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

Transcript of the Videotape-Recorded Interview with
FRED SPIESS

Conducted at
Scripps Institution of Oceanography
The University of California San Diego
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Interviewers: Naomi Oreskes and Ronald Rainger

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FRED SPIESS

February 18, 2000

Naomi Oreskes and Ronald Rainger,
interviewers

[Note: There is a humming recorded throughout this entire interview, and the volume level of the interviewer's microphone is very low, sometimes inaudible. Every effort was made to provide an accurate transcript. Where this was not possible, [unclear] is noted in the transcript.]

Naomi Oreskes: Hello, I'm Naomi Oreskes. This is an interview with Fred Spiess. Today is February 18, 2000. Last week you told me a great story about how you first got interested in the ocean when you were a submariner during World War II. Can you tell us that?

Fred Spiess: Sure. I suppose I had to have been interested in the ocean to be in the submarine business in the first place, but I first really became aware of internal waves in the ocean when I was battle station diving officer on the submarine *Tarpin* operating off Japan, where there was a very sharp boundary between the warm surface water and the cold lower water. I can remember days when we'd be holding at a particular depth and I'd have to pump out a bunch of water, and then about twenty minutes later you'd flood it back in again because our buoyancy was changing because this interface was moving up and down relative to where the submarine was. We had to hold a particular depth in order to be able to have the periscope up to see what was going on up above.

But that was sort of the nice thing about having a good basic physics eduction that I did have at Berkeley as an undergraduate, came out of class of '41, and you go off to submarine school, and clearly all you learned about buoyancy and things of this kind, that's all right there in the practical, everyday world.

NO: So you understood the connection between basic physics and the problem of maintaining a submarine at the right depth.

Spiess: That's right, yes.

NO: Did other people understand that as well, or did you feel that because of your background--

Spiess: I think that people learned it one way or another. We had some instrumentation that was installed sort of after the first year or two of the war that would tell us what the temperature was outside. The recorder that drew the temperature versus depth curve had a little chart on it that would tell you how much water you had to pump in or flood in or pump out in order to maintain your neutral buoyance, so that you could look at that little chart and get a pretty good idea of what was going on.

Of course, we were all well aware that that had something to do with sound propagation, as well, because if there was a sharp interface, that meant a sharp change in speed of sound. So sound waves would be refracted up or down, depending on the circumstances. It was always get under the layer so that you could hide from the people who were trying to find you, either by listening or by echo-ranging on your submarine.

NO: That's one of the things I wanted to ask you about. One of the important things that oceanographers during World War II, people like Maurice Ewing and Joe Worzel, was to work on that problem exactly and to develop the bathythermograph in order to determine the position of the thermocline that affected sound transmission. During the war, that was highly classified, but how much did you know about that, and how much of that information actually got to people like you, yourself, who were actually working in submarines during the war?

Spiess: It was pretty good linkage. A good example would be that I went back in the middle of, or late 1943, to New London, Connecticut, and was part of a crew of officers and men to put a new submarine into commission at New London, Connecticut, and one of the things that happened during that time was while we were fitting out and doing our training exercises and things of that sort, that one day somebody came--in this case it was Bill Schevill, as I learned later, who was biologist oceanographer at Woods Hole [Oceanographic Institution], and he rode with us for a whole day just to explain what the significance of the bathythermograph was to those of us who were on the bottom end of this problem, as well as the top end, which is where a great deal of the emphasis lay in terms of people trying to find German submarines and we trying to not be found by Japanese antisubmarine craft.

NO: That's interesting. So you knew, at a pretty young age, you knew quite clearly the connection between oceanographic research and tangible, real-life military operations.

Spiess: Yes. Well, we really knew, but we didn't know very much about what was known or not known. I couldn't have written a proposal to go off and be funded to do some research at that time in the game.

NO: So you didn't know this was a brand-new discovery? You didn't know that?

Spiess: Well, I'm not sure it was such a brand-new discovery, because things were known in World War I, even. In fact, I started off my paper for the symposium last week with a picture of the 110-foot wooden ship with the gasoline engine in it, which my father was the sonar person, going across as part of the escort for convoys going across the Atlantic Ocean. So things were indeed known. It's a matter of degree, always. We still are learning more about how to do the best kind of signal processing to find submarines, a great deal of it out of the work of the Marine Physical Laboratory, in which I've been practically all my life, has been focused on underwater acoustics, sonar systems, and the fancy kinds of signal processing you can use to find very quiet submarines in noisy environments. So that's been kind of a theme throughout.

It's an interesting thing, because you start in very often as a physicist doing this kind of thing, because it's just plain good classical physics. Lord Raleigh wrote the book a long time ago. But what you find is that what you really have to know about is the environment. So maybe gradually or quickly, you become more of an oceanographer geophysicist using acoustics maybe to learn about the environment itself, and that's sort of been the pattern that my career has followed. I went from devising new kinds of submarine communication and detection systems in the fifties to using acoustics to find our way around as far as learning about the ocean floor, clear up today where we're using acoustics to measure how plates are moving in the plate tectonics world.

NO: Can you explain a little bit more, though, what you mean by saying you need to know what the environment is? What's the distinction between what Lord Raleigh did and what you needed to know in order to actually solve a problem in acoustics transmission.

Spiess: Well, Raleigh wrote the equations, and there are some places in there where you have to put a number for the absorption as a function of frequency or pretty soon you're around to what happens if there are little patches of water that are different temperature than the surroundings and what does that do in a statistical way to the sound propagation. You're also concerned with what the sea floor is like. It's that part of it that has attracted me most in the last couple of decades of my research life. You need also to know what the background noise is like. The ocean is full of noise, and the more you know about the characteristics of that noise, the better off you are as far as finding a target buried in that noise.

NO: So let's talk about, then, at the end of the war, you went back to do your graduate work in physics.

Spiess: Right.

NO: So tell us about that. What did you work on? What were you doing in graduate school?

Spiess: Well, when I went back to graduate school, I went with a situation in which I had the basic physics and I also had the submarine part, because it had been about four years of doing submarine war patrols and taking our licks with the depth charges and sometimes getting the torpedoes to blow up the targets. That played a role in the time that I was a graduate student in physics.

So I was in kind of a combination situation. I was commanding officer of a submarine reserve unit that met weekly and went off to active duty for a few weeks every year. At the same time, I had become involved in a research group that was into trying to understand what you could about the nuclei of atoms, using what were then the state-of-the-art accelerators at Berkeley. In that context, we worked on--let's see if I can even remember what the topic was for my thesis. It's a little tricky.

NO: You mean one day one finally forgets? [Laughter]

Spiess: Right. Well, particularly because once I'd finished, I was in a very good research work. Emilio Segré was my thesis advisor. Once I was out of there, they were able to get the Nobel Prize. So I then, however, was still interested in the submarine business when I finished, so I was looking around for something that would combine both the nuclear physics and the submarine world. So I took a position at the General Electric laboratory that was working on nuclear powerplants for submarines.

NO: So you saw it as an explicit way to connect your experience in submarines with what you knew about physics.

Spiess: Yes, because I was still very interested in the submarine part of it, and yet I had developed the interest and knowledge in the nuclear physics side of the world. I went there and spent about a year in Schenectady [New York], and toward the end of that year, the summer of '52, my wife and I came back out to California, which was home for us, as one of her sisters was being married. While I was out here, why, one of my friends said, "Look. This group down at Scripps [Institution of Oceanography], down in San Diego, is looking for a physicist who can go to sea."

I guess the contact was Hugh Bradner, who was a member of the physics group up at Berkeley, but had ties down here, partly because of his work with the wetsuit initiation and things of that kind. He knew that the Marine Physical Laboratory needed a physicist, and he had asked one of the people he knew somewhat better than he knew me, but who was a good friend of mine, if he would come down and talk. My friend said, "Well, sure, I'll go, but Fred Spiess is in town, and he's a much more likely one to be interested in this kind of thing."

