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WOODS HOLE OCEANOGRAPHIC INSTITUTION

ORAL HISTORY OF JOHN FARRINGTON

Interview by Frank Taylor, April 20, 2005

Tape 3 of 8 tapes transcribed by Arel Lucas, November 2005

TAYLOR: . . . 3, 4, 5, 6, 7. [Tape stops and starts again.] We are at the McLean Laboratory at the Woods Hole Oceanographic Institution for our second session with Dr. John Farrington, Dean of the MIT-WHOI Joint Program, a vice president of the Institution, and used to be called associate director. Is that still part of the title?

FARRINGTON: No, no, we changed some of the titles, but the <u>position</u> didn't change. It just became a little bit more aligned with other academic institutions.

TAYLOR: OK. During the first session, John, we went through your early life, and we went through your educational experiences, and . . .

FARRINGTON: Um-hum.

TAYLOR: Got into some of your postdoctoral work and so forth. There comes a point in everybody's career when you have to face the hard, cold fact that "I got to go to work," and "Where am I going to work?" And "How do I get a job?" and . . .

FARRINGTON: True.

TAYLOR: . . . all this kind of thing. What kinds of things were going through your mind at that point, when you had to make a decision as to where you were going to be?

FARRINGTON: Well, I <u>knew</u> that I wanted to continue in the general field of understanding organic chemicals in the marine environment, and that was both the naturally occurring ones—understanding how they played a role in the <u>carbon</u> cycle and interacted with organisms and sediments—and then also the marine environmental quality question of contaminants like the oil contaminants, petroleum contaminants, pesticides, and PCBs. Both of those came out of my graduate degree, but also continued on in my postdoc in Max Blumer's lab, and it was clear to me that was an area I'd like to go into for a career. The question was where, and I was fortunate enough at one point to uh realize that the Department of Chemistry here was considering my appointment as an assistant scientist, which was the equivalent of uh . . . of an appointment in an academic department. It's interesting, looking back on these days: we have a formalized process, and they had a formalized process in those days, too, but the notification was a little bit less formal, and I found out about the fact I was being considered for appointment when I happened to be going through the Redfield Laboratory Lobby at the same time that John Hunt, the department chair, came rushing down the stairs, off on one of his many trips, and said, "By the way, I'd like to talk with you when I come back. We're considering appointing you to the scientific staff."

TAYLOR: [Laughs.]

FARRINGTON: And then he was out the door, and I had to wait two weeks to hear [laughs] . . . hear more about what this all meant, but I was fortunate to get such an appointment, and that was the beginning of starting my own independent research program, although you never are really independent. You collaborate and interact with lots of people. But that's how I began thinking about the long-term career, as I'm beginning, and I'm beginning to write proposals to the National Science Foundation and the Office of Naval Research, and I had a lot of help in that, in the sense of, in the early days of my appointment, I was still supported in part by a grant that had been obtained by Max Blumer, my postdoc mentor. And he began to pass some of that uh research that I'd begun working on as a postdoc, off into my own laboratory area of research, and I could claim sort of that as my own for writing for proposals, and I wrote my first independent proposal to the Environmental Protection Agency uh, which was not a big funder of research at the Institution, but I did manage to get some funding from them on oil-pollution-related things, and then another grant from the Office of Naval Research, related to marine organic geochemistry and lipids in the marine environment, and that was the beginning.

TAYLOR: OK, couple of questions. First of all, when you were walking through Redfield, and Dr. Hunt said, "We're considering you for an appointment," what kind of emotion went through you at that point?

FARRINGTON: Well, I said, "Gee, thank goodness!" You know, I didn't have to worry now about applying for further positions. I had talked with John Hunt a little bit earlier and said that, you know, given my circumstances with the family and the support and so forth, and all those sorts of things, that I didn't want to continue on much longer as a postdoc, and that one year was perhaps all I could consider then. In this day and age now, many years later, two years of a postdoc would be considered to be a golden opportunity, but in those days I was looking to try to get a more permanent appointment and get more in line with a traditional career path.

TAYLOR: Um-hum, you mentioned Max Blumer as being your sort of a mentor here. Was your involvement with him—did it have anything to do with that barge, *Florida*, that went aground here and spilled so much petroleum?

FARRINGTON: Well, it did in a way, in the sense that I became . . . . I was <u>aware</u> of his research already, from reading papers in the literature, when I was doing my <u>own</u> thesis research, but he had <u>not</u> been working on <u>oil</u>-pollution studies per se. He had been working on naturally-occurring hydrocarbons in the environment, and that was part of what <u>I</u> was trying to look at in my thesis research in Narragansett Bay. When I, in my <u>thesis</u> research, stumbled on the issue of chronic oil coming down the sewer systems—storm sewers and sanitary sewers, it was about the same time that Max <u>began</u> to publish papers or you began to hear about his studies along with Howard Sanders and John Teal on the West Falmouth <u>oil</u> spill, and so I became even more <u>intently</u> interested in that, and in fact came with a fellow graduate student of mine, Phil Meyers[??], who I shared an office with. He and I made the trip over here to Woods Hole to listen to one of the Journal Club talks that Max Blumer presented on the <u>oil</u> spill, and I spoke with Max <u>after</u> that, and then I spoke with again at one of the Gordon Conferences on organic geochemistry that are held every other summer up in New Hampshire, where there's an opportunity for people to meet informally, and in fact many interactions between more established scientists and younger scientists occur out of those types of conferences, and I talked with Max about the possibility of a postdoc, uh, and uh interestingly, when I came over to talk with him in a formal sense about it, before applying—or after I applied, I'm sorry—he had unfortunately had to leave quickly to go to uh Switzerland to take care of his mother, who was ill, and so I sat down and talked with John Hunt and a few other people in the department, and at the end of that day John Hunt said, "Well, you know, Max is a very . . . ." He didn't call him "Max." He said, "Dr. Blumer is a very meticulous scientist and very demanding, and very few people end up being able to be a postdoc in his laboratory," and so I came away from that with the feeling like [thumps], OK, I don't really have much of a chance, and then later on found out that I did get a fellowship.

TAYLOR: You bring up a couple of interesting points, and some of them we'll get back to a little bit later, but you talked about informal gatherings, where you had a chance to talk to senior . . .

FARRINGTON: Um-hum.

TAYLOR: . . . scientists. It interested me because at one point, a couple of years ago, I was talking to a Joint Program student, and I had asked them what they felt was the most valuable part of the program. They said, well, and they gave me a whole bunch of things, but they always thought that one of the most valuable things was, gee, if you could sit out on the porch at one of the buildings and have a beer with one of the . . .

FARRINGTON: [Laughs.]

TAYLOR: . . . senior scientists and just talk. They said they really thought that was absolutely terrific.

FARRINGTON: Well, I think when you get to graduate education a lot of it actually is the result of informal discussions, seminars, one-to-one meetings and so forth. We also tell the students, and my philosophy about this is that they get probably 40 percent of their education—you know, we can't really quantify that—but let's just say somewhere between 30 and 50 percent of their education actually by talking to other graduate students, not just to the senior people, but sort of exchanging ideas, kicking it around with their peers, which is the same thing that goes on in research once you graduate and you get out. I mean it's . . . . Research is rarely done all alone. Sometimes it is a person really lonely, working through an idea, but more often it is an interactive process with a number of people.

TAYLOR: I found even when I was teaching in what we called the AP courses, the Advanced Placement courses, we expected the kids to help each other. That was the way they could kind of get through . . .

FARRINGTON: Right. Yeah.

TAYLOR: . . . this, this kind of communication, and as I say, we'll get to that when we get to the . . .

FARRINGTON: OK.

TAYLOR: . . . Joint Program, but you made that statement. It kind of triggered that in my mind as some of the things that become so important to a young scientist coming in the field, and I wonder sometimes if, at the point in your career you were then that was really basically just starting, and a fellow like Max Blumer and John Teal and Howard Sanders that had been . . .

FARRINGTON: Um-hum.

TAYLOR: . . . working this oil spill, and I recall reading an article by a Texas A & M geologist who absolutely tore apart the research that was done, said it was just absolutely terrible, all of it, and so on. Of course, we know now what they did still kind of stands as the baseline for how you investigate that kind of thing today.

FARRINGTON: Right.

TAYLOR: Did you ever run into that kind of thing as a young guy and be a little bit intimidated when you see someone from another institution come in and tear something apart that you probably thought was reasonably good work?

FARRINGTON: Well, I think it depends on . . . . I don't think I was ever <u>intimidated</u>, per se. I think it was more wondering why people would attack things in a certain way that they did, and also <u>sometimes</u> being, quite frankly, disgusted with the excessive nature of some peoples' pursuit of the, you know, the <u>counter</u>-attack as a way of <u>testing</u> peoples' adherence to particular science. You know, do you really <u>believe</u> in this particular point of view. Can you <u>defend</u> it? That sort of thing. I was actually inspired by Sanders and Blumer and Teal in terms of their willingness to stand <u>up</u> to what, you know, even <u>today</u> is a really large and influential industry, and most of these concerns and <u>attacks</u> came from people who uh thought that they were overinterpreting their data because it was, you know, it was an unusual and first-time type of study, and it was at the height of the environmental movement. Earth Day occurred in 1970, and they were, you know, the spill occurred in the fall of 1969, and the studies were ongoing as the great environmental furtherance, so some folks naturally will be suspicious that people are getting carried away, and in . . . . They hadn't seen some of these things happen in their own area, and so the question was, "Well, you folks have been out there investigating biology in the Gulf Coast," in this particular case that you were mentioning, "so why haven't you seen any effects, and there's a lot of oil and gas production down here." And the answer is that they had seen some effects. In fact, the . . . . But the biggest effect of the industry in the Gulf of Mexico that was readily apparent was the fact that they had made channels and canals and things in a lot of the marshes in order to get equipment into the wetland areas in Louisiana and parts of Texas, and those had really altered the nature of the wetlands, and so there was a physical disturbance that everybody agreed with, but not so much the effects of the oil itself.

TAYLOR: And just recently I saw an article that talked about how much land now, because of that, Louisiana is losing on a daily basis—this messing with topography, so to speak. These things come out. The reason I asked that was because you were just starting off in the career, and you named some people that I think extremely highly of, as being kind of early . . .

FARRINGTON: Um-hum. [Clears throat.]

TAYLOR: . . . inspirations to you. Because it's a field that, when I talk to some of the people here, as I look ahead, what you have to go through to finally reach that senior-scientist . . .

