ORAL HISTORY PROJECT
of
The H. John Heinz III Center for Science, Economics and the Environment

In conjunction with the Colloquia Series

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

Transcript of the Videotape-Recorded Interview with
BILL NIERENBERG

Conducted at
Scripps Institution of Oceanography
The University of California San Diego
La Jolla, California
February 10, 2000

Interviewers: Naomi Oreskes and Ron Rainger

Funded by the Office of Naval Research
Video and Audio Recordings by George Washington University Television
Transcribed by TechniType Transcripts, Davis, CA

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Naomi Oreskes and Ron Rainger,
interviewers

[Note: The volume level for much of this interview is extremely low, especially the interviewers' microphones. Every effort was made to provide an accurate transcript. Where this was not possible, [unclear] is noted in the transcript.]

Naomi Oreskes: Why don't we start. You were talking about Columbia, how you got started [unclear]. Tell us about that.

Bill Nierenberg: Well, whenever you tell me to go.

NO: We're ready. Okay.

Nierenberg: Well, I know the main interest is how I got involved with the Navy and naval activities and so on. It sort of began in a way that was very typical of the post-war period. I was in the Manhattan Project during the war; curiously, not as a nuclear scientist, which I sort of basically was, but as a chemical engineer. But I didn't know it. I didn't know that I was being a chemical engineer until very late in the war, I might add. Of course, that's a separate story.

I do say--this is a little bit of a twist--immediately before the war when I began my graduate work at Columbia, there was a professor there who was a Belgian, who had worked

with [Wernher] von Braun, and his name was Biot, Maurice Biot. I took his course in mechanics, and the only other person who taught as well as he was [Enrico] Fermi, remarkable teacher. He was interested in applications in geophysics, so my first attempt at a thesis, believe it or not, was in geophysics, not nuclear physics.

In fact, the particular subject was to use what was called the theory of finite strain, to see the difference in the propagation of seismic waves in perpendicular directions, like on the American continent, to do with stress. That was perfected many years later here at Scripps [Institution of Oceanography] by Russell Raitt. I did nothing. I mean, I worked on it, but the war interrupted it.

Well, when the war was over, I went back to Columbia to get my degree in nuclear physics with Rabi, I.I. Rabi, the really great physicist, and I did get my degree and I did very well in my field. I went from Columbia essentially to Berkeley, where I was fifteen years, and I worked in nuclear magnetic resonance and so on, but I was still under the influence--in fact, if you knew Professor Rabi, you could say under the thumb of Professor Rabi. Till the day he died, I was a graduate student and had to carry his valises and so on, even when I was a full professor at Berkeley.

But in any event, you see, I was at Berkeley only a year or so--I have to make up some of this, but I think I'm quite right--Rabi, because, you see, Rabi and others were really part of ONR [Office of Naval Research]. You see that growth of--

NO: [unclear]?

Nierenberg: No, no, only in an operational sense. Rabi was at Columbia, a professor, and I was at school. But the physics community was not that large. ONR was running, and their primary advisors were these physicists and so on, and it was very close both ways.

NO: [unclear]?

Nierenberg: Well, they interacted very strongly, you know, and more in an operational sense, scientifically also. He and Alan Waterman were very close, as an example. Manny Piore, who was the deputy director of ONR then, deputy science director, I know Rabi was very close. Manny's still alive, one of the few that are. I got to know Manny very well later, not quite at that point.

In any event, as I surmise, I think it was the summer of '51--it doesn't matter, just about that time--the Navy apparently approached Piore, because Piore--NSF [National Science Foundation] had started, so Waterman had moved over. Piore was running ONR. Apparently the CNO [Chief of Naval Operations] approached Piore and suggested that it would be good to get a university laboratory involved as kind of a basic science support for SOSUS. That was the big developing thing. They'd already put in several arrays. [Interruption]

Nierenberg: We should explain a little bit, because it's going to come up over and over again, if not in my thing, in other ones. I can never remember what the letters stand for, but essentially what it was, was to capitalize on the discovery made at Woods Hole by [Maurice] Ewing and--it's terrible, I forget the Israeli's name who did the theoretical analysis, predicted it. I know him

so well. And Ewing showed it experimentally. What happened is that it showed that there existed essentially a sound channel that carried low-frequency sound. We're talking like 100 cycles per second, at great distances.

In fact, while I was at Columbia, we had a lecture in the physics department by Ewing, first I was exposed to it. In any event, they wanted to capitalize on this by building these arrays. I can say--the secrecy is gone--40 hydrophones in parallel that could look in various directions and they were brought to shore and analyzed. There were at least two of these operating in the Atlantic. Bell Labs was sort of the prime contractor, but Western Electric [unclear] did the building, and the government labs, like NRL [Naval Research Laboratory] and others, were working on many aspects of this, but for some reason they wanted an academic lab involved, and probably people who had never been deeply involved before.

Well, what happened is that Piore, working with Rabi, invited physicists, leading ones, from all over the country to meet for one week, for five days, in McMillen Auditorium at Columbia. That I do remember. And we were there for five days being briefed by the Navy and other people, and it was sort of a bit of a game. He twisted the arms of a lot of people to get them involved, you see. A laboratory was to be formed. Piore spoke and Navy brass spoke, and we sat for four days. On the fifth day, we sort of got together an organization. I didn't particularly volunteer then, but they persuaded E.T. Booth, a very, very brilliant experimental physicist, to be the director of this lab, and they actually rented a building that they called the old Bible Building at Dobbs Ferry, to be the laboratory.

The following summer, I and several other people, one of them a future Nobel laureate, Jim Rainwater, came out to help Booth get this all started. I only worked that summer. My

family came out, too. Some of the others stayed permanently and got it built. That was my beginning in the business, and that was the Hudson Laboratory.

NO: [unclear]?

Nierenberg: Well, I thought it was a good idea to simply get a viewpoint and get more people involved. You see, the Navy labs, you know, were--what would be the right word? Very bureaucratic, very built-in, locked in. [Interruption]

NO: [unclear]?

Nierenberg: Well, it made sense. You see, the other laboratories were very good. NEL [Navy Electronics Laboratory] was excellent. NRL, still a great laboratory. But actually they did more than they expected, because, as I said somewhat jocularly, but really it's more serious than that. I had a very important decision in the lab, even though I wasn't permanent at that point, and one of the things I did was help recruit, you know, get graduate students in. I recruited, among others, they were better than that, two people, one of whom, Robert Frosch, became Secretary of the Navy eventually. It wouldn't have happened otherwise. And the other one was Al Berman, who became head of NRL, director of the Florida school, Rosenstiel School [of Marine and Atmospheric Science], as an example. There are other people. I don't want to expand this.

So not only was it important in sort of looking at the problem from a different viewpoint, the associated problems, but it really brought in talent from directions you wouldn't have

expected. And obviously it was very successful. So I don't know all the reasons. Aside from anything else, I was below the s___ at that point; I only did what I was told. Rabi picks up the phone, said, "Bill, you will come to this meeting." [Laughter] You know, I came. It was all right. Socially it was fine. There were a lot of friends. But that was the beginning.

Then funny things happened, because I think it was that summer--and it's a picture I couldn't find--as an example, then I was exposed to a more serious problem--it still is. You see, this is the period of the Korean War. That's why you had the people at Scripps at that time that I mentioned, like Arnold Nordsieck. They had come from wherever they were, sort of a reincarnation of the University of California Division of War Research before the war.

The other problem was mine warfare. That really hindered the Navy very severely. If you look at it as a war between the Navy and the Army, the Navy really didn't make out very well because they couldn't fulfill their commitments. When you do these things like Inchon and Wusan--I forget which one--they actually were held back so long by the mines that the Army went well past them, and it just didn't get anywhere. Not that the Navy didn't know there was a problem, but it accentuated it and it moved to the top of priorities.

So I got involved in mine warfare from then on. In fact, one of the more difficult mines--I'm trying to explain how I got involved, sucked in, literally--there was a mine that was called the oyster. It's still a problem. What it does is, if you have, say, a mine down at 40, 50 feet in a channel going into a port at 60 feet, there are many ways of triggering it. A classical way, of course, the old way, was the mine would be more or less floating, would suddenly submerge, had horns, the ship would hit the horn, and some acid would pour onto a cap and it would explode. Then the other one was the noise of the vessel, you know.

But then there was a more subtle one. Many of these could be counted and swept very easily. The one that was very difficult was called the oyster. You see, when a large vessel would go over that relatively close to the bottom, the water underneath, just like under an airplane wing, would have to rush by. That gives you the Bernouilli effect, with the lowering of the pressure. That would last for--you know, you could work out, for a ship so long that's moving at a certain speed going into a harbor, you'd get a pulse, a rectangular shape, negative pressure, and easily measured and so on. The problem, of course, is if it sits there for several months, a wave might go by that might do the same thing, but you combine that with the sound, and together you had something very difficult to sweep. In fact, the only obvious way to sweep it is with another ship.

Well, I don't want to boast. I invented a clever device for doing that without using a ship, and there's a picture of me in my shorts, because I did it when I didn't have bathing shorts, in the Hudson River with a fire hose, demonstrating this method of sweeping. You see, in other words, by having a jet go over the bottom for an appropriate length of time, the same Bernouilli effect. I made my first discovery. It's not that easy to sell a new idea to the Navy. I don't want to enlarge this point.

NO: [unclear].

Nierenberg: No, it didn't take them a long time. He's wrong. That they did pick up. That they did pick up right away and it worked like a charm for many, many years, for longer than it should have. We're ahead of ourselves.

NO: [unclear]?

Nierenberg: Well, because, you see, I don't know what's secret and what isn't, but right early they were able to track Russian submarines right across the whole damn Atlantic because they were so noisy. They weren't only noisy; you got even extra noise. It's like you have a generator with a broken tooth. You can even identify the goddamn submarine by these things. Well, of course, because we had SOSUS going, conversely, we worried about sound like mad. You always do in a submarine. Even from the beginning, the sonar was important, so a hammer has got a rubber head in case you drop it by accident. You never drop a tool. You walk around with rubber shoes. This is all important. I don't want to go into detail of all the--some very clever things were done here at NEL to make a submarine quiet, that I shouldn't go into. People have forgotten. They're very good at NEL.

