Dr. Fred Noel Spiess
Emeritus Professor of Oceanography, SIO, UCSD

Interviews conducted by
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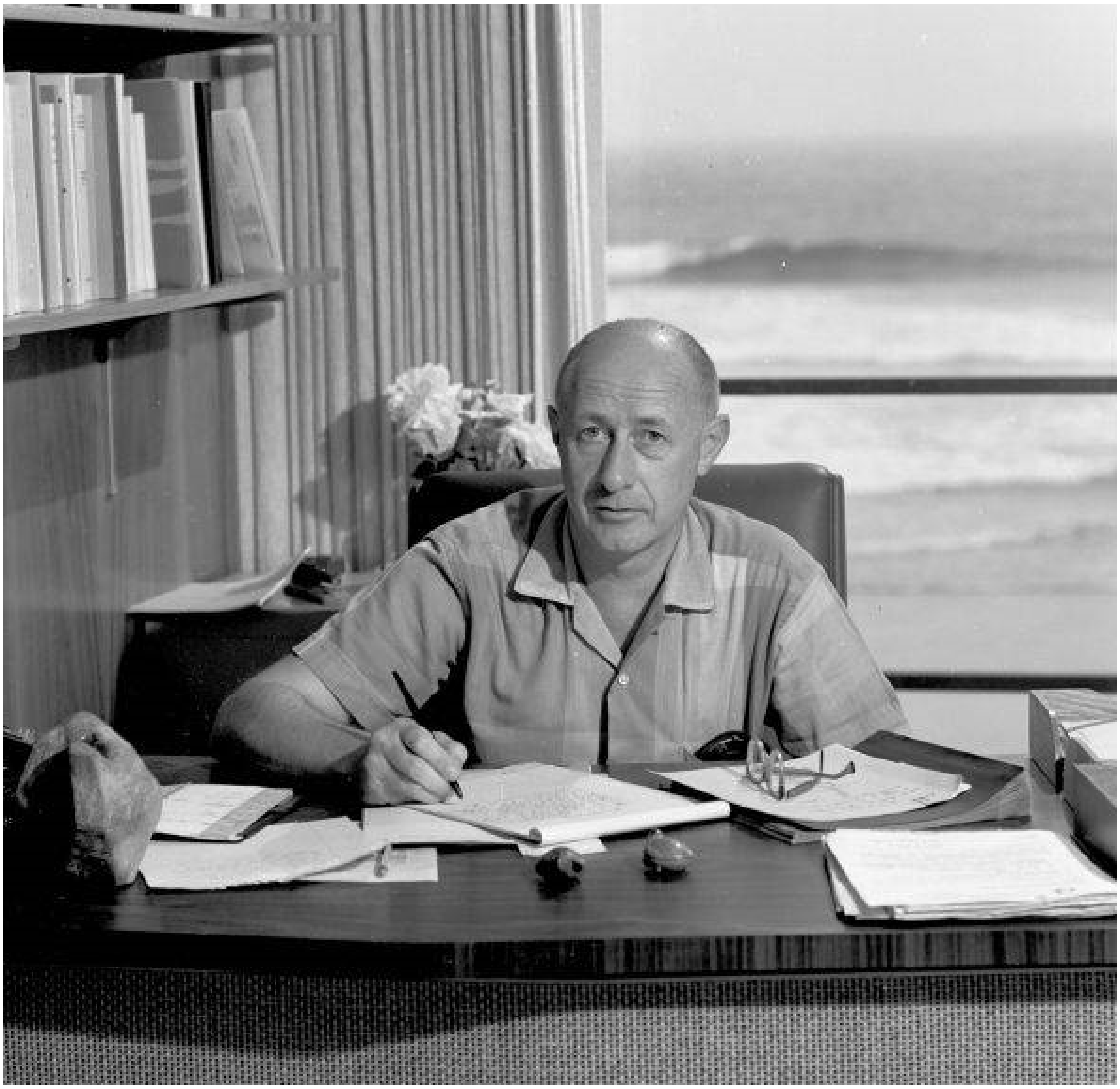

Fred Spiess, May 1967

Dr. Fred Noel Spiess joined the Scripps Institution of Oceanography in 1952, beginning his career at SIO by running an acoustics laboratory in the Marine Physical Laboratory (MPL). Dr. Spiess soon became director of the Marine Physical Laboratory, serving in this capacity from 1958 until 1980. In 1980 he began a term as director of the Institute of Marine Resources (IMR) and headed this organization until he officially retired from the university in 1988. During his career at SIO, Dr. Spiess was perhaps best known for his part in developing research instruments—such as FLIP[1] and Deep Tow—used for exploring the physical properties of deep ocean acoustics, geology, and

[1]Floating Instrument Platform

geodesy. Dr. Spiess's career is also of interest because of his service as acting director of SIO in the early 1960s—a period of great change for SIO as the nascent University of California, San Diego campus developed. Dr. Spiess remains very active as an Emeritus Professor: currently, he is serving on the UC Academic Senate Task Force that is planning the new UC campus at Merced.

The interviews were conducted in four parts over the course of several months in the late winter and spring of 1999. The interviews were carried out on January 20, February 2, February 9, and April 27. The long period between the third and fourth interviews was used to let the transcriber complete the first three transcripts and to allow both Dr. Spiess and myself time to review the first three transcripts as well as plan topics for the final session. In all cases, I prepared questions to ask Dr. Spiess before each session and he compiled a mental log of issues and events he wanted to include. In some cases there simply wasn't enough time to include all the questions I had hoped to ask—each session was planned to take approximately one hour.

In general, the sessions cover Dr. Spiess's life chronologically, but the sessions also have thematic emphases. The first session covers Dr. Spiess's early life and his Navy career, featuring some general questions on the influence of World War II on his generation of oceanographers. The second session follows Dr. Spiess into his administrative career and includes discussion of the founding of UCSD and its impact on the direction of SIO. Session three focuses on Dr. Spiess's career as an instrument builder and devotes considerable time to talking about the development of FLIP and Deep Tow, but also discusses his move into more geologically-oriented research. The final session is broken into four main topics: Dr. Spiess's work in the Academic Senate, his term as director of the Institute of Marine Resources, his feelings on changes in funding for oceanographic research over the years, and some final reflections.

I began preparation for this oral history by examining many collections of documents in the SIO Archives, including the Office of the Director records for the period when Dr. Spiess was acting director of SIO, and many boxes of files from the Marine Physical Laboratory. Dr. Spiess also has an extensive bibliography of published works which I reviewed.

My thanks to Deborah Day, archivist at the SIO Library for her advice and guidance toward the completion of this oral history project.

Christopher Henke, M.A., C.Phil.
Project Coordinator and Interviewer

September 14, 1999

Department of Sociology
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INTERVIEW ONE: 20 JANUARY 1999

Henke: ##[2] So Dr. Spiess, just to start off, maybe we can get some basic biographical information about you, like where you were born and when, and maybe just spend a couple minutes talking about your early life up to the point where you started college in Berkeley.

Spiess: Well, I was born on Christmas Day in 1919. [Being] born on Christmas Day, my mother decided that my name would be Fred Noel Spiess, Fred being my father's name.[3] And so since Fred was my father's name, to ameliorate confusion, I throughout all my young life was called by my middle name. I was Noel, not Fred. And that went on clear on through college until I went active duty in the Navy, and then my orders would go on ahead of me and [I] would go on board and [people would say] "Hi Fred." Fred was a perfectly good name, was my father's name. Nothing wrong with that. And so in the Navy I became Fred. And so Fred is the one that has pretty much stuck in the professional part of my life, but within my family I am still called by my middle name, Noel. So in thinking about all this there are going to be some people who know me as Noel and some people who know me as Fred.

My mother and father had been married while my father was in the Navy during World War I. He had been a sonar man. They didn't call him that, but he was on a submarine chaser during World War I. He had grown up in New York City—parents who had come over to the United States from Germany in the late 1880s. My mother's family, her forbears, had come over much earlier. They came over before the American Revolution from England and Scotland. And my mother had been born and raised in Oakland, California, and that's where my father eventually settled after he ran away from home in New York and thumbed his way across the country. And that's where I was born, and grew up in the San Francisco Bay Area, pretty much staying there until I left, as we'll talk about later, to be on active duty in the Navy.

[2]The symbol ## indicates that a tape or segment of tape has begun or ended. For a guide to the tapes, see page 81.

[3]Parents' names: Elva Monck Spiess and Fred Henry Spiess.

I went to a good public school in Oakland, California for elementary school, and then we moved into a small residential community that is sort of plunked down in the middle of Oakland, a city called Piedmont. They had a very good school system, and I went to junior high school and high school there. I guess in terms of antecedents, my father's involvement with the Navy kind of rubbed off on me. And the world was perhaps different between World War I and World War II, in that the idea that one might have to spend some time in fighting a war was not a strange thing, and between my family and myself I decided that if I had to do something like that, it would be a lot more comfortable to be on a ship than it would be to be slushing around in the mud as a foot soldier of some kind.

Henke: Was this something your father kind of helped you to figure out because of his experiences?

Spiess: Sure. Well, there was also a lot of exposure in those days to the experiences of the people who had been over there in trench warfare in France, and that didn't sound very neat. So that was pretty much part of the way things went. I think that although my mother had only a high school education, she was quite intent on the educational aspects and intellectual aspects and competitive aspects of higher education and going on to do interesting things with one's life. So that was part of how it was as I grew up. There was a fair amount of music in the house. My mother was a reasonable pianist and singer in the church choir.

Henke: Were you always interested in science when you were growing up?

Spiess: Well, more or less. It's hard to say. There weren't science fairs and all that sort of thing in that era, so that there was no real organized outlet for that. But when I was in high school there was a really good math teacher that I had— just by chance—for about three years in a row, and he was the kind of person who looked at his class and would have a few key, nasty problems that he would hand out to those who wanted to be challenged by what was going on.

And our high school was somewhat limited in its outlook, I guess. The fact that I was interested in math and pretty good at that in the high school context meant that the advice that I had as I prepared to go to college was that engineering would be the thing to do. I don't think that people there had a real concept of the fact that you could be a mathematician or something. The students from my high school, nearly all of them, went to college, and most of them went to [UC] Berkeley, which was not far away, and the ones who had more money went to Stanford, and that was just about the extent of it. The idea that one would go across the country or something to go to college was not a particularly well developed thing at that time. So I went off to Berkeley to be an engineer, and started in engineering. Mechanical engineering was the direction that I went. And when I did that, one of the courses that I had to take was an engineering drawing course, and that didn't go too well.

Henke: No?

Spiess: The competition was really quite intense, because in that era there were a number of older people who had dropped out of school after high school and gone to work in the early thirties, when things were kind of tight as far as paying for college. And these people had mostly worked in drafting rooms.

Henke: They had design experience already.

Spiess: And they already could produce things quite rapidly. And I spent Thanksgiving vacation and things like this [catching] up with the drawing part. At the same time I did pretty well in the math part. By then I understood that there were opportunities of quite great breadth, and so I went and talked to the professor with whom I had done the honors math course as a freshman, and talked to him about the realities of being a mathematician. Aside from the fact that maybe I wasn't cut out to be a mathematician, his advice was, "Why don't you try majoring in physics?" I had done very well in the beginning physics course, too. "Why don't you do that? Because in either case you are going to have to do some graduate work, and there's a lot more support for graduate students in physics than there is in mathematics, because the Physics Department has to staff all of the physics courses that the engineers and pre-med people and everybody take, so that there are a lot of teaching assistantships over there." And so that in a practical sense, one could do mathematics if one were a theoretician in the physics world and still work one's way through.

Henke: So was it pretty much a sure thing then that you were going to enroll in the Naval ROTC[4] when you first started Berkeley, given your family background and such?

Spiess: Yes, yes. Essentially that was part of the deal. At that time Naval ROTC was a pretty elite kind of thing; it only existed in six universities in the country—Yale, Harvard, Northwestern, Georgia Technology, the University of Washington and the University of California, which was Berkeley, for all practical purposes, in the late thirties. And the enrollment in those things was only fifty people per class. I didn't make the cut for the freshman class but they took in generally half a dozen over the fifty at the freshman level, in order that they could cover people who were going to be dropping out or didn't make the grade. Following the summer cruise between freshman and sophomore year, I was moved over into the regular cadre and became just part of the NROTC unit. [I] wound up as battalion adjutant or something like that, that let me march out in the front carrying my sword.

Henke: So you were in kind of a leadership position not too far into your career with the

[4]Reserve Officers Training Corps, a program for training officers of the armed services on college and university campuses.

Naval ROTC?

Spiess: Right. And I guess if we go back into the physics part of all of this, once I had decided to do a physics major I transferred over right after my freshman year to the College of Letters and Science instead of the College of Engineering, which was the way Berkeley was organized in those days. And I had a good friend who was also a physics major, and he was much better plugged into the physics faculty than I was.

Henke: What was his name?

Spiess: His name was Victor Waithman. He had a cousin who had done a Ph.D. in physics at Berkeley, and so, starting my sophomore year, because of my interaction with Vic, I started doing undergraduate research project work with one of the faculty members there, Leonard Loeb, who was also a fairly senior Naval Reserve officer at the time. And so he was particularly interested in interacting with me because I was interested in the Naval ROTC and he was a Naval Reserve person. I think he was at least a full Commander at that time, which in those days was pretty impressive.

Henke: Right. Was it common for undergrads to have those research opportunities and to be so well-connected to the professors?

Spiess: The opportunities were there, but not anywhere near as much used, I think, as they are now. There was a regular Physics 199 in which you could enroll, and basically what it amounted to was that I was handed over to a graduate student and helped the graduate student build his equipment, and I qualified in being able to use the machine shop and things of this kind. And I worked for Loeb in that context for a couple of years, and then my last year I shifted over to working for Professor Jenkins,[5] who was an optics person. I had been interested in photography, as was my father. My father, back when he was in the Navy during World War I, had managed to set up an amateur film processing thing so that he could take care of the photographs that his shipmates made while they were overseas. And in high school he and I built a darkroom down in the basement of our house and eventually I wound up being the lead photographer for our high school yearbook operations. So I had some interest in optics and in my senior year I did some spectroscopy research with Jenkins, who was one of the faculty members who specialized in that sort of thing.

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[5]Francis A. Jenkins

Along the way I had been interested in [fencing]. I'm not quite sure [why]—I suppose I just was reading *Three Musketeers* type books [and decided] that I would become interested in fencing. And so when I arrived at Berkeley it turned out there was instruction in fencing and there was a fencing team. I dug into that, and was a regular member of the freshman fencing team my freshman year, and then from there on I was on the varsity fencing team. [During] my senior year I was captain of the fencing team. That was fun. We beat USC[6] that year. They generally had a pretty strong fencing team, because they would have people who were involved in things in movie studios and so on that supported that kind of activity, whereas up in the Berkeley area that was not so much. And I have fenced intermittently since then, but not very seriously. I guess going off in submarines right after I graduated there was no real opportunity to keep that aspect going.

Henke: Yes. There's not much room to fence on submarines, I suppose?

Spiess: No, not—

Henke: After you graduated you immediately went off and started active duty in the—

Spiess: Right. I was in the class of 1941, and when we went off on our summer cruise in the summer of 1940 our Naval ROTC class was out on an old destroyer off the west coast. And at that point the U.S. had started this thing called the Neutrality Patrol, in which U.S. Navy ships were used in the general vicinity of the U.S. shores to provide protection. Essentially to establish those zones as being neutral, in which nobody would be sunk, nobody would carry out military operations other than the U.S. And at that time when we made our summer cruise things were tense enough then that there were rumors back and forth within the ship that probably we would not be allowed to finish our degree work the following year; that we'd probably all be ordered to active duty early.

That did not happen, but as soon as we had our commissions, which came along with our diplomas in May of 1941; orders were forthcoming fairly soon, orders to active duty. And I suppose essentially the entire ROTC class, which was about fifty, were on active duty within a month or so after graduation. There were a bunch of us. There were a couple of officers who had been assigned as instructors for the Naval ROTC who had been in submarines. And in addition to that I had read some books about World War I submarining and found out that one of the lead German submarine commanding officers was somebody named Johann Spiess.

Henke: Oh really?

[6] University of Southern California

Spiess: I had no clue as to whether we were related in any way. Spiess turns out to be a pretty common name in Germany. But anyway, that obviously was an intriguing kind of thing. He had been the number two officer on the first really major destructive operation that the Germans did. That particular submarine sank three British cruisers in one day. And of course the fact that my father had been out hunting submarines during the same time frame—. Anyway, I think there were eight of us out of the fifty graduates from the Berkeley ROTC group who all volunteered to go to submarine school.

Henke: So you could have some kind of influence on where you would go?

Spiess: That's right. Well, as it turned out, they were having a big submarine building program and they really needed submarine officers.

Henke: I see. So if you showed any interested in it, then you were probably likely to go?

Spiess: And so we all volunteered and went off to submarine school—which again, was a pretty elite kind of thing in those days. They had a quota of about fifty and out of the fifty who were there, there were those of us from Berkeley. There were about a dozen Naval Academy graduates who were older than we were, because in this transition period there had been a rule of long standing that, if you were a regular Navy officer, you had to have two years of sea duty before you could go to submarine school. They didn't apply this to reserves coming on active duty, because we had just a couple summer cruises. And then in fact, because they were anxious to have enough officers for the submarines that were coming along, they had gone to the Georgia Tech[7] Naval ROTC unit and said, "We would like to have half a dozen people from this unit go to submarine school." I think they might have said, "Do you not want to go to submarine school?" and it was sort of a negative volunteer. Although, once we were at submarine school, one of the very first sessions was with a pretty hard-nosed Commanding Officer of the school who said, "Look, if any of you people want to get out of here, why just raise your hand." And the Georgia Tech people didn't do that. But anyway, that was an interesting experience because there was a mix of people who knew which end of the submarine was the front end and that sort of thing and a bunch of us who were pretty fresh-caught.

Submarining was a really neat thing for somebody with a good bent for classical physics. The principles of moments and buoyancy and all these other goods things, in the diving part of the game and in the submarine attack part of the game really were all just vector analysis. You had to do relative motion problems in your head and whatever. So it was kind of fun.

[7]Georgia Institute of Technology

Henke: Were there any problems that particularly interested you at that time, that really fit in well with your interest in physics?

Spiess: Well, it was a good general physics kind of thing. You could take what you had learned and apply it in a lot of different ways. And you took your courses in diesel engines and torpedo maintenance and so on. It was a three month thing altogether.

Henke: Was it typical for someone who had their degree in physics to be assigned to submarine duty?