FARRINGTON: Um-hum.

TAYLOR: . . . state is really frightening in many ways. It's truly the major league. So that's . . .

FARRINGTON: Yeah.

TAYLOR: . . . kind of the . . .

FARRINGTON: Well, the . . . the message that I got was "stick to the science and," you know, "stay on message." Those are modern words. They didn't tell me that, but those were the days when you were staying on message, and "make sure you understand what you want to do." You can involved in a lot of advocacy if you wish, but you do so at the risk of losing some aspects of your scientific reputation, and early in your career that's not a good thing to do. It's better to build up your credibility as a scientist, but I remember at various times all three of those people—Blumer, Sanders, and Teal—saying in effect that the public funded us to do research, and when you had things that were important for the public to know that we should communicate to them, not only in the scientific publications, but also in a way that could be understood. And part of the problem was that it takes a long time to get scientific publications out, and so in the initial stages there was an argument saying, "Well, they haven't published in the peer-reviewed literature yet, so that's . . . . You know, we can't really be sure that's correct." Then, when the publications came out in the peer-reviewed literature, it was, "Well, this is an unusual, one-of-a-kind type of spill," and the detractors went to all of the concerns about that, and then there was the concern about the analysis of the scallops in West Falmouth, which still contained some oil, and that led to somebody in . . . in Texas, I believe it was, saying the reason the Gulf Coast oysters tasted so good was they had a little bit of petroleum in them, and . . . and in fact, then other people would argue that you can't taste any of the oil at all. But the problem was that when the compounds that you could taste were gone that didn't mean the ones that were of concern from a toxicity perspective had also left the tissue, and this was one of these things which I'm not sure, in the previous session whether we talked about it or not, but there was a lot of "out of sight, out of mind" type of approach to environmental issues in those days that nowadays we're much more probing about and saying, "Well, OK, if you can't see it with your eyes that doesn't mean that it isn't happening. You need to use much more sophisticated scientific approaches." And that's what Teal and Blumer and Sanders and their groups did.

TAYLOR: OK, and so, in a certain respect, it got you started on a whole line of investigation throughout your research life, then. I mean it was an interest you had, but . . . .

FARRINGTON: It was an interest I had, and I came here, and I never actually originally worked on the West Falmouth oil spill, and . . . because a lot of people were working on it. So I worked on offshore issues. And, in fact, uh at one point it was clear to me, although it was never stated explicitly by anybody, but the expectation was that I would take organic geochemistry of the type that Max Blumer was doing to sea, that he was more of a laboratory scientist. He got very sick going to sea, and he didn't like to go to sea and he was much more at home in the laboratory, and so my role in a sense was, as an oceanographer, formally trained chemical oceanographer, to begin to go to sea and to take organic geochemistry to sea, and that was in parallel with another colleague who was here at the time, George Harvey, who had graduated a lot sooner than I had, came from a traditional chemistry background and had actually been working at Monsanto, but then came to the Institution and started to do research on PCB contamination and DDT about a year and a half before I came as a postdoc.

TAYLOR: Could you talk some about your experiences at sea? You mentioned that Dr. Blumer was a laboratory scientist, and one of the things, when the new Joint Program people come down they've got a situation where their laboratory is nice and stationary and [laughs] no problem, you know.

FARRINGTON: Right.

TAYLOR: And they're going to be out in a laboratory that's pitching, rolling, yawing. Temperatures could be wild. I don't know how you do it now, but I could imagine trying to do a titration onboard ship in rough weather or something like that as being problematic. Could you talk about some of your early cruises and what it was like for you—how you got ready to go to sea, what your work schedule was like when you were at sea, and what you did with the materials when you came back in?

FARRINGTON: Sure.

TAYLOR: And I'm going to throw a few other questions in as you talk about this.

FARRINGTON: OK. Well, true to my background as a formally educated chemical oceanography, I had to, as part of my graduate education, go to sea. And I had been to sea already on a two-week cruise between Bermuda and Narragansett in which we basically stood watch, did a lot of geophysical, geological measurements, and, toward the end of the cruise, took a couple of samples for my own research on the continental shelf off of Narragansett Bay. When I came to the Institution, I wanted to get some additional samples. There were a number of samples that had already been obtained on cruises on the *Knorr* and the *Atlantis II*, sediment samples that needed to be analyzed and looked at on transects offshore. But I wanted to go to the New York Bight and take a look at material that was being dumped, both from the sewage sludge and the harbor dredging in

New York, in the Bight area, to see what the levels of contamination were. And so I went out on uh my first cruise in April of '72 as a postdoc on the R. V. *Gosnold*, which was—as many of us have said tongue-in-cheek—a well-appointed yacht. Not quite. It made 6 knots sometimes going with the tide, then, down the Sound. But in any event I went on that cruise with Gil Row[SP?], who was a biologist, and Hovey Clifford, who worked with him, and Ken Smith[SP?], who's now out at Scripps, and we took some samples in the area, and I brought them back. And then later on in the fall of '72 I went on a cruise with John Teal, who had a series of cruises out of Woods Hole going to Bermuda, sampling the Sargasso Sea, and we again did some preliminary water sampling analysis—Ollie Zafiriou and I together. Ollie had not been to sea on a major cruise, and so we went together and did some preliminary work. Then in the fall of '73, we began the more serious uh work in which I began to collect more samples. I was on the *Knorr* from Bermuda to Woods Hole and we collected a lot of sediment samples again on that same transect between Bermuda and the New York Bight to fill in the holes of samples. And typically what we would do on those cruises is not very much laboratory work but mostly sample collection work in my . . . in my research.

TAYLOR: What kind of instruments did you use for your collecting?

FARRINGTON: We used a series of different types of coring devices, and on this particular cruise we were using what was called a sphincter core that had been developed by John Burke for Vaughan Bowen's research. It took a sample of about, oh, an upper meter at most of sediment, but it was relatively undisturbed if you put the core in carefully, but it's very difficult in deep water at 4,000 meters to figure out what's going on at the end of the wire, and so we spent a lot of time, you know, trying to get this device to work, and it did work periodically for us.

TAYLOR: Was this straight hands-on training to deal with this, or on the job training, you do to learn how to use these instruments?

FARRINGTON: Well, we went through a couple of dry runs on land with John Burke about how it all worked, but then we did it ourselves at sea. And you have to figure out how to make modifications along the way, but we managed to get several good samples, but it . . . . You know, in the deep water you have, you know, a couple of hours down and a couple of hours back, and you . . . and you monitor it on the PGR in the main lab and see what's going on to make sure you're in the right position, and then you get the thing back onboard, and then you spend, you know, another two to three hours slicing it up and putting it in jars and taking them up and recording all the data and then putting them in the freezer and keeping them until you can get into the lab to analyze them. We did take our extraction devices to sea a couple of time, but the fact of the matter is that the extractions of samples usually took a couple of days anyway, and so by the time you'd get through collecting on the cruise, most of your effort is focused on getting the samples, and you finish the extractions once you get back into the laboratory, and that could go on for months. But as you get to shallower and shallower water, all of a sudden, your turnaround time in terms of getting a core onboard, slicing it up, and getting over to the next one becomes harder and harder, and we basically ended up working, as is typical on some cruises, you know, 24 hours and then 36 hours, with people just taking catnaps in between, because one thing that you, you know, you learn early on as an oceanographer is that ship time is extremely valuable, and woe be it to the scientist who leaves a vessel sitting around out there while the bridge is calling down to the main lab trying to figure out, you know, what's going to happen next. You just don't let that happen. You have a very tight schedule, and you try to schedule in as much things as you can.

TAYLOR: Well, I would not want someone that listens to these think that you spend part of your day in a deck chair on the fantail or something like that.

FARRINGTON: No, rarely do you get to do that. Once in a while on transects between stations, if you have a long way to go, you finish up a lot of things, and then you do [clears throat]. And we have had a couple of cruises where, out of 20-some days we'd have an opportunity to, you know, having an equator-crossing ceremony that lasted maybe six hours while we were under weigh from one station to the next, and you might get to relax just toward the end of a cruise for maybe, you know, three to four or five hours. But most of the ones I've been on have been pretty intense.

TAYLOR: Now one of the problems that you run into if you're going to pick up the vessel on a certain leg of a cruise, that might be over in Hawaii or something like this. Now that means a certain amount of preparation you have to go through beforehand if your instruments are going to be . . .

FARRINGTON: Sure.

TAYLOR: . . . where they're supposed to be. How does all that work out? Or how did you work all that out?

FARRINGTON: Well, you learn from various people and from the experience of folks that you have, and fortunately here in an institution like this we have a lot of experienced people you can <u>learn</u> from. You learn about becoming a logistics expert. You have a number of people who are very good at helping you out, and you have a great shipping and receiving department. They're used to shipping things to all corners of the . . . of the earth and figuring out what you can do with them. We have technical <u>staff</u> people who are experienced at both going to sea and analyzing things in the laboratory, and things just wouldn't get <u>done</u> without those technical staff people, departmental assistants who do that. And they also teach the graduate students and the postdocs along the way how that's done, and so we would typically put in for a cruise request—it could be two years before you're actually going to go. You submit a proposal to do the work, and as part of that you also have a form that goes in for <u>ship</u> time. In the <u>early</u> days when I was here at the Institution, in the <u>early</u> '70s, the Institution <u>itself</u> was scheduling its <u>own</u> ships, and then in 1974—I don't know the <u>exact</u> date—'75, we changed over to the University National Oceanographic Laboratory System, so in the early days you used to have a big meeting in Redfield Auditorium, and everybody'd go there and say, "Well," you know, "I have funding, and I want to go and take the ship here and there," and there'd be a lot of debating back and forth about that. And uh, in fact, that's how uh I got to be chief scientist on one of the major cruises and how we'd work things out. Both Bob Gagosian and I would go together. He was an organic chemist who came as an assistant scientist about three-quarters of the way through my <u>post</u>doc, and so we did a lot of work together over the years, and he would have funding from NSF, and I would have funding from ONR, and we'd say, "OK, we've got these X number of ship days together, and we want to go to . . . ." One case was the coast of Namibia and sampling the Benguela current area, because it was very organic rich sediments, a lot of productivity in the <u>water</u> column. I wanted to look at the sediments. Bob wanted to look at the water column. So we would load . . . . We knew the *Atlantis II* was going into the Indian Ocean. A lot of people wanted to go there, and so we said <u>we'd</u> like to have the ship—you know, put our hand up, at this point in time, between Cape Town and Cape Town or some appropriate cruise, and Geoff Thompson wanted to collect some rocks at the Midatlantic Ridge along with his graduate student at the time, Suisan Humphris, and so they had the ship from Recife, Brazil, across the South Atlantic, into Cape Town. So we all loaded all our equipment onboard, pretty much all of it, and when we got on the ship in Cape Town, Geoff got off, and I got on as chief scientist, and uh . . . and then we went on our cruise and uh it was interesting, because there was a little bit of a diplomatic problem. The uh Territory of Southwest Africa, as the South Africans refer to it, was referred to as the emerging nation in Namibia by the Organization of African States, but it hadn't quite yet been recognized as such by the United States government. On the other hand, they didn't want to cause problems, so nobody in our State Department would officially ask for us to have permission to go on this cruise, and so Art Maxwell, who was the provost of the Institution at the time, director of research and so forth, sent a telegram to a US citizen who happened to be on an exchange visit from the US Geological Survey to the South African Geological Survey asking him how, you know, "So how do you think this is going to be received?" And we had a telegram back that said, "Well, I've checked around and I think it's OK. No problem." So that was the only permission the captain and I had

TAYLOR: [Laughs.]

