywhere else now. That's just pure luck to have taught by so many really good people. I just wish I had been able to better maximize it.

PB: How did studying disturbances develop from that, because you haven't talked about that yet? Did you artificially create disturbances and disturbed areas?

PD: No, I didn't. I saw what the limpets were doing, and I was interested in succession. I was interested in how things settled, why they settled in some places and not others and the patchy settlement behavior that they showed. I don't think I answered it, but I was really quite interested, now that you mention it, in sterilizing the rocks so I could look at the settlement biology of barnacles that wasn't affected by bacterial films and algal films that you could see. So first I got one of those tanks you pump up for spraying insecticides on the garden. I filled it up with diesel oil and white gas and took it out and would torch that thing off like a flame thrower.

PB: To burn the rock off?

PD: Yes, I tried to burn the rocks off. It was pretty exciting, but it left this film of carbon. I realized that that wasn't what I wanted but it did a wonderful job starting my campfires in the rain. So then I went back to my trusty dynamite from the Antarctic and from my logging camp, and drove my Scout one rainy day to a lumber supply store in Forks, Washington. My little 1964 Scout I was so proud of was the only car in the parking lot. I walked in and told him I wanted a case of dynamite, and he got it out. 60-40? Yup, 60-40. He put it on the counter, and I paid him and hauled my case of dynamite out and put it in the back of my Scout, came back in. "Now I need a box of caps." He said, "Well, son, you know," looking out at the only car in the parking lot, "I have to remind you, of course, that the state law says you can't carry or transport dynamite and caps in the same vehicle." We both looked out at the Scout, and I looked him in the eye, and I said "Oh, Sir, I wouldn't

even think about that." He said, "I didn't think you would." He gave me a box of caps, and I put them in my glove compartment and drove off. So then I went up the outer coast, I never used dynamite in Friday Harbor. I blew rocks apart in the intertidal to get virgin surfaces.

PB: Large rocks that you cracked open?

PD: Well, I stuck the dynamite into cracks and tapped it and everything that the old loggers had showed me. I also got some fuse, and I know how to put the fuse into the cap. I would run it out and figure it was about two minutes which would give me time to get away. I would light it off, and boom! Stuff would fly all over the place, it was wonderfully fun. I split the rock in half. You can't imagine how much fun that was.

PB: Probably not. And that created the surface you needed?

PD: That created a virgin surface that was my virgin settlement. That gave me an idea of settlement without any disturbance at all, chemical or anything. It didn't work very well, because inside those rocks, it was sandstone mostly on the outer coast, there must've been some sort of chemical because nothing did settle. It took two or three months to weather enough so that things started settling, and by that time I was out of phase with all of my experiments. So it was fun, but I didn't really learn very much. So the disturbance research just came from testing the various animals and things that were disturbing.

PB: I was thinking of more spectacular disturbances like boulders, something cleaning off the rocks, a log.

PD: Thanks for mentioning that, because that was one of my fun projects. In those days and through prehistoric time, intertidals all over the world had a lot of logs on them. There were a lot of logs up there, and they bashed the intertidal, and I could see that this disturbance as stronger in some areas more than others. In some areas logs accumulated in an area and rolled around causing all sorts of damage, and in others the current carried a log around rocks, and the log didn't bash it. But in most areas they were a major factor. So how do you study that -- you can't control that. All you can do is measure it. First I tried gluing in little glass tubes, but they all got battered by algae and things too. So I rented a stud gun and took it out, hammered nails all over the place. It too was so much fun; I had a big box of sturdy nails, or studs, and a big box of .32 blank shells and it really hammered most nails in but some times they went zinging off my leg or around the intertidal. It was really cool. Eventually I was able to evaluate the survivorship of the nails over time as a proxy for log damage. There is a table in my thesis paper that shows that, and some areas have a lot more log damage than others.

PB: Well, you see the logs tossed on the northwestern coastline, it's stunning how huge they are. You can just imagine the impact on the coast of something that big.

PD: When I went to publish it, both the reviewers said no, the logs are an artifact of logging. But they are not, and so I had to have some sentences in my paper about that. When I was doing archaeology in Alaska in 1960, we drove down the Alcan, and there are some museums at Whitehorse that had the ship logs of the old stern-wheelers that came up the Yukon to Whitehorse. The stern wheelers were constantly getting holed and sunk by all the lumber coming down the rivers. And way above the tree line where we were doing archaeology, up north of Kotzebue, in some cases the people would have burials in cedar. They picked out cedar logs for their burials, so there was enough lumber on the beach all over the Arctic.

Then I read about Vancouver or one of the early explorers that were in Washington State, and a landslide that brought a virtual forest down on him killed some of his men. When Lewis and Clark came down the Columbia, they talked about a huge logjam in the Columbia. So logs are part of the system, and there are whole families of clams in the deep sea that live on sunken wood.

PB: If you hike along the Oregon coastal trail in southern Oregon on those cliffs, you see a story of trees falling onto the coastline and into the ocean all the time.

PD: All the time, yes, but all of these things build up, so the competition wasn't a very important factor for much of the time, and that is what my work is really seen for. I didn't sell the wave exposure gradient very well.

PB: Has someone worked on that since?

PD: No, that's sort of lost, it's in my thesis. It's in the papers, you look at it and see, and I talk about it in passing. When I set up the project that was one of the major questions. When I went to write it up, we had just been at an AAAS meeting in Boston at Christmas of 1969. Robert T. Paine had been brought in by Robert H. MacArthur as the token non-competition person for a big symposium on the glories of competition. Paine talked -- giving me credit, he used my thesis in his talk in Boston in 1969, and the audience booed him. J. Frederick Grassle stood up and said, "You have to be lying, this just can't be true." and I'm in the back of the room, and much of the abuse Paine was catching was my thesis. I was just about to start writing, so that was on my mind when I wrote it up. I wrote my thesis with a vendetta, against these dogmatic competition twits that refused to see natural history. I sure wish I had remembered my huge effort to look at physical gradients.

PB: That must have really sharpened your pencil to make sure you were getting it right, because you knew you were firing a broadside at the ship?

PD: The paper was designed to be a rebuttal to the mindset that dominated the 1960s, and in all of that excitement, I forgot my wave exposure, and I really regret it.

PB: You are referring to the ecological monograph in 1971?

PD: Yes, there are two, there's 1971 and 1975.

PB: The paper in 1971 then, did people start changing their viewpoint?

PD: They cited it. It's in that citation story I wrote for Citation Classics. I think the only reason I got any attention at all, because it did go against the grain, was that Joe Connell actually was listening to us while we were arguing with him through the 1960s and wrote his own paper about disturbance in 1970 in Ecology, then another big review in 1972.

So Joe Connell, who is a really charismatic bright guy, and Bob Paine, another charismatic bright guy, were on the seminar circuit selling the intertidal system. So my thesis papers fit into what other people were doing, and I think citation is just a matter of gossip. I don't put much stock in it at all. I think that the recognition almost never came from people actually reading it. It just came from -- well, this is something to cite. I regret that because I think there is a hell of a lot in there that people cited, so I get whatever brownies you get for a citation, but in terms of communicating to the future, I did not succeed. I would have been more successful communicating my work if I had broken it up into a whole bunch of small papers. There's just too much to digest in those two papers. That's been true of my career. The important papers for me have been too big to read. I had a hard time learning that.

Before we leave Friday Harbor as a topic, we were diving a lot, and we were doing subtidal work. Robert L. Vadas was a graduate student at UW when I first came. He then left right after I came, he didn't turn his thesis in until 1968, but it was pretty much done by 1965, I think. His thesis is the single best PhD thesis I think I have ever seen. If you look at it at the time when he did it and he set it up and started asking his questions, he was very much aware of the then-young Thomas Eisner working with plant chemical defenses against insects in the 1960s.

Bob Vadas did that with algae, and so we were seeing algae as chemically defended versus not defended. We were seeing gradients of chemical defenses in kelps thanks to Bob Vadas, who was really very generous and took our hands and showed us around. He would come and find me, dig me out and tell me things. He was a wonderful and selfless mentor to us. Those generous interactions get no recognition but they can make a career!

At that time, Michael Neushul had been at Friday Harbor, and he'd left to go to UC Santa Barbara. He was doing environmental physiology. He had a little train with tracks that went down a ledge to about 80 feet at Friday Harbor, and he would have all of these little train cars that he would lower by a rope exposing algae to all of these different environmental conditions. So field experiments were very much going on. We didn't

create them, those guys did, Joe Connell, Bob Paine, and Harry Hatton long before them.

Joseph Connell has a Citation Classic for one of his papers, and he credited Hatton there and he talks about Harry Hatton, the Frenchman doing it in the 1920s and 1930s. There is a legacy of ecology that seems to be lost these days with students who don't go and dig out the older hard-to-find references. You don't find Harry Hatton's work published electronically [due to its age and copyright protection], you have to go find it.

[Hatton, H. 1932. Quelques observations sur le repeuplement en *Fucus vesiculosus* des surfaces rockeuses denudees. Bulletin du Laboratoire Maritime de Saint-Servan 9: 1-6; Hatton, H. 1938. Essais de bionomie explicative sur quelques especes interestidales d'algues et d'animaux. Annales de l'Institut Oceanographique 17: 241-348; Hatton, H. and E. Fischer-Piette. 1932. Observation and experiences sur le peuplement des cotes rocheuses per les cirripedes. Bulletin de l'Institut Oceanographique 592: 1-15]

PB: You're not seeing Hatton cited in bibliographies of relevant papers?

PD: Oh, you never see it. I cited Hatton, but barely read it. It was in French and I couldn't read French very well. I could look at the tables though, so I had a pretty good idea. I had actually held his paper in my hand and turned pages and tried to understand it and took notes on it. I can't say I really read it. Joe Connell read it. I would say any time you see Harry Hatton's paper cited since the 1960s, I'd be surprised if the citing author has read it. It's pretty unusual to find it cited, and yet he did almost everything we did. In the kelp world John Alwyne Kitching in the 1930s and 1940s did almost everything we. So there is history that is repeated, and it is a real problem in ecology, people don't do their homework.

PB: I wanted to then talk about finishing up at University of Washington, you were looking for positions, how did you transition to Scripps Institution of Oceanography?

PD: Getting this job at Scripps was just another wonderful piece of luck. If I ever wrote a book about myself, the title of the book would be *The Luckiest Dyslexic That Ever Lived* because of the convergence of so much of my life at UW and of those four or five years. Almost everything was swirling around, and even if I didn't understand it very well, I was picking it up, and building on it. What a lucky event for me.

It just got better when I came down here. I came down and presented a faculty recruitment seminar in February of 1970. My thesis wasn't written, and I had just come back from that AAAS meeting in Boston. I was writing my thesis in pencil, longhand, typing it and struggling in my living room. So I got an opportunity to present a seminar at Scripps. I didn't think I could ever get the job at Scripps because I can't do math

and I thought real ecology was still all theory. So a chance to come down here, I just took it but with no serious expectations. At the faculty recruitment seminar, I didn't have my data analyzed. I drew curves on lanternslides, we did not really get good slides of graphs, and at least I didn't. I could show them pictures, and I could assert this and that, and they just took my word for what I was going to do when they actually hired me.

PB: How did you hear about a position being available at Scripps?

PD: Well, Robert Paine was connected, and people, like today, will write about it. I'm not sure that they did formal advertising then.

PB: So you were invited to apply?

PD: Yes, but people knew about it, there was good networking.

PB: Did you apply to other universities?

PD: Yes, I applied to Oregon State University and the University of Hawaii and Florida State University. University of Hawaii was all into modeling and computers, and they took somebody else, I think that they took one of Joe Connell's students, John Stimson. I got offers eventually from other places as well. I was attracted to Scripps because I wanted to work on kelps. Scripps had lost Wheeler North and his kelp program, Wheeler leaving for Caltech in 1962. Scripps wanted a kelp program, and I wanted to work on kelps. I had reasons for it due to parallels with the terrestrial world, which turned out not to be very good parallels, but that's what I was trying to do.

PB: How did you represent yourself as being someone who would work on kelp? Was it based on your intertidal work with algae?

PD: The second part of my thesis, which came out separately in 1975, was all about algae. I did a lot of algal work, and diving too. So there was quite a lot of algae research in my thesis, and I talked about that. Then I gave a totally separate seminar on the Antarctic work that we had done, so I really had sort of two careers I was selling them with precious little data analyzed.

PB: You were a double threat.

PD: Yes, or a good bull shitter, and I think that helped me get the job here. Surely with my lack of quantification, I didn't deserve a job here, but I got it being careful not to tell them how stupid I really was.

PB: Do you know why they were interested in having a kelp-oriented professor?

PD: Not really, no, I don't. As I reconstructed my hiring, I wasn't the first choice. They hired George Somero first, but then Bill Fager managed

to round up some money. I think at that time there were conflicts between marine biology and biological oceanography that I was not aware of.

PB: The separations between the two areas at Scripps went back that far?

PD: It went back long before I came. I think Fager managed to extract something for my position because the marine biologists had done something sneaky to get George Somero. George was a much better scientist than I was, I didn't have any problem having him get it first. We were friends, I knew him from the Antarctic, and he was at University of British Columbia, and it was just fine with me. I didn't know that he got a job at Scripps. I didn't know that I had lost the job, nobody said anything, and then several weeks later they offered me a position too. I didn't know what was going on. Shortly after I came, but maybe about that time, the Sea Grant college system was getting set up. Scripps was involved with that, George Shor especially, and John Isaacs, and the Biological Oceanography group were involved with that.

There were two Sea Grant professorships, and one of them went to Robert W. Elsner, who was an environmental physiologist. I think that contributed to the balancing of power, that they argued that you have to give one professorship to an ecologist as well. So Elsner had one of the Sea Grant professorships, and I had one. Elsner then went to Alaska, and Clint Winant inherited that niche. Clint and I still do, if he hasn't retired, we both had Sea Grant professorships.

PB: What is a Sea Grant professorship?

PD: As part of having the Sea Grant program at Scripps, Scripps negotiated two FTEs. Everybody told me not to worry, it was one source of money versus another, and you are not committed to do Sea Grant oriented research if you don't want to. But of course, it was close to me anyway, I did want to, so that wasn't too much of an issue. So that's how I got hired, while I was still writing my thesis. It was really lucky.

PB: Wasn't there something about turning your thesis in?

PD: Coming to Scripps includes the fairly famous story of trying to turn in my thesis at the last minute at UW. The house was rented out, belongings packed in the car with the U-Haul trailer out in front, and at the very last minute we are typing page numbers because in those days things were typed by typists.

PB: Your wife was working on it with you?

PD: My wife Linnea had helped me with the analysis and stuff, so she and I were sitting in Paine's office while Miriam down the hallway was busily typing page numbers into the thesis because you were always moving stuff around when you're using typewriters to write theses. So you didn't type the numbers in until the last minute.

I went over to the registrar's office about half an hour before the university closed for summer quarter, after which there was a three-week hiatus before fall quarter. I went with my beautiful thesis, and this woman, like many registrars, was known to be a witch... to measure your margins and that sort of exacting thing. So my margins were perfect, I was as cocky as I could be, Linnea and Paine were over there drinking a beer celebrating the end of this day and getting on with my life. I went over to the witch, and she took a quick glance at the margins and then started turning pages. Miriam had skipped page 18... pages went 17, 19. Then there was a hue and cry, "You're going to have to re-register," and I thought I was going to have to come back from Scripps and turn in my thesis again. I needed a page 18. I went running over with the idea of ripping a page out of Paine's book with a random number table, which I had used for randomizing my sampling out in the intertidal. I had used a random number table, and I was going to find Paine's, and I couldn't find it.

I'm desperately in there, and I came in to Paine, and Linnea says, "Paul, how about this." And I had just given Paine the only copy I owned of me shaking Lyndon Baines Johnson's hand when my mother won the teacher of the year award in 1966. There was a picture of me and President Johnson. I had given it to Paine because we were all so angry with Johnson at that point for the Vietnam War. So it was sort of a joke, and there it was on his desk. We all looked at it. Yup... we put glue on the back of it and smacked it on to a piece of paper that I had typed "18" on, and stuffed that into the thesis, and I went running back with a page 18.

The witch immediately turned to it. "Oh, I can't accept that!" She started to close the window, and I was anticipating that because now it was just about 5:00 and I had both arms under her window. I grabbed her phone and dialed Paine's number, with those old hand dials, ring and ring and ring and ring, and then I gave it to her, and I yelled into it, "Bob, she's not taking it."

I could hear Paine screaming at her about how he'd had it with her, she had been giving his students a hard time for years. He had just been involved with hiring a new president at UW, president Charles Odegaard, and said, "I'm going to go talk to Charlie and get you transferred to a broom closet." Paine was just screaming at her with it all going in one of her ears, passing through her empty head and out the other ear for me to hear. His final remarks were, "I am the only person that can pass judgment on whether that page is acceptable. You have no business making that judgment, that is my sole responsibility." He's the advisor, he's in charge, he represents the University, not her.

She just turned bright red, slammed the phone down, grabbed the thesis, and growled, and slammed the door down almost taking my fingers off. So I thought we were done.

PB: I thought you were too. There's more?

PD: Yes, there's more. I'm not sure what the statute of limitations is, but finally I came down to Scripps. I had been here a few months, we had rented a house in Pacific Beach. We were buying my house in Solana Beach at the time. This tube came in the mail with my diploma. I just let it sit for awhile because I was really busy trying to teach and things. Finally I opened the tube to see what a PhD diploma looked like, and it was a Masters. What she had done is changed my registration from 400 to 300, so my thesis was registered as a Master's thesis.

PB: The revenge of the --

PD: The revenge of the bureaucrat.

PB: Making a quote-unquote mistake.