Spiess: No, not necessarily. They were glad to have people who were interested and could make it through the course, and so the training course was in part a screening course, in a sense. Not everybody made it through. The Navy, in their educational programs, tends to be pretty up front about ranking people: one, two, three, four, five in your class, whatever it is. And the ranking in submarine school was based on your academic work. One of the components was a thing called an aptitude grade that was given to you by one of the senior officers. And out of fifty I think I stood about, I don't know, maybe thirtieth in aptitude at the end of the first four weeks. Starting at the end of the first four weeks, every Saturday afternoon there was a three hour exam in one of the specialties that we were doing. And suddenly my ranking started to go up. I knew how to do a three hour exam, and the subjects were all things that were pretty straightforward to control, and so my aptitude grade went up quite rapidly. That did include working with the diving trainer. This was a thing that, for its time, was quite an advanced—

Henke: Kind of a simulator?

Spiess: —simulator type thing. And it had the regular diving stations and the piece of the machinery in which you were, would tilt to the appropriate angles, and the instructor could crank up leakage here and there and present you with problems. And anyway, the upshot of it was that I stood first in the class by the time it was all done. And it was the first time that a Naval Reserve Officer had ever been first in a class at submarine school. There hadn't been that many Reserve Officers in the past to begin with, and the end result of that was that I could pretty much decide where I wanted to go next. There was a list of billets available and—

Henke: As far as different ships you could pick?

Spiess: What ship. And so I looked at the list there were a couple of submarines being built at the Mare Island Naval Shipyard, which essentially is on San Francisco Bay. And so I briefly considered the fact that that would be sort of home base, [because] by then I had a semi-commitment for the long term with a gal about four years younger than I who was getting out of high school when I got out of college. I had known [her] through our church and our families knew each other. And so there was some temptation to just head back [to] the San Francisco area. But I figured that if I was going to do this, I might as well really do it, so I put in for a submarine based out of Manila in the Asiatic Fleet and headed out there.

Henke: So right after sub school you headed out to Manila.

Spiess: After sub school. We went out in essentially October of '41 by commercial ship.

Henke: Just before the war officially started?

Spiess: Right. Well, I've learned to be very careful. I deal fairly often with Australians, and when you say the war started in 1941, why you get the real dirty look, because they'd been sending their people off to fight in Africa or wherever it was for quite some time. And so, some kind of phrase like "when the U.S. became involved," or "after Pearl Harbor" or some other kind of thing of that sort is appropriate.

Henke: Right.

Spiess: We went out on a commercial ship, the *President Harrison*. And just sort of to give some feel for the fact that things were really kind of tense out in the Pacific. . . Of course, those of us who had grown up on the west coast were well aware of what the Japanese had been doing in gradually encompassing a fair amount of what had been China, and there had been an incident of the sinking of a U.S. gunboat that was in the Yangtze River patrol and things of that kind. But when the *President Harrison* arrived in Hawaii there were six of us on board out of this submarine school class who were headed for the Asiatic Fleet. There was one very senior person who was going out to be the admiral-in-charge for the submarines in the Asiatic Fleet, and so we were taken off of the *President Harrison* and sat down for the better part of a day out in the submarine-based Pearl Harbor and then went back on board the *Harrison* and headed on out. It turned out that what was happening was that there was a squadron of submarines based in Pearl Harbor that were being transferred out to Manila. And so there was some thought that maybe they should jerk us off of the *President Harrison* and put us on the tender that was going out with them and we could work our way out. And the senior person, Jimmy Fife,[8] was successful—

Henke: This was the admiral guy?

[8]Captain James Fife, Jr., later Rear Admiral.

Spiess: Yes. I think he was probably a four-striped captain at that time. But he was able to convince that we should be able to continue on the *President Harrison*. But the *Harrison* did not go in the direction that we thought it was going to go when we left Hawaii. It was supposed to go to Shanghai, and instead it headed south. In fact I guess that meant that I did my first equator crossing on that trip with Jimmy Fife as King Neptune. But we went down and went around between New Guinea and Australia and around and up to Manila from the south. And so it was clear that things were pretty tense. The *Harrison*, after it disembarked us, in fact was sent up to bring Marines out of Shanghai and brought out one load and then went back and happened to be up there at the time that Pearl Harbor took place, and so it was taken over by the Japanese.

Henke: I see.

Spiess: So anyway, I joined the submarine [in] November of ‘41, a thing in which there were about five officers, and we wound up being only semi-surprised when things blew apart. We were in Manila Bay when [Pearl Harbor] happened, and I had just taken over as communication officer on the submarine at that point, so I had all the classified publications under my wing, just barely. We went off on our first patrol from there, and were among the Philippine Islands, trying to figure out how to play this game, because it was really quite different than the exercise sorts of things we had done. Because, for one thing, there was supposed to be unrestricted warfare against merchant ships as part of this—Japanese merchant ships, obviously.

Henke: I assume that none of the people on your boat had any combat experience.

Spiess: That’s true.

Henke: So people were all kind of starting out fresh at this point?

Spiess: Right. We had plenty of experience in making [mock] attacks on our own destroyers and their making attacks on us. And that had some negative aspects, because some of the commanding officers had the feeling that if somebody was pinging his sonar that they had you and you better do something drastic instead of continuing to be aggressive about some kind of an attack you were trying to carry out. Anyway, we did that first patrol and didn't really accomplish anything very much other than stay alive, I think. Between Christmas and New Year's, sometime in there, we wound up in a major typhoon—hurricane. And I have been on topside when the eye of the hurricane went by, and our captain figured that we had to have an officer on deck as long as we were in the bridge structure. And there were only three of us who were—there were five officers. Three watch standers. And so we stood one hour on, two hours off, kind of around the clock for a couple of days there, and I happened to be up in the conning tower fair water structure when the eye of the hurricane went past us. Because we weren't going anywhere. We just didn't have the propulsion capability to do anything much. So anyway, we wound up in Darwin [Australia]. After that one we did another patrol without inflicting any great damage, and wound up after that one in Fremantle, Australia, and then on up, and the next patrol [was] through the Dutch East Indies. And by then the Japanese had inflicted some casualties on us. Our submarine was Hull Number 175, and both 174 and 176 had been lost by the time we were a couple of months into the game.

Henke: From other submarines or from surface ships?

Spiess: From surface ships, right. And so we became the first one tagged to go back to Mare Island for a major overhaul/rebuilding/modernization. We worked our way back up through the Dutch East Indies and up to Hawaii and went out and did a short patrol during the Battle of Midway, kind of a scouting type effort, and then went back to Mare Island where we were for about three months, having things like radars put in and good new technology. And in that same time frame Sally[9] and I were married. She had been going to the University of Oregon during the year that I was away.

Henke: During 1942?

Spiess: And so we were married in the summer of '42. That's fifty-seven years we will have been married come this summer.

Henke: So can you give me kind of a nutshell of what some of your responsibilities were on the submarine as an officer?

Spiess: I started out as a communication officer and battle station sonar officer. And then after the Mare Island thing I became the battle station diving officer and the engineering officer on the boat and made four more patrols. Those were the most successful set of patrols I've been involved in. A new commanding officer was sent to us, and he was far more aggressive than the previous one had been, and he had better tools to work with, too. So we figured out how to be effective. And then after that eighth war patrol I was ordered back to a new submarine that was under construction in New London, Connecticut. Big shipyards there that were building submarines at a pretty great rate. Went out on that one as the fire control and gunnery officer and made five more patrols in that boat, part of the time as navigator. And then after those five patrols, by then that was getting to be early '45, and I was ordered back again to do construction on a boat in Philadelphia.

[9] Sarah Whitten Spiess

But just before my thirteenth war patrol, which some of my shipmates were not looking forward to, particularly, just in the superstitious world, I had an opportunity to put in for a Navy postgraduate course. The Navy was pretty good about sending people to school if they needed people who had extra education, and I put in for this figuring that, you know, it's been twelve war patrols, working my way up from being fifth officer to being fourth officer on the submarine, and I was still fairly junior. And so, although I was ordered to be the executive officer on a new submarine in Philadelphia, very shortly after that the orders came to go to postgraduate school in what was called ordnance engineering. It had a number of different options, one of which was a communication engineering option [and] meant a year of graduate work at Harvard in their communication graduate program.

Henke: How did you feel about being pulled off the patrols and being sent to graduate school?

Spiess: Well I figured by the time I'd done thirteen that I had—

Henke: You'd done your share?

Spiess: My gung-ho feelings were—. And it was clear that things were winding down and one should maybe be thinking about how you're going to make the transition back to something else. In fact the war in Europe ended while I was in a refresher course at Annapolis that they did for those of us who were going off to these various universities to do a year's worth of work. And so Sally and I moved to Cambridge, and I did a year of work there, which was really a kind of key to the whole thing, because during that time I had an option to say I want to stay in the Navy and become a regular Navy officer, or to go back and pick up where I had left off as far as graduate work in physics. And a year at Harvard was really pretty nice, because I could take a few high-grade graduate physics courses at the same time I was taking a reasonably decent set of [other] courses. So I could say yes, I am still capable of handling the academic aspects of this sort of thing. And I did well enough that I wrote back to Berkeley where I had been offered a teaching assistantship when I graduated, and the physics people at Berkeley said, "Yes. You can come back and you can have your teaching assistantship that we promised you." And so I went back to Berkeley to be a graduate student in physics. I had by then a master's degree from Harvard. I think Harvard is probably one of the easiest places to get a master's degree, at least the communication engineering option that we were involved in, because if you could make a B average in a certain number of graduate courses, why you'd get your master's degree. No oral exam, no anything.

Henke: So you decided to go into communication engineering based on—?

Spiess: I was really choosing the school more than anything else, because, of the various universities that were available, Harvard seemed to me to be the one where I could have the best exposure to what it was going to be like if I decided to go back to graduate school.

Henke: I see. So it was kind of what helped you decide?

Spiess: So it was really very nice. I could sit there as a lieutenant commander, drawing my nice pay and see whether I could cut it or not. And it turned out I could, so I went back to Berkeley then. Within a very short time after I returned to Berkeley, which was the summer of '46, suddenly the Physics [Department] that had maybe twenty or thirty graduate students when I left in 1941 had one hundred, or something like that. I mean the whole nuclear thing had suddenly come, and a lot of people decided they were going to do graduate work in physics. And so you'd take your first year graduate course, and you'd have more people in it than I had in an upper division [undergraduate] physics course before World War II.

So anyway, I dug in there, and remembering my mathematical bent, I thought, gee, maybe I could be a theoretical physicist. Physics—I think still is—was really a class society. There [were] theoreticians and experimentalists, and the theoreticians were all the bright people and the experimentalists were people who grubbed around in the laboratory and built cyclotrons and so forth. But you could make your Nobel Prize as an experimentalist, and in fact several Berkeley faculty members did, starting with Lawrence[10], who was the inventor of the cyclotron. And so after about the first year it became clear to me that—well, I took my first year quantum mechanics course from Oppenheimer[11]. There were some people who [were] really going to be theoreticians, and you could tell who they were. They sat up in the front of the room, and it became clear that I was going to be one of the experimentalists.

Henke: You sat in the back?

Spiess: Yes. And I had to sort of go around and see what group I was going to work in. I had some friends who were working for Emilio Segrè, who had grown up in the Fermi group at the University of Rome before World War II and had immigrated over in the late thirties. In fact I had taken an upper division course from him the first time he ever taught at Berkeley. And he had a research group that was doing good things. And so [when] I went over to talk to him I wasn't sure whether I could be an experimentalist, because I was not the kind of person who took my car apart or whatever. He said, "Well, when the toaster breaks, do you fix it?" and I said, "Yes." He said, "Okay."

Henke: You must have had opportunity to fix some things on your submarine career too,

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[10]Ernest Lawrence

[11]Robert Oppenheimer

right?

Spiess: Well, yes. But usually there were some other people around who really knew how to do the nitty-gritty nuts-and-bolts parts. But certainly that had helped a lot in terms of being in there. And so at that point I joined Segrè's group.

Henke: At that point were there any things from your time as a submariner during the war that had kind of stood out to you as problems you'd hope to work on [during] your graduate career, or was it not that clearly connected yet?

Spiess: No, no. The submarine business did indeed play a role though, because the Navy expanded its reserve training, and so I was involved in setting up and writing curricula for training submarine reserve people, and I ran a submarine reserve unit in parallel with being a graduate student. Between that and the GI Bill money and the teaching assistantship, or fellowship later, we had a reasonably comfortable life.

Henke: But the two parts of your—?

Spiess: But the two parts were kind of side by side, rather than integrated. ## Segrè had a string of problems and I built some gadgetry and did some work on the cyclotron, the smaller of the cyclotrons at that time. And [I] turned out a thesis that had some useful information in it about radioactive decay of some elements. And I've always said, once I was out of there, why Segrè's group was able to get the Nobel Prize, because in fact they did about two years later for discovering the positron. Or maybe it was the antiproton; I've forgotten which one now.[12] Anyway, when I finished the market was pretty good for Ph.D. physicists. And so I looked around and decided that there was one place that would be really interesting to go—that was General Electric's Knolls Atomic Power Laboratory. Because they were doing submarining engines, and I was looking for some kind of meshing of [my] submarine time with the future.

Henke: Plus you had also just done work on nuclear physics.

Spiess: Yes. I had a nuclear physics degree. So I had a good offer from Knolls Atomic Power Laboratory and went with them. I probably could have had a billet at the University of Oregon or Oregon State, someplace like that.

Henke: So Knolls was a private company but it was solely funded by the Navy?

[12] Segrè's group was awarded the Nobel prize in 1959 for their discovery of the antiproton.

Spiess: Well, it was a combination, but I'm never clear as to which money came from the Atomic Energy Commission and which came from the Navy. But it was an arm, a research arm, of General Electric Corporation. And there were two nuclear power places: Westinghouse had one, and General Electric had the other. And I dug in there, looked as if that might be a good thing to do.

But one of Sally's sisters was being married the summer after we went east, so we came back out to the San Francisco area in the summer of '52. While I was out here, T. J. Thompson—a fellow I had been very close to as a graduate student, we had helped each other—said his friend from Berkeley, Hugh Bradner, had told him that there was an opening down at Scripps,[13] that they needed some physicists. Tommy had been invited to come down and learn about that. So he turned around and immediately said well, Fred Spiess is in town—he said that they ought to invite me down too, so the two of us came down here. The Marine Physical Laboratory, which then was directed by Carl Eckart, who had been the first director of the lab, needed a physicist to come in because Leonard Liebermann, who was running one of the major projects in the lab, had an opportunity to go off for a year on a Fulbright to Yale, and they needed somebody to take that over. They knew that the whole game was expanding anyway, so they were using this as an opportunity to bring in a new person. And so they made us both offers almost right on the spot. University hiring was not as complicated as it is [now]. And so I said, "Well, I'd like to think about this," and we went back to Schenectady and thought about it for a couple weeks. [I] decided that it was pretty clear that working in an underwater acoustic sonar type environment, which was what was involved out here, and having a situation where I could be in charge of a project all by myself instead of being one of a couple of thousand people building an engine, why that was really a major thing.

Henke: That appealed to you?

Spiess: I had to cope with the fact that moving out of mainstream physics was something that, if you were a Berkeley person in physics, you had to think about. And my feeling was that Carl Eckart was the one who was inviting me to come. Eckart had a reputation in real mainstream physics. He was one of the first two people, while he was a young person barely out of graduate school, [who] showed the equivalence of the Schrödinger and Heisenberg formulations of quantum mechanics. He had a very big reputation, and had been at the University of Chicago before he came out here, and he had decided he was going to stay here. And so I figured if he could do that, I could do it.

Henke: There would be no shame in it?

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[13] Scripps Institution of Oceanography

Spiess: Right. So anyway, we came out here—by then we had three kids—and joined the Marine Physical Laboratory in the summer of '52.

Henke: I see. Let me just ask you a couple questions that maybe go back a little bit about what we've talked about.

Spiess: Sure.

Henke: It seems to me that many oceanographers—I guess we'll start calling you an oceanographer now that we've got you up to MPL—it seemed many of your generation who started oceanography in that period, the fifties, sixties, or so, a lot of them had experience in the Navy. Would you say that your experience in the Navy was pretty typical of oceanographers of your generation?

Spiess: I think my experience was deeper than most of the rest of them. The Navy trained a lot of meteorology people during World War II, naval officers to be meteorologists. And so quite a few of them, I would say if you wanted to pick a kind of average thing, that you would find that there was somebody who had been in the meteorology environment. So mine was more operational and I think for the people who came in in that era, I had more hands-on operational experience than most, just because I had been on a ship where there weren't very many officers and where you had a lot of independent operations and we had to think our way out of a lot of nasty spots as we went along. And so in that sense I think I was somewhat different from the average.

Henke: So many others might have had involvement with the Navy, but not necessarily combat experience like you did?

Spiess: Well, they had combat experiences in many instances, because if you're a meteorologist on a carrier in World War II you're going to be out there taking your share of the [action], but I think it was just that I had been in a much smaller environment where I personally had to work on things in a more independent way than the others. But I don't think that made any difference. That was an era, of course, in which there were sort of two things that happened in the fifties: one of them was the early fifties, that oceanography was growing and there were a lot of people who came in who had bachelor's degrees in engineering or chemistry or whatever it might be, and or in fact by then who had doctor's degrees in those fields and were recruited to come into oceanography, marine science, ocean science, whatever. And then there were the others of which Scripps cranked out a fair batch, who came here with bachelor's degrees and did their graduate work here in oceanography. But the number of people with bachelor's degrees in oceanography—with doctor's degrees in oceanography in 1950 or '52 or '53 even was pretty small. It was not a common kind of thing and—

Henke: There weren't many real oceanographers here when you came; they were mostly people that were coming from other areas?

Spiess: They were mostly people who had come from other disciplines. But there was a sort of core of them who had been educated right here. And in fact those people went off to other places and established oceanography departments—the Oregon State Oceanography Department, Texas A&M, Johns Hopkins, University of Washington, a little later University of Rhode Island. Let's see, I've probably left out one or two, but those were all started by people who were educated in the Sverdrup[14] time here in the late forties, early fifties, for their doctorates in oceanography, and they went out and established oceanography departments. That's why we have lots of oceanography Ph.D.'s now relative to what we had in that early time.

Henke: Right. So would you say, in your own specific case and maybe more generally, that say the experience of World War II, whether you were actively involved in combat situations or not, would you say that that had a pretty big influence on the direction that people took in their research careers? Or just their attitude—

Spiess: Now you're groping for a generalization here. Yes, I think that people who might not have thought of being ocean scientists or engineers, having gone to sea for a while knew that it was an environment that was challenging and could be interesting, and so I think they were strongly influenced by the fact that they had spent some time out in the ocean; that their decisions I think to go and be Naval Officers, Reserve Officers in the time of war may have been motivated by other kinds of considerations, but once they were involved and had some skills in terms of understanding ship handling and operations, things of this kind, that it couldn't help but be a factor in making their decisions. I would think in my case it probably gave me somewhat more of an engineering look at things than I might have had if I had grown up without going off and doing the submarine part before a doctor's degree in physics. But more interested in how to build things and how to make them work.