FARRINGTON: . . . for the cruise [laughs] in and out of Cape Town, and then when we got back into Cape Town we had to unload everything off the ship and get it shipped back, and that caused a little bit of difficulty, because prices had gone up in the interim from when we put our grant together, and so I had only a few thousand dollars from Geoff Thompson promised, and Bob Gagosian and I had to get a total of 6,000 between us, and our total bill was going to be something like $18,000, and so there I was, you know, halfway around the world, and we've got to get the stuff off the ship, and we can't leave it on the dock, or else we'd get charged, and we were double berthed, because it was a congested port, and all sorts of things were going wrong, in the middle of which, this very elegantly dressed ship's agent came up and, in his best British accent, asked me about the invoice, which had included something like 35 cans of basalt, but they were listed as cans of "rocks." He couldn't understand why I wanted to ship cans of rocks.

But that was essential to Geoff Thompson and Suisan Humphris's thesis. So we shipped the lot of stuff back and then came back, and we had some help from the Institution in picking up the extra costs. But those are the kinds of decisions you have to make when you're out in the field.

TAYLOR: You bring up a whole bunch of interesting thing here, that someone that's never had any contact with a field like oceanography, which is significantly different than being in a lab scientific . . .

FARRINGTON: Right.

TAYLOR: . . . situation runs into, and you just mentioned a bunch of them. You run into things like what the Africans call "dash," which is bribery. "I need to get X amount to get your stuff through my port onto your ship." You run into all kinds of political problems, like you're talking about.

FARRINGTON: Right.

TAYLOR: It's really difficult.

FARRINGTON: I've never paid a bribe in my entire career. I have paid what are called "nonbaggage revision fees" in Peru, and I have a receipt for them. [They laugh.]

[END OF SIDE 1, TAPE 3]

TAYLOR: Before we had to switch tapes you were talking about some of the things you run into as you try to get an expedition started, completed, and get everything back to where . . .

FARRINGTON: Um-hum.

TAYLOR: . . . it's got to be. You also mentioned the term "chief scientist." What exactly is the difference between a chief scientist and a principal investigator?

FARRINGTON: OK. A principal investigator is a person who has a specific <u>grant</u> and an idea that's been submitted, and they are the principal investigator on that particular proposal. They could have any number of co-PIs (co-principal investigators) or other scientists involved. But <u>they're</u> the ones who will take principal responsibility for carrying out the science and research associated with that, <u>including</u> the responsibility for the expenditure of the funds and how the whole project is carried out, as has been outlined in the proposal. They'll <u>also</u> be the ones who make the decisions of, "Gee, that didn't go the way we thought it was going to be, so we're going to change direction

here." The chief scientist is a person who is responsible for the overall scientific program on a particular cruise, and there could be as many as half a dozen or more principal investigators involved in cruise. One of the is the chief scientist, and the difference is that the chief scientist is given the responsibility of pulling together all the different types of projects that are going to be carried out on that cruise and working with the captain on the scheduling of where the ship's going to be, assigning the appropriate people to interact with the boatswain about handling equipment on the deck and getting things over the side and getting them back, and scheduling all of the time, so that you achieve as many of the objectives of the principal investigators who have been assigned to that cruise. So typically a major person would . . . . I've mentioned before, let's say Bob Gagosian and I would put together a plan to go, and in the first two cruises we went on I was the chief scientist because I was the one who'd had formal training in oceanography, and then by the time we got through the second one, he's now at the point of understanding what needs to be done, and so then we switched off after that, but in that cruise off of South Africa, for example, there were a couple of other people onboard—Mary Scranton, who had an interest in studying methane. And her advisor Peter Brewer couldn't go on the cruise, but she was studying methane in the water column. They had an NSF grant. They had asked for some ship time, and we fit that in. Stan Watson, who was a senior scientist in the Biology Department, wanted to study the microbiology of that particular area, and he had assigned to his grant two days of NSF ship time. So typically what would happen is you'd have, you know, 10 days of ONR time. Bob would have eight days of NSF time. Stan had two days of NSF time, and Mary could get one day of NSF time, so you're stringing that all together. And then it's up to the chief scientist to figure out how to use that, and typically the federal agencies would say, "All right, you've each requested all these number of days," and they'll give you 2/3 of the total number of days, because they figure you can, you know, use the ship 24 hours a day. You'll figure out what's going on. And in fact you do use the ship 24 hours a day.
TAYLOR: And I would imagine—and please correct me if I'm wrong on this—that you'd almost have to guarantee some of those principal investigators so many specimen stops or something like that.

FARRINGTON: It's called consensus building. You build a consensus. You all get together, and you say, "OK, this is what we want to accomplish." And typically, of course, we would like to accomplish much more than we have the available ship time to accomplish, so we set priorities, and then there's always the plan that you have to have in place in case you run into problems. Something breaks. The weather doesn't cooperate. Maybe a lot of different things happen. For example, on that cruise, we were asked if we could have another two days of time in which we would run a final geophysical transect for gravity measurements for the Geology and Geophysics Department, because Ken Emery had been there on a previous cruise in 1974, on the *Chain*, and he and Carl Bowin had collected some geophysical data perpendicular to the coast. But it turned out they needed another line parallel to the coast, and so we agreed that we would do that, so one of the times when we did nothing but steam and collect all of that data as sort of a break in between, midway through the cruise, we went as far as we could go towards the border of Angola. And then one night we got into a lot of fishing vessels that were speaking Spanish, and then an occasional Russian vessel, and then our radar was jammed, and it turned out that we were in the midst of . . . . You know, we were getting too close to the border. This was in the Angolan revolution, war, whatever you want to call it. There was a lot of speculation about Cuban mercenaries and Russians being involved, and so the captain said, you know . . . . And remember, this is when we had only the telegram. [They laugh.] There's our permission. He said, "Well, how much longer do you think we need to go?" And I said, "Well, I think this is far enough," you know. [They laugh.] And we were almost to where they'd asked us to go. So we turned around.

TAYLOR: You know, . . . .

FARRINGTON: And if I might add on, that cruise was very special, because we also—after we turned around—we were at sea on Christmas and New Year's, which was somewhat unusual, so we actually celebrated Christmas and New Year's at sea. And that was a little bit of a [thumps] downer for the crew, as well as for the scientists, but we got through it.

TAYLOR: I asked Dr. Backus. He mentioned being a chief scientist, and I said, "Well, is there anything you wished the Institution did that'd kind of help you out in this?" He said, "Well, you know, I always wished that they had had a course in how you be a chief scientist."

FARRINGTON: Yes.

TAYLOR: Did you find it difficult to get yourself into that and kind of CEO, if you will, this whole cruise?

FARRINGTON: No, because I'd watched uh . . . . I'd gone on a number of cruises before that, and I'd watched how people had done it. You keep your eye on it. But we <u>have</u>, since that time, actually occasionally have these little seminars, which we call, "So You Want to Be a Chief Scientist?" And we would have the graduate students sit in on these seminars. We're due to have one soon. And what we do is we get a captain together, a boatswain if we can. You know, we do it when somebody's around in port—one of the departmental administrators, people from the Port Office, and a couple of experienced chief scientists, and we go through what are the different roles and what are the things you have to worry about, what are the things that other people worry about. I mean I had a great deal of help as chief scientist from Jerry <u>Cotter</u>, who was the boatswain on the *Knorr* in <u>my</u> first cruise, but I'd gotten to <u>know</u> Jerry, as a scientist working on the ship several cruises before that. And, you know, <u>they</u> . . . . The great thing about our officers and crew on our research vessels, and I think to some extent on others as well, is that they're there to help the scientists get things done, and so <u>they</u> help out. But, you know, every once in awhile you come up on a rather arrogant scientist who thinks that a PhD degree has conferred some greater knowledge on them than some of the other people, and, you know, so what happens then is that they . . . theyi usually let the person make a little mistake so that they're, you know, that they're injected with a certain amount of humility, and <u>then</u> they'll help out.

TAYLOR: Everyone that I've talked to about their experiences at sea, as a scientist: certain names keep coming up. Jerry Cotter is one that keeps . . .

FARRINGTON: Umm.

TAYLOR: . . . coming up. Hovey Clifford's name comes up a lot.

FARRINGTON: Yes.

TAYLOR: Captains: Paul Howland, Mike Palmieri, people like that.

FARRINGTON: Yeah.

TAYLOR: Then I talk to Mike and Paul, and they will tell me that they always stayed here at the Institution at a lower salary than they could have had in the regular commercial, because it was so much more interesting what was going on between Point A and Point B.

FARRINGTON: Sure.

TAYLOR: So this really is an enormously cooperative venture, isn't it?

