ORAL HISTORY DEPARTMENT
TEXAS A&M UNIVERSITY

INTERVIEWEE: Fred Spiess

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PLACE: Marine Physical Lab, Point Loma

RC: You entered Berkeley in '37 in physics.

FS: Right.

RC: Okay, did you have any design towards oceanography when you enrolled at Berkeley?

FS: Oh, I would say that I didn't even know there was such a thing as oceanography at that stage of the game. I learned fairly quickly, though, that there was. I guess, within the first couple of years, I ran on to some books in the physics.... I leaned toward physics in lower levels than what I really started in at Berkeley, but nevertheless, I got involved in the physics department at Berkeley fairly early on. And they had a nice little library, and there were some oceanographic books there. But I really didn't have any feeling that this was something in which one could make a living or anything of this kind. In fact, it really was only barely that, as I've found out since. So I really didn't have any thought of that. I was interested in the ocean; and I, as I mentioned, was involved with the Naval ROTC at Berkeley at that period. So I was interested in going to sea and working with the ocean, even though I was, in fact, really pushing toward a career as a physicist.

RC: So you participate almost at once in the Naval ROTC, but I notice

you don't really enter the Navy until June '41.

FS: No, I was in Naval ROTC for the full four years at Berkeley, and I was commissioned.... As a result of that, I had a Navy commission when I graduated in May of '41 from Berkeley. And things were kind of beginning to slip over the brink toward war at that point in the United States. And practically everybody in my class in Naval ROTC was given orders to active duty within the first month or two after we graduated, even though the war didn't start till December. A bunch of us went off to the Navy Submarine School in New London, Connecticut. I had the privilege of being the first naval reserve officer to stand one in a submarine school class and then went off to the Philippines to join the submarines, late in '41; that's where I was in December of '41, when the United States got directly involved in the war situation. So I spent practically the entire war period as an officer in submarines one way or another.

RC: I take it from such things as silver and bronze stars in World War II that you were, in effect, a line officer all during the war, not involved in any of the research by Revelle and these people.

FS: No, that's right. No, I was not involved in oceanographic research. In fact, most of the people I knew at that stage of the game who had not been in ROTC, my colleagues in the physics department, had all gone off to places like Oak Ridge and Los Alamos. And so I really knew by implication more about what was going on there than I did about oceanography. I learned the hard way about underwater acoustics and things of that sort because we had sonar systems--very primitive ones in the beginning--on board submarines. I learned also about internal waves in the ocean and things like this just by the fact that periodic buoyancy changes in your submarine, if you were operating close to the surface,

were things you had to cope with. But I had no intention at that point of becoming an oceanographer per se. Late in the war, as things were winding down, I had the opportunity of going off to Harvard as part of the Navy post-graduate plan. And, in fact, the war ended while I was at Harvard, or just as I went to Harvard, and so I had a choice of either staying in the Navy or finishing out that one year graduate course still in the Navy and then going back to inactive status. And I elected to finish the course, and at that point I chose to go back to Berkeley, which I did in 1946, back to the physics department to work toward a Doctor's degree.

RC: You worked really towards....

FS: I was involved in basic physics still at that point, and the group with which I became involved was, in fact, under Professor Grey (?), who eventually got the Nobel Prize, but not until after I left. And that was high energy nuclear physics. And, of course, that was what that physics department was famous for, but it was also a very good department in that it was very strong on classical physics as well as, in those days, in high-grade research in the nuclear side of the game. And when I finished my degree, I had stayed active in the submarine naval reserve I was in command of a naval reserve unit in the San Francisco Bay area during that period. So when I finished off my degree, I was kind of tempted to do something that would combine the nuclear physics and the submarining, and so I.... Most of the places that I visited--in an inevitable job-hunting period that comes right at the end of a degree--several of them, anyway, were places that were involved in the then very new nuclear power business which was solely tied up with submarine sorts of things. And I wound up going to the Noles Atomic Power Laboratory of General Electric in Schenectady just because

of this sort of coalition between the submarine interest and the physics interest. And while I was there--I hadn't been there even a year--I came out for a visit back to the San Francisco area, which was where I was born. At that point, I got a call from a friend of mine with whom I'd worked as a graduate student (he was still a graduate student just about to finish off),and he had been approached by Scripps to come down here and talk about a possible job in marine physics. This fellow had an army-type background. It happened that he knew one of the people who was helping to search for somebody to fill a vacancy. So he told the people who were talking to him that I was around and that I was a physicist, but I was much more interested in the ocean than he was and that they should ask me to come down. So they asked both of us to come, and we came on down. And it turned out that one of the members of the Marine Physical Laboratory at Scripps Institution was about to go off for a year. He had been running sort of a very nice program in underwater acoustics. It was of considerable interest to the Navy and sponsored by the Office of Naval Research. They wanted a physicist who could carry this on while he was away, and they were looking toward expanding the size of the laboratory somewhat anyway, so they were recruiting. Thompson was the other fellow with me, who eventually became the Atomic Energy Commissioner and died tragically in an airplane crash while he was doing a site inspection. But, anyway, they offered me a job and I really.... The contrast with the Noles Atomic Power Laboratory, where there were a couple of thousand people working and sort of building one very sophisticated engine, and this sort of thing, where I could come and have a little research group and see the whole program in my hand and control it and do something that interacted directly with the ocean.... It was at that point that I clearly made the

move, which I've never regretted at all. It was a very good thing, just personally. So I came here and started doing underwater acoustics and sonar system sorts of things. That was 1952, wasn't it?

RC: Yes.

FS: It built well on my background because I could take the good classical physics that I had worked hard on at Berkeley and combine it with the submarining that I had been doing because it turned out that a lot of this underwater acoustics at that stage of the game in order to have quiet platforms from which to do acoustics measurements, we did a lot by working with the submarine force locally here. We would go out on submarine and _____ and do some propagation tests and things of this kind. And so the fact that I could argue with the submarine commanding officer helped me a great deal in being able to make this sort of thing go.

RC: I have several questions I'd like to ask you from '46 to '51. Did you do organized submarine research while you were attending Berkeley?

