ORAL HISTORY PROJECT
of
The H. John Heinz III Center for Science, Economics and the Environment

In conjunction with the Colloquia Series

OCEANOGRAPHY: THE MAKING OF A SCIENCE
People, Institutions and Discovery

Transcript of the Videotape-Recorded Interview with
EDWARD GOLDBERG

Conducted at
Scripps Institution of Oceanography
The University of California San Diego
La Jolla, California
February 18, 2000

Interviewers: Ronald Rainger and Naomi Oreskes

Funded by the Office of Naval Research
Video and Audio Recordings by George Washington University Television
Transcribed by TechniType Transcripts, Davis, CA

EDWARD GOLDBERG

February 18, 2000

Ronald Ranger and Naomi Oreskes,
interviewers

[Note: The volume level for this interview is extremely low. Every effort was made to provide an accurate transcript. Where this was not possible, [unclear] is noted in the transcript.]

[The first two minutes of this interview are completely inaudible.]

Edward Goldberg: It was just at a time when meteorite research was exciting, and the unique thing about my research, I was one of the first people in the world to use slow neutron activation analysis as an analytical tool. The technique involved irradiating samples with slow neutrons which we received at the Argonne National Laboratory. Irradiating samples made all of the--not all, many of the elements radioactive, and then their radioactivities could be assayed. The amounts of radiation as a function of the type of radiation identified the amounts and types of elements.

I received my degree in 1949. I was a member of the chemistry department there, and in 1949 I needed a job. My professor Harrison Brown knew Roger Revelle, and he convinced Roger Revelle that I might be an interesting addition to the Scripps Institution of Oceanography. I was young, ambitious, and I had some modern tools in radioactivity that were unique.

Ronald Rainger: Could I interrupt you for a second?

Goldberg: Sure.

RR: I wanted to ask you a bit about the work with radioactivity. This is the period soon after the atomic bomb. What was your sense about working with anatomic physics? Was it exciting? Were you concerned about the uses of the work you might be doing?

Goldberg: It was terribly exciting, because the professors there wrote the textbooks and wrote the papers that we used. We knew their names and we knew the names of people who came to visit them. It was very intense work. I guess "intensity" is the best word to describe the atmosphere of the University of Chicago's chemistry department at the time. The graduate students were not unionized, but they did have a camaraderie which was to do the best of all possible work, to get out of there in shortest of all possible times, and to get the best of all possible jobs. It was ambition-centered.

RR: Did you have any concerns about the fact that work in nuclear physics at that time was being used in ways that were related to atomic [unclear]?

Goldberg: As a graduate student and even after I came to Scripps, my initial concerns had nothing to do with the wartime or peacetime uses of atomic energy. They did develop here at Scripps, but it was the peacetime uses of atomic energy that was fostered primarily by Roger Revelle. He recognized that artificial radionuclides, produced either in powerplants or from atomic detonations, were producing one of the potentially toxic environmental contaminants.

Revelle was able to obtain funds for us, or to assist us in getting funds in artificial radioactivities in the environment.

We also worked on atomic bomb tests. One was the effect of nuclear weapons upon submarines. This was a famous expedition called Wigwam. Two or three nuclear weapons were detonated off the California coast in the vicinity of a submarine, and the impacts of these weapons on the submarine were duly noted.

But our concerns were not those of my successors at the University of Chicago, who were fearful of improper uses of our knowledge about nuclear science.

RR: We'll come back to the Revelle stuff a little bit later.

Goldberg: Okay.

RR: And I'm wondering if you witnessed the Wigwam test?

Goldberg: Yes.

RR: Were you also at Redwing?

Goldberg: No.

RR: What was your reaction to the test itself, the [unclear]?

Goldberg: Well, I had been at Scripps, I think, during the tests, I don't recall--maybe six, seven, eight years. By that time I was terribly interested in the interaction of organisms in the sea and the chemical elements of the sea. This gave us an opportunity to study biological interactions--these, of course, were artificial radionuclides--with the biosphere. This was a terribly exciting thing to do. It was novel, and we gained a hell of a lot of information about this. So I was very pleased to be involved in these tests. I still had no concerns about the implications of these tests in a moral vein.

RR: When you were at Chicago--I'm taking you back a little bit--in the late forties and the early fifties, [unclear] is there, and Libby.

Goldberg: Yes.

RR: I assume you're still there at that point [unclear]?

Goldberg: Oh, no. I knew Walter Libby going back to my undergraduate days at the University of California. He directed undergraduate research with me. I knew Libby for a long time.

RR: I've read that at some point you've referred to some of the work that they were doing with atomic as really applied science.

Goldberg: Yes.

RR: I wonder how you viewed your own work. In that same vein, or is it different?

Goldberg: You mean my overall work in marine science?

RR: No, your work primarily with radioactive tracers.

Goldberg: Oh, no. I would say that as a chemist, I was involved in analytical chemistry, how one determines what and how much is there. I would call that basic chemistry. The utilization of irradiation to make elements radioactive was a triviality. The goal was to do better analytical chemistry than had been done before.

RR: And some of it, I gather too, went in to Scripps and deals with uptake in organisms.

Goldberg: Yes.

RR: And animals. So it's chemistry, but it's also sort of biochemistry.

Goldberg: Well, when I came to Scripps, I couldn't spell "oceanography." I learned quickly, though. I looked around for significant, substantial, and novel problems to carry out. I looked at publications in oceanography, and I was absolutely stunned by the fact that many marine

organisms concentrated extremely rare elements from sea water, rare in the sense that they were in very low concentrations, rare in the sense that they were not studied even among pure chemists.

So my initial work was looking at the uptake of elements by marine organisms. I had this wonderful tool of radioactivity. I could prepare radioisotopes at the Crocker Laboratory in Berkeley, and I could buy them. So all of my tools were there. I didn't have much money when I came to Scripps, but I was able to carry out a large number of measurements with the existing instruments that were here.

RR: You referred to your work [unclear] substantial and significant. I wonder what you mean by those terms in some ways. [unclear] to out fundamental research? Is that what constitutes significant?

Goldberg: Significant is that it advances the area somewhat. How do you get a sense that it advances? Other people imitate you. [Laughter] And other people carry on.