So the two of us came down. The university was different in those days. We talked to Roger Revelle and to Carl Eckart, who was the director of the Marine Physical Laboratory at the time, and went away, and within a week I had an offer to--

NO: Oh, that is different. [Laughter]

Spiess: --come down here and be a physicist in the Marine Physical Laboratory. It certainly didn't take me very long to decide that being able to come to a place like this and where I could run a project in a small environment would probably be a lot more satisfying than being one of 2,000 people building a nuclear-powered engine for a submarine, which is what I was involved in at the [unclear] laboratory. So that was how I came to be here, but it's a pretty complicated background, a very fortuitous kind of arrangement, I guess is what you'd really have to say, because it isn't everyone who can, in fact, take quite disparate experiences from their lives and find a thread such that you can weave these together and have a very satisfying kind of experience, which is what life has been for me, fortunately, for lo these many years, since 1952, when I came and joined the Marine Physical Laboratory.

NO: And you have to give Sally credit for dragging you to the wedding. [Laughter]

Spiess: That's right. Yes, yes. A lot of credit is due there. I think one of the things about being an oceanographer is that if you are an experimentalist, which I am, I'm not bright enough to be a theoretician--that was the catchphrase in the physics department. There are only two kinds of physicists: there are theoreticians and the ones who wish they were theoreticians.

NO: It doesn't sound like you fit into that. It sounds like you were very excited to have the opportunity to build things hands-on.

Spiess: I like to build things. It's more fun if you build things in a context in which they're going to be very useful, and you can not only build them, but you can take them out and use them and come back with some more information that helps you define new questions. The kind of experiment that I liked most to be involved in, in fact, is one in which there is some kind of basic science sort of question, but there is also some kind of applied aspect as well, so that what you learn feeds both into the science community and helps that to move forward, but it also may feed into something more practical kind of aspect. Of course, that's why it was fun to be in the Marine Physical Laboratory, where there have always been some thoughts in the back of our mind about how things might relate to the Navy. That was way up in front of our mind in the fifties in the Marine Physical Laboratory, and has become less and less, but it's still there.

NO: Tell me about MPL. What was MPL like when you first came there, and what was Carl Eckart like?

Spiess: Well, the Marine Physical Laboratory grew out of the University of California Division of War Research, which was a pretty big establishment in San Diego during World War II. When the war ended, much of talent in that lab migrated back to the universities from which people had come. A lot of the engineering and technician talent went into the then new Navy Electronics Laboratory. But there were a few scientists who wanted to stick with the underwater acoustics world, so the university and Roger Revelle, who was on active duty in the Navy still, made a close arrangement that the university could establish a laboratory, which was the Marine Physical Laboratory, directed initially by Carl Eckart, who was a well-known theoretician, I guess.

In part, when I came here, one of the things in the recruiting world is I had t make my peace with leaving the world of nuclear physics and real physics, you know, and being recruited by Carl Eckart, who I knew because he had proven the identity of Schroedinger and Heisenberg formulations of quantum mechanics back in the twenties--

NO: That's a pretty significant [unclear].

Spiess: So I figured if he could do this, I could do it. [Laughter] It was honorable enough.

NO: Did he ever talk about how he made the decision to--

Spiess: No.

NO: He never talked about that?

Spiess: In fact, my interactions with Carl were rather sketchy, because at the time that I came, I came to replace Leonard Liebermann, who was going off on--I forget whether it was a Guggenheim or a Fulbright, a fellowship to work elsewhere for a year. Carl Eckart, in fact, at the same time went off on a sabbatical leave to go to the Institute for Advanced Study in Princeton area. So when I came, Carl was here for a week or two or three, and then he was replaced on a temporary basis by Sir Charles Wright, who had been head of the Royal Navy Scientific Service and he ran the Admiralty Research Lab during World War II. So my introduction to Eckart was pretty much confined to the recruiting process in which I was persuaded that this was a good thing to do, and then as soon as I came, he left.

NO: [Laughter] Did that make you suspicious?

Spiess: When he came back, he decided he didn't want to be director of the Marine Physical Laboratory anymore. He had been doing a lot of administrative work, particularly in the latter days of the University of California Division of War Research as well, so he went off on his sabbatical, then he really moved out of the administrative world, not for terribly long, because once UCSC was established, I think he was one of the first vice chancellors for whatever on the upper campus. Of course, he had been the director of Scripps, too, as the interim between [Harald] Sverdrup and Revelle.

This meant that my upbringing in the administrative world had a sort of British tinge to it, because Sir Charles, who was in himself a fabulous character, he had been a member of [Robert F.] Scott's expedition down in the Antarctic and had wintered over and, in fact, led the party that discovered Scott's body at camp. So he had a lot of stories to tell, and it was kind of amusing, because he had gone there as a young geophysicist to do gravity measurements, swinging pendulums. In fact, I have one of the pendulums that he swung down in the Antarctic.

Since I became involved a little bit in the gravity-measuring business in the mid-fifties using American submarines, in collaboration with people at UCLA, that sort of was a nice aspect of it from my point of view. But one of my first few trips to Washington to be on a committee or something of that sort usually managed to coincide with the time when he went to Washington, and so I had a chance to meet some of the people that he knew. It was a good way to start to have a much bigger picture than you would have if you were just running a small research group in the Marine Physical Laboratory, although that had its own nice part to it, from my point of view, because there was very close interaction between the laboratory and the operating submarine people here in San Diego at that point.

NO: So tell me about that interaction. When you say there was a close interaction, what does that mean? Did you sit down and talk with submariners about their problems, about what kinds of things they were worried about?

Spiess: The project that I took over from Liebermann was one where we were bringing to bear some new acoustic techniques for use in submarine sonar activity, and submarines by then had shifted their mission from being a commerce-raiding kind of thing, what submarines were during World War II, to being an antisubmarine. Submarine-versus-submarine game got to be the big deal.

NO: And you knew about that and talked with him [unclear]?

Spiess: Yes. We actually had equipment installed on submarines, and I would go out. My version of an oceanographic expedition in that era was to go out for a week with a submarine that had our experimental equipment. Some other submarine would be out there pretending to be the target, and we would see what kind of detection ranges we could achieve. So we were just living with the people who were there, and we had division commander, local submarine division commander assigned as our liaison person, so it was a very tight kind of loop that we had. I guess there's more about that in the talk that I gave. [Interruption.]

NO: And what about your relationship with Scripps at that time? You have this quite close relationship with the Navy. How is your relationship with Scripps?

Spiess: Well, the Marine Physical Laboratory was, in fact, established separate from the Scripps Institution of Oceanography. It reported directly to the president of the university up in Berkeley in that time. When Carl Eckart became director of Scripps as the interim between Sverdrup and Revelle in '48, he was also the director of the Marine Physical Laboratory. So he decided that it made sense for the Marine Physical Laboratory to become part of Scripps, which it did at that time.

We were pretty much everybody was down at Point Loma in buildings down there within the Navy establishment, and the relationships were rather loose with the rest of Scripps. However, that was something that varied quite a bit from one group to another within the Marine Physical Laboratory. Raitt's group, for example, he had come to UCDWR, had learned how to use explosives to--

NO: This is Russell Raitt.

Spiess: Russell Raitt. To study sound propagation in the ocean, but he was basically a chief physicist. So as things wound down, it became clear that you could use these same techniques to study the crust of the earth under the sea. So his program, which was part of the Marine Physical Laboratory program, was also very tightly tied into the geology and geophysics that other people at Scripps were moving forward at that time.

There was also a close relationship in the geology geophysics world between the Navy Electronics Laboratory and Scripps, which resulted, in fact, in one of the more elegant and eminent Scripps marine geologists, Bill Menard, who started as a scientist in the Navy Electronics Laboratory group, and then became better and better known for his insights and was recruited away to be part of the Scripps Institution of Oceanography. But the relationship in the technical sense was quite close, also, in that there were some of the members were actually regular faculty members of the University of California.

NO: How did that work? Did Roger Revelle arrange that?

