Oral History of

Robert L. Fisher

Interview conducted by Laura Harkewicz

17 January 2007

TABLE OF CONTENTS

ABSTRACT:

Robert Lloyd Fisher was interviewed in his office in Ritter Hall on the campus of the Scripps Institution of Oceanography on January 17, 2007. Fisher was born in Alhambra, California on August 19, 1925. He received his B.S. from the California Institute of Technology in 1949 and both his M.S. in marine geology in 1952 and his Ph.D. in oceanography in 1957 from the University of California, Los Angeles/Scripps Institution of Oceanography. His dissertation topic was *Geomorphic and seismic-refraction studies in the Middle America Trench, 1952 – 1956.*

Fisher served as ship scheduler at Scripps from 1964-1968 and SIO associate director, Ship Operations and Marine Technical Support, from 1974-1980. He planned and led many complex deep-sea expeditions at Scripps and in cooperation with other academic institutions and international agencies throughout the Pacific, Indian, and western Arctic Oceans. He directed Scripps programs for the International Indian Ocean Expedition (IIOE) from 1960-1965, and he chaired the U.S. IIOE Geology-Geophysics Committee and co-chaired its multinational geology-geophysics counterpart.

Fisher served from 1976-1993 on the guiding committee for the IOC-IHO General Bathymetric Chart of the Oceans (GEBCO) in Monaco and chaired its Subcommittee on Undersea Feature Names from 1982-2003. He was elected to fellowship in 1988 and in 2002 named an honorary member of the Explorer's Club, joining the likes of John Glenn and Chuck Yeager. In 1999 he was made an honorary Foreign Fellow of the British Challenger Society for Marine Sciences. In 2004, he was awarded the Drake Medal by GEBCO. The award, its medallion a replica of the medal given to Sir Francis Drake in 1589 by England's Queen Elizabeth I, was specifically designed for Fisher to honor his major contributions to the scientific knowledge of seafloor trenches as well as the crustal structure, composition, and topography underlying the Pacific and Indian Oceans.

The interview stressed Fisher's half-century of work as a Scripps research geologist, including his role as initiator, planner and chief scientist on many expeditions throughout the world. We also discussed subjects as diverse as the philosophy of going to sea and the individuality involved in bathymetric charting, to the naming of underwater features and his views on how changes in society and technology impacted field research at Scripps since 1950.

INTERVIEW HISTORY: The interview took place on a winter morning in the office of Robert L. Fisher in Ritter Hall on the campus of Scripps Institution of Oceanography. Fisher's office walls are papered with a variety of bathymetric charts including a large one constructed through the decades beginning from the IIOE, the first major scientific cruise to chart the Indian Ocean. We talked for a little over two hours with a brief pause when Fisher asked for clarification about one of his responses to interview questions. The pause has been edited from the transcript.

Laura Harkewicz
Oral Historian, Scripps Institution of Oceanography/UCSD
June 26, 2007

Robert L. Fisher plotting track on R/V Horizon, 1959.
Scripps Institution of Oceanography Archives, UC San Diego.

INTERVIEW WITH ROBERT L. FISHER: 17 JANUARY 2007

Harkewicz: This is Laura Harkewicz. It is January 17, 2007, and I am in Ritter Hall with Dr. Robert L. Fisher. Good morning, Dr. Fisher.

Fisher: Good morning, Laura. May I call you Laura?

Harkewicz: Yes. Please do. We were briefly discussing where we wanted to start. So, my first question is always, how did you get involved in oceanography? And you can pick that up however you want. You can start at your birth, if you like.

Fisher: That's an interesting question. It's a bit circuitous but it'll turn out, I think, to be rather obvious. I mean, the flip answer was that as a boy I was a stamp collector of worldwide stamps, and that, of course, gave me an interest in travel. But that's not the answer you need. As a Southern Californian, the schools that I attended, and my father, encouraged me very much to discuss and to look at scientific-type things. I grew up near Pasadena, and Pasadena was a very good place to do that. In the spring of 1943 I passed the examinations and entered Caltech, more or less as it had been expected, I think, from my teachers over the years. I entered, as I say, in the middle of 1943 and then in May of 1944 I went into the United States Navy. I was in the Navy for two and a half years as an electronics technician specialist, which was something that a number of the people with technical training were made to do when they enlisted in the Navy. You'll still find a number of those people in the scientific fields. I came out of the Navy in 1946, a year or so after the close of the war, having been ashore near Manila in the Philippines for about a year, and utterly fallen in love with the Philippines and that part of the world. A very, very lovely place, lovely people. But anyway, I went back to Caltech in geology. And in the spring of 1949, I graduated and it was, at that time, the largest graduating class Caltech had ever had because it had swept up the people who had gone into service and brought them in. I think our graduating class numbered 269. That's a side issue. But anyway, at the close of that time I took the Civil Service examination and entered the U.S. Geological Survey as a professional employee so that I could participate in a field program in the summer of 1949. And incidentally, I had been married in the summer of 1948. So my senior year I was a married man living off campus at Caltech.

I entered this USGS project as a Caltech graduate aiming on getting his doctorate. The project was the examination of the geology of St. Lawrence Island, which is a large island just south of the Bering Strait, and six of us from Caltech became field geologists working on that island, and living with the Eskimos in groups of two. That was adventurous, and we did that for four or five months. And then, coming back in the fall of 1949, I entered Northwestern University in Evanston. The reason that I entered Northwestern was that, when I had been at Caltech—in my last year or so—I'd become interested in statistical sedimentation, which is an

odd business, but maybe not. Later on you'll see how it fits in. But anyway, the world authority at that time, at least in the English-speaking world, was a man named William C. Krumbein,[1] and he was a senior professor at Northwestern. I was accepted there as a graduate student and came back in the late fall of '49. I spent almost a year, a very good year, with my young wife in the Chicago/ Evanston area. One morning in the spring of 1950—and incidentally, while I was at Northwestern, I was, from the beginning of that year, a teaching assistant in the first-year geology class under a man named Robert Garrels,[2] who ultimately came to Scripps for a short period.

But in the spring of 1950 I happened to be in the library there and looking through the *Journal of Geology* and there was a short article by a man named Walter Munk and, I believe, an Air Force short-termer here at Scripps named Melvin Traylor. The paper by Munk and Traylor[3] was on wave refraction in La Jolla Bay. And I became quite interested in that and, as I say, with the sedimentation idea. So I went to Krumbein just to talk about that sort of thing and it turned out that he had been at Scripps during the war for a short period. I don't know how many months. And he knew several people at Scripps. One of them was Francis Shepard.[4] Another he knew was a man named Roger Revelle,[5] and that's the first time I heard of Roger. And he said, "You know, Bob, when you finish here and get your doctorate you might want to think about going out there and doing the sort of things that you're talking about doing." And I thought that was a nice idea, and I went home and talked it over with my young wife, who was also a Californian. A day or so later I wrote a letter to Fran Shepard, the name that Krumbein had given me, just generally a question about Scripps and what was being done and what sort of program they had. And, to our surprise, by almost return mail, I got an invitation from Fran Shepard to come out here as a student, a first-year student at Scripps to study under him. I talked it over with Shirley, and a day or so later, why, we made the decision, and I went in and told Krumbein and he encouraged me to go ahead and do it. And so, in the early summer of 1950 we drove across the country and came out here to Scripps, and found housing up on the top of the hill, right about where Urey Hall is now, a place called "Torrey Pines Homes," which was the former military family housing for Camp Callan. That is basically how I happened to come, from my point of view. Now, in a few moments maybe I'll give you a slightly different point of view from the other side. But anyway, does that answer your question?

---

[1] William Christian Krumbein (1902-1979).
[2] Robert Minard Garrels (1916-1988).
[3] Walter Heinrich Munk (1917-) and Melvin Alvah Traylor, Jr. (1915-), "Refraction of ocean waves, a process linking underwater topography to beach erosion." *Journal of Geology* 55 (1): 1-26 (Jan. 1947).
[4] Francis Parker Shepard (1897—1985).
[5] Roger Randall Dougan Revelle (1909-1991).

Harkewicz: That answers the question. I'm a little curious about what kind of things you and your wife talked about then? How did you come to that decision, a pretty big decision made so quickly?

Fisher: Well, the reason I came to that decision was because, as a Californian, I had come down to San Diego, down to Tijuana and we'd come through La Jolla, and that was nice. It wasn't a case of homesickness for California. We'd enjoyed the Midwest very much. But anyway, we talked about this and the people at Northwestern, the graduate students there studying under Krumbein and the others; they were oil company geologists and that type of thing. Oh, there were some petrologists working with Arthur Howland,[6] the petrographer there, and with Jack Stark,[7] who had worked in the Survey's military geology group during World War II in the South Pacific islands. [Stark] and I had talked about that. I remembered my experience up on St. Lawrence doing fieldwork in a remote area, and that had been very interesting and I had wanted to write that material up. At this time, I was still a "WAE" (When Actually Employed) employee of the USGS. I didn't know whether I wanted to become a professional USGS employee, but I knew I didn't want to be, as I've said, in the third subbasement of some oil company laboratory in Pittsburgh, Pennsylvania, [*laugh*] only seeing the sun occasionally. And so, I think somewhere there was some inspired impulse to go adventuring, or do fieldwork, and that's probably how I decided it. Harkening way back, it may have been my childhood stamp collecting. All of that more or less comes together. It sounds trivial, but I think it may have had something to do with it.

But anyway, it turned out that we came to Scripps. That was the first week of the Korean War, and so I came here and became a member of Shepard's group, informally, at that time. And there were two or three or four people from UCLA, and five or six others. Their names may or may not come up later. Bob Norris,[8] whose nephew is now one of our professors in the department, was here at that time. [Bob Norris], David Poole[9] and I, we three, and a man named Bill Butcher,[10] worked quite closely together. That was 1950. That was about three weeks before MIDPAC Expedition sailed. I was invited to go on MIDPAC, but after the work on St. Lawrence that previous year I thought that wasn't a good idea. And, I think it was a good decision not to go…

Harkewicz: So, you were talking about coming to Scripps. And, I just want to clarify one thing though before we do go on. I know that there was a time period where people actually received their degrees from UCLA, even though they were getting schooled at Scripps. Were you in that situation then?

---

[6] Arthur Lloyd Howland (1908-1978).
[7] John Thomas Stark (1888-1984).
[8] Robert Matheson Norris (1921-). His nephew Richard D. Norris is professor of paleobiology at SIO.
[9] David Melvin Poole (1921-).
[10] William Sherman Butcher (1922-).

Fisher: Yes. That's exactly true. My thesis committee, for example—I think there were seven members—two had to be from UCLA because you needed people from two departments. And so, two people from the UCLA Geology Department, U.S. Grant IV[11] and Cordell Durrell,[12] the petrologist, were on my thesis committee. I never met them except during the two days when I did thesis defense-type things, in my qualifying and my thesis defense. They were nice gentlemen. I've never spent a day on the UCLA campus but my degree is UCLA-SIO—my doctorate in '57. [UCLA] also, incidentally, gave me a masters that I didn't even apply for on the way there.

Harkewicz: I just wanted to clarify that. So, you had mentioned that you were a student of Francis Shepard and we were talking about that a little bit. What was he like as your main advisor?

Fisher: Okay. Fran Shepard, a very gentlemanly man with a lovely lady[13] who was sort of the mother of the group in those years. This was 1950-51. I had been here only three or four weeks when I realized that Fran Shepard did not epitomize what I had come to think rigorous science was in a field. That sounds very unfortunate. Fran Shepard was really a naturalist. He was excellent at what he did, at making observations. It turned out that the real reason he had invited me so precipitately was that [his group was] at a point with the sediment trap studies where they needed somebody that they thought could handle the equations and he thought that with my background I could do that and also do the field work. I didn't know that, really, until I got here. But when I got here I was more or less told in my first few months, well, my thesis would be "the sediment trap," to study the movement of sand because I had been interested in statistical sedimentation. And you had people here, for example, Doug Inman[14] was a young professor at that time working in that, but they needed somebody else to do this sort of thing. So the assumption was that I would come and do it. And in order to do that during the years 1951-52, we went out with the sediment trap and made observations in La Jolla Bay. To properly do that, starting in 1951, we became divers, scuba divers. At that time scuba diving was very new. There weren't wet suits and all of that. But, this was an application of scuba diving that was very obvious. So we would go out in various weather. For example, when the waves were big we'd take out the sediment trap and put it in different depths for different periods through diving. Connie Limbaugh[15] was SIO's head diver then. We were diving in teams and most of my dives, such as they were, were done with a Texan named Richard Mills,[16] who was a natural diver. Some of our people are natural divers. I wasn't

[11] Ulysses Simpson Grant IV (1893–1977).
[12] Cordell Durrell (1908-1986).
[13] Elizabeth Buchner Shepard.
[14] Douglas Lamar Inman (1920-).
[15] Conrad Limbaugh (1924-1960).
[16] Richard Alvin Mills (1928-).

really one of those. And we never had to go super deep. The deepest that I ever went down was about 125 feet. And people say, "Well, that's not very much. People go down 300 feet." But, when you're sitting on the bottom of La Jolla Bay with a slate, taking notes, watching stuff move in the sediment trap, and you think there's a twelve and a half story building sitting right in front of you, all water [*laugh*] why, you begin to think; also, it was cold, cold, cold in those days. So I got to thinking about it and I realized that that wasn't any better than being in the subbasement in Pittsburgh [*laugh*] in terms of what I was doing.