For example, I was the first person to utilize and publish upon radioactive techniques, nuclear techniques in marine science at Scripps. I did have a counterpart at Woods Hole [Oceanographic Institution], and we were in close contact, but also I was led by Revelle to look at the implications of the coming nuclear energy. And so, with time, I gained research support because of this interest. The AEC [Atomic Energy Commission] at the time furnished me funds, without any strings attached, to carry out research in marine science.

RR: That's an interesting point, because we hear of ONR [Office of Naval Research] as a patron of fundamental research in many ways.

Goldberg: Yes.

RR: Would you characterize the work done with the agency as really that they're quite similar as a patron to ONR?

Goldberg: I would say that they were quite similar. Both of these agencies are mission-oriented, and the AEC had a mission to understand radioactivity in the environment. We published books with the AEC on radioactivity and the environment. I think they were quite satisfied with those books. The big trouble was that with time the AEC or its successors, ERDA [Energy Research and Development Administration] and more recently the Department of Energy, have downplayed the importance of nuclear science or, as we say, environmental nuclear science.

RR: That's a very interesting point. Some of the other people we talked to don't really view either ONR--certainly they don't view ONR as having that mission. Was your work at points funded by ONR as well as AEC?

Goldberg: Yes. I'm embarrassed to say I was funded by ONR for over forty years. But let me give you an example of how the ONR utilized my limited talents. At one time I was very

interested in the rare earths and sea water. This is a group of fourteen elements, atomic numbers fifty-seven to seventy-one, and they'd never been analyzed in sea water until I decided it should be done. I had the techniques. I knew about activation analysis. So we irradiated samples or extracts from sea water up at Neutron [unclear] General Atomics here in San Diego, and we were able to analyze these--not all, but many of the fourteen rare earths. I was excited about this. More recently it's become a cottage industry because it's very easy to do. We've reached, in my opinion, diminishing returns with this type of analysis.

But one thing I did, I did this with Navy monies, in part. We analyzed for europium. Now, europium was used in lasers to communicate in sea water. These are high-energy light beams. You're with me. And the Navy needed [unclear] by the constituents of sea water. We had the europium content, so we had, in part, a homogenous problem solved. We thought it was solved. So we had the only numbers for europium in sea water, and I think the Navy was pleased.

RR: That's a great example, because they're concerned probably with communication among submarines.

Goldberg: Among other things. They didn't tell me all their secrets.

RR: One thing I wanted to ask you, I assume that even as a graduate student somewhat later, [unclear] did have to get security clearance. The agency often relied on FBI background checks

on almost anybody who work for the AEC. How well aware of that were you? And did it have any delaying impact on when you could do work [unclear]?

Goldberg: No, I cleared that up very quickly. I spent four years in the Navy during the war to keep democracy in your hands. I've been funded by the Navy for another forty or so years. So I have no high ideals about being associated with the military.

Also, one of my concerns was, and still is, is that we have a military and it should receive the most reasonable scientific input it can on their problems, either directly or indirectly, and I still feel that way.

RR: Do you think that we're getting as much of that these days as in earlier times or, as you said, diminishing returns--

Goldberg: Diminishing. Maybe that was the wrong term. I can't talk to the fact whether the Navy or the Army or the Air Force is getting as much as they should. I just can't, because I don't have any knowledge.

RR: Well, there's an interesting question, then. When you talk to some other people at Cal, what sense do you have that when you do this work for these agencies? I mean, does it just go into some black hole out there and you never really get much feedback in terms of this [unclear]?

Goldberg: Oh, no, you get feedback. Now, let me refresh you with how research is funded. I put in for projects I thought were important to carry out in marine science. The Navy, the AEC, or other organizations, like the EPA [Environmental Protection Agency], would look at these proposals and either grant them or not, depending upon how relevant they thought they were to science and perhaps even how relevant they thought to their missions, so we were funded or not, and this was the way we went. I went that way for forty years.

RR: Well, I realize that. What I'm wondering is--- [Interruption.]

I realize that, that they have to review and provide you with funds, but did you ever get a sense after the research is done, of what impact it had on those particular projects for the AEC or for the Navy? Were they ever in communication with you after the work is done in letting you know what you did for them?

Goldberg: The answer is yes. You see, when you put in proposals, you deal with program managers, and the program managers would talk with you very intimately and in detail about your work and its relevance to their missions. Sometimes the relevance was purely basic science, and there was no denying this.

Times have changed. Where the program managers with the AEC and with the Navy in the past, I suspect were more--what's a good word--intelligent than they are today. [Laughter] We had some very wonderful people who were these program managers. We also often put their names on the papers that were published. We were in close communication with them. They

would come out to Scripps, discuss our results, and there was a nice interplay between the program officers and the scientists at Scripps or other places.

RR: Well, we know many of those sorts of individuals through ONR, like Revelle and Gordon Lill and Art [Arthur] Maxwell.

Goldberg: Yes.

RR: Do you remember any of the individuals through the agency that played that same kind of role?

Goldberg: Yes, one was Arnold Josephs. He was very close to our work. I have something from Alzheimer's. Maybe I'll come up with a few more names in the near future.

RR: That's fine. I mean, we just don't really know about them. The AEC is not quite as open in some ways even today [unclear] as ONR was. I mean, with ONR we always know--in part because Revelle and these people were in the Navy at ONR, back at Scripps, the same with Art Maxwell.

Goldberg: Well, let me make one point here. People like Maxwell, and you've named one, Gordon Lill, were first-class scientists and could have made it in universities, in my opinion. I

think some of the newer crop, because you have so many and because the opportunities for work like this are rather ample, you get both the good and the bad in.

RR: To what extent did classification, security classification, play a role in your work?

Goldberg: Well, as you know, I was involved in Operation Wigwam, which was a classified operation, and the results that we obtained for our researches on Wigwam were classified for some time. I studied the uptake of radioactivities by marine organisms and the characteristics of artificial radioactivities in the oceans. This was classified for a while, but eventually we got all of our work declassified, and this was all published.

RR: Do you know, maybe not in your own case, of people who--you know, one of the things about science is that we emphasize a lot open communication, peer review, ability to talk with people about your work. Did you ever experience constraints in those kinds of plays, either--well, certainly with Wigwam, I guess, to some extent, or in other aspects of your work?