Henke: I just want to go back to the point you mentioned a minute ago when you talked about how at Knolls you were one of maybe a thousand people working on a research project and when you came here you had the opportunity to just have a small group that you were supervising, right? You had your own lab that you started at MPL, I assume, when you first came here?

[14]Harald Ulrik Sverdrup, SIO director 1936-1948.

Spiess: Well, I took over the project that Leonard Liebermann had been running, and that gave me a group of people, a couple of graduate students and a couple of good engineers, all of whom had in fact been working at sea with electronics and hydrophones and a variety of other things for a couple of years before I turned up. So the education that I needed in how to do specific useful things at sea came pretty quickly, because I had a very good group with which to work and I had the opportunity of then thinking of new things that might be useful, largely in the Navy context. In those days right after World War II, the Marine Physical Laboratory was quite tightly tied to the Navy, and some of us knew better than the project officers in Washington what things would be really useful.

Henke: Because of your Navy careers you mean?

Spiess: Because in the mid-fifties the Navy submarine fleet had as the admiral-in-charge a person who was quite research-oriented himself. This was Momsen[15], who had been the inventor of the Momsen Lung, which was an escape device used from submarines, and he had spent a fair amount of his time in what the Navy called its Experimental Diving Unit, learning how to improve diving capabilities. He had a real bent toward this and he made it quite easy for people such as the Marine Physical Laboratory people to have access to San Diego submarines for carrying out experiments. He had a whole research program and he would assign division commanders to look after your experiment, or whatever, and so really it was really a very fruitful kind of thing. That was the early 1950s.

Henke: So that appealed to you, that that kind of thing was going on here?

Spiess: Oh, well, it was great. Here I was able to go and throw my weight around a little bit as a real submarine person and at the same time be able to do some things that were innovative and useful to the Navy. And those things in the long run led to other things that were outside the Navy funding scope. But in the original times it was really a very good match for what I knew and what I was interested in.

Henke: Obviously during your time during World War II when you served on the submarines I'm sure you felt kind of a sense of duty to your country and to the war effort. Would you say that kind of influenced your decision to come here and work with MPL because of all those appealing things that were happening with connections with the Navy and stuff like that? Did that kind of patriotism follow on?

Spiess: I would guess. You know, you're putting some words in my mouth, but—

Henke: Just curious.

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[15]Vice Admiral Charles Momsen

Spiess: Nevertheless they're not too far from the case. I think I was not a—. I guess part of it was in parallel with duty to your country and that sort of thing. There is the adventure that a young man can have going off and doing these kinds of things, and I've had a tendency, I guess, to fairly easily develop loyalty to some kind of a thing. I had some loyalty to the Navy per se. I had, by the time I'd really become heavily involved in the submarine business, I had some loyalty to submarining, as far as that was concerned. I point back to Mr. Johann Spiess, who was in the German submarine world. In fact that was a help even at submarine school, because I know that within a week or two after I was there we had a new commanding officer who had a German background whose name was Hensel. And he was quick to point me to where in the library the war patrol reports were that had been written by Johann Spiess so I could read about that.

Henke: I wanted to ask if you had any last comments on things about MPL and the situation here that really appealed to you and made you—because you really jumped at the chance.

Spiess: The reason that I came?

Henke: Right.

Spiess: Well, I think it was partly being recruited by Carl Eckart. He was a name that I knew from student days and knew his reputation, and it was also the fact that here was a project that was clearly a submarine type project but which clearly was a good sort of classical physics type thing. I had never had a course in acoustics, but the Berkeley education was such that you were driven through electromagnetic theory courses well enough that the equations are all the same and there is a lot of correspondence. But I think I was really more tied in fact to being somebody who used more or less classical physics, albeit I did a Ph.D. in perfectly respectable nuclear physics environment. I think if I had really been wedded to that environment, I would have taken a different job [from] among the ones that were offered when I finished my Ph.D. I think I reverted back to the submarine thing and let that be a big driver, because there was going to be a lot more engineering at the Knolls Atomic Power Laboratory than there was going to be fundamental nuclear physics. And so I think that the submarine time sort of made me more of an engineer than a fundamental physics type person. I think that the advantage that I've had perhaps is that I can kind of do things that are engineering but are done in the context of a real research opportunity.

If I had to pick somebody that I think really did that, it would be somebody like Lawrence. They needed accelerators, and he had a real bright idea about how to build a better one than anybody else could. And so I think my philosophy here has been that if you are going to make progress in experimental science, you'd better be able to think up something you can do that nobody else can do and then be able to do it; not just think about it. And so that's been kind of a guiding principle and to some extent a guiding principle of the Marine Physical Laboratory.

Henke: Well that sounds like a good place to stop, and we'll actually try to pick up on that point then when we start again next time.##

INTERVIEW TWO: 2 FEBRUARY 1999

Henke: ##Perhaps you could give us some background of why it is that [SIO and MPL] wanted to bring you in.

Spiess: Yes. Well, at that particular time the Marine Physical Laboratory, which had grown out of the University of California Division of War Research, was moved into being part of Scripps Institution of Oceanography about 1949 or '50 when Carl Eckart, who was the founding director of the Marine Physical Laboratory, also became director of Scripps Institution of Oceanography. It was not a terribly big group, but it had some very interesting things going. In mid-1952 Carl Eckart was about to go off on a sabbatical leave to spend a year at the Institute for Advanced Study in Princeton and one of the lead group leaders in the laboratory, Leonard Liebermann, had received a Fulbright Fellowship to go and study at Yale for a year. So they really needed some people to come in to take over the major project that Liebermann had been running on submarine sonar systems and to take over the directorship of the lab. So two of us arrived just about at the same time. I arrived as the Liebermann replacement, although with the implication that this was going to be a permanent thing; it wasn't just coming for a year. The other person who arrived was Sir Charles Wright, who came to be the acting director of MPL in place of Eckart.

Sir Charles was an interesting guy. He had been with Scott[16] down in the Antarctic [around] 1910. In fact he was the leader of the party that went out and found Scott's body and his group down there and wintered over in Antarctica at that time. He was basically a geophysicist. By World War II he was the director of the Admiralty Research Laboratory in England and after World War II was in the British Joint Services Mission in Washington, D.C. He was Canadian and had finally retired from the British service and went to live in the vicinity of Victoria in the western part of Canada. And being a known quantity in the Navy research world when they needed a director to replace Eckart for a year, why they selected Sir Charles. And so the two of us came in together. It sort of set a slightly different tone, because Eckart was a theoretical physicist who was involved in research activities; Sir Charles was much more of a pragmatic laboratory director type person, very nice person to get along with. And that sort of helped push me in the direction that had to do with administration as well as with actually carrying out research activities.

[16]Robert F. Scott

Henke: How was it that he did that?

Spiess: In that era there were a lot of advisory committees that existed, and the first couple of trips that I made to Washington, D.C. were done sort of with him as my mentor, so to speak, and he showed me around where the interesting places were in terms of Navy administration research activities. And in those days the Marine Physical Laboratory was operated with funding from what was then called the Navy Bureau of Ships. They had a sonar design branch, and that funded what was then the Navy Electronics Laboratory on Point Loma,[17] a much bigger group than the Marine Physical Lab. And so funding for salaries came to the Marine Physical Laboratory [from the] University of California, and then the Bureau of Ships, which also sponsored the Navy Electronics Laboratory, put money into the Navy Electronics Laboratory and said, "Here, you take care of the needs of the Marine Physical Laboratory." So it was kind of a complicated, two-part thing in which if you needed an oscilloscope you got it from NEL but when you're recruiting people they were recruited into the University of California. But we were all down on Point Loma. The Marine Physical Laboratory was completely down at Point Loma at that time, although parts of the laboratory gradually moved out to Scripps starting about 1952 or '53.

Henke: And regardless of the different parts it came from, it was pretty much all funded by the Navy at that point?

Spiess: It was completely funded by the Navy, yes. Sir Charles came along, stayed for a year, and then when Eckart came back from sabbatical he decided he didn't want to be the director of MPL anymore.

Henke: Eckart didn't want to be.

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[17]Point Loma is a peninsula on the San Diego coastline that serves as the home for many Navy research facilities. The University of California Division of War Research (UCDWR) was founded there, part of which later became the Marine Physical Laboratory, which is still located at Point Loma.

Spiess: Eckart didn’t want to be. And so then Sir Charles stayed on for another year. The next move, of course, was to find a permanent director for the Marine Physical Laboratory, and so the decision was made to bring in Al Focke, who ran a group in the Navy Electronics Laboratory on atmospheric acoustics, primarily very low frequency disturbances in the atmosphere, but [also] a whole variety of other things. He was a broad gauge physicist. And Al came in as director [in] ‘54 or ‘55. At the time that he came in, he had another job that he did not relinquish, which was that he was the chief scientist for a thing called Operation Wigwam, which was the Navy nuclear depth charge development activity headed at that time toward carrying out a major at-sea test explosion of a nuclear depth charge. Or at least a nuclear device moored at depths that would be similar to a depth charge, and there were a whole variety of other things. There were three one-eighth scale model sections of submarines that were to be submerged at different distances from where the bunch of barges would be from which the nuclear device would be hung. And there were obviously a lot of other groups out there to do monitoring. The site at which this was to be done had been chosen pretty carefully, based on some focused Scripps activity to look at what the amount of marine life was and try to find a place that was sort of like a desert in the ocean where you wouldn't be contaminating very much of the marine life that was involved.

Anyway, the Wigwam thing is a whole big story by itself, and I was not involved in Wigwam. The fact that Al Focke was involved in Wigwam however meant that he had a lot of things to do other than directing the Marine Physical Laboratory. Well, at that time my office at MPL was right alongside the office where the front office secretaries were, and the combination of hearing them talk about how things were going and the fact that Al Focke was away a lot of the time and [he] sort of delegated things to me, [so] that I kind of drifted over [to administration] as well as running my research group. The end result was that around 1957 Al Focke had an offer to go off and be the chief scientist at the Navy laboratory that they were just establishing at Point Mugu to do missile shooting and things like that. And so when he left, I sort of was the obvious person to become director of the lab.

I was also involved by then in some Navy advisory committees: by ‘58, I guess, I was a member of a thing the National Academy had established right after World War II that they called the Committee in Undersea Warfare. This was really sort of a continuation of the involvement of senior research people in research planning in relation to the Navy, although it was an organization operated through the National Research Council. And I had come up with some ideas that were presented. We had the opportunity to present those to the Committee in Undersea Warfare in ‘54 or ‘55 or somewhere along in there, and shortly after [that] they asked me to be a member of that committee. I was one of the junior people in that, because several of them were people who had been in charge of major R&D operations for the Navy during the war and had then gone back to their university roots and were planning major roles there.

Henke: So once you got onto some of these committees and stuff—

Spiess: And so that also provided a perspective that was useful for the lab, sort of keeping our priorities and generating new ideas in relation to the Navy. Submarine warfare was primarily what we were involved in, to some extent anti-submarine warfare, but working with the submarines here in San Diego among other things.

Henke: That was the main focus of MPL at the time?

Spiess: And that was the main focus of the lab, although we had begun to spread out from that. There was Russell Raitt's group that Russ had started here during the UCDWR times, and he was a geophysicist who had been involved in sound propagation experiments, some of which were done by going out to sea and putting explosives in the water and then listening someplace else to see how the sound traveled. And out of that, he built a research group that was one of three of this kind in the U.S. in that era that was studying the crust of the earth by going out and doing experiments in which you set off a blast someplace, or a series of detonations with more or less conventional explosives. And from another ship at some distance away you could listen to the returns, and the structure of those returns would contain not just the sound that came through the water but the sound that came through the upper part of the crust. And so you could do what came to be called seismic refraction experiments, and Russ was one of the leaders in that, produced a lot of new concepts about the nature of the Pacific Ocean floor. And he was one of the group leaders in the Marine Physical Laboratory so that it wasn't strictly Navy-oriented hardware things. The understanding of how sound traveled through the crust and the water and so forth was still of interest to the Navy, so the Navy actually continued to fund that sort of thing.

Henke: So the Navy was amenable to that kind of research at that time.

Spiess: That's right. I guess starting in '53 there was some interest also in mapping the magnetic field of the earth in detail to try to learn about things about the structure of the earth from looking at the small variations between the measured field and what you would [expect] if you just had a great big dipole magnet. And there was a group that started to work on that in the Marine Physical Laboratory while Sir Charles was still here, so that would have been '53 or '54. And [they] made a very lovely magnetic anomaly map of the region out to about 500 miles off the west coast of the U.S. This was done taking advantage of the fact that the Navy had wanted to have detailed topographic information in that region because they were planning on installing what now are well known as sea floor hydrophone arrays to listen for submarines. And so Sir Charles and others made arrangements so that we could tow a magnetometer behind the ship as it made the topographic surveys and they had set up a good radio navigation system so it was a well navigated thing [with] close line spacing. And there was a beautiful map that came out of that done by one of the MPL engineers, Art Raff, and a geophysicist from England, Ron Mason, who had been recruited by Revelle[18] to come and be part of the organization here. Ron Mason didn't stay around for terribly long, and it was maybe as late as '58 or '59 [when] Victor Vacquier came to the lab. He was a geophysicist who had worked with magnetic prospecting kinds of things and also with the development of magnetometers during World War II for submarine detection. And so he took over that group, and that was one of the other organizations within the Marine Physical Laboratory.

Henke: So was it fairly easy to go off in these other directions as long as you were able to satisfy the Navy and their interest in—?

Spiess: We had a very close relationship with the Navy. It was quite different from the way things are now. The administrators in the Navy knew the history of the UCDWR just as well as anybody else, and there were a lot of people around who really knew about the role that university research could play in relation to the needs for national defense. So the way things worked was that, through a variety of these committee activities, the sponsoring project officers in Washington D.C. knew what we were doing and I know that when I took over the lab directorship in 1958, I could write a two page proposal and fund the laboratory for a year.

Henke: Really.

Spiess: We just said we were studying the physics of the ocean and how this related to the atmosphere and the earth below, and that this was important to the Navy. And we did everything from sonar signal processing—cutting edge kinds of things—over to the fairly fundamental marine geophysics in seismic refraction and geomagnetic studies. In part these were justified by the Navy in terms of their funding. For example, these were the days when the ballistic missile submarine thing was just barely coming into existence, and I was trying to remember what year the Nobska Project was.

Henke: Which project?

[18]Roger Randall Dougan Revelle, SIO director 1951-1964.

Spiess: The Committee on Undersea Warfare ran occasional summer studies, and the first of those in which I was involved was called Nobska, after a point with a lighthouse on it near Woods Hole in Falmouth on Cape Cod. The study was held and they rented one of the big estates right near Woods Hole as the operating place. I should be able to pick the year—it must have been ‘56, because Sally wasn’t able to be with me there for more than about a week because we were expecting our fifth child right at that same time. And so this was before I became director of the lab, but I wound up being one of the group leaders in that study. It was a fairly high-powered thing in which some of the real base for the entire missile launch strategic deterrent activity took shape. I wouldn’t say that I played a great role in its shaping, but nevertheless it was interesting to be part of that. This related back to the questions of the geomagnetic field and that sort of thing. In fact, I guess the first time I met Vacquier was when he was a participant in the Nobska study. The Navy felt that if they knew the details of the magnetic field that there were enough fine scale aspects to it, that, if you had good maps of the magnetic field, you could use those to determine where you were by map matching techniques. If you were submerged you could tow a magnetometer and be able to tell where your submarine actually was, so that was part of the justification.

The other part of the operational justification for the magnetics was the magnetic detection of submarines. [This] was a powerful tool during World War II and continues to be to this day in the sense that it’s not a great detection tool but it’s a great confirmation tool. If you have some kind of acoustic thing that’s going on down there and you can fly by with your magnetometer and see that it’s a big lump of iron, why then that’s a big help. So anyway, the Navy had a lot of good reasons for looking at or sponsoring basic research, and the Marine Physical Laboratory was kind of in an intermediate sort of situation in which we did a certain amount of quite basic sorts of things and other things that were pretty heavily applied.

Administratively, structurally how things were put together for the Marine Physical Laboratory changed in 1958, just as I took over the directorship of the lab. The Navy decided that the Office of Naval Research [ONR], which had been a very young thing at the time that MPL was established, had become a strong enough entity and that the Marine Physical Laboratory should shift over from this rather tricky support from the Bureau of Ships to being an Office of Naval Research operated contract laboratory. And so we went through [a] fairly complex time for a year or so, sorting out what equipment should be transferred from NEL to us and a whole variety of things of that sort, although we managed to maintain our research activities along the way pretty well.

Henke: So that was something, one of your first jobs as the administrator for MPL?

Spiess: Yes, right. We had to establish liaison with a different group of project managers, and although at that time the Office of Naval Research had an undersea warfare section, it was pretty much staffed by Naval officers. So they had a feeling about what the needs were and if you came up with a bright idea, why then you could kind of say yes, we ought to really push that. Some things were opportunistic in the sense in that you'd think of some—we worked for some little time on acoustic communication, for example, between submarines, figuring out ways of doing that. The initial project that I took over from Liebermann was [on] signals that were emitted by submarines—particularly when they were snorkeling. You remember that this is before the real nuclear days. Nuclear submarines were just one or two, and most of the operational submarines were able to stay submerged for prolonged periods by running their diesel engines using air drawn in through a thing called a snorkel pipe that went up and could be raised and lowered. Germans had started building these during the later part of World War II, and every navy that was around immediately after World War II started doing this. And of course, in the early fifties, Russian submarines were the ones we were really thinking about. Anyway it turned out that—in fact it's noted by Eckart in one of the final reports of the UCDWR—that there were low frequency signals, that is, things that were in the 100 cycles per second range more or less that traveled a long way in the ocean and were generated by diesel engines running submerged and well coupled into the water acoustically. And so that was really the beginning of this system that has been pretty well described in unclassified literature since, of hydrophone arrays on the sea floor to listen for submarines. What had happened at MPL was that a couple of people said well, if you can do this from an array on the bottom, why can't you do it from a submarine itself, so it could listen for other submarines. And so that was what we were working on at that time, putting hydrophones on submarines. I think that was a good way for me to start into this, because I had the submarine background and the physics background and this was a way of kind of pulling things together for me in the research environment rather than in the operational environment.

Henke: Let me ask something about that. Aside from the actual funding of your lab at MPL, how much other contact did you have with the Navy, say on ships and stuff like that? I know on one point you went on active duty actually when you were at the MPL again, right?