FARRINGTON: It is, and . . . and uh I learned early on . . . . And I honestly don't know where I got it from, but I learned and practiced quite uh, you know, <u>every</u> single day that I was chief scientist is that I'd prepare a little schedule, and you <u>want</u> to do that, anyway, but you want to get the schedule ahead of time. And I would prepare a small paragraph, if I could, on <u>why</u> we were doing certain things, and when we were doing them. And one thing that I learned, by <u>experience</u>, was that it wasn't only the bridge and the boatswain [thumps] that you let know [thumps] about this, you know, ahead of time. You certainly let 'em know about the whole question of the schedule ahead of the cruise, but you also wrote a little thing about why were we doing it, under what circumstances, what were the objectives—and that sort of thing. The other place that you needed to give that to was to the engine room, and to the engineers, because when things break on the ship, [laughs] OK, you find a lot of innovation comes from the engineers on the ship, and they can fabricate pieces and things that might help you out. Also, uh, especially on the *Atlantis II*, but on the other ships, too. You want them to have a heads up when they're going to have to get the engines really up and online, when the bridge is going to call for more power. In <u>those</u> days it was much more of a deal to get, you know, a couple of boilers hooked up and running when you had a steam plant, on the *Atlantis II*, for example. You couldn't just uh [thumps] yank in full power without some <u>notice</u>, and so they appreciated very much that type of information. [Thumps.] And in return you got a lot of information, too, uh about, you know, different tricks of things you could use on the ship to get your gear to work, to tie things down better, that sort of thing.

TAYLOR: I have been continually amazed at how you stick this little island out there—this ship—and all these very disparate backgrounds somehow come together . . .

FARRINGTON: Right.

TAYLOR: . . . and produce wonderful stuff, and how everybody at all the different levels now, but I listened to a couple of ship's captains telling about some of the geology they learned and some of the biology they learned, and as a scientist, an organic geochemist, well, I guess you can't do that in this field without understanding some of the biology, some of the physics, some of the other kinds of things that go on.

FARRINGTON: Oh, no, you have to do all those things. We used to go to sea uh with . . . . Of course, today we can go with the information loaded on a computer, but we used to go to sea with a lot of books too, and charts and things, and atlases to help us out in terms of, OK, supposing we ran into something that we didn't expect, you know. Then how do we refocus what we're doing? You mentioned captains, and you went through a few names of people, and uh one of the senior captains that I admired very much and was very helpful to me was Emerson Hiller, and uh Emerson and I used to have great discussions, 'cause it turned out that . . . that my uncle, who I mentioned earlier I had grown up with, actually bought a piece of property for his summer house in Crescent Beach in Mattapoisett from Emerson's uncle, who owned the property there, so it turned out on the first cruise, when I was talking with Emerson, that he came from the Fairhaven-[thumps] New Bedford-Mattapoisett area and so did I. And so we . . . immediately I gained a little bit of an in talking with Emerson, because we'd had some background, but he was always pushing hard and making sure that the [thumps] scientists got things done that they needed to get done. But one of his favorite sayings, and the second day of the cruise was he would come up to you and say, "Well, things are going pretty well. So we'll get in a day early, it looks like," you know. [Thumps.] 'Cause the captain was always wanting—and . . . and correctly so—to give as much relief from the time at sea as he could to the crew, and give them an opportunity to get things ready and get things sorted out, but when you had bad weather then, you know, then he would help out, and then all of a sudden you'd find that you weren't cruising at 10 knots any more, you were cruising at 12 knots, and they were using more fuel, obviously, but they had permission to do that if they ran into difficulties.

TAYLOR: He was the first captain that I did an oral history with, and by the time we finished, [laughs] I really . . . . He'd roll over in his grave if he heard me say this, but I really felt he was somewhat of a philosopher in many ways.

FARRINGTON: Sure.

TAYLOR: I remember I said to him, after listening to him describe what went on on a ship, I'm sitting there thinking, "You really have to know everything that occurs on this ship, don't you?"

FARRINGTON: Um-hum.

TAYLOR: He just got this kind of little grin on his face and leaned toward me and said, "No. You just have to be perceived as knowing everything that goes . . . "

FARRINGTON: Right.

TAYLOR: ". . . on aboard the ship." And I've tried to live by that ever since.

FARRINGTON: Yes.

TAYLOR: You mentioned also the fact of the captain trying to get some relief for people. You had an opportunity to go into some ports.

FARRINGTON: Um-hum.

TAYLOR: Can you talk a little bit about maybe some of your favorites and some of the kinds of things that you might have done there?

FARRINGTON: Well, I don't think I have a favorite port. Every one of them was an interesting excursion. I mentioned South Africa and Cape Town, and that certainly was an interesting experience. I wouldn't say it was "favorite." It was a beautiful area, and we went to visit in Stellenbosch, and the consulate people, the naval officer who was assigned to the consulate there, had a barbecue for us—what they call a "brai." And of course they have wonderful wines which none of us had ever been exposed to, because South Africa was on the "no-no" list in those days. But there was a terrible part of that that I found excruciatingly difficult, and that was it was . . . . Apartheid was . . . was still in full bloom there, and on one occasion we were double berthed, and I stepped on the gangplank to go to the fisheries vessel, and it happened that the people who were reprovisioning our ship—the *Atlantis II*—stepped onto the gangplank of the fisheries vessel the same . . . , you know, carrying heavy loads, and so I stepped off, as I think most of us would do normally, and said, you know, "Come on through." Well, it turned that they were black, and they got beaten because they'd stepped on . . . . I mean literally, uh, sort of hit with a swagger-stick type thing by the overseer, and I got into a tremendous debate with the uh ship's agent about this, and he finally said to me, "You're going to understand, Dr. Farrington, that this is South Africa, and our rules apply." [Slapping sound as of hands hitting clothing.] And he said, "I personally don't agree with them, but if you want to get . . . . If the captain wants to get this ship provisioned, and if you want to be able to get through the port, then you'll have to take care of this[?]." The other problem was that uh it was not unusual . . . . We had uh—I can't remember—something like a half dozen woman scientists onboard. Unheard of in South Africa! We didn't even allow them in the port areas after dark, for the obvious reasons that anybody who was coming into the port area who might be a woman would be a woman of ill repute. This created a tremendous hassle of getting these, many of whom were graduate students, in and out of the port area. So it was my favorite in terms of . . . of the challenges we faced, but in terms of not wanting to go through it again, I think it's probably pretty far down on my list. I always liked Bermuda. I liked Peru and going into Callao, although the last time we were there, that was quite a challenge in '87 because the Sendero Luminoso decided that they were going to be a little bit more active, and to make a long story short we got out of our hotel in downtown Lima, and the next week they blew up the bottom floor [laughs] of the hotel.

TAYLOR: You know, when I give tours I always say this is the "Indiana Jones" of the sciences, and it really is in many ways.

FARRINGTON: Well, we tell . . . . I mean one of the things about being an oceanographer is that you can, you know, you can tell stories too. I mean there are . . . . As one of my chemist friends said, you know, "There are very few chemists who can talk about," as you mentioned earlier, "doing a titration while holding onto their fingernails in the middle of a hurricane."

TAYLOR: That's right.

FARRINGTON: Or holding on by their fingernails. Uh and people relie . . . . You know, like to . . . . There . . . there are some aspects of the adventure part.

TAYLOR: Is that one of the reasons you went into oceanography, the fact that there was adventure to be had here?

FARRINGTON: No, I went into it initially because I thought it was an interesting area of science, which, you know, with a lot of interesting things to do, and then later on I was

<u>glad</u> I did, because you get to not only go onto cruises, but to conferences and stuff. You get to travel a lot internationally.

TAYLOR: So those are all good bonuses, then, for you.

FARRINGTON: Oh, absolutely, absolutely.

TAYLOR: All good things have to come to an end. You come back from the voyage. You get all of these other kinds of stuff taken care of, and then, if you're going to continue to stay with the Institution, and if you're going to continue to get funding, you're going to have to publish something.

FARRINGTON: Yes.

TAYLOR: And I'd like you to talk some about that. I looked at your grouping of papers that you'd published over the years, and I just looked at some of the names, and just let me read a few: John Steele, John Knauss, Ken Brink, Walter Munk, Marcia McNutt, Bob Ballard, Fred Grassle, Bob Gagosian—just to name a few. And . . .

FARRINGTON: Um-hum.

TAYLOR: . . . if someone were looking at the history of oceanography, I'm sure all of those names would be mentioned somewhere . . .

FARRINGTON: Um-hum.

TAYLOR: . . . in the book, and more than just a footnote. When you go about the whole process of publishing and working with someone, now how do you set that all up? I mean you have a first author and then you've got others, and so forth. How does all that happen?

FARRINGTON: It happens in a number of different ways. I mean if you're the principal investigator on a grant, and you've had the original idea, and then you go forward with . . . with <u>pursuing</u> that, then normally you would be the person who would be the first author. The exception to that is when you get an original . . . . You know, you get in a grant, and you say, "Well, part of this is going to support a postdoc," or part of it's going to be to support a graduate student. If it's a graduate student's research, or the postdoc's research, at least the way <u>I've</u> approached it, they are uh . . . . <u>They're</u> the ones who've done the major amount of the work. You try to make sure that they're the ones, or in fact you <u>do</u> make sure that they're the ones who've done the major part of the interpretation, and then you <u>help</u> them with that interpretation and the writing based on your experience, and so then you would become a co-author. Uh they would become the principal author or first author. This practice varies from laboratory to laboratory, and the key part is to make sure everybody involved understands up front what's going to happen. Now, occasionally [??] unusual circumstances, where you discover something along the way, and then you . . . . You start thinking about, "Well, how does this all play out?" And I'll give you one example to that. Bob, as I mentioned, Bob Gagosian and I were collaborating in this cruise off of Namibia. We had obtained some mud. And when I went to analyze this mud, it turned out that uh we had not found a lot of the chemicals that I was happening to look for, but in the process of doing so we'd done some chemical manipulations and analysis, and then in a moment of . . . of uh serendipity in which I made a mistake in terms of entering on the computer on the GC mass spec. the wrong file number in asking it do a certain type of interpretation of data, the data profiler came up, led me to say, "How the heck did that happen?" And then backtracked from that and said, "Well, there had to be certain reaction products from sterols in these sediments that no one had ever found before in these surface sediments." Now Bob was very interested in sterols and steroids, was a, you know, an absolute crackerjack organic chemist and really understood this stuff, and . . . and so I went tearing upstairs, after checking this out with one of our postdocs at the time, Cindy Lee, and said, "I thought I saw this kind of mass spectra . . . ." (This was when we were still in the Redfield Building.) "I thought I saw this in one of the publications we were looking at the other day," and she said, "Oh, this might be the one here." And we looked at it, and I showed it to her and said, "Yeah, this is a compound known as cholestadiene," which is from cholesterol. And I thought, "Holy mackerel!" So I went running up the stairs to the third floor, where Bob and I had our labs next to each other, and I said, "Look at this!" And he says, "Oh, yeah, that's cholestadiene," and I said, "Well, it's in the surface, the upper surface mud from off Namibia." And he said, "Nah, can't be!" And then from that point on we worked part of the . . . . I would say almost day and night for about a couple of weeks, and since he knew about this stuff, and I was busy writing up another paper, I said, "You take the," you know, "We agreed you would take the lead on it," and we published the paper, Gagosian and Farrington. That was our first paper together, uh and it really . . . . And I'm appearing not to be humble enough about this, but I am. I mean it was an exciting time, and it really was a surprise. We found out from some colleagues in Europe later on, that uh . . . who thought they had this of science sewn up, "Who are these guys—Gagosian and Farrington? Where did they come from? How'd they get involved in this kind of research?" Uh and it was serendipity, but we were prepared. And so it was one of these things where, you know, the old saying about chance favors the prepared mind. This was a classic example of that.

