Oral History of

Edward Litton (Jerry) Winterer

Interview conducted by Laura Harkewicz

25 April 2006

Copyright © January 2006 by the Regents of the University of California

TABLE OF CONTENTS

ABSTRACT:

Edward Litton (Jerry) Winterer was interviewed in his office in Ritter Hall on the campus of the Scripps Institution of Oceanography (SIO) on April 25, 2006. Winterer was born on February 14, 1925. He received his A.B. (1948), M.A. (1950) and Ph.D. (1954) in geology from the University of California, Los Angeles (UCLA). His doctoral thesis was titled *Geology of Southwestern Ventura Basin, California.* He worked as a geologist with the U.S. Geological Survey from 1949 – 1953. From 1948 – 2004, he received a variety of awards and honors including: Standard Oil Company of California Fellow, Geology, UCLA (1948 - 1950); Fulbright Lecturer, University of Louvain, Belgium (1960 – 1961); Elected Vice President, Geological Society of France (1976); Shepard Medal for Excellence in Marine Geology, Society for Sedimentary Geology (SEPM, 2000); and Elected Fellow, American Geophysical Union (AGU, 2004). In addition, he acted as the chairman of the Graduate Department at Scripps from 1968 – 1972. He also served on various panels and committees of JOIDES (Joint Oceanographic Institutions for Deep Earth Sampling) from 1970 – 1984. He has been on the staff at Scripps since 1963 and has been a Distinguished Research Professor since 1997. In addition, he has participated in, or acted as chief scientist on, over twenty major oceanographic expeditions. The interview stressed Winterer's experiences as a scientist on the Deep Sea Drilling Project (DSDP) from its early days up until the late 1990s (when DSDP had evolved into the Ocean Drilling Program). We also discussed in detail the organization of Scripps, how it has changed over the years, and how it has affected his research and career. In addition, we talked about Winterer's experiences as one-half of a scientific couple working at SIO (he is married to Scripps research geologist Jacqueline Mammerickx, who is now retired).

INTERVIEW HISTORY: The interview took place in the office of Dr. Edward (Jerry) Winterer in Ritter Hall on April 25, 2005. His office featured a table made from a huge slab of stone of the type that Winterer has worked with as a geologist. His office was located near a construction site as well as a well-traveled campus street, therefore the tape contains much construction and delivery noise that can be heard in the background. We talked for approximately two hours. The interview was interrupted twice by conversations between Winterer and his colleagues. The tape was paused once for one of these conversations; the other occurred during a time period when the tape was being turned over.

Laura Harkewicz
Oral Historian, SIO/UCSD
September 7, 2006

Edward Winterer (right) points out ancient rock specimens in core samples retrieved from deep below the floor of the Pacific Ocean during Leg Seventeen of the Deep Sea Drilling Project (DSDP), 1971. At left is John Ewing, senior research associate at Columbia University's Lamont-Doherty Geological Observatory. At center is Ed Maxwell from Sun Oil Company, on loan to DSDP. Winterer and Ewing were co-chief scientists and Maxwell was operations manager of the 52-day scientific cruise. Scripps Institution of Oceanography Archives, UC San Diego.

INTERVIEW WITH EDWARD (JERRY) WINTERER: 25 APRIL 2006

Harkewicz: ##[1] This is April 25, 2006. I'm in the office of Dr. Edward "Jerry" Winterer in Ritter Hall at Scripps Institution of Oceanography. Good morning, Dr. Winterer.

Winterer: Good morning. I'm glad to see you here.

Harkewicz: Glad to be here.

Winterer: I've been looking forward to this.

Harkewicz: Great. All right. What we want to do here, today, is talk a little bit about your experience here at Scripps. So before I get started with that, I wondered, how did you get involved in oceanography in the first place?

Winterer: I got involved with oceanography essentially at Scripps. I didn't come here as an oceanographer. I came here from my previous post at UCLA as a geologist interested in sediments. And I was amazed one day to have a couple of my now Scripps colleagues walk into my office at UCLA and start to recruit me down here. And I had no inkling that this might be in the offing. And so there was no pretense on my part that I knew anything about the ocean except that it was out there somewhere. Well, I know about the ocean in the sense of any scientist or geologist knows about the ocean. But the knowledge was very primitive.

Harkewicz: So, your work at UCLA didn't involve . . .

Winterer: It involved sediments deposited in oceans, but I knew very little about oceans themselves. And so, and I had some problems in mind that I'd been working on at UCLA that made it very attractive for me to come down here where I would have access to the sea and work on those problems in the actual ocean by taking cores in the sea floor, rather than going out to outcroppings of those rocks in the mountains, which I had been doing all along. So that was my initial motivation in coming down here.

Harkewicz: So how were they able to recruit you, then, to come? Just because of your interest? Or . . .

Winterer: Yes, and Los Angeles is a very attractive academic environment. I was very happy there. I had wonderful colleagues. I was working on good problems. Funding in those days—this is a long time ago. This was in the early sixties and National Science Foundation and other sources of funding were not fully geared up yet and so I had relatively small amounts of money. But working on the land

[1] The symbol ## indicates that the tape or a section of the tape has begun or ended. For a guide to tapes see the final page of this transcript.

rather than the huge expenses of going to sea didn't require all that much money. And so I was able to carry on very nicely with what monies were available to me. So that was not a restriction. But the opportunity to be here at Scripps where one had direct access to the sea on ships, and to get out of the physical environment of Los Angeles, which was a terrible smoggy, crowded place where I'd lived for many, many years, was something I looked forward to. So, I thought, San Diego is better than Los Angeles.

Harkewicz: So what was San Diego like then when you first came here?

Winterer: Smaller. In fact, it was small enough so that after I'd been here six months or so my then-fiancée came here and joined me and we were married right away. And we actually looked for houses; as an associate professor I looked for a house in La Jolla to buy. It was possible to think about it, but we couldn't afford a house in La Jolla. The one that we looked at cost—and I may censor this number—was $37,500. That was way beyond what I could pay. And so we chose to go, instead, to Del Mar, which was a distant village up there that was the low-price area. And a lot of students and young faculty members, including John McGowan,[2] lived up there. And so that was the place where young faculty went.

Harkewicz: Well, Del Mar is very nice, too.

Winterer: One of the differences was the economic climate. It was just vastly different. The institution itself, when I got here, was much smaller than it is today. There were a lot of researchers. I don't remember how many we were on the faculty, but there were like sixty or seventy faculty and a like number of research people. We were organized administratively differently than what we are now, but it was a place of extremely varied interests and a lot of contact with others. So that atmosphere, if anything, was a little bit more collegial across disciplines than it is today. We're just enough bigger, and we built buildings and separated ourselves physically, so that it's not quite as possible to talk regularly with people outside one's own discipline in the ocean sciences.

Harkewicz: So you think people got more distant socially or intellectually because of the physical distance that . . .

Winterer: I think that was a contributing factor. Many of us have, who have stayed here on this level of Scripps, down near the sea, take advantage of being able to eat out here at tables on the lawn and see each other regularly. So I have lunch with John McGowan, and a fish guy, and physical oceanographers, and so forth, and we chat over lunch, and a lot of it's just gossip, but, some of it gets pretty heavy oceanographical and we all learn from one another. And I really like that. That was even more possible in earlier times. We were not so spread out and the offices were mixed up, so that your office might be next to somebody in a quite

[2] John Arthur McGowan (1924 -), professor of biological oceanography, SIO.

different discipline. And at first you said, "Gee, I want to be near my disciplinary colleagues," but it turned out that they were only fifty yards down the hall anyway and meanwhile you had a biologist next to you and that was really interesting. You learned from one another.

Harkewicz: Do you think it was strictly growth of the institution, or do you think there was actually a choice, perhaps, by management to separate people into specific buildings for specific disciplines?

Winterer: I'm not sure what all the forces were operating in that direction. And, mind you, there was some division early on. The earth sciences and the biological oceanographers—I'm sure John McGowan told you what that is as distinguished from the more traditional marine biologists that we had lots of as well—the marine biologists were a little bit apart. I mean, you had to walk two hundred feet to see them and they were working on a different set of problems that were not of so much interest to the rest of us that were out there working in the sea. They tended to work on their plants and animals directly and it was nice that they came to the sea, but that wasn't the essential aspect that they were interested in. Their physiology and so forth was interesting, whereas the biological oceanographers were very interested in their relationships to the sea itself, the interrelationship, which the marine geologists were as well. So even then there were separations in viewpoint. And so when they built the Marine Biology Building[3] and marine biologists all went up there, you know, it's up here a hundred and fifty yards, we thought, "Well, that's really nice for them." At least from my point of view, I said, "Well, I have some friends up there but I'm not going to miss talking to them because I don't talk to them anyway."

Harkewicz: Okay.

Winterer: Whereas if John McGowan had gone up there, I would have said, "Gee, that's too bad. He's too far away." And even in those days, we did have some separation. The Institute of Geophysics[4] is up there, rather far away, and it's always been a little bit of a strain for us to be talking and working with those people, just the physical separation. You have to walk up there or they walk down here. Now you can take the bus if you want to. But that worked against close cooperation amongst geologists and geophysicists at that time, and that persists a little bit even today, although we've tried to break down barriers, and even though there's still a physical difficulty we see a lot more of each other now. People have worked at that.

Harkewicz: That was on an individual-type basis?

---

[3] The Marine Biology Building, built in 1976, was later renamed Hubbs Hall after SIO marine biologist Carl Leavitt Hubbs (1894 - 1988).

[4] Cecil H. and Ida M. Green Institute of Geophysics and Planetary Physics (IGPP). The SIO branch of IGPP was dedicated in 1964.

Winterer: No, I think, institutionally we have. We realize, every time we'd get an outside review—from time to time you get a visiting committee—they would remark on this and make us formally aware of a thing that had been nagging us over the years. And so we've worked at trying to break down any separations that might exist.

Harkewicz: But, that was something you think grew over time or was it always there?

Winterer: I think the original organization—well, I wasn't here when IGPP was first organized. I was at UCLA and knew about it and knew people in it. And there was even a branch of IGPP at UCLA. There is at several other campuses, Riverside, for example. So we knew about the organization and I was very close to the IGPP people at UCLA. I was research assistant for one of them for a long while, and they were not apart from us. Whereas here, there was this little bit of physical separation and so I felt it. Also in those days, when I first came here, we had a different structure. I referred to this earlier on, that things are not the same structurally now. Organizationally we were divided into three departments. You possibly know about this already. And I was recruited here, amazingly enough, in the Oceanography Department. And to the extent that the administrators over there in the administration building initially gave me the title of professor of oceanography, and I immediately, when I saw that, had to go down and object that it was hard enough for me to hold up my head to be called a professor of geology. Professor of oceanography would make everybody laugh. And so it was changed to a more suitable title. But in that Oceanography Department it was very multidisciplinary. So there were geologists, biologists, chemists, physical oceanographers, a whole lot of things were in there. It was very multidisciplinary. And that was my introduction to Scripps. I guess that's why I spoke to you of that original atmosphere. And not long after I came here I became some kind of a vice chairman of that department, and so I had formally to deal with people of these different disciplines. Where as for the other two departments, Marine Biology had the character I talked to you about, and there was the Earth Sciences Department, which was organized, I can't remember, in the very late fifties just before I came here and had a very distinguished faculty, several National Academy members and people of world repute. And I had good contacts with those people but their educational program and their interests were apart from me. And so there was not as much contact between the geologists in the Oceanography Department and the geologists and geophysicists in the Earth Sciences Department. Okay, and that was a bit of a difficulty . . . .

Harkewicz: It does seem rather odd.