Goldberg: Well, our work with Wigwam, the results on the distribution of radioactivity from the nuclear blast, was classified, and we just didn't talk about it with unclassified people. We took our commitments to secrecy rather seriously. We did a lot of other work for the ONR and the AEC during Wigwam which were unclassified and we talked about these. We probably slipped occasionally, but with no ill intents.

RR: I mean, in some cases, I mean, clearly you were already a tenured professor and, you know, even if the literature--even if the work that's being done is not out there as literature, it doesn't have that big an effect on somebody that's tenured, but what about for students that were at Scripps that were working on those projects, or people who would be coming up for tenure?

Goldberg: Let me tell you about my interactions with students. I have very strong feelings about students. I never employed students on any research that could be considered classified. That's my first caveat about dealing with students. Secondly, I always tried to assign students the most important problems, the most important solvable problems in my grasp. And the third thing, I never put my name on a student's paper. I was rather unique with respect to this at Scripps. I thought you trained students to do independent research, and I took that to be independent of myself. So I would never put my name on a student's paper.

Now, I had one colleague at Caltech, a man named Norman Brooks, who also felt the same as I did, but he ended up having a hard time getting money and was forced to put his name on students' papers. But he was apologetic, and we were very good friends. Okay.

RR: Do you think that you were typical or unusual in not employing students on classified work?

Goldberg: I can't answer that, because I don't know.

RR: Back to the earlier point about--you said that the work on Wigwam was eventually declassified. How long are we talking about?

Goldberg: Oh, I think it was a matter of years, a couple of years.

RR: Oh, not a real long time.

Goldberg: Oh, no.

RR: So that it eventually gets into the white literature [unclear].

Goldberg: Yes.

RR: I want to bring you back a little bit to the move from Chicago to Scripps. You said that Revelle played some role in that. I'm wondering if you could expand on this.

Goldberg: Well, Revelle, about in 1949, was hustling for the directorship of Scripps, which he eventually got, I think it was in 1951, but for all practical purposes, he hired me at Scripps because of his friendship with my mentor, Harrison Brown. So I got to know Revelle intimately quite early.

RR: What was your view of Revelle as an administrator or as a person, as a scientist?

Goldberg: Well, quite simply, he was one of the greatest persons I ever knew in my life. If the term “hero worship” could describe this interaction, it was hero worship. This man was a powerhouse, and his greatness went far beyond science. It went far beyond vision. He saw for La Jolla the possibility of it becoming the Athens of the West Coast, and that’s what's actually happened. There’s an Athenian quality about La Jolla, and he was one of the first people to identify this potential.

RR: I know he often used the term in creating UCSD in calling it “The cathedral on the hill," which has that same kind of [unclear]. In terms of him as a scientist, I know you’ve published some things jointly with he and Ted Folsom.

Goldberg: Yes.

RR: He apparently works in a number of different areas in oceanography. What’s your sense of him as a working scientist?

Goldberg: He had few peers at Scripps. I’m not going to name names, but he had few peers. He was a great scientist. Can you imagine a man administering a large institution like Scripps and carrying out full-time research programs like his work on carbon dioxide and its impact upon climate, carrying out these researches simultaneously, administering Scripps, being involved in international and national ventures in marine science? This man did an unbelievable

amount in his tenure. We all recognized--not all, but many of us recognized this and did everything we could to help him.

RR: One of the things you mentioned last week in your talk at the conference was that coming to Scripps in the fifties was a very exciting time.

Goldberg: Yes.

RR: I wonder if you could elaborate on that.

Goldberg: Yes, let me tell you why it was exciting. First of all, it was exciting because Revelle made it exciting. I came from a research institution, the University of Chicago, that was dynamic, it was active, it was intense. I came to Scripps, and initially I thought it was the sticks, but I very quickly learned that the scientists at Scripps were equivalent in competence, in drive, to the people I'd known at the University of Chicago. They were just less intense about it. They were more laid back.

Very quickly, as in Chicago, you were working with the leaders in the field in chemistry. At Scripps, I learned that I was working with the leaders in the field in marine science. There weren't that many leaders at the time. It was very exciting to know these people and eventually became on a first-name basis with most of them, not all of them, just most of them. For a young scientist to be able to sit down and have a cup of coffee with the leading scientist in, say, physical oceanography, like Walter Munk, it was really quite an inspiring activity.

RR: You also mentioned in your talk and other people have talked about [unclear].

[Interruption.]

[Begin Tape 2, Side 1]

RR: Go ahead. You were saying--

Goldberg: I was going to say, when I first came to Scripps, there was an Orwellian mood. All scientists were equal, whether you be a graduate student, a professor, a retired professor. This, of course, led to very dramatic conversations and interplay. In part, this was due to the fact there were few buildings, so the scientists ran into each other constantly. There was a superb Wednesday afternoon seminar which brought together the whole faculty, students, and researchers, led by Revelle, the topics chosen by Revelle, the speaker chosen by Revelle, in which science was practiced, in my sense, in the highest form. It was a high mass of science.

The students who came out of this became the directors of laboratories and institutions all over the world, and they took with them a sense from Scripps that there is not a necessary stratification of people in science. Everyone has the right to the floor if they hold up their hand and they're recognized.

RR: One of the things that you talked about, too, was the interdisciplinary character of the work.

Goldberg: Yes.

RR: I wondered if you'd comment on that.

Goldberg: It came and went. When I came here, as I pointed out, the scientists all knew each other, often ate lunch together. All of their social life was dependent upon being at Scripps, and so this created a tremendously strong camaraderie, but this also went into science.

I published papers with people like Doug Inman, with people like Ted Walker, who was a diatom specialist, where we each contributed our expertise in our fields on what we considered an important problem. Now, this could go on as long as the scientists were in contact with each other and as long as you were doing things that was attractive to all participants.

As Scripps grew bigger with more buildings, there was less and less contact among the scientists. With Revelle leaving, the Wednesday afternoon seminars disappeared, and this interdisciplinary work, it was diminished remarkably. And today there's a little interdisciplinary work, but not with the dedication as it was back in the fifties and sixties.

RR: I'm not going to refer to a name here, but one of the people we've interviewed over the last few days said that he felt that the geochemists played a role in the decline of interdisciplinary work because, from his point of view, they didn't share their knowledge in the way that this person would have liked. Now, I don't know, I mean, that's kind of an awkward question to ask, in a way.