FS: No. You have an abbreviation there, the organized submarine RES, which stands for reserve. I ran a Navy-organized submarine reserve training unit in that area throughout that period--helped to write training programs and things like this, because it was a completely new sort of enterprise. There had never been a submarine component to the naval reserve until after World War II, and so I did that in parallel with the work toward the Ph.D. in nuclear physics.

RC: Okay, and your work in nuclear physics was really in what direction? What kind of research were you doing at Berkeley?

FS: I was working on...well, the thesis had to do with...actually it's been a long time. I was working with the by-then sort of intermediate energy cyclotron in the Crocker Laboratory available. I was head _____

_____and doing studies of heavy elements, bombarding with alpha particles and looking to see what kinds of reactions would result, particularly things around ammonium and lead and this sort of business that were so interesting. Short half-lived (?) meters have not been studied, and it was that sort of thing that I looked at and wanted a few odds and ends, and that became a thesis.

RC: Were you involved with Atomic Energy Commission at this time?

FS: Only in that I held the Atomic Energy Commission Fellowship for one of the years that I was working on a Doctor's degree. Of course, at the Noles Atomic Power Laboratory, that was an organization that was, in fact, run by the Atomic Energy Commission.

RC: When you become director of MPL, were you able then to guide the research activities on land?

FS: Well, directing a laboratory isn't, in the academic world anyway, is not really...perhaps the word "director" is the wrong one; I'm not sure. I had built up my own research in underwater acoustics and marine geophysics. And basically this laboratory--it's a small one. We had a handful of very talented people, and the main function of the director is to see that life went reasonably smoothly for them and to get out of their way at the right times. And at the same time, since it's a small laboratory, I was able to keep my own research going. This has worked out pretty well over the long haul. I'd been here at the laboratory for about six years before I became director. We had had two previous directors who were both, I think, quite influential in a way. One of them was the man who basically recruited me to come here in the first place, Carl Eckart, who was a distinguished physicist who had become involved in the University of California Division of War Research during World War II and played a major role in that organization, and then he'd been a real figure in theoretical matters and development of quantum

mechanics, and thermodynamics, and things of this kind. When he came out here during the war, he became quite interested in the ocean, and particularly in a sort of missionary sense, I guess, because he was a very fine person in his ability to see how to apply high-grade mathematical methods to problems that other people hadn't yet applied them to. And he did this during the war in the sonar and acoustics business--that was our acoustics. And when the war ended, he decided he really didn't want to go back to the University of Chicago, and so he convinced the Navy that they should set up a laboratory within the University of California because he didn't really want to stay in the Navy Civil Service Laboratory situation. The University of California Division of War Research, at the end of the war, was mostly turned into an organization called the Navy Electronics Laboratory. Descendents of that still exist here today. And at the same time, he arranged that the Marine Physical Laboratory should be established within the University of California. It was not really part of Scripps Institution of Oceanography, but an entity within the University, with Eckart as its director. Part of the reason he was able to do this, and was encouraged to do it, was that one of the people with whom they were dealing in Washington in the Navy Bureau of Ships in the Sonar Design Branch was a commander named Roger Revelle, who was on leave from Scripps. And so he was at the other end of this sort of thing. Within that framework, it had been decided that they would go ahead and do this thing of establishing a small laboratory that the Navy would fund and which would focus on problems in underwater acoustics and sonar concepts, which would be somewhat different from the in-house Navy laboratories which were then growing up, which would be rather more engineering-oriented; and this would be more basic science-oriented. And so that was

basically the way this sort of thing started. Eckart in 1948 became director of Scripps for a while. Eckart did become director of Scripps at that point and decided that there was no reason that there should be a small laboratory, the Marine Physical Laboratory, in the San Diego area and the rather larger Scripps Institution. And so he decided to make the Marine Physical Laboratory de facto a division of the Scripps Institution. And the laboratory has maintained that position ever since. In fact, I guess it's the oldest existing administrative subdivision of the institution.

RC: And that explains how you can be director of MPL and also associate director of Scripps Institution of Oceanography?

FS: Well, that goes another way, too, in that Eckart moved from being director of MPL to being, for a brief time, the director of Scripps, because Sverdrup left in a fairly sudden matter. And so Eckart kind of held the place together while they girded themselves up to recruit Roger Revelle to be the director of Scripps. And, as you know, Revelle was the director for some considerable length of time. In 19--must have been late '61, something like that--Revelle (this was sort of a trick situation) had been heavily involved in the development of the new campus here, the full scale University of California campus. And by 1961, things had developed to the point at which they were going to appoint a chancellor for the campus. And the decision was made not to appoint Revelle. He obviously was disappointed, as were many of the rest of us who had worked with him, because he had been sort of using Scripps,or Scripps had used itself,to help with the overall development of the campus. And a lot of us felt we had a lot to put at stake, and we were not really all that pleased when this happened, but nevertheless it did. And so--it must have been the fall of '61--Roger decided that

since a chancellor had been found, Herb York, who was a perfectly good man, very good man, Roger accepted the position as science advisor to the head of the Department of Interior, I guess it was. In fact, I think he was the first of the departmental science advisors. And so he went off to Washington. And what he left behind him was essentially a complex of three jobs: chief campus officer, the head of what was then the School of Science and Engineering, which was the beginning of the general campus here, and the directorship of the Scripps Institution. And I was asked at that point to take on the directorship of the Scripps Institution, which I did, much in the same vein as Eckart before me had stepped from the Marine Physical Laboratory. I'm not sure whether it's because my office is off the campus and all the other people at Scripps are on the campus, and maybe the Marine Physical Laboratory director, therefore, is thought to be a little more neutral and therefore more acceptable in this context. Anyway, Roger left and I took over as the director for the next period, close to two years. It was a particularly interesting thing, if you're interested in coincidences and that sort of business in the history of science, because the three of us who wound up taking the three parts of Revelle's job had all been graduate students together in the physics department at Berkeley simultaneously. Herb York and I were in the same research group. We both did our degrees for Sid Grey. Keith Birkner, who was the theoretical physicist who was head of the School of Science and Engineering at that point, he and I had been teaching assistants together in courses around 1950 at Berkeley. So we'd all three arrived by completely different paths. I guess, in a sense, the only one who arrived really legitimately was Birkner, because he stuck with the physics business and eventually came to UCSD to head up the physics department and then