But the point is that we would have expected the Russians, who knew about this, to get on to quieting their submarines in a more significant way. You see, for years what happened is they didn't. For many years. [Joe] Worzel is wrong. We really tracked them for many, many years. Then we got lazy because the Russians didn't do like sort of on a steady scale of quieting their subs. We sort of forgot about it. Then all of a sudden the year came, many years later, when the Russians suddenly quieted their submarines. And, boy, let me tell you, things changed from that point on. Without going into it, this is a super secret subject, you know, and still is.

NO: [unclear].

Nierenberg: But I don't think that's correct. I think that's one thing the Navy bought up, because the thing was discovered about 1944 with the war on, you know, and the Navy started right away. I even knew--I'll give you the names of the captain in CNO, Captain Fusselman, whom even I got to know very well. I even visited the Office of CNO that was on top of the subject. It had a name: Project Caesar, right away. I bet you a nickel that name was given about '47, because, look, I got involved in '51 and they already had two arrays. That's really pretty good, you see.

I have some criticisms of the way it was done, but it had nothing to do with the Navy. They let Western Electric have a monopoly. So what happened is, they didn't use their best people or their best techniques in building these arrays. They were very cumbersome. We were already beginning to use coaxial cable for a lot of things. They didn't; they used twisted pairs, forty twisted pairs instead of one coax, things like that. They didn't have competition.

NO: [unclear]?

Nierenberg: And if you want to know about it, if you want to get me much later, I'll tell you how indirectly I brought the competition in as director of Scripps. It was later. But, nevertheless, it worked, because the submarines were so noisy, you see, and we didn't have great computers then.

There was something else. If we're doing this, we ought to give credit to somebody. God, am I going to forget his name? There was a physicist at Bell Labs who invented the

important device. Gosh, I'm forgetting all my names. You see, the principle is simple enough. You have these discrete frequencies coming out of a sub, like a spectrum, and you're recording the spectrum. Then you have to have a spectrum analyzer. Well, I certainly can do it as a physicist in the lab and so on, but remember the problem is operational. You have all of this coming in to a site, and you have to train sailors to sit there and run it. Every lab had built a device--we saw them all; they were sent to us--for doing this, but none of them were really very operational. They worked, but so on. What the devil was his name? I knew him so well, worked with him.

Ronald Rainger: Gaylord [unclear]?

Nierenberg: Oh, no. Gaylord was now running a big university. I knew him, too. Gaylord [unclear]. No, no, no. This chap was a physicist. It's terrible I forget his name. He'd already done a very clever thing. He was sort of the inventor of the TV antenna, you know the one where you have the big piece, the little thing, the big thing, the little one? I could explain that, but I won't. Very clever guy.

What had happened was, "Iron Mike" Kelly--I don't know his full name--was head of Bell Labs. He was called "Iron Mike" Kelly. But Kelly had another interest, and that was the opera. The Metropolitan Opera was important to him. So independently of all of this, he assigned this chap the job of building a frequency analyzer so that you could look at the voices of the singers and so on. It was exactly a modification of this device which was called LOFAR,

low-frequency analyzer, etc., range or whatever, called LOFAR, which is pretty much still what they're using to this day.

NO: [unclear]?

Nierenberg: I don't know. That was a joke. I got sucked in because of this picture of me in my shorts in the middle of the Hudson River, with a very crude operation. I caught a bad flu from that poisonous water. That's another matter. That's what I meant; it was a bit of a pun. A bit of a pun. No, I was glad to do it, and it didn't really slow my work at Berkeley. I had, in the end, about forty Ph.D. students and maybe forty others that were associated in a big lab, so it really didn't slow it at all. I was able to do both.

But coming back to this, this device, you see, was an outgrowth, very curiously, of "Iron Mike" Kelly trying to have a device that could analyze the singers' voices. You see, there were a million different frequency analyzers, but this one was superb. It worked in a very simple way and it was a very simple device, and it was one that you could train somebody to use right away and it would show the lines as they developed, listening to a submarine. It was called a LOFAR gram, the actual output on a machine. This chap was quite a genius. Little by little I can get the names back of all of the people. The head of the project was a man named Wiebusch, who, incidentally, was a Ph.D. from Columbia also, maybe from Rabi, from the early days, I'm not sure. But this chap, I feel bad I've forgotten his name, because that was a key thing, that particular item.

In any event, where were we?

NO: [unclear]?

Nierenberg: I had that early interest in geophysics I was telling you about, and even before that. Let me tell you a wonderful thing that a few people only know about. See, while I was at Columbia taking courses, I took a voluntary--Fermi was there, and Fermi gave a course in geophysics that he had given only once before in Italy, because he enjoyed it. It was a two-hour session once a week. I took it. It was fascinating. He was a fascinating teacher, and I was fascinated with the geophysics. I think that's why I got hooked up with Biot, with Maurice Biot, because he actually had some problems in propagation of earthquake waves, seismic waves at sea. So I had that latent interest anyway.

Sort of one thing led to another, you see, just as I said. Then when Booth decided to leave the laboratory to, incidentally, eventually go and run a similar laboratory for the NATO navies in La Spezia, you see, which is still going, a big laboratory, he started that also. I was persuaded to come East to be director of the Hudson Labs, and I did that for a year. But in the process of doing all of that, you see, I started to meet people like Ewing and Worzel and many others, Brackett Hersey. I could go through the list, just about everybody around then, and I admired most of them. They were terrific. They really were great.

So I got a large background. Then I had to read the literature. As I said in my talk, quite early, you see, I was sent down to review this paper of [Carl] Eckart's, which was very important to us. We didn't quite understand it; it was all new to us. But I knew all about Eckart. I had no

idea that he was going to be permanently, like me, in fact, you see, involved in oceanography instead of his great work, very brilliant. He did more than what I've mentioned.

There was a big problem in physics in the developing of the new quantum mechanics. Two independent developments: Heisenberg's matrix theory, and Schroedinger's wave mechanics. They both gave the same results, but the physicists, who weren't great mathematicians then--in fact, not even now--you see, it seemed like two different worlds, like one was invented on Mars and the other on the moon, literally. Two people, independently, showed that they were the same thing. One of them was the great Wigner; the other one was Carl Eckart. They did it independently, you see.

NO: [unclear].

Nierenberg: But that's not the Wigner-Eckart Theorem. The Wigner-Eckart Theorem's even more important, you see. So to meet the great man from--he was a big [unclear]. But he already beat me. He had already been director of Scripps for two years in an interim way, and now he was director of MPL [Marine Physical Laboratory] for a few years, you see, although a lot of that was not significant at the time. I wanted to just talk to him about his paper, you see.

So little by little, just as I said, I started to be exposed to oceanography in general, and Scripps in particular, by meeting individuals. The ones that I didn't think much of, I forgot. But the ones like Walter Munk, I read his papers, you see, and then I met him. I remembered. Then even when I was at Berkeley, you see, I'd be on these committees and met John Isaacs, very important, very important. Then I was on several important committees of this nature, where I

learned more about it. But even when I was in Berkeley, there used to be a biennial meeting at NEL that involved all of these people, and I used to come down. I was invited, not to give papers, but to participate. I'd spend a day and I'd re-meet the people and developed this background. So, you see, bit by bit I was involved, even though it didn't really particularly interfere with my research and my central research at Berkeley.

NO: [unclear]?

Nierenberg: What's that?

NO: [unclear]?

Nierenberg: Well, different things, you see. After all, I entered in a very narrow way, through the physicist side. As I said, I knew very little about the biology, except that it was a nuisance, you see. More than a nuisance, actually.

NO: [unclear]?

Nierenberg: Well, there's a wonderful paper that Brackett Hersey wrote. It was a lovely paper, a very light thing, about an island that disappeared in the ocean. You see, they do these sound surveys, you know, topographic surveys, and there was one island that was on the map for a long time. I can't do it in the spirit he did it--it was very lovely--how slowly they discovered that it

wasn't an island, but it was a reflection from biological sound, you know, in the thermocline, as an example, to emphasize this point.

But I think the thing that struck me--and I'm very proud of it--is my paper that I wrote, it turned out to be a prize paper for the *American Scientist*, it was a prize that I got for a paper I gave at an annual meeting of Sigma Xi. This was about five, six years after the beginning of the deep-sea drilling project. I'm not boasting about the project. I can do that easily, but I'm trying to answer your question. But if you read that people, and other people do, they always come up impressed by the same thing that impressed me about oceanography as a science. You see, on one of the legs, sea-floor spreading was pretty well understood now because of the vessels crossing the mid-Atlantic Ridge. Later on, they did a traverse across the equator from south to north, in the middle of some particular area, and it came up with a beautiful picture in this article. I can describe it for you.

You see, what happened is, as the vessel--as you go across and you're coring the sediments, you see, taking three holes perhaps south of the equator, three holes north of the equator, roughly in the middle of the Pacific, you get a most extraordinary result. We start off with the Cromwell Current. The Cromwell Current is probably the biggest ocean current on earth, of any current. It starts 6,000 miles west of us, you see, at the archipelago, way in the west. It moves eastward against the wind, you see. The surface waters are moving the other way. It moves eastward. It's about roughly 200 miles wide. It's subsurface. It's probably about--I'm really forgetting things I should know. Has a thickness of roughly a couple of hundred meters. It moves at the center two and a half knots, which is an enormous velocity, you know, for an ocean current. Of course, the Gulf Current is even more, but this is subsurface. And it

just goes right across, all the way across, and hits South America. Depending on El Niño and not, it has different strengths and so on.

It's named very romantically after an oceanographer at Scripps named Cromwell, who had a great future, really an important spirit, who really outlined this current in great detail, died in an accident. You know about this. He and his gang were in a plane flying to meet a ship in Acapulco, and they crashed in the mountains. I should remember the year; I've forgotten. So it's named after him, the Cromwell Current.