Harkewicz: Good point.

Fisher: But, the real point was that, as Walter Munk would have been able to advise Shepard, "If you have fourteen unknowns and thirteen equations, you're never going to be able to work out the parameters." In other words the sediment trap was not really a winner in terms with my group.

But meanwhile, during that time, Scripps was much more social from top to bottom. In a sense, I think, people in the organization such as Roger Revelle and, his children, his wife[17] and others. We all saw a lot of each other and we all became friends. And Revelle, more or less, took an interest in me. Apparently he saw in me something that he liked in terms of what should be encouraged. And, at that time, Roger was in the transition of establishing himself as director, 1950-51. So, he had picked people here, or he knew people here, or he'd grown up with people here. I happened to be one of the people, along with Warren Wooster[18] and John Knauss[19] and one or two others that he was really turning out to encourage, for one reason or another. And that's how that got started. At the same time, I became interested in going to sea. I had an opportunity early in 1952 to go out on *PCE(R)*-857, the NEL[20] ship, to an expedition off Central America. I had previously been out on shorter cruises, down to Guadalupe Island in '51, on *E W. Scripps*. I realized I enjoyed doing that [shipboard] fieldwork. And my then wife, Shirley Fisher, whom you'll find mentioned in the 1986 book by Menard, had gone to work at NEL. We had no children, so she was down there pretty well set up, talking to Bill Menard,[21] Ed Hamilton, [22] Bob Dietz,[23] and they were all a very interested group and they got along very well. So, I could go to sea for a while at a time. I went off on the NEL ship with Ed Hamilton, and a man named Maxwell Silverman,[24] who had come to Scripps not as a graduate student but as an engineer. And Max Silverman became my right hand on my own expeditions in '52, '53, '54, '56, and later. He was the "Number One," in other words, the

[17] Ellen Virginia Clark Revelle (1910-).
[18] Warren Scriver Wooster (1921-).
[19] John Atkinson Knauss (1925-).
[20] Naval Electronics Laboratory.
[21] Henry William Menard (1920-1986).
[22] Edwin Lee Hamilton (1914-).
[23] Robert Sinclair Dietz (1914-1995).
[24] Maxwell Silverman (1926-1974).

party's executive officer who really made it possible for us to do that sort of work. He was a major figure at that time. You'll find his name coming in Scripps history later as one of the designers of *Melville.* But then, he was an engineer often working with me.

Harkewicz: So, just so we can clarify here, Francis Shepard brought you here to do something on sediment traps, but you ended up doing your thesis on "Geomorphic and Seismic Refraction Studies in the Middle America Trench." And, you said that Roger Revelle, because of the community of Scripps, encouraged you to expand beyond Shepard's horizons for you, or something to that effect. But, let me just ask you one question, then we can go on to your ocean voyages: how did Shepard feel about you going off on this other tangent? Did that affect his plans for his own work or his plans for you? What happened between you two?

Fisher: Well, we were socially very gracious to each other. I don't know that there was an effort. He had his group. At that time he had a young lady assistant name Ruth Young who sort of bridged the gap for all of this. It's just that Shepard had one or two students such as I was who, if we weren't there, he wouldn't really know it. If one was doing what [Shepard] was really interested in, he was really interested in them. And so, he didn't really realize—had no problem with my doing these other things. I don't think he was really aware of it, except later on when I'd come back from a long expedition, which had been really something to plan, and I might spend a little while telling him about it, and he was politely interested. And this was my thesis supervisor! Now, one of the people who's been here many years (came several years after me and was one of my Caltech '49 classmates), Joe Curray,[25] came beautifully set up to do the sort of thing that Shepard needed, namely to work on API Project 51. In those years, that was Fran Shepard's consuming interest. Earlier in his SIO life it had been submarine canyons. But I was not a member of API Project 51, so I was politely involved here but not directly. If there was any real encouragement for the things I was interested in it would come from Revelle only, by making it possible for me to go out and do field work on these expeditions.

Harkewicz: Okay. You mentioned Hamilton, correct?

Fisher: Edwin Lee Hamilton.

Harkewicz: And you started out talking about NEL, so I guess I'm confused. Was he involved with NEL too? Was it through NEL that you went on your first sea voyage?

Fisher: That's a good question, in a sense. In those years the NEL group, Bob Dietz

[25] Joseph Ross Curray (1927-).

and his boss Gene LaFond, [26] who had been at Scripps with his wife in the early thirties, headed the Sea Floor Studies Group at NEL. So he knew Scripps. In those days we were in very, very close contact. Bill Menard and I were on the phone with each other daily, more or less, in '51, '52, and '53. And then about 1955, '54 or '55, Menard realized he didn't want to spend his life at NEL and he, as they say, persuaded Roger. Anyway, he wanted an academic position at Scripps and Roger brought him over. Bill Menard had a very, very fruitful mind. But he and I talked often about expeditions. We worked on those separately in those earliest NEL years, but we were in contact. And Shirley Fisher, my wife, worked for him. So actually, we all knew what was going on. It was a very fruitful, close time between these two labs. Now meanwhile, you had other groups with NEL and Scripps working together, Walter Munk's group and some of the others. But in ours it was with the Sea Floor Studies Group, and Fran Shepard had been the Illinois professor of Bob Dietz. So there were all of these connections. It was almost incestuous.

Harkewicz: All right. That makes sense. So, what was the first expedition that you went out on?

Fisher: Well, the first short one, of course—very short—was with Ed Hamilton, as a matter of fact, and two others on *E.W. Scripps* down to do the submarine geology around Guadalupe Island. And then in 1952, I was out as chief scientist on a cruise off Cedros, about a twelve to fifteen day cruise trying out the new *Spencer F. Baird,* the ship we'd just gotten, to try the methods with Russ Raitt[27] that we would be using on the forthcoming CAPRICORN cruise. Russ Raitt had been here for several years, as you know. Incidentally, I came to geophysics/geology through geology. Russ Raitt came to geophysics/geology (from Caltech) through geophysics. So it was a situation where we could work together and complement each other. Later on, several years after I came, George Shor[28] came here and worked as Raitt's direct associate. We all became a very, very close group, both socially and particularly with the Raitts, Helen and Russ Raitt, and our wives. We were very much together at that time. Russ Raitt never had a great interest to be the chief scientist on a cruise. But, Russ was perfect as a senior-most scientist of his field doing a program on a larger or another's expedition. And so I happened to be the one that usually led those expeditions. You'll find through the fifties, when I'm working with Raitt and with Shor, I would be the chief scientist. But, his seismic program was one that I was participating in very, very definitely. And then, during my thesis work, I was directing that at sea and later on I did the seismic data analysis—geologic interpretation for the seismic stations for my thesis work.

---

[26] Eugene Cecil LaFond (1909-2002). His wife, Katherine Gehring LaFond (1910-2005), worked as a marine chemist at Scripps 1932-1936.
[27] Russell Watson Raitt (1907-1995). His wife was Helen Hill Raitt (1905-1976).
[28] George G. Shor, Jr. (1923-). His wife is Elizabeth Noble Shor (1930-).

Harkewicz: Okay. I want to ask you something about that time in a minute. But first, before we lose the tangent here, I wondered—you talked about how you and your wives, Russ Raitt, and George Shor, and how you all interacted socially. What was that like and how did it affect your work here? Did you ever have a falling out personally that might affect your work?

Fisher: Thinking back, we did not have fallings out, personally. There were people that you just didn't particularly care to be around very much. But, the groups that I mentioned did. In those years there was, in La Jolla, a bistro called El Sombrero and there were a number of people who frequently would appear there informally. There wasn't stratification. I don't know that there's really that much stratification now, although I have a feeling that these days the director and several graduate students, and their wives and others, would not be sitting in a bistro in La Jolla [*laugh*] listening to live Mexican music and singing along. That was a little bit the way it was at that time. Now, that may still be going on. I am just too old to be part of that.

Harkewicz: So, it wasn't like the experiences you had with the Raitts and the Shors was that unique? There were a number of people that you interacted with like that?

Fisher: Well, indeed, and Russ Raitt would come home and Helen would say to him—this was after they moved up to SEA, which would have been about 1956—"We're having guests for dinner tonight." And he'd say, "Well, how many?" And she'd say, "Forty-two." [*Laugh*] And he's say, "Oh, we're having orphanage stew again?" That's the kind of thing. You would see all sorts of people at that time and you could talk freely and there was an exchange. During the day you would talk to your immediate associates. At night you would be talking across groups, so that it was very much a mixture, a potpourri of all levels, and it was a very fecund, in a sense, group. And we did love what we were doing.

Harkewicz: So, there was a lot of conversation about work then?

Fisher: Well, not that much. There was, yes. Work came up. But, Revelle had a key trait, incidentally. People ask me, "What do you think about Revelle?" Revelle picked good people and then he left them to do it. The magic thing about Revelle, with regard to the expeditions, was that once you were out of sight of Point Loma you were in charge. You'd come back two weeks later, three weeks later, a month later, two months later, and he would ask you in detail—and I mean encouragingly—what had you found and what did you think it meant, and "what can we think about this?" He was never, never a puppeteer. Some of the directors of other labs were puppeteers. He wasn't.

Harkewicz: Earlier you said that Russ Raitt wasn't concerned about being a chief scientist, and he would go out with you and you would be chief scientist on several trips

that he was on. But, I wonder, is there a reason why you wouldn't want to be chief scientist?

Fisher: Well, the reason in Russ Raitt's case is that he was a very modest man. He was superb at what he was doing and he was focused on that with a laser-like breadth of interest. What he wanted to do was to find out what the thickness of the layers was in different parts of the ocean. If they were all just "layer-cake geology" he would be happier than ever. But he was a geophysicist who wanted to make mathematical models, superbly. That's some of the basic work that was finally built into the idea of seafloor spreading and plate tectonics—parts which came from Russell Raitt. But in fall of 1954, I took Shor down on a two-ship expedition. That was one of Shor's first expeditions, my CHUBASCO Expedition down off Central America, when we demonstrated first what has come to be called "subduction," where, at the trenches, the offshore plates are coming in. That turned out to be a rather basic thing that Revelle and I discussed in 1955 in *Scientific American*.[29] So, that's the kind of key result you see, that Russ Raitt wanted to be very sure of in his own program. Every three or four days there would be a seismic refraction station that we'd be setting up, but in the meantime we were doing various other things. I came at it more or less thinking of using the ship as a field geologist would. Russ was looking at a certain geophysical measurement within that overall structure. So from that point of view it worked. We would have a biologist along, because there were targets of opportunity for biology—a man like Ed Brinton,[30] for example, or some of the others doing that. But, it wasn't like a CalCOFI[31] cruise where you're out there focusing on [plankton] work. We did all of that stuff. But, you majored in geology/geophysics, what I called "philosophical geology," and minored in the biology or physical oceanography on those expeditions.

Harkewicz: Philosophical geology?

Fisher: That's what I call it. When people say, "What are you?" I say, "Well, I'm a philosophical geologist."

Harkewicz: Okay. Can you define that for us?