PD: Yes. Paine says not to worry, and the thesis sits up there in the PhD thesis section of the library.

PB: With that page 18?

PD: With the page 18, oh, it's still there. Lots of people go to UW. It started a growth industry in ecology of page 18 for the UW theses. Almost all of them after that had a phony page 18.

PB: Truly?

PD: Yes.

PB: You mean just from your advisor's students, or other advisor's students?

PD: Other students too.

PB: In the department?

PD: Yes.

PB: Page 18 became a legend.

PD: Well, apparently. I'm not sure if they still do it, but it went beyond Paine for a while. I think almost all of Paine's students have a phony page 18. None of my students have had the balls to do it.

PB: You haven't done it here at Scripps?

PD: Not to my knowledge. That's a good story, so I'm not really sure I have a PhD.

PB: You never got the corrected PhD diploma?

PD: No, no.

PB: But you got the Masters diploma?

PD: Yes, and I lost it.

PB: That Masters diploma should be on your wall.

PD: I know.

PB: Along with page 18, a framed presentation of items about your PhD.

PD: Not only that, but I had saved the letter from Dean Liengenfelder telling me I should save both him and myself the agony of flunking me out and quit when I got that C the first year. I would give anything to have those things, but I think I put them in the fireplace and burned them.

PB: Oh! Can't you see it, a framing of that letter, page 18, and the Masters diploma? It would be right in my living room if I had something like that. Not that my wife wants it there because she decorates the house, but that is great.

PD: It would be in my living room too, certainly in my hallway. I'm pretty sure I burned it. I got pissed off, and I have a bad temper.

PB: I'm going to order a copy of your dissertation for the UCSD Library collection, because I don't think we have it. It will come with page 18?

PD: Yes, it'll show it. We typed in a caption, "Former President Lyndon Baines Johnson congratulating the author on a magnificent piece of research."

PB: Oh, is that what you did, you added a phony caption?

PD: We typed that in.

PB: I've heard a lot of good stories in these oral histories I've done, but that's got to be one of the best ones. I can only imagine your stress, though, as 5:00 o'clock approached and you ran back.

PD: Oh, yes, I was supposed to be here in July. They started paying me in July, and I felt every week I was ripping off this wonderful place, and I was nervous. This was mid-September, so yes, there was stress. Paine handled it magnificently.

PB: Well, kudos to him. He kept his eye on what's important.

PD: He's been wonderfully supportive all my life for what's important.

PB: He was obviously very supportive. He supported the Antarctic proposal because of your interest.

PD: All of this time he was supporting me with the Antarctic stuff, which took up a lot of his time with the paperwork.

There was something I wanted to mention, -- when I had that generator on my back and was working the intertidal, you carry that thing around and I had two big fiber buckets that you fold up. We just had a seminar by the Craighead brothers who were at the Montana glacier working on grizzly bears. The seminar was all about grizzly bears and how they were eating people at the glacier that summer, so these guys were in high demand.

They said that they would much rather deal with a grizzly bear any day than a black bear, that black bears kill you. Grizzly bears are proving a point, and if you just lie down, chances are they won't kill you, despite the fact that they killed some people in Montana. Black bears are very bad. If you are within the attack or flee boundary, which is quite large sometimes, you are in real trouble when you encounter a hostile black bear.

I had worked the night tide at Point Portage Head, one of my study sites, and I had my four-wheel-drive up this muddy road. About a mile of trail that I had made down the cliff, down a muddy bank to a cliff and then down the cliff through salal and the coastal bushes, a small trail that I had made. I was plowing up it at dawn having worked the tide that had started at 2:00 or something, and was tired, with my generator on my back and two buckets, slogging up and not paying much attention.

And there was a woof! I had just run into a huge black bear -- I think at first sight we were about ten, twelve feet apart. By the time we actually stopped, he was about six feet from me when he stood up. He towered above me, and he kept going woof! Woof! I just stopped. I knew not to flee. I didn't know what to do. I dropped the buckets, and he went woof! Woof! I'm standing there with my heart thumping looking at him, and he's looking at me from above me, and I figure I'm going to die. So I'll get a picture, I had my old camera, a Voigtlander around my neck. So I slowly reached up to ratchet the film up to get a picture of him standing there. It went ratchet-ratchet and stopped because I had run out of film. You ratchet and it hits the end of the film. It can't shoot or anything, so there's going to be no picture of my demise. As I ratcheted it, the ratcheting scared the bear. With a much louder woof, the bear dove into the underbrush and disappeared. I thought he was still there. I just stayed, my heart thumping. Then I heard him down somewhere off a fair ways, running.

PB: Intertidal predators?

PD: Yes, actually. They really were a force of nature in the rocky intertidal. They got in -- they rolled rocks going after things to eat. They really disturbed a lot of area. The damage reminded me of javalina rooting around hillsides in Arizona.

PB: What were they looking for?

PD: They were looking for crabs. I watched them doing that. I had some other close encounters with a mother and her cubs, and they were all rolling rocks getting crabs.

PB: Interesting, that's part of the intertidal disturbance.

PD: It's part of the disturbance. I didn't write it up because it wasn't a very common thing, but it does happen.

Anyway, we came to SIO. We hauled down here and got lodged out in Del Mar Heights.

PB: No children yet, right?

PD: The kids came a little bit later. Moving to Scripps was just really wonderful, because at Scripps in 1970 and in the 1960s, it was a really cool place with people that got along really well. They talked, everybody talked to each other. The Biological Oceanography group welcomed me. The chairman of the graduate department at the time was Jerry Winterer, and he couldn't have been nicer. They didn't have any space for me, so they took space away from Francis Haxo or somebody in old Ritter Hall, on the first floor right across from the snack bar. My lab was that corner room on the northwest corner across from the snack bar - it was my empire. They got me set up in there, in that one little room. That's all I had. There was no lab space or anything. That room was my world. Space is always an issue here. I was writing my thesis for publication at this point, rewriting it. Susan Jorgensen in the department office typed it for me. Dave Wirth was taking care of me. There were people all over the place that were just extremely helpful.

There was -- there still is an organization called Oceanids. And John Isaacs's wife, Mary Carol Isaacs, was running that. They spent two days taking Linnea all over the county, showing Linnea all sorts of things as a new faculty wife. You just couldn't find nicer people for a young scared dyslexic.

I had friends. I'd met Elizabeth 'Pooh' Venrick, who had been a student at Friday Harbor. She was about my age, maybe a year older. So I knew Pooh, and Loren Haury had been a childhood friend, the son of Emil Haury. Mike and Connie Mullin had been coming to Friday Harbor for years and we were long time friends as well. So I had friends I knew before I came. The Biological Oceanography faculty was really interactive. We met every Wednesday at noon for a lunch which often was just a lunch. We sat around in one of those corner rooms in old Ritter Hall, or in that lab which is now across the hall from John McGowan on the first floor. We would have a lunch where people would eat or just talk about what they were trying to do. I remember talking about my kelp project and plans to go to Amchitka and Chile and having people interact with me with good suggestions.

We would have big arguments, always there were arguments, but they were constructive, not getting mad type of arguments. There was a lot of discourse.

Fager was very much the senior person around, and everybody was very respectful, somewhat fearful maybe. Fager was shy, and it came across as being remote, but he was just shy. He was actually a very warm nice person. So this is what I moved into.

Then we had Wednesday seminars attended by all of the Biological Oceanography faculty -- we all went to the noon lunch if we were in town, and we all went to the seminars. The students would present and then we would interact. So the students got to see us very closely interacting with each other and with the students. I think it was a pretty healthy, nice.

PB: Has Biological Oceanography done similar things over the years to this day?

PD: Well, it's all sort of faded out, we don't do it anymore. In different ways it faded out, and I think with critical mass, with new people coming in, and especially with old people retiring and being replaced by other non-biologists, non-ecologists. So our group got smaller and smaller.

For example, it seemed that all of Sverdrup Hall was called the Food Chain Research Group and that is completely gone.

PB: John D. H. Strickland?

PD: Strickland's group, yes. They interacted with us, they were on our committees. Richard W. Eppley, John R. Beers, Angelo F. Carlucci, and a whole lot of technicians that were really top notch scientists and very helpful. They were really nice people over there, they interacted with our students and interacted with us.

I remember a very young William F. Perrin giving a seminar which he emphasized had to be secret about the tuna fishing porpoise bycatch problems. Perrin was an observer I think and was trying to document it. That's how the tuna porpoise issues broke with Bill Perrin as an observer collecting data on just how many dolphins were caught. The importance of that situation was recognized by a young Bill Perrin and in many ways his sincere conservation concerns and wisdom have had a huge impact. Indeed, it probably precipitated the Marine Mammal Protection Act.

PB: Perrin was with Southwest Fisheries Center at that time?

PD: Yes, I think so. He didn't have his PhD, and he was young, but he was a very good marine biologist. I still remember it as a really good seminar.

Robert May came through several times and would talk to the students, so there was a lot of vital academic intellectual things going on in the early 1970s. Ramon Margalef and Simon Levin were here often, as was Joe Connell.

Other social things going on is that Ralph Lewin was running something he called the La Jolla Naturalist Society. Once a month he would have natural history lectures in Sumner Hall and probably 50 or 60 people from La Jolla would come and learn natural history from natural historians. Ralph ran it on his own energy, and when the energy faded with time, it disappeared.

PB: You would go to those?

PD: I went to several of them. I talked at them and he organized field trips. It was a different -- it was an interesting place. We had other people that were around sort of peripherally, they worked on the teaching faculty but they were very much with us. Ruben Lasker was up at National Marine Fisheries Service. Lasker was just a wonderful guy, an oceanographer, and very friendly to me, really helpful and very wise.

In my hallway, I eventually moved up and was next door to Fager in a room where they had computer printouts. They gave it to me so I could move out of that dungeon and have a lab down there. Abraham Fleminger was there, Ed Brinton was there, Martin Johnson was still there. Johnson came in fairly regularly, and he was very happy to talk to people.

PB: He was very senior by then.

PD: Very senior, to me he was such a grand old man. He was one of those that you just listen to. Edward Goldberg was around. These were nice guys. Of course, there was always John Isaacs. Isaacs would come in and smoke big cigarettes in my office at 6:00 as I was trying to go home and help Linnea put the babies to bed and things. Isaacs would show up. I would sort of moan, trapped in my office in his smoke, I didn't want to insult him and I would sit back and listen to him. Eventually I realized that I was learning really wonderful things as he relaxed with me at the end of the day.

PB: And you were an assistant professor.

PD: Yes, I was very the rock bottom rung. I learned so much from that guy. Isaacs was just a jewel. He was amazingly bright but also one of the nicest most compassionate men I knew. People like that are very rare.

PB: He wanted to ask questions about your interests and stuff and then he would bounce ideas around, is that what it was?

PD: Yes and no. Well, he would ask me questions thinking I didn't know.

PB: In your area?

PD: Yes, sort of, whatever he'd been reading. Sometimes it was da Vinci discovering fossils up in the Alps, and figuring out sea level changes and the mountains rising. Another time he was reading something from an 1880-era Russian artist who had a biography he had written about his childhood in Russia in that era with a pet bear. The pet bear would go out and fetch water. The kids would harass the bear and throw mud balls in the water, and the bear would growl and dump it out and go get more water. Things that you just don't imagine. Isaacs would come in and just regaled me, and I'm sitting there fidgeting wanting to go home and help Linnea. But how can you not listen to a story about a Russian bear getting two buckets of water on a pole and carrying them in. I believed him. He never made things up.

PB: Did he ever chat about your specific research interests?

PD: No, not too much. He was always sort of showing me how much he knew. He would often test to me, ask me a question, and then explain it to me. Probably more than other people in the hallway, I was enough of a dilettante so that I could often deal with those and answer them and actually say something back. So I was never threatened and I think he appreciated coming in to talk to me because I hope it was obvious how much I appreciated him. He was doing it because he enjoyed it. I was doing it to be polite at first but also realizing that these moments were timeless treasures. I needed to relax. Think about what a cool place this was.

Meanwhile, Scripps was having really major problems with the upper campus. Scripps had the people who had been legitimate war heroes in World War II. You know, Fred Spiess and his submarine, John McGowan and his cruiser, and Doug Inman storming the beaches. Walter Monk. People that were very much involved with the Navy and saving our country, but they were all senior scientists at this time. They'd been around, and then suddenly another campus is put up, the upper campus. This campus is full of very young left-wing people that were in your face all the time. There was one time when Nixon invaded Cambodia, and students all over the country were in the streets, it was spring 1970. I had not come yet but the police actually were attacking students on the UW campus and my friends were beat up by thugs that turned out to be off duty cops. I gather that the local students were angry and threatening to riot and invade Scripps, which they thought, was an arm of the military because of the classified research going on here before the Mansfield Act. The Scripps faculty was very nervous as the students were talking about coming down and occupying buildings and destroying records and things. So people like George Shor, Fred Phleger and other geologists and I think folks in the directors office, a lot of guys were packing things up, and trying to defend themselves from the herd up there.

I wasn't here then, but when I came this had been going on just before I came. So the history of Scripps and the upper campus is very much of a love-hate. No love, sort of hate.

PB: Well, Scripps had huge Navy funding.

PD: Yes, and going from the left-wing person that I was in the 1960s, because when we invaded Cambodia, I was in the streets too. Coming down here I suddenly I saw the other side of things, and it opened my eyes.

PB: You had no misgivings coming here for the position in 1970?

PD: No, this was irrelevant to me, this was a science place. I didn't know anything about this political stuff.

PB: It wasn't like talking to Ed Britton when you were in high school.

PD: No, this was just great science, and all those guys were wonderful world-class scientists. They had been training students since the 1920s and 30s, I think, and they had been training the students through UCLA and Berkeley. I mean, Carl Hubbs' whole career of training people was through UCLA. So we had an established record at Scripps of training the excellent students that mostly created oceanography and much ocean biology around the world, came under attack from arrogant young upper campus people.

Now you have this upper campus that is disdainful to Scripps, picking fights. I used to go to all the faculty meetings, and many of us did then. I was there when they imposed the upper campus policeman, as I called it - the outside authority on Scripps' graduate committees. We were made a department of them, despite the fact that we had been here independent for a long time. Then they started imposing rules.

PB: To have professors on the committee from outside the Scripps department?

PD: Well, we always had one. That was our own rule from UCLA and Berkeley, and we just wanted to keep on doing that. They said you can do it, but you have to have somebody up here because -- and I was there. Several people actually said "We don't trust you to train students." They really said that in that arrogant condescending way that one still gets from some people up there. They explicitly said that we were not capable of responsibly training students - which we just counted things and were not real scientists. They were extremely scornful of field biology, of "counting things" said with so much sarcasm that they could not keep a straight face. Jerry Winter just went bonkers, and so did Dick Rosenblatt and Joe Curray. I didn't have the courage to go bonkers, but it was pretty ugly. Point of fact, when they set that campus up, it was a biology stronghold. They did have a physics department, and I know one of the students that was in the physics department, and he was sitting at Scripps before they built the buildings up there, so he has fond memories of Scripps.

The problem was with the molecular biologists, which David M. Bonner brought in, to build a campus in his image, and he hated natural history

stuff. They still don't allow you to have a course up there that is called -ologies. They still hate -ologies, like ornithology or herpetology. They don't exist up there. They did once have an invertebrate zoology course taught by John Pearse, then post doc'ing I think with Wheeler North. But except for Mike Soule, they were very specialized, narrow, knowledgeable young molecular biologists utterly lacking any semblance of wisdom.

We've had tensions over my whole career with getting files through the committees up there. I think every committee I've been on up there has been nasty. I did not know the game at first, I would walk up there as the SIO representative expecting a civil discourse, and it took me one or two tries to learn that I really have to prepare for a Scripps file because I'd be the Scripps representative and I was going to have a huge fight with some of the nastiest academics I have ever met. I often had to get ugly to get our files treated fairly. Once I actually wrote the UCSD Chancellor and said that the chairman and this committee are so biased that they should be reconstituted. The hostility in those committees is still real. Last year we were trying to get Ed Parnell into the Research Series, something they really have no business caring about, they just blocked him as they were just vetoing all of the Scripps files because they think that we don't have any standards. Thank God Tony pushed Ed's file through.

So this hostility went back to the 1960s, with the old guard down here and radical specialized molecular biologists up there. I still think a lot of the people that I run into up there are really very smart, but there is not a lot of wisdom.

Eventually they got some biologists that were really good. When I came Michael Soule was the only ecologist there, he was fighting sort of a losing battle and left. But somehow they quickly brought in Ted J. Case and Jack W. Bradbury and Sandra L. Vehrencamp, and they were first-rate, as was David S. Woodruff when he came. They are all first-rate ecologists, really excellent scientists, as good as they get in the world. So we had some good people, but they have all left except poor David who continues to fight the good fight. And recently there are some new people who seem great, and I sincerely hope that the tensions that dominated my career can slide into history and the new folks will become established and constructive colleagues.

PB: Were you involved in any of the considerations for an undergraduate degree in marine biology?

PD: No, that's quite recent.

PB: I know there was tension there between that being offered by the main campus.

PD: There was a lot of tension over that, and that is fairly recent.

PB: That's near the end of your experience of the relations between UCSD and Scripps.

PD: Yes, it is. I am sad because recent chancellors have focused on outside money and forgetting what a university is supposed to be. We have lots of money for buildings but they have to close the best marine library in the world for less money than a handful of those lawyers in risk management who also do so much damage to our field work and attention to time sucking required training. I think that the recent chancellor did a lot of damage to Scripps, and to the upper campus. She brought in a lot of money, but there is now a vacuum of any sense of the values we are supposed to be espousing in academe. Money and fame is all they care about while the students I interact with are getting abysmal embarrassingly bad educations. This is a reflection of really awful leadership. I think that UCSD has lost its soul.