took over as head of the School of Science and Engineering when Roger left. Herb York had gone the route of government administration and so forth, having gone between _____ and defense research and an engineering establishment in Washington before coming here. He, in fact, had been involved in things of that kind even at Berkeley when he was quite young, because he was.... Well, I can remember he was sort of in charge of scheduling things of this kind on the cyclotrons while I was still a graduate student at Berkeley. I probably arrived by the most illegitimate means because I had, in fact, just plain abandoned physics more or less. Although, up till that point, I guess, I still.... You talk about when did I become interested in oceanography or something like that. When the manpower questionnaires come by, you write in; they always want you to say whether you're a physicist or whatever. I had been putting down physicist or geophysicist every year until I got to be director of Scripps, and then I figured there was no way I could honorably do that any more. So, at that point, I decided I was an oceanographer, and I'd been around Scripps for about nine years at that point. And I think that in much of oceanography, this is the way, certainly in those years, this was the way it went. People came in, usually on an opportunistic basis, which mine certainly was. I didn't come in because I went looking for a job in oceanography. The job came looking for me. Once I saw it, I didn't hesitate very long, but nevertheless that's the way oceanography did develop in that sort of ten year period. And I think that's one of the places where Revelle was very good, that he had tentacles out and could bring in interesting people to populate this growing institution that we have and do it in such a way that the people who would come mostly came initially with the idea that whatever the particular discipline was in which they were involved, was probably

capable of solving most of the problems of oceanography or something of this kind or, at least, they weren't necessarily initially interdisciplinary-oriented people. But by the time they'd been around for a few years, well, you began to interact with the other people in the group, and eventually it got to where a lot of us are pretty strongly interdisciplinary-oriented. You can work with people who have completely different philosophies about how to do an experiment. The last half dozen of my graduate students have been marine geologists,and they just don't think the way physicists do. But I've learned that there are many ways of thinking about science, and that geologists' ways are sometimes the right way to attack a problem, particularly if it's a problem in the real world as opposed to a laboratory-type problem. Physicists are brought up with a sort of laboratory orientation. This is really not the only one that we should have, if we are going to study the ocean.

RC: When you become associate director of Scripps Institution, what sort of....

FS: This was not...I hadn't...I was the director of Scripps Institution at that point. That was in 1961 to '63.

RC: Right.

FS: Then Revelle came back from Washington and took over again as director of Scripps. I had retained the directorship of the Marine Physical Laboratory during this period, and I became also at that point moved over to being chairman of the Department of Oceanography as well as the director of MPL. Revelle then left for good about--when was that--'64 or'65, along in there some place. He was back for a year or a year and a half, and then he went off to Harvard. And that was a permanent dissociation. And again I took over the directorship of Scripps for about a year, during which time we made a search for a permanent director, and the end result was the recruiting of Nierenberg to be the director.

And he started in 1965. And it was at that point that he asked me to be one of the associate directors; I guess he asked me out of politeness or whatever, because I'd been in the front office for so long that in any event this has continued. It's a kind of associate directorship which sometimes has lots of action associated with it and other times has none. It's kind of a special assistant-type thing, as opposed to some of the other associate directors of Scripps who have specific spheres in which they act. Bob Fisher, for example, is Associate Director for Facilities, particularly for ships and that sort of thing. So, that was pretty much how that sort of thing developed. I think that the institution certainly has.... Well, it's grown a lot, but it hasn't changed all that much, I don't think. Some people would say it's changed a lot, I guess. In that it's grown, it's become a little more difficult to recognize explicitly the interdisciplinary things and the sort of oneness of the place. When I first came, we had Wednesday noon lunches, and a fair number of the members of the staff would come and bring their sack lunch,and somebody would talk about whatever was interesting to him. That's the kind of thing that has gone away. I think this is in part due to the growth of the general campus itself, as well as the growth of the institution.

RC: Is the University of California at San Diego a good idea for Scripps Institution?

FS: I guess my opinion about that has had a lot of ups and downs. I think in the long run, yes. It was.... Well, perhaps one shouldn't say good or bad. It was sort of inevitable. The Scripps Institution was part of the University of California; and, as long as it is to remain a part of the university, it would have been silly, I think, to consider establishing a part of the university here in San Diego and not having

it be tightly coordinated with Scripps. As it turned out, I think that the existence of Scripps made it possible for us to build a very strong science faculty quite rapidly. I think the other parts of the University of California, the new campuses of that same era, have not achieved anything like the scientific imminence that the San Diego campus has. We're right up there fishing along with Berkeley and Los Angeles, in spite of the fact that we're only about a third as big. And I think in large nature, this was due to Revelle as an individual, but also due to the kind of high-grade science community that we had here in the specialized realm of the ocean. And a number of well recognized people like Eckart obviously made it easier to recruit good physicists to come. They felt, you know, it was no sin to go to San Diego, if Carl Eckart had done it, and so do I. So, we think that was a big help.

RC: As director and, well, primarily as director at Scripps Institution of Oceanography, and your responsibility in terms of recruiting students and recruiting faculty members, did you feel as if the prestige of the institution allowed you to have sort of a choice of the upper echelon of science in students?