Winterer: Yes. But individually I worked with those people and we went to sea together, and so it really wasn't bad because it was not quite right to split disciplines like that. The split between the biological oceanographers and the marine biologists I think was a more natural split. The fundamental orientation was different.

But the difference between me and folks over there in Earth Sciences that were doing somewhat similar things was zero. We were just in different departments, that's all.

Harkewicz: So, there wasn't a defined difference, as far as you could tell then?

Winterer: No. Oh, well, according to them, they were better than we. [*Laugh*]

Harkewicz: Oh, okay.

Winterer: But we could sneer back too. So no, there was a little bit of aloofness but I think that comes from the population of Academy members and that sort of thing.

Harkewicz: Were you working on the same kind of problems?

Winterer: Not really. The people in Oceanography included several of us that worked on sediments, and we had very close ties to the research people associated with our department. There was, in those days, almost no distinction at all between the research people and the professorial people. They were equally well funded. Senior research people had a lot bigger labs than junior faculty did, and they were treated in a way completely equivalent, except that they didn't belong to the Academic Senate, but they did participate in instruction. They were lecturers, most of them. They were appointed as lecturers as well as their research titles, gave courses, lectures, and so forth. In every way they were faculty members but there was a little bit of feeling of being second class citizens whereas they deserved first class. And so, there was a little bit of resentment among some of those people—not all, but some—and it made for some difficult times. But we had several sedimentologists in there that were good colleagues and we all worked harmoniously, whereas the—for example, igneous petrology—I don't know how much you know about geology, but people who work on lavas and granite are igneous petrologists, and those who work on sand grains and mud are sedimentary types. And the igneous types were over in the Earth Sciences Department, as I recollect. And it's hard for me to remember who was in which department. But there was some disciplinary separation, but we had geochemists in both departments. There was an overlap, and none of us knew what our—you couldn't say what your mission was as a department. That's very fashionable now.

Harkewicz: Yes. You're right.

Winterer: Mission statements. Distrust them—and they want to have mission statements over here now. My god! [*Laugh*]

Harkewicz: Well, it makes sense to me, but I guess if that's not the way you feel, I guess.

Winterer: No. No. It's like having your logo.

Harkewicz: You'd rather have more free-flowing type exploration?

Winterer: I just think, if you don't know what your mission is in life, then too bad for you. I mean, you just go ahead and do your work. I don't need to know what my mission is. Especially, I don't want someone to invent it for me as a slogan. But at any rate, telling the difference between the two departments was not very easy. The training of students was the thing that marked them most. The Earth Science Department was pretty much an Earth Science Department like you'd find at UCLA, where I'd come from. It was very familiar to me. Or at Berkeley, or at Princeton, or Yale, or wherever. Whereas the Oceanography Department was something quite different in the training of the students and it had been that way for many, many years here at Scripps. The original Scripps students were in that department. There was only one department, Oceanography, and they took courses about the ocean. They had to take courses in physical oceanography, and chemical oceanography, and those things, whereas the earth scientists didn't do that. They were taking courses like mineralogy, and had a straight-line Earth Science Department type curriculum. So the curricula were different in the two departments and it marked our students very well.

Harkewicz: But did students from Earth Sciences go on cruises with students . . .

Winterer: Yes, they did. Not as much as the oceanographers did, but they did go. But many of them simply did their doctoral work on the land. They'd go off to Egypt and pound on rocks, or wherever, whereas the students in the Oceanography Department characteristically went to sea. I was regarded as rather a weirdo because I carried on my land work even up to and including my retirement age. I kept a mixture of land and sea work.

Harkewicz: Covering all areas?

Winterer: Well, I was always working on marine sediments on the land, and in the sea as well, and making comparisons. You see, different aspects of them, in the two different settings. But oceanography students went to sea, whereas earth science students sometimes went to sea, sometimes not. It wasn't an essential thing.

Harkewicz: So how did that change then over time and why?

Winterer: Okay. I may come along and censor some of this later on.

Harkewicz: Okay. That's fine.

Winterer: But there are—I was not here during the crucial moments, but the Earth Sciences Department was a fractious group, strong personalities, and they didn't

all get along together very well. And I was vice chair of the Oceanography Department at a moment, which was about 1967 or thereabouts, when a group in the Earth Sciences Department came to us in Oceanography and said, "What would you think if we joined you and left Earth Sciences? Those people are really hard to get along with. We just are always at loggerheads and there are some strong personalities in there that make our intellectual life not as fruitful and interesting as it should be." And so word got up to the then-director of the institution, Bill Nierenberg.[5] You know enough history to know a little bit about him, and Bill thought that was a bad idea and suggested—and maybe the opinion came from others as well—that there should be a big unification. I always called it the *anschluss,*[6] you know. That's a Hitlerian term.

Harkewicz: I see. It doesn't sound too positive.

Winterer: No, when Germany absorbed Austria, that was called the *Anschluss*. Does your family history include some of that?

Harkewicz: No. Well, not to my knowledge.

Winterer: Okay. It means "the joining together."

Harkewicz: Well, that's a more positive way of looking at it.

Winterer: Yes, but it has, it has a political connotation as well: "I'm going to absorb you." So there was this great conjoining then of the three separate departments, Oceanography, Earth Sciences, and Marine Biology, into one department, which didn't even have a name. It was just the Department of the Scripps Institution of Oceanography, and nobody quite knew how to label it, the SIO Department, or whatever. And, so it came into being whilst I was on sabbatical, my first sabbatical from here.

Harkewicz: Oh, great.

Winterer: And I was off in the middle of Australia pounding on rocks, real rocks, not even marine sediments. These were ancient glacial sediments, which interested me at the moment, and having a wonderful time down there. I took my family and we were living in Adelaide, and I was out in the outback amongst the kangaroos. And I got a phone call from Bill Nierenberg saying, "We have done this. We now are operating as a single department." I think Ed Goldberg[7] might have been the first chairman. You know about Professor Goldberg? He's now retired.

---

[5] William Aaron Nierenberg (1919 – 2000), SIO director, 1965 – 1986.
[6] The *Anschluss* was the annexation of Austria into the German Reich through legislation announced by Adolf Hitler on March 13, 1938, one day after troops of the German Wehrmacht and the SS crossed the German-Austrian border.
[7] Edward D. Goldberg (1921 -), professor of chemistry at SIO.

Harkewicz: Somewhat, yes.

Winterer: It might have been Warren Wooster[8] of Physical Oceanography, but there was a beginning chair and Bill said, "Would you consider being chairman of this new department?" And I thought: well, I had an answer. I said, "Bill, there's been some sort of a mix-up and I haven't been getting my paychecks. If you could make sure, immediately, that I get paid here in Australia I'll consider that very positively." So, he did. He said, "I guarantee it Jerry, you will be paid."

Harkewicz: Used your leverage there?

Winterer: So I came back and was one of the early chairs of the department. I wasn't the first. I stayed there for several years and well, helped get the thing rolling. And it has worked, I think, very well, that new organization. And, we were still not spread out so much in all these buildings, so over time the physical oceanographers all moved up on the hill and the marine biologists moved over there. And the rest of us stayed here. And so the core of my world is still here but there are these outlying groups. So the marine biologists are away from us and the physical oceanographers are mainly away from us. They're up there. And when the Climate Group and all the atmospheric types got here, they also went up the hill. And so I see them very seldom. In fact, I see my Climate guys more when I go up once a week to eat at the Faculty Club. That's one of my opportunities to chat with Climate people, over lunch. Because they don't appear here on the lawn.

Harkewicz: So what was the point of this unification if everybody really just sort of drifted farther apart?

Winterer: I think the separateness was regarded finally by everyone as being overly divisive, and we didn't appreciate quite how the new unified system has worked. And in fact, what evolved out of that was a system we call curricular groups, which are tightly disciplinary, so that there is an Earth Science Curricular Group, there's a Geophysics Curricular Group, there's a Biological Oceanography Group. There are six or seven of these disciplinary groups and they have purview over the teaching. They design courses, teach them, and set their requirements about prerequisites, and do the examinations, and that sort of thing. Overlaid on that, if you can imagine this structure—now, you probably looked at the table?—there is a thing called a research division that always existed. I used to belong to the Oceanography Department and the Oceanic Research Division. When I was at UCLA I belonged to the Geology Department. It changed its name from time to time in accordance to the fashions. It may be called Earth Sciences or Earth and Space Sciences or something but it's the same. And the research aspect of it was carried on exactly coincident with the department. The department did research, it did

[8] Warren Scriver Wooster (1921 -), Scripps oceanographer, currently professor emeritus in the School of Marine Affairs, University of Washington.

teaching. It was all one as it is in the upper campus departments. The Physics Department does not have a Physics Research Division. It's just the Physics Department and they do research. Well, we separated the two things here, partly because we have so many research people that are not on the faculty and some of them have a vanishingly small connection with the teaching program. Most of them have a connection but some don't. And to take care of all those people and their activities and administer their research grants, because that's an important thing—we submit grants, we try to get the money, and when the money comes there has to be somebody to administer it. And so we have an office up here on the next floor of half a dozen people that administer my research division. The Department of the Scripps Institution of Oceanography is over there in another building and they're exclusively concerned with the students as students in courses. So they admit them. Although the admission is controlled by the curricular groups: the geologists will admit so many students and the department accepts them. Then the department is also the place where faculty members get reviewed to get promoted. Okay? So, it's an intricate . . .

Harkewicz: Very complicated?

Winterer: Yes. It really is. It's stupid, in my estimation. It's unnecessarily complicated. I come from the traditional University of California way of dividing up the university. Besides having schools, like a School of Medicine or a School of Law, it has departments, and departments are it. And department chairs used to be appointed by the president of the university, and they are the glue that holds things together. There is the Academic Senate, which is all the professorial staff, and we have shared governance and the Academic Senate participates in the governance of the university through all kinds of means. And then there's the administration: the chancellor and all these factotums up there that handle I don't know what traffic, and all the bookkeeping and everything. And the crossover point is the chairman of the department. The chairman of the department is appointed by the administration but he is the leader, academic leader, of the department. And so he is the pivotal point in the thing and that's a very important post. And here it's all scrambled up. When I was department chair I had—the only money I had was to pay the secretarial staff, period. Zero. That was it. And no resources.

Harkewicz: So, then what were you chair over?

Winterer: I had great responsibilities, but I had not resources.

Harkewicz: I see. Okay.

Winterer: And I think that's more or less still true. So, our byzantine organization, I think, works, but it works partly on tradition and good will, and nobody deeply questioning it. But that was the result of the *anschluss* and it stuck with us ever since. And it gets modified a little bit from time to time but it's not going to

change, I think. It's the way we operate now. So, I don't think that our, the "*anschluss,*" resulted in closer intellectual ties amongst disciplinary groups. I think it had little effect on that one way or the other, although people visualized it as a grand kind of pooling. And, I think, the spatial arrangements had as much as anything to do with separations.

Harkewicz: What happened to the people from Earth Sciences and Geology that wanted to come down here . . .

Winterer: Well, they were—oh, you guessed who they were.

Harkewicz: No, I just wondered what happened to them? They wanted to come here and they were told they couldn't? So . . .

Winterer: No. No. They were in the *anschluss* then, and so we all became one department. In other words, there . . .

Harkewicz: Yes, but you said that the *anschluss*—so that sounds more like almost a . . .

Winterer: But they were, their difficult colleagues were diluted now.

Harkewicz: I see.

Winterer: Yes. So the pH changed.

Harkewicz: I see.

Winterer: And it wasn't as much as an acidic environment.

Harkewicz: That's a good metaphor. I like that.