Spiess: Well, in the arrangements to set up the laboratory, the-- [Interruption.]

NO: Sorry about that. Start again.

Spiess: Let's see. What were we doing?

NO: About the faculty appointments.

Spiess: When the Marine Physical Laboratory was first set up, part of the agreement-- [Interruption.]

NO: So tell me about the faculty appointments and how that worked out for people from MPL to also be appointed on the faculty at Scripps.

Spiess: Well, they were appointed in the faculty of the University of California, is really what they were. The arrangements for establishing the Marine Physical Laboratory included the fact that the university would indeed come up with three full-scale faculty billets for members of the laboratory, and Eckart and Russell Raitt and Leonard Liebermann were the people who had those billets.

NO: How was that decided?

Spiess: It was decided that Eckart would, because he was in charge. [Laughter] The other two were pretty much--this is before I even arrived on the scene, so I don't really know for sure, but the other two, Raitt had been in the organization for a long time and was really a very good, insightful person as far as understanding what you could do in the geophysics world. So it was logical that he should do that. Liebermann had been at Woods Hole during the war and was recruited by Eckart because they had some kindred interests in--they had some to do with the ocean, but had to do with the fact they were both interested in acoustics as it can be used to study chemical reactions and related problems. That had turned out to be important because that was the clue to solving a riddle people had been up against in the early stages of the war when they were doing real quantitative sound propagation measurements and found that the energy was absorbed a lot more rapidly in the ocean than they had expected. If you just had pure water, it would not have been that way, and it turned out it was the chemicals in the sea water that were the culprits.

NO: You mentioned Bill Menard. Some people have said that Roger Revelle raided the Navy Electronics Lab faculty. Is there any truth to that?

Spiess: Well--

NO: Bob Dietz was also at NEL, right?

Spiess: Yes, but he wasn't on the Scripps faculty.

NO: But later on he was, wasn't he?

Spiess: I don't think so. No.

NO: What about Menard?

Spiess: The raiding was a raid of one, I believe.

NO: [Laughter] A small raid. Selective raid.

Spiess: I think Scripps was, in the long run, a better environment for Menard. Although for a long time there was a group at the Navy Electronics Laboratory led pretty much, I guess, by Gene LaFond, who was a physical oceanographer who had been a Scripps student, Scripps Institution student, and that group was really a very powerful group. There were both the sound propagation people, physical oceanographers, and sea-floor geologists. That's where Dietz was and some others who made very good careers for themselves.

I can remember Bill Menard went off at one point later on in his life, not a lot later, to some job in Washington. This was not when he went to be head of the Geological Survey, and while he was there, he put together some kind of compilation of which institutions produced the most marine geology and geophysics, biggest output, and at that time my impression was that his result was that Scripps was first and Lamont [Lamont-Doherty Geological Observatory] was second, and the Navy Electronics Lab was third.

NO: Interesting.

Spiess: So they were really a very powerful group. But the Navy's view of that laboratory gradually changed to be much more engineering oriented, rather than being oriented and learning about the environment. So that group gradually came apart. Menard would have wound up at some university sooner or later.

NO: [unclear]. [Laughter]

Spiess: Right. [Interruption.]

Ronald Rainger: You know Charles Wheelock.

Spiess: Oh, yes. Sure.

RR: Before he came to Scripps, he did the Navy's internal review for about a year, of NEL, and I don't know who--you're absolutely right that it's just Menard, but then Revelle brings Wheelock to Scripps, to IMR [Institute of Marine Resources], and I guess I thought he had some inner thoughts through the information he had gotten through Wheelock, that he might be able to get some additional people from NEL. Maybe that's not true.

Spiess: I don't know. My guess is that he would have been--if I think of people who were there--

RR: They don't end up here.

Spiess: They don't end up here.

RR: Gene ends up here. No, no, he's at NEL for the whole time.

Spiess: Yes. He retired from there. The others went off to other jobs.

NO: Dana Russell.

Spiess: Dana Russell I knew. I carpooled with him, in fact, for a little bit. There was Bob Gill, Dave Moore--

RR: Gordon Hamilton. Not Gordon Hamilton.

Spiess: No.

RR: Ed Hamilton.

Spiess: Ed Hamilton. Ed stayed with the lab throughout, and he's probably the one who would have come closest to finding a real place here. He would have been a good person in the Marine Physical Laboratory, for example. But when you have a limited number of billets, you really have to be kind of careful about how many you take.

RR: I think you're right.

Spiess: I had not been aware of the Wheelock NEL involvement particularly. He was Vice Chief of the Bureau of Ships and in charge of ship construction. That was the way it went.

NO: I have to say something. I don't think two hours is going to be enough. We're going to have to do more. [Laughter]

Spiess: We've sort of used up a lot. We've not even started. [Interruption.]

[Begin Tape 1, Side 2]

NO: [Discussion about the volume of the recording.] Let's not worry about it.

Gravity. Tell me about gravity work and why that was important.

Spiess: Well, the measurement of the earth's gravitational field, as it varies from one point to another, can tell you quite a bit about the distribution of the density of materials that are down in the crust below. It's a technique that's used very much, particularly was used in early days in oil exploration, for example, because there were places where there were big discontinuities in the density of material down below, and oil would concentrate in those salt domes, so that there was a lot of interest in that sort of thing.

The measurement of gravity at sea is a kind of hard thing to do, because when you measure gravity, what you're really doing is weighing something. If you're out at sea and the ship is going up and down, then the apparent weight, it just fluctuates all over the place. Since in order to do anything useful in the geophysics world you have to measure two part in 10^5 or something like that.

There was a Dutch geophysicist, Vening Meinesz, who figured out a way of doing this in a slightly moving environment, and he devised this because of the--in fact, he was trying to make gravity measurements in Holland, and the underlying terrain was itself not very stable. He parlayed that fairly quickly into being able to go out in a submarine, have the submarine dive down deep enough that the wave motion was much less than up at the top, and go ahead and make measurements there. In fact, the Dutch Navy allowed him to ride their submarines. And subsequently in the U.S., Ewing and Hess and some others did this in the thirties with U.S. submarines. So there was some interest in continuing that kind of observation.

The Institute of Geophysics up at UCLA, Slichter, very young institute at that time, managed to inherit or obtain on loan a set of these pendulums. When these people needed to have a submarine, however, I knew where the submarines were, so we were able to make a deal between the two institutions that we would collaborate in these measurements. That was important to Roger because he was a little bit put out that Slichter was invading the ocean, which he felt was his territory.

NO: Not exactly [unclear]. [Laughter]

Spiess: So we did indeed have a modest gravity program in which I was a collaborator with a succession of people up at UCLA.

NO: Was the Navy interested in that, though? Because I know when Vening Meinesz does his early work, the Navy's not particularly interested; they just let him ride.

Spiess: That's right. Yes. I think that in the long run, the Navy became interested. This would have been after the time in which I was involved. Because the small-scale variations in the gravity field are sensed by some of the navigation things for long-range missile, and so really if you're going to find a place out there somewhere fairly accurately at very long range, you need to know a lot about the details of the gravity field. So the Navy became interested.

NO: Once they'd become involved in missile guidance.

Spiess: Once that sort of thing came along.

NO: But you didn't actually work on that?

Spiess: I was not involved in that, no. That was kind of peripheral, almost, because my real involvement in the fifties was with underwater acoustic sonar systems and things of that sort, and that was what led--well, the project in which I was with Leonard Liebermann, took over from him when he came back. He did not take that project back, and we've moved forward to where we had a system that was sort of a prototype on a submarine that was likely to go far away from San Diego, and the Navy decided that it would be a good idea to send this submarine out to the Western Pacific and for them to be able to use the sonar system. Since they couldn't just pick up any civilian and send them out on this, so since I was the project leader anyway, I went to active duty for three months on the *Black Fin*, and went out on this run.

My one-year-old son thought I was on the water taxi all this time because that was the last he saw of me. In San Diego there used to be a real water taxi service that went out, because there were a lot of Navy ships tied up alongside on tenders, destroyers, and submarines both. So when asked, he said, "He's on the water taxi."