PB: What do you mean lost its soul?

PD: In my youth I was led to understand that a broad understanding of the humanities was central to the concept of a good education in general and a major one involved both breadth and depth understanding of a field. For example, in my case a zoology major in the early 1960s involved requirements to pass many courses in both the humanities (history and arts) as well as comparative anatomy or embryology, comparative physiology, and three classes from mammalogy, ornithology, herpetology, ichthyology, entomology, ecology, and invertebrate zoology (often with many required field classes), genetics and at least two classes in other biological disciplines such as botany. In those days all of those classes were offered by almost all colleges and all Universities. UCSD has a really good humanities program but most of the biology students are in Muir and graduate without ever having taken any humanities courses. None. Unbelievable but true. I quiz most of the undergraduates I interact with and their knowledge of history and humanities is abysmal and embarrassing. For example, only 5 of well over 100 Muir students have been able to quickly find Greece on a map and they know nothing beyond high school readings of Homer about Greek contributions to Western Civilization. Indeed, it is embarrassing to ask them any question about Western Civilization or the essence of their own culture. And the poor kids get virtually none of the general education in biology that went into all college biology degrees at one time. This may be changing now with some new hires, but I would challenge you to find those courses on this campus, especially a course dedicated to living plant and animal species. There is no evidence that the new hires are remotely interested in teaching outside their specialties, especially outdoors where one usually finds nature.

Furthermore, in response to the litigious nature of our society, risk management has stepped in with the simplistic philosophy of reducing risk by preventing the activity. At a time when there is a desperate need to educate the public in environmental sciences, it is virtually impossible to teach a field class because of all the restrictions on travel and field activities. In essence I think that UCSD has reneged on all of the ideals

of what a University education should be going back to the 1200s. Perhaps worse, the students themselves seem to have little interest in the ideals of a broadly based university education. They might as well go into trade schools and learn to be technicians and drones, the very thing that the medieval universities attempted to go beyond as they brought students knowledge beyond the well established guilds of the era. But this is largely what we are turning out.

Anyway, back to the happy era of 1970. The other tension that still exists that I think is now history, was between Marine Biology and Biological Oceanography. I don't know how that started. It must've been a pretty ugly fight between Edward W. Fager and Per F. 'Pete' Scholander. Neither is ugly in any sense of the word, but the tensions were.

PB: That would have started in the late 1950s, that division or tension?

PD: Yes, it did, and I didn't know anything about this when I came. To me, Scholander was a well-known name. I had read his papers and I was really looking forward to meeting him. I walked right into it. I went to some gathering that Gerald 'Jerry' Kooyman got me to in the Physiological Research Laboratory, which was Scholander's new building then. Maybe it was a housewarming. Scholander there was there, and the room just sort of got quiet when I walked in.

PB: Because you are from Biological Oceanography, and you are at a Marine Biology gathering in Scholander's building, the Physiological Research Laboratory.

PD: I was an ecologist in his hall. It's hard to imagine -- Pete had his own mind, and so did Fager. To me they were both gentlemen, so I don't know where the tensions really came from, but they were palpable when I walked in there. I vividly recall it, and Kooyman didn't know what was going on. He was not aware of it, he had just invited me because I wanted to meet Scholander.

PB: You were Jerry's friend from your Antarctic research days.

PD: I was his friend from forever, as you know. I managed to deal with this because I had read much that Pete Scholander had done in the Arctic. Scholander and Laurence Irving were really important Arctic physiologists during World War II, and they set up the research lab at Barrow, Alaska. I'd been there when I was doing Arctic archaeology, and I had met Larry Irving at Fairbanks in 1960, so I knew him, so I could talk to Scholander. I had met Irving's son, William Nathaniel Irving, who in the 1960s was an Arctic archaeologist. I was respectful about Scholander's history with lichens, I knew about that. So I think I became a "good ecologist" and he was very warm to me. I always felt quite welcome in the Physiological Research Laboratory, but I walked into that and didn't know anything about it.

PB: No one had told you.

PD: No, not really. But I knew all the players because I worked with Kooyman and Elsner in the Antarctic.

PB: Was that when you first became aware of the split or the tension at Scripps between the two divisions?

PD: Yes, when I went to this party at PRL, but that worked out pretty well.

PB: So how did you get into kelp research - did you get into it pretty soon after you came to Scripps, you started looking at something or doing something?

PD: As soon as I came here, I got access to the ocean. I got an old boat that they took from Isaacs who had let it go to hell. They gave me \$500 as startup funds. It turned out when I had spent it trying to fix Isaacs' old boat so that I could go out there, that the funds were William Newman's money. He didn't even know, and he went to spend it, and it was gone. David Wirth managed to save the day, and the boat was terrible anyway.

There were a lot of divers then. All of the students were divers. They were good divers. James Stewart is one of the most magnificent figures in Scripps history. So many DSO's wallow in their authority and enjoy exerting their power, but Jim was a model we need back. He had a diving program that was safe, with good scientists made into safe divers instead of machos trying to do research. All of the students were divers, they were all happy to go out with the new professor, so I could make dives all over the place.

PB: So did you start out just observing and seeing the kelp and where you wanted to study something? You went out La Jolla here, but when did you go to Point Loma?

PD: When I first came that fall while I was writing my first thesis paper, I went out with all the Fager students. I was just their dive buddy, and went to their study sites. I did a lot of diving with Ann C. Hartline at the time. She studied an interesting ephemeral barnacle. I went out with Fager and Arthur O. Flechsig, and Jim Stewart took me out to the kelp beds. He had a boat that he took his dive classes on, so he was really good for me.

PB: Out to La Jolla offshore here?

PD: The boat was in Mission Bay, so I went all over Point Loma with Jim. People were working deep. There was a lady working on some gobies, and somebody else working on a deep seaweed at 120 feet, Maripelta, it's that little round algae. I went out with them and with Jim Stewart and Joan Stewart, his wife at the time, would come out. I would just puke my heart out, because I hate boats and I puke off boats anytime I'm on one. Jim

just made great fun of it, "Anybody call for Dr. York?" Joan was just all over him to be kind. So in that fall of 1970, I saw quite a lot of the kelp bed. John Pearse took me out one time to see where Wheeler North's stations were. So I knew where Wheeler had his stations, and I incorporated mine there. My first station was in the middle of the bed on one of Wheeler's sites so I could do the continuity. Then I got an NSF grant. I think I had the proposal in and got the money by February in my first year.

PB: And that was to study what exactly?

PD: The kelp forests and canopy competition and community dynamics. I got the grant, and it had money for a technician. I had been working with Rick Rosenthal, but he was working on kelps for Westinghouse that was up in Sorrento Valley. He had a station out at Del Mar off 15th Street, and he had two or three stations out in that area. I went out with him several times while he was working for Westinghouse. Then they folded and I hired him. He was actually quite important to my kelp program, because he was very good at doing surveys. In that time, a lot of the people at Scripps especially were really into sampling. They weren't very much into asking interesting questions, but they were into sampling, which was Fager's big thing. Rick was into sampling, and I wanted to do the experiments. Sampling, I thought was something you do that once just to draw a picture of what's there, and then you do the experiments and dissect nature that way. Basically Rick just wanted to lay out lines and sample.

I did the things that he didn't have any use for in my little experiments. Somewhere fairly quickly we had Brock B. Bernstein, who was also working in the kelp bed, and he put some lines out in different places as he also was doing good experiments. We had my experiments in the middle part, what we called virgin reef at the time and Brock had them along the inside and outside parts of the forest.

PB: This was all off Point Loma, you weren't doing anything out here at La Jolla?

PD: No, not off the Scripps pier, because the pier was so hard to get in and out of. You could put your boat in and get through the channel at Mission Bay.

PB: So you settled on Point Loma right away.

PD: I settled on Point Loma because of Wheeler's legacy and for logistic reasons, and I think for logistic reasons it was a good decision. La Jolla is a more interesting place in a lot of ways. I'm just as glad it all worked out the way it did.

Over time Rick's stuff was much more valuable than my experiments. I don't think people have paid any attention to my experiments, but the baselines that Rick started and that we continued after Brock Bernstein

are my most valuable legacy, and they were not mine! But those baselines are what make that program so viable now.

PB: Are any with respect to commercially fished species?

PD: No, we were just looking at kelps. That's the mistake, I wish to hell I'd looked at fishes too. I didn't know any of them, and my whole mindset was on kelps and invertebrates. I had been following sea otters from the beginning. From my first year as graduate student, we knew what they did because there was a paper by James H. McLean in 1961. We at Washington were very much aware of sea otters. They were trying to get some down from Amchitka to Washington State. So sea otters were on my mind. At that time the Atomic Energy Commission blew up a bomb in Amchitka, so they had to do a whole bunch of environmental monitoring. I got a chance to go and make two trips to Amchitka in 1971, maybe 1972 or 1973. This involved AEC clearance, FBI doing background checks on you, right after I got to Scripps, people going around asking about me. My colleagues must have wondered, but they didn't mind that I was being checked up on. I told them at the Biological Oceanography luncheons I'm going to Amchitka and here's what I hope to find. It was all about community dynamics with otters at a high density, the only place in the world at that time with otters at their carrying capacity.

That paper is probably some of the cleanest kelp experiments I've ever done, just two trips. That was in the early 1970s.

Following on the sea otters, I thought that there were otters in Chile, but it turns out they were river otters and not sea otters at all. I also had a proposal through George Llano, the NSF Antarctic guy, to take the Hero around Chile on two cruises and do some experiments in the Chilean kelp beds that I thought were there from reading Darwin. I had two cruises in 1972 and 1973 in South America, and a lot of it was in southern Argentina around Terra Del Fuego and Staten Island. All of this was going on in the first three years that I was here at Scripps, and it was pretty kelpie. I was pretty well immersed in just kelps, but I never really learned the fishes. I learned the invertebrates, of course, but the invertebrates weren't players in the later conservation traumas. The fishes were there if you pay attention to them. This was the beginning of the kelp program for me.

The other two things were my teaching, and consistently the most important thing for me were the students. The teaching was pretty much of a disaster for me. I came and they gave me the first year off, I didn't have to teach. Then they asked me to teach seminars. So I was teaching students, I was interacting with students, I did two seminars - very badly I am sure.

Then I started teaching -- I had to teach my own class. The only thing I really knew was ecology, and Fager was teaching an ecology class. His class was mostly sampling. It was different for me because I'm dyslexic and don't do numbers anyway, so I set up a course on general ecology. I

tried to cover all of ecology including terrestrial ecology so the students could understand what the rest of the world was actually doing. They were doing oceanographic things, and they weren't really aware of all of the stuff that was going on in general ecology, the stuff that we were very much aware of at Washington.

Conceptually the class was good, but the delivery was awful. I was god-awful. I was scared to death. I have always had panic and anxiety problems with speaking. Fager and all the heavyweights came and scared me even worse. We had just had a baby so I was up night and day, and I was falling asleep in my lectures. I remember those classes as just being a nightmare. Easily the worst days in my career.

The good thing that came out of them was I picked up the sense that a lot of the students were interested in terrestrial stuff, and they didn't have a chance to learn or experience it. I set up a field course in 1971 or 1972 which I still do. The field course was deliberately terrestrial. In hindsight I wish I'd done a diving field course, but they didn't need it at the time. They were all diving and they knew more than I do. Now virtually nobody dives anymore. The whole system is sort of structured to make it really hard. If you want to take a dive class, you have to go do it outside and get PADI certification. Then the dive class -- they call it scientific diving, but it's not science, not learning natural history and asking questions which is the essence of science. Science is asking questions, and the techniques come out of the questions. There is a lot more bureaucratic control now than there was in Stewart's era, to some extent because the world has become so litigious and there is a very strong air of authority. I am not blaming the lack of natural history on the diving restrictions although they don't help. The problem reflects a global trend to pretend to study nature in the lab and never actually experience nature in person. This is far broader than diving into nature, but the trend has reduced scientific diving to a shadow of what it once was and the situation seems to be getting worse.

Wonderful world leading natural history that was just everywhere when I came, if you look at the natural history of Conrad Limbaugh, Quast and the others, you could see Scripps was a rich mecca of marine natural history, really the foundation of marine ecology, and now it's gone. That's sad and even sadder is the fact that I did nothing to improve it.

But really, my career here in my mind is defined not by this stuff, but by the students. I'm really, really proud of a whole flock of them.

PB: Did you teach other courses?

PD: I taught seminars and some other courses. I fairly quickly discovered that they didn't know any philosophy. What is a hypothetical deductive approach to doing ecology, what is the difference between induction, which is what they were doing, and deduction, which is when science becomes so powerful. So I started doing a seminar on the philosophy of science fairly early, maybe 1973.

George Somero and I were friends, and he'd actually had a philosophy course or two, so unlike me he actually knew something while I just waved my arms, so together we taught a philosophy of science course until he left Scripps: George teaching and me being a windmill. Somewhere around 1980, I got tired of the philosophers, where it's all about their theories and ideas and silly semantic things, and I realized that we really needed a scientific ethics course more than philosophy. So then I started teaching that ethics course in the early 1980s. Michael M. Mullin joined me on that because he was into that as well. I think it was a good course, and I still think it's a valuable course.

So I was teaching other courses including several years when Mike Mullin and I taught a very large marine ecology course on the upper campus, but outside of my ecology course and a couple other lecture courses, most of my SIO courses weren't formal standup lectures so much as discussion types of things. For the philosophy and the early part of the ethics, those were pretty much lectures where I would talk about stuff, we'd give them things to read, and I would give a lecture on it as best I could, which wasn't very good. I am sad that my classroom stuff was not very good. Still isn't. I'm not comfortable talking in front of people.

PB: It's not your strong suit?

PD: No, to say the least. Now they give you all the student reviews, and the students are very generous, but I know I don't do a very good job.

My students, starting in the very beginning, were wonderful. And again, if you think about the Chile, Amchitka, in 1974 the Antarctic again, the kelp program here, teaching courses that were making me go crazy because I was so scared and incompetent, two little babies to help raise, there was also Sturm und Drang with the students. When I came I wanted students.. Dave Checkley and Glenn Van Blaricom actually took courses that I TA'd at UW, and my wife TA'd, at University of Washington, and they were both here as students. Larry Deysher was one of Paine's field assistants at University of Washington, and I knew him well and liked him very much and I got him down here to work with me.

PB: To be a PhD student down here?

PD: Yes. Noel Davis was the prize undergraduate of James Morin, whom I knew at UCLA, who had a really good diving program and was and probably still is a spectacular naturalist. Noel was a very strong diver and excellent ecologist. So I brought in those students on my own. Checkley was Fager's and Mullin's student all along, not mine, but I knew him. Then Fager got sick. I'm not sure when he went down, but it was like 1972 or 1973. I had to pick up a bunch of his students. Those earlier students -- I think that their theses were really, really good. Larry Deysher worked on Sargassum, which was an introduced species, it was taking over Mission Bay and all over the place. He actually learned enough Japanese to go to Japan to study it in its natural habitat. We got him some money, some

university or a Pacific Rim funds or something, so he went to Japan for two or three months and looked at Sargassum around Japan. Deysher was onto a really good thesis, and then he lost interest and disappeared. He eventually finished with Francis Haxo, but the Sargassum stuff was lost for the most part, which is sort of too bad. He's a great guy and a good friend, does interesting kelp work, but he just didn't finish with me.

PB: Then Larry Deysher went on to work for environmental consulting.

PD: Yes, he became a consultant, a good consultant. Then I had Glenn Van Blaricom and Noel Davis, who were interested in the sand community.

Noel Davis did a great thesis, and it's not published. That's one of my great regrets is that I did not realize she wasn't going to publish it. She was very professional and a really good graduate student, and I assumed she would continue to be aggressive and get it out. I was tied up with all of these other crises and I didn't keep up with it. It didn't get published. She was working with Astropecten and the foraging ecology of Astropecten. She has all sorts of still terrific data on the prey abundance -- she was looking at catchability and abundance -- of all the things that Astropecten ate out there, and they ate everything. So she really had that bottom in front of Scripps wired.

So did Glenn, because he was working on bat ray foraging and all of the holes that they dug, and every piece of the bottom in his thesis era was bitten three times a year, and about a third of the bottom at any one time had these big holes. The big holes were different ecosystems, and that's what his thesis was about, was the infauna critters that were responding to these disturbances. Then there was a slump into the Scripps submarine canyon, a whole bunch of sand moved and sterilized many acres of that sand. That was a wonderful natural experiment, because now he had succession, and it went through all the polychaetes and things.

So those two theses were early on and they were really good, very strong, and I'm proud of them and my lab had a great soft bottom program. Then Fager got sick, and he went into a coma and just basically he was there one day and gone forever. It was two years or so before he died. At first I was sure he was coming back. I was next door to him, so I took on his students and had to sign off and be supervisor for Arthur O. Flechsig and Thea Schultze in addition to all the other stuff.

PB: Because they were working for Fager as staff?

PD: They were Fager's people, and I was looking out for Fager's interests. Anybody would have, but since I was next door and the diver, I was the natural person.

PB: They knew what to work on?

PD: They kept going for some time, but they didn't have any sense of what to do. I was just too busy to really make good use of them. I managed to

keep the O&R funding for Bill Fager going until about the time he died. Then Flechsig had to go away, which was sad because he had trouble finding another job, but I just couldn't do it, I tried really hard.

PB: You already had somebody working for you on the kelp work?

PD: I had my own people. It was Rosenthal and then Vicki Curry. I had my own world, and I could not handle an entirely different program. Thea was Thea: spectacular. She has done her own oral history for Scripps Institution of Oceanography Archives, so you can look at that. She was wonderful. She was a unique force at SIO and was sort of a mother for all of us.

Fager had a whole flock of students. I took most of them on - at least for a while. At one point I had 12 students.