Goldberg: It is an awkward question because I can't answer it. I haven't the slightest idea of what he's getting to. The geochemists are a fractious group of people, not only at this institution, but at many institutions. I can't tell you why, but this is their reputation. And they also were noisy here, but they were successful. Can I make a little divergence?

RR: Of course.

Goldberg: One received awards without expectation while you were at Scripps, at least during the Revelle years. Many of us received--well, we went on sabbaticals which Revelle supported. In fact, I went to Switzerland. In 1959 I was doing some what I thought was successful research. Revelle saw me in Switzerland, and he said, "How are you doing?"

I said, "Great. I'm sorry I can only stay here for six more weeks."

He said, "Oh, stay on. I'll pay for it." That was the attitude they had. People were encouraged to be members of national and international groups. A very curious thing is that people like Revelle and Munk--there's a name I'm missing, I'm sorry, I'm missing one name here--pushed Scripps scientists into the National Academy [of Sciences]. When I was here during the--in fact, now we have an unusual number of Scripps people in the National Academy. This was done by intent. This activity has ceased. I can't tell you why. Our present director, for some reason or other, doesn't push Scripps people into the Academy, and he's the one with the most power.

RR: I want to ask you, to a certain extent, I wondered to what degree--well, I've seen things from people like Carl Hubbs, Claude Zobell, Brad Shepherd. Hubbs particularly, I think, would not view it as one big happy family. Zobell might not, in the early days. They clearly had some troubles with Revelle.

Goldberg: Well, they didn't like him. I can't talk to that too much. I think Revelle was outspoken. He did not ever hesitate to say what he thought about marine biology or biology at Scripps. Of course, that offended some of them. Also, he was a whippersnapper. He was young. These were older people that had been at Scripps for many years. So that atmosphere contributed to, I think, unpleasantness between these people and Revelle.

RR: Is it your sense that when we talk about the interdisciplinarity of oceanography in the fifties, I mean, a little bit after [Harald] Sverdrup, I think, that the biological oceanography was not as integrated in as the geological, the chemical, and the physical?

Goldberg: I couldn't say that. I don't know if that's true.

Naomi Oreskes: [unclear].

Goldberg: Watch your hands. Girls always tell me that.

NO: [Laughter] Okay.

RR: Is the fact that there was such an interdisciplinary environment, did that help enable you, who didn't come--I mean, you come from a rather different background. Did that help you fit in more quickly?

Goldberg: Yes, very much so. When you recognize other people's talents and goals and problem areas and they recognize yours, it was much simpler to come together and do joint projects. Also, you were close. You often lived together because, you know, the early days the Scripps people occupied housing on the campus. All of this led to, I think, a very dynamic scientific activity.

Now, you're right that a number of the older faculty resented Revelle. There were many reasons for this, but I don't think this played much of a part either positively or negatively in the development of Scripps. It was not of that much significance to him.

RR: He just plowed ahead anyway.

Goldberg: He plowed ahead anyway. Right.

RR: Well, in the case of Hubbs, I know he grudgingly came to respect Revelle. Maybe "grudgingly" is a little strong, but he eventually realized that what Revelle was doing really was positive.

Goldberg: I'll give you a bit of history you don't know.

RR: Okay.

Goldberg: My father and Hubbs were roommates at Stanford.

RR: Oh, really? You mentioned that in the mid-1950s, there begins to be a fair amount of work under the AEC funds here at Scripps on peaceful uses for atomic energy. I wonder if you could comment on what kind of work you and Revelle and others were doing in that vein.

Goldberg: Let me take a look. I have Alzheimer's badly. I think terminal.

NO: [unclear]. [Laughter]

Goldberg: I am. I never pull anybody's legs.

What we did of consequence to the AEC in the fifties, you know, if I have--well, one of the things that we did do, we published papers on nuclear science in oceanography. We were concerned with the factors of the uptake accumulation and loss of radionuclides by marine organisms and we published in this one paper at the National Academy of Sciences.

Also one of the things we did was to look at the distribution of nuclear species in the environment. We discovered a number of radioactive species, natural radioactive species, in the marine environment because of our abilities to measure very small amounts of radioactivity.

One of the nuclear species we discovered was silicon 32. Silicon 32 became later useful as a tracer of silicon geochemistry in the oceans. We also used nuclides produced by bomb blasts like iron 55 to study the flow of iron in the atmosphere and in the oceans.

I haven't talked about what we did with the sediments, but our abilities to measure radioactivities both in sea waters and sediments allowed us to put time markers in both waters in the ocean. I think you are well aware that a lot of very important work was done on carbon 14 and tritium in the oceans, primarily by a very distinguished scientist named Hans Suess. My interests were directed to time scales in sediments primarily utilizing naturally occurring radioactivities, such as uranium and thorium. So one of our powers here at Scripps was to among the first to introduce reliable time frames, other than paleontological ones, into geochemical systems.

RR: May I ask you, why does the National Academy become so concerned? I'm sure the AEC is concerned, but why does the National Academy of Sciences become so concerned with radioactivity in the mid-fifties and begins to fund a lot of these studies and conferences and all?

Goldberg: I think because people like Revelle and [Columbus] Iselin, all members of the National Academy or to-be members of the Academy, had tremendous powers of influence. They were very influential, and they could identify first-rate problems that were taken up by other people. One always hears Iselin, Revelle, and [Maurice] Ewing as mentioned as the triad of great scientists at the time, but there were a large number of other scientists who had similar concerns. You mentioned one that been at the University of Washington whose name--

RR: [Richard] Fleming.

Goldberg: Yes, Fleming. Fleming was a scientist interested in these problems. But their interests spread, and it spread very quickly because these were important problems. They were problems that youth could take, have a great input in, because the techniques were part of their training.

RR: Is it also the case that the National Academy and others become concerned because of some of those really huge tests, like Bravo and Castle, and the possibility of diffusion of radioactive materials through the oceans worldwide?

Goldberg: The answer to that is one of the reasons the AEC was concerned with radioactivity is the most susceptible group of organisms is man, encompasses man. The AEC was concerned about radioactivity and public health. And even to this day, there isn't too much of a concern about radioactivity in the oceans and the health of organisms.