Spiess: Right.

Henke: So were you pretty often going actually onto submarines to do work, and was that pretty indicative of—

Spiess: That program involved working with submarines. The [Navy] submarine force in the early fifties had a Pacific fleet commander of submarines named Momsen. And he was very heavily into the idea that there should be a lot of research activity, and that there were a lot of new things in which the submarines could help out. So they provided the opportunity for us to work with submarines. The project in which I was working, one that I took over from Liebermann, we wound up bringing that to a condition where it was really not quite operational. It was still an experimental system, but it was quite useful. And so it was decided by the Navy that they would like to use this capability to go off in the forward areas and listen for whatever kinds of things might opportunistically come up. And at that point somebody from MPL had to go with the operation, and since I was at that point a commander in the Naval Reserve, why I simply [went] to active duty for three months and went out on the *Blackfin* to do that cruise into the Western Pacific.

So I had stayed active in the Naval Reserve. I had command of a submarine reserve unit during graduate student days, and when I came here I was initially involved with the submarine reserve. And then the Office of Naval Research had a research reserve unit here. And after about the first year or so that I was here in San Diego I moved over into the research reserve unit. There were a number of Scripps people involved in that. It started in the Navy during World War II. So there was some advantage in having your foot in both camps, so to speak, and that was an interesting kind of thing.

I guess one of the interesting things about the way funding was handled in those early times was that there was an advisory committee, one of the first ones that I was asked to join, to the Navy that was called the Deep Water Propagation Group or Committee, and it was concerned with understanding how sound traveled through the ocean in the long range sense. And the committee had on it myself and Brackett Hersey from Woods Hole[19] and Joe Worzel from Lamont[20] and somebody from Bell Labs, because Bell Telephone Laboratory was the laboratory that was actually putting the hydrophone arrays out on the sea floor and doing the long range propagation studies related to that activity. And we would gather for a meeting, and they were really very good meetings, because they fulfilled two different kinds of things. There weren't very many of us and so each of us could say, "Okay, what's going at Woods Hole?" or at Lamont or at Scripps or at Bell Labs that has to do with the sound propagation world. And the other part of the meeting was that usually the meeting was held because—well, there was one very astute civilian project manager type in ONR who was very good at understanding where the Navy ought to be putting its efforts in long range underwater acoustics, as differentiated from things like destroyer sonars that are going to sit and ping away or whatever. And he drew part of this understanding from this small committee, but whenever he had some funding for something or other, why he would gather the committee together and we would say, "Yes, that sounds like an experiment that we would like to do." And so we would sit around the table and we didn't write competitive proposals: we looked at what the amount in the budget was and said, "Okay, we can do this part and you can do that part." And then you'd go home and write a one-line proposal to satisfy the administrative requirements, one

[19]Wood Hole Oceanographic Institution, Woods Hole, Massachusetts.

[20]Lamont-Doherty Geological Observatory, Columbia University, New York.

line of content. Obviously there were budgets involved, too. [We sent] it in, and then the money came back. I mean, it was really quite, quite direct.

Henke: Pretty easy, huh?

Spiess: There were so few people in laboratories that had the capability to work in this field that one could really satisfy the need for giving everybody a shot at what it was, and you could do that in an environment in which you could sit and discuss the topic in an in-depth manner that had to do with what your own laboratory capabilities were and what aspects you would be able to undertake in some kind of cooperative venture. The committee thing had moved on up into a higher level around the early sixties. The Navy established a thing that they called the Undersea Warfare Research and Development Council, and that was another interesting group in which to be, because there the primary representation was from the big Navy laboratories: Navy Electronics Laboratory and Naval Ordnance Laboratory. They were represented by both—they had Navy people as commanding officers, but they all had a civilian technical director. And so this council was made up of a couple of people from the Office of the Chief of Naval Operations plus two people from each one of these labs, the commanding officer and the technical director, and then there were five laboratories, [each] somewhat similar to the Marine Physical Laboratory here: the University of Washington, the Woods Hole group, and Penn State had a laboratory that was called the Ordnance Research Laboratory. In the late sixties they changed it to the Applied Research Laboratory. And then there was the Mine Defense Laboratory, which also became an Applied Research Laboratory at The University of Texas. And those [labs] were represented just by one person, and I was the Scripps representative, since nearly all of the Navy stuff at Scripps was being done in the Marine Physical Laboratory. And that group did a variety of things: reviewed the Navy's undersea warfare budget every year, commented on that, and had a lot of good interactions.

It was really out of that there was funding to build FLIP in the first place, for example, because the Navy was starting around 1960 to develop a thing called the SUBROC. This was a device that could be squirted out of a torpedo tube and would zip up to the surface and then would launch a rocket that could go anywhere up to maybe thirty miles in some predetermined direction, and hopefully sink a submarine that was over there at the other end of the trajectory. The question of how you would know where the submarine was obviously was something that had to do with acoustics. If the submarine was making a noise, then you listened and carefully measured the direction of arrival of that noise. How would that direction that you measured at your own submarine relate to where the target really was? And so there were questions about how much refraction there would be and what would happen when sound bounced on the sea floor, the sea floor not being a horizontal plane but being a tilted sort of place where. In any event this—

Henke: So it sounds like a lot of the questions that you were working on during this time basically had to do with practical but also kind of more theoretical problems about how do you find out where something is underwater.

Spiess: Right.

Henke: That it's making some noise, but maybe not a lot of noise. How do you figure out where it is, where it's going.

Spiess: Right, yes. In this program, the Naval Ordnance Laboratory, which was one of the big organizations on the east coast, had a plan as to how they were going to spend the money for [SUBROC], and pressure was put on them to be sure that they looked at the acoustic problems carefully. In fact they didn't do this [research] within their own big organization [nor were] the commercial people who were carrying out a lot of the hardware things. And so this is how the impetus grew that they should put some of their development money into acoustics research, and an advisory committee was established just for that one project, and I wound up on that committee. The end result was that we began to understand what kinds of experiments you needed and what kind of facilities you needed to do those experiments and that in fact led to the funding of FLIP as the platform to do sonar bearing accuracy experiments. In fact [it led] to the beginning of our Deep Tow system, too, because we needed to know something about the statistics of the slopes on the sea floor. A big part of the sound was going to go bouncing around. So those things all tied together quite closely.

Taking this outside of my own personal research activities that were integrated into the Marine Physical Laboratory, we had a very powerful group led by Dr. Victor Anderson, who was a graduate student in the group that I took over from Liebermann when I first came. Shortly thereafter [he] finished his Ph.D. and we established another group within the Marine Physical Laboratory to support the signal processing kinds of things that he was very clever in instituting. Essentially, the beginnings of digital processing in sonar systems came from that. We were thinking in terms of a set of hydrophones on a submarine, and these individual listening elements all kind of hear from all directions all around them because they're pretty small. But if they are distributed over a fairly large area you can then combine the outputs of these so that their attention will be focused in a particular direction. And in fact that had been done in some clever ways in the German Navy during World War II. What Anderson came up with were ways, if you had a kind of simple digital representation of the signals, that it became possible to have a multiplicity of outputs from what we would call a beam former. You could have a multiplicity of outputs so that you could look simultaneously in a whole variety of different directions. And so you'd have essentially an output for every different direction around the azimuth, or even taking into account both the horizontal and the vertical direction of arrival of the signals. And that became a very important development that quickly found its way into some of the Navy sonar systems, and we built some prototype. The laboratory in this sense, as part of Anderson's activities, built some prototype beam formers that we put on destroyers and tested.

There was a submarine, *Albacore*, that was built as a test bed for a whole variety of things built by the Navy, initially to look at what was involved in high speed underwater operations, because clearly with the advent of the nuclear power thing you have the horsepower to push things along pretty fast. And so at an early stage before there was much experience with the nuclear submarines, they built this submarine that was battery powered but which was built so that it could go at high speed. It was also used for other things, and Anderson and his engineers built a very fancy submarine sonar system to go in front of that.

Henke: What time period was this in?

Spiess: That would have been ‘58, maybe, or so.

Henke: Late fifties?

Spiess: Well, it was after I was director of the lab, and after Vic got his Ph.D., so it had to be. . . . By the time we were into something like the *Albacore* it was probably 1960 or so and—

Henke: So let me ask you a question about your involvement on these committees that you’ve been describing. It sounds as though there was something that was appealing to you about being on the committees and then going into your job as director of MPL. Could you describe what it was that appealed to you about that aspect of your career?

Spiess: Well, it was fun to be able to know what was going on on a larger basis than just what your own experiment might be. There are people who don’t play in that direction and who get their inspiration from the context within which they are working. I think the real [question] is, why are you doing anything at all, and there are a variety of reasons almost invariably, but part of it is where does all of this sit in some kind of a world. ##

It was a way of keeping in touch with what was going on in all of the other laboratories that were involved in the kind of work that we were, and at the same time to keep close contact with what the operating Navy people were doing. And so it was just a way of being part of the community, is really what it amounts to. It’s also a way of, in the committee world, of course. You generally have two reasons for being on a committee, I think. One of them is that you have a capability to influence how things are going to play out in some sphere; the other is what I would call a defensive reason to go, to go to the committee in order that you can see whether something is likely to come up that you think is not a good result. And so you say, “Yes, I’ll be on that committee,” but you know that it may not be as much fun as some other committee. If it’s a committee that’s trying to think about what things ought to be done—usually you evaluate a committee meeting by how many good ideas did you have while in the context of the session, and did this stimulate

you to think of something new or to consider some new way of doing things. So it's a sort of opportunity for creativity that isn't there in every committee, because there are a lot of committees that are there to do jobs that are more assembling things out of existing building blocks maybe, and there the challenge often is, are there some new ways of putting things together.

Henke: During your time as director of MPL, what would you say the relationship was between MPL and SIO? Was it changing or—?

Spiess: Well, before I was director, back when Eckart was director, MPL became a division of Scripps Institution of Oceanography.[21] And it was a little bit outside of the main part of the institution because, for one thing, the headquarters were and still are down in Point Loma. But gradually, group by group, [they] moved from Point Loma [to La Jolla] as space became available. And their research activities sort of moved along in a direction that made more interaction with the rest of the SIO community an essential element of their activity.

The fact that MPL was not quite as heavily involved in the day-to-day activities of Scripps—I shouldn't say it wasn't, because I think in Roger Revelle's era he [was] not renowned as a great administrator, but he was really very good in making an organization run. And that may sound contradictory—

Henke: Yes, it does a little bit.

Spiess: But he would be willing to look around the institution and say, "There is a group that can look after this kind of thing." And he was fairly well-placed in the science community, and so some of the time I was on a committee because he was asked to be on the committee and he knew it was a Navy-oriented thing, and so MPL is the group that he directed to be involved. He did this in a whole bunch of different areas, in the planning of international expeditions, the operations of the Marine Life Research Group. And the reason it was successful was that if you went off to a meeting, you were really there as the Scripps representative, and if I made some kind of commitment that said, "Yes, we'll do this," and I went home, he would invariably back me up. And this was true for the other people as well. There are some people that you aren't always sure, if you make a commitment that's a little bit outside the envelope, whether when you get home that's going to be a comfortable situation or not. But he was very good at letting us use our imagination and go ahead and say sure, we'll do whatever it seemed that we could fruitfully do and that would be fruitful for the organization itself.

Henke: It seems like during—

[21]The Marine Physical Laboratory became a division of SIO in 1948. Fred Spiess was its director from 1958 to 1980.

Spiess: So that the relationship was really fairly close. He did not have weekly staff meetings or something of that sort. That came along at a later time. But somehow the combination of being involved with some major element of what was going on at Scripps and at the same time being a little bit remote from the competitive activities within the institution when Revelle left—when Herb York was appointed chancellor [of UCSD] and Revelle left [SIO] to go to Washington—why I was the one who was asked to be the acting director.[22] I think that was partly because I was a little remote from whatever the pushing and pulling was that was going on among all the other divisions out here on the campus. I think to some extent that's—

Henke: So you were seen as a little more objective than some?

Spiess: Well, Carl Eckart had done the same thing. Once Sverdrup left, why Eckart became the director of Scripps. And so I did a year as acting director while Revelle was in Washington.

Henke: Was it pretty clear that he was going to come back after going away?

Spiess: It was not clear at all. He did come back, and took over again as director of Scripps, and simultaneously he was given a university-wide position as some assistant vice president for research or something of that sort. I've forgotten the exact topic. But by then I had become involved enough in how Scripps operated that when he came back there was an Oceanography Department in Scripps within UCSD. Because Scripps was by then, for all practical purposes, a graduate school within UCSD. It was still, in that era, a pretty big chunk of UCSD. And when Revelle came back, I became the chair of the [Oceanography] Department. That's not the same department that exists now. This was an oceanography department. It just had the one, well, let's see, I think that we did [have] both the Oceanography and Marine Biology degrees, but—

Henke: There's a chart[23] here that shows the Department of Oceanography. This is a chart from 1964, so this would have been right after Revelle came back. And you're the chairman of [Oceanography]. And then there is also a Department of Marine Biology.

Spiess: Yes, right. And so I moved over into that slot, and we also have a thing called the Scripps Staff Council. In those days it was really just the principal investigators.

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[22]Fred Spiess was acting director of SIO from 1961 to 1963, and director from 1964 to 1965.

[23]"Scripps Institution of Oceanography" [organization chart], 23 April 1964. SIO Subject Files, Records, 1890-1981 (AC6). Box 1, Folder 4, SIO Archives, UCSD [following page].

Now it's a very large thing that has quite broad membership, all the professional engineers, the programmers, and so on. But in that day it was a kind of much smaller operation, and I wound up—

Henke: So it was the Academic Senate of Scripps or something like that.

Spiess: Yes, that was the sense of it, and I wound up being in that, in this same period. So when Revelle really left, why Herb York was the chancellor—and there was a sort of side issue that's a little amusing, to me anyway. When Revelle left the first time on leave, he was the chief campus officer, he was the dean of the School of Science and Engineering, and he was the director of the Scripps Institution of Oceanography. He was replaced by three people, all physicists: York came in as chancellor, Keith Brueckner took over as dean of the School of Science and Engineering, and I took over Scripps Institution of Oceanography. All three of us had been graduate students at the same time in the Berkeley Physics Department in the late forties. In fact, Herb and I both did our Ph.D.'s for the same professor, Emilio Segrè. And the three of us had all gotten there by different routes—because Herb had made it there by going around the senior administrator route, and he had been a senior person in the Department of Defense. I guess he was maybe the first head of what now is ARPA[24]. And I had abdicated from basic physics and gone off to ocean physics pretty quickly after Ph.D. Brueckner had stuck with the real physics game and he was an imminent theoretician at that time.

Henke: So would you say that your connection to York in some sense helped you to move up into this position as director of SIO? You were a known quantity to him, I suppose.

Spiess: I was sort of a known quantity. We were not close when we were graduate students, but I was, well, I guess physicists tend to go for other physicists. So there were a lot of things that I think were involved there, but certainly that was a very likely part of what it amounted to. But there was a brief moment when Revelle left and Herb asked me if I would take over again. I said, "Well, I don't want to be the acting director this time. I either want to be the director of Scripps or you can find somebody else."

Henke: This was the second time [Revelle left].

[24]Advanced Research Projects Agency

Spiess: This was the second time. And that time it was run through the Regents and all the procedures so that, for a brief moment, I was the director of Scripps, the director of MPL, the chair of the department, and the chair of the Staff Council. And I quickly got rid of the department job, and other things. That was the most jobs I've had around here at any one time, I think.

Henke: So would you have liked to have maintained the directorship of SIO if they had chosen you? Was it a job that you would have liked to have?

Spiess: I think if I had been asked I probably would have said yes. Yes, right. Bill Nierenberg[25] was brought in, another physicist from the Berkeley Physics Department, and whom I had known a little bit at Berkeley because he arrived as an assistant professor while I was in the latter stages of my graduate work. And I think there are a lot of things about being director of a place like Scripps. It's a high profile place, and I think that Nierenberg was better plugged into the national- level science community outside of the ocean science community by quite a bit. He had been doing [leading] things in the American Physical Society and for a little while was the director of Hudson Laboratory, which was a laboratory like the Marine Physical Laboratory. It was established by Columbia University but substantially after the war, about 1951 I think it was established. And it didn't last forever. It lasted 'til the early seventies, I guess, and then was disestablished. But he had played a role there. He had his Ph.D. from Columbia University, and there were some faculty members he had worked with who were leaders in the physics world, and since he had stayed with the physics world why those contacts were still there. Whereas, the contacts that I might have had if I had stayed in the real physics world were not of a sort that would result in enhancing the interaction between Scripps and the broader scientific community. I think that was probably the realistic way to do it. It would have been fun to, and [SIO] would be a different place if—[laughs] but that's. . .

Henke: What ways do you think it might be different?

Spiess: Well, I'm not sure. Just his style and my style are different, and places tend to reflect the style of the director to some extent. He brought in some things. He had a lot more exposure to the beginnings of the computer world, for example, working at the Radiation Laboratory at Berkeley, and so he brought the computer world into oceanography faster than perhaps would have happened if it had been the other way. Although my research group and the Marine Physical Laboratory people in general were probably at sort of the front edge of the digital computer activity as far as ocean science was concerned at that moment. But that was not the same place that you would have been if you were analyzing stuff from bubble chambers in the nuclear physics world.

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[25] William Aaron Nierenberg, SIO director 1965-1986

Henke: When you were director of MPL and also when you had your position as director of SIO, did you find it at all difficult to reconcile your administrative career and your research career? It sounds like they are pretty well connected.

Spiess: No, no, I think they were pretty well connected. You'd have to ask somebody else whether I was even-handed enough. I always like to think that I was, that I tried certainly in the MPL context. There was always an attempt to look at what the environment was in a broad way in terms of what roles can the various research groups in the laboratory play and to try to enhance the laboratory in general. And I was very fortunate in two ways. One is that the Marine Physical Laboratory had a very good administrative structure so that I could go off to sea for a month and come back and there wasn't any great disaster. At the same time, in my own research group, I was fortunate enough to have a succession of lead engineers who I could sit down [with] and brainstorm about what good things we were going to do the next time out. And then there could be a period of several months maybe in which we'd get together once a week or once every few days. It was not a matter of being in the laboratory doing things with my own hands. My hands were sort of remote. They were the hands of the engineers who were working with me. And that's where their imagination played [a big] role in the success of our group, because they had a little more freedom than they might have had if I had been in there every day. They had to have their own imagination about how to do the forward looking things that we were trying to accomplish.

Henke: I suppose in those days the way funding was given out probably helped a lot to—

Spiess: It helped. I did not spend the time writing proposals that PIs[26] have spent now for the last ten or twenty years. The funding sort of just came out of the committee, it came out of actually doing the work, and so the time spent on that, that sink was not there during this particular period.