PB: And raising children at home!

PD: And my little kids at home. And the Amchitka and Chile and Antarctic programs needing a lot of attention.

PB: To get a larger context -- you got your own grants, and then when did you start going back to the Antarctic again?

PD: 1974, right in the middle of that.

PB: Unreal.

PD: It was unreal, and it didn't help my career. I had to do something for all of those students. Some of Fager's students went to other people and finished because they were really doing things that were outside my domain. That was fine, and I was glad to shift them around. I ended up out of that with George S. Lewbel, who was there when I came. George was there, but I had to get him qualified. He ended up working on caprellids, he didn't have a lot of focus at first, and then he focused on caprellids, and I was too busy to hold his hand and was pretty useless to him. I didn't do a very good job with George, but he did a very good job on his own.

PB: Yes, but let me stop a minute. Eric Vetter told me when he was your student that you weren't going to spoon-feed someone an idea to do their research because if it was a good idea, you were going to do it yourself. You wanted students to develop their own ideas and pursue things.

PD: That's right.

PB: So I think what I'm hearing here with Lewbel and the caprellids, you wanted him to --

PD: To find his own thesis.

PB: Yes, because if he's going to be a good scientist, spoon-feeding him isn't going to help.

PD: That was absolutely right. I came from University of Washington, where we had these wonderful interactions, mutually beneficial teaching interactions among the students. The students here at Scripps, I think from Fager's perspective, were extraordinarily critical. They could criticize anything, and they were embarrassed by their theses. They couldn't tell you what they were doing without apologizing for it and telling you all the things that they couldn't control. They were very, very insecure and hypercritical. Those seminars -- they would just destroy whatever the topic was. There was one on Vero C. Wynne-Edwards' book on group selection, everybody agreed it was a controversial book, but they just destroyed it - almost in a personal way. They would destroy themselves the same way. They would come and tell me what they were doing, and there wasn't enthusiasm, there was embarrassment from their own theses. It was so sad. It was shocking to me, because we had students interacting constructively at UW, and here they weren't interacting. They were friends, they dove with each other, but they weren't helping or talking to each other, being open. I had a bunch of parties at my house where I'd invite students over and we'd drink, and I would not let them be critical. I spent several years trying to get out of that hypercritical mold, the people in my group and around me, anyway. It was hard. It's easy to criticize. It's really hard to create, then to create and to be proud of it and be willing to see the problems but defend them, not be embarrassed by them -- so yes, George and all the students were part of that hypercritical culture and I did not help them enough. At that I was well into my ideas of students asking their own questions, but I wish I had spent more time with George.

The ones that I brought in, Larry, Glenn and Noel, had not been exposed to the hyper critical atmosphere and were able to define their own theses very well.

PB: And they developed their own ideas?

PD: Absolutely they did - as did George Lewbel.

PB: I certainly saw that firsthand with Eric Vetter, it fits in right now. I went diving with him when he was trying to find topics, and we did all this diving right out here off the Scripps pier in the sand flats, but he couldn't find something really interesting. So then he shifted over to Scripps Canyon, and he started looking around there. I was going on those dives with him, and he talked about it. He finally realized that the debris in the canyon was the interesting story. Then of course, then he got that big finding. I know exactly what your method was.

PD: That's my method.

PB: Then Eric was totally pumped up, totally into it. It was something he did because it was his own.

PD: These days I would be looked at as a lazy, unhelpful, bad professor. Yes, those now all those students are very independent scientists.

PB: Eric told me how hard it was because you gave him comments, but he had to go out and find his own thing, it was stressful, but when he found it, it was wonderful, that was definitely the way to do it.

PD: Yes, and Paine had not put his name on my papers, and I never put my name on their papers.

PB: Oh, really? So this was the same thing you had Paine?

PD: Paine treated me like an adult, like a colleague. They all did, of course.

PB: So you brought that here to Scripps.

PD: I didn't put my name on their papers. I still don't. Sometimes I do, sometimes it is my paper and it's a side project for them, so I would put my name on it, often as a lower author. But yes, you look at those thesis publications and you won't find me often. I realize I should have given, Noel, for example, two years to publish it at which point I'd become a junior author and publish it. I should have done that, but I was too busy or in over my head, because I really liked her thesis. But anyway, yes, when George Lewbel's thesis came out -- he ended up describing his caprellid himself. He did a really good job absolutely on his own. In hindsight I wish I had worked with him better, but he came out of here a really good self made scientist.

Tim Gerrodette had come to work with Fager, and I still remember his application. It was a really neat application from Thailand. He was in the Peace Corps at Thailand, and he had been an undergraduate at Carleton. It was a very thoughtful, careful letter of all of these things he wanted to do, which was Fager-esque, and I think he pretty much came to work with Fager.

Rick Gunnill, the same way, he wanted to do Fager type of sampling projects, so they came to work with Fager.

And then Fager was gone. He was gone as they arrived basically, and so I got them. The upside of my technique of being a lazy advisor, if you want to look at it that way, is that in the end I am learning more from these guys than they do from me. It's wonderful, because they go out and do all this wonderful background work as they are defining their theses by themselves, rather than me defining it for them. So I'm learning, I'm a sounding board. I think probably I was helpful to some of them, but I didn't assign them, and they did really good theses.

PB: It's so different than what we see in molecular marine biology where they are just kind of spoon-fed.

PB: They are in a lab and they do what the advisor does. I think that is destructive to student creativity - that is, the professor's work obviously is highly creative, but it is not the students learning to be creative on their own. I can't train an individual that can go out on their own unless they've experienced it, and have experienced the hardships and been supported and stroked. They all make mistakes, we all do, and I tell them, I'm kind of mean. I make people cry, but they come out of it independent. That's what you want out of your kids too. It's been a philosophy that I developed when I came down and saw all of this hypercritical stuff. I don't know if Paine had done it on his own or on purpose, but we were independent. So I got these guys, and they did really good theses. Tim Gerrodette worked on that cup coral which was brooding, it was a really great thesis, and he got it right out in the Ecological Monographs.

PB: The orange cup coral in this area, or somewhere else?

PD: Yes, right out here, the orange one. There are two of them that he worked on. He was doing some deep diving, he's a wonderful diver. He worked on my kelp program and really helped a lot with my efforts to analyze my data.

Rick Gunnill's thesis was really hard to read, but it was a spectacular thesis of island biogeography questions from little tiny harpacticoid copepods and microinvertebrates that settle on clumps of algae, and you can vary the size of the *Pelvetia* clumps and things and make the islands bigger and smaller, and look at what was going on. Just a huge amount of work. It was a wonderful thesis that that never really got published.

Rick worked in industry and passed away recently of a massive heart attack and his wife left us a stunning legacy. It seems that he continued monitoring in minute detail all his study sites and his wife gave us boxes of these extremely detailed maps of almost 40 years of changes of the intertidal algae. Ed Parnell is working on it with several undergraduates, but it just boggles my mind to learn that Rick has been working so hard alone and with no support to collect so much priceless information.

Anyway, those guys were just amazing. About that time, maybe 1972 or 1973, it was in the fall and we were looking at student applications for next year, and this obnoxious redheaded guy came into my office.

PB: Uninvited?

PD: Uninvited. I remember it well because he had this spiffy expensive leather jacket on. He was very cocky, and he started dumping on my thesis papers, which I thought were bombproof.

PB: He had read them and came in to see you?

PD: Oh, yes, to tell me all the problems. I had just got out another paper that I was quite proud of on deep-sea diversity. Hessler had got involved with it, on deep-sea diversity and disturbance. It was something I had largely written as a student and then came down to Scripps, and got Hessler to go along with it and he helped by adding a lot of his great knowledge, and I thought that was a pretty cool. This guy was John Oliver, and he was trying to get into Scripps by insulting me. So of course I got pissed off and got my back up. Then I had the sense to listen, and all his points were well-taken. The more he talked, the more I realized this guy really, really knows soft bottom ecology. At that point I was interested in going into soft bottom ecology.

PB: Why were you interested?

PD: It's a different habitat that is difficult to intuitively understand but is the largest habitat on earth and very important.

PB: But you weren't seeing it off Point Loma?

PD: No, I was diving with Glenn --

PB: Oh, here, off of Scripps.

PD: Yes, and I was interested in mud since my first summer at Friday Harbor where I did my little summer project on the foraging of the nemertean *Paranemertes* that ate polychaets and everything else in the mud. So I have always loved mud! Eventually Lisa Levin came and really got into mud, but I like diving over mud. Mud is a wonderful habitat.

PB: So you wanted to work on more diversity in bottom environments.

PD: So I was interested in mud, and what I really was interested in is recruitment processes in mud habitats. I think it was in the proposal I was writing in 1972 to go back to McMurdo in 1974, I was doing that on my own. So here's this guy that not only knows more than anybody, but he'd done a huge experiment at Moss Landing where he got a barge and dumped dredge spoils on the bottom, so he had a really big, really elegant experiment. I figured this guy's going to teach me something. Oliver is a force in his own -- still is. So he showed up and he wanted in. I decided to take him. The rest of the people looked at his file, and I think it was maybe a C average - maybe worse, and he never did let us see his GREs. Somehow he managed to hide them. But he had a brilliant record at Moss Landing.

PB: So he had a Master's degree from Moss Landing and he wanted to get into the PhD program?

PD: He wanted to get into the PhD program, but his track record as an undergraduate was just god-awful. I think he flunked out, but then he got A's at Moss Landing after he got oriented. People saw the old grades and judged him when he was a different person. "Oh, Paul, he got a D in

calculus." Here I'm thinking well, so would I, if I could ever be so lucky and probably have to cheat for it, I'd be happy for it. I would be very proud of an honest D. So I'm trying to defend Oliver, and they weren't going to have him. Finally I got mad because I supported a lot of students at that point. I had a lot of money from Sea Grant and NSF, and I was supporting a bunch of their students. I said well, I'm just going to have to get this support back and hire technicians to do all of this stuff, because I'm not getting anything from your students. With that blackmail -- that's the only time I've ever been an SOB and blackmailed my colleagues -- they let me have Oliver.

PB: That's interesting, you felt that strongly about him.

PD: Yes, I did. It was the only time I've ever sucked it up and really played hardball with my colleagues.

PB: Well, you just had this strong gut feeling you need to go in this direction, and this was the guy.

PD: Well, this guy knows everything, and he's really a nice guy in his own way, you just have to get used to him. So I think I eventually did get, or he told me, his GREs, and they ranged from the 20s to the 40s. Oliver has been a presence in my life ever since. He did his thesis in the Antarctic through the 1970s, and he was quite a big player in the Antarctic - still is really.

PB: So you worked with soft bottom communities in the Antarctic with him. Did you work with soft bottom communities with him anywhere else in your research?

PD: No.

PB: And then of course that defined the McMurdo Sound soft bottom community, that was a very seminal paper.

PD: That's right. It wouldn't have been done without John.

PB: Before that you just had the sponges and anemone community, which you thoroughly defined, and then here you went over to the soft bottom community.

PD: I was interested in the deep sea. I was arguing with Howard Sanders about the deep-sea diversity, and I had just written that paper. So I was really into the deep-sea issues. John said the ice is only ten feet thick at New Harbor, why don't we go dive over there because there was a Dry Valley drilling project at New Harbor at the time. As soon as I heard that, off we went. I talked about it in my Antarctic oral history, but it was one of the big moments in my life. It was like stepping out of Alvin at 4,000 feet.

PB: Right, because within scuba diving depths you had a similar environment to the deeper ocean because of the thick ice cover.

PD: Oliver suggested it. He talked to the people in the Dry Valley drilling project. We were both rooming with the drillers. He knew that we could get in the water there. I always thought it was impossible to dive there, that the ice would be way too thick and things. Meanwhile, during all of this -- this is just my first four years at Scripps.

PB: I know, that's what I'm trying to get a handle on... how you are juggling all of this.

PD: One of my volunteers as I started diving in the kelps was Mia Tegner, who was David Epel's urchin physiology student; she was doing fertilization work on urchin eggs.

PB: So he was her advisor?

PD: Yes, but she was diving with me, and a good diver and a nice person, I liked her. Then she took my field course, the first one I offered in 1972 and 1973. Meanwhile I had seen that the urchin fishery was just starting to gear up. I knew the urchins were players in the kelp forest and I felt we should get some money from Sea Grant and look at what was going on here.

PB: With the red sea urchin?

PD: Yes, so we got money from Sea Grant to study it. It was my grant, Mia wasn't involved at all at that point - I don't think she had finished her thesis. It had a postdoc position, because I realized I was overcommitted and I needed somebody to step in and to be independent at help me. Not a student, but a postdoc was what I wanted. Mia applied, and I searched all around before she applied, so I knew what candidates were out there.

PB: Mia applied for a postdoc position?

PD: With me from Epel's lab to be my urchin diver. She was taking a big jump in faith and confidence in working with me, to change her whole career and become a field biologist from a lab biologist. As she was finishing her PhD, she applied for this postdoc. I was pretty uncomfortable because she was my friend, and she wasn't an ecologist at all, she was a physiologist. I interviewed quite a few people, and then I decided to hire her because she actually knew more than they did and was going to be more useful. I knew she was a good diver, and most of all I liked her and knew I could work with her. I hired her. That's how Mia got involved. So a lot of the 1970s my research was urchin stuff which was really joint with Mia and me because she was learning ecology, population biology, she didn't know, that wasn't her field but she had a very steep learning curve and got everything immediately.

PB: And were you the PI on the grants with Mia?

PD: I was, yes. I was doing the field experiments, I was setting them up. So she was doing another PhD really, but she was so competent, and she took over a lot of the other things that were driving me crazy.

That was going on in the early 1970s. There two other different things that happened in the early or mid 1970s. The Natural Reserve System in the University of California was starting. It started around 1969. It came out of three really wonderful people: Kenneth S. Norris, Mildred E. Mathias, and Wilbur "Bill" Mayhew, who started the natural reserves. Every campus had to have a faculty person representative to this fledgling system that wasn't called the Natural Reserve System at the time, it was the Natural Land and Water Reserves System run out of the systemwide development office because it involved donated land. Michael E. Soule was our UCSD representative, because he was the only ecologist on campus.

Shortly after I came in 1972, Soule went on sabbatical to Australia, and I was appointed while I was on a cruise in Chile to take over UCSD's NRS position. It was one of those letters -- 'if I don't hear from you in five days, I'm assuming you have accepted,' and I got back three months later.

So now I had the Natural Reserve System. And honestly, as I look back on my life, besides my kids and my students, the things that give me by far the most satisfaction is the NRS system and the Marine Mammal Commission. I spent much of my career as our campus person because UCSD doesn't do ecology, and except for Ted Case and Dave Woodward, the ecologists that were here did not want to invest any time in something that wasn't going to make them famous or pay them money.

I carried it for a long time. Ted Case took it some, but basically it was something I shouldered for over 20 years when Dave Woodruff took it on but eventually it came back to me. Dave Holway has had it the last few years.

PB: Was land being acquired, was the reserve being increased?

PD: Yes, and I was deeply involved in it. I think we met four times a year in Oakland, at Berkeley at the time. We would go up a lot. In the office up there, besides these three heroes of Mildred and Ken and Bill Mayhew, there was a staff guy named Norden H. "Dan" Cheatham, who was a plant ecologist, and Roger Samuelson was the Executive Director, which was a part-time job for him. Maggie Drake was Roger's secretary and in many ways actually ran almost all of the program in the early years. Her legacy is huge and needs to be told.

We would evaluate land, and the campus representatives advisory committee to the President, would argue about what types of habitats to encompass in the system. We were defining a wish list of habitats. You can't have everything, and we were prioritizing them. Prioritizing habitats for a state as complex as California is really a hell of challenge. I am not

sure it could be done now although the nature conservancy spends huge amounts of money every year struggling with exactly the same thing we just did based on our collective understanding of natural history and priorities. Yet we did it, and it was mostly those three unbelievable naturalists and a few other campus representatives bringing their own wisdom. I think I helped a little now and then as I was taking my kids all over and visiting the habitats as we talked about them. As I think about those few years it is amazing what we did. We tried to tabulate habitats and rank them based not on hard science as ecologists would insist on these days but really based on the sense of place of a small number of very good old-fashioned biologists. There is a lesson here that needs to be understood to the effect that the human mind can incorporate a sense of place and sensitivity to nature that allows it to make much better decisions than TNC science and computer models with all the utter BS ecologists indulge themselves in. The modern focus away from nature into natureless modeling is very hurtful to understanding our environment, and I think that this is a very large lesson to be pulled from the success of the UC NRS.

PB: that is a very important point.

PD: I think so. It was all on terrestrial plants, nothing marine. Meanwhile, we did have two marine ones that were de facto. Carl Hubbs had taken Mission Bay in on his own when that land became available and it had been donated to UCSD, and that was factored in when they first started the NRS.

PB: The Kendall-Frost Mission Bay Marsh Reserve?

PD: That's right, the Kendall and Frost families donated it to SIO or UCSD via Carl, who put it up into this new reserve system. In the 1920s, the Scripps pier and the land around it became a reserve for research. Dike Rock was also incorporated into it. We had the Dawson Los Monos Canyon Reserve, it was just coming in when I came and I remember doing a site visit with Mildred who was very impressed with its potential. Then there was Camp Elliott land for the Elliott Chaparral Reserve. There was a brief reserve out in Escondido that was a mistake and we got rid of it. Those were the UCSD ones, and I was the only one looking after them.