FS: I think in the case of students, for a very long time Scripps Institution was "the place" if you wanted to do graduate work in oceanography. There was no question that it was the outstanding place to go. As the world has developed, other places have grown, too; and I think that the other two places that really compete strongly with us now are the MIT Woods Hole Complex and Columbia University. Those are the other two that we battle with occasionally these days for the best of the incoming students. In terms of recruiting staff, that's again a kind of place in which we do not have too much trouble. Again, we do indeed compete primarily with MIT and Columbia, particularly the MIT group

in the geology-geophysics side of the world particularly. They provide some fairly formidable competition. You were asking about whether the development of the university, or the full scall campus, here was something that I thought was good or bad. It had a very interesting effect on the Scripps Institution in this period in which I had to sit in the director's seat, because that was a period of some really deep soul-searching on the part of a lot of people in the Scripps Institution. As the new people arrived in science, there was a very strong sort of centrifugal effect that all of a sudden here were an eminent chemist, eminent physicist, and so forth. And if you looked around, you could see the physicists and the chemists in the institution beginning to think less about the ocean and about working together to solve the ocean's problems than about what kind of interaction they were having with the physicists or with the chemists, the people in their own disciplines. We'd been living in a kind of vacuum with regard to that for many years and so there was a strong centrifugal effect. And I felt the main thing that I really had to work at, while I was director, was the business of just keeping the institution from flying apart. Gradually this wore off, and we went through a completely different stage some several years later in which the problems of general education and things like this--people who had had the luxury in earlier times of being able to specialize and not having to interact with too big a group and all that sort of thing--these problems began to overwhelm people, and there was a great rush back toward the Scripps Institution in the sort of late '60's. In fact, there were people on the upper campus, as we called the general campus, who wanted to join the rush. And there was a department, the whole department of the earth sciences department, which had been built within the first college up in the UCSD general campus, (the earth sciences department staffed

about half with Scripps people and the other half with new recruits from various distinguished places) and that department just plain completely clovered. And all the people dashed back to the Scripps Institution because their interests were with really doing oceanographic research. And I guess this is the place that underscores the fact that Scripps is a sort of an anomalous place in the University of California. It's a research institution with graduate educational responsibilities and public service responsibilities. I don't think there is another part of the University of California that is built like this. And this means, every time we change administration and a new chancellor on this campus or a new president of a statewide organization, we spend some fair amount of time having to go through indoctrination kinds of things to remind these people that this is. Well, the Scripps Institution part is neither a conventional university research institute, nor is it a conventional university department. It sort of sits in between these. It's far more a research organization; however, it's also a public service organization with in the University of California, being the basic documentation that talks about what is the mission and the goal and so on and what kinds of contributions do they expect faculty members to make. The three things that are brought forward are teaching, research, and public service. This third item is much more of a factor, I think, in a state university than in a place like Harvard. It really dates back to the days of the agricultural impact and all that sort of thing. But Scripps Institution has much more of the flavor of this public service thing than other parts of the university and, in fact, is a major part as far as university support. It's funded out of the same budget as the budget that supports much of the agricultural research and so forth and the organized

research budget, so-called at the university, as opposed to the construction budget.

RC: Now, when you become chairman of the oceanography department, does that really change your role at all in Scripps Institution?

FS: Yes. Well, I was chairman of the oceanography department back in 1963 or '64. I passed that along to somebody else when I went back to being director of Scripps again. In the late '60's, that department was reorganized, combined with the marine biology curriculum we had and this previous existing earth sciences department, and we established a thing that we decided we would call the Graduate Department of the Scripps Institution of Oceanography, which essentially is an organizational entity that looks out for the students in a sort of formalistic way. The students, since these are all graduate students and nearly all Ph.D. type, the students have to become involved in research. And that involvement has to work through the research-administering subdivisions of the institution: the laboratories like the Marine Physical Laboratory, the research divisions, and that sort of thing. But what we had found out in the early '60's was that if we didn't have somebody who was charged with seeing that the students, in fact, had some financial support and that they, in fact, moved fruitfully towards finishing their degrees as opposed to getting the research done--those are two rather different things. We would have students that were around for eight or ten years and as oceanography became more widely known in the '60's, the number of applicants went up, and it became clear that there was no way that we could take in the numbers of people who were applying and expect them all to stay around for six or eight years. In fact, there was no reason for them to stay around that long. So we established this departmental structure which then sees that students take their

qualifying examinations at reasonable times and that committees are properly appointed and things of that sort. And essentially I took over the responsibility of the chairmanship of that department just this last January,so I'm doing essentially a second tour as department chairman. It does change your role, or it adds to your role, I suppose--because I didn't drop any responsibilities when I took on this one--in that you have to think about the role of the institution in a different way, in terms of what kind of a contribution is it making specifically to the students as opposed to what kind of contribution is it making to oceanography.

RC: Now, when you're Scientific Liaison Officer for western Europe, what does that mean?

FS: That was an interesting interlude. The Office of Naval Research has been overall a very enlightening organization. It really started out right after World War II as the only really effective supporter of research in the United States. That role is no longer its role anymore; the National Science Foundation has grown in strength and purpose and capability. But one of the things that the Office of Naval Research did, starting right after World War II, was it established an office in London whose initial goal was to help scientists on the European continent to get back in action after World War II. It was pretty successful at doing this, partly by providing communication liaison, finding out where people were, providing some monetary support, a lot of equipment,and things of this kind. That went on for several years and then, of course, the scientific community in Europe became its own community again. But the Navy decided that it would be useful to maintain, for the benefit really of the entire U.S. scientific community, some kind of a liaison with the European community,

so that this London office stayed in existence, staffed by people who are scientists on leave from various universities or government laboratories or whatever the typical duration of the tour might be--well, they'd like them to be two years or something like that; in recent times, it's been difficult to get people to go for that long. When you go over there, the name of the game is, in fact, to make contact with as many people as you can throughout Europe in whatever your field may be and go visit them, learn about what they're doing, but also to help them to find out what's going on in the United States. So, it's sort of a two-way street, and you don't go off visiting without providing something in return--make a seminar of your own while you're there or a great deal of helping people to be in touch with particularly the sort of gray literature that most of the scientists in Europe are just as good as we are about reading the publications, but they don't always have access to reports that are done within ONR contracts or NASA contracts or something like that, which sometimes have very useful information in them but which are not always easily accessible to the people in the European community. And so one of the ways that you can help people over there is to put them in touch with that sort of thing and with other people in the United States who are working on things that seem to be similar to theirs. I went over there in the summer of '74 and came back in the fall of '75. In my case, it was particularly nice because it was a chance to expand a little bit on my areas of interest in that I was the only person on the staff at most of that period either in acoustics or in oceanography, which meant that I could go around and see what was going on in at least acoustics in diagnosis in hospitals and things like this, or go off and see what was going in fisheries research and things that I have a marginal interest

in but don't always have much time to keep track of while I'm involved here.