Henke: Just a couple more quick questions. During this period that we've gotten up to—say [when] Revelle left and then came back because of the start of UCSD—it seemed to be a time of transition for SIO. In fact, I have this kind of interesting letter[27] here from Revelle to you and one Dr. James Arnold where he says, "I spent a sleepless night after our discussion about SIO" and basically about kind of reorganizing it in light of—

Spiess: The date of this is '61?

[26]Principal investigators

[27]Roger Revelle to Dr. James R. Arnold and Dr. F.N. Spiess, 21 September 1961. SIO Subject Files, Records, 1890-1981 (AC6). Box 1, Folder 6, SIO Archives, UCSD [following pages].

Henke: Yes, it's '61. Kind of when UCSD was starting up and SIO was trying to figure out—

Spiess: Well, that was an interesting period, and I guess now that I'm involved with establishing the University of California Merced campus, people ask a lot of questions about what was it like in the early days. And a number of the Scripps people were very heavily involved in the building of the [UCSD] campus. I have always looked back on that period, when I was standing in for Revelle and all the rest of it, as one in which my role was to try to keep the Scripps Institution alive in the face of the forces that existed. The primary force, the most general version of that force, is that in the fifties era a large fraction of the [SIO] faculty members were people who had their Ph.D. in physics, biology, chemistry, whatever, and had for one reason or another decided to go to sea and to leave the mainstream of their parent field. All of a sudden there were physicists being recruited [to UCSD], there were chemists being recruited here: there was a Physics Department, there was a Chemistry Department. It was an environment which, for a lot of the people at Scripps, it pulled them away from Scripps. They wanted to be part of not just seeing UCSD grow; they wanted to be part of the Physics Department or the Chemistry Department or whatever.

Henke: Like a joint appointment or something?

Spiess: Well, it wasn't even clear that they were interested in joint appointments. Some of them—in fact both Eckart and Liebermann took appointments in the Physics Department. And so there was this kind of separation phenomenon going on. I felt that my role was to keep Scripps alive. Jim Arnold was one of the lead chemists who came in the Urey[28] era, and he became one of the real leaders in the development. He was obviously a Chemistry Department type chemist and he hadn't drifted away from his main roots, although he had established roots outside of what one might think of the narrow range of chemistry. And well, he has become a real leader in the chemistry things that NASA does. I had forgotten about this letter. I think that the problem of holding Scripps together was really a kind of intriguing one. We recruited some good people in that era: Andy Benson[29] for example, one leader that we brought in during that era. We went through a time when the establishment of an earth science department was part of the game, and there was a move to disband oceanography, essentially; that everything that was involved in oceanography could be done within the framework of biology and earth science. And that was a sort of tense moment. I was not as closely involved in that as many others, although I was intimately concerned, because at that moment I don't think I was actually a faculty member. I was still in the research series.

[28]Harold Clayton Urey and James Richard Arnold were both appointed to UCSD in 1958.

[29]Andrew A. Benson

Sometime about ‘61 was when I was moved over into the Academic Senate world, and some of these were Academic Senate type meetings in which I was not a participant in 1960, early ‘61. I’ve forgotten when my appointment came through, but it was [around] ‘61. It may have been right about this time. And it was touch and go as to whether oceanography was going to survive as such. And a number of people gathered themselves together, Fager[30] and Isaacs[31] and several others, and made enough noise that things stayed the way they were.

Henke: So in an attempt to kind of keep SIO as a unique and oceanographic—?

Spiess: I don’t think there was ever a thing that said Scripps ought to go away. It was a question of whether academically, in the degree-granting side of the house, Scripps would become a strictly research entity within the university or whether it would maintain this unique position that it has among the organized research type units: of really being a school, of having both the educational and research functions.

Henke: I see.

Spiess: And that was really what was at stake at that point. And we managed to hold it together well enough that we then survived on into when Nierenberg came. When he came, UCSD had an Earth Science Department. Scripps had the Marine Biology and Oceanography departments. The Earth Sciences Department, Physics, Chemistry, Biology departments existed in the general campus. It was an interesting kind of oscillation between people going out of Scripps and people coming back: the group that had gone into this Earth Science Department that was established on the upper campus, and recruited a bunch of other people, after a while they seemed to realize that being in a graduate school was a different kind of job than being in a place that had undergraduate education as a primary element. And at some point in the late sixties it was decided that the Earth Science Department on the upper campus would be disbanded and the billets were all transferred to Scripps, and some of the people were transferred to Scripps and some were kind of frozen out one way or another over the year or two. And that was a move that was great for Scripps and for which Nierenberg gets a fair amount of credit. He also gets a lot of blame from people on the upper campus who remember that a bunch of upper campus billets were shifted over to SIO. And I have run into that sort of thing. Anyway, a little bit at least on the topic that you asked about. ##

[30]Edward William Fager

[31]John Dove Isaacs

INTERVIEW THREE: 9 FEBRUARY 1999

Henke: ## Dr. Spiess, perhaps today we could start off with more of a general question. You have a reputation as being an instrument builder kind of guy, right? Is that something that you started off thinking of yourself as having an interest in?

Spiess: Well, I think instruments can be of all kinds. One of the instruments we've got involved in is 350 feet long and twenty-five feet wide. I guess in the world of physics in which I grew up there were experimentalists and theoreticians, and I wasn't bright enough to be a theoretician, so I've always thought of myself as an experimental physicist. And the line there between engineering and instrumentation—the name of the game if you're a really good experimental physicist is to understand some of the questions that are perplexing other people in your field and think up some bright way of building the equipment to carry out something that nobody else can do. And that very often of course leads you into a string of things in which you're really exploiting some one particular line that you've thought of. I guess having grown up in Berkeley where Ernest Lawrence got his Nobel Prize for thinking up the cyclotron, this was a pretty honorable thing to do. When I came to Scripps I was in underwater acoustics and marine physics activities in the context of helping the Navy to do things better as well as understanding the ocean in general. And the thing that impressed me as I became more involved across the whole spectrum of Scripps activity was that this line of endeavor is viewed in different ways in different fields.

Henke: Oh? How so?

Spiess: Well, at that time at least [among] the geologists, for example, if you thought up a new way of doing something that involved a new piece of machinery or something, this was a thing the technicians did. This is not a thing that geologists did. And as a consequence, in fact, the development of things like that in geology was not very vigorous. To some extent this was true in the marine biology arena as well, although there were some people around here doing some innovative things in that area. In any event, that was still what I was—that was my bent to do that. Whether that's [being a] technician or whatever, it has been an interesting game. Well, I did indeed do some innovative things in the underwater communication world and so on in connection with the Navy's activities in the fifties, but it was around 1960 that a combination of circumstances in terms of what the Navy was interested in led to an opportunity to do some pretty innovative things.

Henke: What kind of things were they interested in?

Spiess: Well, the problem was how do you understand how sound travels through the water in some detail—trying to determine the direction to a sound source that might be twenty or thirty miles away after the sound had traveled through a fair amount of water and perhaps bounced on the sea floor. So the question of doing an experiment of that kind triggered the development of FLIP, which was one of the major things I was involved in the early sixties. The main purpose there was to be able to have a system that could sit on the ocean and not be disturbed too much by the wave motion. And [also], in the sonar bearing accuracy context, to be able to have acoustic receivers down fairly deep in the water—that is, deep in the submarine operating sense, down to as much as let's say 300 feet—and at the same time to have enough structure up in the air that you could make independent determinations of the direction to a particular sound source. And that was operated from a ship or something of this kind. So I can remember there was a study I was involved in which we talked about vehicles that might fill this bill a little bit, and then there were some studies that people did on manned spar buoys and things of this kind. Because the spar buoy is a natural thing to think about. In its simplest form, it's just a pole that sits vertically in the water and as a result the waves can't get hold of it very well. And the deeper down in the water it extends, the less easy it is for the waves to get hold of it, because the amplitude of the pressure fluctuations associated with the surface waves falls off as you go down in the water column. So at MPL we thought about this sonar accuracy problem, and Fred Fisher, one of my colleagues, did some experiments using an operational submarine as the platform basically to carry out experiments of this kind.

Henke: That's the Navy sub, I assume?

Spiess: Yes. But that was pretty cumbersome because you're into a big arrangement in order to pull off an operation and you didn't have real control over when and where you were going to do things. So we started thinking about, in the first instance, finding a surplus submarine and just turning it up on end. And we looked at that. That was an idea that had been suggested by Allyn Vine, who was a very innovative person at Woods Hole. That turned out not really to be a practical [option] by the time you did all the modifying inside the submarine to make it a tenable thing on which to operate. So we took off and did a study of what you could do if you just plain built the right platform to start with. And it was at this point that another one of the Marine Physical Laboratory physicists, Phillip Rudnick, entered the scene. And we talked about how one could shape the underwater body of a thing like this such that the cross-sectional area at the sea surface is very small, and yet the volume below the water line is large. The restoring force if you just push it up and down is very small because it depends on the change in buoyancy just because of change in displacement due to the waves going up and down at the top. And the thing you're driving is the total mass of the ship. So we did some calculations. Phil Rudnick was the leader in that. The first step that we did was simply to have something that was a fairly thin cylinder in the upper half of about a 300 foot submerged portion, and then the lower half was about twice the diameter. Another aspect of this is that the wave forces on a thing of this kind depend on the fluctuations in the pressure field, and the step between the narrow and the thick parts then was a surface on which the pressure could develop a downward force. Then there was a corresponding upward force on the bottom of the thing, except that by the time you get to the bottom of this the wave amplitude is less. So you can find a place where in fact the wave forces just plain cancel out at a particular wave period. Anyway, we went ahead with that, and in the context of the Navy SUBROC program we were funded to go ahead and build this thing. And so we did.

Henke: So the Navy was pretty interested in this idea.

Spiess: The Navy was interested in the results that we would hopefully get out of building it. And that [led us] to contact Larry Glosten, who is a naval architect up in the Seattle area who had a small consulting company, and he was the one who did our final naval architectural design and oversaw the construction in the shipyard in Portland. We were fortunate. If you have a project that's pretty big in a shipyard, you really need somebody working for you who stays in the shipyard. And in my submarine Naval Reserve activity when I was a graduate student at Berkeley, there had been a naval officer assigned as an assistant to our group, an ex-enlisted man who had been in the Navy a long time. And I had worked together with him quite well in developing training capabilities for the submarine reserve. He had subsequently been a division engineer in the submarine squadron down here in San Diego, and it turned out that he had retired a little bit before the time we were going to build FLIP. [He] lived in Oregon and was tired of his decision to become a farmer, and so he became our man in the shipyard.

Henke: What was his name?

Spiess: Earl Bronson. He was very good at maintaining good relations with the shipyard people, and if there were change orders and things of that kind he was a good negotiator to see that we didn't lose too much money over them. The nice thing was that we really had full control over the whole process. We didn't have any other administrators involved in this. We had managed to keep it out of the Navy Bureau of Ships cognizance. It was a barge; it was not a ship, had no propulsion of its own. And so we managed to stay away from the bureaucracy that would have attended building a more conventional ship.

Henke: Was it pretty unusual to get that kind of authority over it?

Spiess: It didn't seem so unusual at the time. We worked at it, of course, and occasionally we would have a review session where we'd bring in a few people from the Bureau of Ships, just so that they would feel that they had some sense of ownership. But in retrospect it was indeed pretty unusual. And it led to our being able to get something built that was new and different, and I guess one of the fun things that I can remember in my life is that eventually it was indeed built and we towed it up to a deep inlet in Puget Sound, Dabob Bay, and that was where we did the first flipping. I had the opportunity to be in charge of the trial crew for that, simply because the shipyard, which normally would have put on the trial crew, didn't know anything about what it was that we were doing, or what the ship was all about. So there was one person from the shipyard, Earl Bronson and myself, Fred Fisher, who had been a close colleague and had done a lot of the work in the design phase, and Bud Bundy, who was in charge of our machine shop at the Marine Physical Laboratory at that time. We were the trial crew, and so we got to sit up there while we opened the valves and flooded one end of the ship and turned it on end.

Henke: And it worked.

Spiess: And it worked, and it worked quite nicely. And from there on it was a matter of making incremental improvements and in fact going out and using FLIP—not just for the bearing accuracy work, which in fact was all done by Fred Fisher, but since it was a stable platform this was a good thing to use to suspend hydrophones down in the ocean and do studies of long range sound propagation. That was one of the arenas in which I was working at that time. I can remember sitting out on FLIP a hundred miles or so north of Hawaii while other people ran sound sources up and down, and sat there in lovely tropical weather, looking out at the surroundings and you knew that the people running the sound source were up there toward the Aleutians and the weather was not so nice, and you could sort of feel that you owned the ocean. It was a really satisfying experience.

In one of the very early operations FLIP was used as part of an experiment that Walter Munk was doing. He had a study going of the propagation of long wavelength ocean surface waves from big storms in the far South Pacific. The idea was to look at how those decayed as they traveled all the length of the north-south link of the Pacific Ocean. And Walter had island stations at which he could measure wave amplitudes strung out between—I guess Fiji was the southernmost one and then they were on up on various islands up to Hawaii and then up in the Aleutians. There was a big gap between Hawaii and the Aleutians and so we took FLIP out there with appropriate instrumentation about halfway between the two places. That was the first long-term deployment for FLIP, and it was an exciting one for Earl Bronson, because I was not out on that trip. They were towing along, and the weather got to be kind of nasty, and suddenly they realized that you could see that where the cylindrical part of FLIP's hull joined against the spoon shaped bow section up in here. But the drawing that you have has a bunch of gussets between the bow section and the cylinder.[32] Those were not there in the original design. And what Bronson found was that the plating at the after end of the hull section, and that's where the engine room was, was beginning to crack right where the cylinder joined the flat section of the hull. And so he did the courageous thing of just deciding, well, if that was the case that he didn't want the thing to come apart, but he took the chance and flipped into the vertical. And once it was vertical why then the two things were held together pretty nicely by gravity. The towing ship at that time was the *Horizon*, one of the Scripps fleet. The captain, John Bonham, was a good ship fitter, a welder, and he had some spare iron plate and they simply boated him over onto FLIP and he came on board and welded a bunch of doubling plates around the zone that was beginning to crack and then went on about their business. Once they came back, we had a major reconstruction job in that area and Glosten designed some additional structure to go in there.

Henke: This wasn't the incident where there were actually waves starting to crash over the top?

Spiess: No, that was a later time. That was an operation in which FLIP was out doing some deep water propagation experiments with hydrophones hung down from FLIP's winch. There were a couple of big storms that coincided up in the Aleutian area and created pretty big swells that came on down, inundated some of the smaller islands in the Hawaiian chain, and FLIP was out on the station at that time. And the waves were big enough that the [towing] ship that was nearby had no problem at all, because the ship just went up and down with the waves. FLIP was designed not to go up and down with the waves. When the waves got to be very big, that meant that when the trough came by the center of gravity of FLIP was above the center of buoyancy because the water line had suddenly gone way down, and so that FLIP would do something that was kind of like falling over, except of course the wave came back up again. But it was a reasonably impressive experience I guess for the people who were on board. And in fact the crests of the waves came well up onto the living quarters of the upper part of the vehicle and flooded into the engine room through ventilation ducts and they lost all their electric power and decided that they would abandon ship. This other ship was nearby, and so they put out their rubber boat and people went—you could almost step over the side when the crest of the wave came by. One person who had bad timing, one of the technicians who was on board, hesitated when the crest came by, and so he went down quite a ways because the wave was going back down with gravity just the way he was, and so he resigned from our service after he came back from that trip. But that was an exciting one that showed that FLIP really could take care of all this. The people went back on board,

[32]Dr. Spiess is pointing here to a blueprint of FLIP on the table during this interview: L.R. Glosten, "FLIP Construction Views," 7 October 1965. SIO Marine Physical Laboratory Records, 1941-1990 (AC 15), Box 8, "Research Platforms - Spiess." Scripps Institution of Oceanography Archives, UCSD [detail, following page].

and the only damage was to the electrical system, and the ventilation system was modified to be sure that we weren't going to take in any water, even up from a place up quite high on the ship.

But it's been a very successful thing. It supported a lot of different kinds of acoustic and physical oceanographic research, supported the research of one of my students, Rob Pinkel, who is now a full professor [at SIO] these days. He did his thesis using FLIP as a vehicle to study internal waves in the ocean, by putting booms out and hanging temperature sensors down from the buoy so that he could measure the way the stratification of the ocean oscillates up and down.

Henke: Was it pretty common to—when you say started thinking about designing an instrument—to have so many things in mind, or did you just usually have one thing in mind and it just kind of went on from there.

Spiess: Well, in this case I think this is an example of a different way of designing something of this kind. The idea was to make something that would be flexible enough that you could do anything you wanted with it, or that you could kind of visualize a wide range of things you could do. Following our initiative, the Naval Ordnance Lab built a [ship] that was somewhat similar, but they took the attitude that they knew what the experiment was they wanted to do, and they built something that was good for that experiment. They decided that they wanted it to operate in an unmanned mode and that you would just have a cable that went over. Then you'd build yourself a telemetry system on the spar buoy and it would send all the information over to the towing ship once the thing was up vertical. Well this meant that your opportunity to try something out without going through a big system design was quite restricted, because you had to know what everything was you were going to do so that you could build everything to go on the spar buoy and send all the information that you needed over to the ship, rather than being on there and being able to cobble up an experiment that was maybe kind of marginal to begin with, and you could then improve as you went along.

Henke: Did that happen pretty often?

Spiess: That you start with something that's marginal and—?

Henke: Yes.

Spiess: Or maybe marginal is the wrong word. Something that's basic. And sure, that's one way of approaching research problems, and it's the way that I prefer myself. You should have some particular thing in mind, because if you can't do experiment number one then it's not going to be around to do experiments ten through twenty.

But in this case we did this knowing that this was going to be just a nice, stable platform out in the ocean, and it would be up to the imagination of other people as well as our own imagination to think of other experiments for which one could use it. And so two of my students got Ph.D. theses out of this internal wave thing. The first one was a naval officer who was here as a graduate student, Robert Zalkan, and then Rob Pinkel followed along after him, and has continued to use FLIP in much more sophisticated ways to study internal waves and turbulence in the upper part of the ocean.

And in this same SUBROC context the other part of the game was to understand what the fine scale nature of the sea floor was in terms that would be relevant to what would happen when a sound wave bounced on the sea floor, where would it go. In that context it was clear that one needed to know more about the fine scale topography of the sea floor, than you could find out with an ordinary surface ship echo sounder. And so, given the interest of the Navy in this, we proposed to build something you could tow down very close to the ocean floor, and then essentially you do shallow water echo sounding in the deep ocean just by being down there so close to the bottom. We didn't propose that in a complete vacuum, because again, we knew that there were a lot of other reasons why you'd like to be down close to the sea floor.