NO: For three months. [Laughter]

Spiess: For three months. [Laughter] But that sort of put some impetus into our thinking in the groups of us at Marine Physical Laboratory who were doing the underwater acoustic sonar things. Our involvement in the system that was on the *Black Fin* but which we put on a few other submarines as well sequentially, led to some of the major signal processing equipment developments that Vic Anderson, who was my colleague in the Marine Physical Laboratory for many years, was the deputy director of the lab, but a very brilliant electronics and signal processing person. He came up with several major advances. I guess I was back to my submarine interests, exposed to the submarine world a lot more, because my earlier times had been during World War II, and now it was a different game.

It was in that era that I came up with a way of--let me go back a second. If a submarine is going to find another submarine, usually you don't want to use an active sonar system because then the other submarine finds you. So you rely on information. All you have is directional information. So there was a question of how you could use directional information only and still find out where your target was and how fast it was moving. There were several attempts to do that, but they all left us a fair amount of ambiguity in the answer.

Somehow after this *Black Fin* tour was over, I thought a lot about that problem and suddenly it just jumped out clearly that it was just a simple algebra problem and that if you have four equations and four unknowns, two of which are velocity components and the other two are position, then you can solve this. The problem was that people weren't maneuvering the ship in such a way that you could come up with a unique solution with this set of equations. So it turned out that if you made some drastic maneuvers with your submarine while you were gathering the information, you could indeed solve the problem.

That probably would have been just an interesting artifact or something, except for the fact that one of the officers who had been at sea with me on the *Black Fin* had been transferred to the local Navy sonar school here in San Diego, and so he was running courses for submarine officers on sonar systems and how to use them, and so the two of us got together, and he became the conduit by which this became an interesting and, in fact, quite a useful thing. I was kind of amused. I occasionally will run into some lieutenant commander and he'll say, "Oh, are you any relation to the Spiess ranging method?" "Yeah, I am."

NO: And he said, "Oh, I would have thought you were dead." [Laughter]

Spiess: Right. [Laughter] Well, and I ran into some of my friends, younger people who were in town for a Navy meeting just last week, and one of them was saying, "You know, historical thing here," because it still is a concept that underlies the way you do this in terms of passive detection or passive localization.

NO: It must have been rewarding for you to make that contribution.

Spiess: It's kind of nice, yes, yes.

NO: 1954 is a kind of tough time in world politics. Were you frightened, going on active duty again for three months in a submarine?

Spiess: Well, on a relative scale--

NO: Or don't you talk in those terms?

Spiess: On a relative scale, nobody was going to come around and depth-charge me or whatever. Compared to doing war patrols in World War II, I was already living on borrowed time. So it was no big deal. In fact, it was sort of an interesting aspect from a family point of view, too, because one of the things, if you're an oceanographer, a seagoing oceanographer, obviously you go to sea. That's the definition. That means that you're away from your family, and if you're pretty energetic at it, you may be away from your family quite a bit. Sally has stuck with me through all of this time and managed to prosper in her own inimitable way, and we've always said it was because we started out--we were married in 1942, and so she was at college during times that I was away for very long times and under circumstances that were quite a bit more chancy than going off on an oceanographic expedition. So it meant that our family life didn't have to be reorganized to accommodate this. That was just a natural part of the game. "Where's Daddy?"

"He's off at sea somewhere." So that was, I think, part of the sort of family side of the support that it takes to run a really satisfying life.

NO: Just looking at the 1950s and that whole period, would you say your work on acoustics, was that the most important thing you did, or the most satisfying? Are there some other things that stand out?

Spiess: I think that was, yes. We did a number of things. I invented an underwater communication system and a variety of things. That was pretty much what our corner of the Marine Physical Laboratory was all about. There were other people--Vacquier--doing geomagnetic work, Raitt doing seismic refraction work, joined by George Shor at an early time. Those were our links into the more basic side of the world.

If there had been an engineering department here in the early stages, why, we would probably have been allied with that group more. In fact, once UCSD was established, Vic Anderson became a key figure in the electrical, computer, engineering department.

NO: Let me move you a little bit now to the sixties and ask you about *Flip*. *Flip* is a ship that you designed and hoped to build, I guess. It's a ship that flips.

Spiess: Yes.

NO: Tell us about that. What possessed you to design a ship that flips on its side?

Spiess: Well, we usually say it stands on end. [Laughter]

NO: Sorry. Okay. Stands on end. [Laughter]

Spiess: That came out of the Navy thing, because I was involved in some committees that had to do with the generation of a new Navy missile. It was a missile that you could shoot out of a tube, a torpedo tube on a submarine. It would come up to the surface and fly through the air, then come back down someplace else, and something on it would go "bang" and you'd sink a submarine at the other end.

NO: That's the Polaris?

Spiess: No. Polaris was for a shore bombardment-type thing. But this was an antisubmarine thing. It was called Subroc, which is not a very imaginative name.

NO: Tells you what the purpose is. [Laughter]

Spiess: The question was, how well would you know where the target is. Obviously, since again we're dealing with what's the direction, what's the azimuth, what's the bearing of the target, the question, how the environment would confuse you or make you have big errors in your estimate really had to do with sort of two things. One is that as the sound would come toward you in water, if there were inhomogeneities in the sound field, then the sound would be wiggled back and forth in the direction it might be going, so that the direction it might arrive at your receiver might be somewhat different than the real direction, geographic direction to the target. To some extent, that's like a twinkling of stars. It's the same kind of thing, inhomogeneities in the environment at the speed of light, so that refracts the light rays around it, and stars twinkle. The other thing is that when sound bounces on the sea floor, if the sea floor is horizontal and smooth, why, then the sound arrives at the direction that is relevant to where the target is. If the sea floor is sloping, it doesn't.

So we became interested in how these environmental things, how one could measure what the environmental parameters were that were relevant, how well could this system perform. We were funded then to start some research to learn about this, and *Flip* was funded so that we could have sets of acoustic receivers down at the bottom of *Flip*, down about 300 feet down in the water column, at the same time the top end of *Flip* would be up out of the water, and you could have an optical or radio-type line of sight to some ship that might be operating a sound source, so you could make a comparison between the direct path in the atmosphere and the path in the water and see how closely they agreed. So that was why *Flip* was funded in the first place.

I think that we did something there that I've come to--I didn't think about it quite explicitly at the time, but has been a major element in how I think about how to do experiments, how to build new things to do experiments at sea, and that is that it's nice to have something that will solve a particular problem, but it's also nice to build it in such a way that it can be modified so it can solve other problems. Actually, in that era our laboratory built *Flip*, which was a thing that we could put people on, and electronics, and tow out to sea in a horizontal mode where we wanted to work, we'd flood the back end of it and it would stand up on end and would sit stably in the ocean, as well as having equipment down at 300 feet or so, which is where we wanted it. So it became a good platform for hanging things further down into the water because it wasn't bobbing around the way an ordinary ship would.

The other thing that came out of this same program was--well, I should say that because we built this so that it was quite flexible, and we didn't have a lot of equipment on it, but you could have people on it, and we had spaces for electronic racks and things of that sort, so that as other experiments came along, we could do a whole variety of things. I had a pair of students in succession who studied internal waves in the ocean, using *Flip*. One of those people is still here right now as a professor.

NO: Who is that?

Spiess: That's Rob Pinkel. We did some other wave-type experiments. And *Flip* is still being used in both physical oceanography and underwater acoustic experiments.

NO: How long did it take from the time you first had the idea for *Flip*, till the time you actually [unclear]?

Spiess: It's hard to say, because I don't know when I had the first idea.

NO: Roughly, the time when you got serious about it.

Spiess: I can remember there was a Navy summer study which must have been in '56, called Nobska. It was held back at Woods Hole. I can remember discussing this kind of problem with Allyn Vine at that time. He was a very imaginative physicist/engineer, whatever, at Woods Hole. He had this bright idea that one could have a stable platform in the ocean by taking an ordinary military submarine and turning it up on end, and it would be a manned spar buoy [?]. That's what we called them.