The Elliott Chaparral Reserve was a land grab by the University from the Government Service Agency, that was giving surplus World War II military property to academic institutions, but they had to use them and document the use for ten years or so before the GSA would sign it over to the campus. We were given the responsibility of using the Elliott Chaparral Reserve, which was out at the end of that ridge. It's a really nice small habitat. And I was quite enthusiastic about it because I envisioned someday that there would be real biologists coming to UCSD that would use the military flight approach path to Miramar which was kept from development as one great huge natural area. It still is, but of course there is nobody at UCSD that cares about that type of nature enough to use it that way. My dream was to keep that Elliot alive, and keep it in the

reserve system for people to track coyotes or raptors or bobcats; to do real research, field research over a big area via the use agreements that I tried to negotiate. That's still an option, but every two years the commanding officer would turn over at Miramar, and it was in the Navy then. I would go out, and having had Antarctic experience with the military I could talk military talk from McMurdo. I would go out and be a good old boy and explain to them how we were good neighbors and we wanted to have this use agreement. We continued to be good neighbors and there was no threat or anything, all he had to do was shake my hand and we went away. Unfortunately the Marines took it over and are very officious with several people who earn a living controlling these things by some vague set of rules.

That when the University got the property, the business office tried to sell it. I was enough of an SOB to poison that and piss them off, because it was really in the Natural Reserve System, and it didn't belong to UCSD, it belonged to the NRS system wide (that is, the UC Regents) and to me it had this bigger value as an entre to the large military area. So even though nobody was using it at the time, we managed to protect it, and I think it's a useful reserve and maybe one day the campus will get a field biologist to use it. Probably not, but the reserve is a good habitat in any case.

The knoll is another sly acquisition.

PB: The Scripps Coastal Reserve on the bluff just north of Scripps?

PD: On the bluff, yes. In the 1970s, they were going to sell that for \$20 million, which was a huge amount of money at that point. Yet it was a nice natural area. The Scripps staff that lived along the adjacent Scripps Estates Associates there, including the Shors and Pat Masters and Doug Inman, and Carl Hubbs wanted to maintain the adjacent open space. Pat Ledden on the upper campus was extremely helpful as well. The Scripps Estates Associates people didn't want a bunch of condominiums over there. They were anxious to try to protect the knoll by putting their own canyon into a use agreement. They owned the canyon adjacent to the knoll that is a spectacular natural area, and the knoll really doesn't have anything, it was just an old farm. There were plow marks, and it was a firing range during World War II. But the use agreement with the canyon made it something we could try for.

PB: I think there's some bunker remnants or something there.

PD: There's a ditch and bulldozed hill used for the old firing rang, so it was going to be kind of hard to protect that. The trick, which I got from my archaeology days, was to use the American Antiquities Act and go find some archaeology, which I quickly did - it is easy as the whole that has not been ruined is a big midden.

Pat Masters was interested in archaeology, and she went and looked at it with me. We got some archaeologists to verify that this was an important

archaeology site - it really is. That took the value of the land from \$20 million to about two or three, because then it was going to have to have remedial salvage archaeology.

Then they were starting to be willing to consider moving it into the reserve if the use agreement would go, but I had to make the case for that knoll to be a reserve value, which I was aware of how hard that is because I had been evaluating other lands over that time. I ran around getting local botanists to verify that it was rich, but it was not until we got Frank Vasek to help that we realized just how rich it really is.

Frank Vasek, a plant ecologist at University of California Riverside, came down and found God knows how many species, all sorts of species. This is the advantage of having really good academics to employ such as Pat Masters and Frank, an exceptional plant taxonomist. With their help, we made a really good case.

PB: Yes, I've been up there, there are a lot of different plants up there on the bluff. You've got ospreys nesting on the cliffs there too.

PD: Now there are even peregrines, they didn't have them then, but there's archaeology all over this place, so that went into it. And the canyon is spectacular.

Now there was another hero in my early years in the upper campus planning office, a lady named Pat Cullom, who married and became Pat Aguilera, and then she retired. Pat Cullom was in cahoots with me for saving UCSD land. We made some campus ecological reserves: the Rattlesnake Canyon [east of Birch Aquarium at Scripps], and the canyon over by Genesee [north end of UCSD campus]. Pat and I protected those de facto natural lands with various planning things that she was involved with, and I helped her find the areas to protect. Pat was working with me quite closely to protect these areas around the campus.

Meanwhile, Soule had gone. Ted Case had shown up, but he hadn't gotten into any of this at the time but I think he was proactive to protect the forest of gum trees to the south of Genesis Drive. This was going on while I had all of the Antarctic work and everything else going on as well, some of which was challenging. I still remember when we finally got the Knoll reserve through, we had to go up and defend it to the UC Board of Regents. Pat and I went up to this meeting all dressed up in our suits. It was the biggest thing we had ever done. She was just sweating. I was pretty nervous. We (almost entirely Pat) made a pitch for the great value of this old farm, and that's how we acquired the knoll. It was really Pat Cullom's energy and wisdom, and the Shors, Betty and George, were really useful, very critical for that. Carl Hubbs was involved too.

So that's my history with the NRS. Over the years there are similar stories for me with the Big Creek and Granite Mountains and North Coast, some of the big reserves that we have now that are jewels of the system. I think the NRS is the world's best system of that sort in the world, and

I'm very very proud of it, and I was involved in sort of engineering the acquisitions and I consider these to be arguably the most important things I did with my career except for my students.

PB: You should feel proud of working, helping --

PD: Really, it was those three people that were visionaries. I came in after their vision had already taken off, but thereafter, we worked together quite closely.

PB: You were involved during a seminal period where the land would be hard to get now.

PD: Imagine getting Big Creek, that's in Big Sur, it's an entire drainage, and it was very hard to get, because we had to get the landowners to give it to us. To do that, we had to have it appraised at some significant amount. We had to hire appraisers, and it took two years of our budget mostly to pay for the appraisers to help the landowners decide to give it to us.

Internally, in the NRS system, it was my idea to make the Campus Advisory Committee more democratic by bringing in reserve managers, which was some wild hair I had. So the managers were now in that family as we were debating whether we could spend all our money on taking the risk that they would give us this land.

The managers were just outraged. They wanted pickups, and they needed this and that - we were over committed and irresponsible. They were really negative as we struggled for that and we knew there was a high risk. So getting that Big Creek thing was basically sort of a one-vote decision, it was very close. Now of course the manager representatives are some of the most useful members of the committee, but at the time they caused internal problems for acquisitions.

Then at the end, one of the landowners sort of double-crossed us, and it was all lost. Ken Norris I think, went to the Save the Redwoods League, and they wrote a check, that same afternoon as I recall, for \$1 million, and we bought the guy out.

PB: I can see the landowners' concern. You don't want to sell too low.

PD: Well, the appraisal we bought gave them a very generous tax right off.

PB: And that was the reason for spending so much on the appraisal?

PD: Yes, and now it's a wonderful reserve. Several of the reserves have that sort of story to go with them. Somebody should do a really good history of the NRS, because it's a history of really good leadership from those three people, and a few followers like me that were basically helping. Even though I wasn't the leader, I still put at least 25 years

into it, and I'm proud of it. And in hindsight, the careful deliberations involved with prioritizing the habitats we wanted to protect was very ground breaking conservation ecology decades before it became popular; this aspect of the NRS is totally forgotten but was a huge achievement.

Let's see, I should talk about my sea otter project. That also started in the mid-1970s. I'd always been interested in otters and I'd been driving up to the Stanford University Hopkins Marine Station where the otters were coming around the point, and I was doing experiments there.

PB: You were doing experiments related to the sea otters?

PD: At Hopkins, yes. Then I learned that even though the people there, Anson C. "Tuck" Hines in particular, were helping me and were quite happy and the people at Hopkins were happy, but some people at UC Santa Cruz felt that their turf was being stepped on, and they were much less happy.

PB: They felt that UC Santa Cruz owned sea otter research in Monterey Bay?

PD: Well, they taught at Hopkins and students did help research there. I can understand. It really was John Pearse's stomping ground. And he was ok with my working there.

PB: Did they use Hopkins as a location for their work?

PD: Yes, they used it, I think, in their teaching and things, but they weren't doing the research that I was. Tuck Hines was there, he was working as a postdoc for John Pearse, and he was very helpful. When I found out I was causing problems, I stopped; it was a long drive anyway. I had too much going on so was not doing a good job, so I pulled out.

As I pulled out, I'd already written the sea otter paper for Amchitka, so I had some sea otter bona fides. The U.S. Marine Mammal Commission was just getting started in Washington DC in the early 1970s, 1972 or 1973.

In 1974 or 1975 -- maybe 1974 -- they put me on the committee of scientific advisors. And with the NRS this is the other life long project for me that give me a tremendous amount of satisfaction. They were meeting four times a year in those years. We were fighting for the Marine Mammal Commission ideals that for me were the ecosystem component that allowed me to contribute to marine conservation, my biggest passion. So this is the other big achievement in my life, my association with the Marine Mammal Commission. They had a brilliant leader. John Russell Twiss, Jr., the Executive Director, and he were just wonderful. We all worked hard. I served on it until just two or three years ago when I finally got off. I was the longest-serving Marine Mammal Commissioner. I was the only Democrat appointed by the first [George] Bush. So I have this long history with the Marine Mammal Commission, and it was just starting up in the early 1970s. It had all sorts of interesting political headaches that should represent another book somehow.

But getting back to my otter program, I was still interested in quantifying the sea otters community role, something that has always been controversial. I got a small grant with the money for a postdoc to work at Piedras Blancas, the lighthouse by San Simeon. The Coast Guard was just pulling out when I first went up there and looked. I think they pulled out in 1972 or 1973. I was driving up there, and I had the combination to the gate. The Coast Guard pulled out, and I think they turned it over to the U.S. Fish and Wildlife Service, but that took a while. So in the mid-1970s, I was going up there and they had the windows blocked off, the doors blocked off with plywood. The doors weren't locked, so you could take the plywood down, and live in the buildings. So as I started that project, I was doing that. I took field classes up there too, and on my field trips we always stopped there and went into those buildings and took the windows apart and the doors off and slept on floors.

The U.S. Fish and Wildlife Service was taking it over at that time, but they weren't living there. My idea was that I would get Brian D. Keller, who was the postdoc I hired, to live there with David Andre VenTresca who I recruited from Moss Landing, Thankfully Dave did a lot of the work, but together they accomplished a hell of a lot with me going up there almost monthly. So I had an otter component that was built into the 1984 paper.

PB: What were you studying?

PD: Otter foraging biology and the kelp ecology. We got good data. Brian and Dave did dive - a lot and well, and they got good data. Eventually the U.S. Fish and Wildlife Service ran the Piedras Blancas lighthouse, and they had really good people living there. It was really an active and healthy and vigorous lab. Then for some reason they closed it and turned it over to the Bureau of Land Management, which turns out to have been a terrible mistake because the BLM became very hostile to education and science. They hooked up with a local botanists in Cambria who was doing wonderful re-vegetation, getting rid of all the ice plant that the Coast Guard planted to keep fires away and re-establishing native plants. However she persuaded the BLM that nobody should be allowed to walk across the plants even though there is a path to the intertidal access. So it's just been really hard to get permission to take classes up there and to walk across to the intertidal, in my mind the best rocky intertidal habitat in the world. You have 50 yards or something, a path to get down to the intertidal. It's the best intertidal habitat in California for teaching, and it's really hard to get in there now. Nobody can go in there because the BLM is protecting her plants from respectful people who would never hurt them anyway.

PB: So with the marine Mammal Commission, you were involved in the review of sea lion impact on salmon ladders? You were involved in all the marine mammal controversies over your time period of service?

PD: Major controversial things, from the bowhead whales to the monk seals to the sea lions wiping out the last of the steelhead and salmon. Major things, yes, and sea otters were always there festering away.

PB: That's a major contribution to the nation, to be involved in that work.

PD: I don't know about the nation! But it has been important to nature. They're meeting in La Jolla in mid-May, and I think they're going to meet in the new NMFS fishery building. It's a wonderful organization. I just really respect it so much, and I'm glad that it's still got good leadership and things. One of the students that I advised is the Executive Director, retiring right now, Timothy J. Ragen.

PB: Did you have to excuse yourself from reviewing the Acoustic Thermometry of Ocean Climate (ATOC) project sound effects on marine mammals?

PD: Yes, I did, thank God.

PB: Because you were here at Scripps?

PD: That's a big deal. I was getting abused by my colleagues who saw us hurting their work, and some of my colleagues were killing beaked whales. But at the same time the ATOC work was brilliantly conceived and critical to understanding several important global problems and I did not think it was doing any harm to the mammals. But clearly I was compromised and had to excuse myself. The other thing I recused myself from just because I'm a coward, and I just didn't want to get involved is the harbor seals at La Jolla at Children's Pool. That's been a festering sore for a long time. I have my own strong opinions, but I've just been able to say well, look, I'm totally compromised. I work at Scripps, I live near La Jolla, and I'm on the Marine Mammal Commission. You have to get your opinions from somebody else. The thing is, they're not endangered. They could be shot like those fishermen shoot nuisance seals all the time. It's the only place in the mainland south of the Bay Area, anyway, where people can see harbor seals pupping and nursing.

PB: I know they're not an historic presence, but still it's an incredible nature experience.

PD: It's a wonderful thing for kids, for the public.

PB: The Indians would've killed them and eaten them if they were there, historically.

PD: And before the Indians, the California grizzlies and before that the saber toothed cats. I know all this, of course, but my grandkids love them. The La Jolla people don't like all these poor people parking and not spending money. They say we don't have any other beaches. Good Lord, you can't tell me you don't have any other beaches. On the other hand the

Children's pool is the only access for the serious free diving spear fishermen who go out through the kelp there and kill coastal pelagic species like white sea bass. So they do have a legitimate concern being excluded from using the beach.

PB: Well, it's just some La Jolla people that purport to speak for La Jolla.

PD: Yes, for the most part, I mean this is why I've chickened out. Now I no longer can chicken out. But for years I chickened out.

Let me go through my students, because they really are my lifeblood and what I'm most proud of from my career. I got myself all of these students in the mid-1970s, I didn't have tenure. It was a bad time for me, but the students were just kept me going. People talk of students as being a burden for which they want academic brownie points, but in my case the students saved my career by being my brain and keeping me going and teaching me everything I know.

PB: Bad time for you in the sense that you were trying to build your career, to get tenure, you had that insecurity about that?

PD: Yeah, all sorts of insecurities. My kids were trying to grow up, and I'm trying to be a good father, and go camping with them and things. I can offer a nice story about this aspect of my life.

The first big NRS meeting I went to was in Mammoth, a big systemwide meeting. I went up there with my little infant daughter who had just been born and my just turned 2-year-old son. We got there late because I'd overestimated how far it was and had to camp. The meeting was underway when I dragged in, and here I was suddenly thrust into this room full of very famous people. I was just really insecure. My wife is nursing and had the little baby, and the two-year-old got loose from her and came into the meeting to find me. He just totally disrupted the meeting, and they had to be polite but in the end they just adjourned. I was just livid. He was barely walking and he was just looking for me and wanting to interact with me. As I headed out angrily to find him, Robert M. Norris, who is the uncle of Richard D. Norris currently at Scripps Institution of Oceanography, intercepted me. First he started saying how nice it was to have kids there. He was trying to calm me down. He gave me the spiel about how we all forget about kids. Then, I don't know whether he was still calming me down and he got really reflective and he said that his kids are now in their 20s, and he said, "And I haven't really gotten to know them very well. They really seem like nice kids." I think he was just trying to calm me and make me listen to some real values for once! I thought back to all of these really important people in the upper campus. I was meeting their kids in the nursery school and getting stories from colleagues. The parents may have been in the national academy and very important people, but their kids were messes. They were into drugs and having all sorts of problems including suicides. Famous people can be terrible parents. Thank God, I heard old Uncle Bob. I changed my life,

and I put my kids at the top of my priorities. It is a no brainer. I didn't give any more seminars and stopped going to meetings. I decided I didn't need fame. I wasn't going to take all that time flying around the country and trying to be famous like other people were doing. Screw it, what's really important here? I didn't do summer research. I would go on long trips, camping all over the west with my kids, from when they could barely walk all through high school. So that was going on in my little head while all of this other stuff was going on to. It was pretty interesting.

The students kept me going because they were just so wonderful. One of my favorites was Brock B. Bernstein. Brock had been a competitive swimmer in college, but he came from one of those Claremont colleges. He was a literature major. His application was so bizarre but so literate and so interesting that we took him. We called him an assigned risk. Robert Hessler had money, he had needs for someone picking samples. So Hessler was going to pay him to work 15 or 20 hours a week. The assigned risk was going to come and figure out what he wanted to do at Scripps, having written this wonderful letter. Old Brock was a good-looking guy, and he did his work, he was really brilliant, he was one of the smartest students I'd ever had. He did all of this great work in Hessler's lab. Hessler couldn't fault him, but Brock liked to sit on the lawn in the sun in his swimming suit reading the paper, relaxing. That wasn't really something that Scripps often saw students do. Our students are supposed to be nose to the grindstone, miserable, hard-working folks. Here's this guy out there with his feet up on the table and coffee or something reading the paper, having breakfast. It just drove Hessler crazy. It was right there by Hessler's office, and he would look out of his office and here is Brock sunning himself. I just remember that with so much pleasure. Then Brock wanted to work on the kelps, and he did one of the best theses that I'd been involved with. He got it published right away, and it forced me to look at scaling across the whole kelp forest instead of just my area. He showed me that things were really different in the different parts. I learned so much from that guy, and I learned a lot about life. I learned about King Arthur and British history. He always seemed to know everything. Brock is a genius. He's been a consultant. I don't know that he ever tried to get an academic job. He works out of Ojai. I haven't seen him in decades, but I see his name now and then. He's very good at solving problems. He can articulate things really well and immediately perceives different people's perceptions. Brock is just a wonderful part of my history.