Winterer: So, it has worked out. We still have the separation problem with IGPP, and we have now been smashed into one research division, so that we're no longer Geological and Geophysical, I think. I don't quite know how it worked, but there is a—and I don't have to because I'm emeritus now. And I don't have to think about most of those problems. In fact, I should tell you that each department on the campus can vote internally about the role that emeritus people have in departmental affairs. At Scripps, I don't even know when there's a departmental meeting. They do not notice me. I am forbidden to attend, and I'm not told when and where there is a meeting.

Harkewicz: Is that good or bad?

Winterer: I don't have to go to meetings. [*Laugh*] And they obviously can do without my advice. Now, in my division, which is an entirely different organization, it includes the research people as well as the faculty, I can go to the meetings and

speak. I have privileges of the floor. I mean, that sounds like a very formal thing like Daniel Webster standing up. [*Laugh*] But no, it's just I can speak up, express an opinion, but I can't vote. And that's fair enough. You know, I'm emeritus. I shouldn't. But, so practices vary all over the campus on that. Some departments, the emeritus people are very welcome. And when I was a beginning professor at UCLA, assistant professors didn't get to go to many department meetings—if it was too important, just as we couldn't teach graduate courses. We could teach only undergraduates. And there was a title of instructor. I never was one of those. And instructors could teach only lower division courses. So there was a real hierarchy. Well, we're sort of running this subject down.

Harkewicz: Yes . . .

Winterer: Well, I got it off the track.

Harkewicz: Well, no. No. I think it's interesting. It's very confusing, but it's very interesting and I guess what I wondered was two things. First of all, I know you don't have a lot of experience in other places, except for UCLA. So I guess the two questions are: "Has Scripps gone through more departmental changes than other places?" And the other question is, from what you know about other oceanographic institutions: "Do they have similar problems or is this unique to Scripps?"

Winterer: No, it's not unique to Scripps. Our main comparison institutions are the Woods Hole Oceanographic Institution and the Lamont Observatory. They keep changing their name. They observe something. I don't know what it is. And, they have had somewhat similar divisions even expressed in the physical location of the scientists and various buildings. And I have spent sabbatical time, or at least a summer, at Lamont and watched how they worked, and I couldn't see that they were all that different from us. Although they had a much more cohesive group of scientists than we did. They didn't, they had some biologists but there were minor, minor, minor, and so they were essentially an Earth Science Group. Woods Hole is much more like us. They are our counterpart, and I know only the Earth Science part of Woods Hole. That's where my colleagues are. And I used to go there frequently on committee business but I haven't been back there in ten years now. So I don't know how it's functioning. But I can imagine that they have problems just like ours, but I don't know. And they have complicated relationships with MIT. They have a joint program and I don't know how that works. It's very complicated business.

Harkewicz: Do you think some of the problem is the affiliation with UCSD? Or is that just another complication?

Winterer: No, it has enriched and complicated our lives, a lot. I mean, we were alone for a long while we were down here. There wasn't even our own Senate down here.

It was part of UCLA. When I was a young assistant professor at UCLA my visits to Scripps were almost always in connection with my being on a doctoral committee, and I'd come down here and sit the exams. My colleague, Joe Curray:[9] I sat on his committee. And he's now emeritus, too. But I'm only about two years older than he is. So, it complicated our lives enormously in a physical sense because we have undergraduate students enrolled in Scripps-taught courses. There is an Earth Science curriculum that even has several tracks, and there are other environmental-science type programs and Scripps faculty are engaged very heavily in undergraduate teaching. We work hard to get credits for the so-called—what are they called—*Penner-ratio points, Penner points*. That's a way of getting credits for what you do. The fact that those undergraduate students were up there, and we're down here, meant that we had to travel up there to teach large classes, because they had classrooms, and they traveled down here for some of their lab classes and smaller classes. And so this physical separation takes time. There was an inadequate bus system and it didn't meet the schedule requirement. So it really, to this day, complicates life. When you have upper campus teaching responsibilities you've got to plan to get up there, find a parking space, and so on. So, yes, that has been trouble. Also, the upper campus people have always regarded us as being really rich down here: we have money to do anything we want. Well, we struggle just as hard for money as they do, but there's this illusion up there that somehow we're just rolling in the stuff… I mean, look how much money it cost to drive those ships around and guys have got all that money, $25,000 a day to move a ship. Gee. So there has always been a kind of natural tension between here and the upper campus departments. And we don't see much of each other. We serve on campus-wide committees. The chairman of the Academic Senate right now is a Scripps person, Bernard Minster,[10] and there have been others in the past. So we make an effort to be represented on upper campus committees on personnel advancement committees, and all kinds of committees on the upper campus. But we don't see those people regularly at all. We don't see them at all.

Harkewicz: How much teaching responsibility have you had over the years?

Winterer: I had—I shouldn't call it "responsibility" but teaching activity I've had. I was accustomed to teaching at UCLA. We had—normal load for an assistant professor up there, we were on the semester system—was three semester courses per semester. I taught six courses a year.

Harkewicz: That's a lot.

Winterer: Including the Saturday field course which kept you out there all day Saturday somewhere in the mountains. It was a lot but we regarded it as normal. I didn't know any different. I was dumb. Here there are people in this department that teach one quarter course per year if—but that's really when they're putting out a

[9] Joseph Ross Curray (1927 -), professor of geology, SIO.
[10] Jean-Bernard H. Minster (1947 -), chairman of academic senate, UCSD; professor of geophysics, SIO.

lot. I remember as department chair having to, I remember turning down a senior professor who hadn't taught a course in three years. I said, "As long as I'm chair of this department you are not going to get an advancement. You've got to show a leg. You've got to teach." And he did. He was a good teacher. So he just needed a little kick. But the teaching loads are generally fairly light here. And in recompense for that the Scripps faculty is expected to be outstanding in research. If you're not doing your research and publishing in leading journals, and being very productive, then you are not doing your job at Scripps. That's what we're supposed to do. And so the teaching loads are a little bit light.

We also have a peculiar thing, and I digress a moment here. There are two types of professors in this university, besides good ones and bad ones, and that sort of thing. There's the ordinary professor called an instruction and research professor, and that's what everybody on the upper campus is, essentially. And then down here at Scripps we also have types that are called organized research professors. And these were people in the research stations, like the Citrus Experiment Station at Riverside, and places—and the astronomers used to be like that. And they didn't teach classes. They just looked through their telescopes or sprayed their lemon trees, or whatever, and we went to sea. Okay? But, in order to attract good people here you gave them a professorial title. It had no formal teaching responsibility in it. And when I was chairman of the department . . . . ##

Harkewicz: ##You were talking about the organized research professors…

Winterer: And we have a number of them. I think we have something on the order of twenty or twenty-five positions, and then a much smaller number, like ten, of the ordinary instruction research. They're called IR billets. And in counting your teaching load, so far as the people on the upper campus are concerned, the chancellor's office, you count only the instruction and research billets. The organized research billets don't have any teaching responsibility at all, formally. Now, they always did participate but what that means is if you have, say, twenty organized research and ten I&R, that everybody has to teach only, essentially, one-third of what a comparable department on the upper campus would be. Okay? Play a numbers game.

Harkewicz: What do you think about that, though, personally? I mean, do you think that's okay?

Winterer: I think it's terrible. I think professors profess. And I come from that tradition. If you're not professing, you're not a professor, you're something else. Okay?

Harkewicz: I see. So, we did get off on, I don't think I'd call it a tangent because it was very interesting but I do have some more specific questions for you. One thing

I wondered about. I know you said you and your wife[11] came here together and I know that you were both here on the faculty together, for a while at least?

Winterer: No. She never was faculty. She was on the research staff.

Harkewicz: Oh, okay.

Winterer: She was in—when we first came here and she was on the research staff she had to be in a different department from mine. In those days it was considered nepotism if two people at the level of research or professor were in the same department. I don't know what we would do, but you could influence the other person's advancement, was the idea. But she was on the research staff. And then after the *anschluss* we were in the same division, the same research division and it didn't make any difference. You just see to it that the spouse or significant other, I guess they call it, never had any access to or influence on the file for advancement.

Harkewicz: Would that mean you weren't allowed to work together?

Winterer: No. We could work together. There was a lot of sort of looking down noses at helping one another too much. The traditional attitudes toward women scientists were in full bloom, still, and so some of my colleagues, were doubtful about the talents of my wife. And so we stayed pretty much apart. I think we wrote one or two papers together, and she was always, I think, hoping that I would be closer to her scientifically. Some couples nowadays here work together very closely and all the time, and nobody thinks anything of it. It's normal. So, those attitudes have changed hugely. So we did not work together much.

Harkewicz: Well you may have already answered this but I want to ask you this anyways because I want to ask her this too. So would you say that her experience of Scripps was different than yours?

Winterer: Yes.

Harkewicz: And how would you elaborate that?

Winterer: Uh . . . [*sigh*], well she had troubles here in the environment, I think, with the usual male attitudes. Some of my colleagues were really just out of a novel, you know, their attitudes. And then she had difficulties in the funding agencies as well at getting sufficient funding, long-term funding. She had a hard time, and she'll explain this to you, I'm sure. What she was doing, which was looking at the tectonic development of the Pacific Ocean, and she did it via making very detailed charts, bathymetric charts, showing the contours of the bottom of the ocean, all the mountain ranges, and canyons, and everything. She was very,

[11] Jacqueline Mammerickx Winterer (1935 -), research geologist at Scripps.

very good at that. Her training was not as a geologist. She's a geomorphologist, which is a person that studies landscapes or seascapes. And so people put her down, as oh, just mapping, you know the drill: "Anybody can do that. You just send a machine out there and draw lines." But there was a lot more to it than that. I mean, when some of her colleagues tried to make maps they made fools of themselves. But it was never fully appreciated, scientifically.

Harkewicz: Do you think if she was a man it would have been more appreciated?

Winterer: Yes. Yes. It was easy to put her down.

Harkewicz: Was her foreignness a problem also?

Winterer: Foreignness, and we had children so it was difficult for her to travel. I mean, she assumed an enormous responsibility in looking after them when they were young. But it was just more difficult for her to participate in the scientific life of her discipline and go to meetings. She did some of that but it was always more difficult for her. Always more difficult.

Harkewicz: Do you think that if she had been at another institution while you were here that might have been different? Was it the time or was it Scripps?

Winterer: I don't know. That's hard for me to know. She did teach down at San Diego State for a while but she didn't like that kind of teaching. She'd done teaching in Belgium. She was a research assistant there and taught some classes. But at San Diego State she did it but didn't enjoy it at all, because, partly because she had to deal with some not-very-bright students, football players. "Ma'am, I just got to have a B in this course. You understand, don't you?" [*Laughter*]

Harkewicz: A little intimidation there, huh? I understand. Yes. So, you were a couple here and you talked about eating lunch with people out here at the, whatever that's called, the Snackropolis[12] or whatever. How much socializing did you do with people here when you first came here, and did it change over time?

Winterer: Socializing scientifically or just socially?

Harkewicz: Well, I know that there's at least stories, of Scripps parties in the sixties and stuff.

Winterer: Yes. The big party era was either happening mainly before I got here or it involved a different subset.

Harkewicz: I see. Was it all an illusion, then?

[12] The Snackropolis is the snack bar located near the Scripps pier on the SIO campus.

Winterer: No, there were parties. We entertained a fair amount, gave dinner parties, and things out in the backyard and that sort of thing, amongst our colleagues—and then I got involved in international committee work, where there'd be meetings here of eighteen people from all over the globe talking about the drill ship, different panels and committees. And so it's traditional that the host institution, the person who's on the committee for the host institution, invites. And so there'd be a whoop-de-doo at my house. So we entertained a lot and that started what, in the late sixties, after we'd been here a few years? Before that we were entertaining mainly just a few colleagues. But Jacqueline, early on, was unaccustomed to American ways of entertaining. And in those days if you got invited to dinner at our house you got invited to dinner, with linen, and candles, and silver, and a carefully prepared, planned meal. Everything just right. And, Americans were intimidated by that. And so . . .