TAYLOR: Very much like you make your own luck kind of thing.

FARRINGTON: Right. That's how we decided in that particular case that could have gone either way. And that's how, you know, you decide on the senior authorship on that basis, and then later on I'd write a paper. He'd write a paper, and one of the postdocs, Cindy Lee, who was a colleague of ours, and Stuart Wakeham, who was a junior colleague of ours—they all would take the lead in writing papers.

TAYLOR: Was that a difficult process for you—writing?

FARRINGTON: Writing?

TAYLOR: Yeah.

FARRINGTON: No, it was exciting. Uh I like to write, although I can tell you, you know, that I have to have a good editor or somebody look over what I write, because I tend to uh go into Germanic prose, which means it goes on forever and ever, you know, a sentence with commas and parentheses and things, and Bruce Tripp, my colleague of many years, who worked with me in the lab, would do a good job with a red pen and help me fix it up.

TAYLOR: Well, even the most known authors of even general publications for the public [laughs] have someone go over all their material . . .

FARRINGTON: Right.

TAYLOR: . . . for just those reasons—the spelling and everything else that goes with that. One thing I was curious about that you mentioned, and I'd like you to talk about your philosophy about it. Over the course of the years you talk about having uh a predoc or a postdoc or someone . . .

FARRINGTON: Right.

TAYLOR: . . . like this that you had a sense of mentoring. How do you go about doing that? They're still a student. You still know much more than they do. How do you work with them? What's your philosophy?

FARRINGTON: Well, we've had . . . we've had undergraduates, summer-student fellows in our lab or summer employees, and those are people that you're introducing to research. But they've already been introduced to science. We normally get them when they're in their junior year. And so, you know, you bring them along. You say, here's some exciting stuff to do. And you give them a project, and the key to the project is it's a doable project. You want something that they can do in the period of time that they're there, and so they can see it from beginning to end, including getting to write a report on their own. It may not be—and oftentimes is not—a publishable paper yet, and the project may not be completed yet, but they have contributed significantly to it. And the key is to talk with them about the idea, but also introduce them to the people in the lab, you know, the people like Bruce Tripp and Hovey Clifford and others who know a lot about the day-to-day things of the science, too, and they contribute to that, because if you're a PI you've got a lot of other things going on that you have to take care of—writing the proposal, writing the progress report, going to the meetings, figuring out where you're going to get the money from the next time, and so forth, so you need to have . . . . In the ideal situation you need to have long-term coworkers, and I was extremely fortunate in having people like Bruce Tripp, and then, later on in my career, Hovey Clifford and Alan[SP?] Davis, who was with me for many, many, many years and ran all the analytical chemistry, and the students learn as much from them as they do from the "formal advisor." And so that's how it's done, and then you mentor the student about writing and presentations and . . . and . . . . There was one student we had who didn't get it, in terms of, you know, you've got 20 minutes and that's it. And so we actually had a practice session after the . . . after the exam that we gave him originally, said, well, you've got to figure out how to give a presentation and get it done and get the message across. And the first time we had the practice session, there were three of us in the room, three of the scientific staff, the faculty, and the student came in, and we said they had 20 minutes, and they were a third of the way through, and the 20 minutes was up, and . . . and as terrible as it may sound, we just all got up and walked out. But we drove the lesson home. OK. And then some students have a great deal of trouble in getting . . . doing things, writing. I have a great deal of trouble sometimes, uh sort of getting going. I always wanted to have a little bit more data, finish a little bit more. I didn't want to publish the so-called least publishable unit. And I also had a terrible propensity from time . . . you know, of being interested in too many things, and so all of a sudden getting myself overloaded with projects and . . . and uh my junior colleague of years ago, Stuart Wakeham, who was an assistant scientist in the lab: we used to have these wonderful discussions. I'd come in and say, "Hey, we need to do this, and this," and so forth [thumps], and "Let's go on this," and then he said, "Well, now, this is project #269, and now I'm on project #15, so where do you want to put this in the priority list?"

TAYLOR: I love that approach, because this field, of oceanography, was really built on that approach, this all-encompassing interest. I mean I look over some of the things that K. O. Emery, who was getting towards the end of his career when you first came here, . . .

FARRINGTON: Correct.

TAYLOR: . . . and I mean my heavens he had things on archeology. The range was just enormous. And then by your time it got a little bit more focused, I think.

FARRINGTON: It did, but, you know, there are scientists and there are scientists, OK? And so when you talk about people like Hank Stommel or Ken Emery or Holger Jannasch—those people. I mean those are real giants in the field, you know, Ruth Turner, and people like that. I mean they're just, you know, and then there are other scientists, like me, and we make, you know, some contributions, and so on, but it's not at the same level. You fill in the gaps, and you make some contributions, but, you know, there are those others who really move things forward.

TAYLOR: But yet it seems as though you're talking almost a lot of commonalities in there. One of the things I would compare, let's say, you and Hank Stommel with is he always had all these ideas. You just told me that you came in, "Well, that'll be 290," as another thing that you'd like to look at. Isn't that the same kind of thing?

FARRINGTON: Yeah, no, I mean I have to insist. You're comparing, you know, the mountain to the molehill here, OK? I mean Hank was a one-of-a-kind person, and we're talking about fundamental insights to things, and then lots of other ideas. You can have a lot of ideas. I mean several of our colleagues: one colleague I used to like a lot, was a lot of fun—Egon Degens, who was in our department. He was a senior scientist when I came in, and we used to joke that Egon . . . .

[END OF TAPE 3]

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WOODS HOLE OCEANOGRAPHIC INSTITUTION

ORAL HISTORY OF JOHN FARRINGTON

Interview by Frank Taylor, April 20, 2005

Tape 4 of 8 tapes transcribed by Arel Lucas, November 2005

TAYLOR: . . . switched tapes I asked you about uh former Woods Hole . . .
FARRINGTON: Right.
TAYLOR: . . . people and you essentially brought up the fact that in the field of science, like in the field . . .
FARRINGTON: Hm.
TAYLOR: . . . of sports, there are some real stars.
FARRINGTON: Of course.
TAYLOR: And then there's a lot of people that I would have to still classify as major leaguers. 'Cause I think if you're a scientist here you're a major leaguer.
FARRINGTON: Well, I think, to some extent that's true. There are major leaguers elsewhere, too.
TAYLOR: True.
FARRINGTON: But this . . . you know, in ocean sciences this would have to be . . . and ocean engineering, this would have to be looked at, without a doubt, as being the major league. It's the difference between--you know, I don't want to contrast Red Sox, but—

it's the difference between being Ted Williams and, you know, Rico Petrocelli or something like that. And even that doesn't work well in science because there are people who have tremendous influence based on their own ideas and their brilliance, and then there are other people who sort of bring folks together and synthesize things, and so, for example I would say that Hank Stommel, without a doubt is probably one of the more brilliant oceanographers of, you know, the last two centuries, OK. In my field of organic geochemistry, if you think about the petroleum geochemistry part of it, we have to look at John Hunt, and John Hunt in my mind is . . . . You know, he had a lot of very good ideas of his own, but he was also the great synthesizer. He would take ideas from different people and results and put them together in a whole that he published in his book on petroleum geochemistry that, you know, is one of the classics, and will continue to be one of the classics. And he used to go around [thumps] to the oil industry people. They'd pay him after he retired, even maybe give him an honorarium before he retired, to give classes, because he synthesized this information. I would say Ken Emery is a similar type of person who had a grand vision of continental-shelf geology and geophysics, and he would synthesize things and bring them together. Uh and Holger Jannasch was pretty much, you know, working on his own, more or less, looking at deep-ocean microbiology, but always with the fundamentals of microbiology, always the fundamental microbiologist, always back to the fundamentals of that science and how did it apply to the ocean [thumps], always interpreting the results he saw in that way.

TAYLOR: OK, wonderful description. One of the things that I think that you were involved with that was pretty significant was the mussel watch.

FARRINGTON: Ah, yeah.

TAYLOR: Now I saw names like Dr. Edward Goldberg, Dr. Eric Schneider, Dr. John Farrington, Dr. Robert Riesboro[SP?] and so, . . .

FARRINGTON: Right.

TAYLOR: . . . were involved. How did all of it come about, and how did you get involved in it?

FARRINGTON: Well, this is one of these things where people who are going to be looking at this tape are going to say, "It couldn't have happened that way." But . . . and . . . and when I tell some of the students today how it happened, they say, "Oh," you know, "that was a crazy time." And it was. At the time of uh 1974 there was a bunch of money that became available as part of environmental impacts on energy-related projects. And Eric Schneider, who was the director of the labaoratory for EPA at Narragansett, was a real rainmaker, and he had a lot of interesting ideas, and so he went to Washington, and he surrounded some of this money and said it had to be related to environmental-quality things. In the meantime, Ed Goldberg, who was at Scripps, and . . . and, you know, by some measure of some people you talk to, a real cantankerous scientist, but, in my own career, was one of my senior mentors for years and years and years, as an external [thumps] person. He had gathered a group of people together and said, you know, "We really need to take a concept" that had been developed, actually by a fellow named Philip[SP?] Butler earlier to take a look at the southeast coast of the United States and measure pesticides, and say, "We need to use bivalves." In this case we picked mussels as a sentinel organism, and he had a little meeting that was organized in Washington, DC, under the aegis of the uh Environmental Studies Board, at the time, of the National Research Council, and he invited a few people he knew over the years—you, know Bob Riceboro[SP?] and I had been working together in the International Decade of Ocean Exploration on the analysis of contaminants in the ocean, along with George Harvey. And then there was uh C. S. Guyan[SP?] from Texas A & M University, Pat Parker. Vaughan Bowen and Ed Goldberg had interacted over the years in radionuclide analysis. And the question was we all talk about environmental quality in the coastal zone, but when you go to talk to people on the Congressional staff about contamination, they say, well, you measured something in the crab here, you measure something else in the fish there, and how do you know . . . ? You know, what is the real sense of how can we make a comparison over the entire coastline of the United States, on the status and trends of the contamination? And I mention the status and trends, 'cause that's going to come up at the end of this little brief history. So we're in a meeting in Washington, and we came away from that after this one-day discussion, and Ed said, "Well, write me down how much you think it would take to analyze X number of, you know, 50 or 100 bivalves, each of you in your laboratories, if you did this." And so we wrote this little thing down, and I sent it off to Ed, and then we went to sea. In February of '75 I went to sea. Came back, and I had a phone call: "Call Ed Goldberg." OK. This was just a letter I put in.