But now, having that in mind, if you're a reasonably good physicist and you understand rotation of the earth and Coriolis forces, given the current, you say there's such a current, you can quickly figure out how wide it's going to be, where it will die out on the edges, and then you can even do some more subtle things. You can show that because of the Coriolis forces operating on it, that the very center at the equator you're going to get an upward flow. That upward flow, of course, brings up nutrient, which means along the equator you're going to get fish and so on in the middle of nowhere. So why is that remarkable? The middle of the ocean is pretty much barren. You only get life in great amounts at the coasts where you get upwelling. All of a sudden, in the middle of the ocean, because of the Cromwell Current--I can't work it out now--you use the right-hand rule to show the rotation of the earth and you put it all together. You can work out how big this upward drift is going to be. It all fits. You have to be given the fact that there's a current. Once there's a current, you get all its characteristics.

But now, you see, something else happens. Okay. That's the current. So you have life and radiolaria and all sorts of things, you know, living and dying and slowly sinking to the bottom. Think of it this way. These little shells sink to the bottom at a steady rate, so they leave

a chalk line, like you would do a woman's hem, you see. But that plate is moving at 10 centimeters a year to the northwest. Right? So this chalk line is moving, but it's doing more than moving, it's aging, because over the course of a million years, you get evolutionary changes. In fact, geologists use these changes to date sediments, in general.

But now this has been going on 40, 50 million years, you know, and so on. So now you take these cores and it's really wonderful. You have a three-dimensional picture of what's happening. You have this physical oceanography taking place, caused by air, climate, and so on, and all these details fit physics very beautifully. Now you drill these holes and you see these layers of radiolaria and related little animals, and you date them. You find that those dates are consistent with this ten centimeters a year moving, which you already know anything, you see, by other methods and so on.

To me, you say what impressed me in oceanography, when I saw all of this wonderful picture--and it's in my article--this wonderful thing where you have the physical oceanography, the biological oceanography, the paleontology, the marine geology, the atmospheric bit, all together in one picture that goes back at least 40 million years or more, including the plate tectonics, you see, then I realize that we've done something great in that 1955 to seventy years, and the drilling. You see, the fact that I was part of the drilling operations, to me, is a big thrill. We've done other things. What is the right word, when everything puts together?

NO: [unclear]?

Nierenberg: In such a grand fashion and it all fits, you know. I really have answered your question. I'm sorry.

NO: [unclear].

Nierenberg: When I had all of this. I made this speech, you see, with that in mind, and it's in the paper very beautifully. Somebody else did the drawings and so on. It was a big thrill. It was a big thrill. There were some lesser ones.

NO: [unclear]?

Nierenberg: As I say, there were some lesser ones like that.

NO: [unclear] [Interruption.]

Nierenberg: The what?

NO: [unclear]

Nierenberg: Oh, that's a wonderful story.

NO: [unclear]

Nierenberg: That connection is one of the most fun and interesting and, in one way, disappointing. I'll explain why. Here again, I've got to remember names. What happened was--let's start at the beginning. It's a long story, but well worth recording for history. It's in my memoirs, too.

What happened was, at that point I had two high-level appointments in the government. I was the first chairman of the National Advisory Committee on the Oceans and Atmosphere. There's a reason for bringing all of this in. I chaired that committee, a presidential committee. I was also--was that important at that time? Yes, it probably was. I was on a National Science Board, National Science Foundation.

Bob Frosch at that point was Assistant Secretary of the Navy. One day Bob said, "Bill, you've got to come to Washington. It's very important. See me."

I came in, and he said, "Bill, I'm arranging for a two-day briefing of super top secret codeword material for you." It was really impressive. I was buried in a cell somewhere, I don't even know where, and three-star admirals briefed me for two days on three or four projects of a very secret nature. The one that was important was the *Glomar Explorer*. The reason for that briefing, incidentally, was not that they wanted help from me, I'm very embarrassed to say, but two people on my committee, Chester Baird, who was a former Under Secretary of the Navy, and Elmer Wheaton, who was vice president of Lockheed, were on the committee. Both of them had clearance to this project, about which I had known nothing until I had this.

Let me mention something else. Two other people I knew had clearance: Victor Anderson here at Scripps is one, and Fred Spiess was another. They didn't even know that they

each had clearance. It was really secret. But they were called in for specific expertise. I was called in, I'm sorry to say, not because I was a great whiz in all this business, but sitting as the head of this committee, Bob Frosch was scared. We talked about many things. We had a very wide spectrum, a presidential committee that you don't fool with. He was afraid that one of those two guys might say something that would lead the committee in a very sensitive area, and he wanted me, as chairman, to know all about this so I could cut it off. Anyway, that was my introduction to the thing.

What happened then was, I didn't follow it very closely, I really didn't, although I knew what the project was. We had a very great person here, Bill McLaine, who's the investor, incidentally, or headed the team that invented the first guided missile. What was it called? You know. Sidewinder. Out in the desert. He came director of NEL, a very good one. Somewhere in this period he retired, and they had a very big party for him at Icelandia, one of the big parks in Mission Bay, and, of course, I went to it. David Potter, who was then Under Secretary of the Navy, of course, was there. I knew David very well for many years. David gets a hole of me in the middle of the field and tells me--I can't say too much about it. I don't know what is cleared and what isn't. But what most people know now, that the project was over, it was completed. I knew what had happened, actually, at this point moderately well, not in great detail.

And he said, "Bill, we've kept the secrecy. They did beautifully, this board. We're going to want to turn the ship over to Scripps to use for whatever you could use it for." I saw right away it could replace the *Glomar Challenger*, you see. "We're going to give you a small amount of money, like 5 million dollars, just to facilitate the transfer. We will clean up the ship." By that he meant he would remove all traces of what it had really been used for.

You see, the cover I have to come back to. The cover was manganese modules. See, I already knew that manganese modules were for the birds. That's another story. That had to do with my role in the Law of the Sea. I was a permanent delegate to that.

NO: [unclear]

Nierenberg: Could we come back to that? Yes. But it was well done. You see, that's where Elmer Wheaton came in. We even faked it, you see. That's how Lockheed got in, had a big operation going with particles that simulated manganese modules, and all right up here, you know, in Southern California, and he took a whole committee out to see this operation, even bamboozled our committee, showing the chemical and the calculations and how you're going to bring them ashore, and this mill that was going to grind them up. That was something that was really going as cover, you see. I don't know if it fooled the Russians, but it was cover.

NO: [unclear]

Nierenberg: But even our committee, you see, as an example, is a vehicle, without their knowing it, as cover. And Elmer Wheaton did all of that. That's why he knew about the whole bit, you see.

Well, in any event, let's get back to where we were on David Potter. So he offered, and I'm just glowing. I think it's fabulous. Scripps is now going to get this marvelous ship and the Navy is going to give us all the money to operate it for [unclear]. Five million dollars is just

peanuts. Not to me, you understand, to them. They knew I'd have a job. I'd have to hire engineers and do God knows what. So I said, "Well, you really should clear it with the head of the NSF." Or was he President's science advisor then? I forget. I was just glowing, all set to get this magnificent gift, when the story broke in the newspaper. I always forget his name--you know, that columnist.

I don't know if you know how it happened. It was a disgrace. The secrecy was magnificence at the working level. The operating institutions were the Global Marine Corporation, and I always forget the name of the company that makes switches in the Midwest. They were in charge of the electronics. In other words, they were in charge of the inside of the vessel. Global Marine built the vessel and Global Marine did a great job. It was just terrific. But they didn't get along, so Lockheed was called in. They were basically the managers. Besides the cover story, they managed the project.

For reasons that are beyond me--and that's the point you may be making--they brought in Hughes, to say that Hughes owned the ship--they didn't own it--that Hughes owned the ship and they were going to do the recovery of manganese nodules. Well, what happened is, you know, when you do that, you run into trouble. Some damn vice president at Hughes, who offered nothing, didn't do a damn thing, had a document related to the project that he left on his desk, that should have been locked up. It probably didn't [unclear] anyway. It disappeared. That, you see, unfortunately, trying to find that document and what happened, now brought in other people who had nothing to do with the project, looking for this document. And that's what leaked the story.

It turned out that the document had been picked up by a janitor who had put it in a coat pocket and hung it in a closet. That's a story that still has never tracked, but that leaked the story. That ended up my getting the ship and this nice money and the project. It could have been used properly. That ship had the capability of enabling us, you see, to do drilling in dangerous sediments, so that we could control the possibility of a blow-out. The ships we have now don't have that capability, strength, and so on.

Coming back, then--it's not the end of the story--the story broke. Yes, I was on the National Science Board. It was arranged that we meet with the President of the United States. That had been decided before any of this happened. We meet with the President, [Gerald] Ford, and, of course, we had a worked-out program of what we would discuss and so on and whatnot. He was just glowing. He upset the whole program. He said, "This is tremendous technology. Being able to do all of this is just so wonderful," and whatnot.

Well, I have to say, our leader was very nice. He said, "Well, you know, we have on the board Bill Nierenberg, you see, who is head of the project that developed this technology," which is essentially true. He said, "Let Bill tell you about the technology." So I had five minutes to tell the President about it.

I get back to my hotel. I was staying at the Cosmos Club. I had a message from the office back home here. These reporters, TV reporters from Los Angeles, were trying to get a hold of me because they were completely convinced that--in a way, they were right--that this whole Hughes business was a cover, that Scripps Institution of Oceanography was completely in charge of the whole operation, because they knew about the *Glomar Challenger* and all of this. They went at me for an hour to get me to say that. The problem, of course, as you might

understand, you journalists, is how do you deny it in such a way they keep believing that it's true? [Laughter] I succeeded, but it didn't get anywhere. You see, aside from the fact that it just simply wasn't true, what they had gotten hold of was just not long before that submarine sank, we had been drilling in that area, very close to it.

NO: [unclear]

Nierenberg: By coincidence. What they didn't look at was the complement. You see, we have an international team of scientists. There were two Russian scientists on board at that time. They really were, you see. They didn't know that. So it was quite a story from beginning to end, you know, and so on, but it was a big achievement. My Russian colleagues--it was not received too well in this country by certain elements that never think that whatever we do is right in science. I can tell you that when I talked to my Russian colleagues about it--we actually had a bunch of them come over on an exchange. I remember one of them, Igor--what was his name? I'm popping so many names right now, I can't remember it right off. Just saying, "You know, Bill, that's exactly what we expect of American technology." They were impressed. They were impressed by it, you see. Petalnikof [phonetic]. No, it wasn't Petalnikof, another guy.