In the 1970s came Robert K. Cowen who wanted to work on sheephead fish. Mia was involved with them because they were eating urchins. Bob was going to work with us on sheephead, but since he was going to do fish work, I think he got most of his intelligent advice from Richard Rosenblatt. Bob was one of my students shared with Dick, but I think mostly trained by Dick. But he taught me a hell of a lot of biology and has always been a really good friend.

Then I think the same year, Lisa Levin and Janice Thompson applied to be students. Lisa had finished Harvard in three years with a straight A average, had worked as a consultant and wanted to do theory when she came. I thought she was wonderful because she was so smart, but she went to work with Goodman to do theory. Goodman set her working with fruit flies, which wasn't what Lisa wanted to do. Lisa found her way into my life, which has been forever enriched. She worked on the Mission Bay mud flat. Her thesis was just terrific, it was on dispersal of worms. She did the first experiment to look at big picture dispersal. She put computer printout cards, those old cards we used to have by the thousands, and she would follow them. Then she worked with Mia Tegner making little test tube drift things that drifted better with the current. Mia was doing it looking for abalone and urchin larvae from different islands, and Lisa was doing it from Mission Bay. I helped make the drifters and was really excited by their work. Lisa's thesis got published right away, and I think it's a classic paper. It's really important.

Janice Thompson came from Cal Poly Pomona, and she worked on sponges. She did chemical defenses of sponges, and she was my student, at least on paper. I was working on sponges in the Antarctic, so she came to me. She did a lot for me but I was gone so much and don't think I contributed anything to really help her. She taught me tons; I didn't teach her all that much. She was working with John Faulkner on chemical defenses of sponges. Nick Holland was working with her, helped her with the cellular aspects of it. She showed that if you have a bunch of different species of sponges combined, their chemical defenses are much better than if you just have a monoculture. She was doing all sorts of really cool natural experiments out at Quast Rock and Point Loma and different habitats. She was a really good diver and her work was ahead of the curve and in fact nobody has really picked up on the important chemical defenses that she described.

So those were two students that came in the 1970s who finished in the early 1980s.

James Barry came in the 1970s, and was the other really important person in my life. He stayed around as a postdoc, and was here for maybe 10 years, for quite a while. First helping with the Antarctic and then as a student doing really interesting stability work in the intertidal showing how the turf and the hard bottom, the barnacle communities interface in the role of predation and that. He didn't publish much of his thesis, which is too bad. He was the brains in everything I've done in the Antarctic. Jim is extremely bright and one of the most capable people I know. Literally, he can do anything and seems to know everything from astronomy to auto mechanics!

PB: When did Jim start going to the Antarctic with you?

PD: Late 1970s, and then all through the 1980s.

PB: Was Jim your lead person on the data?

PD: Yes, and in the field, he was just a wonderful field person. Jim came on a lot of my field courses as sort of the real teacher because he knows so much. Just an amazing guy, and he's a really good friend. He's at Monterey Bay Aquarium Research Institute doing well. He was the best uncle I've ever seen for my kids, because as teenagers, of course, I was stupid and hopeless. It was always Jim that was there, that would get them straightened out, a really important player in their lives as well as my life.

Then in 1979, I was worn out and decided that maybe I wanted to be a terrestrial ecologist. I did get tenure, but I was beat. I'd worked so hard and at the time had not seemed to accomplish much. I spent the year at Flagstaff at the Museum of Northern Arizona where they were trying to set up a research group of ecologists. Actually before I went, I changed my mind and told them I was staying at Scripps, but they brought me over and set me up for a year. I took a leave from Scripps. It wasn't a sabbatical, but it was a leave, not paid. That Museum of Northern Arizona research group was set up with some really interesting people. Jim Richmond is the guy that set it up, and he did an amazing job. He brought in H. Ronald Pulliam, who was an excellent ecologist from Arizona, I considered him one of the best in the world, and Peter W. Price and Thomas G. Whitham, people that were household names in terrestrial ecology were there in 1979, 1980, 1981. We set up and ran a field program out of that area for several summers in Flagstaff, bringing in really good graduate students from around the country. I participated in that. I met another superstar in my life, a guy named Larry Stevens, who was a river runner at the time in the Grand Canyon, but the best naturalist I've ever met. Far and away the best natural historian I've ever met. He got me into the Grand Canyon, so all through the 1980s I was doing river trips with him in the summer. I've done eight river trips variously. One was my private trip and once I met Larry hiking down with my son Gage to Thunder Springs from the north rim. It is a significant hike for a 10 year old kid. But in the 1980s I spent a lot of time in the Grand Canyon, all spinning off of that one year in Flagstaff while I decided I was not cut out to be a terrestrial ecologist.

PB: In Flagstaff, if you were interested, could you have gone into a position there?

PD: Well, I changed my mind before I went, and I told them that I was not going to stay. I'm going to go back to Scripps. All my natural history strengths, my heart really is in coastal ecology. Give it to somebody else. They didn't -- at that point it was too late to find people. So they were sort of stuck with me, and they persuaded me to come anyway. Which -- why not? But then they went under, and all those nice guys were left high and dry. I sort of felt guilty with them desperate for jobs while I had kept mine!

PB: So you made a good call?

PD: Yes, but not because I worried about their finances. We are still all friends, and what a good year that was in Flagstaff -- just a great place. While I was at Flagstaff, I got a letter from someone named Amatzia Genin, an Israeli student. Amatzia is a pretty name and I thought it was a female, so I wrote back, "Dear Ms. Genin." He got that corrected right away! He wanted to come work with me, and he had some funding. He had done an excellent Masters in Israel on marine snails, and he wanted to do something like that at Scripps. He came about 1980 or 1981 to work on snails and took our oceanography course, the required Biological Oceanography course and got really interested in seamounts. So I told him, Amatzia, seamounts are way out there, you can't do a thesis on seamounts, it's not possible. Never tell Amatzia Genin that something is not possible. He hooked up with Fred Spiess and Larry Armi and some NOAA people. All these people that could work on seamounts and he did it.

PB: He networked his way at Scripps into working on seamounts?

PD: Yes, absolutely amazing effort. Fred especially. Fred Spiess was just one of the huge heroes in all of Scripps history because Fred was very anxious to help biologists. But the others really helped him a lot as well.

PB: With Deep Tow?

PD: Deep Tow was his main bread and butter, and grabs, and he had various cruises where he actually found a Taylor column, an unusual thing. It's a current that spins off the top of a seamount and goes up to the top bringing nutrients and such up to the surface. It's something that oceanographers know theoretically but you don't find them very often. Fred's work was studying the seamounts. He put all the stuff on Deep Tow for Amatzia. This is how Scripps is so wonderful. You get really good students that can exploit a really diverse set of excellent scientists. Amatzia couldn't go anywhere else in the world and write a really good thesis with the Nature papers and everything on seamount ecology. Scripps was a unique place for that type of collaborative research for good students with a strong self-starter. I am not sure Amatzia had a self-starter, he never stopped!

Lisa Levin does deep sea work now, but at the time Amatzia was doing it with Fred Spiess.

Terrie Klinger came from University of British Columbia with a masters, where she'd been working on seaweed. She was a bona fide kelp taxonomist and biologist. She was my only real kelp person that came full-blown as a kelp ecologist. She was a really good diver and a wonderfully enthusiastic person. I took her to the Antarctic as well.

At that point Janice Thompson who had graduated, finagled a sabbatical for me at Australian Institute of Marine Science. She got me a trip out there to show me how much I could do with the questions I was interested in regarding recruitment biology on coral reefs. I did a sabbatical in 1985

and 1986 in Australia. I think that paper that came out of that is arguably one of the better papers I've ever written. It's never cited. It just disappeared from the literature. I had a great time, and became very much in love with Australia. I started going back, I still do go back fairly regularly. I started working on soldier crabs on another sabbatical in the mid-1990s. We drove around Australia. All my Australian work started thanks to Janice, hauling me out there to show me all the good things.

Somewhere in the mid-1980s Jesus Pineda came from Centro de Investigacion Cientifica y de Educacion Superior de Ensenada, in Baja California, where he had been working on anemones. He had a really nice anemone Master's thesis, and he came up and was thinking about working on anemones. Then he got into barnacles. He was going to do barnacle recruitment across the upwelling points in Baja and found that nothing would work very well. Eventually he figured out that internal waves were really driving the barnacle recruitment, breaking the shore barriers and getting them through the surf and such. Really he got almost all of his training from Clint Winant. I put Clint on as a co-advisor and Clint did most of the advising for Jesus. Jesus was another one of those really bright people that was quietly working away in my hallway.

As an aside, mentioning his name, Clint really has been really important to me and to SIO in general. He is one of the few people who does really excellent stand up lectures that are more focused on student learning than being entertained. I think of him in many ways as being my most important colleague in training all my students about coastal physics and importantly he was extremely important as a stabilizing and rational friend during the whale lagoon debacle. And when Sea Grant was in trouble, it was Clint who volunteered to resurrect it into a much more healthy and stable organization with focused goals and excellent leadership. He was really an important player at SIO.

PB: ok, back to the students?

PD: Yes, thanks. Tim Ragen dropped into my lab as a transfer from another group at SIO. As I remember he took my field class and came to talk to me about transferring from Marine Biology into my crowded lab and he wanted to work on mammals. I sent him to another campus where there was more mammal work being done. That did not work out and he came back very discouraged but still anxious to work with mammals and do something to make the world a better place. He has to be the most idealistic person I know and is mathematically capable and I sent him to work with Alec Mac Call who was an SIO adjunct at that point and my role was simply a facilitator as Alec did all the mentoring. Tim worked on Alaska fur seal ecology, especially pup Tim was employed by NMFS and eventually got a job as the Executive Director of the Marine Mammal Commission where he surely has done a lot to make the world a better place.

I'm not sure when Eric Vetter came, but I think it was in the mid-1980s as well. He went to the Antarctic with me in 1988 or 1989. He came to work

with James Enright. Enright was giving him the money, and Eric was going through the stress of finding a thesis with me. Hessler was quite involved with Eric, and so was Rick Brusca, who was at the Natural History Museum here in San Diego and was on his committee and was very helpful. Eric was a wonderful friend, and he stayed for a few more years working on his postdoc with the submersible.

Now we've gotten up into the late 1980s. Mia Tegner probably was the only person keeping the kelp program going all of this time while I was having all of these issues and Flagstaff and going off to Australia and the Antarctic and things.

PB: So Mia kept the kelp work going?

PD: Mia was the main player that I don't recognize enough, she was stable. She was always there, always together, always honest and competent. She was carrying it, doing things.

PB: She was the one that dealt with the staff that worked on the kelp project?

PD: She actually took over the kelp project and supervised them, thank God, because I'm not a good supervisor.

This was in 1989, I think. We were having trouble with our NSF support, and we were really struggling because I have more enemies than friends when it comes to anonymous reviews. Of course, they are all friends when they see me at WSN, but not when they are getting in cheap shots. So we were having a lot of trouble. In the meantime, there was some hue and cry about the sewer outfall. I wasn't paying too much attention to it because it wasn't affecting the kelp bed.

PB: This is the San Diego Point Loma sewer outfall, the issue being whether it should upgrade to secondary treatment because it was at the advanced primary treatment level.

PD: That's right. This is another interesting part of this history, because it was a big deal. I wasn't involved at first. Mia was involved. We were socially involved in that we knew some of the players that worked for the City of San Diego such as Susan Hamilton, were good friends. They worked for Oliver, they worked for us. So we were involved a little bit, we knew them.

The City capitulated and agreed to go to secondary treatment and spending some \$8 billion. A Republican City Councilman named Bruce Henderson intervened and sued the city not to go to secondary treatment, and ended up suing the city and EPA at the same time. Henderson was friends with Roger Revelle. Mia got involved because Roger went to Edward Goldberg who came to Mia. Mia then got involved and agreed to look at this for Bruce Henderson. Mia came in and tried to entrain me. My position was that,

good Lord, why do I want to get involved in sewer outfall stuff when I am a pure ecologist? So I refused.

PB: They wanted an expert opinion, right?

PD: They wanted a kelp expert.

PB: Was it obvious when you were approached that they were looking for an opinion in a certain direction?

PD: Oh, yes, because I knew that Henderson had sued the city not to go to secondary.

PB: So they felt there was enough scientific basis to support that position?

PD: They felt that there wasn't harm, they wanted a scientist to say there wasn't harm. You know, I'm very green and concerned about conservation.

PB: So Roger Revelle thought that wasn't harm because the ocean dilutes?

PD: And Goldberg found the same thing, and so did Mia.

PB: So then they want the kelp guy?

PD: Well, then they wanted me. Roger Revelle and Ed Goldberg came to my office, came into my sty, where the black dog was at that time. They were very decent. They weren't twisting my arm. They just wanted to see what the problem was, Roger said, "Here, Paul, here are the data. Look at the data. Just look at the data and tell us because we don't want to get embarrassed either. We don't want to be ugly or make a mistake either. Is there harm?" Then Revelle pointed out that I had been doing research for society, and my knowledge, I owed something back. They did not twist my arm at all, they were good friends and simply wanted my thoughts for themselves.

I realized he was right. The taxpayers have been paying me, they should get what I know. I shouldn't just hide in my ivory tower, which I very much was. So then I started looking at just the kelp information. Mia and I already knew there wasn't an effect, and we talked about that. Then I looked at the soft bottoms offshore, and good Lord, there wasn't an effect there either. There are four tiny species that are marginally significantly changed. One of them was an ophiuroid which was having its fingers bitten off by the fish and crabs attracted to the pipe, not the sewage. Then of course right of the middle of it, there is an area with some bad stuff, some pollution indicator species. That was true was then, they've extended the outfall and now there's no hint of a problem. But the mouth of the pipe is an area where they are allowed some impact, a zone of initial dilution is what it's called.

So I started looking, and these guys were right. There really was no ecological effect. Society shouldn't have to pay \$8 billion to build a secondary treatment plant that won't do anything.

So we got involved in that. It really changed my life because then I met lawyers. We got deposed, and we were basically in the end fighting the government, because the City did not want to build it. The City was quietly on our side, but it was us against the EPA really.

Meanwhile, my friends on the upper campus, David Woodruff and Ted Case, were being questioned by people in suits trying to find some sort of dirt about Mia and me. You know, are they having an affair, do they drink a lot. Nobody could figure out who they were. I still don't know, but I'm pretty sure that they were probably lawyers working for the companies that would have built the plant. There's big money in building these huge secondary treatment plants. So now I'm starting to get defensive and kind of angry. We all testified and worked really hard at it. You got all that in the Scripps Institution of Oceanography Archives, you've got a lot of the data, with Mia or me.

Mia did the kelp which was a slam-dunk. We should have won that. I did the soft bottom which did have four species which were marginally significantly affected. Interestingly, Mia lost. I thought we really had the kelp part of it won. The judge decided that there wasn't sufficient evidence for that. At that point, he ruled on the kelp first. I was just sort of appalled. I wrote him a letter, which I later learned you should never ever do with a sitting judge, but I just wrote him this letter about you can't prove a negative. You made a mistake.

PB: Paul, Paul, Paul.

PD: yeah, it was really stupid but genuinely done innocently - I figured the poor guy did not understand how science works! I outlined how science can be applied to these sorts of decisions, and the science works negatively. You negate things. You don't prove anything in science, and you can't therefore prove a negative impact.

In his statement he'd been talking about how we hadn't proved that the kelp forest was not affected. So it was right out of my scientific philosophy course, so I gave him a little philosophy lesson, which I thought the poor judge needed. That was just a terrible thing, everybody slapped my hand, and I felt terrible. I then still had to do my own testifying for the soft bottom. Then I brought in a meter square to show him how big it was. I set it up in front of the court. Then I showed him the animals impacted to the tune of a fraction of this little almost invisible amphipod that was changed in a meter squared, and this small brittle star. Their whole thing was that if that animal was affected, it affected the whole food web, and there's only one thing that might eat that, which is a little fish. I gave him specimen of the little fish as well as the other tiny critters. I asked him to imagine the little fish being affected by the change of a fraction of one of these tiny prey

species in this big area within the meter square sitting in the dark on the floor. The judge looked at the meter squared, and he looked at the little thing in my bottle, and in the end I won the soft bottom phase.

PB: By bringing in the visuals...

PD: I think it helped, but I felt abused by the lawyers. I was just stunned at how aggressive and how they work with every intent to deceive. Now I understand how the system works. I thought that they were trying to find the truth, they weren't, of course. Now I realize that this is the way the system works and now I realize that the Judge was extremely intelligent and engaged and I still feel really bad about embarrassing him.

When the final decision came out, there were whole bits that were in that letter I'd written him about proving negatives and things.

PB: He understood the logic of what you were saying?

PD: He understood what I was saying. In hindsight, as awful and illegal as the letter was, it did serve my purpose and may have been important!

During all this, the environmentalists were all over us for being whores for the City of San Diego. Not only the NGOs, but also many academic colleagues, our good friends from WSN, were all over us for selling out.

During all this two things happened. We never got another NSF grant. It's because -- and we got this in two reviews several times -- if they want to whore for the City, let the City support them. I never got another NSF grant. I didn't tell the City anything about it, but the City realized they needed a kelp program and had made the rest of my career possible.

PB: Are you saying you read reviewers' comments that said that?

PD: Yes, the reviewers said that, twice; They were that overt. You know, these were my buddies, always so glad to see me.

PB: You had to be politically correct in your grants, it can play out that way?

PD: The peer review system is flawed but it is what we have. We never got another NSF grant.

About the debate on the outfall pipe, there was another round five years later. I'm not sure when Edward Frieman stepped in, it may have been the second round. Barbara Boxer, the California senator, called Ed Frieman, the Scripps Institution of Oceanography director, at home for two hours twice to tell him to fire us, to shut us up or fire us. To his huge credit he just protected us, without us even knowing he was protecting us. We never knew that, it was years later that Ed told us about it. This is

another thing that makes Scripps so great. You have people that have your back. Ed was protecting us and we did not even know, and then that thing eventually did go through.