OK. He says, “Well, we’re funded.” [They laugh.] I said, “What are you talking about, Ed?” He said, “Well, we’re funded. I put together all the different letters; wrote a little proposal; and I sent it in.” And he’d found Eric Schneider, who was sitting on all this money that he had corralled for environmental quality research, part of which went to the Marine Ecosystems Research Lab at URI, at the Oceanography School that was next door, that I got involved with as well, and the other part went to mussel watch. And so I said, “For cryin’ out loud, Ed, I only proposed what I thought we could do. We can’t necessarily do all this stuff.” And so we actually had to hustle, and uh Nelson Frew, who was one of our senior technical staff members who was working on the GC mass. spec. at the time, uh and I worked together very carefully on putting together a . . . a quantitative way of measuring polycyclic aromatic hydrocarbons in tissue of mussel. And that was the first time that that kind of measurement had been made on that scale. And meanwhile Ed’s lab and Vaughan Bowen’s lab were analyzing the uh radionuclides. I left out a very important person that I shouldn’t forget—John Martin, who was the director of the Moss Landing Marine Lab and did trace metal analysis and later became uh famous and almost infamous in the public press for proposing iron fertilization of the ocean possibilities, but John and Bob Reisboro[SP?] and Ed Goldberg and Vaughan Bowen and I had worked, and George Harvey, in IDOE, and so as the next sort of step we knew coastal waters were contaminated compared to the open ocean, but how much, and how intense? And so we did that. And it was a prototype. We did it for three years, and then we put the data together, and then uh I made a presentation to the group of PIs out at Moss Landing Marine Lab in which we had talked about how to put together the data, and I said, look at these different interrelationships. And what came out of it was, OK, I was the one who put this thing up, and so all of a sudden Goldberg and Bowen were saying, “Well, you write the paper.” [They laugh.] And I’m saying, “Oh, my word,” you know, “trying to get these . . . .” I mean they were notoriously at odds when they came to details of writing papers, OK. And so I thought, you know, I’ve been handed a real hot potato, which, indeed it was, but we managed to get through it. That program then went into a stasis, in which EPA didn’t want to fund it any more. The . . . .

TAYLOR: That was 1978, wasn’t it? You ran out of money or something at that . . . ?

FARRINGTON: ’78, but we still had a little bit of money coming in, but it was for more focused research efforts, ‘cause we always argued that you could not run a monitoring program and be able to interpret the data disconnected from ongoing research. In other words, it would be a mistake to just set up a bunch of routine measurements and then not continuously put it together with what’s going on and understanding the transport processes. One of the papers that we wrote, there’s a clear example of that, and it was that the data got loose [laughs], if I could use that term. The tables of data on radionuclides on the West Coast showed a slight bump in plutonium and cesium, but mostly plutonium. It happened to be near—not too far away from—the Farallon Islands. Somebody in the Congressional staff looked at that and said, “Oh, this is due to the dumping of radioactive waste at the Farallon Islands dump site,” which they were dumping low-level radioactive waste. And this was brought up at a hearing that a Congress subcommittee held out there. In fact, John Martin plotted data on trace metals which were biologically mobilized--cadmium, a case in point, versus plutonium—and showed that what was happening, actually, was upwelling of water at about 900 meters in the Pacific, which was enriched relative to the other waters because of the nuclear weapons test fallout from the ‘60s, and it just happened to be where . . . . You know, we knew from other data, from research that was going on in Goldberg’s lab and Bowen’s lab, that this plutonium maximum was at that depth, and so the plutonium maximum in the mussels had nothing to do with the disposal of radioactive waste in the ocean but rather to do with what the natural processes were ongoing, focusing for a time the plutonium from the fallout at that particular water depth in the ocean. And so you disconnect . . . . If you disconnect understanding of processes from raw monitoring data you get . . . . you could get into serious difficulty.

TAYLOR: Yeah, and yet you’ve got I don’t know how many thousands of miles of coastline that could have multiples of different kinds of processes going on, . . .

FARRINGTON: Sure.

TAYLOR: . . . and that’s one of the things I wanted to ask. How did this whole mussel watch, how were you able to get it all operating on the same basis? I look at, oh, for example, some kind of analytical machine. How could you make sure it was calibrated the same on the East Coast as it was on the West Coast?

FARRINGTON: Yes, we did. We did a lot of intercomparison exercises. A good part of . . . . A significant part of my career was spent—especially in the early days—doing nothing more than verifying quality control, and quality assurance of analytic methods, 'cause if you don't get that right, then you can analyze until the cows come home, to use that old phrase, and your data is worthless. And so on the mussel watch we did everything we could to make sure we were intercompared, intercalibrated. We routinely analyzed the same set of samples uh periodically between two different laboratories, uh . . . .

TAYLOR: You had more than one kind of test sample, didn't you, on this?

FARRINGTON: We had mussels, and we had oysters. And they're a so-called cosmopolitan species.

TAYLOR: That was the reason for them being selected?

FARRINGTON: That's right. They were cosmopolitan species, so you didn't have a problem of comparing species so much one to the other. Although it turns out, and . . . and . . . we discovered this sort of midway through, we were looking at subspecies in some cases, OK. They are sedentary. In other words, they're . . . the mussels and the oysters pretty much are in one place, so they're fixed, so you don't have to worry about them migrating in and out of contaminated areas. They have minimal uh capacity—minimal capacity—they have a little, but minimal capacity to metabolize uh the organic chemical contaminants. So if you had DDT or PCBs or PAHs they'd be there, whereas if you analyzed fish liver, for example, a lot of that would be metabolized, and you wouldn't have the original exposure. The other aspects were that there were large enough populations of these in different places, so that our actual sampling of the population could go back to the same place year after year, and our own sampling would not be the biggest impact on that population. Many of them were rugged enough, and they were resistant enough to pollution that they could grow in . . . in some relatively highly contaminated areas, so you get a good gradient. And then there were other aspects of this. Obviously oysters were considered to be part of the delicacy of, so they were a food organism. So they were, in a sense, biological monitors. They were sentinel—what we call the sentinel—organism approach, OK. And . . . and we never claimed that that was the only thing that you needed to do. I mean when you found hotspots you needed to go back in and look at them, and we weren't looking for hotspots. We were looking for regions. We were not looking for a specific location, although we did use that data to <u>convince</u> a rather doubting state representative and mayor from New Bedford that the PCB contamination in New Bedford Harbor was probably one of the worst on the coast of the United States by simply <u>showing</u> the analysis of mussels from the hurricane barrier in New Bedford, nearby, with those of the entire coastline of the United States, including, you know, New York City, New York Harbor, Boston Harbor, San Pedro Harbor in Los Angeles, San Diego, you <u>name</u> it. It was pretty difficult to argue against it. I mean there were like <u>orders of magnitude</u> more PCBs in the mussels in New Bedford Harbor than there were in hardly any others.

TAYLOR: So then it was a successful program, then?

FARRINGTON: Successful prototype, and then later on uh NOAA decided that they would take over, and . . . and thinking about taking over, and theyi called it the Status and Trends Program, meaning the status of the coast at one time, but then the trends—both geographically and over time, and now it's been ongoing since 1985.

TAYLOR: OK, so it's become a standard kind of testing.

FARRINGTON: And it's also something that was used elsewhere in the world, too, and there was an <u>international</u> mussel watch program which Ed Goldberg and Bruce Tripp and I and several others tried to launch and get off the ground, but it only got through the first two prototype phases. One was in South America, and the other was in the uh Asian Pacific mussel watch, which is actually . . . we handed off to our colleague Shinsuke Tanabe in Japan, who ran a rather successful program there, but nothing on the international scale like what this Status and Trends Program has been in the United States, or a similar program in France. They have a longstanding program there, too. It wasn't something that was invented all by itself in the United States. There was a <u>lot</u> of international collaboration, but it <u>did</u> go back to a program in the '60s run by one scientist, uh Phil Butler, in an agency, that showed that the concept worked on a <u>regional</u> basis with pesticides, and so then we could <u>build</u> on that. Ed Goldberg proposed it an editorial piece in *Marine Pollution Bulletin*, and then made it <u>happen</u>.

TAYLOR: What is the status of the coast right now, pollutant wise.

FARRINGTON: Well, it's not surprising that in areas where you have a lot of people and a lot of industry, a lot of discharges you have high levels of contamination. What has been happening, and has showed up in the data, is that uh levels of DDT pretty clearly were dropping, and they were dropping when we were looking at the data, because there was a reduction in the use of DDT, PCBs in the late '60s—I'm sorry—DDT in the late '60s, early '70s. PCBs, because they've been phased out of open use and then new manufacture: they've also been dropping in concentrations. Uh the trace metals: there are a few places where there are some elevated concentrations of lead still that are associated with urban harbors. There are interestingly areas where you have elevated concentrations of arsenic in comparison to other areas of the coast, but that has got nothing to do with human activities very much. It has more to do with the natural geochemical processes in the type of soils that are feeding the coastal waters there. The polycyclic aromatic hydrocarbons is an interesting one, because there the concentrations have not gone down as much as for the other organic contaminants, and you'd say, "Why not?" Well, pretty easy. We're still using fossil fuels. We're still burning a lot of fossil fuels and producing a lot of polycyclic aromatic hydrocarbons, uh and that may be one reason. The other reason may be that there is just such a big load of them sitting in the mud, in the sediments, that that's leaking back into the water column, and that's where, you know, we have to do research. We have to figure out: so the mussel's got a highly relative contamination, or the oyster, but . . . but exactly where's it coming from? And how's it fit into the habitat, and what does that mean for the habitat, and so the National Status and Trends Program actually has hot-spot measurement programs that they've had ongoing, looking at biological effects as well.