NO: [unclear]

Nierenberg: What?

NO: [unclear]

Nierenberg: Well, that's another story. I followed that from the beginning. I'll tell you why. First of all, we have the biggest collection of manganese nodules here at Scripps that we had scraped up from the bottom. Whenever we had people visiting, big shots, they were delighted. I'd always take two of them out of a box, put them in a linen envelope and give it to them as a gift.

Then one time we had--what the devil was it? He wasn't President. Governor [Ronald] Reagan came down on a big oceanography meeting that somebody else arranged. Ed [Edward] Goldberg said, "Wouldn't it be great to do something." He took a few of the manganese nodules, put his chemists together in the lab, and he extracted a nugget of copper from it, cut it in half, bought a cheap pendant, gave it to the governor as a little gift. Then the other one I eventually gave to Vice President [Hubert] Humphrey. Didn't know what to do with the other one. I was going to give it to Governor [Pat] Brown later on, but we gave it to Vice President Humphrey. To the best of my knowledge, that's the only extraction of copper ever done in history.

Let me tell you why I was somewhat sophisticated about it. So we've done a lot. Scripps, before it was popular, had already done a lot in here. People like Gustav Arrhenius had worked out all the chemistry and everything. The geologists knew about the depth and whatnot. Scripps was well on top of this before it became popular and everybody went out on these expeditions and so on.

You see, I had had a prior experience. As I said, I was a chemical engineer during the war, without knowing it, but I knew some good ones. Manson Benedict was one of the really

great chemical engineers. It turned out he ended up as a nuclear thing. But he and his assistant, who was my age and I was very close to, by the name of Arthur Squires, undertook a major project for the oil companies after the war. They got about 60 or 80 million dollars. I forget one of the majors sort of led the project, but they pooled to do the oil shale business, and they did a great job. Arthur Squires and Manson Benedict.

[Begin Tape 1, Side 2]

Nierenberg: ...great job. You see, they had it all worked out. They could work the costs, they could use the economics of scale, and you used all the byproducts, the benzine. I remember toluene was a big deal and so on. And they found that they could, when they're all done, you know, do better than by a half-a-cent a gallon than you could do at the pump. Then the project dropped, was all shelved and so on. Why? Because the price of gasoline is artificial. The price of any major product--wheat, gasoline, iron, or whatever--they're all carteled, either obvious cartels or implied cartels. The prices are completely artificial, like you see OPEC controlling, opening spigots, closing spigots. All it means is, what the oil companies learned, if somebody went into it, they'd have no trouble just dropping half-a-cent a gallon and that would be the end of the efforts, you see.

Then I looked at the price of all the calculations on manganese nodules from the same historical viewpoint, and you see what they would do, the big thing then was not the copper; the big thing was the nickel. I'll tell you why I knew a good deal about nickel. In my Manhattan Project, the big program was to make an uncorroding system, an enormous system, that wouldn't

corrode against hydrofluoric acid, fluorid compounds. It turned out that the thing that solved the problem was nickel cladding pipes. By cladding, you roll nickel like an eighth inch on top of, and then you make whatever it is you make.

Well, it turned out we are short of nickel, in a sense. Nickel comes from Canada, comes from Cuba, and everything comes from Russia, and so on. So then I got an idea, we're not exactly short, you see. It's just that it's important to those countries, and they're going to undersell us, no matter what, so we never develop an industry.

Why do I know that? Another one is chrome. When I was head of NICOA, we made a big trip all over Alaska, because that really is a country of water, an atmosphere of fishing and everything. To name-drop, Arthur Godfrey and I were close then, and I flew with him in his plane. He had a beautiful map somebody had given him, of Alaska, that was a big aid in flying, not as a navigation aid, but identifying things. I noticed on the other side of the Kenai Peninsula, on the other side of the Cook Inlet, there was a mountain called Chrome Mountain.

Well, we actually ran in, later on, to a chap who was about sixty-five or seventy, just like you see in the movies. He had prospected all over Alaska. He could tell you about every addit in every place for every mineral. He said, "Chrome Mountain? Of course. I worked it once. There are three addits in that mountain. It's 30 percent chromium. That's why it's called Chrome Mountain. All you have to do is dynamite the stuff on to barges right there and take them down to Seattle someplace and do it."

Well, why don't we do chromium in this country? Why do we have to import it from Russia and so on? The Russians know all about Chrome Mountain. They probably know it

better than we do. And the day we start getting our first chromium out, they're going to sell it for 2 cents a pound less, no matter what.

I'm saying, I knew all of this. That's true of the manganese nodules, you see, and the nickel. There the nickel was the big thing, and I already knew about nickel, you see.

NO: [unclear]

Nierenberg: Was nonsense.

NO: [unclear]

Nierenberg: It wasn't actually cover. People didn't realize that it was kind of a driving force. It wasn't deliberate. I knew all about it. I was there, every bit of it. I used to listen. You see, these lesser developed countries--there are several streams here--which just drool at the mouth at any possible source of income, you see, and the whole idea was, you're going to drill, bring up all of this nodules, you're going to process them, they're going to get 10 percent of the profits. Not of the profits, of the gross, you see. And they're going to get all this technology.

In fact, that's where we stopped. There was a technology-transfer thing there, and that was the big stumbling block, why the U.S. didn't go into it. You see, the way they wrote it at the time was that you go out privately and you find apart of the ocean that has lots of nice, fine manganese nodules. The enterprise, this international enterprise, would have first choice, even

though you discovered it. Then the countries like the U.S., with their free transfer of technology for processing all of this--without going into detail, you see, it just got to be tasteless.

We went into it, you see, naively. The reason the U.S. went into it was really very good, I can tell you that. The U.S. went into it whole-heartedly. We were concerned about keeping the peace in the world. The passage through straits have always been a source of war, and getting worse. The idea was, the three-mile limit, we wanted countries--you see, we always talked about a three-mile limit, nautical mile. Everybody knew we meant twelve miles, you see. It was a bargaining point. We finally gave up.

Do you know what the end result of the whole business was? I'll give you the bottom line. I'll tell you more of the story for different reasons. Our Navy, of course, was very active in all this. The bottom line is, when all is said and done, the U.S. ended up with the largest fraction of fishing rights in the whole world, because we were driven, vituperatively--you should have heard the language used by China and the other countries against this horrible monster, the U.S. I'll come back to the Western European countries.

The end result is, we have the economic zone out to 200 miles, which, except for the West Coast, which, up till you get to Seattle, is not worth much here, but the gulf, the East Coast, and Alaska, and not only that, we're the one country that has really decent control of fishing. The Western Europeans have ruined the Mediterranean, they've ruined their area by overfishing, and so on. We have really preserved. That's why the salmon is doing so well in Alaska and so on. The Alaskans are very good this way, I might add.

But it's an interesting story. This happened twice. If you'd listened and followed it for those ten years or so, you realized that the U.S. was being abusively driven into this position that they didn't want. That's the way you read the record.

Well, there's a point in my life--not anymore--where I had very good relations with the French and the French diplomacy. Whenever a new consul general came into Los Angeles, he always came down with his team. One of them--gosh, am I going to forget his name, too?--was there because he had been [Georges] Pompidou's P.R. person, public man. When Pompidou died, he got this very nice, considered very good job. Rogagneau, his name was. Rogagneau came down with his people a couple of times and we discussed a million things. The French are completely convinced in two cases--this is one of them--that it was the brilliance of our State Department that tricked all these stupid countries while they stood by, incoherent, watching us getting all these fishing rights. I speak French fluently. I said, "Rogagneau, you don't understand. You're the only one in the United States at this moment that has that opinion of our State Department." [Laughter]

But wait. I'll give you another one. You don't realize--remember the oil embargo? That's a separate matter, but it involved transport. The oil embargo. You may not realize it, but if you did the total sum, we came out ahead in dollars and cents. The reason was, while we suffered at the gas lines, you remember, to get gasoline, we are one of the three countries of the world that have an infinite amount of coal. We're so loaded with coal, it's unbelievable. The price of coal just skyrocketed, and we, as a nation, we ended way ahead, as usual. Do you know the French are convinced that we deliberately tricked the OPEC nations into that oil bit to make all this money?

NO: [unclear]

Nierenberg: No, they're absolutely convinced. They're absolutely convinced that our State Department is so brilliant. I was sitting here, stunned, listening to somebody say something like that about our State Department.

NO: [unclear]

Nierenberg: Oh yes.

NO: [unclear]

Nierenberg: Well, it's very tricky. I have some opinions. I don't know about the most rewarding. I certainly think it happened that NATO is an extraordinary international organization. Most of them are not. Let me explain why. NATO has some weaknesses, too. I'm talking about science now. We had real money, which is unusual. No, we really did.

NO: [unclear]

Nierenberg: The famous NATO summer schools. Incredible success. I'm very proud of my role in that. I didn't invent it, but I picked them up from--Norman Ramsey was the first, and

Fred Seitz and I, and I expanded them, and they're still going today. Internationally, they're still the greatest thing in science. We had real money and we spent very little on overhead, to this day. We kept a very small office. Most of these places get money and they spend it internally on studies. We did very little in the way of studies of that nature. So I found that international aspect very rewarding. It really accomplished it.

The trouble with most of these international organizations--and I saw it in the Law of the Sea business and then in the current one--is that very much of it is driven by these lesser developed nations, particularly, to find a place where they could put their nephews, who can't stand the countries they live in. You can quote me on this widely or anything.

NO: [unclear]

Nierenberg: I saw this really clearly. See, I saw a little of this in NATO, you see. In other words, the countries pushing back home, pushing their nephews, of course, if you came then from Turkey, this is a cushy job.

NO: [unclear]

Nierenberg: No, no, no. Just plain jobs. I mean, you have a nephew who's had some physics or something back in Turkey, and you want a fraction of the jobs in Paris, and the guy gets five, ten times his salary in this kind of job, travels, his kids can go to international schools and so on.