RC: The Conrad Award for contributions to the Navy of Science Program in '74. For what was that given specifically?

FS: For good behavior. That's an award that is handed out by the Navy to people who played a role, not necessarily as individual research people, but in pulling together, stimulating, seeing that the research goes on a fruitful way, in a way that's fruitful to the Navy. It's an award that over the years has been given to a number of directors of naval laboratories, upon occasion directors of research groups, an occasional admiral here and there who has made major contributions to research activity. This laboratory and the Physical Laboratory have been successful within the ONR context over the years, that eventually got to be enough that they decided to make the award.

RC: What about the award from the Franklin Institute in 1965?

FS: That was a different kind of thing. It was really related to Flip, which was a vehicle that I had my hand in bringing into the _____. Well, it's like a lot of other medals. It's hard for science these days to give a medal to a paid person, because in general you don't do anything alone. In fact, in this particular instance, I guess, the main thing I can say is I raised the money and did some of the design calculations. I guess I wasn't the only doer by any means, but I suppose I was the wheel (?) in getting it done, including the laboratory. That was a craft that was a lot of fun to do and has been an important tool for carrying out quite a wide range of research things. And it grew out of a Navy program in which I was a part and could see that there was a role that our laboratory could play. And we sort of visualized this craft that we could take out to sea. And, well, I

mentioned the fact that we used to go to sea in submarines to hold hydrophones nice and still; well, once we had Flip, we stopped going to sea in submarines because we could hang hydrophones nice and still in the water. And there were only about half a dozen of us out there, scientists, and the remaining half a dozen crew men, instead of half a dozen scientists and 80 to 100 crew men, which you generally have in a marine military submarine. I think it was really pretty good.

RC: What exactly was the innovation with Flip?

FS: Well, it was.... The idea was to provide a platform which would sit stably in the ocean. And we were initially studying a thing that is kind of like the twinkling stars, namely, the fluctuations in apparent direction of a rattle of sound as it's transmitted through the water. And so what we wanted is a place where we could mount hydrophones down well within the water column--in this case, down, say, 300 feet, which is far as the bottom of Flip and then one could go off in a distance and hang a sound source down from a ship, and you could sight from up at the top; out in the air, you could sight off to where the sound source was. And at the same time, you could determine the direction of the sound and how that direction fluctuated back and forth because of the small temperature differences within the ocean. And so it gave us this kind of a rigid structure, and, as with other things we've done, the idea was to see if we could put something together that would solve one problem, but would be flexible enough that it could solve a bunch of other problems later on that we only vaguely had assembled. And, in fact, it's been pretty successful in regard that it's a good thing for hanging things down farther in the water.

RC: Now, it's been suggested, at least, that the '60's were, in effect, particularly the middle to late '60's, were sort of an age of invention

which brought together all kinds of techniques to sending oceanographers out to sea. This may be over, that oceanography may now revert to having more and more work done in the lab and systematic work with the data already produced. Would you agree with this?

FS: Well, to some extent this has to be true, partly because communication has become so much better, and we've developed, or are in the process of developing, ways at looking at some aspects of the ocean anyway, using things like satellites, for example, where you might just as well be sitting in your lab as any place else. It doesn't do you any good to be on a ship, if you're going to settle with data as if it's going to come pouring down out of the sky by whatever radio he uses. And you can sit at home near your computer and try to understand what that information means. I think, on the other hand, there is much... In some ways, there is less of working in the laboratory--I shouldn't say that; we need to be careful of the laboratory--working in the data manipulation business after the operation is over. It used to be that you'd go out and make some measurements, and you had to come home and work those over very, very heavily before you could write a paper. The ability to take computers to sea with us and to take more sophisticated instruments for doing chemical analyses, whatever, also, has meant that the turn around time for a problem of given level of complexity has gotten to be a little shorter. So we have these two things kind of working against one another when you try to guess what the oceanographer of the future is going to do as far as going to sea. On one hand, he has the capability to go to sea and get his data in shape to draw some conclusions a lot faster than he could before, so that his turn around time to get ready to go back out to sea and find something else has increased. In fact, almost the limiting factor there is the publication

time or something of this kind, because the granting agencies like to see something in hard print before they like to keep on funding you to go on out for the next trip. And yet the early part of the data analysis can go very, very fast. So there is that working against the fact that there are other buoys, satellites, and things of this kind that make it possible to do perfectly good oceanography without ever going out aboard ship at all.

RC: Okay, what particular problems in the immediate future do you see oceanography addressing itself to?

FS: There are two kinds of problems. There are scientific problems. The air-sea interaction problem is a major one that is attracting a lot of attention and is going to require a lot more attention before it really is solved. The biological questions, you see, still remain kind of elusive in terms of what one can do about them. I think that the geology, geophysics part has benefited greatly from improved instrumentation--building to make measurements that are relevant in quite a variety of ways. The geologists who 10 or 15 years ago played just qualitative games with rocks and things like this, now know as much geophysics as geophysicists know. In many instances, it's hard to draw the line. They've learned to work with sophisticated instrumentation and draw conclusions from this in conjunction with whatever things you do with an actual sample. The biologists haven't gotten quite around to this place yet, partly because it's not as easy to solve their problems within the instrumentation side of the game, because at least you know where the seafloor is and it's likely to stay there. It's only going to move a few centimeters per year or something of this kind, whereas the biological situation will change very rapidly. It changes rapidly in both space and time: the question

patchiness of distribution of animals in the sea--plants and animals, why this occurs, what kind of scales, and how much of this is behavioral, how much of it is hydrodynamic, things of this kind. These are all the questions that I think are remained to be answered. There is question, in sort of sociological terms, I suppose, as to who wants to know about the ocean anyway, you know. The biological situation often is justified in people's minds by fisheries-type activities, but really--and that's a perfectly good justification in its own way--the question of what level of research activity you can justify, however, is a different matter. And this is why I think that some other aspects than the fisheries one are beginning to drive the biological situation in the sense that people would like to know, if you spill a bunch of oil out of a ship, what's that going to do to the biological situation in the sea. A supertanker full of oil represents as much money as an awful lot of fishing and so, although the destruction of life in the sea, or something like that, is approached on a sort of esthetic basis almost, there's the question of whether people are going to exact retribution from oil tanker drivers who do pollute the ocean. If they are, then there has to be a lot more argument about marine biology involved in all that, because the tanker drivers and oil companies are going to claim that, in fact, you know, I haven't done anything to the ocean anyway. The amounts of money that are going to be involved between governments trying to just maintain the nice values of life may all get too involved in the money than you think of in the fisheries business.