TAYLOR: One of the things I've always wondered about: when you're very much involved in the kind of work that you're involved in, and you're showing hard data about . . .

FARRINGTON: Um-hum.

TAYLOR: . . . there's some real problems here, and it's not acted on readily by a governmental body or something like this. Is that part of your concern, or is there a level of frustration there, or anything like that? I mean scientists like yourself can show the data. That doesn't mean it's going to be acted on. It doesn't mean you're going to get the non-point-source oil coming off the highways into the waters, and that's still going to be there. Is that an issue?

FARRINGTON: Well, I think that we know that that's still happening to some extent, but I think also that . . . that if you're persistent and patient, then you can see some results. And uh . . . and certainly every time I go by a place where somebody has stenciled a storm drain that says, "Don't dispose of any oil," or something like that, then I think, "Gee, we've had some impact"—not just myself but colleagues. But, you know, a handful of us have had an impact in the research that we were doing. When I went to a science fair here locally, uh right after the Exxon *Valdez* oil spill, and I've been involved in a number of oil spills, and certainly they're spectacular in terms of the responses in looking at them, but then, you know, the question is how does that compare to the more chronic inputs? I went to a science fair in the local uh I think it was grades 5 through 7 or something had posters that they had made using recycled paper. They learned how to make posters out of recycled paper; that was one of the things. And then they had to have an environmental message that they would put on the poster. And you find the students saying, well, you know, that most of the oil is coming from the use of oil that people have. You know, don't pour it down the drain. Don't do this. Don't do that. Now, for heaven's sakes, if that message is getting through in a grade 5 through 7 classroom, and the students are putting it up on a poster, OK, then there is an education impact, and you can't help but feel, OK, there's been a contribution here. And . . . and uh you know we have opportunity to testify before Congress, a congressional committee, and you get that information in there.

TAYLOR: OK, so that's a part that you personally are going to feel a great deal of satisfaction in it.

FARRINGTON: Yes, I do. I feel like, OK, that that happens. But I also want to get across to the public that . . . that if Max Blumer and other people hadn't been doing their fundamental scientific research, funded by NSF, funded by the Office of Naval Research, we never would have been able to get that information. And, you know, people are always looking for this magic shortcut that goes from fundamental science to the application of that science. Let's shorten the pathway. And it's very hard to predict, very hard to predict when that's going to happen. And it's the fundamental base of knowledge that we move forward, and then from here, there and everywhere you could figure out what was happening. Who would know, for example, in . . . when I went to the oil spill in the Gulf of Mexico, a huge oil-well blowout, the Ixtoc I in the Bay of Campeche, and I was funded. I had ONR funding, and I just phoned up the people and said, “There’s a cruise going there; can I go?” And they said, “Sure, fine.” Now you’ve got a heck of a time getting some program managers these days to fund that, but I went on a NOAA cruise funded by the Office of Naval Research, ‘cause they’d been funding me to look into the fundamentals of organic geochemistry. Was there a Navy application? I didn’t know for sure, but when we got there, after we’d been on the outskirts of the slick for a long time, we figured out that the oil was actually coming on enough of it into seawater that was going through the flashback ration system and was in our drinking water on the ship. So now you fast-forward to the first Gulf War, and Saddam bleeds all the oil wells, and all of the oil is going into the water, and it’s coming down the coast, and it’s headed for the desalinization plants, and everybody’s worried about the plants getting mucked up, OK. But what we knew was that wasn’t the only problem. The problem was also that that stuff could get through—the low-molecular-weight compounds could get through—and in the drinking water, even if you kept the oil slick out. But that wasn’t the intent of going on the cruise originally, in the Gulf of Mexico. So here’s a cruise, 1979, [thumps] data applies 1990s.

TAYLOR: Well, you’re making a perfect point for the people that say, “Hey, what’s in this for me?” And the idea of coming up with truth, if you will, just . . .

FARRINGTON: Right.

TAYLOR: . . . trying to find out how something operates. React to this: you have been really, but maybe a little bit more. I remember years ago, showing a—in the days of the film strip—showing a film strip in class, and there was a woman that was taking temperatures and cores of glaciers, and the kids looked at that, and they said, “Geez, what a waste of money!” Well, now, in terms of stuff like global warming and things like this, . . .

FARRINGTON: Sure.

TAYLOR: . . . that work all of a sudden becomes very, very important. That’s critical data.

FARRINGTON: Correct.

TAYLOR: And that's the kind of thing you're talking about here, that the so-called "pure" research, lots of time, further down the road, has a very practical use.

FARRINGTON: It does, and also observations have a practical use, too. You hear people a lot of times talking about the scientific method, and you have to have a hypothesis, and you have to be testing hypotheses, and that's true. But you don't in the natural environment. A lot of times you have to do a little exploration and observation first to figure out exactly what the hypothesis is going to be that you're going to be testing. So, for example, we knew that . . . that maybe—we, the scientific community in the '50s knew—that perhaps that . . . that carbon dioxide interacting between the atmosphere and the ocean would be important. And maybe something was happening in the atmosphere, but if Roger Revelle, the director at Scripps, hadn't supported Charles David Kuhn[SP?] in his efforts to make measurements of $CO_2$ at Mauna Loa, they never would have had that long-term record that began. And people would say, "Wow, what's Kuhn doing? He's just measuring $CO_2$ in the atmosphere, and, you know, what hypothesis is he testing?" Well, he's testing a hypothesis, a really big one. OK, but it started off as observations. Now we have a record that goes on for . . . you know, that went for a long period of time that is the key to all the other monitoring that we do of carbon dioxide out there in the world, and a lot of times people in present day, students come through and say, well, you know, what are the issues that we're dealing with? Well, climate, carbon dioxide, the carbon cycle, things like that. These are the same things people were doing research on in the '50s. It's not new. The accuracy and precision to which we would like to know what's going on now has been refined tremendously, and we now know for sure we've got a serious problem we need to deal with and understand, but if they hadn't been doing those measurements in the '50s we would have had to start all over—start, you know, in the '60s, and we'd really be in trouble by now.

TAYLOR: What you've just said is almost a history of the Woods Hole Oceanographic Institution, because I think back to its inception in the 1930s, and some of the first hires—people like Al Woodcock, . . .

FARRINGTON: Right.

TAYLOR: . . . people like that—and I remember talking one time to Arnold Arons.

FARRINGTON: Right.

TAYLOR: . . . and I happened to mention that I'd just done an oral history with Al Woodcock.

FARRINGTON: Right.

TAYLOR: And I always remember: Arnold kind of shook his head and said, "You know, here was a guy that was a high-school dropout, and I came in a newly minted PhD, and this fellow was just seeing things that I didn't see."

FARRINGTON: Sure.

TAYLOR: And you just made me think of that so much, the whole idea of those original people that we had here . . .

FARRINGTON: Um-hum.

TAYLOR: . . . that were observers. A lot of them didn't have much of an academic background—if any.

FARRINGTON: Well, that's true, and . . . and we need to keep that in mind. And that's what so important about our technical staff people, that they don't have what many people in the academic community . . . . Fortunately you don't hear it much around here, but you hear it in universities, talk about the terminal degree. By that they mean the doctorate degree. You can't get any higher degree. Sometimes, unfortunately, the terminal degree is terminal in terms of the intellectual capacity of a person, and they somehow, some . . . once they reach that stage they seem to stop in terms of common sense, you know. I mean think about . . . about . . . uh you know, Hank Stommel. And I remember Arnold Arons telling me a story about Hank Stommel going to him and saying, "Gee, you know, if I'm going to make progress further I should get my doctorate degree." And Arnold saying, "Good heavens, Hank, who do you think is capable of advising you?"

TAYLOR: Or testing him?

FARRINGTON: Well, but I mean that's the . . . . Yeah, but that's what he meant by that. You know, "Who's going to be your supervisor and advisor? There isn't anybody capable of doing that. You're way ahead of the . . . ." And we have to keep that in mind when we get hung up on process as opposed to results, and I'm not saying by that that the ends justify the means. I'm just saying that, you know, we got to get some of this arrogance out of the way sometimes that, "Well," you know, "I'm a senior scientist, so I know better than an assistant scientist or a graduate student," or something like that. That's what's so great having the graduate students and postdocs around, is these are . . . you know, these are high-quality, bright, early-career minds, and they . . . they ask questions, and they probe, and they say, "Well, you've always thought that way, but why?" And you say, "Well, because . . . because . . . well, that's a good question." [They laugh.]

TAYLOR: And for you that must be a wonderful personal feeling, when that kind of . . . .

FARRINGTON: Well, it's a wonderful personal feeling when it's happened with my own graduate students, and I . . . and . . . .

[END OF SIDE 1, TAPE 4]

TAYLOR: They don't think of scientists getting the same thrill of sitting down and kicking ideas around and having exactly the same kind of feeling as a basketball coach has bouncing ideas among their contemporaries or whatnot.

FARRINGTON: Um-hum.

TAYLOR: It's a pretty special thing, isn't it?

FARRINGTON: It is, and I suspect that it happens in different . . . you know, in different ways in different careers. I'm sure the same thing happens to some extent in the medical profession when they're trying to figure out, you know, how do we respond to certain diseases? You know, how do we respond to trauma in the emergency room? Uh I think it happens in the software industry. I mean if you think about the people who do programming and the ideas that have led to the . . . you know, the revolution in the Internet and things like that, and engineers. They do the same thing. You know, it's . . . . What it is is . . . it's . . . it's human. It's the mind being engaged in something exciting and new, and how are we going to do something? In our case it's not how are we going to win the game or have a different play or something like that; it's how are we going to figure out the next interesting secret that nature is going to yield, and when's that going to happen?

TAYLOR: Yeah, and you've had a career here that's extended long enough so you've seen some of this development I talked about, this idea of the first group that came through here, and this is not a negative term, but I would have really called them naturalists in many cases. Then they became kind of, in K. O. Emery's day, really great data collectors, and then things started to really zero in more and more fine, as they went on, but you also had the advantage here of having a wonderful engineering staff that could build you a better machine to give you better data, . . .