Well, when the bearing accuracy problem emerged in the Subroc context, which was like probably 1960, that was in the back of my mind. In fact, some of the early studies that we made, we actually went through the business of how complicated would it be to take a surplus submarine and just turn it up on end. That turned out to be a pretty messy game, because there was a lot of stuff inside that just wouldn't stand it. So we decided it would be better just to build something from scratch, which turned out to be the case. So the idea that you should be able to go from one position to the other, from horizontal to vertical, I guess really was inherent in the business of being a submarine at all. You could be a surface ship or a submerged ship. This was a surface and a half-submerged ship.

The ballast tank system looked an awful lot like what I grew up with during World War II. The systems we used to blow the water out were pretty much--we had a high-pressure air system and low-pressure blower, and these are things that submariners just naturally know about. So it worked out pretty nicely.

At the same time, this same Subroc thing triggered another line that I followed, which was that we needed to know what the slope of the sea floor was on a scale that you couldn't measure in those days from up at the sea surface with ordinary just sequential pings from an echo sounder, didn't have the resolution. So I decided that the best way to do this was essentially to do a shallow-water problem, build yourself a device that you could tow down there in the sea floor, and put a good echo sounder on it, but now you're down within 100 feet or something of the bottom, so you can really make quite detailed measurements of what the slope of the sea floor is. So we convinced people that in the context of this Subroc thing, that would be a good thing to do. So we started to build that.

We already had some feelings about wanting to put instruments down, other geophysical instruments down near the sea floor because in the era of about the late fifties, there was a move to bring small-scale submarines into the research world. The first step in that movement in ONR [Office of Naval Research], because it was an ONR movement--NSF [National Science Foundation] would never have been able to cope with that in that era--the idea was that Louis Reynolds and the Reynolds Aluminum Company had decided that in conjunction with an engineer who had been in a Navy submarine pressure hull design group, decided that aluminum was really a good way of building a pressure hull. You can build a pressure hull that would be quite resistant to the loads at thousands of feet without having to have so much weight n the hull that you could go ahead and build something that was a practical submarine. So Reynolds started to do that, and the Navy thought that--the Navy at this point was in the person of then Captain Momsen, who was the son of the "Momsen lung" Momsen, who, in fact, the father was the Commander, Submarine Force Pacific Fleet in the era in which we were doing the sonar things in the early fifties and had such close relationships with the operating forces.

But anyway, Momsen was in ONR at that time, and he started squirreling away money to lease this submarine. When it turned out that that was much too complicated to cope with Louis Reynolds' ideas about treasure hunting and things of that sort, that he had this money put away, so that was the money that funded construction of *Alvin* as the original entity in the deep-diving little submarine world.

NO: Let me interrupt you for just one second. I've read public relations brochures that Reynolds Aluminum made for *Aluminaut*, and one of the things they talk about is people living on the sea, they talk about aquaculture, farming on the sea floor, food from the sea. I mean, did you guys take that seriously, or would you say that that was just a kind of [unclear]?

Spiess: We did not take that seriously.

NO: Okay. [Laughter]

Spiess: There were some of us who were involved in the early stages of that *Aluminaut* thing as consultants from the Navy side, as to how you could use it, what kinds of things could you do, and there were clearly two different kinds of things you could do. There was the one that wound up being the *Alvin* job, which was to poke around in the nooks and crannies of the sea floor where things were pretty complex and let your geologist do ordinary horseback geology on the bottom of the sea. The other was much more adapted to *Aluminaut*, because *Aluminaut* was not going to be as little as *Alvin*, and it was not going to be as maneuverable, but it would be great for going down near the bottom and doing magnetometer surveys, gravity surveys, topographic surveys, because you were close, so your resolution would be that much better.

It was out of that context, in a sense, because as the liaison person for ONR from Scripps with the *Aluminaut* thing, I had gathered up a group of people here, Vacquier and Menard and others, and *Aluminaut* kind of evaporated, but we had this opportunity to tow something down near the bottom, and that was just as good, for our purposes, as having a submarine. In fact, better, because it was a much more adaptable kind of device. You didn't need anything like--

NO: [unclear]. [Laughter]

Spiess: You weren't into the dangers of putting a man down. You just plain put the thing over with a wire and gather your data. That's where we started building, on an incremental basis, actually a device that we could tow down near the bottom, that would answer the questions that Subroc people had about what the statistical nature of slopes of the sea floor were, and at the same time had the ability of expanding to include a whole variety of other kinds of measuring instruments. I think that development is, of all the things that I've done, the most satisfying, because that was what brought me really into the mainstream of the Scripps Institution of Oceanography in the sense that suddenly I had a tool that nobody else had, and the brightest young geology graduate students were quite enthusiastic about doing this. [Laughter]

NO: [unclear]. [Laughter]

Spiess: So we had lots of fun. I learned a lot of geology from those kids. So it was a beginning of something that lasted for quite a number of years. Well, it still does last. It lasts today. But we went through a period where there were really a lot of great graduate students who came through. There were twenty-some-odd of them, great people to work with.

NO: You mentioned Allyn Vine. One of the things that Vine said to the Navy, as far back as 1946, was that it would be good to have a submersible vehicle for salvage operations if and when nuclear material or submarine was lost. Did you talk much about salvage, about the use of salvage in that issue?

Spiess: We talked about it some, particularly right after *Thresher* was lost.

NO: I was going to ask you about *Thresher* as well.

Spiess: The summer after *Thresher* was lost, the Navy put together a summer study thing. Admiral Stephen, who had been the Oceanographer of the Navy, was the person in charge. Allyn Vine was involved in that. I was also. I led the sea-floor search component of that, because people had realized that there was no organized capability to find anything on the bottom of the sea, and yet they immediately realized that if they could find a submarine, that would be great, but if they could find something littler than a submarine, that might be great, too. So we were down to talking about a target as a basketball or whatever. Nobody bothered to ask what was the real thing that you were going to find, because you could imagine all kinds of things that people might put on the bottom that you'd like to find. So in that context, there was talk about salvage, too, but that sort of wasn't in my--

NO: Tell me more about the *Thresher* search. [Interruption.]

[Tape 2, Side 1 is blank. Begin Tape 2, Side 2]

NO: We were talking about the 1960s. I'm just curious to ask a little bit more about how you experienced that, because throughout the 1950s you had been doing work on behalf of national security, and I'm sure that in some sense you felt proud of the contributions you were making to national defense. Now it seems like the ground shifts under you and suddenly you're being attacked for work that's the same as what you've been doing all along.

Spiess: Well, I'm not sure. The ground shifted alongside of us, not under us. There were some repercussions that were a little awkward because the fact that there was all of this disturbance and within university campuses more or less in general, meant that the people who used to talk to us in very relaxed mode began to worry about what their bosses were going to say because they were doing that, and I'm not sure how that was true in detail, but there was kind of a tightening-up. I guess it was somewhere along in there that the Mansfield Amendment came in, that put more pressure on the administrators in Washington to justify their programs in narrower terms than they had in the past. That didn't necessarily change what was going on, but it did make for a more complicated dialogue. I think that's part of what really happened.

In some sense, that was just part of another trend that was going on to more and more program control at Washington end of food chain than there had been before when administrators in the fifties, if there was a Navy problem, they would gather a handful of us from different labs and we'd sit down and talk about it. Then they'd tell us how much money we had, and we'd say, "Okay," and we'd divide it up and go home and write a one-page proposal, and the money would come.

That sort of thing gradually was fading away, irrespective of this other element. So I think that if we had been sitting up on the upper campus, we might have had more exposure. We had some, in any event. I guess I told you about spending an afternoon standing at the podium over in Sumner Auditorium, answering questions of whoever wanted to ask about what we did in the Marine Physical Laboratory. But I think that it didn't do Scripps' relationship with some parts of the upper campus any good, because some parts of the upper campus felt that we should take the side that they took. I know that [Bill] Nierenberg wasn't about to do that. Walter Munk was pretty much Navy support side, and certainly a lot of the rest of us were, too.