RR: But you do deal somewhat with aquatic organism uptake of radionuclides.

Goldberg: Yes. Well, we just didn't know at the time. Let me go a little further on this. As time went on, we realized that certain marine creatures had the unique ability to concentrate certain elements. For example, tin is concentrated by gastropods in the form of tributyl tin. This

is a very unusual thing. Chlorinated hydrocarbons like DDT, polychlorinated biphenyls are concentrated by other groups of organisms, and this specificity at concentrations, of course, led to catastrophes. In fact, I've made the statement, I think it's correct, is nearly every environmental pollutant has been identified by a catastrophe. You need catastrophes first, and DDT--I had a little input on DDT. DDT was identified when we had mass extinctions of a variety of organisms in coastal marine waters. Tributyl tin was identified as a pollutant when we had mass mortalities of other groups of organisms. So there is a great interest in looking at organisms' populations for potential impacts of single or groups of species.

RR: One of the things in the mid-fifties that continues, I guess, it was certainly beginning then is whether the oceans were a safe place for disposal of radioactive wastes.

Goldberg: Yes.

RR: I'd like to get your views on that and to what extent DNA studies included that.

Goldberg: Well, I never was involved with the waste disposal of radio waste in the oceans, the disposal of radioactive wastes in the oceans, per se. There were two or three people on the East Coast who have been much more deeply involved.

On the other hand, I have been deeply involved with the disposal of nonradioactive wastes in the ocean, and I've argued that the oceans have an unused capacity to accommodate some but not all of man's wastes, some of man's wastes, in certain parts of the oceans at certain

times. I stood by this and got into lots of trouble, because putting dirt in the oceans does not appeal to our more religiously involved citizens, "religion" in a rude way. So even to this day I still think that we under-use the ocean for waste disposal.

RR: There were a number of people, though, in the mid-fifties, Willard Bascom, I think Revelle, and others who did think that the oceans were a safe place for waste disposal.

Goldberg: Yes. I was one of them. I knew most of those people quite well. In fact, I still see Bascom a lot.

RR: At that time, did you assume that it would work, and do you still think that it would work with radioactive [unclear]?

Goldberg: Well, let me rephrase what you just said. There is a carrying capacity of the oceans, an accommodative capacity of the oceans for certain wastes, be they radioactive or nonradioactive. Although it's not strongly publicized, we continue to put radioactive waste into the oceans, and safely. For example, the British do it at a place called Sellafield in the United Kingdom. The French do it at Cap la Hague, and very successfully. These are diluted wastes. If you're opposed to doing this, you have to give an option, where on land do you place it that's more secure.

RR: Or in space [unclear]. I wonder if you could talk about, to some extent in the mid-fifties, there's begins this bigger emphasis on atomic and radioactive tracers, and Scripps grows in some ways, in terms of new people coming in. I'm thinking of Gustav Arrhenius, Hans Seuss, and others. Could you expand on that a little bit?

Goldberg: Sure. Arrhenius came about the same time I did, one year later. He never got involved in these radioactive problems, either applied or basic. As a matter of fact, these didn't appeal to a lot of people.

I think Ted Folsom and myself were two of the pushers on studying artificial radionuclides in the ocean, and Folsom continued to do this until his retirement. He was a very successful innovator in science, a very peculiar guy, too.

But let me tell you, I branched off. Let me tell you why I branched off from radioactivity to other pollutants. The reason I branched off, I was studying atmospheric dust. I was curious about what atmospheric dust was composed of, and I found out in atmospheric dust there was a large amount of a mineral called talc, and talc is a rare mineral, and yet every atmospheric dust I looked at was loaded with talc on the Pacific coast. This was a consequence of talc being used as a carrier for DDT in the spraying of farm crops.

Then I got interested in DDT because DDT was being spread about the world attached to talc in winds. So we wrote a few papers on the movement of DDT about the atmosphere. That's how I got into the DDT business. And once you get into these types of pollutants, you're in forever because they're so unusual and so difficult to properly assess.

This is how I got into the business of how does one analyze a large number of materials getting into the atmosphere, into the oceans, and on land which can affect living processes so different in composition. You had the radioactive nuclides which expand the whole periodic table in various forms. You now had the pesticides like DDT, dieldrin--blah, blah. You had the polychlorinated biphenyls. You had oil. Petroleum was being identified as a nasty pollutant. You had other organic materials that could endanger other life processes, and more recently you have plastics, which I think are a terribly nasty substance.

Well, all of these things had to be studied and input given to appropriate agencies, but never a straightforward thing of giving your results to, say, the DOE [Department of Energy] or to, later, AEC or to the Department of Interior. They sort took it out of the literature.

Let me talk to this for just a little bit. I once got interested in tributyl tin. Tributyl tin scared the holy pee out of me. This was the most toxic substance, in my opinion, ever directly introduced to the marine environment by our society. It's introduced as an anti-fouling agent in paints. We were slopping all over the bottoms of ships. It was getting into coastal waters and killing everything in sight.

I became very concerned about this and decided we should have good measures of tributyl tin in our California coastal waters such that we could make a judgment as to whether they were safe or not and whether the levels constituted a hazard to marine species. I got some money to measure tributyl tin in California coastal waters. I went to eighty marinists, as a matter of fact, and I had a hell of a time getting this money. Such organizations as Sea Grant--I shouldn't discuss Sea Grant, because it'll make me furious. It's been run by incompetents, in my opinion.

Anyway, I got money from other agencies and found out these tremendously high levels of tributyl tin in coastal waters and sediments. These numbers were hazardous. By this time lots of people were working on tributyl tin. It became the pollutant of the decade, and subsequently its use was regulated strongly in California. But there was a time period of about six or seven years between my submission of my publication and my results that were used widely by the California agencies and the regulation of DDT. One accepts these time scales as nothing's going to happen tomorrow. It does take time.

RR: And EPA didn't do that on national level.

Goldberg: Never did. We were measuring tributyl tin before they could spell it. In fact, before they knew what it was. We were far ahead in California. But it was not only California. The people in Virginia were advanced on this, and there were two other states, individuals and all of us knew each other. We all recognized the problem of tributyl tin because the French had identified it as a very serious pollutant.