FARRINGTON: Sure.

TAYLOR: . . . and when you got better data you got better science. So it's kind of wonderful . . .

FARRINGTON: Yeah.

TAYLOR: . . . mix.

FARRINGTON: Well, I think they were naturalists, but I think they were also people who were trying to understand nature, and I think that there were people who were naturally fascinated by the oceans and how the oceans functioned, but they were probably in the grand tradition of the naturalists and scientists of the Renaissance too, in the post-Renaissance period, and people have always been interested. I mean, you know, a very famous chemist, Robert Boyle, was interested in understanding salt and seawater, and, you know, Lavoisier and people like that all did work related to the oceans at some point in time, in chemistry.

TAYLOR: When I talk to some of the people that were originals, how come these pieces of sea grass as they get ripped up happen to fall in these straight patterns and then . . .

FARRINGTON: Sure.

TAYLOR: . . . and all that kind of thing, and that's what I meant by the observer. Al Woodcock . . .

FARRINGTON: Um-hum.

TAYLOR: . . . thinking about how does this fog always form over the canal here that separates us from the . . .

FARRINGTON: Sure.

TAYLOR: . . . mainland and so forth. That's a very interesting tradition, the way this has developed, and it's only been 75 years.

FARRINGTON: That's right, but the . . . we still have a tradition here of . . . of exploration, in the Institution, and when I talk with the potential incoming graduate students or postdocs, rather than say, "What are the strengths of the Institution?" and "What are the strengths of the MIT-Woods Hole Joint Program?" I would say, "Well, you have the opportunity to pursue uh one of the three or four legs of science in ocean sciences right now: theory, observation, experimentation, and now I would add the fourth—although some people would put it into experimentation or . . . or observation and interpretation--and that's modeling. And you have a critical mass of people, critical number of people in these areas that you can kick ideas around, and progress is made usually by uh breeding one or two of those together at the interface between uh, you know, between observations and theory, or theory and experimentation, or, even better, across so-called sub-disciplinary boundaries, because, despite the fact that in the old days people talked about oceanography, OK, now we're pretty much—we have been for years—divided up into biological, chemical, geology and geophysics, physical, you know, engineering. But there's been a lot of crossover as well, and now we're trying to make it a lot easier for people to cross over the disciplines in different ways.

TAYLOR: Well, as you know, I give tours in the Institution, and sometimes, when someone asks me a question about a particular individual or something, it's really difficult in this institution, to really classify them. I remember one case in particular, I said, well, I guess I would have to call that person a biogeochemist.

FARRINGTON: Well, you know, the biogeochemistry thing is very interesting. That uh I recently was helping Dickie Kahn[SP?] with the 75th anniversary history by writing. Since no one else would do it, I did some of the history of the Chemistry Department. And uh I thought, you know, well, biogeochemistry, people talking about it now . . . . We're going to have a new building in biogeochemistry. It's a new, cutting-edge, interdisciplinary thing. And I said, "Wait a minute," you know, several of us wrote papers in the '70s called biogeochemistry, OK. Well, I went back through the history of the annual reports, and . . . and in one of the reports in the '50s on chemistry it says something about Vaughan Bowen doing things and pursuing the "biogeochemistry" of elements in the ocean, so, you know, it's a case in point of what I was trying to say before, and that is it's very difficult to discover an entirely new concept. Rather what you're doing is you're advancing that concept to the next level of understanding in terms of its details, in terms of its resolution. That's a . . . .

TAYLOR: But you're also doing it in the total environment.

FARRINGTON: Yes.

TAYLOR: And this isn't one specific . . . like you take the drive from La Jolla Village up to Scripps. You pass all kinds of Salk Laboratories and things like this, and he was looking at a specific thing. He was looking at the polio virus, and you folks: I mean, if you look at a hydrothermal vent—dozens of things that you're looking at. And there has to be—now correct me if I'm wrong—there has to be an understanding of your specialty (geochemistry), but you've got to know a little something else about what these things are that are sticking their heads up and what they're doing there, and the currents that move by it, and all of the physical oceanography part, and so on. So it's really . . . . You have to know a lot of stuff and have a specialty.

FARRINGTON: Well, you need to have a thorough grounding in the science, and I think that that's the point. You can do interdisciplinary science, but you have to have a thorough grounding in the science, and the most important thing is to be able to know when to go ask a question of another discipline and who to go ask it, and that's one of the arguments of having a critical number of people around, like we have here at the Institution, or at Scripps, and so forth, is that there's a . . . . You know, meeting on the Internet: it's fine. You can converse with people all around the <u>world</u> now, but it's this whole different thing if you're all of a sudden walking with somebody to . . . to lunch or something, and then you say, "Ah, I was thinking this morning about something. Seeing you," and <u>I've</u> had this happen to me, "seeing you <u>reminds</u> me. I wanted to ask you about" such and such. And from <u>that</u> then comes an idea. That's the same with the engineers, having the engineers around, and going to the seminars. A lot of people have this mistaken idea that it's the scientists, or <u>only</u> the scientists who go to the engineers and say, "Gee, I want to make this measure, and . . . and can you <u>design</u> something for it." <u>I</u> really, a lot of times, including myself, have had the experience of an engineer coming up to <u>me</u> and saying, "You know, I remember listening to your seminar" or "We talked on the last cruise about some of the things you were doing, and uh . . . and, you know, it seems to me like maybe you would like to make this kind of sampling. You know, would you like to get" this kind of sample? "Would you like to make" this kind of measurement? And I say, "Well, yes, sure, but, you know, there's no way to do it." "Well, I think I have a way of doing it." And so that is the inverse, in a way, the engineers knowing enough of the science to say, "Gee, you know," they read something or learn something from another engineer, and then all of a sudden you have a new method. So it's the interactivity of people and ideas that's key to advancing.

TAYLOR: One of the people I've talked to said that at one time there used to be a weekly seminar. Someone'd come and give a paper on something, and anybody could go to it. He said that sparked more different ideas, "my sitting there listening to them." And he said, "It wasn't my field, necessarily."

FARRINGTON: Right.

TAYLOR: But more new ideas and ways of approaching things and so on.

FARRINGTON: Right. That was called the Journal Club, . . .

TAYLOR: Right.

FARRINGTON: . . . and it used to be on Monday nights, and I . . . . It was a tradition of the Institution for years and years, and you'd go, because the director would go. The department chairs would go. Everybody would go, and . . . and I remember going as an assistant scientist, giving my first talk to the Journal Club, in the Redfield Auditorium, and . . . and, you know, being nervous enough as it was that all of these eminent oceanographers were there, but then seeing Alfred Redfield with his cane come down [machinery starts up] the steps and sit in the front row. And I'm saying, "Holy mackerel! [They laugh.] This is the history of oceanography sitting here right in front of me in large measure," to some extent. And as an assistant scientist right after that, I was put on the Journal Club committee. There was representatives from different departments, and I was told, you know, we need to . . . to get some new and interesting ideas in there, and it would be a mix of outside speakers and inside people. [Thumps.] And there was a fellow in the Biology Department by the name of John Kanwisher. [Machinery noise escalates to a shriek.]

VOICE: Sorry.

FARRINGTON: Yup.

VOICE: That's pretty bad. [Laughs.] [Tape stops and starts again.]

TAYLOR: OK, we just had to pause for a minute. It sounded like somebody was doing a root canal on an elephant out there, so [They laugh.] we got that stopped, and we're talking about different kinds of things on dealing with the field and the kinds of experiences you can have in an institution like this. John was talking about a club . . .

FARRINGTON: The Journal Club.

TAYLOR: . . . Journal Club that had speakers and so on, and he mentioned John Kanwisher, who was another one that I had done an oral history on, and you had a comment on that.

FARRINGTON: Well, I thought, talking with John, he's a very interesting fellow, and we had a lot of discussions at the Endeavor House, which was when they had a coffee house in the basement [thumps] of the church, which is now the Exhibit Center of the Institution. And so I said to John, "Gee, have you ever given a Journal Club talk recently?" And he said, "No." And I said, "Well, we ought to get you to talk on something." And he said, "OK, I'll talk." And uh, you know, several of my colleagues, when I told them I was going to get John Kanwisher to talk, they said, "Oh, <u>that</u> will be interesting." And so I, you know, I waited and waiting, and, you know, we were coming sort of the week before, and I kept bugging him for the title so we could publish it in . . . you know, the <u>weekly yellow</u> sheet that came out, and finally he gave me the title, and I thought, you know, "This is it. My career's at an end." Because his title was "Net Tows, Current Meters, and Other Nonsense." All right. And so the first, you know, [thumps] first five, ten minutes, he took on many of the biological oceanographers in the audience, and saying, "Well, you know, net tows that we use in the ocean these days, in the deep water, to try to figure out what's going on, basically catch the lame, the sick and the dying," and, you know, "The thought that we <u>know</u> what's going on in the deep ocean based on net tows is kind of ridiculous, you know, because fish will have evolved over time, or . . . organisms will have evolved to figure out that, you know, a net coming at them in a horizontal way is like a <u>predator</u>, and it makes a lot of noise, and so they get <u>away</u>." Then he moved on to current meters. He talked about how current meters had uh uh, you know, measured, depending on how you put <u>moorings</u> out, were measuring nothing but <u>mooring</u> vibration, which wasn't quite true, but, but he went beyond that. He said, "I've been working on inductively coupled current meter," you know, "and I've got it hooked up actually in my lab, and I've run the cable down to Redfield 204, and so afterwards . . . ." We used to have beer afterwards, you know, refreshments and stuff and keep talking. ". . . I invite you to come in and take a look at," you know, "what it's measuring right now underneath the drawbridge in Eel Pond." And so he, you know, he was <u>criticizing</u> people, but he had a suggestion. And, you know, even though he criticized them, the physical oceanographers, people were <u>crowding</u> around him in Redfield 204, looking at, you know, what he had come up with, and it was that type of, you know, <u>irreverence</u>. You had to have these people around. I called him the gadfly-in-residence, Kanwisher, and, you know, he could . . . . A lot of times he could be sarcastic, and he was wrong. But other times, you know, he was right on, and there was no doubt there was a brilliant mind at work there, no doubt whatsoever.

TAYLOR: OK, that's a good point to stop, I think, for today.

FARRINGTON: Right.

[END OF TAPE 4]