RC: And finally where do you think your career is going to go?

FS: I don't know. I have a couple of...I have some research things that I hope I'll be able to work on, but it seems to me to be interesting whether I can convince other people that they're interesting enough to fund them. I'd like to do some very careful measuring of the displace-

ment of the floor of the ocean. The tectonics model that has come out of recent geology and geophysics certainly says that pieces of the crust of the earth are sliding around and people are beginning to be able to measure these motions directly on land like around San Andreas Fault, in Iceland, where the spreading comes up to the sea surface. And I'd like to go ahead and make some measurements of this kind on the deep-sea floor. This, I think, is within the realm of technology. I guess I'm getting to a place where I probably have to think rather more about looking perhaps at a broader picture, and I've tried to look at the broad picture for a long time. But, I think, I question interactions outside of the sphere of just this laboratory and this institution. There are numerous opportunities to do some other kinds of things in that area, and perhaps that will happen.

FS: Well, there is some activity going, I think, on the history of ONR perhaps right this year, because this is their thirtieth anniversary and they're asking people to write articles. And I wouldn't be surprised if they have a historian somewhere who's worrying about this. I think that certainly in the period just after World War II, there's no question but that the Navy built oceanography, at least in the United States. Well, there just had been no deep water oceanography in the United States before World War II. On the other hand, after World War II, it grew very, very fast. And it grew because, well, there were two kinds of contributions by the Navy, three kinds of contributions of the Navy. One was the obvious one of providing dollars for research. There was a second one, which was that there were a lot of people who, one way or another, had become interested in the ocean during World War II by being naval officers. And, in fact, if you look around, you'll find a fair fraction of the people of my

generation in oceanography who were indeed naval officers,or somehow or other got involved in either that, or were involved in naval research during World War II. It was just at that time when there was a lot going on. The third thing that happened was that the Navy had built a lot of ships during World War II and didn't need them all when the war was over. And so there were a number of institutions that were able to put their hands on floating facilities in the period of the '50's, that they never would have, that just plain never would have existed, if it hadn't been for the naval build-up during World War II and its subsequent decline afterwards. We had Navy tugs and things like this that we operated as our key research ships in the early days of the institution. Well, this went on long enough that the Navy began to feel that it had a responsibility for providing research ships and went ahead and started building a whole class of research ships. And of the ships that Scripps operates right now, a fair number of them, all the biggest ones, are hulls that were provided by the Navy. The Navy doesn't fund the operation in any sense, but we have to justify the operational funding on the basis of research programs. And some of those research programs are Navy programs, some of them are National Science Foundation. But the ships are made available to us without any...we don't have to pay any rent or anything of this kind. They're just plain mailed to the institutions partly on the grounds that the programs are indeed federal programs. And I think that this is a fairly important kind of thing. It's given our Navy a really strong base in the scientific world and the capability to do things and to call on people for advisory help and to have a good base of understanding the ocean that probably couldn't have existed in any other way. I think that really you look at the different populations

in the United States, the fisheries people, the manganese-nodule people, the oil people, or whatever; and you see that, at least I think, that if you look at the deep ocean, the major consumer of information about the ocean is the United States Navy, so that they have a vested interest in seeing that there is good work done. But I think that people who work in the sea also have a kind of vested interest in seeing that the Navy operates in an intelligent manner and so have an obligation in return to conceive good research things and be sure that they're well understood once they're finished.

RC: The ONR has, of course, become more missioned-oriented rather than research-oriented in the very recent past actually.

FS: Yes. I guess that may be one way of saying it. I think I would look at it in a little different way. I think what has happened is that science has grown fantastically. The Office of Naval Research budget has not grown in pace with this. I don't think that ONR, in its major oceanographic program, has necessarily become any more applied; it's that the field has gotten so big that the options have gotten so big and their budget has not increased in proportion so that they've had to say, "Alright, what are the things we're going to fund?" And so out of the range of scientific endeavors that they could pursue, you see, that had naturally, since they can't do everything, they've picked up the ones that seem to them to have the most relevance to the Navy. There's been a lot of talk about relevance, and there was a Mansfield Amendment gained where I felt was used by a lot of people to cut research budgets rather than to improve relevance. If you say that basic research is not relevant, why, then you could count almost anything. On the other hand, there is hardly anything you can study in the ocean that isn't in some way... A knowledgeable person can show

that it's in some way relevant to the Navy. And so the question of cutting down the oceanographic research on the grounds of lack of relevance is pretty hard to justify. And, in fact, if you look at what happened to ONR during that period in which that was going on, it squeezed its programs; but it squeezed the non-ocean programs a great deal harder than the ocean programs. I don't know, I guess that oceanography is sort of like a third of our budget at this point, whereas in other years, there was a lot of nuclear physics and things of this sort. And basically I think the decision was that the Navy would plunge very heavily in the basic research areas from which no other mission-oriented agency in the federal government had much at stake. And so this pretty much says that other people who fund, that the early people who fund, nuclear things and new uses of nuclear power won't put much money into basic research in that category; but they'll put a lot into oceanography, because there aren't many other agencies that have the radius.

RC: Do you think that the funding ONR did earlier, when it just simply gave lump sums to institutions to be sort of parceled out to researchers, think it was more creative then than it is now, when it asked for specific proposals?