I guess that was the era in which I tended to look back on the Loyalty Oath times and tried to draw some kind of parallel. I think that one of the elements that I may probably have put forward before in sociological kind of terms was that, by and large--there probably are exceptions--but looking at the physics community and the oceanographic community and the communities that were over on the other side of the ledger in the sixties, you could almost say that the people who had to work with a wide variety of other people in order to get their work done tended not to stand on principle quite as strongly. I think when you go to sea, you know you're out there with people who don't necessarily look at the world the same way you do, except in terms of getting the work done, and then you all look at it the same way.

I had sort of a division line between the theoreticians and the experimentalists in the physics world, and again there are some exceptions, but the ones who had to make do with the people in the machine shop or whatever, you know, they didn't have quite the same assurance that the principle they were standing on was necessarily the one that everybody in the world should follow.

NO: That's a good point.

Spiess: So anyway, that's my philosophical argument of the time.

NO: Think a little bit more about what the argument was in the 1960s. When people suggested either to you particularly or to Scripps, MPL, in general, that scientists, oceanographers shouldn't be taking military funding, what alternative was there? Was there an alternative, as far as you were concerned?

Spiess: Well, no.

NO: In a word. [Laughter] Okay.

Spiess: I think that was the era in which there were alternatives in a sense, but there was no way you could run the Scripps Institution of Oceanography if in a matter of a year or two you took away all the ONR funding or Navy funding in general, and there wasn't anything that was going to replace that level of activity, because we were part of the Navy research program, really, and we knew that. So there was no other way. There were other fields in which that was true, I believe, as well. The Air Force research establishment, the Air Force equivalent of ONR, was funding an awful lot of the research on the upper campus. In fact, ONR was funding a reasonable amount of upper-campus research. This was all unclassified basic research, and you couldn't really fault people on the fact that the military was willing to fund basic research. I mean, that made it kind of an awkward argument to figure out how to follow up with the fact that people were against the military. So I don't quite--there was a certain amount of ambivalence in some quarters.

NO: What about the academic freedom arguments? Were there any aspects of those that you agree with or credited to some degree?

Spiess: Certainly in the ONR programs--I guess you have to say what do you mean by academic freedom. Often that's phrases as you ought to be able to do whatever the research is that you think is right. Unfortunately, it costs money to do research in many fields. This may be another reason why people in some fields are more likely to be able to take a purist stand on things than people in other fields, because if you're in the medical world and want to do research, you'd better make your peace with the National Institutes of Health. If you don't like that, why, then you can do a little bit of work with private foundation money.

So in one sense, academic freedom means that I ought to be able to go to ONR and sell them what I think they need to do, what I would like to do, just as well as I should be able to go to the Rockefeller Foundation, National Science Foundation. So that's one way to look at the academic freedom side.

I think that a lot of people have some kind of view that if you're working for the military, they told you you had to work on that project, and that couldn't be farther from the truth. You, one way or another, agreed that you had a common interest, and if you didn't have the common interest, you didn't do the work. It was as simple as that. So this misapprehension that because the military puts money into your research, that they are directing your research, is sort of an insult to the character of the people who are doing the research, because they shouldn't be taking that money unless they believe both in helping the sponsor and in doing what they're doing. I think if you're the kind of person who will take money under false pretenses in order to get your work done, I don't think that's a very good deal.

NO: It's interesting you say that, though, because in some sense, some scientists have claimed that, because, I mean, especially in the literature on the history of physics, they took military money, then they really did what they wanted with it. Do you think that's just [unclear]?

Spiess: The military wouldn't have kept funded them if that hadn't been what the military wanted, too.

NO: Right.

Spiess: Of course, a lot of people who have not been involved as deeply in the administrative side of all of this have a lot more the view that you just--in many instances, anyway, that you've just expressed. The reason that they're able to live on that viewpoint is that there were some administrators in their organization who understood both what the military wanted and what the people in their lab wanted, and made the connection so that they were comfortable connections. You don't connect your person with somebody where the sparks are going to fly. I think that I guess I looked upon that as one of the roles of people who had jobs like the director of the Marine Physical Laboratory, although there it was not as difficult a thing because we all knew what everybody else was doing and so on. But to me, a good laboratory director is somebody who knows what his people are good at and what they would like to do, and knows what sponsors there are out there, and then helps to make those connections go.

NO: So would you say Revelle was someone like that?

Spiess: Yes, Revelle was like that.

NO: [unclear]. [Laughter]

Spiess: I think Revelle was like that. He allowed people to go their own way. There are two different aspects to the Revelle thing, as far as I'm concerned. One of them I would characterize as the Ralph Keeling sort of thing, where Revelle sees that there is a real thing that ought to be done, so he finds somebody and makes that connection, sees that it is healthy and prospers.

There's another kind of connection that was the Marine Physical Laboratory connection, where if Roger was appointed to some kind of a Navy committee, very quickly one of us, usually myself, because I was the director of MPL, would find ourselves on the subcommittee. Roger wasn't there, but you could go to the meeting really as the representative of the Scripps Institution and you could make promises, and you could go home realizing that--well, you wouldn't make absurd promises, but promises that had to be backed up by the director. You could go home and--

NO: [unclear].

Spiess: Sure. That's why he let you go in the first place, I guess.

NO: He wanted you to [unclear].

Spiess: Yes. So this sort of led to the fiefdom view of the Scripps Institution of Oceanography, I think, because he had his things that he wanted to see happen, but he also had a big institution, so he let different groups more or less go off and do what they thought they ought to do. There wasn't a micromanagement-type thing in which Isaacs' group or Munk's group or Marine Physical Laboratory or Scholander's group or whatever, but that kind of philosophy was really comfortable for Roger. He didn't have to run all the programs. He only had to run the ones he was most interested in, and the institution ran because he realized somebody had to run Problem A or B or C. I think Warren Wooster was the sort of international program person and I was the Navy program person. This was sort of the ultimate in successful delegation of authority.

NO: Let me ask you a little more directly about funding and ONR. Last week, one of the common themes of the colloquium was the idea that ONR funded basic research, but obviously that was within the context of a mission. So how did you understand the distinction between the mission and basic research?

Spiess: I'm not sure exactly what the question is there.

NO: I guess the question I have is sort of how do you define basic research.

Spiess: That's a better question, because I don't know the answer. [Laughter]

NO: [unclear]. [Laughter]

Spiess: Because, to me, all of oceanography is applied research.

NO: [Laughter] You're the second person today who's said that.

Spiess: You learn your chemistry or your physics or whatever it is, and then you apply it in the ocean. Geophysics is an applied science. So once you've taken that step, it's downhill all the way. [Laughter]

NO: [unclear]. [Laughter]

Spiess: Well, and you get to say, well, you know, how applied should it be? Should it be applied enough that you can build a sonar system, or should it be applied enough that--is there some line in there, some magic line that says sort of applied is all right, but very applied is not? I think that that's the thing where you have to answer the question yourself and make your way.

NO: What do you think people understood when they talked about ONR funding basic science? What do you think they meant by that?

Spiess: I think they meant that to them it wasn't obvious why the Navy wanted to know. And yet you can say that it's the same reason that the Atomic Energy Commission, which was interested in building bombs and reactors, funded people to build cyclotrons.

NO: That's a good example. I mean, it's pretty obvious why the AEC wanted to know about distribution of radionucleides into the atmosphere or--

Spiess: Or did they really need to know about element number something or other as you went farther and farther in the Seaborg and Perlman world out into the periodic table? But in the ocean, it's even fuzzier, because as you think about what the Navy ought to know about the ocean, the more you know about it, the more you find that they ought to know more than they do. So it really becomes more of a matter for ONR to make sort of two decisions. Now I'm talking ONR in the fifties and sixties, not ONR today. ONR in the fifties and sixties supported things that were not directly contributory to building a sonar system or were not directly related to sound propagation. It's this very fine line. You fund physical oceanography because you want to know about the condition of the water. Why do you want to know that? You want to know that because sound travels through there in strange ways that are controlled by this. But ONR had no program in underwater acoustics until--I was trying to remember when Brackett Hersey went from Woods Hole to Washington to be deputy director or something like that.