RR: It's interesting. I mean, the Navy was the one that wanted to paint its six hundred ships with this, a dangerous substance. Did that create tensions?

Goldberg: I don't think so. People got curious what one should do with this wonderful biocide which really kept the hulls of ships clean, too clean, but also kills everything else in sight. It had

to be regulated strongly, which it is today. Look. If you're going to kill things on the hulls of ships, you're going to kill things in the water. So it's a Hobson's choice.

RR: So is it enforced beyond California?

Goldberg: Tributyl tin? It's strongly regulated. The EPA is the whipping boy of science, and I'm sort of ashamed to say that, but it is. When you put a lawyer at the head of a scientific organization, that's a tragedy, even though she's pretty.

RR: You were involved in a number of other environmental projects. I wonder if you could talk a bit about some of those, like Mussel Watch.

Goldberg: Mussel Watch came about when I recognized, as other people did, that there was more than one pollutant or two pollutants at any given place, and one was going to have to have a technique to analyze a lot of pollutants easily. It turned out that the mussels were an ideal organism to do this. They're cosmopolitan, they're ubiquitous, and they do concentrate many of the terrifying contaminants in our environment--heavy metals, radioactivities. So I thought it would be a hell of a good idea to just go out and collect mussels and analyze them. That became the Mussel Watch. It was easy to do. You have mussels in most coastal waters, and they concentrate much the same things. It doesn't take any brains to go out there and get some mussels and analyze them.

But now I think it's the wrong way to go. There are newer techniques I hope will be used in the future, techniques involving enzymes. Enzyme chemistry is a method of detecting pollutants. But this is for the next five or ten years.

RR: Was the Mussel Watch program nationwide?

Goldberg: It was nationwide. I'll tell you about it. I conned a friend of mine in the EPA, a man named Eric Schneider, who is a great friend of science. His second wife became Claudine Schneider, who was in the House of Representatives from Rhode Island. She's a neat lady, too.

Anyway, I conned Eric Schneider into initiating a two- or three-year program in the Mussel Watch with a hundred stations around the United States. What we did was to collect mussels once or twice a year at these stations and ship to three analyzing laboratories, one here at Scripps, one at Woods Hole, and one at Port Aransas.

[Begin Tape 2, Side 1]

Goldberg: The Mussel Watch.

RR: Right, right.

Goldberg: Yes.

RR: So why don't we do [unclear] women [unclear] and then come back to that, unless you want to continue with--

Goldberg: No.

RR: Okay. You were just saying that you thought all your colleagues were wrong about the role of women at Scripps. I wonder if you could comment on that.

Goldberg: Well, my sense is, since I've been at Scripps, there never has been a sense that a good scientist is either male or female. In the original days, Scripps was practically--very few women. I think this was just the mood of the times. There weren't as many women with Ph.D.s, and there weren't as many women who seemed to show an interest in oceanography.

My suspicion is that many of my colleagues were biased against women because this was the mood of the times. I thought it was rather tragic, because we had some first-rate scientists here who were women, like Betty Boden Kampa, a first-rate scientist. We had Margaret Robinson, a first-rate technical person. She never had a Ph.D. in marine science, but she certainly did a first-rate job in the assimilation of information. But I am convinced that many of my colleagues were inherently biased against women because they just were unfamiliar with having women around.

One of the greatest moments of my life was escorting a woman who won the Nobel Prize to a concert. Her name was Maria [Goeppert] Mayer, and her husband couldn't stand classical

music, and one night I had the great honor of taking this Nobel Prize winner to a concert. She had had a heart attack and needed assistance in moving.

Right now we're seeing the feminization of all science, especially in marine science. This is an interesting question. I think we're getting a higher proportion in marine science than we are in physics or chemistry. And I can't explain that.

RR: Actually, there were a fair number of women at Scripps in the twenties and the early thirties.

Goldberg: Yes.

RR: And the first woman with a Ph.D. in oceanography, Easter [Ellen] Cupp, got her Ph.D. here in the mid-1930s.

Goldberg: Could be. Yes.

RR: What I want to ask in a way is, do you think that particularly for expeditions, if the Navy had a role in kind of limiting women's opportunities?

Goldberg: Can't speak to that. I don't know.

RR: Because they really weren't around on lengthy expeditions. There were so many expeditions with Scripps with such emphasis on blue-water oceanography.

Goldberg: Well, I took women to sea when I first went to sea. We never thought anything about it.

RR: Overnight? And more than one day?

Goldberg: Oh, yes.

RR: Oh, really.

Goldberg: But you always took two at a time. We didn't do chromosome tests, but we took two women at a time. [Laughter]

RR: Really?

Goldberg: Yes.

RR: Were these near-shore studies?

Goldberg: Near shore. It wasn't for a matter of days.

RR: [unclear]?

Goldberg: These were my technicians. I know their names. I don't think that it's important to give their names now.

RR: Well, [unclear] could name a couple of names.

Goldberg: Okay. Alice Whisenad. I'll spell her last name. W-H-I-S-E-N-A-D. And Binky Baker. I don't really remember her first name but she was know as B-I-N-K-Y Baker, Marjorie Baker, Marjorie Baker. She went to sea with me.

RR: I think that's very valuable because, you know, the names get lost.

Goldberg: I know.

RR: And years later, you know, most people, you know, a lot of people don't tell of the earlier women in the twenties and the thirties.

Goldberg: Right.

RR: And I think they are the firsts in some sense, and so it's very valuable.

Goldberg: Well, perhaps the most distinguished of the early women scientists, in my eyes, was Betty Boden, Betty Boden Kampa. No, there was another one. Oh God, it's an Italian name.

RR: June Pattullo?

Goldberg: June Pattullo.

RR: She worked with Walter and Roger to some extent.

Goldberg: Yes.

NO: [unclear]?

Goldberg: How did I know her?

NO: No, no. I'm just wondering, you said she was really good, so--

Goldberg: Yes, she was. She studied the eyes of marine organisms, and had quite an international reputation.

RR: Well, that's very valuable for us. Why don't we go back for a second. You were talking a bit about Port Aransas and Mussel Watch. If you could continue on.