Harkewicz: It wasn't a barbeque out in the backyard?

Winterer: Yes. She just didn't know how to do that.

Harkewicz: I see.

Winterer: And, over the years, in going to other people's houses she's become very accustomed to that and she can put on any kind of an affair and it just goes very well. So she's totally Americanized now. But then, she was very traditional European in her attitudes. And in Europe, in French-speaking society—I can know a French colleague, have an office next to him for twenty-five years, and not even know what part of town he lives in. We can have dinner together. We'll meet in the café and have lunch together, have a restaurant dinner together, the families, but I don't know where he lives.

Harkewicz: Hmm. I see. There's compartmentalization?

Winterer: Well, partly it's the conditions of housing. Ordinary people don't have very good housing and so he doesn't want you to come in to his flat.

Harkewicz: I see.

Winterer: So, it's very different. And she grew up in that tradition. And so we've had a very good social life and made a lot of friends in Del Mar. And our social life consists mostly of people in Del Mar. She's become very political up there.

Harkewicz: Yes. I heard something about that.

Winterer: Yes. Well, she tends to the river valley mainly, but she was a councilwoman and mayor of the town, and different things like that. And so she has another life and our social life revolves around that as much as it does around this.

Harkewicz: Were there any other couples that worked together here? I mean, scientific or—I know sometimes there was women that worked in the office.

Winterer: Got one right now, right across here. Lisa Tauxe and her husband, Hubert Staudigel.[13] People often sail under their maiden name—as my wife did. She never used the name, *Winterer* here at work. She had published under her maiden name in Europe and retained that professional name always. So, I still have people ask me, "Did you ever run across this person called Mammerickx there?" [*Laugh*]

Harkewicz: No. I just see her every night. [*Laugh*] But, at the time when you were working here together there were other couples?

Winterer: Yes, there were a few. Yes, indeed. Not many, but there were a few. In Biology there was, was then and is now, Ralph Lewin and his wife, Lanna Cheng.[14] And again, she kept her maiden name. And my deceased colleague Al Engel worked closely with his wife, Celeste Engel.[15] She was not employed by Scripps. She was an employee of the U.S. Geological Survey, but working here she had an office and a lab, and so forth. And there were others of that kind, but it was unusual. It was unusual, as it is to this day. There aren't that many. Up in IGPP there's a couple, Cathy and Steve Constable[16] that are both there. They don't work together, so far as I know. They do different things. And Lisa Tauxe and her husband, Hubert Staudigel work together on occasion but mainly they do different things. Yes.

Harkewicz: Just curious. So, you mentioned the—I don't want to get too far afield here and not talk about the Deep Sea Drilling Project because I know you've been very active in that. But I know you wrote like a brief history[17] that I read about the Deep Sea Drilling Project and I wondered if maybe you could give me a synopsis of that so we could have it on tape.

Winterer: Okay. When I came here in 1963 there wasn't any such project. There was a little bit of talk about it, but it was pretty vague. And, in 19—I think it was in 1965, before the program really started, there was enough talk about it so that we thought, "Well, why don't we . . ." Well, I guess there had been a thing

---

[13] Lisa Tauxe (1956 -), geologist and professor of geophysics at SIO; Herbert Staudigel, research geologist at Scripps.
[14] Ralph Arnold Lewin (1921 -), professor of marine biology at SIO; Lanna Cheng (1941 -), marine entomologist and research biologist at Scripps.
[15] Albert Edward John Engel (1917 – 1995), research geologist, SIO; Celeste Gilpin Engel (1923 - 2004), SIO igneous petrologist.
[16] Catherine Gwen Constable (1958 -), professor of geophysics, SIO; Steven Constable (1957 -), professor of geophysics, SIO.
[17] Edward L. Winterer, "Scientific Ocean Drilling, from AMSOC to COMPOST," 1998. Biographical Information Files, Scripps Institution of Oceanography Archives, UC San Diego.

called the *Mohole Project,*[18] which was run at Scripps, and they drilled a test hole down in Mexico, the Guadalupe test hole. I'm sure you can read Naomi's[19] writings about that. And Scripps was the one who operated that thing. And I don't think I was even here then. But then in 1965 there was a scheme that we would drill some holes out here off Southern California, and I was named as chairman of the Planning Committee for that. Because, partly because I'd had a lot of experience in Southern California geology. I did my Ph.D. thesis on Southern California geological problems. And so I was named to that. It never got drilled in those days. It was, in fact, stopped by political opposition and the oil companies. They said, "get off my land," like an Arkansas farmer. So then, in about 1968 or so, they established that the Deep Sea Drilling Project as a consortium of four institutions at that time: Lamont, Scripps, Woods Hole, and Miami. Was the brainchild of a guy at Miami, Fritz Koczy.[20] And they set the thing up and we bid for the contract to run it, and got it. And I had very little to do with it. I was on maybe one of the committees, but it was housed in a building up halfway to the aquarium, the Deep Sea Drilling Buildings we called them, and one of my colleagues here, Mel Peterson[21], became the manager of the whole thing, and stopped doing science. A wonderful scientist. He just stopped doing science and went to do that. A terrible thing for science. He was really good. And another colleague, Jerry van Andel,[22] did a lot of the physical planning of things and arranging for the drilling contractor, and all that. That was really heavy, heavy work. And so finally we had a ship and we had technicians and everything, and the ship set sail. I looked at the thing and said, "My god, what an opportunity, scientifically." That was my attitude. "Aren't we lucky that this is starting at this stage of my career? To hell with the projects that I had in mind out here on the borderland, and why I came to Scripps in the first place, to go take some cores of some funny rocks called turbidities. I'll go to sea on this drill ship." So, sometime in 1969 after the project had been going, I think, one year, I went out and started drilling in the Western Pacific and just fell in love with the whole business. I just thought it was incredible. There were small scientific parties. I think there were nine of us on this ship as a scientific party and now there are about thirty. I mean it takes you the first two weeks to learn the peoples' names. And so it was all very exciting at first. We knew almost nothing about what we were going to run into. Mainly, just blank mystery. And so I simply oriented my career thereafter on a study of the kind of

---

[18] For information on the Mohole Project and its relationship to the Deep Sea Drilling Project, see: Arthur E. Maxwell, "An Abridged History of Deep Ocean Drilling," *Oceanus* (Winter 1993/94): 8 -12; and David K. van Keuran, "Breaking New Ground: The Origins of Scientific Ocean Drilling, " in Helen M. Rozwadowski and David K. van Keuran, eds., *The Machine in Neptune's Garden: Historical Perspectives on Technology and the Marine Environment* (Sagamore Beach, MA: Science History Publications, 2004): 183 – 210.

[19] Naomi Oreskes (1958 -), professor of history, UCSD.

[20] Friedrich Franz Koczy (1914 – 1968), Swedish-born oceanographer, chairman of the Division of Physical Sciences, University of Miami.

[21] Melvin N.A. Peterson (1929 – 1995), associate professor of oceanography and director of the International Deep Sea Drilling Project at Scripps.

[22] Tjeerd H. van Andel (1923 -), research geologist, SIO.

sediments that occurred way out there in the main ocean basin, so-called pelagic sediments, far from the land, and just out there. I carried on doing that then for many, many, many years, right through the end of my career. I went on my last drilling leg clear back, I think, in '93 or '94. So whatever that is, it's many, many years of doing it. I think I was out on seven different expeditions doing that. And my interests evolved over time, mind you, but it was the director of my career. That opportunity presented itself. I seized it, associated myself, and worked like hell for the project itself. I was chairman of the Planning Committee and served, spent a lot of time going around to meetings and organizing things and just working like hell for the thing. So I was not only willing to participate as a scientist but it meant enough to me so that I said, "Okay. You got to pay your dues." And so I did.

Harkewicz: Well, in the history that you wrote of the project you said that, "The NSF[23] has spent billions of dollars on it," and "We have received extraordinary value for the money."[24] And I wondered, you were very . . .

Winterer: That's a Briticism, you know.

Harkewicz: You thought they got what they paid for?

Winterer: I think we spent one billion.

Harkewicz: But why do you think that it was worth all the money that they paid into it?

Winterer: Oh, I think that a whole new prospect, scientifically, scientific prospects opened up. We began to understand the oceans and their history in a way that was totally out of our reach before. Just, I mean, the comparison that I used to make, when I'd get interviewed on the radio—you'd come back from an expedition and they stick a microphone in your face—I'd talk about telescopes, that this ship was to the Earth Science community what a telescope was to astronomers. Each group of astronomers had a different set of problems they wanted to look at up there in the sky. Sometimes, very different notions. And, what you did is competed for time on the telescope. I get to look on Mondays and Wednesdays, and you get Thursdays. And so we went out and looked at a number of different very fundamental problems that were completely unavailable to us from any other method. There was just no way. The layers beneath the bottom of the sea, more than, let's say, ten meters down beneath in the mud, were inaccessible to us without drilling. And so we figured out how to drill into the sediments, distances of two kilometers into it, in water depths of several kilometers, so that the ocean basins became essentially accessible to us clear down to the beginning of their history. That is, all the accessible record. You'd drill down to what we call "basement" where rocky crust is. And now we're drilling deep, deep, deep into the rocky crust. Remember, the project

[23] National Science Foundation
[24] See footnote #17.

started out to do nothing but drill that rocky crust. The project was called the Mohole. You were going to drill through the rocky crust down to an important change in physical properties called the *Mohorovičić Discontinuity*, the *Moho*, which Naomi's written about, I'm sure.[25] And I think I mentioned in that article the original Mohole, which involved the same cast of corporate characters that are playing games with us in Iraq, the same ones that are stealing all of our money.[26] But I don't know if I recited any of the particular scientific accomplishments of the drilling project. The first one that was attacked was the very notion of seafloor spreading: they drilled a set of holes across the South Atlantic that literally nailed it. All the predictions came true. They had the magnetic anomalies to start out with. They predicted this, "The rock underneath all these sediments here is going to be 34.5 million years old." Drill a hole, bingo, it's 34.5 million years old. "Okay, let's drill the next hole. That should be 24.2." "Well, 24.2." And so forth. It was just an eye opener. It opened up the whole field of paleooceanography. What was the world like fifty million years ago? All these funny worlds that we have to worry about now because of climate changes. Climate is changing. We know that and we should learn lessons from the past. There have been different kinds of oceans out there that most of the modern climatologists cannot imagine, but we can. We can say, "Look, the ocean used to be very different. For example, the Atlantic was anoxic, no oxygen in the bottom waters. No life down there, zip." Or, clear back in the Precambrian people talk about the frozen ocean in the Precambrian. Pole to pole, right across the equator, the ocean frozen to a depth of one kilometer. I don't happen to believe that story in its rawest form, but there's a whole community that is chasing it and they have a lot of evidence. So, at least with the Deep Sea Drilling Program we can reconstruct ocean history back about 150 million years and learn the states of the ocean and the circulation patterns and how it was. And, that's an enormous advance. So, I think that compared to the NASA[27] Program, in which a billion dollars is just bingo. And a billion dollars is what, two weeks in Iraq?

---

[25] The Mohorovičić Seismic Discontinuity is believed to mark the border between the earth's mantle and the base of its crust. It is named for Croation seismologist Andrija Mohorovičić who, in 1909, discovered that there was an increase in seismic velocities at the base of the crust. For more information see: Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Sciences* (New York: Oxford University Press, 1999): 259.