NO: [unclear].

Spiess: But there was no per se program of that kind. ONR did have a program office that was called, I think, undersea programs, and it was all the project officers, nearly all of them, were naval officers, regular line officers, not oceanographers. Well, I'm not sure that there were oceanographic-classified people. That was the sixties and that time.

So that in the fifties and sixties, I guess another aspect of the way the situation was, certainly in the fifties, I guess I would say on through the sixties and began to change in the seventies, was--I have to go argue with Deborah Day. I was just reading a thing that she wrote, which I should have read before because she sent it to me for comment, but it's been several weeks. But she has the phrase up in the front end of this paper about how ONR funded ideas, not programs. Not so. They funded research groups. They funded people, and they trusted those people to think of things to do that were appropriate to Navy concerns, although you might have to take a very enlightened view of what that meant relative to what the view of some conservative admiral might.

NO: [unclear].

Spiess: Yes. So the Scrips-ONR contract talked about things that you could do, but basically how that money was going to be spent was up to Roger.

NO: It sounds like [unclear] basic research.

Spiess: Well, no, not necessarily, because I would claim that when I was generating a new communication system, I was doing applied. I was doing engineering for all practical purposes.

NO: So you [unclear].

Spiess: And I wouldn't have argued that that was basic research, but once I was around to having a deep-tow system that I could use to do fancy kinds of geology and geophysics, but at the same time I knew that the Navy was interested in being to find things on the bottom of the sea. I was not above saying, "You ought to fund this because we're learning about the background against which you're going to have to find something."

NO: [unclear].

Spiess: And, in fact, I would go off to one of these classified meetings and tell them, "Here's the background and here's what kind of signal you're looking for." Many is the time I've taken a magnetometer signal over the *Thresher* and put it against the magnetometer records that we gathered with our machine down close to the sea floor. It's relevant. It's one of these things that I've said earlier is kind of fun because you get to do both. I think there are more of us in the world of experimental science than people perhaps might care to admit, that have a feel for wanting to do something kind of tangible as well as wanting to do something that the theoreticians over there are going to be amazed about.

NO: [unclear].

Spiess: Well, pretty much--

NO: [unclear].

Spiess: And I think that's probably more in physics than in anything else, because the line between theoreticians and experimentalists can be a lot more clearly drawn. This is one of the things I had to learn when I came to Scripps, particularly as I moved up toward being in the director's chair, that there were many different structures to different fields. By my reading in the physics world, physics had the most clear division of labor kind of thing. I think I grew up with the idea that the highest goal that an experimentalist could aspire to was to prove that some theoretician was wrong. [Laughter]

NO: [unclear]. [Laughter]

Spiess: That would be the number-one thing. Number two is verifying that they're right. You can get your Nobel Prize for that, too. That's what's so great. But I think that it's a very clear line of distinction. Whereas you move over into other kinds of science and it's not so clear anymore, and I guess it took me a while to become used to working with geologists who--

NO: [unclear]. [Laughter]

Spiess: Well, they don't have any theorists, but they have this feeling that every paper must have not just the observations, but a little diagram about how the earth might be underneath and whatever. If you only produce data, then you haven't made any dint and you don't be published.

NO: [unclear].

Spiess: Yes, sure, but in the geology world, I think that's a much more strong kind of thing, whereas in the physics world, I did a Ph.D. thesis. I gathered some data from the cyclotron, from the polonium that I was working with, and I made the measurements and found out and showed that in one minor instance, at least, the theoretician was wrong. [Laughter] But the whole structure of Scripps is interesting because of this wide variation in how different sciences, if you look at them individually, have different ways of deciding what's good work and what's publishable and whatever else. If you're going to really do interdisciplinary things or live in a multidisciplinary place, I guess "multidisciplinary" is a more important word than "interdisciplinary." If you're in a multidisciplinary place, you have to be willing to look at and appreciate the cultures of these different groups.

I think my time as director of IMR was the most enlightening. I already knew about this by the time I was director of IMR, but some of the best examples, I can remember there are people who do ocean physics who will come up with a good paper in a year, maybe two or three. But I can remember Bill Fenical coming to me and saying, "Gee, I don't think you should put me up for a merit this time. I've only published nine papers since the last time." Or, "I only published nine papers this year." And in certain fields of chemistry, bang, bang, bang, you just keep writing. I guess you go into the lab and do something, and a month later you have a paper. Whereas if you're an experimentalist who has to build the gear and go out to sea and use it, it takes a while.

NO: [unclear].

Spiess: And so you have to realize that when you're writing people's promotion things or evaluating their performance.

NO: I wanted to ask you one more question about the funding.

Spiess: Yes. I wanted to say something about funding and the way it changed.

NO: Yes, exactly [unclear].

Spiess: In the fifties, you and Ron have told me some stories about how nasty contracting was, apparently, in the forties, early forties. By the time ONR came along, things were smoother. The contract we had running in the Marine Physical Laboratory through the Bureau of Ships started in the beginning. In fact, at the time I joined the lab in '52, it was still that way, that the Marine Physical Laboratory was located--most of us were on Point Loma, but we were treated as if we were a division of NEL. NEL was funded by the Bureau of Ships also, so the Bureau of Ships gave NEL a project that said, "Support the Marine Physical Laboratory." And the University of California saw the salary money, but they didn't see any money for equipment, supplies, anything else. We chitted stuff out of the storehouse at NEL. We used instruments out of their pool. So we were really that sort of organization.

Toward the latter part of the fifties, it for one reason or another became clear that the Bureau of Ships was not going to forever be the most comfortable place for a university lab, and that ONR would be a better place. ONR did have this Undersea Programs Office. I guess it was Rawson Bennett who was the Chief of Naval Research at the time, had been the Director of the Navy Electronics Laboratory, and he knew what the Marine Physical Laboratory was, and he thought it ought to be part of ONR. So along about '58, as I became director, we made the transition from being sort of part of NEL to having a separate thing in which we had a contract with ONR. But even so, I could write a proposal that said, "We're going to investigate the nature of the ocean--" [Interruption.]

[Begin Tape 3, Side 1]

NO: So we were talking about funding and the ways in which funding changed through time. One thing especially I was curious about is whether--you talk about ONR being enlightened, but was there a sense over time, in the sixties or seventies, that its mission became interpreted more narrowly or things became more difficult?

Spiess: I think--

NO: How did things change?

Spiess: I think that was not the major change that I would attribute to or that I would use in describing what happened as I saw it, anyway. The concept of having people out in the field gin up what was good research became more constricted in the sense that the administrators were perfectly willing to have people out in the field suggest things that might be good, but they wanted to decide which ones they would fund. This led to the end of this institutional-type block funding.

It ended for another reason, I think, that had to do with the people who were the eventual recipients of the funds, the working science groups, in that there was not always unanimity at the working level with the program decisions of the director. It was the case of the sort of benevolent dictator, and if you're one of the things that he likes, then you're in great shape. But if you're not, then you're out of business, or your business will be funded in some marginal way.

What this led to was individual scientists going to the administrators in Washington and saying, "Why don't you put some money into my project," as opposed to putting it into the mainstream block thing.

Eventually what this turned into was that what had bee a block-funding thing became a block-funding approach in a much more narrow sense, in that the institution would patch together proposals from a whole bunch of investigators, the ONR site visit would take place, and the investigators would look at this institutional proposal, but they looked at it project by project, and some projects they thought ought to be funded and some shouldn't be, and it didn't matter what the director said. There was very little funding that the director controlled at that point. So that was a step along the way.