Meanwhile, that was in about 1990. The kelp issue had to do with the fish all disappearing too, and I realized they were right. The Greens were blaming the pollution, but I knew what was happening. The fish were being killed because people were out there killing them all the time. There were traps, there were gill nets, all sorts of things. So that's another time I fell out of the ivory tower.

First I fell out when Roger Revelle et al came into my office to get me to step out and work on the outfall. Then I realized that somebody has to speak up to overfishing and the fishing impacts. I started in about the late 1980s wherever I went, and I was still serving on NRC panels. That's my big thing while Mia is making sure the kelp program goes for the next 10 years. I'm on the trail fighting fishing impacts.

That played itself out on an NRC panel on reauthorizing the Magnuson-Stevens Fishery Conservation and Management Act in the early 1990s. Many of the panel members were people that had actually the written original flawed act, so they were not very interested in changing it and almost none gave a damn about the ecosystem impacts.

I thought I was going to be a minority of one, so I started writing a minority report and documenting fishing impacts all over the world in this report, -- people were sending me stuff. It was all very gray literature. I was writing this minority report for this committee about why fishing was having an impact.

I sent it around to the other committee members. About half of them said that they agreed with me one way or the other. I am not sure there was a cause and effect, but the chairman resigned. Another reasonable guy came in. The eventual report had material I wrote on ecosystem-based management and critical fish habitat. Some of those words I wrote are still floating around and an important part of the ideal of the law.

So I was out of the ivory tower. That paper that started out as a draft minority report took a long time to get it out, and it finally came out in 1995. It's on fishing impacts, and I think is one of the better papers that I've written on conservation.

In the 1990s, there was another intellectual adventure for me with a big workshop for several months in New Zealand. It ended up in several papers on scaling, because scaling was a big issue for me from the 1970s, as I realized from Brock Bernstein who showed me how important it was to look at different areas in the kelp bed and to build oceanography into it. George A. Jackson, one of Wheeler North's students, came to Scripps, and had done some of that for his Caltech thesis. So Mia and I were into oceanography and scaling kelp forests in the 1970s. It was a big thing for me. We wrote a book chapter about it that came out in 1984 that really

pissed off Bruce Menge! Then finally in the 1990s, the Kiwis in Hamilton, Simon Thrush organized a big workshop of really smart people, and it was all on scaling and mud flats. That was important.

Then there was another partial sabbatical chasing soldier crabs in Australia. Another wave of students came as Eric Vetter and Jesus Pineda moved on in their lives. Arja McCray was a student from Berkeley who wanted to work on estuaries and wetlands. She came to my lab and worked in the wetlands Spartina association at Mission Bay. We set up a timed system to spray nitrogen onto her plots with nitrogen free water being sprayed on as controls. It was hard to set up and I really knew very little about that system and Lisa Levin stepped in and co-advised her as she finished a really nice thesis.

PB: Were the students targeting you in applying to Scripps, or were they here first and then found you?

PD: By this time they knew my name but I would like to think that they were just interested in the system. Dale Stokes came to SIO from Ontario, Canada where he had done a joint geology and biology degree and was interested in the biology of the famous Cambrian Burgess shale fossils. He was and is a brilliant biologist and thought about sharks in addition to the weird fossils. In the end he mostly worked with Nick Holland on Amphioxus biology. He is a terrific scientific diver and photographer in addition to all the other things he does. He is at SIO in the MPL.

PD: Alastair J. Hobday came as well.

PB: An intellectually diverse fellow.

PD: Very much so, yes. His thesis was on kelp paddies, and it was logistically very hard and he did a terrific job looking at the animals that associated with the paddies. But he did a lot of other stuff, especially a really good paper on sheep crabs and another on the white abalone. Alastair is an Australian who went to Stanford and he now has a very high profile job in Tasmania.

George M. Watters came from Humboldt State, which isn't normally thought of as a great place, but my son went there and I think it's a great place. I think it might be one of the best schools in the west. George came and wanted to do fishery stuff. He fairly quickly went up and spent all his time with Richard B. Deriso at the Inter-American Tropical Tuna Commission. He worked on Antarctic king crabs for his thesis, I was co-advisor, but Deriso did all the training -- I facilitated. He is also an exceptionally bright scholar who now runs the AMLR program. It is a very high profile job, but I think he is really exceptionally good.

K. David Hyrenbach came to Scripps, I met him when he was a freshman at UCSD. He was working as a volunteer on my terrestrial projects, and he wanted to come to Scripps. He was hard to get into Scripps because we didn't have any support. David had a rich uncle that promised to

underwrite him, and the department only agreed to do that if the rich uncle deposited the money into escrow for David's career. David bounced around. He wanted to work on mammals. He went up to UC Santa Cruz and was going to drop out of science. Tom Hayward suggested he consider working on CalCOFI cruises that gave one a lot of opportunities to look at larger animals and David did this and fell in love with sea birds. Because I don't know anything about birds, I got him hooked up with George L. Hunt, Jr., a really good sea bird ecologist who was at UC Irvine at the time, so it was nearby. David got his training from George Hunt, really, but he was my student and we interacted a lot; he now is a PEW fellow and doing very well.

Karin A. Forney applied about the same time as Sue Moore, Karin was from the upper campus and I think she already had a job at NMFS. She worked on dolphins and Jay P. Barlow was really her advisor. Karin came to see me about advising at the same time as Sue E. Moore came, who was also working on marine mammals. I wanted to entrain James Enright, who was a good friend and quantitatively capable into marine mammal science. I persuaded Karin Forney to put Enright as her SIO advisor, but she was still interacting with me. Then Enright had medical issues, and de facto I was her SIO advisor, but Enright was on paper as her advisor. I think of her as one of my students, but really she got all of her training from Jay.

Sue E. Moore had worked for John Oliver in the 1970s picking samples. She went off for ten years to do surveys on Arctic whales and had all of this survey data. Then she showed up at SDSU where she did a really sophisticated MS project and applied to SIO. I got her into Scripps to work up her 10 years of arctic whale observations for a PhD, which she did with Douglas P. DeMaster. Both Sue and Karin are now very important players in their respective NMFS labs.

So these were people that came to work with me or that I was involved with, and some got most their intellectual guidance from other people with me being a facilitator.

PB: You've been involved with a lot of students.

PD: Yes, 35 or so.

Michael H. Graham came, he's another bona fide phycologist from Moss Landing. He worked in the kelp bed on spore dispersal. He is now doing very well at Moss Landing.

Tonya M. Huff was from Nebraska, and she'd applied with a letter from a friend of mine at Nebraska. She didn't get in, and then Mia and I got some money in the late 1990s and got her accepted. She was going to be a diver, but she really was not terribly comfortable in the water, so she ended up doing a really good thesis in the intertidal on trampling and things. She was really good and I'm really proud of her because she's gone on and become an excellent teacher at the community college level.

We need much more of that, we really desperately need people to be good teachers, and she's one of the best. I'm very proud of her.

Edward Parnell came back to work with us as a postdoc.

PB: What do you mean by "came back"?

PD: Ed had worked as an undergraduate for Mia and myself long ago. He worked for several years while his wife went through medical school. Then she went to Hawaii for medical training, or whatever doctors do. Ed went over and got a PhD in Hawaii. Then he came back as a full-grown scientist, a super scientist. He didn't have any money, we didn't have any money, so he just sort of fit in with us. Jesus Pineda gave him some money to help with his work with Ed working in our lab. And after that Ed's whole career at Scripps has been made with thanks to cobbling together donors. Now he has taken over the kelp program, so he's on firmer ground. He did a lot of really valuable things on conservation with the reserves. He was by far the main player on all of that. He came in 1996.

PB: Would you talk about your consulting for San Ignacio Lagoon? That was to provide opinion on building an evaporative salt production facility at San Ignacio Lagoon similar to that at Guerrero Negro, Scammon's Lagoon, right?

PD: The company is at Guerrero Negro, where it has an existing desalinization process that circles the Scammon's Lagoon, a big one. It's another one of those things like the one with the City of San Diego, where they came to Scripps and somehow they found me. I don't remember if they found me and then I brought Clint Winant into it or vice versa. Maybe they came to him. It was an environmental issue, they were trying to get an appraisal of environmental impacts of flooding a big evaporite, which is a big salt plain, around the backside of San Ignacio Lagoon.

That whole area had a big evaporite through the ages. There's something like eight feet of solid salt, which is really critical because it makes it a rare opportunity to put sea water on top of it and not penetrate through that and get to the groundwater. The groundwater is protected, and you have much more efficient evaporation pans. I didn't know anything about it. I had been to San Ignacio, I had been to Scammon's Lagoon with my class and gone out and looked at the whales. I just knew nothing about it the process.

These guys in their suits showed up, and I was pretty aggressive about hurting the whales and hurting the desert and didn't want to be involved. Again, it's one of those things that if I'm not doing it, somebody else will do it. Better my camel's nose in the tent, because I thought the tent was full of bad things, and I would be able to publicize all of the environmental horrors that I was already being told were going on. They came and they persuaded us to at least come down and look. Clint was heavily involved from the beginning and was by far the intellectual leader of our group.

PB: You were asked to determine the biological impact on the whales, and Clint Winant was determining the physical oceanography of drawing water out of the lagoon?

PD: That's right, although I was not focused on the whales, they had real whale experts to do that; I was to look at benthic ecology. Our project really was about physical oceanography and marine ecology. As it turned out my concerns about the desert were mostly unfounded as there is nothing on the evaporate.

PB: There's nothing there, literally.

PD: It's the damndest thing. They hauled us down there and they rent us four wheel ATVs. It's the first time I'd ever ridden a four-wheeler, because I think they're so bad for the environment because I'm such a Green twit, but God, it's fun! As far as you can see, there's nothing. It's flat as far as you can see, so that's where the water was going to go. I could not find any macro biota on the crusted salt flat, it was just like Bonneville Flats in Utah. They were going to pump sea water on it up like they did at Scammon's. We spent a lot of time at Scammon's. So okay, I agreed to do this.

Somewhat earlier than this, a young Catalan student, Enric Sala, had showed up in my lab with a small Catalan grant to come study at Scripps for a year. That money disappeared right away, so I needed money for Enric's salary. This money still did not bring him up to anywhere near par, Enric was working for almost nothing, but at least it kept Enric in my world. Enric could speak Spanish, and we had already been working in the Gulf of California on conservation things. So I bought Enric into it and the little money we got from ESSA for the project made it possible for Enric to stay on another year of so.

PB: Didn't he work in La Paz?

PD: Yes, that was his original project. His main work was La Paz. There is a Science paper that we wrote, it was something that I sort of started. That was Enric's project when he came. That was negotiated through the Birch Aquarium at Scripps that was trying to collect fish down there. A nice Mexican scientist came to me the same day Enric showed up in my office for the first time. So here's the guy from La Paz, Carlos Sanchez, and Ned Smith the director of the Birch Aquarium at Scripps had come, and Sanchez, the Mexican guy who wanted to work on reserves around La Paz as something that Ned wanted to sponsor. They had just showed up in my office and didn't speak any English to speak of. I speak no Spanish and we were really struggling, and down the hall comes this wonderful Catalan student who had written us, I had never met him or anything. But his thesis had been on marine reserves, exactly what we were trying to talk about in my lab! He walked into this meeting, and that was sort of the beginning of Enric's charmed life in our lab. This was well before the ESSA adventure.

Mitsubishi subsidiary ESSA - Exportadora de Sal, S.A (a joint venture with the Mexican government).

So we have Enric working down in Baja California in the fishery and he is so damned good that he just took it over and ran with it brilliantly. But originally this was the project that Ned Smith set up, and we ran with it struggling to get additional donor support from the Tinker Foundation that John Steinitz facilitated. Then into this the ESSA salt works company came. We had this competent presence, that's probably why they got us. Clint Winant, thank God, was involved because he's really mature; very much more than I am. He carried most of the politicking and the things that needed rational discussions.

It soon was clear that there was no problem with the salt development. The whales were not going to be affected. The whales loved Scammon's Lagoon with its huge salt works. They were leaving San Ignacio Lagoon in droves because the tourists were loving them to death. Those tour boats - - some of the whales really like it and they do come up, but a lot of the whales are being harassed out of the supposed pristine lagoon. So actually the whale population was going up at Scammon's Lagoon surrounded by the salt work that had been in place for decades. Salt works didn't affect the whales... zero. But the whales were happy to leave San Ignacio where they were being loved to death and move to Scammon's where the salt works protected them from all of the love!

My real work was to look and see whether the pumping at the back end of the San Ignacio Lagoon was going to affect the benthic stuff.

PB: They were concerned about the shallow water benthic environment?

PD: Well, they're going to take larvae, and I was concerned that if there's a population of the invertebrates that are only in the very back parts of the lagoons, that they could in principle deplete their larval pool. So that was the only ecological impact I could see.

PB: That's very shallow at the back of San Ignacio Lagoon.

PD: It's very shallow, but there wasn't such a population.

PB: You took samples?

PD: Yes, we did experiments and we did a lot. I was lucky, I had a wonderful Irish postdoc, Graine Lynch, who was down there helping us too. We had quite a lot of really good free labor. We did a pretty good project. Clint did a wonderful project, and I think he published it, it was really good. There really isn't much of an impact. But with Enric's help I learned a lot of cool natural history. One example is that the coyotes get the crabs out of their burrows by sticking their tails into the entrance of the burrow so the crab grabs the tail and is pulled out and eaten.

Why this affected me so much is that I again came into the crosshairs of the environmentalists at this point. I just had no idea how utterly and aggressively dishonest the Natural Resources Defense Council (NRDC) was in particular. Mark J. Spalding was their point man in terms of bullying us and he was particularly odious. They just excoriated Clint and me. I was getting letters from people in Germany and Australia about what had happened to me to go so very bad so fast, didn't I have any character. They did it by name. I keep wanting to write it up and expose NRDC for what a horrible outfit they are. I have a whole file of stuff that I will give to the archives. They had big ads that were published in the New York Times and elsewhere with Nobel laureates and important environmentalists ripping us.

PB: Did they name you in the ads?

PD: I don't think they did, they may have, but it was us that they were targeting because we were the only people standing in the way. I was named in a lot of the press releases. The poor Mexican company was really hard hit by this dishonest barrage of lies; the salt company totally open and honest. They were very up front and it was a pleasure to work with them. There was one good honest newspaper person that did not fall for the NRDC publicity, his name was Tom Knudson at the Sacramento Bee. He was writing stories that supported our integrity.

There was an article in a Los Angeles paper that really did a pretty thorough job of showing the real dishonesty of the NRDC. The reason those guys alleged that we were so corrupt and so awful is that we were getting paid by the Japanese, it wasn't the Mexicans, although it was a Mexican company, but it's 49 percent Japanese. So it was the Japanese whale killers who have bought out these horrible dishonest Scripps scientists by name. That's what was getting around in the press releases. So I'm catching all this abuse, and it's all because I'm getting paid, that was my crime. Now it turns out that the chief dishonest person was Roger S. Payne, whom I'd always thought was a hero, and now I know he is just as sleazy and dishonest as Spalding. Both of those guys were being paid way more than I was!

PB: You can edit that out of the transcript.

PD: Yes, well, I probably should, but it is already published and I would love to use that big file of history and back it up if called. I can say what I want about those guys given what they said about me, and especially Mark Spalding and the NRDC mammal people. They are just utterly dishonest.

I realized, I looked back and I saw the NGOs fighting the truth on the sewer outfall, I see them exaggerating all over the place. The fishing impact stuff, you find the environmentalists can be as dishonest as the other side. Now even the Marine Sustainability Council turns out to be just another advocate for big business. And on the other side the vested interests can be pretty bad, especially the sports fishing organizations.

This whole business is all just horribly dishonest. I really have given up on working with the conservation establishment after that exposure to the NRDC. Just thinking about this scene makes me want to take a shower.

PB: Given up on being asked to give an expert opinion or review something?

PD: Yes, well, I'll do it because I'm still Green, I really care about my kids' future, but I really dislike and distrust those big NGOs that so often simply represent money.

PB: The NGOs meaning the Green organizations?

PD: Yes, like Greenpeace. They are green because they like green money. I had a go at Greenpeace and Environmental Defense Fund when I was talking about the Antarctic in the other oral history.

PB: Have you had any good experiences with some environmental NGOs?

PD: Yes, the Ocean Conservancy, I was on their board. They're good. There are some good outfits. The San Diego Ocean Foundation is great. There are quite a few good groups but it is hard to know whom to trust.

It is complicated, consider the local Bay Keepers.

PB: Who became Coast Keepers, right?

PD: Coast Keepers, bays keeper, whatever. They were responsible locally for helping us get the reserves. At the same time, they were suing the City about things that they know was not true. It is so frustrating.

Other Coast Keepers, that general big organization, have been milking the government for millions of dollars to reestablish kelp. It's all a fraud. They haven't reestablished anything. The only thing that they can do is get volunteers to bash urchins in Crystal Cove that they planted with kelp spores when probably the kelps would have dispersed there anyways as they do everywhere else. Otherwise they've taken huge amount of NOAA money and money donated by well-meaning working class folk to reestablish kelp. They still say that the kelp beds are down by either 75 or 90 percent from what they were 50 years ago, so we have to go out with all of this money and replant them all. Fifty years ago we were in the middle of one of the most massive El Niño's in history. There were no kelp beds. Now the kelp beds are thriving. The fish are gone, but the *Macrocystis* are doing fine; their entire premise is a flat out deception - a lie.

So I see all of this dishonesty in this business, and it breaks my heart because of the 1960s and 1970s they were our saviors.

PB: It's like an industry, the Green industry.

PD: Yes, exactly.