Henke: Were one of those reasons the incident with the *Thresher*?

Spiess: Yes, but the *Thresher* thing came after we had started building the Deep Tow system.

Henke: I see.

Spiess: What happened was that the Navy was building—shouldn't say the Navy. Ed Wenk, who was an engineer, an imaginative guy at the David Taylor Model Basin studying submarine pressure hulls, had deduced that if you made your pressure hull out of aluminum you could have a much better strength-to-weight ratio and you could therefore have a better, at least for deep diving, [submarine]. You could build yourself a submarine that would be big enough to walk around inside of and could go down to ten, fifteen thousand feet. He left the David Taylor Model Basin and interested the Reynolds Aluminum people—one of the managers, Louis Reynolds—in the idea of building a very small submarine that could indeed go way down deep.

The Office of Naval Research, which the Marine Physical Laboratory and Woods Hole operated with in the Navy applied physics realm—the head of at that time was a fellow about my age, Charles Momsen. His father was the Momsen of the Momsen Lung. The younger [Momsen] was in charge of this part of the Office of Naval Research and felt that some kind of small submarine to go and explore the sea floor down deep was a good idea. And he put together a group of about three or four of us to think about how you could use a submarine of this kind with the idea in mind that the Navy would encourage Reynolds to build this submarine and then charter it from him. And Momsen began to squirrel away the necessary budget to do that. In the context of thinking about how this submarine might be useful, I rounded up several people here at Scripps and we had some brainstorming sessions and realized that the kind of submarine that this was going to be, maybe fifty feet long, was not something you could go around poking into little holes with; it would be much more effectively used if you simply used it as a survey ship down near the bottom. Because you could then do gravity measurements, magnetic measurements, topographic measurements, the whole suite of things that you would do in the geophysical world. But you could do them very close to the bottom so you could see fine scale detail that you wouldn't see from a ship up at the surface. And, in particular, there was some interest in what the fine scale magnetic field measurements might show. By then the initial Mason-Raff anomaly maps off the west coast here showing the striations in the magnetic field were well known, and [there was an] idea that they might be related to something, which eventually turned out to be plate tectonics and sea floor spreading and that sort of thing. Kind of an embryo sort of thing. But if you could get down close to the sea floor, you might see something about those anomalies that would let you decide whether they were deeply seated down in the crust or whether they were fairly superficial. So there were all these different things, and when the opportunity came through the SUBROC program to say, "Yes, we'd like to be able to do survey things down here at the bottom and we could get a leg up on it by building something that would do the survey work for the fine scale topography that was relevant to the underwater sound propagation problem." So we put all that together and were funded.

Henke: This was the committee you were on with—?

Spiess: This was through the Navy committee. The Marine Physical Laboratory was funded almost in one lump to do several [things]: to build FLIP, to bring a small ship out of mothballs here in San Diego to be a towing ship for FLIP and for a deeply towed echo sounder system, and to build the initial version of the deeply towed echo sounder system. And all of this came about in 1961, I guess, and it was really quite a windfall that set us on the road to a whole bunch of things. The deeply towed echo sounder clearly was another instance in which we went at it with the idea in mind that it would be great to know what the fine scale topography was but there were a lot of other things that we could find out about if we had a vehicle that we could tow down near the bottom. And so that enjoyed an incremental growth over the years.

Henke: So in talking about the Deep Tow system and the way it turned out, it was decided that a submarine style of thing wouldn't be as useful as the—?

Spiess: Oh, well, I didn't follow along with what happened to the submarine end of that. In the first place it became very difficult for the Navy to deal with Louis Reynolds. He wanted to keep his hand on that little submarine so he could go treasure hunting or whatever, and the Navy did eventually wind up using it in some development work later, after the *Thresher* event. But in the 1961-62 time frame it became clear that was not going to be a way that the Office of Naval Research could move into the little, deep-diving submarine game. And since Momsen had been clever enough to put aside budgeted money for something of this sort, that money was available, and so he sat down with Allyn Vine at Woods Hole and they built *Alvin*, the little three-man submarine that exists to this day, although substantially modified. So the end result was that the Office of Naval Research did indeed get a submersible capability. The people in the advisory group that the Navy put together in the context of the original Reynolds thing went in two different directions. The group that I was involved in went off in the near bottom survey mode; the other group was personified by Allyn Vine, and Al Vine was into the business of poking around on the bottom on a sort of small scale basis. And by the time it was clear that the Reynolds thing wasn't going to work, we were starting the business of doing the survey work simply by towing something down near the bottom, which really was a much more practical way to do it, and the poking around on a small scale basis was then implemented in the *Alvin* context at Woods Hole.

Henke: It's kind of ironic that you as the submariner actually went in the opposite direction there.

Spiess: Yes. I've often thought about that. Although I'm really quite attached to the submarine force, world, whatever, and proud of having been part of that. When I first came to Scripps, if I had been just a little quicker on my feet, everything might have been quite different. One of the first things I thought about, obviously, was gee, it would be neat to have a submarine for a research ship. And as it turned out, the Navy, right in the late stages of World War II had built a couple of small submarines that were fairly simple. They had built them to be training ships for the submarine force. And those were taken out and scrapped. And if my timing had been a little better I might very well have gone the other way and gotten one of those submarines. Because the business of bringing surplus ships into the oceanographic world was pretty well-accepted, and San Diego was full of people who had submarine experience, more often in the civilian world.

In any event, [during] the first couple of years [at MPL] I was pretty heavily involved with the submarine force activities in the sonar and communication world. But from there on it seems as if my goal has been to liberate us from having to use submarines. They are complicated, and they involve some, well, including the small submarines, maybe even more evident in the small submarine world—is the fact that you're out there doing research in an environment in which people's lives are really at stake. If you lose your little submarine, why you've lost a couple of scientists and a pilot, and that would be a real setback to any kind of research. People who operate *Alvin* have done a very good job of not getting caught in anything of this sort. They've been very careful and well-trained. But it's a lot more comforting to be able to sit up on the ship and put your thing in the water, the great tow device or whatever it may be.

And I have upon occasion had the wire break, and you lose what you had down there. In fact [on] one of the early Deep Tow operations we had the wire break, and this was down south at the end of Baja California. We were doing a thesis thing for Bill Normark, one of our graduate students, and the wire broke. I should say that one of the things alongside of the Deep Tow game that we did indeed become heavily involved in was the question of how to use acoustics for navigation down near the sea floor using acoustic transponders. Nowadays you can buy these things off the shelf. In that era we invented and designed our own transponders, and in fact, since we did that under a Navy contract, why we passed along the recognition circuits and things of that sort to some commercial people who wound up eventually being the Sonatech transponder builders in the commercial world, selling a lot of transponders to the Navy. When we went off to do a survey, we would put down some transponders so that we could track where our deeply towed vehicle was and handle the ship and the vehicle as a unit. And so when the wire broke, why we knew where we were in the transponder net pretty well and felt a challenge that we ought to try to get our thing back out of the bottom of the sea. After we came home we did indeed spend some time devising a thing we could drag on the sea floor to engage the presumed pile of wire that there was down there and bring that all back up again. We actually went out on the mud flats alongside of Mission Bay and tested our cable grabber in that context, towing it over the bottom with a pickup truck and having adequate chunks of wire out there that the thing would run into and see whether it would grab the wire properly and bring it back up. We were worried. The wire was a little bit on the old side, and we were worried that if we just went at it with a real simple grappling hook or something that it would be bent sharply enough once the load was on it that it would break again, and then we would just be into more problems. So we built something that would be much more robust in the way it picked up the wire, and went out about six months later on the next expedition, which was primarily on another graduate student thesis.

This was a nice thing about the Navy programming at that time. We were really by then into the follow-on from the *Thresher* game and learning about how to do sea floor searches and what the natural background would be against which you would have to find a submarine. And the Navy was pretty easy about what areas we would go to to learn about these things, and so we could pick out the areas in ways that had meaning relative to some kind of good geological project. Because from the beginning with the Deep Tow thing, knowing that we could do a variety of tasks with it, we involved graduate students who were geologists. And usually they were pretty imaginative geologists, because they were willing to take their chances with a new kind of device in order to have new kinds of data to which their colleagues did not have access. And so there was a string of very, very good graduate students that went through here. And the problems that we investigated were largely ones that had to do with deposition and erosion of sediment and with a lot of work at ridge crests, because if you look there at the fine scale stuff you're really seeing brand new crust.

Henke: So that's how you got involved in a lot of these articles that came out in the early seventies and so on, like for instance this one on "Abyssal Bedforms Explored with a Deeply Towed Instrument Package"?[33]

Spiess: Right.

Henke: So you started to get into the geological areas.

Spiess: That's right. It became much more geology oriented, and I went from some vague ideas about the fact that this could be a useful tool—some of the vague ideas were not too vague in the sense that as a physicist the step into geophysics measurements of magnetics and building side-looking sonars and so on, those were not strange sorts of things.

I guess the most disappointing thing to me was since I have been involved in doing some gravity measurements in the 1950s with Vening Meinesz pendulum systems, and had in fact built a gravity measuring system myself in the fifties, one of the things I thought that was maybe that this towed vehicle down at the end of all this wire sitting behind the ship, that the vehicle might be much more stable than the ship and consequently could be used as a platform for measuring gravity down near the sea floor. It turned out that that was not the case. ##

##There was no easy way to measure gravity down there. Eventually Mark Zumberge here at Scripps, who was a gravity person, has indeed put together a system that can be towed down near the bottom [and is] more or less a two-part tow. You have something that's on the end of the wire and then streamed out behind that is a more or less neutrally buoyant device that has your gravimeter in it and consequently is not jostled up and down to the same degree as the instrument would be if it were right on the deeply towed system.

Henke: So with each deployment of the Deep Tow system you were doing multiple things at the same time. There were things that the Navy was interested in, and there were things the geologists were interested in?

Spiess: Right. Eventually the Navy interest waned, and we wound up being primarily supported in Deep Tow activities by the National Science Foundation [NSF]. At that point it was all strictly geology, geophysics. Well, there was some capability to make modest improvements in the vehicle. Add some additional instruments to do

[33]Peter Lonsdale and F. N. Spiess, "Abyssal Bedforms Explored with a Deeply Towed Instrument Package" *Marine Geology* 23: 57-75 (1977).

optical properties [of] the sea floor, and CTD[34] to look at the salinity distribution, temperature distributions. I guess some of the major things were ancillary devices that other graduate students here at Scripps put together and then we made it possible for them to go out to sea and use those in conjunction with the other, more standard devices on the vehicle. A real strength of the vehicle is that you make multiple kinds of measurements all at the same time, and so they're all there in the same navigated reference frame. So you map the fine scale magnetics, the fine scale topography, the sea floor images, acoustic images using side-looking sonars, and all of this is there at once. And if you really need to document something in a little more detail, you can fly down a little closer to the bottom and use film cameras or video cameras to determine even more precisely what's down there.

Henke: Was that how the realization of the things on the East Pacific rise crest were—?

Spiess: Well—

Henke: How did that develop?

[34]An instrument that measures conductivity, temperature, and depth.

Spiess: Karen Wishner, who was a graduate student biologist, wanted to understand about what sort of planktonic things there were down near the sea floor, and she built a set of nets that we could put underneath the vehicle so that, as we towed along, we could push a button and a particular net would open, and then push another button and it would close and a second net would open, so that you could do controlled plankton sampling. And that was basically the bulk of her Ph.D. thesis here at Scripps. She is now a professor at the University of Rhode Island. The other thing was that Ray Weiss, a graduate student here at Scripps, built a set of, in vernacular, water sampling bottles. They didn't look like bottles. They were really pretty good sized cylinders with some trick closing mechanism so that you could mount those on the underside of the vehicle and tow them along and meanwhile watch a temperature sensor or something of that sort, and if you came upon something that seemed anomalous you could push the button and grab a few liters of water and bring that back up to the surface when you brought the vehicle back up again. And he built a set of these, and we did an expedition.

I guess Peter Lonsdale, who is another one of my students who grew up in the Deep Tow group, was the expedition leader. They went down to the Galapagos spreading center, which at that time was notable because Dick Von Herzen at Woods Hole had done heat flow measurements with sufficient density to show that there were some interesting patterns that indicated a couple of things: one was that there was warm water coming up at the ridge crest. This was something that people were gradually realizing, from the heat flow measurements, that as you went closer and closer to the ridge crest and you stuck your heat measuring probe into the sediment you got a bigger number. When you got really close where there wasn't very much sediment, there seemed to be a decrease in the amount of heat that you would see being conducted through the sediment. And the conclusion that people came to fairly quickly was that in fact a lot of the heat was being taken away by diffuse circulation of sea water through the cracked crust that was not sealed off by the sediment in this region, so that a lot of the heat could be dissipated in that way. The whole heat flow question at that point still had a lot of question marks in it, but it was clear that there were some things happening and that there should be some sort of diffuse—what was imagined to be diffuse—circulation of water through the sediment, through the cracked fresh rock and up into the water.

And so Lonsdale went down with Weiss and the water sampling things and they indeed found places where the water was warmer than the surrounding water and they grabbed samples from that, and when they were analyzed at a later time they found the trace elements in them that indicated that the water had indeed been down in the crust for a while, circulating around down there. That was really the first sampling of the warm vents. On that expedition they did photograph some fields in which there were bunches of what looked like dead clams. And so those were the first indications that there was something down there. I wouldn't say that that's what triggered the main expedition that actually found the first of the hot springs down there on the Galapagos spreading center. That was Corliss's[35] expedition--Corliss, another Scripps graduate student, but a geologist. And that expedition would have taken place whether or not we did ours. Because both of those expeditions were predicated on trying to learn about what these heat flow patterns meant. So it was the heat flow pattern that drove both our expedition and Lonsdale's expedition and the subsequent *Alvin* operation that found the tube worms and that sort of thing.

Henke: I believe this is a picture from the *Alvin* in there.[36]

Spiess: Mm-hmm. So the Deep Tow thing has been used—well, it was used in the manganese nodule arena. There was an era of very intense commercial interest in [undersea] manganese nodules, and people carried out a lot of expeditionary work looking at how manganese nodules might be formed, and from the commercial side simply analyzing how much there was down there. And there was a thing called the International Decade of Ocean Exploration [IDOE] that represented a really major infusion of funds into oceanography in the seventies. In that set of big, cooperative, multi-institution programs, one of them was called the manganese nodule project, whose goal was to try to understand how the nodules came to be. And our group did several site surveys for that. They had several sites that were sort of different environments in which there were nodules, and so the idea was to study those one by one. And basically we did the site survey work for all of those. Then commercial interest had grown to the point where there were concerns about pollution due to disruption of the communities that burrow in the sea floor mud, and we did an expedition. One of the companies that had been doing development work in the mining [of nodules] had built a prototype mining device that they towed along on the sea floor for an operation. And a few years later we were funded by the Mineral Management Service to go out and look at what had happened in that case, and we actually found the tracks of the mining device and took box cores both in the tracks and off to the side so that people could—

Henke: With the Deep Tow system you did this?

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[35]John Burt Corliss

[36]Referring to a picture on page 232 of: Fred Noel Spiess, "Some Origins and Perspectives in Deep-Ocean Instrumentation Development." In: Mary Sears and Daniel Merriman, editors, *Oceanography: The Past*. Proceedings of the Third International Congress on the History of Oceanography, 22-26 September 1980, Woods Hole, Massachusetts (New York: Springer-Verlag, 1980) 226-239.

Spiess: Well, the Deep Tow system [did] the survey work, and then the coring was done with more or less standard box cores. That was an expedition in which Bob Hessler here at Scripps and I were the co-chief scientists. And at the same time we were working on a project to see how much we could learn about the manganese nodule—or about sea floor properties, as far as that was concerned—from acoustic back scattering. And there was a student who was working [on] a thesis in that regard, and so that thesis was a piggyback operation on this. We had a bunch of extra acoustic transducers so that we could look at sound back scattered at several different frequencies. And that was another Ph.D. thesis for Marc Weydert.

Henke: It sounds like it was pretty common for students to design projects and have them brought out onto the ships and the Deep Tow system or whatever.

Spiess: Yes. It's a tricky game, because you have to fund them, and so it was a matter of getting enough geology understanding to be able to develop and successfully fly an NSF proposal for an expedition and the subsequent data processing and analysis. By and large, for a while every expedition was just plain a Ph.D. thesis expedition. There was a graduate student involved. Usually there were other graduate students on board who were sort of groping for another project, and so it was kind of a continuing thing there for quite a number of years, and pretty successful I think in producing students who have gone off and done good things.

Henke: You mention the NSF funding. We've covered a period from say around 1960 up into the seventies now. How was the situation for funding changing over that time?

Spiess: In NSF it was not all that bad. Certainly it was not as competitive as it is today. I essentially wrote a series of NSF proposals to investigate the fine scale properties of the deep sea floor. I kept having to think of some way of changing the phrasing so that I could tell one from the next. And of course there was other funding. We had some industrial funding for some of the manganese nodule things we did, although that was at a fairly early time in the manganese nodule interest.

Henke: Was ONR funding still important at that point?

Spiess: ONR was still doing some of the funding. And in fact the incremental improvements that we made in the instrumentation of the vehicle, over the period of the sixties and seventies, a great deal of that new stuff was ONR funded. The fact that we were sort of breaking new ground in the technological sense was of as much importance to the ONR people as the new science. Not that they weren't interested in the new science. They wanted to support things that seemed as if they would be productive in that direction.

But I guess one of the other things, we did an expedition in which we went Deep Tow-ing in the Aleutian Trench. That was the deepest one that we've done. We built a new vehicle for that. We got ourselves a surplus hydraulic actuator of the kind that they were using to open missile silo doors in land-based missile systems. And the diameter of the actuator was just about right, the cylinder, to make a good, quite strong pressure case for electronics. And in that case we wanted to tow at about 7,000 meters depth, and if one did the calculation first you found that there wasn't really quite enough wire in the 9,000 meters that we generally had on the ships that we were using. This was special wire that had a [coaxial cable] core, so we could have all the signals going up and down. And in addition, although you might marginally get down there, the mass of wire that you had was such that if you visualized a reasonable sea state, the additional forces on the wire due to the acceleration up and down of the wave motion would just be more than you wanted to try to cope with. So what we did was we—there was an auxiliary winch on the ship we were using at that time, the *Thomas Washington*, and so we decided we would have a two-part system. We had some smaller wire with [coaxial cable] in it, but it was not capable of handling the heavy loads that we had. But what we did was we rigged it so that we could lower the vehicle over using this length of the smaller wire. We had 10,000 feet of that, and we put that in the water, and then we had a coupling on the end of that so that we could then swing around our crane with the other, bigger wire that we used, and couple those two things together, and then swing away and pay on out, and have a capability to go down to the 7,000 meters in which we wanted to work.