But NATO wasn't that bad. But you get something like Law of the Sea comes up, they're going to build something in Jamaica, a big enterprise.

Let's put it another way. Let me give you an example. This is my experience. I went to the first big Law of the Sea conference, the opening one, in Caracas. I only spent a week. It went on for quite a while. But it was a crucial week. We were lucky. They had built, in the middle of downtown--the city is like in a big valley, and they've got these *barrios* up the side, like Tijuana, worse, maybe. But down in the center is the opposite. Well, Tijuana's like that, too. Down in the center are these elegant tall structures and apartments, upper-class structures. The reason they build them--this is after the fact. I'm giving you the result of my reasoning. That's the cheapest way to protect yourself against riff-raff and so on, you see, because you have a smaller boundary for a very large volume. Well, these things were built and furnished and they were going to be sold, but the government took them over for the delegates, like ourselves. We had an apartment. A very interesting experience.

They also put in, on the ground floor of at least one of the buildings, a really decent international phone system, where you could go to a booth and a lady would connect you up and you could talk to your wife, connect right off the bat. It was the only decent system in the whole country. They had to put it in special, you see. Nothing else would work.

Then, of course, being Americans, we had a delegation--that's another story--of about 120 people. It's a madhouse. I'll give you an example. The British maybe had ten people, the French about that. We had about 120. What happens is, though, therefore a lot of them were going back and forth to the U.S. You get to meet people from Sierra Leone or someplace, you know, pretty decent guys. They hear you're going back home. They give you a batch of letters

to mail from the U.S., because there's no way they could mail a letter from Venezuela that would get to Sierra Leone. But if you take it back home and put American stamps on it in Washington, it will get there. So I went home with twenty, thirty letters, whatever. They probably even gave us some money for it. That was not the problem. The problem is the letters getting there.

Well, when you realize that's what they live in, you can see why they want their children, this upper middle class, to spend their lives, you see, in Paris, even in Caracas, in Washington or whatever and not--not--back in Sierra Leone. And so the big ambition is to create, keep creating these international organizations for that purpose. And who ends up paying for half of it? The U.S., you see.

NO: [unclear]

Nierenberg: We're not that influential.

NO: [unclear]

Nierenberg: No. You mislead yourself. I was going to mention that. That's the second side of it. When you asked me what did I find was so interesting, sure, I like to think, surprisingly, of our society as integrated. I think the reason science is successful is the same reason other things are successful, like working agriculture. You name it. And, you know, when you see the difference, to give you a very narrow example, I think that's why we did so well during the war, why the Germans couldn't. Our society is so beautifully integrated in being able to do things.

You see, if you're a working scientist, you're working in an environment where other things are important. You may not believe it, when you're in a laboratory at three o'clock in the morning, you suddenly find you need a razor blade--now, you know, the Russian scientists don't. They'll tell you that. They can't just go out to a local store in five minutes and buy a razor blade. But you need it in the lab. You see, you don't know that you're going to need it, but you're going to need it.

In my project during the war, I can tell you the big item for us in the laboratory was the refrigeration industry. We had to be able to put together the systems in the laboratory that were noncorrosive, but were vacuum-tight for months. That's exactly what refrigerators are. We had a refrigeration industry and those are the fittings we used in the laboratory: copper and brass fittings that didn't leak, but also didn't use gum and other things that would corrode. It works both ways. The science feeds them also. Well, that's the big strength. So when you say as scientists you have a big influence, you do and you don't.

NO: [unclear]

Nierenberg: No, no. I don't mean it--

NO: [unclear]

Nierenberg: Yes, but look in Europe. They have them in government. They've copied us, but it isn't working very well. Did you read the *Times* today? My poor colleague that we all know

very well--what the hell's his name?--the geologist at the Academy. I know very well he's being besieged, the Minister of Science, because he's trying to make decent changes, and he's just being murdered, you know. You can read today's *Times*. I really think the reason scientists are effective is they also have a government that understands science, but understands other things as well.

NO: [unclear]

Nierenberg: It's the thing we were talking about. It's sort of the integration. It's the way ONR worked with the scientists right after the war to develop a system. You see, that coupled everything together. It really worked. It's breaking down a little now, but we're so rich that we can absorb it, because, you know, you build one Berkeley, every state wants to have a Berkeley. Then it's really hard. I saw this at NSF. NSF in the beginning was very good. They would put money into Scripps and Woods Hole. They built buildings here and all sorts of things, but then when every other school in the country wanted to have a Scripps, they couldn't give a library to every single--the end result is, they give none.

NO: [unclear]

Nierenberg: Yes, sure. [Interruption.]

NO: [unclear]

Nierenberg: Well, I don't think it's really all that different. They have a narrow view of things that they have to cover. Right at the beginning, you may not realize it, but, say, in late forties or fifties, so on, they were in high-energy physics. They built another cyclotron. Partly it's the connection with Rabi and Columbia and so on, but they did other things like that. But they got us Nobel Prizes. I mentioned Felix Bloch. He was supported by ONR when he did his nuclear magnetic resonance work. That was ONR. But that was good, because it turned out, whether they realize it or not, the magnetometer, proton magnetometer that I mentioned Victor Vaquier used, was his thing.

Walter put his finger on the difference, but he wasn't even exactly right. NSF, in principle, is forced very heavily into day-to-day grants to peer review. ONR, on the face of it, is not that restricted. It's a free thing. But in practice, I think it's more blurred than that. ONR does careful reviews. I mean, if it's going to be big projects, they may not do a very formal procedure, but, boy, they do a peer review. They don't want to be caught out. They're respectable scientists and so on. They don't want to be caught out putting money on something that's stupid, you know.

To pretend--I can say this for the record--that NSF is purely peer review, without--I mean, to avoid getting into some kind of a snitty kind of argument, I know specific examples where peer review was violated at NSF, you see, as an example. But by and large, there is this difference, that if ONR feels that they suddenly have a pressing military problem that requires stronger emphasis in a certain basic science, they'll move more rapidly than NSF.

NO: [unclear]

Nierenberg: No. I don't know. They both have their weaknesses and strengths, but I don't think they differ all that much. But the point is, in time, you see, very often ONR was the pioneer in the field. Then NSF took over. You see, ONR was the oceanographic support for many years and really built the damn thing. A lot of things you read are quite correct. But by a certain kind of bureaucratic maneuvering and whatnot, NSF took over more and more of oceanography and less and less the Navy, and it was a little unfortunate, because what you get in a lot of science, unless one institution is in charge, is the fact that the infrastructure, you see, will be--the idea is to get the other guy to pay for the infrastructure. Let me [unclear].

In the case of oceanography and ships, in the early years, you see, particularly, NSF wouldn't pay for ships, in effect. They'd pay for the researcher, the program, the travel, and the instruments, and they wanted ONR to pay for the ships, you know, and so on. That's a very common thing. If only one agency ran oceanography, like the AEC [Atomic Energy Commission] running nuclear physics, then it's only an argument internally, but it's easily settled by the head of the agency or the committee. So you have that problem in oceanography.

NO: [unclear]

Nierenberg: Oh, sure. I think the Navy was wrong. Absolutely.

NO: [unclear]

Nierenberg: And I'll tell you the reason, but it's a political reason. Actually, it goes back to one of your questions. The Navy should be active, in fact, the military, in building floating islands that could be used as mid-ocean air drones. We developed them at Scripps. We even made an A-scale model to show that it could work. I'll tell you why. Never mind the cost. Whatever it costs, it's going to be cheap. You see, what is opposition now, in fact, we're even losing it, is we have these very expensive bases that cost us an arm and a leg, directly and indirectly. Whenever there's a crisis, we can't use them. The thing is grotesque. But, you know, we could put a floating island base in the middle of the Mediterranean legally.

NO: [unclear]

Nierenberg: Well, for a number of reasons. First of all, it's not elegant. It's something we always say. It's not like being on an aircraft carrier or destroyer with the wind in your face and you say, "Steer right. Steer left." They're very ugly structures and they take very few men, very little manpower, you see, and there's no PXs, really. There's no place for families, you see. They're very bare-bones structures. They can hold huge amounts of fuel, I might add. They're indestructible because all you have is sheet metal. You drop a bomb, it's just going to go right through. What you'll have are *Flip*s, dozens of *Flip*s vertically supporting the structure. Even if you knock out one of them, nothing's going to happen. And all you need is about 5,000 feet in length. It's expensive. And they're immune to storms, *Flip* is, totally immune. They just sit there, you see, and they can drift or not, it doesn't matter.

NO: [unclear]

Nierenberg: No, no, just one purpose. Just one purpose: simply to be a--in fact, you could put in a refueling place for aircraft.

NO: [unclear]

Nierenberg: Well, that's more complicated. I think they're doing them all, in effect. I'll tell you, there are two problems. I don't mean this as a criticism, but the project that they're talking about is the ocean observing system. I'll tell you why it's so difficult to do. You would think that from everything you read, global warming and power from the wind and this and that, that we have an atmospheric observing system. Would you think that? We don't. You know, I don't know what fraction of the observations come from airports. Airports need--every four or six hours, they put up a radio [unclear] balloon, and they measure the wind regularly. When you're a pilot, in fact, some airports like Los Alamos have vanes at both ends of the runway. They need them at Los Alamos.

So most of your information has not been put in for scientific reasons. I mean, the sites have not been selected for scientific reasons, you see. In fact, it comes up in odd ways. For instance, just that, if you want to find places where you can put up, God help me, a wind mill, you know, wind-powered stations, it turns out your databank is lousy, because most of the data

comes from airports, the place where the wind isn't strong where, in fact, you don't want strong winds. What I'm trying to say is--

NO: [unclear]

Nierenberg: Well, you see, to fund a real ocean system, you don't have the equivalent, you see. We don't really have a real air one, is what I'm trying to say. It's being paid for in other ways. It's being paid by you as an airplane passenger, you see, so to speak. So it's a tricky business.

I'll tell you one thing that's gone down, I think badly, certainly at Scripps, is the science of fisheries. The biological science related to fisheries is in very bad shape.