Fisher: No. It's just arm waving, you know, and talking. But, it's talking about big structures that don't lead to anything useful in terms of finding oil, for example. And if you really think about it, that's the kind of stuff from sea-going people in the fifties, and by the sixties—after the magnetics came in with SIO's Art Raff[32]

---

[29] Robert Lloyd Fisher and Roger Randall Dougan Revelle, "The trenches of the Pacific." *Scientific American* 193 (5): 36-41 (Nov. 1955).
[30] Edward M. Brinton (1924-).
[31] California Cooperative Oceanic Fisheries Investigations.
[32] Arthur D. Raff (1917-1999).

and Ron Mason[33]—all of those pieces started getting put together. My work in the trenches was also part of the jigsaw puzzle. Finally, by 1960–63, a few people were able to beautifully work out and put in the final pieces, and the lights came on: Harry Hess[34] came up with "seafloor spreading,"[35] and then others in the NIO and the Madingley group that we worked very closely with in Cambridge, Fred Vine[36] and Drum Matthews.[37] And then other people later on, at Scripps such as Dan McKenzie[38] sometimes in from Cambridge, and Bob Parker here,[39] were able to use their mathematical smarts to put some of those pieces in place. And suddenly, as Menard wrote, "they lifted the curtain and there was the elephant." And voila! It was plate tectonics. How's that for a philosophical answer?

Harkewicz: That sounds good. So, are you trying to say that you're interested in one of the pieces but not necessarily the …

Fisher: My interest was trenches and finding out what they are actually like in different places. Hamilton, Menard, and Dietz and those people at NEL found some very interesting things. And, we were beginning to fumble around with explanations as to what they meant. It was an exciting time, and that's the point I'm trying to make, Laura, that this was a time of excitement. You could go out for two weeks or three weeks, do good work, make observations, and come up with something that turned out to be truly important. That was why it was so exhilarating. That's why I would be able to go out for two or three months a year. Now ultimately, I was no longer married to the lovely lady, but in all loyalty, for years she supported that program. But, it was still an exhilarating time!

Harkewicz: Well, as long as you brought it up, and you can refuse to answer this if you want, I'm sure that this going out to sea for these months at a time was stressful to your personal relationship. You've already said that your wife was no longer your wife after a certain time period. What was the effect of this on your personal life? How did going out to sea for so long…

Fisher: Well, I think, speaking frankly, when you're at sea and you're in charge or nearly in charge of a program, there's never enough time. If, on the other hand, you were a crewmember or one of the junior scientists you might be crossing off the days on the calendar. But, when you're in charge, there's that excitement. So, I didn't mind doing this. And people such as Silverman, that I mentioned earlier, had no wife yet when he was doing it. From 1950-1954, I had no children—I had no son. In '54, my son was born. But, before that time, I had been on a number of these expeditions, including Transpac while my wife was pregnant with Carlos. But, I

[33] Ronald George Mason (1916-).
[34] Harry Hammond Hess (1906-1969).
[35] Fisher later added, "Bob Dietz coined the term."
[36] Frederick John Vine (1939-).
[37] Drummond Hoyle Matthews (1931-1997).
[38] Dan Peter McKenzie (1942-).
[39] Robert L. Parker.

was not intelligent enough to really perceive how much this was costing her. She, working at NEL, had an interest in these things. And, there was something of a support group, but it wasn't a cutie-poo support group. It was just people who happened to like each other and knew what was going on. In those days you had a radio station here and the word would get out from there. You weren't using email! There was a coherence of people focusing on how to learn about things. I was married for eighteen years before finally, in 1966, it got to us.

Harkewicz: But, that didn't stop you from going out again in the future?

Fisher: No, but I had a lot to think about. But no, it didn't. And, I had a son by that time who was eleven or twelve years old, and it was very hard on him and it has been, I'm sure, the rest of his life. But, on the other hand he has survived and done very well on his own.

Harkewicz: Okay.

Fisher: But those are very personal points.

Harkewicz: Well, thank you for sharing that. It's a question that I have because it's something that most people aren't familiar with, going out to sea for months at a time, and how you have people at home waiting for you or not waiting for you, or whatever. We've been talking about this for a while but I just wanted to note that it's been written that you may hold the record for the longest time at sea of any Scripps scientist.[40] Can you maybe describe a typical day at sea for you?

Fisher: I could, if I may, comment on what you have just said. The point you just made: in Bill Menard's 1986 book he mentions, in passing, about this. But, it's not about setting records. Let's put it this way. The man who has done the most time on the bus, even though you commute every day, is the bus driver. [*Laugh*] Okay. I'm not putting down crewmembers, quite the opposite. But, I'm just saying that if you're starting to count days, that's true. The point that Bill Menard was making was that putting together the expeditions, planning them, justifying them, getting the people together, and going out and leading them and doing it for all those years, for long periods of time, not for these two or three day or a week cruises, but long cruises, perhaps Fisher had done that. But it's interesting because that particular sentence from Menard's book is brought up very often when I'm introduced. And there's always somebody in the audience who'll mutter, "Well, I'm sure I've done more than that." So, that's why it's fruitless.

---

[40] Fisher later edited the transcript as follows, "Quoting more correctly, '…Bob Fisher, who has been active at sea from 1951 to the present (1986), may hold the record for scientific exploration, anytime, anywhere.' Henry W. Menard, *The Ocean of Truth* (New York: McGraw Hill, 1969) : 43" The reference for the question as posed was actually from Elizabeth Shor, *Scripps Institution of Oceanography: Probing the Oceans 1936 to 1976* (San Diego: Tofua Press, 1978): 291. Shor writes (in regard to Fisher), "He probably holds the record for time at sea among Scripps senior scientists."

Harkewicz: So, you're saying that it's not just the time at sea? It's from the planning to the completion?

Fisher: What Menard was trying to indicate, I think, was about those people who had some scientific impact going out and doing this. Now, there are certainly, as far as Scripps is concerned, people who have been out in dozens and dozens of MLR[41] cruises, and they found out all sorts of things. But, what we were trying to do was to have twenty years' experience and not one year's experience twenty times. In other words, the cruises evolved, the ideas evolved as the equipment evolved. The things that we were able to do by the seventies we could only dream about in the fifties, because there wasn't the equipment, there wasn't the positioning. And now, small boys and small girls, of either sex, can do what we could barely dream about in the seventies.

Harkewicz: Now, that brings up an interesting point, though. Because, that seems to me, to imply a certain amount of continuity in personnel, or ideas, or something.

Fisher: Which is a very key point.

Harkewicz: How much of that can be explained in writing or in conversation at Scripps and how much of that has to be an actual tacit knowledge that you get by being onboard ship with different people at different times?

Fisher: Well, you're speaking about different types of things. In terms of relationship, as I say, I've mentioned one or two names and many others have gone out with me. Ashore, I had a long and very, very fruitful association, for example, with a woman geochemist. Well, not strictly a geochemist. She was a wet chemist basically but a geological chemist, named Celeste Engel.[42] And, that was a very close relationship professionally for a number of years, very fruitful. I had similar relationships at sea with certain people. Some of the technicians would go out nearly every year. My own proudest statement was that people would go out with me a second time, or third. If you think about that, it sounds trivial. It isn't. I mean I'm trying to give you some of the philosophy of going to sea, because that's what you seem to be interested in.

Harkewicz: Right. Well, it's not something that most people know about. So share why is it so important to you then?

Fisher: Well, in the first place, the friendships, the relationships, but when you have somebody who's very, very good you want to have them continue to work with you on these things. And you get ideas from them when you're at sea, as when you're ashore working it up later, and then when you're ashore you regret you

[41] Marine Life Research.
[42] Celeste Gilpin Engel (1922 -2004).

didn't do certain things at the time, so you want to do it again. In other words, you're fitting in new pieces, so the whole thing is an evolution. But, you do tend to have a key group that you take out with you. Russ Raitt had certain people who were out time after time with him. Very often, I had the same people with me—sometimes with him, sometimes not. And, when I say, "I had," it sounds like I was more or less puppeteering. I wasn't. I just had to have responsibility for the results. But, the reason that people such as myself and a few others whom you know would stay here—the reason for being at Scripps—was that if you had an idea about something geological/geophysical, almost anyplace in the world, within three years you would get the seagoing facilities to go out and investigate it. And that's a very powerful argument to stay at a place like Scripps. You get other offers to go other places, but …

Harkewicz: So, how was that possible then?

Fisher: How do you mean?

Harkewicz: I assume you're talking about support, as far as intellectual support but also as far as financial support?

Fisher: No, not only intellectual. Intellectual support, yes—and certainly in the fifties and sixties and seventies in our field, it was very active. I mean, the people you would just see, and talk. It wasn't forced. It was just a natural time. We were learning things, and the population not only at Scripps but also with Cambridge, with Lamont to some extent, although Lamont had always been xenophobic in those years, very xenophobic, but with other places we were back and forth and we were friends. And during the years when maybe around the world there were twenty-five people putting together the different parts that bit by bit became the jigsaw puzzle that turned into "revolutionary enlightenment," whoever you want to say are the big names, the biggest name of course is Harry Hess. But those stray bits came together and that was an intellectual thing. But then you needed specialists who could analyze samples, engineers who could develop certain equipment, and so on. For example, I'm sure you could talk to people like those in Fred Spiess's[43] group and see a different emphasis, the engineering. And in our group we had certain abilities. So, you would tend to work together and that's how techniques evolved, and stayed there.

Harkewicz: Okay, so then you're saying Scripps had people that worked together better than some other institutions then?

Fisher: No, I'm saying that each institution had its pattern. They were very different. There's a point here—maybe it's boringly reiterated—that Scripps, in the fifties and early sixties, as it's probably been since, but then it was very obvious to some of us at that period: Roger Revelle was the godhead of Scripps Institution and he

[43] Fred Noel Spiess (1919-2006).

tried to pick good people and let them do their thing. And as I said, once you were on that ship, he didn't puppeteer or anything of that sort. He supported you in terms of having it made largely possible, because, in the fifties and sixties, the funding procedures were different. So there would tend to be funding. People tend to say now, "It was easy." No, it wasn't at all that easy then, but Revelle picked certain ways of supporting. When I say, "Revelle," Revelle and his organization.

Harkewicz: So, you mean the organization of Scripps financially supported the people of Scripps?

Fisher: The Navy, the Office of Naval Research that Roger Revelle had been one of the founders of, was the largest supporter of Scripps at that time. Now, the state of California was and is supporting CalCOFI. But the major supporter for Scripps at sea was the Office of Naval Research. They were supporting things that weren't right there to kill our enemies, but we were finding out basic science, and that's why it was a good reason to be at Scripps and it was a fascinating time.

Harkewicz: So, Revelle's organization took some of the pressure off the scientists, that's why if you had an idea, two or three years later you could go out to sea and see it in action?

Fisher: Scripps had the facilities, and Scripps ships were largely being used by Scripps scientists. So you had the usual suspects year after year. For example, Warren Wooster was an expedition leader that did a number of cruises. John Knauss did several of them. Bill Menard, by that time, did them. I did them. Joe Reid[44] is another example. These were the group. Maybe eight or ten people, but you see their names would come up again and again. It was a very well integrated group.

Harkewicz: So, that was sort of unique to Scripps?

Fisher: I think what was unique to Scripps is that, in those years, a number of individuals were being supported and encouraged, and various people then went off to become directors of labs. John Knauss in Rhode Island. Warren Wooster in Florida, and so on—more or less in the tradition that they had seen at Scripps. Now other good oceanographic institutions had a different pattern.

Harkewicz: I see. Earlier you said that the best thing that somebody could say about you is that they wanted to go on a second cruise with you?

Fisher: What I was saying is that, I guess, if I were taking satisfaction in terms of relationships that is the one that I was happy about. But, this is the first time I've put it that way.

Harkewicz: But, I mean did you mean that in a professional sense then or in a personal sense?

---

[44] Joseph Lee Reid (1923-).

Fisher: I mean that we had very, very good people and they [became] available, or they would go to some trouble to go out and do it because they were interested in what we were doing. They saw the reason. They also saw the reason they were getting credit for it, and it was interesting, and they enjoyed it.

Harkewicz: Okay. Now, what I wanted to ask you too, though, is before we even started recording, you mentioned something about not having a typical academic career, like standing in front of a classroom and talking. But, I wondered about, you know, being on all these cruises, since you've had so much time at sea, how about your being a professor at sea, so to speak? Was that something you did?

Fisher: No. Well, you see, I spent, say—well, this is going to sound a bit disorganized. But, the day—I came here in 1950. I didn't write and defend my thesis until April of 1957. And the reason I defended the thesis on that particular date in 1957 was that it was Fran Shepard's birthday and he wanted to invite his friends from UCLA down so they could get together and have a birthday party for him that night. The party for me was up at the Revelle cabin. They took a couple of cases of champagne and some good friends and we went up to Julian and had a party for me. But, the point is that in that interim, from 1950 to 1970, I wrote and published a number of papers with people, and alone.

Harkewicz: You mean 1957?