Okay, there was another flock of students in the 2000s.

Megan C. Ferguson, who worked on beaked whales, came from Friday Harbor. Again, I was just a facilitator and she worked with Jay Barlow. I think most of her education came from Daniel Goodman, actually, in Montana State University as well as Jay. I was co-advisor but again that meant simply being a facilitator. She went right into NOAA and got a job with whale surveys.

Bonnie J. Becker is an interesting story. She had applied to graduate school, and Mia and I liked her. She was from Harvard. I'm always a little chary of bluebloods, but Lisa came from Harvard as did Bob Paine, so I have to give Harvard some credit. Bonnie applied, and we didn't have any money. Meanwhile I was working on reserve stuff with Gary E. Davis, who was chief ocean scientist of the National Park Service. Gary Davis and I were friends. He negotiated a salary for another person to work at Cabrillo National Monument as a full-time ranger and to be a marine scientist, and this was going to be one of my students.

Davis negotiated this with Terry DiMattio, who was the Cabrillo superintendent, and he was happy enough. Bonnie worked under another supervisor, so there was quite a lot of political moving around. They weren't going to bend any of their rules, so Bonnie had to wear a uniform and the cool sombrero. Bonnie liked wearing a uniform anyway, so it wasn't a problem. But she was an excellent park ranger while she did her thesis! It was a terrific score by Gary Davis, one of those huge unsung heroes working behind the scene to make the world a better place.

PB: Bonnie worked on the Cabrillo tide pools or coast, or do something for them?

PD: She had to put in her hours down there and I think she did a wonderful job for the Park. Bonnie was a full-time ranger, so she was getting more money than most of the students. She had a full-time job and was also a student, and the Cabrillo people made it possible for her to do her classes and take her courses and do my field class and be a student. It was a wonderful arrangement, and they got a really good ranger for awhile. I hoped she was going to stay there, but when she graduated she got a job up in Seattle. Bonnie was looking at mussel dispersal, the larvae. It was a good thesis, and in the end she got a lot of her advice from Lisa Levin as well who became her co-advisor. Then she went away to a job at the U of Washington and they hired Benjamin A. Pister as the Chief of Natural Resource and Science at Cabrillo National Monument. He worked on the upper campus biology department, but worked in my lab a lot. So we had another friend out there in Ben Pister, but who has now left them and gone to Alaska.

PB: Does Cabrillo still have that position?

PD: Presumably, but it's not being filled by one of our students. Life goes on.

Cynthia A. Catton (married name Button) came from Friday Harbor as well, and is a really good invertebrate zoologist. She worked on the abalones, the recovering pink and threaded abalones out at Point Loma. The sea urchin fishermen, especially Pete Halmey, were really helpful because they were seeing the abalones and they knew where they were, and they took her out. We have a good relationship with Pete Halmey helping our various students. He made Cynthia's thesis correct. James J. Leichter took over and shared her as an advisor.

Marco Hatch arrived to work with me in the early 2000s. He had worked on ancient salmonid DNA from coastal middens in the Puget Sound and he was and is very interested in integrating the deep time information from old middens into modern oceanography. He used very sophisticated isotope data to reconstruct oceanographic conditions over the last 3-4,000 years. Technically his project was extremely difficult and I think it is amazing that an ecologist was so successful with a project that involved so much geochemistry. He is now working at an Indian College in Bellingham, Washington.

The last one that I graduated -- again, it was more Jim Leichter than me -- is Talina Konotchick, who worked on kelp physiology. She sort of bounced around, and once she got focused she did a really good job on, interestingly, looking at RNA production and going at kelp physiology from the molecular side. Now she's at the J. Craig Venter Institute.

PB: Are you high in number of students as professors go at Scripps?

PD: I'm not sure, Dick Rosenblatt had a lot and they were very well trained - I think he was a much better mentor than I was.

PB: Taking students is up to you, right? There are professors here that don't seem to take in many students. Is that because you've always had funds to support them?

PD: No, funding them has been a nightmare for me because I have trouble getting money. Many of them fund themselves with fellowships and grants and things.

I think that I've certainly appreciated that I learned more from them than they do from me. I think in most other faculty relationships at Scripps this is not true. I think mostly the professors teach their students a lot. I get really smart students and get out of their way, and help them when I think they need it. If you get good mature colleagues as students, and treat them as colleagues, it's win-win. They are my life. If I think back, the papers and the awards and stuff are just stuff. To me, good science goes extinct right away because people should build on it and move on. A good student who's turned out to be independent and creative, as opposed to one who is technically very competent but works on assigned

projects, continues to be independent and creative and trains more students in that mold. It's the best thing you can do for the world, to train really good people. I really believe that and I'm really proud of them. I take pride in my children and my students as my legacy.

PB: I know you take pride in your students because it's evident in the hallway outside your office with the photo gallery up there.

PD: Well, they're a part of me. There are a few more Scripps things that are sort of interesting.

My memories of William Nierenberg as a director -- I thought he was a good director. The other people that knew more didn't like him, but he was always fair to me. He directed; he was a director in the old sense. He could be heavy-handed, and he certainly made some enemies, but I didn't have any real problems with him. He was friendly to me and he didn't interfere with me. The faculty meetings then were pretty aggressive because when people retired and there were new hires, all the various groups fought for them. The geologists at that time were really well organized and very effective. They worked together much better than they biologists that couldn't get along. In this case the geologists were running a geology search. Nierenberg came in at a meeting that was held in 307 Vaughan Hall, the room above the Vaughan Aquarium. That was where we had our big faculty meetings, and it was sort of my favorite classroom, it was away from things.

Faculty issues are faculty driven, and they should be. They should be Academic Senate people making decisions. The Scripps Director shouldn't be making academic decisions. Nierenberg came in and announced that we had to do an affirmative-action search. He told about how it had come down from above that Scripps was a little deficient. I looked around -- all males, all white, more than a little deficient. There are not a lot of non-white oceanographers out there to hire anyway, and I'm all happy for diversity given my political thoughts. Nierenberg is carrying on about how we need to do this.

I always was sort of afraid of a director, but the geologists especially weren't. Harmon Craig and Albert E.J. Engel were particularly vociferous about how he had no business telling them whom to hire. They'd hire whomever they wanted, and blah-blah-blah. This was mutiny right there in 307 Vaughn Hall. I was just quiet, a little fearful and very apprehensive as I had never seen a mutiny.

Nierenberg said, "Well, test me. I do have it, I'm Vice Chancellor." Or something. I do have some power, test me. They were getting pretty mad. Nierenberg then played the right card, which to me seemed obvious, me being in the back of the room and very Green and liberal.

He looked around and said, "How many people of diversity do you see in this room? Do you see any brown skins?" No, there were none. And they

said, "Well, there aren't any black oceanographers. They don't exist, we have to go to India and get some."

"That doesn't count," Nierenberg said. "It has to be an American diversity issue."

PB: Were they only talking about people of color, or were talking about women as well?

PD: That's what came up. The American issue only applied if you were bringing in a black male. You could bring in a non-US citizen female. So Nierenberg said women count. I'd never thought about it because from the very beginning, we were full of female graduate students. We were cranking them out. Fager had a several, Elizabeth 'Pooh' Venrick was an lecturer then. We had women in the research series, but apparently not on the faculty. Women were all over the place, and women of some prestige. So I'd never thought about it, but there were none in the room!

So then there was a search, and a bunch of women came through giving really good seminars. I went to all of the seminars. One of them just blew me away. It was a young Miriam Kastner.

PB: Oh yes, an impressive scientist.

PD: Oh, my God, was she good, and she just blew me away. They asked her really aggressive questions, and man, was she all over the questions. It was one of the most impressive job seminars you can imagine, and she was very alpha. She wasn't taking any gaff from anybody. So here is Miriam up there, and I'm just thinking oh, my God, you know, this person is a genius. This is obviously the most intelligent self-confident person I've ever seen.

The questions ended and she continued explaining things to all these guys as I left and their body language was of great respect. So then of course, they brought her in as a candidate to hire, and we had to discuss her in a faculty meeting. This is what sticks in my mind, I just can't get it out of my mind. Everybody is sitting in there, and Miriam's file is going around.

PB: You were junior, so you were being low-key and quiet, right?

PD: Oh, very much so, yes, I was as silent and invisible as a coward can be. Al Engel is the big superman, and Harmon Craig, I am not sure whether Harmon was at that meeting, but they were studs. Al Engel was a National Academy member, a big name and a huge presence at Scripps.

So somebody said, "What do you think, Al?" Al, in the back of that room -- I still remember it, he was in the back right-hand side of that room -- he sort of half stood up and then leaned on the chair in front of him and said, "Miriam Kastner is the second coming of Mary Magdalene." "We will never find her equal." I'm just stunned at the imagery. I'm trying to

think of this really aggressive Israeli woman being juxtaposed with Mary Magdalene. So her file quickly went through the system. Every time I saw Miriam Kastner over the years, and sometimes I still do think of it, I think of this Christian biblical figure juxtaposed with Miriam Kastner. I'll never forget that.

PB: Did that metaphor mean Miriam is like a female Jesus?

PD: Oh my god, imagine what Miriam would think of that! Good lord, but this is exactly the juxtaposition that overwhelmed my little mind! I think for Al it simply meant that she's wonderful, about as wonderful as it is possible to be. Had Al considered his words more carefully, but still wanting to use some sort of Christian comparison, he might have compared her to Hildegard.

Oh, man, she won over everybody. She still does, of course. She is a very important international player greatly respected by everybody here and around the world.

PB: Anything else?

I have talked about the early directors that I knew, but not much about Charlie Kennel.

When he first came Mia and I went to a meeting at UCLA that was designed to be a power grab of the MLR money that Bill Hamner had found at Scripps. The meeting is a story in its own, as he was pressured to include people from Scripps and Mia Tegner and I went up and sat down for breakfast at the hotel they put us. A bearded guy with a table full of worshipful female graduate students sat down right next to us and the guy started loudly telling them that they would get to see a real eco-extremist in Paul Dayton whose exaggerations will be really interesting. Mia had a real temper and started to leap up and get in his face but I shushed her so we could learn more about this awful person. I learned that I virtually had no soul. Neither of us knew who it was until we went to the talk where Bill dumped my slides all over and my talk was a disaster. But the guy was Milton Love!

I came back with a good plan that I had written into a proposal to set up regional CalCOFI type coastal monitoring programs and I tried to take that to the advisory group Charlie had set up. Charlie was there and I thought I was talking to him as I made my pitch, but Bill Hodgkiss had been set up as some sort of assistant director, and Bill chewed me out for telling Charlie what to do and any proposal such as this had to go through him and the committee and I was dismissed. I tried to send it to the Packard Foundation on my own, but they never responded and it came back as PISCO! I think that might have been more Bill than Charlie, but I was deeply insulted and I gave up trying to work with the Director's office if I had to be insulted by Bill Hodgkiss who was probably just doing his perception of the job with a lot of vim and vigor, misguided authority and a total lack of wisdom.

But when Jeremy Jackson came and got to Charlie, Charlie became a very effective and strong proponent of conservation issues. I was often impressed with his willingness to take on those challenges, and in particular was happy when he supported our Scripps Aquarium video that Debbie Zmarzly produced.

One thing I might complain about a little bit is that Charlie did some serious damage to Biological Oceanography when he took over as Scripps Director, and really did take over the faculty's job of choosing whom to hire. He got way too active in that. I think he is responsible for basically wiping out Biological Oceanography in that he took away a job that we already had, and we had a candidate that we had seminared that we were about to hire. Then he took away three other promised positions leaving us too small to be very effective.

PB: Three other positions in Biological Oceanography?

PD: Yes, that were on the docket that Biological Oceanography was supposed to be given. We'd lost everybody, we were down to four people or something, having once been a very large and effective group. We were slowly getting our identity back. It was a serious blow. I think Charlie fell for the mantra that with the miracles of molecular biology is the future. I heard him say that several times and once praised a student poster that was about DNA as "finally a modern ecologist." I wanted to puke.

PB: Meaning molecular marine biology?

PD: Yes, he kept talking about the ocean and "all the ocean in a thimble," and that BO didn't do enough DNA research and so we weren't really serious scientists in Biological Oceanography. I thought it was extremely ignorant and insulting.

But losing the job was bad enough but the way it played out was worse. I gather that Jeremy Jackson and Ron Burton had protested loudly about the committee choice. In truth the guy gave a bad seminar, but he was by far the best of the people we interviewed and he had a really good record as a mentor and he had not understood our seminar approach, thinking it was supposed to be an example of a classroom lecture. But instead of killing it, we might have been asked to bring him back for another look. That would have been ok, and if was still a dud it would have been a good catch. But Charlie was brutal and Dave Checkley and the others that were in there with me were stunned. Dave had chaired the committee and worked really hard and then Charlie took the other jobs as well.

Dave walked out talking about going after the job in Alaska and I went home and researched an early retirement rather than stay in that sort of institution. I realized that we could afford to retire and I downloaded the documents but then got mad as I realized he could not fire me and that I really loved what I was doing with my students and research. I decided

that I would sit there in my diapers drooling on my keyboard rather than allow Charlie Kennel to replace me.

Later I think that Charlie and I became friends and he included me in a really interesting project with the Venice Lagoon and both Charlie and his wife are around interacting with my wife and me. And one other set of really positive memories of Charlie are the sad days when both Mike Mullin and Mia Tegner died within a few days of each other. Charlie did a terrific job dealing with the grief and the services. I spoke at both, cried at both, and Charlie was simply superb in every respect. Mia had died on a Sunday and Monday the whole group at Scripps was in total shock. I had asked Jennifer to send us a grief counselor but John McGowan beat them to us when he came up with a bottle of Irish Whisky so we might have a proper wake. So we were talking and drinking and crying and Charlie walked in to see what he could do to help. That was a huge and gutsy thing to do and I will always remember it. We offered him a drink that he held while he talked to us. It was a wonderful act and by the time the grief counselor got to us, we were way ahead of him, not to mention rather drunk.

I think that the impact on BO of Charlie deciding all our hires was but temporary collateral damage compared to what it did to the Department as a whole when a Director usurps the academic faculty's responsibility. In our case this resulted in a handful of people remotely lacking any wisdom of biological oceanography persuading him that the discipline should be all molecular biology, "the ocean in a thimble" as he liked to say. This sort of decision-making is extremely narrow and hurtful to an independent and vibrant academic department.

But back to the near demise of BO. Eventually we hired Michael R. Landry, maybe later than that. He was and is a star, and Biological Oceanography slowly turned around, it was not the same but surely not dead.

The issue is much less important now. Biological Oceanography trains their students be generalists in biological oceanography, and the Marine Biology group emphasizes an early specialization with the students being pushed immediately into a specialty. We are all different. The players are the same and the students' moves back and forth, so now we are all biologists. That's a healthy thing as long as we can have a Biological Oceanography program setting up criteria that they want. I'm not complaining about where we are now. But I do think that Charlie damaged BO at the time.

PB: Through influencing recruitment?

PD: Yes, I think it was just a terrible thing to do, to take over the academic senate's job of running an academic department. I still think that was a very serious mistake.

Nierenberg did it to get Miriam Kastner, and it was extremely valuable. But now almost all of these things sort of go through the director, and with all sorts of advice. A search decision is given to the Department

Chair. I don't think it should be that way. I think faculty should be making their own decisions.

PB: Do the students that apply here, are they aware of the distinction between the Marine Biology program here and the Biological Oceanography program?

PD: No, I don't think they are.

PB: And what are the tensions when they get here and they discover hey, what I like is over there on that other side, not here.

PD: I think they don't know. They come with complete freedom to join whatever group they wish, and from the very beginning -- I think we have had a seamless transition -- students in my mind are perfectly free to move back and forth. If they move into Biological Oceanography, we can have them take a cruise and do a few other things but they go both ways with no problems that I am aware of.

PB: There's a lot more coursework in Biological Oceanography though, so do they need to switch pretty early?

PD: There's more coursework, and insecure students are terrified of the oral departmental exam. They need to agree to be generalists, but it is not a big deal so long as they get the material. I think Tim Ragen made the switch to my lab rather late in his SIO career. It seems to me that most of the students are more attracted to MB these days, often I think for the wrong reasons. Otherwise, I prefer that the groups not be separated. The present model is fine with me. I think that we should let this division die. It was started for bad reasons in the 1950s, when Fager and Scholander didn't get along. I could get along with Scholander just fine, and some of my best friends have always been in Marine Biology. The faculty members are perfectly wonderful.

I'm happy to have the division disappear as long as Biological Oceanography can train generalists with a few more courses. It's just up to the committee. You don't need a separate division for that really, if you have a committee that agrees how we're going to train. It's an historical thing that I think can just go away. The thing that Charlie Kennel did was a slap at Biological Oceanography as Biological Oceanography, and it really bothered me at the time, but I'm happy with the present system.

So then we didn't get a fish stock assessment person. But things change. Interestingly, Ronald S. Burton, who was fighting Biological Oceanography then, has done a very good job of helping us try and get these things back. We hired Jim Leichter and now we have a good fish stock person, Brice X. Semmens. We hired an excellent invertebrate zoologist and two tropical ecologists (and while she works in the tropics, Jennifer Smith is also able to teach one of the few phycology courses being taught on the west coast). There is a search for a coastal ecologist that Ron supported

strongly. I think Ron Burton did a lot to get that job open. So if you look at the big picture of ecology at Scripps now I feel good about it.

My sense is to let all of this history go away. I think Burton has done a great job as division director. An excellent job, actually he was probably been able to do more for ecology than an ecologist would have done as he has more credibility with the bigger Marine Biology group.

So as I retire and go away, it's been wonderful to be here at Scripps. Can you imagine a better job for a dyslexic?

PB: I think you just came up with the closing sentence there.

PD: I think so too.