I think there's another aspect, of course, of the ONR contract activity, or the existence of the ONR contract, because the fact that they had been written to begin with in a fairly broad way meant that for some of us, when we, along with our project officers, found that some other part of the Navy was interested in something we were doing, that the easiest way to move that money from there to us was not to generate a contract with the Bureau of Naval Weapons or whatever it might be, but to have them internally, within the Navy, use the ONR Contract Office simply to establish another task in our contract and put the money in.

So, in my mind, in many ways the ONR Contract Office was as important a part of the ONR structure as the program offices were, because it really made for much easier access to other Navy funding. In fact, I think if you decide you're going to track how much ONR funding went into Institution X or Y or Z, you have to be very careful, because some of that money may really have been money from some other Navy organization that was just handed through, handled in the ONR contract by establishing another task, and the ONR Contract Office was the office that really facilitated having that happen. I think that was a role that people perhaps don't recognize.

NO: [unclear]. What about NSF? Did NSF [unclear]?

Spiess: Oh, yes, because as in the fifties, when NSF was starting up, it was something that was off in the distance somewhere. It was basic research. [Laughter]

NO: [unclear]. [Laughter]

Spiess: We were doing Navy research. But as we began to move along and build the breadth of the interest, let's say, within the laboratory, increasingly we would turn to NSF for part of the funding. Once we had built the deep-tow system and had a couple of papers under our belt, why, there were a number of years when I almost became tired of writing a proposal to NSF to study the fine-scale nature of the deep-sea flora, rearrange the words one way or another, but that's what it was. So we had NSF funding along the way. That would have started probably late sixties for us. I think Raitt and Shor had NSF funding, Vacquier had NSF funding.

NO: How would you contract the NSF funding with the ONR funding? Did it make a difference really who you got your money from?

Spiess: Didn't make much difference who it came from, because both of them were pretty good about things like writing a proposal that said, "I'm going to spend this much on salaries and that much on something or other, on equipment," and as you went along, you decided that you wanted to do it differently, you could change things around in the budget pretty easily for both. I think that it was in the selection process as to whether you were going to be funded. That's where the real difference was between the two approaches.

NO: [unclear]?

Spiess: Difference in mechanism more than anything else, because you fund the same thing in both places in many instances.

NO: [unclear]?

Spiess: On the one hand, in the NSF thing, you were into the straight-out peer review system, and as long as you were writing things that were popular with your peers, you were in great shape. ONR was a better place for something that hadn't been quite established yet, and for those of us who were innovative environmentalists, there tended to be kind of a pattern in which you could go to ONR and there you could talk to the program officer, and if you could convince the program officer, then you'd be funded, and you might be funded for a couple of years before you got a paper out. Whereas in the NSF world, it was much harder to do that. So it was almost a pattern that whatever new equipment was going to be built, we did that under the ONR part, and then when it came time to use it, by then ONR had usually lost interest and it was time to go to NSF.

NO: [unclear]. [Laughter]

Spiess: So it was a back and forth kind of game. There were real differences. In the most recent world, ONR has changed in two ways. One is that the applied research funding, so-called 6.1, 2, 3--these are budget categories in the Navy budget--that do tend to say basic research, applied research, or basic research application and system development, whatever, and these have been folded all under the hat of the Chief of Naval Research. So the world is different now than it was before.

The other really big change has been that ONR is much more narrowly tied to Navy interests and, consequently, with Navy interest in the deep ocean having evaporated with the end of the Cold War, if you have something you want to do in the near-shore environment, you're in great shape. If you don't, go talk to NSF. That has been a little hard for some of us to cope with, because we had enjoyed working with the Navy, and now the Navy doesn't want us anymore, so to speak.

NO: [unclear].

Spiess: That's a tricky question, because the answer is, I can think of lots. The other part of the answer is that when I was turned down, I figured out how to apply again and again and again. One of the things that Sally--we went out and did an expedition, first time that we'd put equipment into a deep-sea drilling hole at the bottom of the sea, and we went out, and the first two holes that we had planned to use turned out to be plugged with sediment, so we couldn't do that. We went back to Woods Hole. We were operating out of there, but with the Scripps' ship *Melville*. We worked at that and we plugged away, finally we said, "Yes, let's try one more hole." We went down and there was some confusion even about where the hole was in the survey that people had done. But we found it and we went ahead and we did the experiment.

After the fact, I was given a medal by the Navy that was the Distinguished Public Service. When I went to the Assistant Secretary's office to have this presentation, Sally and I went, and they read off the citation. It's one of these nice flowery things and it tells what great things I did. One of them is "consummate persistence," and Sally just broke up, because she knew what they meant. [Laughter] So that's my medal for consummate persistence.

The things that I've really believed in, I just kept plugging until some agency was willing to fund them. Sea-floor geodesy is my best example, I guess, because I plugged at that for a long time, and I managed to get some NASA funding that carried us along for a while. I'd had some ONR funding in the beginning, and pretty soon I was around to NSF, really believed that I could do this. I had some good NSF things intermittently from the equipment development part. But it was just that, just go ahead and do it, keep doing it. So I have a bad record if you look at what percentage of my proposals have been turned down, but it's not so bad if you look at what percentage of my ideas have been turned down.

NO: [unclear].

Spiess: That's right, yes.

NO: [unclear].

Spiess: By that I'm really speaking from a kind of point of distance. I haven't had much ONR funding for quite a while. Mostly NSF funding. Probably one expedition per year or something that might have been Navy funding, sort of, went away for me when this turn to shallow water took place.

My involvement with the Navy in the more direct sense of advisory committees and that sort of thing took a nasty blow at one point, and that was that I'd been on a lot of Navy committees and I was appointed to the Naval Research Advisory Committee, which is the top-level Navy committee. I, in fact, served on it for a while and the Defense Research Board, DRB, a high-level thing. I was having a lot of fun on NRAC, and when my term ended, about 1980, and the Assistant Secretary of the Navy, who came in at that point when [Ronald] Reagan took over the administration, reappointed me to this committee, along with a number of other people. There was a Stanford aeronautical engineer and an ex-leader from Naval Research Lab.

Some months later, the word came down that that three of us, our appointments had not been approved. Since I'd been at this a long time, I was kind of taken aback, so I took the trouble to dig around and work my way through the Pentagon, to where this kind of decision is made, and it turned out that the person controlling this kind of thing in the Reagan administration was now, for the first time that anybody had ever heard of for NRAC, operating on a basis that if you're a registered Democrat, you couldn't be on that committee. I was a completely inactive Democrat, but I was.

NO: [unclear]. [Laughter]

Spiess: The other two were also. They were Democrats, but they hadn't gone out and campaigned against Reagan or anything of that sort, nor had they campaigned for him, of course, and they probably had not voted for him. But suddenly NRAC became a politicized entity. I think this goes back to some of our discussions about security clearances and whatever else, that there can be much more trivial criteria brought to bear as far as your participation in government affairs.

NO: [unclear].

Spiess: Certainly NRAC did. I had never heard of anybody even knowing what the political affiliation was of any of us, and I had been on a lot of advisory committees. So it was a real surprise to us.

NO: [unclear].

Spiess: That's right. So that was a disappointment. That was at the same time that I took on the directorship of the Institute of Marine Resources, so I was still running a research group and the Marine Physical Laboratory, but I was no longer the director of MPL. So my involvement in Navy committee work pretty much ended about that time, because suddenly with IMR I was embroiled in the Sea Grant Program and a variety of other things. From there on, I've been more or less like an ordinary research person as far as occasionally having ONR support.

NO: Was that a disappointment? Do you miss having that close involvement?

Spiess: Yes, I do. But life's like that. I've been very fortunate in a lot of other ways. It isn't as if I didn't have anything to do.

NO: No. You've been incredibly great. Is there anything else you want to cover before we stop?

Spiess: No, I think we can stop before we fall down. [Laughter]

NO: We've done a day's work. [Laughter] Okay. It's been incredibly fantastic. Thank you so much.

[End of interview]