Fisher: Nineteen fifty-seven. Between 1950 and 1957, there were a number of papers published so that, by that time, you're making an international reputation, so to speak, in meetings and committees. And somebody such as Revelle really encouraged that. I keep bringing up Revelle because he's a symbol of that type of operation. But, I guess what you could say is that Revelle gave you the platform—I mean; he made it possible for people to sing their songs. And that's why his name comes up for those years.

Harkewicz: But, I guess what I was getting at is, you know, I mean again going out on research cruises or expeditions is something that is not familiar . . .

Fisher: Okay. I see what you're saying now. All right, now . . .

Harkewicz: What I was talking about is your educating other people at sea. That's what I was getting at.

Fisher: Okay. All right. No, I see. And, that makes a good point. The day that I passed my thesis defense Roger and another came to me and said, "Do you want to be a professor at Scripps?" And I won't go into that in more detail, but I said "No, not really." And basically, I was feeling it was just such an exciting time and I was working with people I really cared about. I mean then you were treated as an

equal all through Scripps. I was more interested in thinking "This year's expedition, exactly what are we going to work on now?" In other words, I wasn't thinking of teaching. I was not thinking that when I got older and perhaps couldn't get funding I'd need to have somebody paying my salary. When you're that young, you're immortal and you're not thinking about that. There were too many things to find out first, and I was doing it with some wonderful people. Not only at Scripps, but the Cambridge group, and others—and shortly after I spent fifteen months at Cambridge, in Britain, with the Madingley Rise group that was doing some fundamental, really significant work. Later on the names that people heard often were Vine and Matthews, John Sclater[45] (a co-author now at SIO), Teddy Bullard,[46] and Maurice Hill.[47] That was an exciting time and that's why I didn't want to say "I can't take an expedition out because I'll have to be teaching."

Harkewicz: Right. But, I'm still trying to push you here, because what I'm saying is, didn't you teach at sea?

Fisher: Okay. Basically what I was doing there, if anything, was tutoring one or two at a time. The people that I had on those trips—when I say "I had on those trips," or I was on their trips—they're still good friends. I don't have any "grandchild." Academic grandchildren really, but I certainly have academic children who were out on these cruises with me and went on to other labs. We taught each other. That's the key point. We had people who had different specialties. Raitt taught me. Some of Raitt's people taught me. I didn't know the nuts and bolts of his recording equipment but I knew how to use his equipment geologically with those people. And that's what we did. So that from that point of view it was good. Or some of these people, particularly in the early years, would get together, the fellow graduate students. For example, in mid-1953, I organized a one-ship program for a scouting cruise called TORO to study the Middle America Trench, and we went down to Acapulco, and that was five or six fellow graduate students— just as I would have gone out on their trips. And, that's how we taught each other.

Harkewicz: But, you were still going out leading cruises in the eighties, correct?

Fisher: Oh yes.

Harkewicz: So, what was it like then?

Fisher: Remember, though, that in '57, we had three cruises as part of the IGY and Bill Menard and I co-led the two halves of the four-month DOWNWIND cruise, in the Southeast Pacific. That was the first time that Scripps went to Easter

[45] John George Sclater (1940-).
[46] Sir Edward Crisp Bullard (1907-1980).
[47] Maurice Neville Hill (1919-1966).

Island. I took the SIO ships *Horizon* and *Spencer F. Baird* there in January 1958. But I— with Russ Raitt—was particularly studying the South American trench structure, and Nazca Ridge. Again, that was with the same group of people.

Harkewicz: Okay. So, it was mostly with fellow research scientists than graduate students, then?

Fisher: But, you had scientists from other institutions, too. I mean, professional types. Bob Norris from Santa Barbara, for example, was on DOWNWIND with me. You got people from around the world, actually. In those years our core scientific parties were primarily Scripps, but you would have two or three senior scientists or "distinguished foreign visitors" as we called them. But then we also had students, and we also quite often had what were called "warm bodies," people who were not Scripps students but were volunteered one way or another, or came just to help out and learn.

Harkewicz: I want to talk about you being the ship scheduler.

Fisher: I didn't know you knew that.

Harkewicz: Yes. Well, I have here that you were ship scheduler from '64 to '68.

Fisher: Yes.

Harkewicz: So, first of all, what does that mean and how does that affect all these expeditions you went out on?

Fisher: What it means is that you are—what the ship scheduler does is decides how to fit the various pigeons into the pigeonholes, in terms of what's going to work. You could have a whole two-hour interview on that. Key is that there was support for various things early on. What year was that I started, '64?

Harkewicz: Sixty-four.

Fisher: Okay. As I remember I followed George Shor in that position, and he'd set a good pattern for what we were doing. But, in those times, the expeditions were such that you'd start out from San Diego and you would try to build coherently from there. I've used the analogy of you're building a wall and the bricks on each side support each other and then they support the one on top, and so on. So that you would ask, "Where shall we go? What would be logistically best, what would be geographically coherent to build together? What sort of scientific things can we do?" In other words, "Fisher wants to go to the Middle America Trench to attempt certain observations in a certain place. What else could be done in that particular area, and who could we get to do it?" Primarily from Scripps, but there might be someone from somewhere else. Or somebody would come in just out of left field

and say, “I hear you’re going to so and so. I’d love to go there and make this observation.” And, you’d find a place for them. That’s the way Scripps expeditions were done in that time. And we would try to include quite a few people. A model for that that‘s been written up many times was the 1952-1953 CAPRICORN cruise, in which you had “very, very distinguished” senior Scripps people, and “suitably distinguished” foreigners, such as an Australian by the euphonious name of Rhodes Fairbridge,[48] invited to come on various legs of this two-ship seismic cruise. But, that was one common pattern for cruises at that time.

Harkewicz: So, would people come to you and say, “I want to do this work in this place,” and you would have to try to find a place for them?

Fisher: They’d come into Ships Scheduling and they would have gotten funding one way or another. Or, you would have to find out how to get it. In other words, would some block funding take care of this type of operation? Certain programs at Scripps had built-in support. They didn’t have to apply for it, so you could put those as “givens.” But you would want to know in which order to try to do these operations within the availability of the scientists, and the ship’s facilities, and the ship’s maintenance, and all of that. So, you’d fit those things into a schedule. George Shor, for example, was very good at that. In a sense, I followed the same way. I evolved a little bit differently because there were different strictures in my time. But that’s exactly what you do. What is the most coherent pattern you can work out to build a viable economical operation with what you have available? That’s what the ship scheduler does. Now these days, it’s a very different type of environment and I think you need to talk to current people about how they do it, because the funding situation has been completely changed.

Harkewicz: But people would normally come to you with some sort of funding or something like that?

Fisher: Well, they would come with an idea about where to go, how could SIO do this, what ships might be available, and so on. And when I say, “You came to me,” our office did help out but they would have to find funding for their own people. They would organize a group just as I had done earlier on my expeditions. PIs would come with an idea, and where can it be done, and what ship would best do it? So, you fit the people to the ship, the range, the probable weather, all of that, and the support. Basically, what’s the most coherent package? In my view, certainly today’s national program is much more a case of Brownian movement, in terms of ship itineraries. But then, that’s a very personal feeling.

Harkewicz: Did you feel like it was a political position?

[48] Rhodes Whitmore Fairbridge (1914-2006).

Fisher: Yes. Of course it was political. You worked with all sorts of people. You worked with international groups, national funding groups—that was during the Nierenberg era, primarily Nierenberg. And, the reason I was asked in 1970 to be an associate director and the head of SOMTS[49] was because of that type of experience.[50] You have contacts with Washington, you have contacts internationally, and you have to put those things together.

Harkewicz: Okay. What about work with the . . .

Fisher: And, by the way, throughout those years, starting from early 1962, I had an assistant who was not a scientist, a woman named Marie Jantsch. And, you'll find that I published with her, and so did others. She was a superb organizer and she could handle the problems that I and my personality, or they and their personalities, would bring out. She was just as much ship's scheduler in terms of making it successful as I was. What I had was the academic position and she had the human smarts.

Harkewicz: Marie Jantsch?

Fisher: J-A-N-T-S-C-H. She passed away several years ago. And by the way, she went to sea on two of my expeditions, too.

Harkewicz: Did you have to concern yourself then with the ships and their crew members also, or did the crew come with the ship?

Fisher: In those days, we tended to keep our crew members for a long while. Starting before the fifties, [MarFac][51] had a lot of people who had been in World War II in the Navy and they stayed around until about 1960 or '65. So, we knew officers and crew intimately, just as we knew people ashore, in terms of their abilities, their personalities, their likes and dislikes. So you would say, "Well, so and so can't very well go out with that one because of this type of program. This one might be a better choice," and so on. Those aren't the instances that got written down.

Harkewicz: That sounds very complicated, trying to match personalities?

Fisher: Well, it's kind of obvious when people don't like each other. I mean, within an organization. And remember, MarFac was an organization as much as Scripps.

Harkewicz: Was that a very time consuming type of thing?

Fisher: It could be.

---

[49] Ship Operations and Marine Technical Support.
[50] Fisher later clarified this as planning experience.
[51] Marine Facilities.

Harkewicz: Did it affect your job outside?

Fisher: Yes, it was time consuming. Furthermore, all that while I was organizing major expeditions, and going out, and while I was out on expeditions, I had a staff ashore for the academic work we were doing. When I say "a staff"—three or four people. Well, technicians or my assistant, Marie Jantsch, who would run the lab when I wasn't ashore. She kept the ships scheduling evolving while I was gone. Just as your ship scheduler, Bob Knox,[52] has a superb staff now that does exactly the same sort of thing, except I don't think Bob goes to sea.

Harkewicz: So, if I'm understanding you right—now, I'm backing up here a minute, though. You haven't taught at Scripps?

Fisher: I've taught at Scripps. I give lectures from time to time on specific areas. When we come back from an expedition, of course, but what I've never done is signed up to give a class in something, not since Northwestern. But frequently, students or colleagues would come in and want to know about this or that. I'd sit down and talk or advise then, or I'd go to them. So, too, many of the students—I've been on thesis committees. But what I never wanted to do was to have to know that every year I was going to be teaching this particular class, and that's what I learned when I was at Northwestern as a graduate teaching assistant, I didn't want to do it.

Harkewicz: But did that ever cause you any concern with your position?

Fisher: It never caused me a concern in terms of—it never limited me at what I wanted to do—I never felt insecure. Now, there are academic people here who are not in the research position who think that they have got it made, that they'll never have to get personal support, they'll always have a salary. That was never a problem. And the reason it wasn't a problem is because that was an exciting time to do new things at sea and there was support for ship-type exploration, ONR[53] or NSF[54] support. And then also, I happened to have a reasonably large share of SIO hard money. I wasn't hanging on by my fingernails, like some people think research people do.

Harkewicz: And, how did you happen to have this?

Fisher: Well, for whatever reason, I don't know. It just happened to be there and it came.

Harkewicz: The things that you were interested in got paid for?

---

[52] Robert Arthur Knox (1943-).
[53] Office of Naval Research.
[54] National Science Foundation.

Fisher: Exactly. Somebody somewhere, or a group, thought that having me here—when I got my degree the first thing that was said was, "We want you to stay here."

Harkewicz: Again, let me just ask you this, though. Because, I know that there's been this sort of division between the research and the teaching end here at Scripps.

Fisher: Yes. And very, very rarely there's somebody who says, "I don't want to be on the teaching staff. I want to be on the research staff." I was one of those. Willie Riedel[55] was, too. A respected biological scientist named John Strickland,[56] who died some years ago, was that. That was his preference. But, ninety-eight percent of the people would give everything they have to have a teaching position somewhere.

Harkewicz: So do you think that was unique to Scripps? I mean that other places might require you to teach?

Fisher: Well, let me put it this way. For me, Scripps was an ideal place and I could live that way. I could do what I needed to do, when I needed to do it, and didn't have to be subject to these classes. But then, keep in mind part of the reason for that was that during the time I was a graduate student, to the extent that I was "mentored" by anybody, it was Roger and him only as a warm, supporting presence so that we could talk about results. We published together—that early *Scientific American* trench article by Fisher and Revelle, 1955, for example. Because of Revelle and people of that kind, and there were others here who were very supportive. It bothers me very much when a student will come in to me and I'll say, "Well, can you try this?" or "Why don't you do this?" "Well, my professor wants me to take my orals at such and such a time." And my general feeling is "Well, thank God that didn't happen then."

Harkewicz: Well, you've been here for a long time, have you seen it change?

Fisher: Yes, of course. But we had . . .