That was an interesting one to do, because the business of how you transfer the load from one of these wires to the other and so on was something that we had to think about pretty hard, and there was no chance to practice. So we just had to go off and do it the first time, had to do it right the first time. A real key figure in this whole Deep Tow thing was an engineer who worked with me for many years, Tony Boegeman,[37] who was great at thinking up things of his own, but also of implementing bright ideas that the graduate students or I would have about things we ought to be able to do. And he was not the lead engineer right at the beginning of the Deep Tow work, but by about 1969, when we were beginning really to go to sea, he was the lead engineer. And there's a picture of the two of us on a kind of foggy day above the Aleutian Trench, standing on each side of this thing, wondering how we're going to make the next move. And that's one of the pictures that I treasure.

Henke: Was it pretty often that on a cruise like that you kind of were just trying to figure out how—I mean, you don't always get a practice session.

Spiess: The nice thing about the Deep Tow game was that we established at an early time there were some very useful things we could do. Doing the near-bottom topography and photography and magnetics, those were things that, once we had those figured out, then we could go out and do something else that was new, and if it didn't work

[37]Dwight E. Boegeman

then we still had a viable expedition.

Henke: I see.

Spiess: And so of course in this case if this hadn't worked, we either would have left our vehicle on the floor of the Aleutian Trench or we wouldn't have had a viable expedition or something or other. So it had to work.

The whole Deep Tow thing has been a really satisfying game in that there's been so much interaction with graduate students and new ways of doing things. I learned a lot of geology. I'm no geologist even today, but I know a lot more than when I started because there's nothing like having a succession of geology graduate students to help you to cope with all the terminology or whatever else.

Henke: Let me ask you a question in a slightly different direction. One of the things I found interesting when I was looking at the archives, especially about FLIP, and Deep Tow to some extent as well, is the reaction that these instruments got within the public arena. It seemed as though there were all kinds of people wanting to know more about FLIP and have pictures, and school children writing in and stuff like that.

Spiess: Right. FLIP, well FLIP really—

Henke: Really caught people's imagination.

Spiess: Caught people's imagination. It's big enough that you can see it and you can see what it's doing. In fact it had people's imagination adequately that when we did the first flipping in Dabob Bay, *Life* magazine sent along Fritz Goro, who was one of their top level photographers, to cover the event, and we had about a four-page spread in *Life* magazine,[38] which in those days was quite a bit of publicity. And it is a thing that school children can understand, elementary school children. And I guess I was amused. . . . Well, you run into people here and there to this day who remember learning about this in school.

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[38]"We Race for the Ocean's Secrets," *Life* 53 (25): 40-43 (21 December 1962).

And I had a situation where I had been out at sea on some other ship doing some different things, and as we came in to San Diego we went past FLIP, which was out sitting there vertical doing something. And I snapped a few pictures, and later on went off on vacation up near [Lake] Tahoe, where we have a little cabin, finished off the roll of film, and took it down to be developed at South Lake Tahoe. I went in to pick up the films afterward and the fellow who processed them said, "Are those pictures of FLIP in there?!" So it's really been a thing that continues to catch people's imagination. There's been a recent BBC[39] special on FLIP, and we have Bill Gaines,[40] who is the current assistant director at MPL, a retired naval officer, has made a lot of good connections. FLIP has had its ups and downs in the funding world, and right now the funding for FLIP operations is something we have to keep thinking about pretty hard. And so Gaines has been pretty good at seeing people who are in the Navy or other agencies that might use FLIP for chances to go out on short test trips or whatever, and we also have had a number of TV special kinds of things in the last three or four years.

Henke: Would you say there are any pros or cons to having one of your instruments be so much in the public view?

Spiess: I don't think it has any drawbacks. It could happen that somehow or other [the publicity] would interfere with the research use, but I think that has not happened, that the research use and the research funding in fact seems to be more or less independent of the public feelings about this. The public is entranced by the unique business of flipping from horizontal to vertical and of sitting quietly in water in the vertical; the public is not all that interested in long range sound propagation and so—

Henke: I imagine that a lot of that research was probably classified anyway. Is that correct?

Spiess: A lot of it was, but the work for example that Rob Pinkel has done on internal waves and turbulence is just perfectly open stuff.

Henke: It seemed pretty clear, especially when you started talking about your getting involved with geologists and such, that a lot of your research involved interdisciplinary work. Would you say that that's been an important aspect of your research over the years?

Spiess: Well, any sensitive person who is at Scripps for very long becomes somewhat at least multi-disciplinary if not interdisciplinary, and I think that my brief tour as director of Scripps kind of meant that I learned more about what was going on in

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[39]British Broadcasting Corporation

[40]William A. Gaines

other parts of the institution. And very often the tools—if you bring forth something that is as versatile as FLIP or the Deep Tow system you wind up understanding that you have the capability to help support some other kinds of investigations that are different from just basic geophysics. And given that the Deep Tow thing is sort of a sea floor-oriented [device], it's natural that geology should be the first order. But biology, there's been a fair amount of biologically oriented work done with the Deep Tow system.

Henke: So do those people tend to come to you then, and say, "Hey, I've got this project I want to do"?

Spiess: By and large you don't start the project, I think. You may start with a vague idea and you talk intermittently about something, and then somebody says, "Ah yes," and, "This looks as if it might work into something interesting," and then you begin to look seriously at the capabilities of the tools and at the possibility of, in these days, generating the funding to carry out the work. So I think the sincerest form of flattery is that there are a number of other systems out there now that can do much of what the Deep Tow system can. In general we've had maybe one or two Deep Tow expeditions a year in recent times, whereas we had more than that in the seventies and early eighties.

This of course has led us to move off—not that in particular, but after a while you've sort of done all the inventing you can do, or at least all the inventing that seems fruitful, in a particular arena, and so that has led us to parlay our interest in transponder navigation gradually into sea floor geodesy, of trying to measure actual plate motion. In fact the very first time we went to a ridge crest for Roger Larson's thesis expedition—which in fact is the same one on which we recovered our Deep Tow device from the sea floor—it began to be clear that you ought to be able to devise an acoustic system that would allow you to measure the spreading across the spreading centers. And so we picked at that here and there, and finally for the last six or eight years that has been a major part of our program, doing this sea floor motion measurement. We finally indeed have a paper out showing that yes, there is convergence across the Cascadia Subduction Zone, and what direction the Juan de Fuca plate is moving relative to the North American plate.[41] And that's been a long haul.

[41]F.N. Spiess, et al, "Precise GPS/Acoustic Positioning of Seafloor Reference Points for Tectonic Studies" *Physics of the Earth and Planetary Interiors* 108 (2): 101-112 (30 June 1998).

The other thing [developed from] the fact that, in earlier times, when we were trying to take pictures of a particular place with the Deep Tow system, we'd tow around and we devised a number of operational methods. If you have a geological feature or whatever that is maybe only fifty meters across or less and you want a photograph of it, and the water is 4,000 meters deep, that takes quite a little time in general. And we've devised some operational approaches to making that feasible. But as we did that it became clear that gee, it would be a lot easier to be able to push on the bottom end of the wire than on the top end of the wire. And in fact holding onto the edge of the plotter and pushing on that doesn't do very well either. But there's a tendency to do that in lieu of being able to do anything practical, you just tend to tense up and grab hold of the plotter and wish that the thing would move over there another 10 meters or something. So we made a proposal to the National Science Foundation to build a gadget that would have the capability of pushing on the bottom end of the wire and—

Henke: Of steering [Deep Tow] from the bottom?

Spiess: NSF funded that development, and it turned into a thing in which the real goal was not very much related to the survey operations at all, but was related to the fact that this approach would allow you to place fairly heavy objects where you wanted them on the sea floor, just because the weight of the object could be handled by the wire itself and the control of where it was could be fine tuned by propulsion down at the bottom end. And that led us into the business of doing wire line re-entry in connection with the Ocean Drilling Program, where we can go out and put instruments into the holes that the Ocean Drilling Program people have made on the sea floor to do seismology and hydrogeology and things of that kind. So these things sort of built along into some new directions.

Henke: Would you say Scripps is a place that is unique in the extent to which people do these kind of interdisciplinary projects, or is it just more of a feature of oceanographic work as a whole?

Spiess: I would not say that Scripps is unique in this regard. I think that the interdisciplinary thing is part of oceanography as a whole—good oceanography anyway, or good oceanographic institutions. It's hard to do in these days, unless you can do it as part of a major national interdisciplinary program. There's been, for example, the RIDGE[42] program that has brought together investigators interested in processes and phenomena at ocean ridge crests, and has led to more expedition work that involves the biologists and the geologists and the geophysicists all at once. Although to some extent that was kind of a self-fulfilling effort, because when you have tube worms and clams and hot water circulating around and coming out in a concentrated manner—live geological processes going

[42] Ridge InterDisciplinary Global Experiments, an NSF program.

on before your eyes—why, in the present world you don't have the resources to have everybody go out there on a different ship. And so you gather yourselves and go out together. ##

INTERVIEW FOUR: 27 APRIL 1999

Henke: ## Dr. Spiess, let's begin by talking about your career as a member of the Academic Senate. I know you've had a lot of involvement with that, and I'd like to know why it is that you got interested in participating in the Academic Senate and some of the things you found most interesting working in that area.

Spiess: Sure. The University of California is unique in that it has the opportunity for a lot more participation by the faculty in how the university is run. And quite a long time ago the Regents delegated to the faculty full authority over courses and degree requirements and things of that kind. In addition, the Academic Senate has had a major role in the appointment and promotion of faculty. There is a campus-wide review of any appointment or promotion of a faculty member. And so that has put the Senate in a strong position as far as their interaction with the rest of the university's activities. This has resulted in a concept that is called shared governance, and this goes a lot farther than just the involvement with courses and appointments and promotions of faculty: on the San Diego campus it has led to involvement of faculty members in administrative committees that have to do with campus planning or with budgets, a whole host of things.

That idea of shared governance was very strong in the Scripps Institution of Oceanography just before the campus was developed here. And so the entire [research staff]—not just the ones who were anointed as true faculty members with billets as assistant professors or professors, but people who were in the corresponding non-Senate academic personnel, of which there were quite a few in Scripps in the early stages—[were included in] a series called the research series, and so you could be an assistant research oceanographer, and, as far as pay was concerned, that was the same as being an assistant professor of oceanography. The activities of the Academic Senate are pretty broad, and if you have concern about how the place is going to run, why then you need to play whatever role you can and discharge your responsibilities in that arena. And I guess within the Scripps Institution it just sort of naturally occurred that I wound up as early as 1961 or '62 as chair of the Scripps Staff Council, which was [like SIO's] Academic Senate then. At some point a little bit beyond that, there was some concern on a university-wide basis about the status of the people who had non-Senate academic appointments. There were quite a range of different types of appointments of this kind, from supervisors of physical education to librarians to the research series people. And so the university Senate decided that it needed to understand what the lives of these people were like and what was controlling their lives in ways that were parallel to the situations for faculty members. And so there was a committee formed by the university-wide Academic Senate. I don't remember exactly when. It must have been late sixties or early seventies, and I was asked to chair that university-wide Committee on the Status of non-Senate Academic Personnel.

Henke: That was the title of the committee?

Spiess: Yes. And that was a very interesting committee, because I had been unaware of the diversity of non-Senate academics, and we ferreted out quite a number. One can never say one has found them all. And made some recommendations with regard to what sorts of things might be done to make their lives better, more nearly equivalent to the privileges that go with Senate-type appointments.

Henke: How was it that they chose you for heading up that committee?

Spiess: I'm not clear, except that I had a lot of experience in the non-Senate series myself [during my first years at SIO]. I was in the research series, I guess, until I was about to be appointed acting director of Scripps, and then they decided that maybe that ought to have a professorship with it. And so I had a lot of background, and Scripps itself was well-known for having a very good environment for the non-Senate personnel. So I never really went back to see why I was asked to do that. Somebody obviously put my name up and that came out to be the thing. And that was my first major activity in the academic center.

Henke: That was something that was interesting to you when they asked?

Spiess: It was challenging, certainly. It was a job that had to be done. And it was an interesting job, so I did it. The recommendations we came up with were implemented in spotty fashion because they were more or less up to individual campuses and even subdivisions of campuses in the long run, but I think it was a useful report that other people could use as a base. So anyway, from that I became involved in more of the activities that were going on here on the campus once it was pretty well established, and [I] started being active in local Senate committees and chaired the Committee on Privilege and Tenure here, and then the university-wide Committee on Privilege and Tenure, which looks after the processes that go on if there is some kind of severe problem with a faculty member and it looks as if they need to be let go or encouraged to leave or whatever. Serving as chair of the university-wide Committee on Privilege and Tenure was kind of interesting because I could look around and see what the differences were from one campus to another, even as to where were the places that were most likely to have problems, and which were the ones in which the Senate was quite well established in a cooperative way with the administration as opposed to other campuses in which they seemed to be in a more combative mode. That's not to say our Senate was not ever combative, because Chancellor McElroy[43] left his position because the Senate voted no confidence in him. So one could say that the Senate actually had fired a chancellor here. And I did not have a major role in that, although I was probably on some committee at that time, and also there had been some brushes with him, Chancellor McElroy.

I think that the need to keep Scripps involved with the larger university enterprise was really brought home to me during the late sixties, when we had the disruptions on campus and I was director of the Marine Physical Laboratory. We were doing classified research off-campus, which was what the rules of the academic Senate had said should be the case, and the Marine Physical Laboratory fared much better than several other corresponding laboratories on other campuses. And they got into all kinds of deep trouble. The Marine Physical Laboratory did a lot better, because in fact, at least my view, we were much better integrated with the total campus—there were other people outside of our own organization who knew what we were doing and who had respect for how we were doing it and were willing to share with us whatever the problems were. And some of them had problems too, and so we had a larger community, and I think that was part of how I gradually developed a view that we really had to take a part in what was going on in the upper campus, what was going on university-wide. And this led along to eventually, I kind of worked my way up in terms of the importance of the committees that I have chaired. I chaired the Graduate Council and then was asked to run for chair of the division, which I did, and the first time I ran I didn't win, the other person did. And but a couple of years later I was asked again if I would and I said I would, and that time I was selected to be chair.

Henke: What division were you chair of?

Spiess: I'm sorry, the Academic Senate of the University of California is organized in terms of divisions. There was a San Diego division, a Los Angeles division, and so I was then chair of the San Diego division of the Senate.

Henke: I see.

Spiess: Which meant that I participated in the university-wide Academic Council. And that experience was pretty good, and so a few years later the Academic Council nominating committee asked me if I would be the chair of the Council. That's a two-year job because the way things are organized, you are selected to be the vice-chair and you serve for a year as vice-chair and then for a year as chair of the Council. And this was right at a time when—well, I had been asked a couple of

[43] William James McElroy

years earlier to do this, but I was heavily involved in research and then the directorship of the Institute of Marine Resources, and I had decided along about 1988 that I would resign as director of the Institute of Marine Resources. So having shed that then, it was just about then that I was asked if I would be the vice-chair and then chair of the Academic Council. And since I had shed one major job, why I decided it was a good time to take on the next one.
And that was a very interesting period, because during that time the vice-chair and chair serve as the two faculty representatives on the Board of Regents, so there's a chance to see how the Regents operated in full detail, because you were a regular member of the Board, except for voting—which the Academic Senate long ago had decided it did not want to be put in the position of voting on various things in the Board of Regents. You sat in on things that had to do with the salaries and finance and everything else. I think the rationale for deciding that the faculty representatives should not have a vote was partly that one of the major members of the Board of Regents is the President of the university, and if there was a real place where the Senate and the President were at odds it would be better not to push that clear around to voting in the Regents. It would be a matter that just a couple of individuals would have to look at their own consciences about, but would look as if it was the entire faculty voting that way. And so that was a fairly reasonable thing to do.

Anyway, it was an interesting time. We [went] through the business of renewing the contracts between the university and the federal government for the operation of the weapons laboratories, and that had its own major pushes and pulls within the faculty, obviously. Once my term was over it turned out to be just about the same time that I went to emeritus status because there was still a rule that said that at a certain age you had to retire. And so I retired, at least in [pay] status, and went on—

Henke: What year was that?

Spiess: That was 1990. And I, for some time, did not do anything more in the Academic Senate. I sort of retired from that. And then a couple years ago I began to be involved in some more of the local planning committees and then I was asked to take on the job of being the chair of the Academic Senate university-wide task force for the establishment of the new campus at Merced, and I took that on just last December. So that is where I'm putting my Academic Senate emphasis at this time. And it's a real interesting job.

Sort of going back to—what would it be?—around 1980?

Henke: 1980, I believe.

Spiess: I had been director of the Marine Physical Laboratory for twenty-two years by then, and while it was still challenging, it was becoming a little bit too routine and it seemed to me that I was going to have to retire. I had seen some other laboratories like the Marine Physical Laboratory in which the director just hung on right on into retirement and left behind a kind of a wreck because he didn't want to give up the reins of power or whatever. Power is kind of a strange word, because there is a lot of consensus.

Henke: Didn't train a replacement or something like that, you mean?

Spiess: Well, in what condition do you leave the lab? And it's probably better to do that before you are forced to retire, because those two things are not always compatible. So I had decided a little before 1980 that I would like to move out of that. It was going to be a little awkward, having been twenty-two years in the driver's seat—although mostly the director of the Marine Physical Laboratory had to be sure he was staying out of the way and seeing that people didn't have any excuses for not getting their work done, rather than telling them what to do. But in that era there had been established, quite a bit before that I guess, the Institute of Marine Resources,[44] which was a university-wide institute that had different flavors depending on its different directors. The original director[45] was a retired Navy admiral, who was a very thoughtful person, and he was more concerned with the engineering things that might go on, on a university-wide basis, interactions among different groups. One of the next directors was a fisheries person, Benny Schaefer,[46] and he was obviously more interested in the marine biological resources and what those meant and how to work with them.

And then, after a bit, John Isaacs became the director of that group, and he was a very imaginative, broad-gauged person who had all kinds of things that were of interest to him, including fisheries. During his administration the nationwide Sea Grant college program was established. And originally the Scripps Institution had an element of this Sea Grant college thing, but then that expanded to be university-wide, which still exists, and the Institute of Marine Resources became the home for the Sea Grant program. So that was one part of it. A lot of it had to do with Isaac's own personal interests, which were varied enough that one could hardly classify him as being a particular kind of oceanographer. In any event, he had decided to resign about 1978, I guess. His health was not too good, and he also felt that he should get out of there before he had to go out for some other reason. And so there was a search going on that lasted for a couple of years for a new director for the Institute of Marine Resources. And during that time Isaacs kept the

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[44] The Institute of Marine and Resources was established in 1954. For more on its history see chapter 6 of: Elizabeth Noble Shor, *Scripps Institution of Oceanography: Probing the Oceans, 1936-1976* (San Diego: Tofua Press, 1978).