NO: [unclear]

Nierenberg: Well, I don't know. I'm not that expert, but I can tell you, you see, when I came to Scripps, I was in the tail end of that. We got here--I wish I could give you the names real quickly, but mostly because of my association with John Isaacs. We had--God, what's his name? A member of the Academy.

NO: [unclear]?

Nierenberg: No, no, no. He's not in fisheries. No, no. These are real fisheries experts. Benny Schafer. You see, Benny Schafer, he was in the Academy quite early. The we had--oh, gosh, I'm sorry--another chap who was with the fisheries people on the outside, but he had--

RR: [unclear]

Nierenberg: What?

RR: [unclear]

Nierenberg: No, no. Oh, God, he was a very difficult, irascible man. This is awful. I'm unprepared, you know, for this. We can get his name later. He was an associate at Scripps, in effect. But there are others. I can give you the names. Of course, we had a state committee that had been formed. Sedi [phonetic] was the name of one of them. There were several good people, whose names skip me for the moment, up at the fisheries lab here, worked very closely with Scripps. In other words, we had about eight people. There was the chemist who had that kidney problem here at Scripps, who was very good, who did the kind of research that had to do with the nutrient aspect of the whole business. Our people were strongly coupled in to the [unclear] Fishery. In other words, it was quite an active thing, probably partly a fallout from the failure of the sardine fisheries, you see.

Well, a variety of things happened. I tried to stimulate it, to get them to teach a course, a major course. I failed. That's one reason they didn't get along with each other. The reasons are obscure to me.

The second problem is that most of them died young. It was one of these odd things. Strickland. John Strickland was a chemist, very good. He died of that--you know that disease of the kidney, inherited disease. John didn't live all that long, but he outlasted the rest of them, Benny Schafer, Sedi, and I'm leaving out at least two other names. So they were a very strong group, but as they disappeared, even John and I tried to reinvigorate this activity and go out, and we simply couldn't. There was only one person, and he was in Rome and he was not moveable.

NO: [unclear]

Nierenberg: Yes. Well, there weren't that many. They weren't being turned out to have this deep interest. You see, it's a mixture. Somebody like Benny Schafer was a combination of scientist, came from the University of Washington. That was the main source for these people. He was a combination of a marine ichthyologist scientist, a diplomat. He knew what was going on in the world in the State Department, you know, and so on. I mean, he had all of this. Economist. He knew the economics of the business very well. It was true of most of these people to a greater or lesser degree. We don't have them. We have some fisheries economists around the world. What's his name? Rothschild, Brian Rothschild, as an example, who used to be the director about that time, you see. After we had an Englishman, Longworth, who was very good, but he went back to England. Then Rothschild took his place. Rothschild is still active,

but he wasn't as rounded as somebody like Benny Schafer, you know. It was quite remarkable. John Isaacs was very good at this, maybe less on the economics, but better on other things. Strickland was pure science, but very, very good, and he invented devices that are still being used for measuring the productivity or the basic productivity.

Well, we lost that, the whole community has. So a lot of the talk that you hear today about the vitality of fisheries is just talk. It is not supported, you see. I don't want to be critical of it. I know some things about it empirically, but it's lost the basis that these people used to give, you see. You don't have many people like them left. The FAO's not bad, I might add. They're pretty good. So I really feel that. Don't think I didn't make an effort as the director. I worked with John to try to rebuild it. I tried a couple of tricky things, but I failed.

NO: [unclear]

Nierenberg: Well, I exaggerated a bit.

NO: [unclear]

Nierenberg: No, no. They wouldn't go into fisheries. That should have been NOAA [National Oceanic and Atmospheric Administration], and it is NOAA's area, you know, and so on, but the people--what I'm trying to say is the kind of people--you know, actually I probably am appreciating them more in retrospect than in some ways I did at the time, you see. It's more missing them and realizing the resource that we lost. But it's partly their fault. They were not

doing the right thing. They should have devoted more of their time to giving courses and training new students. That I did try to do, to get them together. I failed. I failed. I wanted them to pool their resources and give a major graduate course, you see, in fisheries.

NO: [unclear]

Nierenberg: No, no, no, no.

NO: [unclear]

Nierenberg: CALCOFI was very important. These people, in a sense--I don't know the full history before CALCOFI, but CALCOFI is what put this group together. It was CALCOFI--well, I have to tell you a story. This is anecdotal. As director, you have to have these stories to entertain people, to some degree you. So I have a standard story that I would tell. This is a fascinating story, very similar in spirit to the story I gave you about the equator and sea-floor spreading and the Cromwell Current and about the climate. It's when I was pushing climate very heavily and the disappearance of the anchovy, you see, the anchovy fishery. I had a punchline that I'll come to.

But I would tell this story, how it was remarkable, how they called on Scripps, you see, to bring back the fishery, and how that developed a whole lot of science via CALCOFI. It did. There was a lot of spinoff. We never brought back--I'll come back to that--we never brought

back the fishery, but we brought in a lot of science. And I would say this great noble effort at Scripps.

One day I happened to be on a stage with Walter, and [unclear]. After I sat down, glowing, and I got the audience going, Walter said, "Well, Bill, the real reason it got started was we had lost our one and only ship and [Harald] Sverdrup had to get another ship." [Laughter] You know, *c'est la vie*. He was there, you know. Well, it's something in between. You know how these things--but the end of my punchline would be, you know, I'd say, "Well, we didn't really bring it back, things like climate, you see, and so on, all of this wonderful work, one of the great things we did was we do know our California shoals, and that's valuable." Our beach is the most valuable property we have. No question. Dollars, whatever. And we've contributed to a way of evaluating conflicting--and they are--conflicting interests. But then at the end I always grin and tell the audience, I said--sometimes use rough language--I'd say, "You know, if we brought back the stinking food processing plants--" They have one model one there. "If we brought the fishery back, you'd have these stinking things along the shore. We'd have been run out of town. Look at all the money Monterey is now making of these beer joints and red-light districts and the whorehouses and everything else." I say, "You know, way more money than they ever made from the 500,000 pounds of sardines a year that stank like hell, I have to say." So, you know, *c'est la vie*. But it did pay off, and CALCOFI is still going on, and it's very valuable.

I'll tell you, as an example, but we had the oil spill off Santa Barbara. People were talking right away about the damage that was done. One of the things we could do--and there was, to the seals, the birds, you know, and so on--is we sent a vessel up to do a plankton count at

the CALCOFI stations. There was no change in the plankton. I suppose when you think about it, why should it affect it? To some degree you say, well, the sunlight, the photosynthesis, but there was no change. We could go back to same spots we'd been going to for years. But there are other uses and so on. It's been very valuable.

There was a small tax that was paid for initially by small tax on fisheries. That was part of the problem. You see, it was a small amount of money. There was a committee--I wasn't involved with it much--that advised the legislature on how to spend its money that they taxed, and there was a lot of squabbling over that money. I never got involved in it. It wasn't worth it. It wasn't that much money.

NO: [unclear]

Nierenberg: Well, I'm skeptical about very specific claims.

NO: [unclear]

Nierenberg: I'm skeptical about whether the sea level rise is going to amount to anything. You see, the new numbers--they're not new numbers. You see, the kinds of thing we're talking about might be visible about 150 years from now.

NO: [unclear]

Nierenberg: No, they're not. It isn't a lot.

NO: [unclear]

Nierenberg: Let me give you an example. There's this writer, this reporter--very amusing to look at semantics--by the name of William Stevens, who's been in the *New York Times* and has been talking to the wrong people regularly. I told him that in the beginning. He called me a year ago. First of all, he started off for years--he was talking about the majority of scientists.

NO: [unclear]

Nierenberg: Wait. Wait. Wait. Wait. But you didn't watch it. Then about a year and a half ago, two years ago, there was this chap, Robinson, that did send out a--on his own money, sent out a questionnaire to about 18,000 scientists who might know something about it. The questionnaire was simply about the Kyoto business. "Do you approve or disapprove of it?" And then there were three boxes. "Are you a Ph.D. scientist? Are you a geoscientist? Do you just have a degree in science?" The overwhelming--about one-third of them were Ph.D.s. About two-thirds of them were geoscientists. Even though he sent out 17,000, he got 20,000 sponsors. They just Xeroxed them and gave them to friends. They were overwhelming, I mean like 98 percent, totally opposed to Kyoto.

Not a word of that in the *New York Times,* not a word. Not Stevens. All these things show up there, but you know what happened? He changed his word. He went from "majority"

and "fringe." Right after that, he now talked about "mainstream." Do you know what his name was yesterday when he was writing about the climate oscillations? That was yesterday or day before yesterday in *Science Times*. "The dominant thinking." That's been his change. It's like the sea level.

NO: [unclear]

Nierenberg: Now, look. The sea level started that in forty years, by the year 2040, fifty years, whatever, the sea level rise would be 25 feet. Then it dropped to 7 feet. Now it's going to be 2 feet in 150 years. You won't find a word of that. They don't even talk about sea level rise. He doesn't. Other people still talk about the atolls drowning. Well, what should I do as a scientist who has some personal respect? These aren't my numbers.

NO: [unclear]

Nierenberg: The things that you heard the other day. People are now so corrupted in the business, when you have thirty people and you're running a 50-million-dollar-a-year computing operation, turning out a bad model, they're all bad. I can tell you in detail, and so can everybody else, what's the matter with these models, and they make no effort. The physics is wrong, the basic physics.

NO: [unclear]

Nierenberg: No, but I'm just saying, I mean, you know, what I'm telling you is correct. The accepted number now is--and that number, incidentally, implies there's no change in technology, no change in the way you're doing energy, you see, and it's already showing his CO_2 curve is slowing down. It's not going up as fast as--and they're still projecting the very rapid upward growth. And even with that, they just get a two-foot rise. And what's worse is, it's being measured. The sea level rise is being measured very beautifully by the Poseidon satellite, and it's coming out between zero and 2 millimeters a year.

NO: [unclear]

Nierenberg: What's it got to do with climate? I could show you a record of supporting climate before these kids were born. I was telling you about CALCOFI, and that work was on the climate basis because there's good evidence that a lot of that was climate-induced. I can show you my record on that, but I don't know what the hell they're doing, with what climate's got to do with what they're doing when they're making guesses about what CO_2 is going to be 150 years from now. That has nothing to do with climate. Population growth and so on, where is climate related to that?