[26] The Mohole Project was first proposed by members of the American Miscellaneous Society (AMSOC), a combined social and professional group of earth scientists, in 1957. On February 23, 1962, National Science Foundation (NSF) director Alan Waterman chose the Texas company, Brown & Root, Inc. to serve as operations contractor for the Mohole Project. The selection caused a political uproar because Brown & Root had not rated highly in early evaluations of bids and due to the appearance of conflict of interest—Brown & Root company president, George Brown, had close political ties with Vice President Lyndon Johnson as well as to Representative Albert Thomas, a Texan who chaired the House committee that had oversight responsibility over the NSF budget. As Brown & Root proceeded with plans for the Project, cost estimates escalated from $30 million to $127 million. In 1966, skyrocketing costs prompted Congress to pass legislation abolishing the Project. However, the idea for deep sea sedimentary ocean drilling continues in the Deep Sea Drilling Project and its present-day equivalent, the Ocean Drilling Project.

[27] National Aeronautics and Space Administration

Harkewicz: Yes, I know I don't want to get into that. But, . . .

Winterer: But I care about Iraq.

Harkewicz: Yes, I understand.

Winterer: You see that young man in the photo over there in the middle of the wall?

Harkewicz: Yes!

Winterer: That's my grandson.

Harkewicz: And he's in Iraq?

Winterer: He's in Iraq.

Harkewicz: Oh. How long has he been there?

Winterer: Only weeks now.

Harkewicz: Oh, gees.

Winterer: Only weeks. Yes. So, I worry about him.

Harkewicz: I can understand that. But, I wanted to challenge you about the whole seafloor spreading plate tectonics thing because of something Naomi has noted. She suggested that the impact of deep sea drilling on the seafloor spreading revolution has been over-exaggerated, and that by the time the program, the project, actually, I guess you might say, proved the hypothesis, the controversy was already concluded.[28] And, I was just wondered what your viewpoint on that was?

Winterer: No. No. I think it was widely believed but it was not quantitatively understood at all. I mean they had the magnetic anomalies in a couple of places. There was a transect in the South Atlantic and another one that extended out from South America. And the magnetic anomalies could be correlated so you knew that this was a global phenomenon. The anomalies six, seven, and eight, in the Atlantic, were same as anomalies six, seven, and eight in the Pacific. Spreading rates were different because the accordion was pulled apart or collapsed, according to where you were. But dating those things was very flaky. It depended on some wild extrapolations. The dating was confined initially only to the last few million years at best, where you could go to Iceland or the Sierra Nevada and places like that and find the magnetic anomaly in the pile of lavas, and radiometrically date those lavas, or find sediments in between them to help you date. But that extended back only a little ways and the rest of it was just

[28] See note #18, van Keuran, page 203, footnote 5.

extrapolation, straight-line extrapolation, boom, just like that. Leg Three of the Deep Sea Drilling Program nailed it for, I can't remember, the top fifty or sixty million years in the South Atlantic and put real numbers on it, backed up mainly by fossil dates from overlying sediments. So it made it quantitative. And we've gone on since then to nail the thing all the way back. So I don't think Naomi is right. I mean, we understood in a primitive way, okay, and the ideas certainly weren't widely accepted, but in the oceanographic community they were, but the understanding was very primitive. I think that we now have, over the years, developed better and better. We stopped drilling magnetic anomalies for their dates years and years ago: we realized that we had it pretty well nailed.

Harkewicz: And you didn't have to do it over and over?

Winterer: You didn't have to keep doing it.

Harkewicz: Right. Right.

Winterer: I mean, you're not going to repeat Leg Three.

Harkewicz: Right.

Winterer: I remember on one of my legs we specifically went—was where Anomaly M7 was perfectly expressed, out in the Central Pacific. So, with John Ewing,[29] I drilled a hole there and got good fossils from the bottom and said, "The date of Anomaly M7 is such and such, plus or minus, according to the fossils. It has to be older than these fossils." And so here and there people have taken advantage of the presence of the ship to say, "Okay, we need to know more about this anomaly." They worried a lot in the far Western Pacific, which is the oldest datable crust around, so we drilled a special set of holes out there to try to date the oldest oceanic crust. And we got dates on it and so it limits the thing. But, you're right. It's playing with the details. You're filling in all the blanks now, so that your interpolations are over much shorter distances. You know it here, and you know it here, and there are a bunch of anomalies in between. "Why don't we just straight line it over that short distance instead of straight-lining it from between one and a 180 million years?" So in a sense, Naomi is right, but I think that an important thing was done on Leg Three.

Harkewicz: That was early on.

Winterer: Early on. And the two chief scientists, Dick von Herzen and Art Maxwell, were both geophysicists.[30] They were interested ideally in getting only one core at

[29] John Isaac Ewing (1924 - 2001), geophysicist, emeritus senior scientist, Woods Hole Oceanographic Institute (WHOI).

[30] Richard Pierre von Herzen (1930 -), oceanographer, emeritus scientist, WHOI. Arthur Eugene Maxwell (1925 -), professor emeritus, Department of Geological Sciences, University of Texas at Austin and director, Institute of

each site. You took a core at the basement with about that much sediment and about that much basalt and that was enough. But they had with them a couple of sedimentologists, Ken Hsu and Jim Andrews,[31] who said, "There's not anything for us to do. Why don't we core continuously and see what happens?" And so they did. They cored continuously, and when the cores came in these guys made a fantastic story out of it. They invented paleooceanography on Leg Three. That was a big leg. They analyzed the sediments and said, "These represent different amounts of dissolution in the sediment. These are very well dissolved. In these the calcareous fossils are almost undissolved." And they made a story out of this. They got the story backwards but it doesn't make any difference. They said, "Oh, the Mid-Atlantic Ridge must be going up and down like this because everyone knows that the dissolution level is a constant in the sea. It's at 4,500 meters." My colleague Wolf Berger[32] said, "Why do you look at it that way? Why don't you say that the dissolution level goes up and down like this and the Mid-Atlantic Ridge stays still?" That's an alternate interpretation, which turns out to be the correct one. But it didn't effect what they found. They had the data, so they get the credits for it—I've written a paper about that.

Harkewicz: But they, it was just by chance that they decided to do the whole . . .

Winterer: Well, they had to press on those geophysicists to do this. They pressed very hard. I think they realized they had the time to do it. The coring technique was good enough so they could get most of the interval and still get there and not waste—well they had to drill through it anyway, why not core it on the way? And so they won that little argument and . . . [33]##

Harkewicz: ##Let's see. What were we talking about here?

Winterer: About the Leg Three paleooceanography. They sort of opened up a whole field, and that, that is one of the major things that happens out there on the drill ship today. And the idea of continuous coring. And when they started to do continuous coring they began to do it in a technically-correct way so that you got everything and not just fragments and slop, and things all deformed—because coring is a violent process and they learned how to do it so that it wasn't violent. So it came back with all the layers and they then looked at the layers and figured out that they could see these rhythmic alternations at different scales in the cores. And with careful dating they gradually came to realize that these were attuned to the orbital frequencies in the earth, the so-called

---

Geophysics, University of Texas at Austin. Maxwell was a junior geophysicist at Scripps from 1952 – 1955 and an assistant oceanographer at SIO from 1950 – 1952.

[31] Kenneth Jinghwa Hsu (1929 -), geologist, visiting professor, Colorado School of Mines; James Einer Andrews (1942 -), geological and naval oceanographer, director Office of Naval Research.

[32] Wolfgang Helmut Berger (1937 -), professor of oceanography, director of California Space Institute.

[33] Recording paused while Winterer spoke with a colleague.

Milankovitch Series. Do you know anything about this or am I in mystery land now?

Harkewicz: No.

Winterer: Okay. There are changes in the orbital relations of the Earth and the Sun. So, sometimes we're near and sometimes we're far. Sometimes it's winter and sometimes it's summer, and those relationships change in such a way that the Earth tilt changes periodically. And those all combine so that the climate in any one place will be different. It would be as if I'm at a different latitude here. And or the winter lasts longer, or it's colder, or whatever. And those produce effects in the sediments, these kinds of rhythmic alternations. And that has enabled us now to date sediments at a resolution of about 40,000 years—and geologically, that's not very long. And they now have carried that continuously back to about forty million years. And we have big sections similarly organized that we haven't fixed yet in terms of their absolute ages, but we can pin the radiometric ages in them, and the rhythmic ages together so we have developed an incredible timescale that's cosmopolitan, that works everywhere in the world. So I can correlate an event here with an event in the Indian Ocean, in some places, to a resolution of forty thousand years. You can, as I said in the article, "You can start looking at chicken and egg problems." I mean, "This continental motion did this, and then the ocean did this," and so forth, and which came first anyway? And which was perhaps the cause of the other. The effect cannot be the cause and so you have to get things properly ordered in time for cause and effect.

Harkewicz: Okay. So maybe that explains something that you wrote in that history where you said that these holes could be used as natural laboratories? And I guess I was trying to understand how you would use them over, or why you'd want to use them over and over again?

Winterer: Okay. That's a different thing. We have instrumented some of the holes, put seismometers down there, for example. Or to look at fluid flow, or leave recording observational devices in the hole so that they make measurements and you can come back later and ask the instrument what it's found. You can telemeter the results. And that's been done, particularly at seismic and at fluid flow things, there's a lot of this monitoring using the deep sea drilling holes as access to where you want to be. That's the natural lab.

Harkewicz: So, you're not actually going to take another core sample and see differences in a short time frame?

Winterer: No. It changes in the general environment of fluid flow or temperature, or seismic activity, or those variables. And the hole is merely a way of placing your instruments there.

Harkewicz: All right. I understand now.

Winterer: Okay.

Harkewicz: I wondered about the funding though. Was NSF the main funding group for all of the groups of these drilling . . .

Winterer: Yes. And early on, all the money, every last scrap of it, came from the National Science Foundation. And I don't remember when the internationalization of the program began. We always had international participants. Early on, we would invite non-U.S.—we always had to call them "non-U.S." They're not foreigners, that sounds bad—but the non-U.S. scientists were aboard because of their expertise. Sometimes in a subject, sometimes regional, whatever. But I remember having Russian scientists early on, Leg Six, which is a long time ago. And then at a certain point, and I don't remember when that was exactly, not many years into the program, the NSF went out and solicited contributions from other nations and so it became internationalized in a more formal sense. There were, essentially, treaties. And so the British, and the French, and the Russians—and I can't remember now—there were five or six nations in the thing. And they had to put up dollars, and but they had the privilege then of sending scientists to the ship to be participants. They could put so many people aboard.

Harkewicz: So eventually they had to pay for their participation, then?

Winterer: Oh Yes. They paid for their participation. Yes.

Harkewicz: I see.

Winterer: So, they put up x-bucks in dues this year and you have to send your scientists out to the ship at *your* expense. But you can also be members of the planning, various planning committees. You can participate in the governance of the whole thing. So it became a very international program.

Harkewicz: Did you work, I mean were you able to exchange ideas, then?

Winterer: Oh yes. Oh, oceanography has always been a very international thing so that, all of us have worked over the years.

Harkewicz: Even in the Cold War environment?

Winterer: Oh yes. Worked with the Russians a lot. Sure. Sailed on their ships and they sailed on ours. Yes. At the level of science these things are not as frigid as that. Now, people sometimes are closed-mouthed about things, and you don't get to do some things with them or they with us. I mean, you don't invite them on a nuclear submarine, but they didn't invite me, either. So there's a lot of

international cooperation. There has to be. A lot of international data exchange, very free data exchange. It works almost like mailing a letter to Russia. It gets through, right. I mean, the international postal system works.

Harkewicz: I guess. Yes. I don't know if you're going to see black lines, blocked-out data.

Winterer: Yes. But that's censorship. But your letter gets there. There's an agreement.