[45]The first director of IMR was Charles Wheelock.

[46]Milner Bailey Schaefer

job.

Meanwhile, I had been asked by the University of California at Santa Barbara to be one of three people to review their marine science program, which was kind of embryonic. Their new chancellor wanted to beef up their marine science program, and so there were three of us: one of them a chemist from Woods Hole whose name slips away from me for a moment; a senior administrator who was the dean or maybe vice president by then; and John Knauss from the University of Rhode Island, who was a Scripps graduate of long ago, but had run the program at Rhode Island. Those two and myself sat in and talked to a variety of people at UC Santa Barbara. And I was really taken aback by the view that the Santa Barbara people in marine science had of Scripps. They thought of [SIO] as a sort of Big Brother who was going to chop you down if you moved too tall. It was a very defensive attitude, and it seemed to me that this was not really very good for the University of California. And so there was another round of recruiting going on for the director of the Institute of Marine Resources, IMR is what I will call it here, and I decided that I didn't know anything about marine resources, but I did know something about the university on a broad basis. And I grew up at Berkeley, I have a great loyalty to the University of California, so I decided I would say, "Look, I'd like to be the director of IMR," in order to try to pull together the elements of marine science and research on the various campuses. And after a little thought, why the university administration up at Berkeley in the Office of the President decided yes, that I should take on that job. And so in 1980 I did.

Henke: Because they liked your idea for taking it on, do you think?

Spiess: Partly that. Partly they needed a director. They had other candidates, so they did have the opportunity of selecting, and in making the selection they were clearly looking in a particular direction, as far as what the options that were on the table in the persons of the various candidates. And so it looked as if that would be a direction that they would like, that the university administration thought was a good idea.

And so I took it over, and I did several things to pull the university together. I managed to have a council established that was made up of a senior representative from every campus, whether they had a major marine program or not. So we had a forum in which we could gather every couple of months and talk about what was going on. For the only time that I know of in marine science activities, we put together a university-wide, hour-long presentation for the Regents to tell them what was going on in some balanced way across the entire [marine science program]. One of the most powerful committees at Scripps is the Marine Operations Committee. It really has a strong hand in how the ships are operated and what new equipment is requested from various supporting agencies. And so I arranged that the two major players outside of Scripps, which were Santa Barbara and Santa Cruz by then, would be represented on that committee on a regular basis. They don't always come to the meetings, but they do indeed have the agendas and the minutes and see what's going on and have their own opportunity to make an input. And I had several university-funded research assistantships. Those are fairly rare things to have. And I experimented with those in a couple of different ways, but the most satisfactory way that I found to use those was to spread the word to all of the marine programs in the university, and the Sea Grant mechanism was a good way to have those contacts because that was a statewide organization. In any event, if there were graduate students who wanted to spend a quarter or a year on some other campus in order to have interaction with a different marine science group, I would be willing to fund those people out of these university-funded [assistantships]. Because, if you're working toward your Ph.D., as you can well understand, you do, certainly in the marine science world here, you become quite dependent on the person in the faculty who is raising the money that is paying your salary, and that person very often is deriving those funds from outside grants that reflect that person's own area of expertise. So if you want to go off somewhere else for a year your sponsor is not likely to have the resources to do that even if he or she appreciates the nature of what you can achieve.

Those are all things that I pushed in the IMR context, and I stayed with IMR until 1988, a point I picked because I was coming toward mandatory retirement, and also I could kind of see that this university-wide [Academic Council] thing was going to come. And in addition I timed it so that it would be just about thirty years from the time I had taken on the directorship of the Marine Physical Laboratory, which was my first administrative slot. So I said I would retire from the directorship in 1988, and the university administration didn't—I don't think they took me seriously. They said, "Sure, we'll find a director." And I decided I was not going to do what Isaacs had done, which was to hang in there for several years while they fiddled around and finally found a new director. So I decided that I really meant it. The one thing I didn't give up was this office, which is where I established the business office for IMR. And so when the date came that I had specified to the university that I was not going to be the director of IMR anymore, I said, "Goodbye. I'm not the director." And they had started a search in a half-hearted way. Bill Fenical—who had his billet in IMR in that time frame, a marine chemist here at Scripps who has been a big worker in the pharmaceuticals-from-the-sea field—I felt that he would be a good person to do this, and so he was named as the acting director of IMR while they went to search for a new director. And things sort of trickled along for some little time, and eventually the various groups on the different campuses more or less decided that they didn't need IMR. And there were a bunch of IMR billets here in La Jolla [as well as] some up at Davis, because of food processing and things of that kind that were related to marine resources. And so the billets were handed off to the individual campuses, rather than any longer belonging to the university-wide institute. Because I had in fact moved some of those billets around from one campus to another.

Henke: Why did they decide they didn't need IMR anymore?

Spiess: Well, because they were interested in their own institutions' activities and had no particular desire to help out the other ones, and so without somebody in the university-wide institute who was going to watch over the question of some university-wide coordination—and there was a lot of coordination that we generated, because I was able to get some of the Sea Grant money to do inventories of what all the courses were in different places, so that people on one campus could know what the facilities were and the courses were on [other] campuses. There were turnovers in directors at different places, and they decided that they would rather have control over billets themselves than have somebody else have them. And so IMR sort of still exists I think on the university's books, [because] it was never disestablished. One of the things about the university is to establish a university-wide institute, or even a campus interdepartmental institute or something, there is a great deal of review and process that goes on. The end result is that when you get to the place where a unit is no longer really being supported or being useful or being thought of as useful, then you're probably better just to let it kind of quietly die than to actually formally disestablish it. If at some future time somebody wants to have a group of this kind, then you still have to go through a fair number of hurdles, but not as many as if you were starting from scratch. So IMR I think is still on the university's books.

Henke: I assume you were somewhat disappointed that it faded away after you lost your directorship.

Spiess: Yes, I was. I was disappointed. But I was busy doing some other things then, because I had moved into the Academic Senate Council chair slot, and that was using up all the time I was willing to put into that.

So I guess we were talking a little bit about it before, about the patterns of support in oceanography and how funding worked. If you go back to the very early times—in the fifties, oceanography, all of science I think, was expanding in terms of federal support. Particularly within the framework of the Navy, the funding was very much on a block group kind of basis. The Marine Physical Laboratory was funded sort of as a unit. A few people might generate a little bit of extra money from someplace other than the Navy. But gradually as the National Science Foundation grew, the concept of the individual investigator going after his or her own money became a major driving factor. And that is both good and bad. The good part of that is that if you have good ideas and are capable, you can go ahead and put in your proposal and it will be reviewed by your peers and—

Henke: Make your own way?

Spiess: You make your way. In the marine science world NSF realized after a while that there were some aspects, however, of oceanography that you couldn't handle this way. For awhile in NSF, when you put in your proposal you put in for so many days of ship time on such-and-such ship, and the ship was operated not by you as an individual investigator; it was operated by some institution like Scripps or Woods Hole or whatever. And so there was a period when you might have the money for *n* days of ship operations in your grant, and the operating institution didn't have any control of that. Ships cost a fair amount of money. In today's world you pay $10,000 to $15,000 a day for a reasonable ship. If you get a really good ship you'll pay close to $20,000 a day. Well, if you're an individual investigator and you have this big lump of money for ship operations you might well decide that you'd rather go to sea for two less days and fund a graduate student.

Henke: Two graduate students.

Spiess: It was not clear how the overall system could operate, because the Navy had been more or less block-funding the ship operations. And so NSF then decided that when you put in a request for research support in your proposal you could say how many days of ship time you wanted, but you couldn't convert that money over into something else.

Henke: So that would go to the institution.

Spiess: And it would go to the institution. And in fact the institutions then became more or less block-funded both by NSF and by the Navy for ship operations, and the marketplace was felt in the sense that, if there were lots of requests for one ship, NSF would then be sure that that amount of money went into the appropriate institution. The thing that has happened more recently—well, I guess in parallel with the NSF taking a bigger and bigger role—the Office of Naval Research funding moved from being laboratory- or institution-oriented to being oriented toward individual investigators. At one time Scripps had an ONR contract in which there was a description of a whole bunch of different things that might be done but in which there was a total amount of money, and then the director's office parceled that money out to the various investigators. Marine Physical Laboratory's funding operated in the same way. In Scripps and in other places there was occasionally a situation where an investigator in the institution felt that he or she was not being given the fair share of the money, and so they would go around to the program officers in ONR and complain. And after a while the program officers decided that there would still be an ONR contract with Scripps, but that it would really be just a pasting together of a bunch of individual proposals.

Henke: What time was this change happening?

Spiess: That probably was around 1970. You don't want to take these dates too seriously, because, since I was running the Marine Physical Laboratory and we didn't have this problem, it came upon me kind of gradually that this was happening. And gradually it [changed] from putting together a set of tasks more or less with dollars noted for each investigator. But then the administrators in ONR began to say, "Yes, but we don't like that one over there." And so it became a thing that was much more parallel [to] NSF, in which you dealt with your program officer in geology or in physical oceanography or whatever it might be, and there was not a peer review system. The [ONR] program managers decided how the money was going to be distributed, because they were more of a mission-oriented agency and they had to justify how they were spending their money to other people in the Navy in order to justify their budget. So the situation really changed quite a bit, and the complexity of things changed, and gradually what has happened is that in more recent times funding has become somewhat tighter. For one thing, with the end of the Cold War the Navy has reevaluated how it was spending its research money and decided that they didn't want to spend very much money on deep ocean kinds of things. Well that was a big shock because the Navy had been a strong funder not only of deep ocean research, but of funding ideas for gadgetry, new instruments, new devices that might be useful in that framework. And so when they pulled out of that, or at least drastically reduced what they were doing in deep ocean research, suddenly there was a kind of influx of more physical oceanographers, marine geologists, whatever, writing proposals to NSF.

Henke: This is in the early nineties?

Spiess: Yes. Well, the Cold War ended when? Sort of '93, '94, '92? Someplace in there. And it took the Navy a year or two kind of to reorient its thinking into a shallow water context. This [combined] with the fact that we've [reproduced] a lot of ourselves to go out and be oceanographers. I guess I was impressed by that thinking back, and you tend to think of generations of people. You have some students, and those students have students, and the student's students have students, and the length of a generation is really not all that long. And I know that I was pleased when a couple of my students jointly at MIT/Woods Hole had a really promising good student that they produced. And that student has been for some time a full professor at Santa Barbara, producing students also. And so there are people out there who are three or four generations down from me, and there are a lot more people out there than there used to be. ##

Henke: ## As far as these pretty large changes that you've been talking about and how, oceanographic projects get funded, I just wonder what your feelings are. I mean you did talk some about the good and the bads, but I wonder if you can say what some of the consequences of that are. And, say if you were made king for a day, would you change anything back to how it was before?

Spiess: There are some things that don't work well in the individual peer-review system. NSF addressed this for awhile in a program called the International Decade of Oceanographic Exploration. [They funded some] expeditions that in an earlier time in the fifties were put together by a group of people deciding that there was a string of things that would be interesting to do and the institutions had a fair amount of control so that the ship could go out and do a set of expedition legs that might take it around the world or keep it at sea for anywhere from six months to a year, but doing something useful all the time. Once the individual project system became the driving factor, it became more difficult for people to put together expeditions that would have some coherence even in a geographic sense, because someone from one institution might be funded to go out on a Scripps ship. That's another thing that happened I guess that I haven't mentioned, which is a good thing I think, that increasingly the ships were thought of as a cluster of facilities—not as something that belonged to one institution or another institution. The operating responsibility and the fundraising responsibilities remain within the institution, but you may be at the University of Kansas and if you come up with a good oceanographic experiment that needs some ship time you can put in for ship time on a Scripps ship or a Woods Hole ship or whatever it may be. And if your grant is funded, then you go out and you become the chief scientist on a ship operation. That led to a continuing problem of what are called deadhead legs, which you have [when] a ship goes out, there are several different legs that it's going to do that belong to one investigator or another, but they aren't always joined together geographically as well as they could be. And the NSF and ONR have not figured out how to short circuit the long time frame that's involved in putting together a normal NSF proposal that includes ship operations. For that there should be some kind of a short fuse sort of thing where people could make a proposal that says—or NSF could say we have three deadhead legs in this expedition, we're not going to look for the best science, we'd like to look for the best science, on a relative basis, that anybody could propose to do with these. And that's something we've talked about that hasn't worked too well.

The IDOE program, while it was great for putting together big programs, was a program that was almost designed to destroy institutional loyalty, because one of the criteria for that was that it had to be an inter-institutional proposal. You couldn't make a proposal with three people down the hall. You had to have three people at Lamont or Woods Hole or University of Washington, or wherever it was. And in addition the IDOE was billed as something that was a—decade was the word in there for D—and at the end of the decade NSF in its wisdom decided that they didn't want to play the game that way anymore, that they would do away with the part of their operation that was devoted to large scale programs. But at the same time, people began to gin up large-scale programs anyway, and so NSF gradually drifted back into that. The first of those was WOCE, the World Ocean Climate Experiment. And that was again a multi-institutional thing that was put together, and with a lot of trappings of workshops and planning that went on for a long time before they ever got out to sea.

Henke: I'd like to know what you think is the legacy of your research. What would you like to see?

Spiess: Of all of this time? As far as the research part is concerned, I think the real legacy is in the students that have come out of this, that a lot of them have gone off and done good things. And to some extent there is also a legacy of having broken new ground in the instrumentation world. FLIP is still out there being used. The Deep Tow thing has, to me, an even more impressive result—namely that there are quite a number of Deep Tows out there now that other people have built. For the first decade or so of [Deep Tow research] we had a lot of good students, a lot of whom wanted to go out and do the same thing, or use the same tools to do new things. Particularly the work at the spreading centers in the ocean—the ridges, rises, whatever name one likes—that's particularly amenable to towing instruments down near the bottom and looking at what's going on, because there's very little sediment covering up the rocks down there, and you can deduce quite a bit from that from close observation. But I think that that was something sooner or later somebody would have done it, but we were the ones that did. My group has been the first one to be able to have some capability to put instruments down at well defined places on the sea floor—particularly putting things into drill holes that there are out there that the Ocean Drilling Program has created, and we are still the one group that can do that without using some submersible or a big drilling ship. We can do that from an ordinary research ship. And that is another thing that I'm sure is going to proliferate. I know the Japanese are in the process of building something of this same kind. And in fact we had a Japanese scientist engineer over here for a year working with us so that they could learn how we do it.

The other thing that we've been pushing that I think has been a very long kind of thing is sea floor geodesy, the business of being able to measure the small displacements on the sea floor that go with plate tectonics or with the building of volcanoes. And that's something that we have pioneered and is gradually spreading. There is a group at Oregon State doing this now, and a couple of Japanese groups.

Henke: So it sounds like from what you were saying just a minute ago that in a lot of ways your research has changed over the years from really focusing on acoustic problems to being more interested in, say, sea floor spreading and geological issues. In a lot of respects that may be kind of fortuitous because, with the end of the Cold War, perhaps research on acoustics, stuff like that, is not as crucial. You said yourself that the NSF is kind of downplaying some of that stuff.

Spiess: Right.

Henke: Is that a good tactical move on your part?

Spiess: It was not a tactical move as I saw it. It was simply that there were challenges in underwater acoustics and applied Navy kinds of things in the early fifties and the mid-fifties and on into the sixties. But that's another way in which things have changed, [because] the Navy laboratories have taken on a lot more of the kind of work that we used to do, and in fact private industrial or—I'm groping for the right word—the consulting companies have sprung up all over the place and for-profit organizations are out there doing a lot of what we did as a nonprofit thing in the fifties. And so there has been more of a squeeze back into just the basic research part of the game. I think that in the seminar talk that I gave a few weeks ago I guess I was harking back to the fact that the kinds of things I like to do are things that have both some fundamental significance in the science world and also some fairly obvious applicability to the world in a useful manner. And the changes that have taken place overall have been such that the applied part has to a much greater extent been moved over into for-profit organizations. So it's harder to put together the kind of experiment that I really admire, where you can go out and be gathering data with some kind of new piece of machinery and the output will be interesting, not just to your fellow scientists but to people farther out.

Henke: So when you say applied you mean that the commercial enterprises are doing more of the machinery, i.e. instrument building, stuff like that?

Spiess: No. Well, they are, but beyond that they are also in fact doing the sound propagation experiments and whatever else. In fact the Navy laboratories, some of them have become almost completely contract administration places because they don't have the capability to carry out the work themselves. They hire people from SAIC[47] or wherever else to go out and do the work. And that's the kind of activity that used to be part of the university activity. [This was] a legacy really from the World War II involvement of many university people in the development of radar, sonar, whatever else, because those people were the leaders in the World War II regime, [and] were nearly all drawn from the university community one way or another.

[47]Science Applications International Corporation, a research and engineering corporation with headquarters in San Diego.

These [kinds of research problems] are not necessarily long-term kinds of things in themselves, but they push over some barrier that has been there. And I guess that's been a thing that I've enjoyed. In addition just to going out and considering the ocean as your competitor or whatever, how can you get some data out of it—that it may not be all that kind about yielding up. And at the same time it's a place of wonder in which to work. You get to see the green flash and the occasional wonderful sunrises, and so the ocean is not just challenging—[although] it is, and if you allow it to be challenging you can find the challenges there—but it's also an environment in which one can work in ways that are interesting and wonderful.

[When I've reviewed the] transcripts of what we've done so far, one thing that I realize is that it sounds as if it's all good, hard science and good hard University of California loyalty and that sort of thing. The fact [is] that there were moments of relaxation and social fun along the way. Graduate students would bring their banjos and guitars out to sea and we'd sit around and sing, or if there was a committee meeting in town why we would round members up along with some local people and have them up to the house and have some—well, in other days, more martinis than today. Today it's more wine drinkers. Interspersed with all [the work] there were moments that were really a lot of fun in the sense that normal people outside of the science community would think of as fun. Enjoyment of family or wife or whatever was all part of this, and I guess if you divide this into the administrative part, including the Academic Senate stuff, and the science part, there is a third part, which is the people part. Interactions with the people in the lab were not confined just to figuring out how to build the next gadget. They included how was your family doing and gathering together and letting spouses know what's going on. And so there's a real social aspect to all of this as well, that really is almost as big as these other things.

Henke: And has links to it I assume too?

Spiess: Yes, in that it makes it a really great life to be able to interweave all these things together.

TAPE GUIDE