FS: Yes, I do. Well, what's happened over the years is that, as we moved from the sort of block funding things into more and more narrowly defined projects, on the one hand.... The amount of paper work that you have to do for a research dollar these days has gone up and is probably a factor of 10 in the last 15 years. It's way, way up. It's primarily the result of just plain projectizing everything and cutting it down into smaller and smaller pieces. It's around now to where International Science Foundation, for example, the average size of

grant in oceanography has not gone up in the last half dozen years, in spite of the fact that inflation has gone up. And it's clear that to get a given amount of work done, it costs more money. What this means is that people are dividing their work up into smaller and smaller pieces, and this then means that the money is compartmentalized and you have to plan the spending on each individual little project to within a tenth of a percent, whereas in other times.... I think people may be under some misapprehension with regard to this block funding sort of business. There was always a program presented,and certain investigators were clearly going to be supported. It was not just a sort of chunk of money that disappeared in the director's office and came off the other end according to his whims. He had to present a program. The big advantage was that you were ~~xxx~~ sort of morally bound to put the money pretty much the way it went, but this was sort of to within 90 percent instead of 99 percent. And that meant that, if somebody within your organization had misjudged his budgetary requirements, usually there were about as many people who misjudged them high as low, ~~and~~ you could move the money around and compensate for this. And everybody could get his work done right, and the money will be spent in the most fruitful manner possible. I think that money is not spent as fruitfully when it is divided into well trimmed packages.

RC: What about ONR and, well, your own NSF, in effect, encouraging and creating departments of oceanography? Do you think this is essentially a good thing?

FS: Well, I don't know. Up to a point it certainly was a good thing, because oceanography had grown, and there was no reason that it should grow only in places where there had been oceanography in 1948 or something like that. So, in that sense, it should have been done. It had to be

done. Certainly if you took three to four big places and said that they were going to handle all the oceanography, all the academic oceanography, they'd be considerably bigger than they are. It's not fair that the world would be better off. I think there was sort of an overdoing of this, and we're seeing that now. And we've felt a kind of retrenchment,and groups are kind of withering and dying. I think that there was no way to know what the outcome would be, and so there was an expansion which became over expansion, which is now turning into contraction, which I hope won't become over contraction.

RC: In terms of naval stimulation of these departments, do you think possibly scientific research is suffering now because of the necessity of ONR in dividing its funds up to a multiplicity of departments rather than a few institutions?

FS: I don't know. I think that ONR--I can't speak for ONR, of course--but I think that they are gradually adjusting themselves to the idea that they can't support a large number of places, and that there is a certain kind of.... You have to have a certain dimension before you can operate a ship effectively and things of this kind. And it probably is best to have oceanography done in a fairly modest number of fairly good-sized places rather than supporting work.... I think this happens naturally to some extent with the people who are in disadvantaged institutions, institutions that just plain can't quite get up to the threshold. They can't do as good work, and so they don't fair as well when proposals are reviewed because they don't have the facilities and they don't have the...another part of the game is the strong interaction with other people. I've been invited by a variety of places to leave here and go off and take a job, be director of this or a professor there or something. And every time I've searched my soul and stayed here. And one of the major reasons I've stayed here has been

that there are people here that I can talk to who are the caliber that I can't find in this kind of quantity if I go off to some other place, and I enjoy that. I think it helps me to do better research, and so that's a part of having a big enough place. A lot of people think Scripps has gotten to be too big. I haven't heard of anybody who really left because of that reason. In fact, most people who are here, even the ones that say it's too big, would really like to have one more post-doctoral, maybe a couple of more graduate students, or maybe an electronics technician and someone who seems almost to go that way.

RC: Now that the research facilities, let's say, are divided up between NSF and ONR, do you see several projects which emerge which somehow seem to fit neither NSF nor ONR standards and thus fall between the cracks in the floor funding, if I may use that metaphor?

FS: I don't know. I don't really recognize any major problems in this regard. There is a changing pattern in the sense that people are.... There are other agencies coming into working in the ocean, particularly the near shore part of the ocean, and some of them want information about the ocean. And people in the institutions are willing or interested in providing that information in return for the chance to get out to sea and learn some things that they would like to know for their own research. By and large, these other agencies are not as skillful at working with research people as the Office of Naval Research has been historically and as NSF is reasonable. So groups like ERDA and Bureau of Land Management and so forth find that the scientific community finds it very difficult to do things frequently with them. And the fault is probably on both sides of the venture in the sense that one thing you do have to recognize is that places like the Bureau of Land Management or whatever have only a kind of transient

interest in the ocean. They want to find out about the strip that's going along the coast; and once they have that, while there is a lot to be learned there, they don't feel in the same sense that the Navy does or that people charged in NSF with oceanography, that the ocean is something that is their primarily scientific object of interest.

RC: Traditionally, ONR and NSF, well, now even the National Academy of Science, have drawn the people who gave them guidance, I mean to say, from oceanography, sort of a moving of civilians into these administrative posts. Do you think that's good for the military to do that?

FS: Well, of course, it.... I'm not sure exactly what you mean.

RC: Ned Estenso, Richard Vetter, Fain Jennings.

FS: Yes, but you mean to say, is it good for the military to do that? I'm not sure which way we're thinking of the migration as taking place. A lot of the people...some of the people you've named are people who were in the Navy establishment as civilians and went out of it, off into NSF and other places. If you look around Washington in the oceanographic administrative business, you'll find, I'll bet, that half of the leading administrators did a tour, did their first administrative work in the government in ONR. Gordon Will over in the National Ocean Survey, part of NOAA, was one of the leaders in the geophysics branch of ONR way back in the early '50's. Fain Jennings was one step after, I mean, between the charge of Lord Maxwell who was at Woods Hole and Will, Maxwell, and Jennings and that part of ONR. Jennings, of course, is the ideal _____man now. They've just the places to shop: NSF at ONR. I think that.... I enjoy, in the Navy research business, interacting to some extent with the naval officers, I should say, as well as with the civilian scientists. They take a completely different viewpoint, and there are times when I appreciate a pragmatic

viewpoint, an applied viewpoint, on some problems perhaps more than some of my colleagues would. I find it's fun sometimes to try to cope with a problem of how to find something on the bottom of the ocean or something to that effect. It kind of tests my ingenuity and maybe not add a lot to my understanding of the ocean--if we ever will understand the ocean--but it may help somebody solve a problem.