Goldberg: Mussel Watch was going. The United States had three active laboratories, one at Port Aransas in Texas, one at Scripps, and one at Woods Hole. We analyzed samples from a hundred coastal areas in the U.S. and identified areas where there was high contaminants, usually high DDT or high polychlorinated biphenyls. It was a successful venture, but I quickly realized that this was something that a university should not be too deeply involved in. This is an activity of government agencies, and they could do it better and more effectively than I could. So I got out of it.

But before I got out of it, the technique was adopted by many countries in the world because it was a simple technique to apply and the results could be compared. The people in Washington, D.C.--who were they? Give me a minute to think. There was a group in Washington, D.C., that took over the Mussel Watch and carried it out quite effectively and still carry it out. It's a status-and-print, status-and-trends program.

RR: Apparently these programs that you get involved in, you got engaged a lot in working with a lot of international scientists from various places. I wonder if you could comment on some of the places that you worked and some of the groups you were involved with on an international scale.

Goldberg: Well, yes. Let me enter that in the following way. I had developed a technique to be able to put time horizons, dates, in the coastal sediments. It was called the lead 210 method. I discovered lead 210 as an environmental, natural substance in sediments and in the waters, etc., back in about 1960. This is a radionuclide naturally produced in the uranium series, which allows you to have time scales of up to a century. So this is a wonderful thing to look at coastal sediments.

Well, the first thing I did once I had this, one of the first things I did was to date sediments, get time horizons. They had to be anoxic sediments, that is, free of oxygen, in various places such as Narragansett Bay, San Diego Bay, and a number of inland waters like that where we could get these time frames and then measure pollutants at the various levels in these time frames. This was quite successful.

One of the things I did was, at this time I was hustling Japan. I was interested in Japan's activities against environmental pollution. They were way ahead of much of the world because they had a monopoly, damn near, on environmental pollution. I went over to Japan, and I wanted to see if any improvements were taking place. So I took a sediment core in Tokyo Bay. I had a technique to put time frames in Tokyo Bay, and I had a number of things we could measure. We actually looked at heavy metals, and we looked at the growth of pollution in Tokyo Bay in the last century. It was a very simple technique, lead 210 for dating and, of course, our abilities to core. I also did it in Beijing, too. In the waters of the Summer Palace in Beijing, we also measured pollution as a function of time using lead 210. These were things I just did. I was really horsing around. But we were able to show how these countries were being industrialized by the pollution in their cities.

RR: You said that Japan seemed to be ahead in antipollutant kinds of activities?

Goldberg: No, Japan was ahead in identifying pollution problems. I once argued that every new pollutant had a Japanese name, and it's true. Minimata Bay was first where mercury was identified as a pollutant. Then they had another one. They had a number of pollution episodes because you have a highly industrialized city, a country with people living in small areas, so pollution very rapidly escapes its source point and spreads about the environment.

RR: A lot of this work, I assume, gets you involved with various government agencies or various planning regulatory agencies. I wonder if you could talk about your view of the role of the scientist in working with those kinds of agencies and those kinds of problems.

Goldberg: Well, the scientists provide the information. The application of that information which we can call policy are usually in the hands of government working, although certain government agencies do gather their own data. My sense is that we want to put the best people in government agencies to use this information successfully.

My sense is that originally in the AEC, which became ERDA, we had first-rate scientists who understood what the problems were and could equally as well have traded places with the scientists who were gathering information. That was a wonderful experience to work with these people, because they furnished you money, they appreciated the results, and it was one big, happy family. But as these things became bigger and bigger, we were tyrannized by size, and

the environmental community became bigger and bigger. Look, you have scientific meetings like the AG, where three thousand people came. You don't do science at those meetings.

RR: Besides providing information, were you ever called upon as an expert in testimony?

Goldberg: Yes, yes, yes.

RR: If you could--

Goldberg: Well, I went before Congress any number of times to discuss the health of the ocean. I once wrote a book called *The Health of the Oceans*, and this became popular, only because I did it first. So I was called to testify before various congressional committees on the state of the oceans, the state of the coastal oceans. I enjoyed doing it, as a matter of fact. In fact, I met some very interesting congresspeople.

RR: Were you called upon in ways to help define or design regulations?

Goldberg: Yes, I was involved with inputs to the Clean Water Act. When that was being penned up, I worked with the staff. You see, it's the staff that does everything, as you are well aware, in Washington, D.C. And they need help.

RR: How did you find that work? Is it frustrating because there were delays? Is it frustrating because of the bureaucracy? Or is it very worthwhile, very rewarding?

Goldberg: It's rewarding in a way because you have the sense that maybe science can serve a role to give a better environment for our population. It's as simple as that.

RR: Which of your projects, of these environmental projects, do you think were the most successful?

Goldberg: I think the Mussel Watch.

RR: Why?

Goldberg: Because it's so simple. It's been used by so many people. And even though it may not have stopped or improved the environment [unclear], it made people aware, made people aware that the environment has a certain fragility to it and that we should be aware of its fragility. If we can protect environments by our work, we're contributing to society. For example, I think the ban on tributyl tin was a great step forward. The ban on or the restricted use of DDT was a great step forward.

RR: Let me ask you a somewhat related question in some ways. Some of this involves, one of the things about science, particularly after World War II, is that oceanographers are oftentimes

on committees, military people, government people. What are some of the committees that you took part in for planning purposes or--

Goldberg: A number of committees of the National Research Council, which is the operating arm of the National Academy of Sciences, and these committees usually involved waste disposal in one form or another. I actually called a number of meetings on what are the problems with the disposal of wastes, etc.

I remember one of the projects I enjoyed the most was the social and economic problems of the disposal of high-level radioactive waste. That was very exciting thing to be involved with, because we're building up more and more radioactive waste in the United States. We're putting them in a variety of sites. How do we manage these things? This is still a first-rate problem for our government. And it's sort of fun being involved in these initial discussions.

RR: Besides the NRC [Nuclear Regulatory Commission], can you think of--first of all, when was this?

Goldberg: The NRC was probably the sixties and seventies.

RR: Besides the NRC, can you think of others that were particularly significant? I know you worked with UNESCO to some extent.