Harkewicz: Since Revelle? Or . . .

Fisher: In here—no, well I mean, here we seem to be canonizing Roger Revelle.

Harkewicz: No, but, I know he was a unique person.

Fisher: Remember that when I was associate director, I daily was very close to ground zero with Bill Nierenberg,[57] because we were the on-site advisory group. But, that's an interesting point about Nierenberg, by the way. Nierenberg had a very

[55] William Rex Riedel (1927-).
[56] John D.H. Strickland (1920-1970).
[57] William Aaron Nierenberg (1919-2000), director of SIO 1965-1986.

small operation group of his own. There were only five or six of us including him, and we'd meet once a week. But he had what he called his "barons." And his barons were—like the Pope has his *in pectoris* cardinal—Nierenberg had five or six barons. Five, I would guess. And you'd routinely hear the names of four of them but the fifth one was not named and that let everybody think that he could be that, too. [*Laugh*] So, it was really a good way of doing it. But, you can imagine who the barons were, if you knew anything about that time. And I'm not going to name them for you.

Harkewicz: So, as associate director, you weren't a baron?

Fisher: I was not one of his barons. I'll name one that's obvious, Walter Munk. Okay. Now, no, I was not that. As an associate director, I was a member of Bill's three or four tactical "advisors at hand," and in my case, my responsibility was SOMTS, and also counseling him on certain things, and once or twice going to the opera, and things like that. He liked opera—an opera fan.

Harkewicz: Well, as long as you brought that up, I mean, that's—we're jumping ahead in time frame here, but we might as well talk about it.

Fisher: Well, go ahead.

Harkewicz: I wondered if the two of you got along, in general. Were you usually in agreement as how Scripps should be run?

Fisher: Who is this?

Harkewicz: You and Bill Nierenberg.

Fisher: Oh, Bill Nierenberg, he certainly was the director. I didn't tell him how to run Scripps. Bill had a point of view. Remember that Bill had grown up in an entirely different milieu than, say, I had, and he thought of me as a Southern Californian who had an academic background. And I thought of him as a city New Yorker who had an academic background. But it wasn't just Fisher and Nierenberg; you have all of these people. I was just a *fonctionnaire* of his immediate staff.

Harkewicz: So, you were just running one part of Scripps?

Fisher: Yes, but sitting in formally and giving advice on other things; and some Saturday mornings he would wander in and speak to me about them. But, it turned out that we had very different opinions on certain things.

Harkewicz: Did he usually let you run SOMTS the way you wanted to?

Fisher: Well, the thing to be recognized: a lot of people at Scripps had had a lot of sea time, and Bill Nierenberg wasn't a sea-goer. For example, George Shor was another one of the associate directors at that time, and in those years, George was something of a, well, "ambassador without portfolio," in terms of being around to give advice on a lot of things. A lot of this stuff you probably shouldn't be using.

Harkewicz: Well, that's up to you.

Fisher: No. It's going to be up to you, I'm afraid.

Harkewicz: All right. So, let's back up here again, because we've talked about expeditions. We've talked about the different positions that you've worked in at Scripps.

Fisher: You've also talked about my feelings about the evolution of Scripps.

Harkewicz: Yes. But, if you're talking about the evolution of Scripps, and we had sort of talked about this a little before we were recording, what about some of the changes in society? How did that affect Scripps?

Fisher: How do you mean?

Harkewicz: Oh, well, we talked about, you know, things like the Environmental Movement, or the Women's Movement, and things like that.

Fisher: Yes. I see what you mean. Okay. In the fifties and sixties we could, as far as Scripps is concerned, be free. Scripps was part of the outer world but it wasn't really. Most of the longtime Scripps people, they might do their thing—botany, biology, marine geology, and all sorts of things—but they weren't closely interested in its impact on society, in general. It appeared there was almost never a question about getting adequate support and would the Navy ultimately find this worth supporting. And we were not the ones who had to take a loyalty oath to the Navy. But I didn't have any problem going out and doing topographic work, for the Navy was interested in knowing what the bottom of the sea was like for its own uses. But I was not involved in their missions. Some people at the Marine Physical Lab were. And SIO people like Russ Raitt; some of the restricted work was like that. I guess we didn't use a word that you hear very much now; it wasn't "What's the societal impact?" That was something that didn't come up. We weren't selfish people, but on the other hand we were focused on learning what was there, finding out how features worked, how they evolved. In other words, finding examples, making observations, drawing conclusions. In a sense, as I've said over the years, an exploring shipboard expedition is like a large-scale experiment. You frame it properly. "Where should I go to find this out? How do I go about it? What do I need?" And that's how you develop and make up an expedition. We didn't think, "What will be the effect of the Middle America Trench on society?" Even though we were perfectly aware that, for example, an

earthquake in the Middle America Trench could devastate a large part of Mexico. So, for our point of view the interest was, "What would possibly be the overall seafloor effect and how would it happen, and when would it happen?" But not what it would do to Mexico City.

Harkewicz: But, is that something you have to be concerned with now?

Fisher: These days, in terms of getting support, yes.

Harkewicz: And how do you think that affects the science?

Fisher: Well, certainly you must see the examples of that—the seismology programs, for example, at Caltech. It used to be they would talk about them more or less describing the basic scientific findings. Now, the *L.A. Times* articles have to do with "What's going to happen when 'the big one' comes to this part of Los Angeles?" And that's exactly what it ought to be. It's just different.

Harkewicz: So, you don't see that it might be detracting from basic science, then?

Fisher: No. As for the basic science—now data cascades in. We were laboriously trying to find our positions, for example—and now people sometimes dismiss that—but in the fifties and into the sixties, ship's officers would work hours to take and work up sights to find out where we were, if the conditions were right. Then I, and Menard, particularly, and geologists like us would spend hours, really, further refining where the ship had been in terms of what the soundings indicated, and basing our whole sampling program on it. You would do that for hours and hours. These days you want to know where you are and you push a button on a computer and you say, "Where am I?" and the machine says, "Where'd you think you were, dummy? You're right here, to the three decimal points." And boom, there it is.

Harkewicz: So, that's changes in technology, then?

Fisher: Well, exactly. It's the evolution of the thinking *and* the technology. You think about changing the tools and then it becomes possible to change the technology. And, a few bright people like Fred Spiess come along to do that, and then we can do things that earlier we just dreamed of.

Harkewicz: Okay. Here's a really abstract question for you that just occurred to me. You were talking about philosophical things before, or philosophical geology. How do you think that affects the way people think then? I mean, do they think in terms of the technology involved then, instead of the bigger ideas?

Fisher: No. You get curious. For example, you're curious about how the Indian Ocean differs from a geological/geophysical viewpoint from parts of the Pacific? What would be characteristic of this? How do we find out what it's like? And what sort

of instrumentation do we need and what sort of people do we need? So you focus on that. Then you call that in and apply it to that particular problem. Or, there is something like this tsunami, which is societal, with immediate impact, a tragedy, and you bring in these specific techniques from around the world to study that type of thing. So that's the evolution to watch, but, in our case, in the fifties and sixties and seventies, the evolution of equipment is what let us go from having a vague idea of what the seafloor was like to being able to find out exactly how deep, to within a very few feet, the deepest part of the ocean world was. And, we did that on *Stranger* in mid-1959, and then six months later the bathyscaph went to the bottom. And you know what? They measured "depth" a different way— by manometer—and it came out to be just the same. That was because of the technology.

Harkewicz: Well, let's talk about some of that technology and some of the work that you've done over the years. I know that – and since I'm not a geologist, maybe you can explain some of these things. I know that on CAPRICORN it was your job to separate out the soundings from the Tonga Trench, the sides from the bottom. How did you do that?

Fisher: Oh, I would—okay. In 1952, '53, when we were planning CAPRICORN—and with CAPRICORN Scripps basically went to sea in the sense that its director went to sea. So various other senior people definitely wanted to be there, and that's why it made an interesting study of personalities in Helen Raitt's book.[58] But, my prime interest by that time was trenches, and so that's why I went along. I went to study the Tonga Trench. We all stood watches and made lowerings. But the equipment at that time—for example, you didn't have echo sounders that could really do good, deep work. So, in 1952-1953, we had a two-ship operation and Raitt was doing the seismic refraction work and we on *Horizon* were doing that with him—he and his MPL assistants on *Spencer F. Baird*, Alan Jones, and others whose names will come up in Scripps history from time to time. We had only a few days to work there, so we had to push what we had. What did we have? We had rather crude stuff. The way we had to get the soundings for those deepest areas, I was having Max Silverman make up half-pound TNT blocks, throwing them over the side every one minute, and that got enough energy into the water so that we could come up with echoes. And, using Russ Raitt's oscillograph recording equipment, I could then record that explosion and find out what its shape was. Later on, echo sounders were developed that were powerful enough and beautifully enough timed so you could easily do that sort of thing. But, in 1952-53—I mean it was subtle as a sledgehammer, putting dynamite in there just to get enough energy. You took those crude records and you had to then do the side echo analysis, with various geometrical projections of different surfaces for doing the echo train analysis. So I was able to say, "The conditions of this, the depths of this, are probably about this, plus or minus this much." In other words,

[58] Helen Raitt. *Exploring the Deep Pacific* (New York: Norton, 1956).

you took what makes sense topographically, in terms of the physics of the whole thing. I was using explosives to get echo trains so as to do a physical analysis of returns to see what the geological conditions were down there.

Harkewicz: Were bathymetric drawings involved with that or was it more of a statistical thing?

Fisher: That *was* finding out the bathymetry. Bathymetry is "measurement of depth" and that's what we were doing. That was the method we used. Now, not long after that, people from Lamont, using the *New York Times* wirephoto equipment, came up with better echo sounders having very precise time bases and they were able to take very good clear records that usually were easy to analyze. But even those would not really work very well in the very deep Pacific trenches, so we had to work that out. And I developed simple methods of doing that, ways of doing it aboard the ship in "real" time to make those measurements. Other people did that in their own fields, for other measurements. At that time, we were still using celestial navigation so you didn't know exactly where you were, but you could probably eliminate where you weren't. So you were using the ship to do field geology. But, when you start playing with the physics of great depths in terms of geometry, uncertainties increase. Later on we were able to obtain other systems as they evolved, and now, people can go out and map topography "precisely."

But, the other great discovery, in terms of making our work much easier in the sort of thing I was doing—studying the trenches—and remember there were only a few people around the world that were working at any of that, and Scripps more than anybody else. But anyway, the next great discovery was an improvement by a famous man from MIT called Harold Edgerton,[59] the man who invented the stroboscope. In retirement he got interested in marine work. He had become a millionaire with the AEC stroboscopic timing devices that they needed for the atomic bomb tests. But he also got interested in oceanography as a hobby, and he met a man named Cousteau, who you've perhaps heard of, and they decided to start developing underwater cameras. The Edgerton (EG and G) strobe camera, with the timing system and the way it was engineered by their group, was very expensive. But those cameras for the first time started making it relatively easy for non-specialists to get bottom photographs from the greatest depths. Before that we had people like Carl Shipek[60] at NEL in the 1950s, who had camera programs. Maurice Ewing[61] back at Lamont had a program. But it was Doc Edgerton who really made the man on the street begin to be able to use it as a tool, to not spend his life being a photographer, but using photography to be a good geologist in those areas. That's the effect that he had. That was the second advance. And the third great discovery that made it easy, and this came toward the latter third of my own seagoing career, was satellite navigation. That took out a lot of the

[59] Harold Eugene Edgerton (1903-1990).
[60] Carl Joseph Shipek (1917-1969).
[61] William Maurice Ewing (1906-1974).

uncertainty, the dreary labor of track adjustment. It doesn't take away all of it, and it doesn't take away the physical uncertainties of sites having to do with long wires and geometry, in terms of knowing where the dredged sample is really from. What you do know is exactly where the ship is. But you want to know where the seafloor-sampling device is relative to the ship. And that's why Fred Spiess's Deep Tow arrays and their setups were wonderful things. We didn't use those, but that's why they were able to do the classic work that they did.

Harkewicz: Well, a number of times you've talked about trenches, and I know that Roger Revelle nicknames you "Trenchant," and I wondered, I guess, why did you find trenches so attractive—if that's the proper word?