You see, climate is only half of that problem. If they'd just stick to climate, it's one thing. But it's like the sea level rise. And then there is alternative data they're not using. But I'll give you something worse than that, that's being thrown back in the face of this administration right today. Did you notice? You see, in the debate that's going on now--and, of course, our

administration is right in that respect--they've just concluded this so-called agreement on the modified foods, the biologically modified foods, and we're pushing the hell out of it. The reason it's being blocked by France and so on is not because they're scared of it, it's because they'd just be overwhelmed by food.

This happened before. You wouldn't know about the chicken war in the 1960s. When I was at NATO, I could watch it. Same thing. We were using hormones, you see, and the argument in France was it would affect the male virility. It did that until their industry caught up ten years later, you see.

NO: [unclear]

Nierenberg: But I'm just saying, so here--but let me tell you what happened in the debate, you see. We were arguing, of course, that you should do all of this on science, you see, that science should be the basic for all of this and so on. That was the U.S. argument. Really what was at stake, it's always the same thing, was we want to sell the stuff. And the French don't want us to sell it, no matter what the cost, you see. That's what it amounts to. But we were arguing science.

You know what the counterargument was? They said, "Then why don't you use that argument in Kyoto?" In other words, the Europeans are saying it isn't science; it's preventive. It's something we imagine might happen, you see. Therefore, since maybe we imagine it could happen, the hell with the science. You see, we're scared. But that's the argument in Kyoto. Two-foot rise doesn't bother anybody. So they went and they said, "How come you have that

position in Kyoto?" That just happened the other day. And I think that's what forced the closing.

So you ask me about--I don't know what that has to do with climate. I don't think it has anything to do with climate.

NO: [unclear]

Nierenberg: Well, it's kind of tricky. That's the system we work on in this country. It's not bad. It's a little better now, but, first of all, it's not done on a campus. I mean, that's absolutely not done. They're very funny rules. You may not understand this, but--well--

NO: [unclear]

Nierenberg: I won't go into detail. My office is cleared for secret, and I can keep secret documents in my office, but we don't do secret research on the campus at Scripps. We never have. I mean, not in recent years. In fact, one of the things I did, our crew had to be cleared for secret when I came to Scripps. I got rid of that requirement. But the secret work is done at Point Loma.

NO: [unclear]

Nierenberg: That's MPL. That's what I'm talking about. It's only MPL, pretty much.

NO: [unclear]

Nierenberg: Oh, that's perfectly normal. The other thing, this is really a little subtle and to some degree not proper, but you can't get out of it, somebody like myself of Spiess or other people doing secret work--well, I'll give you one beautiful example. UC will recognize a large amount of it that's transferable, like certain instruments and so on. It gets into the mainstream, but in a certain sense it means that other people don't have that privilege to kind of get a leg up. But you did it, you see.

But let me give you the example off the top of my head. I said something, one of the greatest things invented at Scripps was the fast Fourier transform. People really at Scripps to this day don't realize it, it was done at MPL by Phil Rudnick, whom I knew before he came here, incidentally. He came with that wave of the Korean War. And the reason he developed it is quite clear, very secret work that had to do with the steering of these arrays, not the low-frequency ones, I might add. High-frequency arrays.

But in the steering of these arrays, the other breakthrough was Anderson, Victor Anderson's digitizing and using the digital procedure for steering arrays and also for u. What he did was, you had ships making noise in the background, you would put zeros where the ships were to suppress them. So he did this by manipulating all of that. Anytime you do anything like that, it always requires Fourier analysis just automatically. But then, you see, here you had this genius developing the fast Fourier transform, which reduced the work like this.

Well, it's a peculiar thing. Here in this institution we had all this work going on in this building, they didn't know anything about it here. They could have, but they didn't.

NO: [unclear]

Nierenberg: What's that?

NO: [unclear]

Nierenberg: Lots of people. Not because it was secret, no. His work wasn't secret. That part wasn't. Some of it was, some of it wasn't. It's just that people weren't bright enough.

Let me tell you a story, since you want to keep a record of it. There is a story here. I don't know, he must have been doing this work for years, Phil, and very quiet man, actually with a religious streak, too, but a very quiet person. A very bright physicist. I can't give you the time scale, but certainly years after he started to do this, there was a very powerful group at Bell Labs, headed by a guy named Tokey, John Tokey. He was a very good statistician, very well known. They were fooling around one day, and suddenly they came up with the fast Fourier transform.

Well, to bring in another character we all know, Dick Garwin, very brilliant, who was at IBM then, but one of our well-known physicists, very versed in secret work and unsecret work and everything else.

[Begin Tape 2, Side 1]

Nierenberg: You could see why I follow your school's history, and I know about the Westinghouse Prizes.

NO: [unclear]

Nierenberg: Only they're not Westinghouse anymore.

NO: [unclear]

Nierenberg: Intel, I think.

NO: [unclear]

Nierenberg: What happened, let me go back. So Tokey and his people discovered, in a straightforward way, this marvelous thing called the fast Fourier transform. Garwin was very much taken by it. He's a very direct person. I was even subject--he went around the whole country, like Mohammed selling Allah. I'm serious. Everywhere, just telling people about this wonderful thing, describing it. Again, my time sense isn't there.

After some period of this, somehow he discovered that some unheard-of person in the Marine Physical Laboratory had been using it and publishing. You'd have to know Garwin to know this, Garwin was immediately indignant at this person he didn't know. Picked up the

phone and called him directly and gave him hell, you know, for not advertising it, for not turning out something useful like this to the world. You know, Phil Rudnick, as I tried to describe to you, a very modest, quiet guy, finally when he got a chance to put a word in edgewise, said, "Well, sorry," he said, "but it never occurred to me anybody would do the calculation any other way." It's a true story.

NO: [unclear]

Nierenberg: It's a problem. I'll tell you something that Spiess doesn't even know. It was a problem. See, there weren't many professors in MPL then. Two, maybe. Three? I don't know. Spiess was one of them, and he wasn't originally. What happened was, I think even when he was head of MPL, he may not have been a professor. I was in Berkeley. As I say, I'd become well known in the field. I knew Roger and people like that.

So one day--this was routine in our university--I get a letter from Roger asking me could I write a letter of recommendation. Our school is very good about promotions, very thorough in review and so on. I knew Spiess' work, and I wrote a letter, and a very strong one. But, you see, I knew the secret work and I could describe it well enough without violating secrecy. So he got his promotion. There were probably a couple of people here, but he had to have a letter outside of the local area, obviously, and I was a very logical person.

NO: [unclear]

Nierenberg: I have to say, just for the record, I was a little bothered. I don't know how they got out of it, but some years later when Spiess came up for full professor, they wrote to me again, and I maybe was too honest. I wrote back, I apologized, I said, "I can only stick by my last letter," because I hadn't learned anything in between about the secret work in between, and to do that, I would have had to come down and read papers. But he was promoted, obviously. But that's a way out of it.

NO: [unclear]

Nierenberg: No, they don't have to be university-wide committees, by no means, but there are different rules. The review committee, say, from associate to full professor, whatever, you know, would consist--it used to be around five people, only two from the department, three from other departments. But usually in science, but not the same science, one in chemistry, one in biology. That's one of the great things in the University of California. In fact, it's sort of a big argument.

Part of my talk may have mystified you when I said the great support we get from the University of California. There's a bit of tongue in cheek there, because they rob us blind, you see, in overhead. But still, in an absolute sense I'm right. Every once in a while, when I was director, people would come to me and say, "Why do we want to stay with the university for? We could go out. The faculty's big," and so on. And I would discuss it with some of the senior serious members. The almost important reason was exactly this review, this one narrow thing. If the institution were by itself, we would not have enough base for reviewing people, and you

start getting internal promotions without adequate review. Being part of the university and so on, sometimes reviews aren't fair, or this or that, but you know it's a real review and gone over very thoroughly and goes through a process of higher committees, you see. It's not just review committee, then you have your academic personnel committee. I don't know what they call it now. I think that's what they call it now. It used to be called budget committee in our day. So when I really made that statement, that's what I was referring to probably more than anything else--the broad intellectual climate of a great university.

You go a little too far if you start talking about art and music, but if you're talking about chemistry, even political science, you know, it dies away toward the edges, but it's still a big thing to review your work in scholarly nature and so on, whatever people think of, and that's what I mean. You see, on the narrow side, if Scripps were an independent university, we would do far better financially, way better. Way better.

NO: [unclear]

Nierenberg: Well, not frustrating, but I had failures. One of them really was very successful. See, I brought to this place my experience in international affairs. And if there's a subject that's [unclear], we still have that group of people I mentioned, Benny Schafer and so on. I got, actually with the help of somebody on the outside, who was here in town running the Salk Institute, who had come from Ford Foundation, we got a nice lug of money from Ford Foundation to start an international institute. But anybody can do that without a real goal in mind.

I'm going to give you another long story. It's not simply to do that, even aside from anything else, I looked at great laboratories and had experience with them, like the Lawrence Livermore Lab. It's enormous. You have no idea what capabilities are [unclear]. Here I came in as a professor from elsewhere. I was well in my research and I was on the campus, but well associated with the upper campus, and I needed, believe it or not, a cubic foot, more or less exactly, cut of soft iron. I put in an order for it, expected three or four months before it would come. That afternoon it was delivered from the upper campus. They had the stock and they had the kind of torches that could cut it and bring it down, you know. It's really something.

Why did I bring that up? What got me on that?

NO: [unclear]

Nierenberg: Oh, yes. I said, but still, as big as that laboratory is, and it's a huge thing, they're not directly connected to the real world. They're not big enough. Let me give you an example, one that really worked. I mentioned Bell Labs. Bell Labs did the basic design work and thinking in the laboratory, they were great, but the actual array was done by Western Electric. You see what I mean? They were the big outfit that did things. You see, a laboratory like the great lab, like Lawrence Lab, I realize had to have a connection to the outside world, and they did through a variety of companies and connections in the federal government and the military, just like, in a sense, a part of Scripps had a connection through the Navy, you see, and so on.