Harkewicz: Was there ever any security issues with the Deep Sea Drilling Project?

Winterer: Virtually none. Virtually none. We tried very hard to stay away from anything like that. Now, we always had problems, political problems, with some countries about the program itself, what to do. I mean, I can remember one of my British colleagues saying, "The Chancellor of the Exchequer[34] wants to see that drill ship sailing by in the English Channel about once a year. If he doesn't, then it's going to be hard to get money from us." So we would find ourselves drilling holes of interest to the British scientific community, and sometimes of interest to the British, generally having to do with oil and gas. You don't drill for oil and gas. That was a big no-no. In fact, we spent a lot of effort avoiding doing, ever discovering—because we had no way to control it. And so we didn't want to have anything leaking out of the seafloor. So, we stayed away from that. And we had—I sat on safety committees that would pour through all the evidence and say, "No. No. You can't drill here. You've got to move over here because there's a risk here." But foreign countries were always at different objectives. I remember we wanted to put a hole out in the far North Pacific and instrument it with seismometers. Well, our Pentagon-types were very interested in that because that would monitor Test Ban Treaty violations. You can listen there. And the Russians didn't want us to drill that hole at all and fought it. And so, and then I remember in the . . .

Harkewicz: What was the scientific rationale for that?

Winterer: Oh, to learn about the seismicity of the region. It was an interesting area. But, we didn't drill it, finally.

Harkewicz: Because of political problems?

Winterer: Yes. We did it somewhere else, where the Russians didn't give a damn, and scientifically it was just as interesting. So, we were pushed a little bit toward that region by the folks in Washington to, "Why not do it there?" "It'll be fine." But, the Russians said, "Hey. Hey. What you guys up to? Stay away." So, we drilled it in the South Pacific instead.

Harkewicz: Okay.

---

[34] The Chancellor of the Exchequer, often simply called the Chancellor, is the title held by the British cabinet minister responsible for all financial matters.

Winterer: And then I remember—I can't remember which was which now, but we were going to drill some holes in the Eastern North Atlantic, out off of Africa, or something, and, I think it was the French who were very interested in possible nuclear waste sites— put your waste down in the ocean bed, to bury it. And the British had—I think it was the British—opposition to it. I think that was the polarity of the system. I don't remember now. ##

Harkewicz: ## The problems with the British and the French, and some sort of drilling for nuclear waste?

Winterer: Oh. Yes. Well, it was just an example of a political difference, that one of the nations was very interested in our drilling holes in this area because as a side benefit you'd learn something about the properties or the material, and they leaked it out that they were interested in nuclear waste disposal. Whereas, the other nation would not have been able to get money out of their treasury to devote to anything that had to do with that because the political opposition in their country would have been so opposed to that. And so we just didn't go there. Just said, "Okay, we're not—I mean, that's not one of the most essential things that we were going to do anyway. Why don't we just stay away from that?" So, there were political things that came into it.

Harkewicz: But can you explain to me though how they had that ability, that pull, then? I mean, was it because you'd have to go into their waters and it was . . .

Winterer: No, it was just, these were all in international waters but it was just a question of the purpose of the expedition: one country thought that would be very interesting and the other didn't. We'd learn something about that because it was a potential area of nuclear waste disposal.

Harkewicz: But you didn't do it because it was more diplomatic not to?

Winterer: Yes. The other nation said, "We'll have to withdraw from the project, then."

Harkewicz: Oh, okay because it was . . .

Winterer: "We can't do that."

Harkewicz: It was the money that was involved?

Winterer: Oh, yes. Money.

Harkewicz: Okay. All right. Okay. So, that's how they were able to . . .

Winterer: Yes. They said, "We can't, we just can't do that."

Harkewicz: Okay. I was, I just wanted to make sure it wasn't just being good natured.

Winterer: So, you go somewhere else, which has many of the same purely scientific objectives but is not scrambled up with these other things. And so you. . . that's one of the things we're paid to be is smart enough to figure out how to skin that cat in another way.

Harkewicz: I understand. Okay. Well, that makes sense. Yes.

Winterer: Okay.

Harkewicz: But you said something before about not being allowed to drill for oil because of the dangers involved, but I know that sometimes you found existence of salt domes which are related to oil?

Winterer: Yes, indeed.

Harkewicz: So, was there ever any conflict between basic science and applied science? What did you do with that information about the salt domes? I mean, did the oil companies come asking you to look for them or anything like that?

Winterer: No, but there was always a little bit of subtle maneuvering on the part of some of the oil companies. They had a lot of data to guide places to go to drill. They had a lot of seismic information, particularly in the continental margin regions, which is where so much oil is. It's not out in the middle of the ocean. And so, they would be helpful in showing us their seismic recordings, which show the conformation of the sediments. And sometimes they were obviously very interested in our drilling in that region. Not to find oil, but just to know the kind of sediments that were there, and the ages, and that sort of thing. And we wanted to know it, too. And so sometimes our scientific and their long-term economic interests coincided. I mean there was no harm in our drilling there. There was plenty of scientific payoff for us, and if they learned something, fine. We hoped that all kinds of people were learning from what we found in the sea. It wasn't of purely scientific interest. There were all kinds of other communities interested in the results. And specifically about the, for example, the salt domes which are obviously a red flag kind of thing. We had to be particularly careful of that. There was, early on in the project, a salt dome drilled on Leg Two. Doc Ewing[35] himself was on the ship and they drilled a thing called the *Challenger Knoll,* which is a knoll down in relatively deep water off the coast of Texas. That was before there was a safety panel. And many of us wondered if Doc Ewing weren't taking a big chance there. But it was explained later—and I don't know how true this is. This is the story: that in fact, one of the major oil companies had been out there and poked around at it a

[35] William Maurice Ewing (1906 – 1974), geophysicist. Ewing started his career at WHOI in 1935 and founded the Lamont Geological Observatory at Columbia University in 1946. In 1972, he moved to the University of Texas at Galveston.

little bit and thought with shallow drilling, if you didn't go in too deeply, you were not in any danger. They didn't see any signs. But I don't know whether Doc knew that or not. But many of us were spooked by that. And so these safety panels were set up to look at these things.

Harkewicz: Did anything happen when they drilled in it?

Winterer: No. We did tap into some oil shows off Norway one time, again by Lamont people, Manik Talwani.[36] The Norwegians were upset about that. But it was accidental. The trouble is you have no way of controlling it. With the mechanical scheme on the drill ship, at that time, you simply drilled a hole in the bottom of the sea and it was open to the sea floor. And so if something came out of it just went into the sea. And in oil field practice you don't do that. You put a control system on the bottom of the sea, valves and everything, so the return is to the surface. The Japanese have just put a vessel to sea that is going to drill in places where we need to have such well control and it will have it. And so one can drill in more risky places and control anything. But ordinarily we don't have any means of controlling it except to stay out of there. Now, I did drill a salt dome once. We were very careful. We had a lot of seismic information from the oil companies again and we were very careful not to go in very far, and we got back absolutely wonderful, wonderful results about the earliest history of the Atlantic.

Harkewicz: So, it was more just an exchange of information then with oil companies—you never were sent out to look for anything specifically?

Winterer: No, the only trouble that I can remember having with the oil companies was the secrecy of their seismic information. I remember before drilling in that very area off Morocco where we were dealing with some salt domes, they sent a little group of oil company people, four or five of them, and they laid out their records and showed us. And so we took note of where some interesting places were. Not dangerous places at all, just ordinary, but they had good records. And, we were very thankful for that, but then they wouldn't give us copies of the records. No copy.

Harkewicz: Is that because they were afraid some other oil company would find out?

Winterer: Well, they all point at each other and say, "Well, we have a secrecy agreement with our consortium here and if there's anybody in the consortium that says 'No.'" And so I say, "Yes," but we, maybe somebody else will say, "No." The answer is "No." Always, "No." And then immediately after that experience the deep sea drilling organization, the whole community, passed a rule that you cannot use any information, like seismic information, to site a drill hole unless that is freely available for publication in the volume in the open literature. That

[36] Manik Talwani (1933 -), professor of geophysics, Rice University. Talwani was a staff member at the Lamont-Doherty Geological Observatory, Columbia University, from 1957 – 1981.

stopped some oil companies from cooperating, but it was a rule then that you could not be stymied. Because we couldn't publish those profiles in the paper, in the literature.

Harkewicz: Did that affect the science that you did, then? I mean, did that limit what you—

Winterer: I don't know the extent. What you don't know, you don't know. So the oil companies, in many places, have got beautiful information but if they won't allow us to publish it then we say, "Okay, we won't look at it, then."

Harkewicz: So, if it's not publishable it's not really worth knowing, so to speak?

Winterer: Well, or we deny ourselves the privilege of knowing. We have to figure it out in another way.

Harkewicz: Okay. Gotcha.

Winterer: And so we spend a lot of money now on site surveys, seismic surveys, ourselves, to get publishable open-literature data.

Harkewicz: That might already be available?

Winterer: Yes.

Harkewicz: I see.

Winterer: The Shell Company's got it. We know they've got it. But we'll go out and do it again.

Harkewicz: Okay. All right. I know that the *Glomar Challenger*[37] has been described as a remarkable technological feat and that the Deep Sea Drilling Project weren't possible until the technique to be able to, what is that, go back into the . . .

Winterer: To do the positioning?[38]

Harkewicz: Right. So, do you feel like the technology drove science in any way?

---

[37] The *Glomar Challenger* was the first research vessel specifically designed for drilling into, and taking core samples from, the deep ocean floor. The ship was designed by the National Science Foundation and The Regents of the University of California and operated by the Global Marine Drilling Company. The ship's name is both a tribute to the British oceanographic survey vessel, the HMS *Challenger* (1872 – 1876), and a recognition of its operating company—Glomar as in GLObal MARine. The *Glomar Challenger's* first expedition was a year-long scientific cruise that started in August 1968 and criss-crossed the Mid-Atlantic Ridge between South America and Africa as part of the Deep Sea Drilling Program. It was taken out of active duty in 1983 and later scrapped. Its successor, the *JOIDES Resolution,* was launched in 1983.

[38] "Positioning" involves holding the ship still in varying wave and wind conditions while continuing to drill and collect samples.

Winterer: No, it simply facilitated science. Yes. And, we got that technology from the oil industry: they had it. In fact, it was the Shell Company, I think, but I—my memory is getting poor now—that had a good dynamic positioning system for their ship, or ships, and by an—and I don't know who negotiated this, how it was done—because we had some powerful friends in industry that were scientifically-oriented and they were helping us along. We were able to get that technology and install it on this vessel, and that was enormously—I mean, without it we couldn't have done the project. You needed that. Absolutely needed it. The rest of it was mainly just standard oil company technology. If you can stay in one spot then you just do your drilling, and we found we could do it in very, very deep water because the positioning system worked so well.

Harkewicz: So, you're saying that once you had the technology it wasn't a matter of getting new stuff over and over again. It was more, just more scientific?

Winterer: Yes, gradually we built up an engineering capability within the project to develop new tools. For example, the tool that I told you about that could bring back sediments with all the beautiful layering correctly preserved, that was our doing. We, the project, in fact it wasn't really—yes, I guess it was our folks up here at Deep Sea Drilling that did that. It was on Joe Curray's leg. In the Gulf of Mexico they were able to do that so-called "hydraulic piston coring," which is just a technical word. But we were able to do that and there were many other engineering developments that were self-generated inside the project. We paid our own employees, or contracted out to people to develop better drilling bits, better coring devices, better everything. Yes, and heave compensation so that when the bit is fixed in the ground and the ocean is going like this, so the ship is going like that. And so everything is doing funny things up there, and you try to develop tools that minimize those effects at the bit level on the seabed. You want it, you want the bit down there, as its going ahead, not to know that it's in the ocean. It's just going down into the earth as if it were being done right here.