Goldberg: Yes, I've worked with UNESCO. I've worked with a group called GESSAMP. You've probably never heard of that. GESSAMP has a very unpretentious title. It's the Group of Experts, G-E, to Study the Scientific Aspects of Marine Pollution. And the problem with both the national and international groups, unless they have a goal, a really well-defined goal besides publication, I don't find them terribly that exciting. You can publish anything, anywhere, at any time you want. But my sense is that if you gather material that is incorporated into public policy, then it becomes worthwhile.

RR: What about in terms of the impact of being on those committees for you as an individual scientist, your career? Is it worthwhile in those senses? It's a lot of [unclear].

Goldberg: Well, I enjoyed it. Let me point out. I think my career was strongly influenced, one, by coming to Scripps, by working with people like Roger Revelle, who elevated us to levels that we would never have achieved if we'd gone to Oregon State [University]. I hope [unclear].

NO: Off the record. [Laughter]

Goldberg: The power of this place to take ordinary scientists like myself and to elevate them to interesting heights in science, it's wonderful.

RR: You talked about that, I know, in a little article that you gave as a sort of biography. To some extent, Brown did that, too, didn't he? You say at one point that you got a sense of sort of

the value of science at Chicago and, I guess to some extent, Scripps. I wonder if you could say what you mean about the value of science.

Goldberg: Well, you're doing something worthwhile. You see, I think people who sell shoes don't do--but someone's got to sell shoes, and some people enjoy selling shoes. It was interesting, the psychology of foot fetishes. Somehow I got--what is it--intoxicated at Chicago with the value of science. You see, I graduated from University of California in '42, went into the Navy. I thought I was going to end up selling insurance. I didn't know what I was, what the hell I was going to do. And then the GI Bill came out, and I said, "Aw, maybe I'll study some more chemistry," and I went to Chicago. Well, this was a whole new world for me, because you saw people accomplishing, people with humane views about life.

RR: I don't know if you want to name names or anything like that, but are there people these days that you can of working in science that are still operating with those senses of the [unclear]?

Goldberg: Oh, there are a lot, there are a lot. Let me give you one, a man, Matthew Messelson. Does that name mean anything to you? M-E-S-S-E-L-S-O-N. He's a professor at Harvard. He's very much concerned about biological warfare, and this guy's a real wheel and very good.

RR: I was thinking acid rain [unclear].

Goldberg: Acid rain, yes. When I try to remember a name, I die. But you still have a lot of people concerned about the toxicants in air and CO levels, carbon monoxide levels. I think the interest in a healthy environment remains steady. I don't say it's more today or less today. The important thing is you still have people that are worried about it who are good, who are good scientists.

RR: I know you did quite a few years of teaching, and I wonder how do you convey that, a sense of values to students?

Goldberg: Well, let me tell you. I didn't do many years of teaching. I produced about twenty graduates of Ph.D.s, and I think they're worthwhile. But in 1970, a great change took place at Scripps. We had new people in charge, and I didn't couple with them. So I totally lost all interest in Scripps, and they lost all interest in me. So until I retired, between '70 and my retirement age, which was a period of twenty-five years or more, I had very little contact with Scripps. The directors didn't especially find my way of life appealing.

RR: But did you try to convey to your students that you did have, this commitment to--

Goldberg: Yes, I taught a course. I have very few students in it. I taught a course in environmental marine chemistry, in which I tried to point out that we were doing a lot of things to the environment we might not wish to do, that we should take advantage of these tracers that we're putting into the ocean system and to expand our knowledge about how marine life works.

A lot of my students did go into environmental science. By "a lot," maybe five or so, I don't know. That's a lot to me.

RR: One of the things that we found as a commonality on many of the people we've interviewed is an experience in the Navy. I didn't ask you about that earlier. What was it you did in the Navy?

Goldberg: I joined the Navy to avoid the draft. It was in 1941, and a Navy officer came to the University of California and said, "Look. You guys are going to be drafted. You're going to become soldiers and privates. Why don't you become an officer in the Navy?"

I said, "Why not?" So I joined with a thing they called ordnance volunteer specialist and became an officer, having trained one month in Swog's Neck in New York. You must know Swog's Neck. And became a degaussing officer. I was concerned during the war with demagnetizing ships, which was no big deal, and also with HECP, Harbor Entrance Control Post, where we listened for submarines in advanced spaces. I spent over, way over two years in the South Pacific in Talogi near Guadacanal and places like that. So I did have experience with the Navy. I wasn't intrigued being in the Navy, but officers in the Navy had a nice life. We were bombed occasionally, but the bombs was more like going to a football game and watching who caught the ball and who didn't.

RR: That's interesting. I guess it didn't really come into your career, but harbor control and harbor defense is obviously--

Goldberg: They were related to what I later did in life, but when I was doing it, my sense was to get out of the Navy as fast as possible.

RR: Let me ask you one last question, the one we usually sort of finish up with. That is, what should I have asked you about that I haven't?

Goldberg: Oh, let me think about that a bit. What should you ask me about? Let me think for just a moment. You got all the important things that I think I know about.

RR: Well, then one other last question. Was there anything that you wanted to do in terms of being assigned a big project at any point in your career and didn't get a chance to do it, you would have liked to have?

Goldberg: Yes. Late in my career I identified a terrible substance that's polluting our environment, and this is plastics, plastic debris. I made the argument that plastics are like diamonds, they're damn near forever, and they're coating our sea floor. You can see them on beaches. They inhibit plant activity because they cover up areas of the beach, and below these areas that they cover up the chemistry changes. They become anoxic. They become very nasty. I wish I could have spent more time working on plastics. Tragically, very few people work on plastics. It's not an exciting area. I think it's one of the most important pollutants we have

today, because, as I say, they're like diamonds, they're forever. They ain't going to go away. And we're adding more and more plastics to the oceans.

RR: Did you not get funding for it or [unclear]?

Goldberg: I didn't get funding for it. I put in for a couple of proposals to work on plastics. As time went on, I lost friends in Washington by saying the wrong thing at the right time or the right thing at the wrong time. Toward the end, although the Navy supported me, I walked out of my position with Navy money. I saw the writing on the wall that I was going to lose support in the future.

RR: Do you have any sense if there's going to be any more support for anybody doing the work on plastics? It sounds like a very important--

Goldberg: I can't answer to that. I don't know.

RR: Well, thank you very much.

Goldberg: Did you get what you wanted out of me?

RR: Yes.

[End of interview]