RC: There's been a great deal of pressure--let me begin by asking you a loaded question--there's been a great deal of pressure in the '60's on the part of young scientists in cooperation with the military--now, I really can't say, you know, sixty percent of the scientists.

FS: Pressure against.

RC: But I mean pressure of institutions not to be as cooperative with the military. Did you,as a scientist coming from the military, ever feel this kind of uneasiness in terms of cooperation with the military?

FS: No, I haven't felt any uneasiness. I guess that's partly because of the situation in which I found myself, in that I guess I sort of went at it with a missionary viewpoint. I figured that I had enough independence sitting in the University of California to have enough audacity perhaps to think that I could influence the military to do things in a more sensible way in the areas in which I understood. And I think that's been true of some of the other ones who have played this game, who have done it with the feeling that we were really helping to keep the military from getting in any worse trouble than they would have gotten into or something like this. I have a lot of respect for the Navy people, and I have had practically no contact with Army and Air Force type people. And there are a lot of Navy people for whom I have a great deal of respect, and, I think, have worked very fruitfully. And I think it's been kind of unfortunate that that move in the late

'60's, the Vietnam War and the rest of it, and among the other things--I think it was strongly the Vietnam War that has made it take that point--I think that's been bad for the country,in the sense a sort of division sprang up in that the scientists who were young in those days developed a "standoffish" attitude toward the military, which in the long run has meant that they don't have enough perspective to see what some of the problems are that are relevant in the ocean, and at the same time, it developed on the part of the military people the reaction that said, "Well, to hell with you." And what that has lead to is the growth of the inhouse Navy laboratories to a quite substantial size, much bigger than anything we had before. And those laboratories serve their function. They get things done for the Navy, but they don't provide much of a backtalk kind of aspect, which is what the academic community can provide if you know enough about the problem and feel some responsibility for helping to solve it in a rational way. You can have a little less compunction about pointing out when you think the other guy is doing it the wrong way. And the people in the Navy laboratory have a very difficult time of seeing how to do that.

RC: There also have been complaints rising in the scientific community about the interaction of oceanographers in particular with private enterprise as consultants and so forth, on the grounds of, obviously, ecology in the oceans. Have you ever felt any uneasiness about this sort of association?

FS: Well, I don't know. Not really. I haven't had any of that sort of association in particularly myself. I can't think of any really good examples of professionals that have been running into trouble with this sort of thing. There are a lot of marginal oceanographers who rate environmental impact statements, and they're doing the best they

can. And I don't feel that they're necessarily being pushed or pulled one way or the other. They're just doing their job, and sometimes, in fact, I suppose you've got to expect that an environmental impact statement may come out in such a way as to favor the development. There's this sort of possibility. That's not the thing that I thought to be particularly bothersome here at Scripps. I've been aware of odd situations. I was sitting on an advisory committee for the governor of Alaska temporarily, for a short period of time, an ocean _____. I found that University of Alaska had done a thing that University of California would never have allowed to happen. They took some money from the pipeline company to do an oceanographic survey in the region of Evalees, where the tanker port is going to be beside the pipeline. That in itself I have no objection to. The thing was, however, that they were willing to take with the money a restriction on release of the data to anybody for five years, something like that. And"anybody " meant even another part of the Alaskan government. And they found themselves in a very awkward position when the Alaskan government fisheries groups and so forth started doing physical oceanography. And they had to do the whole thing over again. They couldn't use the University of Alaska data because of the agreement that they'd loaned the money. That's the kind of thing that can give the whole game a bad name, it seems to me, and it's not a very easy situation.

RC: Now, I want to return to ONR, if I can for a second. In terms of ONR's funding of projects, was it your experience that the things ONR was interested in tended to attract the scientists to it, that is, the grants themselves sort of pre-determined research? Let me try it that way.

FS: I don't think so. It's always hard to decide. I guess I was using

some words like "basic research" and things like that, while I find those really are not very satisfying terms. I think, in fact, that one man's basic research can be another man's applied research. The doer doesn't have to even have the same evaluation of it as the sponsor does. And if you run a good institution, why, the idea is to see that the people who are interested in certain kinds of things make contact with sponsors who are interested in those same kinds of investigations. That's a kind of idealistic picture. It doesn't really work that way except perhaps in _____, but nevertheless that's the way ideally it should work. These days I think that a scientist is faced with enough choices about what to do that you can't claim that you do a particular experiment just because in some abstract way it is more scientifically interesting. You're rattled around by your peers in terms of what you're working on, and you're rattled around by the funding agencies in terms of what they're interested in putting money in. On top of that, you're rattled around by just what kinds of tools you have, what your own capabilities are. And with all of those, you have a lot of options as to what the next thing is you're going to work on. You sort of take a weighted average across all of these different things, and sometimes you'll come up pushing pretty hard to work on something where the funding doesn't come very easily or it may burn out, in fact, that funding and your own inclination go in the same direction and go pretty easily. I think that what you wind up with, of course, is some sort of natural selection in the sense that physical oceanographers have an interest in a particular kind of problem and are more likely to be supported by ONR than those who aren't interested in solving that kind of problem. This is why I am very much opposed to various congressmen's ideas about having a single

agency for supporting ocean interests and these sorts of things, because if there's only one, then you'd only have this single monolithic thing to deal with and probably the kind of research we can do would lose its sort of resiliency and diversity, which now exists because you can do certain things within ONR's sphere. You can do other things within NSF's sphere. If neither of those like what you want to do, why, you can go try Bureau of Land Management or Geological Survey or whatever. I think we'll have a healthy community as long as we have this diversity of interests on the part of the users. And I think it's a mistake to try to think that you can fruitfully take the needs of some agency and represent them in some other agency and expect this as going to be having fruitful results. But if an agency needs to know about the ocean, it needs to have some people working for it who, in fact, know about the ocean or are trying to learn about the ocean. And this almost means that it would be real. Even within the Navy, I have regretted some of the moves that they have taken to try to centralize oceanography. I think that there is a limit to how far you should go in that regard, and you really should leave some of the oceanography scattered out around, because it would be used better than if it's just lying there on the desk next to you. If you're sitting in a building where there's an oceanographer, then you're more likely to think about the ocean.