Fisher: They are the extremes. For example, if you wanted to win beers in a bar you would say, "What's the deepest part?" and people would bet beers on it, people are interested in any extreme. As a third-year undergraduate at Caltech, I attended a graduate class taught by a man named Hugo Benioff.[62] And he was a world-class seismologist in every way, like Beno Gutenberg[63] and Charlie Richter,[64] and all of those. Caltech, even in those years, did not have the penchant against mobile earth. Seismology at Caltech was really relatively flexible. Gutenberg and Benioff, specifically, were at least feeling their way toward—Gutenberg was a European—feeling toward a mobile earth type thing. Today the historians, you know, pop their corks writing about that and about the evolution of the thinking on mobility. But anyway, one of the things that Benioff used in his pictures way back in 1947 was the trenches, and I learned that those were areas of earthquakes at great depth, so why don't we examine them? Nobody else had the facilities to start doing it. We had crude facilities; we had ships, and we had echo sounders, and we had some people who could use these things. So we started putting those together and that's where I came into it. What can we use at Scripps to go out and study these trenches?

Over the years, Bill Menard and I more or less had an understanding. We never talked about it, but there was no reason for both of us trying to study all of these things. Certainly, I stopped to look at seamounts, look at other places, and look at abyssal plains, but I was really interested in trenches. Bill Menard really was interested in the mid-ocean ridges and examining—classifying—topographic species. Well, he was interested in all phases and he contributed to all phases. But you might say that my group and co-authors primarily did the work on trenches at Scripps. And Menard and his students were doing a lot of the other stuff. We all did everything, but some of us did more of certain things.

Harkewicz: Well, as long as you—again, I'm jumping all over in time here, which I don't really like to do, but you mentioned trenches. You mentioned seamounts. And, I

[62] Hugo Benioff (1899-1968).
[63] Beno Gutenberg (1889-1960).
[64] Charles Francis Richter (1900-1985).

know that since 1982 you've been chairman of the GEBCO Subcommittee of Undersea Feature Names.[65] And I guess, being a non-geologist, I don't understand the significance of all that, and maybe you could explain why the name of a feature is so important?

Fisher: Well, we could have quite a discussion about that. It takes a little background. The so-called group GEBCO was founded by Prince Albert I of Monaco,[66] the "sailor-prince" that more or less organized it way back in the 1890s. And by 1950, it was quietly based in Monaco with a few old men, mostly retired admirals. It became somewhat revived by a number of the younger people, such as Menard and Bruce Heezen.[67] I got involved in the fifties and sixties. There were subcommittees and I was named to the international panel for naming seafloor features. For example, in geology there is a subcommittee that formally rules on the names of formations, so you don't just get a babel, a completely chaotic set of names. When you go out on an expedition and you "discover" something, assumably you're the first person that's ever [learned] that it was there. Well, as I was quoted in print once, "If everybody who'd ever been there suddenly became visible, you'd think you were in Carnegie Hall." Possibly you're not really a discoverer. The key to being a discoverer is to do your homework; to being a "discoverer" is to not do it. That's why you have a panel of people that agree to review these things. You know, there are many panels. The United States government has one that American government employees "must" use for naming. But what you want are names that are descriptive, are appropriate, or that, for example, recognize discoveries. People who make discoveries tend to think a bit about their legacy, or at least quite often they do. You look at Antarctica and you'll find the names Byrd this and Byrd that, many. Okay. On the seafloor somebody discovers something, a seamount, say, and who is the most important person in the world? Well, the new grandchild. So, "I will name it for my grandchild." After a while that becomes rather bad, so there are these committees. And having an international committee means that you can consult many, many sources. That was the SCUFN committee based in Monaco and I was named to it in the seventies, and then in the early eighties, I was made chairman. And, for twenty-some years, to 2003, I was chairing it. That was a group of about ten people from around the world, Russians, British, Japanese, etc. and, we'd meet at Monte Carlo and we would discuss these issues. The reason I was chair for such a long time was because of historical perspective. You try to bring that in so you're not using chauvinism, nepotism, crudity and so on, and those are very natural tendencies. Of course, if you're a Russian, you want everything to have Russian names.

Harkewicz: So there's a certain amount of international agreement then?

---

[65] General Bathymetric Chart of the Oceans. The GEBCO Subcommittee of Undersea Feature Names is abbreviated SCUFN.
[66] Albert I, Prince of Monaco (Albert Honoré Charles Grimaldi, 1848-1922).
[67] Bruce C. Heezen (1924-1977).

Fisher: There is agreement and there is disagreement, and there are national feelings that come up. For example, when two of the Lamont people were doing a GEBCO chart in the South Atlantic, way back many, many years ago, they "named" a fracture zone *Mandela Fracture Zone*; this was back in the eighties. This story you may want to delete, you may not. I was chair in '84, but it sort of slipped in and it got on the GEBCO chart and it got printed, *Mandela Fracture Zone*. When I went to Cape Town for meetings in 1986, and I was there in the laboratory at a University of the Witwatersrand meeting, the hydrographer of the South African Navy came in full uniform, with all his decorations, to meet me that morning. He was very impressive. Complete costume. And he said that he wanted to register a formal protest on behalf of the South African government, that this feature had appeared on GEBCO charts as *Mandela Fracture Zone*, and wouldn't we revise it, wouldn't we take it off? I said, "We'd do what we could on that particular thing" because it didn't really fit, actually. You name them for various reasons. You try to name them, ordinarily, for people who are significant but dead. In other words, who'd truly done exploration, or you name topographic features for ships, and so on. And Mandela was definitely a political choice, just like the same young people had thought it was very cute to "name" a seamount for Che Guevara. And of course, other people became very upset about that. Well, arguably, features aren't really named until the official committees agree to it for their gazetteers, now digital databases, so you can take it off later printings. So anyway, the South African hydrographer was making this protest. We couldn't do much about it at the time. And then, ironically, time has overtaken the situation. Nelson Mandela became president of South Africa and there is no problem now with people looking at Mandela Fracture Zone, except that he's still alive. But if he were not alive, why there'd be no problem. And since he is alive, we're still glad for him. [*Laugh*]

Harkewicz: But, there's a *Bob Fisher Ridge*, correct?

Fisher: Where did you get that?

Harkewicz: I'm not sure where I got it. I think one of the press releases in your biographical file.[68]

Fisher: Well, that may be what happened there, after many, many years working in the Indian Ocean. That particular feature is south of Madagascar and with the shallowest part of it at about 1,500 meters. The scale of the feature is very impressive—if it were on land, it would be a tourist attraction because it's about twice as long as the Sierra Nevada and at least as high. But on the other hand, on the seafloor nobody knows about it. But anyway, the International Committee decided to name it after me—they thought it was nice to do.

[68] Biographical Information Files, "Robert L. Fisher." Scripps Institution of Oceanography Archives, UC San Diego.

Harkewicz: And how do you feel about it?

Fisher: I had nothing to do with it, nothing whatever to do about it.

Harkewicz: But, what does it feel like to have some physical feature named after you?

Fisher: Well, many people have biological species named after them. Some people really seek having something named after them. I didn't. And the reason it's "Bob Fisher," is because, quite frankly, "Robert L. Fisher Ridge" would be a bit pompous.

Harkewicz: But, there's never a question about what the feature is?

Fisher: We'd be much better off talking about the several thousand other names that have nothing to do with me. This particular committee, SCUFN, is the designated "international committee." Now, people don't have to abide by it. In other words, if you were rich enough to publish all your own charts you could name all these features for your children, but it wouldn't be helping anything. Harmon Craig[69] could have referred to the street in front of his house as Craig Boulevard and that name would thunder from one end of his house to the other, but it wouldn't really matter much to the rest of the world.

Harkewicz: I see.

Fisher: You see?

Harkewicz: Yes. But is there a problem with describing a seamount as a seamount—

Fisher: There, yes.

Harkewicz: —or naming it a seamount?

Fisher: Yes. Yes, I see. That's a whole different side of it—in other words, you can't blandly rule and say something has to be so high to be a seamount, but we still try to do that. For example, a seamount should be at least a thousand meters high, with its summit deeper than a bank's. Now that we have something called SeaBeam, all sorts of pimples on the ocean's bottom get called "seamounts," and they are just little features. But as to whether it's a seamount, or a sea knoll, or something of that sort, no, there are "definitions" that are not universally accepted. But when there is a set of definitions, people then know what you're talking about; dictionaries are good.

[69] Harmon Craig (1926-2003).

Harkewicz: Well, I didn't mean to get us off on another weird tangent here; I know we have to talk about your involvement with the INTERNATIONAL INDIAN OCEAN EXPEDITION.

Fisher: All right. Yes.

Harkewicz: And, I know that that probably is like a five-hour interview, at least, but can you tell me about the expedition? I understand that it started in 1960; did it run from 1960 to '65?

Fisher: Well, the INTERNATIONAL INDIAN OCEAN EXPEDITION (IIOE), 1960-1965, which was a loose thing, basically, was dreamed of and first put together at the 1959 meeting at the United Nations, the first International Oceanographic Congress, and that was by Revelle, and the SCOR[70] group. SCOR initially back then was the "five wise men." There were people who sat there as advisers, and I was there as one of those. Revelle had me there because of my work in the Southeast Pacific. The IGY was eighteen months, just before that, and during the IGY countries around the world learned how to cooperate scientifically, and how to trust each other in a large way. People have written Ph.D. theses about that sort of collaboration and why. Okay. So, we asked, "Well, what area is far enough from everybody, everybody of significance in terms of A-bombs and so on, that we could collaborate and study on? Well, how about the Indian Ocean?" You see, that's not in anybody's backyard. And so ideas were put together as to why that should be studied, from a sociological point of view. That was one time where that question did come up. But just where and what were we interested in studying? People from around the world could agree to collaborate on a study of the Indian Ocean. It was primarily the so-called western nations. We collaborated with the Russians (then as Soviets), and there were joint committees in terms of international composition, and there also were national committees. I mean, various specialists were chosen to chair different panels. And there were four or five main programs which had evolved based on the type of studies that were done in the IGY, the collaboration. The key word in all of this is collaboration. And so we planned operations as to how to best use the facilities from different countries, in terms of time and type of ship, and interests of people, with all of that to put it together. That's the reason that would be a long interview, because of the different philosophies involved. You look at a chart of what different ships did and you could immediately recognize Scripps ships. You could immediately recognize what work was done by Soviet ships, for example. The philosophies of exploration, of finding out, of making observations, were just so completely different, and the factors that decided how people would do them, and how stereotyped they were, how rigid they were, how freeform they were. You could just tell, lab by lab and country by country, the differences.

Harkewicz: Can you give some examples of that?

[70] Scientific Committee on Ocean Research.

Fisher: Well, yes, I can. An easy example, one of the principal names that come from the German work after World War I in the *Meteor* expeditions in the South Atlantic, a man named Georg Wüst,[71] a noted chemist, and he was still alive by the beginning of the Indian Ocean program. I later met his sister in Cape Town. I never met him. Yes, I did meet him at committee meetings, come to think of it, in the fifties, sixties. Anyway, he became involved and he was a "great man." He was listened to in many ways. So, not only in the Physical Oceanography group he wanted the pattern of the Indian Ocean study to be very much like *Meteor*. North-south-east-west—in other words, the way a physical oceanographer, a classical physical oceanographer, would want it done. Well, people such as Fisher and Lamont's Manik Talwani[72] in this country, and the British Tony Laughton,[73] and some of these others, we tended to think as geologists that geology doesn't evolve in grids. Water movement you may study by doing grids, at least at that time. But in terms of geology, you want to trace out something, a relief-type of feature. Let's say a trench, or whatever. Or, in doing geophysics, it wouldn't do any good, for example, for Russ Raitt to go off and do one station every two hundred miles on a perfect grid and say, "This is what the Indian Ocean is like." So what you do is you find that feature, whether it's a trench, a fracture zone, or whatever, and you map it. You find out where it is and then you guess, "Why is it like that?" and you develop it, you keep building. That's what I said to you earlier. These are evolutionary things. They are not clarified by Brownian maneuvers, even though some of the tracks look like it. So Scripps participated in all four phases, but most strongly in geology/geophysics, partly because I, and several others, was involved in that and we got into it very early.

Harkewicz: So, were the Russians then more interested in doing the grid-like thing you were talking about?