Harkewicz: Right. I understand. The dating that's done, is that actually done on the ship then?

Winterer: Yes, to a certain extent. The paleontological dating is done by—the first pass at it is made by paleontologists on the ship. There's a whole group that you recruit from specialists from around the world and there are usually, oh, three, or four, or five of them who specialize in different kinds of fossils, foraminifers, or coccoliths, or radiolarians, or whatever. And as the cores come aboard and are split open they sample them and look. Usually aboard the ship, because things move along quickly, there is time for looking at only one or two samples from each ten-meter core. And so it's a very crude scaling of the ages. And then after the expedition gets back those scientists come and resample, but they'll resample the whole core, many, many samples, to get a detailed picture. Radiometric dating is not done on the ship. The business of counting all those

rhythmic layers, on some expeditions is done on the ship by specialists. So, that's the extent of it. Yes.

Harkewicz: I know that, at least I've read somewhere, that at least in the past these projects would run twenty-four hours a day, seven days a week, for extended periods of time?

Winterer: Yes.

Harkewicz: And, it's not clear to me whether scientists were actually onboard for extended period of times or if that was mostly the crew members?

Winterer: No, the scientists are there also. The ship, in the past at least, has characteristically gone out for eight weeks, or the cycle is eight weeks, essentially. So it would be out there for, yes, fifty-some odd days at sea. Some of that time is used up getting to where you're going to go, and then getting from site one to site two to site three. And when you're on a site you are there for often a number of days. In some places we've spent, essentially, two months at one site just drilling in the same place, because you want to go very deep or do something special. And the ship, like any oil well in the world, or any ship, operates twenty-four hours a day. Everybody works twelve hour shifts and the drill crew is that way, and you know people have overlapping shifts so that things are going all the time, and the scientific people are doing the same thing because those cores are coming aboard and you—and the ship is then like a big sausage factory. You've got to process all that core and run it through various measurements and cut it up, and examine it, and write reports and everything because you're doing all this writing at sea as well. So you have a preliminary report at the end of the whole thing. There's a huge effort of twenty-five, thirty scientists, and a drill crew that's operating all that machinery and the crew of the ship that's driving it around, and the engineers down there that are keeping it on position. And then the mess department, and the guys—it's luxurious. You get….somebody comes in and makes up your bunk every day.

Harkewicz: Really?

Winterer: You don't have to do that chore. It's not a very good bunk, but…. So, yes. You run right around the clock and you lose absolute track of time. You just, you do your work. It's a very intensive activity.

Harkewicz: Well, Pooh Venrick was telling me about some of her experiences on expeditions and the various things people do to occupy their time, more social-type activity.[39]

---

[39] Elizabeth Louise (Pooh) Venrick (1941 -), research oceanographer, co-director of Integrative Oceanography Division at SIO. See *Oral History of Elizabeth Louise Venrick*, 15 December 2005. Available on eScholarship Repository website: http://repositories.cdlib.org/sio/arch/oh/ .

Winterer: Yes. Oh yes.

Harkewicz: Do you have time for that?

Winterer: Oh, yes. No. Your shift is twelve hours. I mean, we don't have that many people so you have twelve hour shifts. They serve meals all around the clock. So there are at least four major meals a day. So, if you want to have lunch at four o'clock in the morning, go ahead, it's there, a good hot meal. They have a big bunch of movies, and music, CDs and everything. And usually there's more or less a gymnasium so people can exercise, and they run around on deck and do that sort of thing, sunbathe, and just a lot of socializing. I mean, I know technicians who got married with a fellow tech that they met on the ship and became well acquainted. And so, yes, there are romances. I know of divorces. [*Laugh]* So yes, there's socializing. Life aboard ship is…..you're confined to that environment and there aren't very many places to get away from everything, so they try to make things more or less comfortable. A lot more comfortable than they are on one of our smaller Scripps ships like Pooh is accustomed to. That's much more confined, but you're not out as long. You're not out there for two months.

Harkewicz: So, being out that long, how did that affect your personal life?

Winterer: Oh, it's a strain on the family. Yes. There have been some bad family effects on this. You have to work this out with your family. Otherwise it could be disastrous.

Harkewicz: I guess you and your wife figured it out, right? Since you are still together?

Winterer: Yes, but she didn't like it when I was away. She's been to sea some while I've stayed home. I once had to move the whole family to Paris while she was happily sailing around somewhere off South America. [*Laugh*]

Harkewicz: Sounds fair to me.

Winterer: Rent the apartment, put the kids in school, the whole thing. [*Laugh*]

Harkewicz: Did you ever have any port visits on these deep sea drilling?

Winterer: Only the beginning and the end. Yes. You go to a port, get on the ship, and usually you don't have any time at all. Sure, you go, everybody goes in and if it's, if the port is Papeete you go in to a bar or restaurant and you have a nice time, but you don't have much—the port stops are about four or five days, and that's just enough for the off-going party to get off the ship and take their stuff out, and the new people to get on and get aboard and start getting their equipment set up and everything. So, you're busy all the time. And when you

get to the end of your seven or eight weeks, you don't hang around in the port. You're going ahead and looking at airplane reservations.

Harkewicz: Trying to get out?

Winterer: Yes.

Harkewicz: How often, I mean if you're out there for eight weeks then you'd have to come back and work on the stuff that you collected. When would you be likely to go back out again?

Winterer: Oh, usually people don't go back for at least a couple of years. Yes. It takes you a good year of working on your material, usually, to ready it for publication because there's a whole series of publications, which in the early days of the project were in big thick books like that blue one down there. One for every leg. In more recent times they've reduced that to something like these thin blue books here which is just a brief summary and then nearly everything else goes to the regular journal literature.

Harkewicz: Which journals do you usually write for?

Winterer: It depends on the specialties. So the paleontologists are off there in their journals, and the geophysicists in theirs. Many of us liked the old way of having most everything between two covers so that you could crosscheck on everything and look back and forth in the book.

Harkewicz: So, that would be like a book then that . . .

Winterer: Yes, I can show you as we go out here.

Harkewicz: Other people from other places could get access to it then, somehow?

Winterer: Oh, it was published by the United States Government Printing Office and widely, widely, widely distributed. Oh, the Deep Sea Drilling volumes are . . .

Harkewicz: There are probably some in the library so I should look at those.[40]

Winterer: Oh, yes. Well, I'll show you a rack of them, which is incomplete because I haven't followed it near the end. I used to get the volumes for nothing because I was on all these different committees and they'd just send me them. And so, I built up a big collection. And people liked them so much that I thought we ought to get out—they were a particular shade of blue, green, blue-green and a

[40] *Deep Sea Drilling Project : Legs 1-25* (Falls Church, VA: American Geological Institute, c1975) is available at the SIO library, floor 3, call number GC380.D42. Information is also available as *Marine Geological and Geophysical Data from the Deep Sea Drilling Project* in CD-ROM format (SIOC 70 CD-ROM) at the SIO Library Circulation Desk.

big book. It would fill a bookcase like that. I thought maybe we should get out a beach towel like that, so that if you went to a hotel somewhere in the far corner of the world you could hang up your beach towel and feel right at home. There are the volumes, the Deep Sea Drilling—the "blue books" we call them.

Harkewicz: I like that.

Winterer: The blue books are right there. [*Winterer indicated several volumes in a book shelf across the room.*]

Harkewicz: I see. I like that. It's a good idea. So what is the state of the Deep Sea Drilling Project now? I know it evolved into the Ocean Drilling Project.

Winterer: Oh, it keeps changing names over the years. It's had three or four names. But the status of it now is the following: It's very international. It comprises three separate types of drilling activity. One is a drill ship like the one that's been in the water for the last twenty years, which is not the *Glomar Challenger*. It's the next ship, the *JOIDES*[41] *Resolution,* it was called, which is not a real ship name. It's an unofficial name. It was officially the *Sedco 471*, which doesn't really make your heart beat faster. And they are refurbishing that ship now, at a cost of many tens of millions of dollars, to bring it up to the mark for the new work, and it will go out and continue to do the kind of work that has been done on the *JOIDES Resolution* for the last twenty years. Different problems, different places, but essentially the same style of work. It's what we call a *riserless ship*. It does not have well control. And it is being paid for by the Americans. It will be an American-leased ship. *Sedco 471* belonged to Schlumberger Company, and some other company, and we leased it, which is a typical arrangement.

Harkewicz: "We" meaning Scripps, or "we" meaning the U.S.?

Winterer: Well, no, in the original lease, with the *Glomar Challenger,* "we" were Scripps. But now the management of the program and the running of that ship and everything is down in Texas at Texas A&M. It has been since the project left Scripps in the mid-eighties. It's been in Texas. The Japanese have just built a riser vessel, a huge thing which many of us call the *Godzilla Maru.* It's a giant ship and it is being specifically designed to drill where you need well control. So it'll have very elaborate control and big hoisting equipment and lots of space and will be starting out drilling around, near Japan, on some important problems near Japan. It cannot do the things that the *JOIDES Resolution* does because it can't drill in such deep water, yet. They may modify it to do that, but as of now it can only get, I think, to something like 4,000 meters of water depth, whereas the other ship can go to 6,000, at least.

Harkewicz: But, if you wanted to do work with that ship would you be able to go there and—

[41]Joint Oceanographic Institutions for Deep Sea Sampling

Winterer: Oh yes. Yes. It's still run under the same big international consortium.

Harkewicz: Okay.

Winterer: The third ship is not a particular ship, but a "ship of opportunity" that is for particular projects. For example, we sent a drill ship, leased from the Norwegians, I think it was, that went up within two degrees of the North Pole the summer before last and drilled very successfully. It was accompanied by two, count them, two icebreakers which kept clearing space for it to work. And they successfully did this venture and brought back a treasure-trove of information about the history of the Arctic Basin. And then, more recently, we had a very small drilling vessel drilling on the reefs around the island of Tahiti, sort of circumnavigated Tahiti and drilled, I can't remember, eight or nine holes going around the island. And that was another special kind of vessel. And you can imagine for different purposes there will be different kinds of vessels needed. And the Europeans are paying for that. There's a consortium of European countries who pay in dues according to some complicated formula that they—having to do with the gross national product and population of sciences and divided by the number of cafes or something. I don't know. So—

Harkewicz: So, ocean drilling, in one form or another, is alive and well?

Winterer: Is carrying on. And there's a huge…..you can't imagine, when the project first started, when the four American institutions, the Planning Committee, would meet, there was essentially only a Planning Committee. And there were, therefore, four guys who met. And I was on that committee at one time. There were five of us by then and we arranged it so we could—Chicago was the place. We'd fly into O'Hare, arriving there by midday from no matter where you were in the country. We'd meet all afternoon and quit just in time for everybody to get on an airplane and get home that night. And that was it. That was the planning structure of the Deep Sea Drilling. And now, I don't know how—it's just huge. So it's changed character a lot, and of course the costs—whenever you put three ships in the water and do all these things it's a big cost. But Congress has been—and that's the essential thing—and the like-counterparts in other countries have been very impressed by this. They have been very supportive. The National Science Foundation has really stood behind us. They've got some very adept program managers back there that have kept the money flowing, and these are big dollars. So it's a completely separate item in the budgets. It just stands out there all by itself. And it's sort of become like, oh, I don't know, like farm subsidies or something like that. It's going to go on.

Harkewicz: I have just a few wrapping-up questions. These are my generic questions that I like to ask everybody just to see how they respond. I know that the sixties have been referred to as the "golden age of oceanography," and having had all the

experiences that you've had, and coming to work in oceanography in the sixties, what do you think of that kind of a statement?