Fisher: The Russians were doing more grid-like stuff. Later on, in terms of biology and physical oceanography they were definitely doing that. So you could look at tracks: the Russian trips sometimes evolved into large zigzags. It's very much the analogy I sometimes use in this philosophy, and I'm talking to you on the philosophy of expeditioneering: it's like a man with a bird dog. And in a sense the physical oceanographer is the man trudging across the field doing this. Now, if physical oceanographers were here they would laugh and mock me right now because they'd say, "Well, we go out and we do all the variations . . ." and they sometimes do. But in those years, the idea of the grid pattern, as you have with MLR and all of that, was exactly the way to do it. But today, even in biology, you may be following a particular feature, much more the type of thinking we did. You see, in biology, for example, then a team would go out and sample the ocean

[71] Georg Adolf Otto Wüst (1890-1977).
[72] Manik Talwani (1933-).
[73] Sir Anthony Laughton (1927-).

on a grid just to get a distribution of what different kinds of animals were in different latitudes.

Harkewicz: So, you keep talking about international collaboration, but was the collaboration on specific ships? I mean, were there Russian scientists on Scripps and Scripps scientists on Russian ships?

Fisher: Yes. For example, for some Scripps people not on our IIOE vessels, the British had a program. There were, I think, more than twenty countries and about sixty ships overall, so it was a big operation. But the South Africans would have maybe one or two North Americans who wanted to study just where the South African ship was going to be working. So that's how you went on that ship. But in our work, for example, on the IIOE we decided that we should train "UNESCO[74] Fellows" from that part of the world. So on MONSOON, which was Scripps' first IIOE cruise there on *Argo* in 1960-61, and the first time Scripps worked in the Indian Ocean, we had a Pakistani Fellow, Mohammad Islam,[75] a navy officer. We had a Jewish Fellow, Gidon Almagor. And, we had an Indian Fellow, Kolla Venkateratnam, who then became a graduate student at Lamont. Once aboard, they got interested. At least the Indian got interested and has made a marine geological career out of it. But, the U.N. also set up a training program ashore, and the U.N. was deeply involved in this overall collaboration program. As in most cases with initial funding, you early had an official international coordinator and he very soon became a figure of fun. People went off and voted with their feet and did what they were scientifically interested in.

Harkewicz: Was there any thought—you said that the United Nations was involved and I talked with Douglas Inman about this a bit in his interview—the whole idea of science being a way to make bridges between countries?

Fisher: Well, of course there was that. And also, in terms of the Indian Ocean, one of the people still here at Scripps spent a good part of his life, several years, in Southern India, running a [training] program, part of IIOE. That's Ed Brinton. I don't know that you're going to be speaking to him, but he was based there in Cochin, directing the sorting lab for the biological program. Some other Scripps people did similar service. In other words, there was "what's good for society in the Indian Ocean area?" And most of us learned to say that the Indian Ocean effort was to teach that part of the world where to get food from the sea. But the problem with that is that in that part of the world Indonesia, of course, lives off the sea, but India doesn't. So, you didn't really get the idea that you were going to teach them this. When Roger, who later was very much involved with India, would speak early about the IIOE he would always say, "We're going to find out, to teach them to eat rastrelliger." So, we learned to say, "Part of the expedition's

[74] United Nations Educational, Scientific, and Cultural Organization.
[75] Mohammad R. Islam.

job is to teach them to eat *Rastrelliger*." But, I don't know what a rastrelliger is. [76] [*Laugh*]

Harkewicz: So do you think that this was sort of a sociological front, so to speak, so that science could go out and learn something?

Fisher: But that sounds so cynical. But in terms of the advantage from including geology/geophysics, you might find some seamounts that were places of congregation for not only sharks but also for fish and that would help their fisheries. Most of us didn't believe that you were going to find diamonds in the Indian Ocean, gold, and all of that; all of these came up in the newspapers' operation.[77] The IIOE, if anything was sold upon the resources of that area, primarily of the biology. And in geology/geophysics around the world then, we weren't really talking about that. Now, what we were finding was areas where manganese nodules were common, but that came incidentally, with the dredging. So, that was a fallout of it. And then along came the "Law of the Sea" that used a lot of that "resource" material. And today, a lot of the work from the IIOE 1960-1965 and its follow-ups in terms of topography is being used by the sea lawyers of the Law of the Sea to work out what these boundaries should be. Except it's become much more hard-nosed. At that time, people could "claim" all sorts of things and now there are formal committees fighting over wording.

Harkewicz: So, how did you go about planning this type of a cruise? My understanding is, and you sort of said this before, was that nobody knew anything about the Indian Ocean, and that was the advantage of going there. But, how do you plan, "Well, we're going to go here to do, looking for this" if you have no idea? As sort of like a blank slate?

Fisher: Well, all right. Let me speak organizationally. Early on, by 1959, one of the first things that happened there was a man, and I won't mention his name, who was named the international coordinator. He happened to be an American. He'd previously taken care of other such projects as this. So, we had an operative. He was going to get "collaboration." Meanwhile, most of us already knew people in a lot of these countries. Not completely, but some did, or we knew the names. Since the baptized INTERNATIONAL INDIAN OCEAN EXPEDITION of 1960-1965 had SCOR's weight behind it, committees were set up to more or less think about, and decide about, and organize the biological program, the physical oceanographic program, the geological/geophysical program, the monsoon (meteorology) program; Indians, Americans, or French, or British, or Russians, or Australians were chairs or co-chairs of those different groups. I happened to chair the U.S. National Committee on Geology/Geophysics, and I happened to co-chair the International Committee for those; there I was co-chairing with a Russian, P.L. Bezrukov, who was a sedimentologist. We met occasionally, usually at

[76] *Rastrelliger* are mackerel.
[77] Fisher later clarified that he meant to say newspapers' fervid operation.

UNESCO in Paris, or we would meet—and this started in ’60, ’61, ’62, or ’63—with the British, Tony (now Sir Anthony) Laughton, my longtime friend of fifty years, was very busy in the Indian Ocean Expedition. But the British worked out of Mombasa—Arabian Sea, in the western Indian Ocean. Scripps tended to work throughout, as did Lamont. The Australians chose to work off both their coasts, but primarily right there out of Fremantle, or out of Darwin. And the Indians had a naval ship that didn’t do much of anything. The U.S. biological program chartered the old Fleischmann (yeast) yacht *Te Vega* from Newport Beach harbor, got it there and sailed it, and it was almost useless. You know, a hundred and fifty-foot yacht for doing oceanographic work was a drop back to the days of *HMS Challenger* almost, except not as good as *Challenger* in the 1870s. Biological sampling programs could use that type of facility. Later on the U.S. program very cleverly got the ex-presidential yacht, *Williamsburg*, and took it over and did their biological sampling from that for a cruise or two, before giving it to India for research with Gene LaFond. Meanwhile, the Australians were using their two hydrographic ships. There were a couple of U.S. Navy or Coast and Geodetic survey ships that went over through there, incidentally just basically to show the flag. And we did have Navy participants who would come in, but they were using primarily a Woods Hole ship, *Chain*. WHOI did several cruises; we did several cruises, as did Lamont. And there were people from other U.S. labs that were aboard from time to time, or from other countries, depending on their expertise. I happened to run the overall IIOE program for Scripps, and report. So these are just the different committees. You can see how the lines were drawn among these and how we got to know each other, and evolve, and our interests. So that the whole thing was just bubbling around; in the United States, ONR and NSF in those years, starting about 1960, were jointly more or less equally supporting the Scripps IIOE program.

Harkewicz: Okay. That was my next question.

Fisher: At the so-called close of IIOE, about 1966, by then we at SIO knew better what we really wanted to go there to study, and then later on along came the DSDP[78] program. I was co-chief on *Glomar Challenger* Leg 24, in 1972, the first time in the Indian Ocean there had been several legs there. So, we went back to use the drilling techniques to further study some of the things that we had more or less seen a shadow of earlier.

Harkewicz: I see.

Fisher: So, that the evolution. There are still expeditions going on out there.

Harkewicz: I know that in 2000 the forty-year compilation of topographic interpretation of the greater Indian Ocean was published. That was your topographical data, I guess.

[78] Deep Sea Drilling Project.

But, I was curious about—I had interviewed Jacqueline Mammerickx,[79] and I wondered if your work was similar to hers, or if not where it might have differed? I mean, as far as bathymetry goes.

Fisher: Well, Jacqueline Mammerickx was, rather is, a geomorphologist, and she made such charts. Her mission was very similar to that of a dedicated woman from Lamont, who recently passed away and has gotten a great deal of publicity, you may know her, Marie Tharp.[80] But Jacqueline had a background in geology and she also had a contact she spent a good deal of time with, namely her husband, who is a geologist. So there was direct interest in that type of thing. But, I think that Jacqueline Mammerickx and I are not that similar, except that we both worked in seafloor topography, as did Menard. But then, Menard definitely had some other interests, and I had the structural interest, in terms of shipboard geology-geophysics. Jacqueline put topographic things together in the Pacific, but she stopped earlier. I was much more interested, I think, in terms of the tectonics implications. You look at this map of the Gulf of California on walls around here, and that interpretation was my work—largely based on the results of the 1959 two-ship expedition, VERMILION SEA, with George Shor and these other people, throughout the Gulf of California. And that was one where, again, we were testing methods on shipboard. That integrated program revealed the real seafloor pattern in the Gulf of California, and that's why you see that chart all over Scripps from that particular period. It shows very well what the overall tectonic pattern is. That's what we were trying to do in the collaborative Indian Ocean cruises, the results from that to work out what the tectonics are. Then the drilling program came in to get age dating from the different samples, from the foraminifera and so on, by means of assembling long cores. You see, as I was saying in the analogy: there the physical oceanographer was the man with his gun, and the geologists are the bird dogs that are ranging all over following things. That's of course what was done with the drilling ship; the man trudging from place to place, selectively, based on what we had bird-dogged.

Harkewicz: But were your techniques similar in the drawings that you did or would it not be similar because you were looking for different things?

Fisher: Well, you can, as one of our South Africans, now dead, used to say, "You know, you can sit across the room and know whether Fisher did this or Jacqueline Mammerickx did this: Bob likes cabbages, she likes meat cleavers."

Harkewicz: Okay.

Fisher: But, on the other hand, it would be one thing if we really knew what it looked like and could do it. Ultimately, if one drained an ocean and flew over it and did a topographic chart, it would be identical whether Fisher made it or Jacqueline

[79] Jacqueline Mammerickx (1935-). She is married to Edward (Jerry) L. Winterer (1925-).
[80] Marie Tharp (1920-2006).

Mammerickx made it. But you identify a Jacqueline Mammerickx's chart, for example, you equally could say Marie Tharp, you could say Bruce Heezen, you could say Bill Menard, you could say Bob Fisher. You can tell from what their philosophy was, what their background was, what their prejudices are, and so on.

Harkewicz: Really?

Fisher: Yes.

Harkewicz: Interesting.

Fisher: You don't see that on land, where you fly over it and take stereo pictures and make a topographically-precise contour map. (And, with SeaBeam work, if you did the same you tend to get a very similar picture because there's much, much more detail.) It's just that when Jacqueline Mammerickx was doing most of her Pacific work it was about twenty percent data and eighty percent judgment, more than where I was doing most of mine. No, that wasn't quite true. It was probably fifty-fifty.

Harkewicz: All right, then, that brings up the next question, too. So, do you think if you two, or three, or four, or how many people you were talking about before were drawing these things today with the technologies that are involved today, they would be more similar then?

Fisher: They would. Oh, of course. Well, these days we wouldn't even be drawing them. Very few people are doing that. Government agencies do it for special jobs. But that was the beautiful thing. We were using our smarts, our individual smarts, to say "What does it probably look like?" For example, when Menard described me he talked about my contouring as "bold." In other words, I was trying to push it. And the thing that was nice is that it usually came true to be fairly close to that.

Harkewicz: Well, what do you think about satellite usage for charts?

Fisher: Oh, the satellite altimetry is a very, very useful thing to get a good picture. I always tend to describe it as "heaven-sent" or "post-impressionist" topography. But its proponents call it "predicted bathymetry;" i.e. "depth measurement." I sound like I'm prejudiced. What I'm saying is, it's very useful to get a picture, and in detail it's very wrong when you try to determine, "What are the actual depths?" It often is very bad. The more you use it, say by a geologist such as Joe Curray or myself and some others, the more you recognize the inadequacies. Even so, you are impressed with how wonderful it looks when you see it on an overall plot. In terms of giving you ideas, general shapes, and so on.

Harkewicz: But is that the technique that's being used now for it?

Fisher: That's being used almost exclusively because it's a very easy thing to do.

Harkewicz: And so, is something being lost?

Fisher: No. Well, I mean, what is being lost is having experienced people taking the time to sit there and really do it in detail, reasoning geologically, and having to do it that way. (Similarly, with SeaBeam anyone can go out and, as the expression is, "mow the sea floor." And if you have good people reducing data, you come up with a very beautiful picture of it.) It's just that the quantitative values may not be as correct as – in other words, you see a plot of the satellite altimetry tracks and it's black, because for about every mile there is one, and that is very useful to get the overall picture. But in the physics of the thing certain entities would never show up and others are the wrong depth. But the argument is, "But you still know ninety-eight percent of the stuff, because we've saturated it." In terms of what we were doing earlier, when you were using your geological smarts to guess what should probably be connected to what among the various choices, it didn't come out of the machine.

Harkewicz: So that's why it was much more personal, then?

Fisher: Well, it was a much more personal thing. But in that particular area, I mean, we are dinosaurs.

Harkewicz: It seems sort of sad in a sense, though.

Fisher: Well, it is and it isn't. You know, the point is, today, you are probably talking to the last gasp of one of the last dinosaurs. [*Laugh*]

Harkewicz: But tell about this book that you published, though, this forty-year compilation.

Fisher: That's a misunderstanding. When I came to Scripps I was interested to find out what the seafloor was like, to use for our expeditions. And over time you built up a database, but not just from Scripps ships, from all sources. And depending on your contacts, on your collaboration, on your patience, on your stick-to-itiveness, on your just plain boring dullness, you put these little numbers together. Today you don't have to do it quite that way. But, in the fifties, sixties, and seventies you did, but you had friendships, or agreements with people, about trading the data. For example, "I'm going to go to such and such an area. What have you done there and can you give me what you know so I can put it with what I know so that when I come back, we will have found out everything that we can probably know, or at least I won't waste the ship." You see? So, bit-by-bit these things add up and, of course, you can see why the Navy would be interested in your work, to make a discovery of where there's a certain seamount in a part of the world that's of very great interest to the Navy. So, those things gradually build up, aided by the contacts that you've made.

You spoke about a forty-year compilation. That's something like a newspaper would quote. The Indian Ocean overall is an area where we were interested in getting everything together and finding out definitively what that part of the seafloor is like, and what does it mean in terms of A, B, C, D, E, F, G? And after a while, with all that time, and with the help of people who are very good with computers, and having means to digitize things, you assemble competence for brute-forcedly reducing the pictures. Now, some people can do that superbly, so that again here's where the personality comes in. And, I happen to be fortunate enough to have known one of the real innovators, a man named Andrew Goodwillie, a young Britisher. He's an Oxonian Ph.D., but he's now at Lamont being one of their brainy types. Anyway, doing the things that I am incompetent to do he was able to carry it, and then by collaboration, the British data labs were available to use their printing-out facilities to make these charts. So, what you see bounded is actually the area of approximately one-quarter of the Earth's surface, and the largest part of that is the seafloor. One of the charts you're looking at shows that seafloor. It's a Mercator. So it's very, very misplaced and deceptive. But the other chart, fitting it at the same scale, is the track chart of where ships providing soundings have actually been, and maybe two percent of those are Scripps ships. Two percent or five percent are Russian ships, or whatever. Over the forty years, trying to obtain, to clean up, and take out, and work out, and get the crossings to come out, to get the best possible data set, then I used my own geological topographic experimental smarts to know which things should be connected in which way, probably, from a point of view of geology. What makes sense geologically and so on. And that's what has been done.

Harkewicz: Okay. You know, you've talked a lot in this past two hours about collaboration, and interaction, and things like that as far as scientific goals, and personal career goals and things, do you think that that kind of interaction is still in existence or has it disappeared?

Fisher: Yes and no. For some of us it's a little discouraging to walk into a room and see the backs of heads of six people, most often students, each with his computer cutting the provided pizza into smaller and smaller pieces. Whereas, what you really want to see is the breadth of the pizza being increased, or more flavors. The computer can count on its fingers very, very fast, and I'm not very good with it. Thank God there are people like Andrew Goodwillie who can lead me out of the darkness. But yes, it will continue and people are going to find all sorts of new things. I'm asked all the time, have we found all the seamounts? Of course we haven't. My own feeling is that as far as the trenches are concerned, yes, we know their components, the order of their precedence in terms of depth, almost certainly. We know very well that their crustal structures make sense geologically. It looks like, as far as "plate tectonics" is concerned, you can explain these things. But, all we can say is today plate tectonics is miles ahead of anything else. Not necessarily definitive, except that a lot of us, as far as we can

tell, think it is essentially definitive. But, as far as doing more of this work, shall we say, no. We're going to see stuff like satellite altimetry showing seafloor in a general way. But, you see, if you have a thousand lines that are not quite good, I mean a thousand blurred lines are not always as good as two or three meticulously accurate lines. On the other hand, what happens five miles on either side of the meticulous line? That's where you use the judgment. You're getting a lot of philosophy on this. That's what happens when you're my age.

Harkewicz: Yes. I guess, since we have been talking here for a while, that maybe we should sort of wrap this up for today, at least, so that we can think about what we've talked about. But, there are a couple of questions, or three questions actually, that I always finish the interview with that I'd like to ask you. And, first of all I wondered, in your opinion, what do you think has made Scripps succeed?

Fisher: The fact that it studied the ocean itself and people were free to follow what they needed to do, that the directors have, in general, let them do what they needed to do. And so people had confidence to try what they wanted. We've been very fortunate, I think, in that. And one could cite his favorites and I'm sure you could do that, but that's not what we need to do.

What other things do you think you might like to ask about in terms of strengthening it? One of the things that strengthened Scripps is that you had a comparatively superb fleet for a number of years, at Scripps and also Lamont. Lamont scientists were seagoers because of their director and so they've had people out there. Their pattern of going to sea was very different from Scripps. Certainly they went out to study the oceans and they used the same overall tools, similar tools, but the philosophy in which they did it, how they were directed, all of that, was very different. But we had the facilities and we had, at least from time to time, supervision, directorial supervision by people who really loved the sea. It didn't only filter down. It percolated up, as well as that. In my view it percolated up and we had people who went to sea because they really cared to. Now, that doesn't mean it's more significant than people who never go to sea. We had a man here named Carl Eckart,[81] and he didn't need a ship. All he needed was the back of an envelope.

Harkewicz: Do you think the days of going to sea are over?

Fisher: There will always be—in terms of going to sea in the way we did, yes. I think that is largely over. When you're talking about leaving the dock, no, our ships will continue to go to sea. As to whether or not they go to sea with Scripps people, obviously not. The use by Scripps people is down. And I'm not saying that from an embittered thing. I'm just saying that there doesn't seem to be the lure. Over the years, there were people who couldn't not go to sea. They were really excited about it. And now, many of the problems that people are studying, superbly, do

[81] Carl Henry Eckart (1902-1973), SIO director 1948-1950.

not require a fleet. It's a facility that I needed, Russ Raitt needed, George Shor needed, and I'm sure that Peter Lonsdale[82] needs.

Harkewicz: Do you have any guess as to why that might be, that there seems to be this emphasis?

Fisher: Well, I mean they have other interests. The fields have changed a great deal. Scripps in the fifties and sixties and seventies had a very strong emphasis, a very strong group, aging over the years, but evolving from young men and young women, people who were interested in geology, geophysics, tectonics, and all of that. And look, the result of that is some major, major contribution in all of them. Not just at Scripps, but also, and I've emphasized what you just brought up, collaboration. We knew people in other parts of the world. Our present Scripps people working in the labs know people in other parts of the world. They sit there and e-mail and talk to them day and night. I mean, there's that contact. We didn't have those easy contacts. What we had largely were personal face-to-face contacts, or occasionally shipboard, or snail-mail contacts, but it got done. So, yes, it'll still go on. I don't see the impetus, the pressure on people here to want to go to sea and investigate problems at sea. I may be completely wrong. Of our own people, there are exceptions.

Harkewicz: But, before you said that was partially from the directorship, but also that it percolated up—so I guess that would be true, what you're saying then?

Fisher: We have a new director.[83] Let's give him a chance.

Harkewicz: All right.

Fisher: In other words, it's better really, if you're going to talk about the effect of that type of thing. Woods Hole in the thirties, forties, and fifties was the second of the three so-called big ones, and from World War II was led by a man, Columbus Iselin,[84] who loved the ocean, and went to sea, and was a superb yachtsman. He died early. But he collaborated, and he set the pattern for that institution, and, by God, they did it! They went to sea and did it, but largely physical oceanography. Okay, in Revelle's case, he set the pattern and we did it, and it was, in his case, opening his hands and saying, "Stand here and sing." You know, in a way. [*laugh*] I mean, Roger loved to be on the sea but didn't get the chance in the latter part of his life, as he got involved, as you well know, in many other things. But the director of the third lab, Maurice Ewing, loved to go to sea but he was a very, very strong personality. So that was an entirely different pattern of

[82] Peter Frank Lonsdale (1948-).

[83] Anthony (Tony) Douglas-John Haymet (1956-) became director of SIO in 2006.
[84] Columbus O'Donnell Iselin II (1904-1971).

operations. But all three of those institutions in those years, during the forties (Lamont appeared), fifties, sixties, and so on, going to sea was a very large part of research. Now it is not that large a part of "oceanography."

Harkewicz: So, we talked about what you though made Scripps successful. What do you think has or may threaten its success?

Fisher: How can I say, really? Really, young people look back now and they say, "Well, life in the fifties and sixties here was easy because of all that Navy support." Well, you don't have that type of strength anymore. It's an entirely different world. You look at the local TV: when you do see people from Scripps on TV right now, they doing very, very good basic work, but they're not being shown aboard at sea. Somebody has collected some of the samples at sea perhaps, and they're being worked on here by researchers using their special smarts. That may turn out to be just as good.

The other thing that we talked about earlier, touched on, about the effect on society and the outreach-type things, that's an entirely different world. During the days of the expeditions we trained people. We brought people in. We had volunteers, young people, older people, or students from other places, and that was our outreach. But we never knew that word "outreach." That was just the way it was done. And now it is definitely a pattern. When you make a proposal you have to say, "What effect is this going to have on outreach?" We never thought about that.

Harkewicz: You never thought about it specifically, but you did it anyway?

Fisher: We just saw somebody we wanted to have with us and who would be good at it, and was fun to have along. And what I used to say, Laura, before a cruise, I'd say it. It sounds so silly now. I'd say, "Let's go adventuring!" If I went down the hall saying this now they'd say, "What's the matter with him?" you know, [*laugh*] and, "There's the senile set again."

Harkewicz: But unfortunately though, wouldn't you have to say—I mean, not to beat a dead horse or something like that, but if you don't have the money then you have to worry more about adventuring?

Fisher: Of course. That's why we're doing it, we're fitting in. Yes. But the ships are very, very expensive, so you want to use them. But the most, but really the most, expensive use of a library is to have all the books on the shelves. The most expensive use of ships is to have them at the docks. So we either need to have fewer ships or find a way of keeping them active, with good ideas.

Harkewicz: Okay.

Fisher: But God, it's exciting to go to sea and to find "that thing" and to see the changing conditions, and the storms, and everything. I mean it's been a wonderful life. It really has!

Harkewicz: Well then, maybe that answered my last question, which is, what did Scripps mean to you?

Fisher: It meant that, a wonderful life! You and I were talking about the effect on families, and you can find people who write books on that. I'm not going to play penny psychologist. And there are things that happened that you wish hadn't happened that way. But, overall, the people I've met, and seen them doing things, and their ideas, and so on, that's really made for a wonderful time. So yes, it's been good.

Harkewicz: So, I know you still come in here on a regular basis. What kind of things are you working on now?

Fisher: Oh yes, but you know; if I don't come in the SIO world will go right around. [*Laugh*] No, actually what I'm doing is still international committees that I'm involved with, some of this nomenclature stuff. People still come in for advice, to talk about things. And then, I'm curious about what's going on at Scripps.

Harkewicz: Okay.

Fisher: And Scripps is a place where you can see seminars every day of the week, if you really want to.

Harkewicz: That's true. So, we talked about a lot of things, but I'm sure we didn't talk about a lot of others. And, I wondered if there's anything that you wanted to mention, throw into the mix, before we end our conversation today?

Fisher: No. I think that that's probably enough. There are aspects, technical, or things about the evolution of certain theories that became very important. We haven't gone into that. And in terms of in my own particular research, we talked about the trenches. And, that was something that's been very, very satisfying. But, the deep crust/upper mantle petrological work with Celeste Engel, for example, turned out to be very important, very different, and very fortuitous. That's a whole side of it that we haven't touched.

Harkewicz: Okay. Well, thank you for talking with me today.

Fisher: It's been a real pleasure, overall.