Winterer: I would agree with it, and the reason is partly that the things that opened up before us, like the deep sea drilling technology that made things available. But in large part it was the funding climate. There was money. The National Science Foundation was supportive of a lot of things here at Scripps, not just earth science but everything. So they were putting in big money in the sixties. And the Office of Naval Research[42] was a big supporter. We got I don't know how much, on the order of half of our money here, and I was a recipient of the largess of the Office of Naval Research. And I don't know why they supported me. I talked to one of their program managers from the past, at a meeting here the other day, and he said, "You don't realize it, Jerry, but I was a big supporter of the work you were doing in the Navy." And I said, "Why?" He said, "Well, I just thought it was really good science that you were doing, and I would explain this to the admirals and the managers there in terms that were Navy relevant." Because in those days, in the early days that I was here on the Office of Naval Research funding, we did not have to make the connection between what we did and what the Navy's interest was in the work. I mean, you could guess that they were interested in finding Russian submarines and hiding from them, and so forth, but there were a lot of things that I just didn't understand. And they said, "Don't worry. You just do your science. It's our job to be the translators."

Harkewicz: ONR's job?

Winterer: ONR's job. "We're your program managers. We are the translators. You just do good science and we want you to be working . . ." about the second sentence in every ONR proposal was, "The fundamental problems of." You had to have that statement in there, what you were doing. And then, as time wore on the managers got more and more interested in *your* explaining the relevance of your work to the Navy mission. Well, my work just dropped right off then. I obviously had no connection with it.

Harkewicz: You couldn't translate your work into a Navy interest?

Winterer: No. I didn't know how. Maybe it was and didn't know how to put it, but I was untrained and inept at doing that, but that was true across most of the earth science program here. So the Navy just stopped being interested. So our ONR budgets here at Scripps went way, way, way down relative to their previous good times. But now apparently NOAA[43] is a major supporter. Something like half of Scripps money now comes in from NOAA and there are a lot of people around getting money from NASA as well, as you can imagine. But I was back

[42] ONR
[43] National Oceanographic and Atmospheric Association

in the sixties with NSF and ONR and the two of them together supported me just fine.

Harkewicz: Okay. Then what do you think made Scripps successful, in your opinion?

Winterer: Then, you're assuming that it is? [*Laugh*]

Harkewicz: Well, okay. The opposite question, then, is what has threatened its success? So those go hand in hand. So I'd like you to think of one and then the other.

Winterer: I think that the fundamental reason is that we are, I think, careful and successful in our recruiting of scientists to work with us, and we put a lot of effort into that. And so I think mainly we have. I have brilliant colleagues and they're very creative and so they bring in the necessary funds to keep us doing wonderful new projects. And they're not, they don't keep working on old solved problems. They keep moving along. They are leaders. And I think that is the, whatever success we have, I think, is owing to the scientific staff here. But to keep that up, you have to recruit new people. You have to keep recruiting new people. And that's hard to do now to get the really young ones because it's so costly to live in this region, as you've doubtless discovered. And so we tend to recruit a little higher up in the scale, I think. I'm not engaged in that much anymore, but really proven quantities. And that is people that are at the associate level, whatever. And it's—I would rather see us hire a lot of brilliant-looking young people and then be very selective in keeping them. When I joined the university the ratio of people making tenure was like one-third. It was really hard. You recruited a lot of young people and then you threw them out.

Harkewicz: So, now if you hire older people they're guaranteed tenure?

Winterer: You tend to hire more seasoned people.

Harkewicz: I see.

Winterer: We have this system now of postdocs everywhere in the country and so people are not unknown. If you're in French Literature you're an unknown. I've served on a lot of committees on the upper campus where they were hiring young people, and in the humanities they're just unknown. They're on promise. You have their thesis and that's about it. Whereas in the sciences, there is this system of funding of postdocs and so they've already published a few papers and you can judge. And so it's different.

Harkewicz: So then would you say that the high cost of living in the San Diego area has threatened Scripps's success?

Winterer: It's a difficulty for the hiring of young people. Yes. And, we do not have, in the university, a very good system of subsidizing housing for young people.

Harkewicz: That would be your response for what's threatened its success most, do you think?

Winterer: Well, that and the scramble for funds. The dropping out of ONR, I think, was a serious problem. Yes.

Harkewicz: Did that have anything to do, you think, with management?

Winterer: Well, of course the running of the institution is critical and some directors have been more talented at this than others. And, as you know, we're searching for a new director as we speak. And some of them have been truly memorable. We had our director who had already gone before I came aboard, Roger Revelle, who was almost a mythological figure. I went to a meeting last week in Denver where I thought I was listening to the ghost of Roger Revelle[44] stand up making the same kind of wonderful statements about, effectively, "Do you need a smart Navy or a dumb Navy?" which is the argument Roger used to use for getting those admirals aboard. And so directors do make a difference, and their style, and the way they know what we're doing. And some are more talented at this than others. Yes. I don't think we've ever had any bad directors. Maybe we have in the past, before I came. But some . . .

Harkewicz: Not in your time frame?

Winterer: Some have been better than others. Yes.

Harkewicz: That's to be expected, I guess?

Winterer: Yes.

Harkewicz: So, finally then what would you say Scripps has meant to you?

Winterer: Oh.

Harkewicz: There's a look of shock on your face.

Winterer: Has meant to me? This, so I don't violate my wife's statement for now, "I married you for better or for worse but not for lunch." So I have somewhere to go for lunch. No, I think it has brightened my scientific life in the most extraordinary ways. I think if I had stayed at UCLA—I could have stayed, I was tenured—I would have done interesting work but I don't think it would have captivated me as this has. This has really been exciting. I mean, the chance to participate in sea-going things, especially the Drilling Project—I'm

[44] Roger Randall Dougan Revelle (1909 – 1991), SIO director 1951 – 1964.

completely out of drilling now—but while that was going on, that was really very exciting, very challenging. You'd just wake up just trembling with excitement to go do it. And it offered that opportunity to me. And that's what it's meant to be. It put scientific spice in my life. Yes.

Harkewicz: I like that. That's good. What kinds of things are you doing now?

Winterer: Oh, they're not related to deep sea drilling so much. I work, I got very interested, because I drilled some of them, these ancient coral reefs that rest atop seamounts, so-called *guyots*.[45] I don't know if you've heard of a guyot? And I drilled on those and I've written papers about them. And then I got interested in some of their modern-day equivalents, the reefs out in the middle, especially atolls, that Darwin described, and barrier reefs. So I've been writing about those.

Harkewicz: Are you still going out to sea then, or just writing about it?

Winterer: Well, I'm going out to sea at the end of May for a week to dredge up some corals near the island of Oahu, just off shore. And so, I'm still at it.

Harkewicz: That's great. I just want to ask one other thing that I forgot to ask you before and I'm just curious. I noticed that in the—you had an article in the *North County Times* about, I think it was, it was soon after the tsunami in Indonesia and . . . [46]

Winterer: I did?

Harkewicz: Yes. It was something online about a tsunami, the chances of tsunami coming and drowning Del Mar, and you talked about the different structures along the coast of California, that there are barriers that would make something like that less likely to happen here.

Winterer: Oh, I guess I mentioned that there are these offshore islands like San Clemente and so forth that are helpful in breaking things up that come in from certain directions. We're very open to things coming from the southwest. And . . .

---

[45] A guyot is a flat-topped seamount that is commonly found in the Pacific Ocean and is considered to be an extinct volcano. Guyots were first identified by Princeton geologist Harry Hess (1906 – 1969) while he served in the Navy during World War II. Hess named them after Swiss-born geographer, Arnold Henri Guyot (1807- 1884), the first professor of geology at Princeton. Hess postulated that, although the seamounts are now deep below sea level, these once-volcanic islands were beheaded by wave action. This hypothesis contributed to Hess' theories of seafloor spreading as well as to the theory of plate tectonics because guyots show evidence of gradual change through stages—from a fringed-reefed mountain to a coral atoll, and finally to a flat-topped submerged mountain. Hess suggested that the convection of the Earth's mantle was the driving force behind this process. Hess was also involved in the Mohole Project (see footnote # 26) that led to the Deep Sea Drilling Program.

[46] Peter Kaye, "When a Tsunami Strikes Del Mar." *North County Times* (4 January 2005). Available at: http://www.nctimes.com/articles/2005/01/05/news/columnists/kaye/23_14_091_4_05.txt .

Harkewicz: But, I wondered how much of this mass media type interaction you actually did?

Winterer: Have done? Very little. I've tried to steer away. I have been available to my local civic folks so that when there have been problems along the sea cliffs of Del Mar crumbling and work that they want, remedial work, that they want to do, I've made myself available as a consultant to them to help in those technical problems. And I've served on that kind of committee. I used to be on the Lagoon Committee and things like that. But, that's just your ordinary civic duties.

Harkewicz: You've said you've tried to keep yourself separate from that? Why did you see that as important?

Winterer: Oh, I was so busy here.

Harkewicz: Oh, okay. It wasn't something about . . .

Winterer: No. No. I don't mind doing that stuff. I'm always a little bit leery about newspapers and newspaper reporters. Not that they're not of good will but sometimes it's hard for them to listen in a knowledgeable enough way to understand what you're saying. So the message gets warped in some way, and a flashy headline will appear. So, on the Deep Sea Drilling thing we used to work out very carefully a kind of script, so that if the people came aboard you had a thing that you said that you'd thought out, and you tried to anticipate what questions they might have. But people ask funny questions anyways. I mean, you're out on some small island in the Pacific and they ask you a question that you just never thought of before. "Are we in danger of a tidal wave here?" Well, you've never thought about that.

Harkewicz: Trying to get a scoop?

Winterer: Yes.

Harkewicz: Okay. Is there anything else that you want to add in that I didn't ask you about?

Winterer: No. One of the things you ought to know, and it's a feeling shared by people who stick their heads in the door here, is that many of us are emeritus and so far we have the luxury of having an office, an adequate office, a pretty good office, and the institution treats us very, very well. So . . .

Harkewicz: But, is there a foreseeable problem with that?

Winterer: Well, if we ever got very crowded here then you become like some of the departments on the upper campus… ##[47]

Harkewicz: ## …And the emeritus professors?

Winterer: Well, so far that lack of space has not been a problem at Scripps. It could arise in the future if we expand and don't build new buildings, just for example. But not during my lifetime. I think I'll be able to have some kind of office. Emeritus professors are expected to shrink their space that they occupied when they were active and had students and so forth. So I think they treat us very well, the people in the administrative office in the division are very helpful to us. I ask practically nothing of them, but you know whatever honest request I have they just jump right to it and help me out. They're very kind. So I think we're made to feel welcome, happy, and . . .

Harkewicz: That's good. When you go out to sea you're still going out with other people, right?

Winterer: Oh, yes.

Harkewicz: So, I'm sure that you're affecting their work somewhere?

Winterer: Oh, in fact, on this little tiny cruise out by Oahu it's a so-called student cruise. There's a certain amount of, essentially, ship time paid for by the university that is mainly for the purpose of use by students and training of students. And so I dreamed up a project out there having to do with dredging corals and submitted it and it was approved. So, I get a little bit of ship time shared with some other users. And we're going to have a whole gang of students out there, and teach them how to dredge and how to do other things. And we'll get some science out of it.

Harkewicz: These are graduate students?

Winterer: Yes. Yes.

Harkewicz: Sounds good.

Winterer: Yes. That'll be fun.

Harkewicz: Okay. Well, I guess we will . . . ##

[47] The first few seconds of this portion of the interview are missing for some reason. Winterer was talking about the good fortune of emeritus professors who still have access to office space despite the present lack of space at Scripps.

TAPE GUIDE