So the question is, how are you effective? As an academic institution, no matter how good you are, you can't be effective, for a number of reasons. First of all, you're more focused

inside. You don't want to be too de-focused outside, you see, to do these big things. So you want to have some connection, some group that connects you to the real world.

I had enough experience at Berkeley to political science department, saw how they worked, and with NATO and the professional diplomats and so on, to realize that they're the guys to connect with. They're the ones that sell. Just like the [unclear] economists, you see. The physicist that makes the discovery doesn't really sell it; it's the economists that do, the good ones, you see. So I want a group at Scripps which is a bridge that would connect us to the real world diplomacy, Law of the Sea or whatever you have, because then the people in this would be a halfway thing. The people in Scripps, they enjoyed this, would be mixing with the political scientists who would not have [unclear] at Scripps. They'd get enough money, but they would be like associate professors in political science departments here and elsewhere.

They'd come and spend a year here, paid for and everything, and they did. For a year or two it worked. I had the money for it. They even went on board the ship for a week or two, to get some idea of what the real world was, and then they'd go back home as graduate students and start to worry, like in Law of the Sea, and many of them, I still meet some of them who have now retired, actually, full professors at MIT and other places, one guy in Virginia, so on.

But, you see, one of the problems--that was a success, and I had a terrific steering committee from outside, some of the best people in the country who knew all this, including the top man in science at the Department of State. But what happened is, you see, you start something like this going, as director you can't run it yourself. My approach is to start something, you know, and then after it was running well, to turn it over to somebody so I can

start another project. Deep-sea drilling was that. Sea-grant colleges was done this way. I tried to do a project a year, you might say, this way.

The trouble with this one is, you needed a sophisticated person to run it, and that was Warren Wooster up in Washington. He loved to do that. He'd had this experience in Paris, and he did a great job.

NO: [unclear]

Nierenberg: He quit. He quit Scripps, no, no, for another reason. You can turn that off when we get to it. I can tell you why he did. He quit and went to Florida. Just like the fishery business, I couldn't get anybody of that sophistication, and I just wanted that sophistication, to take his place. So it was not a lack of money.

NO: [unclear]

Nierenberg: Yes, but the people that were here in those few years went on to great careers as political scientists. But, you see, it's this [unclear]. I don't look at it as kind of a toy or it's fun or whatever; I look at it as a way to bridge to a community that will sell what I consider the important aspects of ocean research. A lot of that was Law of the Sea. But there's minings, the [unclear], the environment, and so on. That's what these politicos do. I mean, I'm talking about the good ones, you know. In the climate area, I know some very good economists like this.

NO: [unclear]

Nierenberg: No. I think that would be an error. See, that starts to make you a whole university, but you're not. Not only that, you have trouble judging. You see, you're back to the judgment. You're depending completely on outside opinion as to whether he's a good economist or not.

NO: [unclear]

Nierenberg: That's exactly right. And the other problem, the other side of it--you're taking me back now, this is--in the fisheries area. Benny kept trying to get the upper campus economics department interested in the fisheries problem. They were totally uninterested. I had this problem in Berkeley. They want to know what happened to the [unclear] and the upheaval in 1860 or something, very safe things that won't get you into a controversial area or whatever, you know. But you have to then go to other schools or you train people. What happens is, we got these young political scientists, [unclear], they went elsewhere and built a school on that, you see.

You want to turn it off? What happened to Warren Wooster, you see, that's part of the problem. He-- [Tape recorder turned off.]

You know, even before the Mohole [Project], the marine geologists--Emiliani is a typical example of a very good one, and [unclear], a lot of guys, they knew that the important thing to do was to drill the sediments, you see, and get that history of the earth. I'll come back to that.

Then you had this wild-eyed group, just crazy, who said, "What we want to do is a big thing and drill to the mohorigic [phonetic] discontinuity." Just on a scientific basis, there was an error in both cases, but in the first one it didn't matter. The mohorigic discontinuity really doesn't exist, you see.

NO: [unclear]

Nierenberg: It was a refraction thing. And I already had learned that in my work at Hudson Labs. Reflection shooting will show you changes. Refraction shooting is very complicated to invert, and a mohorigic does exist, but it's not a sharp break. They would drill through it and not know it. Not that drilling is a bad idea, but the whole thing was unsound basis. We could go into the politics. I know that very well, completely.

The other guys were wrong for a different matter, but it worked out, you see. Actually, before the drillings really started, it had already sunk into them that the idea that the sediments would give them the whole history of the earth was wrong. They suddenly realized that it would never be more than 300 million years [unclear] Jurassic, you see.

Then the other era was they hoped, guys like Bill Reidel, that they would get--you know, like in the opera, the Holy Grail, that they would get a core that would be continuous, all the way down in history.

NO: [unclear]

Nierenberg: That's not the way geology worked. But, in effect, they did. Of course, you get one here, you get one there. It's like tree rings. You put it together, you know. But because of the plate tectonics, as far as the sediments were concerned, you got nothing more than about, I forget, 170 million years?

NO: [unclear]

Nierenberg: A hundred eighty? I forget. You're right. Not quite 200. I know where it is, too, in [unclear], yes. But it paid off in a hundred different ways, you know.

The other thing, then, of course, got mired in idiocy. There are two problems with the Mohole. One was very poor management on the part of everybody. The Academy loused it up. I heard the story from everybody. I had very good luck; I was away for those two years. I'm the only person. I probably would have been sucked in, probably on the Academy side, you know, so I heard the story. In fact, Fred Seitz, I don't think to this day he realizes, was president of the Academy, I see him all the time, and I knew the head of the--I can tell you another story of how it affected Scripps. The man who was director of Brookhaven--that's an earthquake.

NO: [unclear]

Nierenberg: The man that was director of NSF. But the other problem, you know, everybody was managing--there was a committee of the Academy, you know, and then it ended up in the Office of the Director of NSF, and then the bidding process was stupid, mishandled very badly.

Then, of course, it got in the hands of the Senate, a great row. Then that $25,000 gift of Brown and Root to Johnson just killed it, you know. They were cutting steel. They were cutting steel at that point. But the big problem was the mismanagement. Here I'm doing badly again--I know him like a brother, the man who was head of the NSF then, the physicist who had come from heading Brookhaven.

RR: [unclear]

Nierenberg: No, not Bob Wilson. He was never at Brookhaven, no. What the devil is the matter with me? All these different things are popping up at once. But what happened was, I knew him very well. I knew him as well as Rabi and anybody else. I'm a new director of Scripps, you know, and I don't know at that point if we were already starting to do the contract for deep-sea drilling. I get this telephone call from him. Lee Hayworth. Lee Hayworth, whom I really knew very well. Lee called me and said, "Bill, I've just come out of a meeting of the National Science Board."

[Referring to earthquake] That's a big one. Not too far away. I can tell from the sharpness.

NO: [unclear]

Nierenberg: That's right. I've gotten pretty good at it.

What happened is that he called me, said, "Bill, I've just come out of a caucus, a meeting of the National Science Board, and we would like very much for Scripps Institution of Oceanography to take over the Mohole Project." They're cutting steel and so on. He said, "We would like you to join with Hawaii," because, you know, they chose a spot north of Maui, that flat area. In fact, we did most of the surveys. George Shaw had done most of the survey work there. But he said, "If you feel you can't work with Hawaii,"--that was, of course, [Senator Daniel] Inouye--"If you can't work with Hawaii, that's all right, too."

So I thought about it by myself, I didn't talk to anyone, and I realized this project had been going on this time--I didn't know all the scandal yet. I knew they were having a hard time. But, you know, I'd been at this university a long time. If we had been in the beginning like we were in deep-sea drilling, you start small, you have a few dollars, and you slowly build, you create engineers, and you sort of grow together. This thing, you know, they had the contract out on, 25 million dollars, they were cutting steel at NASCO [phonetic], they're cutting steel at Brown and Root, you know, and I had Jeff Frauchy, who was very good, but I needed six engineers tomorrow, you know. So I thought about it, and I knew I'd never get it, the university would never give me the money for that, no matter what.

So I called him the next day, very sorrowfully, and said no. I said, "It's very attractive, but we could do it [unclear], but just no way."

NO: [unclear]

Nierenberg: The next day I get a call from the vice president of the university. He was a mathematician. I'm forgetting his name again, from UCLA originally. Giving me hell. It seems that a member of the board was a geologist, very well known, who, of course, knew all of this, and he was very upset that I was turning down this plum, and he called the president's office. So the president's office called me and gave me hell for having the temerity to turn down such a plum without talking to them and so on. I forget his name, but I knew him.

I said, "Look. This is only twenty-four hours later. I can pick up the phone and call Lee right now and say we're on. There's no problem," I said, "but, you know, I need about five engineers and I need the money for, say, six months' worth before we can get money flowing from the NSF to take over this contract." I said, "You're in the president's office. Can you get me whatever the money is so I can hire five or six engineers right now (this is really paper engineers, you realize) to catch up, to keep even with these people?" I never heard from them again.

But that isn't the end of the story. About six weeks later, I was in Washington, as I always am, eating in a hotel I can no longer afford to stay in, the downtown Statler, in the coffee shop, and I'm reading the [*Washington*] *Post*, you know, and about the fifth page near the bottom, there's a little article like this, dated back to about when he telephoned me, saying that a secret meeting of a certain Senate committee, they moved to kill the project. I was very upset. He was a good friend, he really was. The way I read it was that he called me, knew about this, and the only way we could go the political route is with Scripps and its position, and the University of California behind us could do something to reverse the whole bit, you know. And that's the way I felt for quite a number of years.

Well, then I don't know when it was, many years later there was a biologist married to Bill Benson. Bill Benson was the geologist that was sort of over all the whole business, a wonderful guy and a good geologist, but very naive. He had been manipulated into all of these crevises [unclear] where everything fell apart. I was talking to Bill about that, saying of my unhappiness with Lee, and Bill said, "You know, Bill, I know what you're talking about. I remember that meeting. I was there. I also remember that article. But I can tell you that Hayworth knew nothing about that meeting." Hayworth was dead. But I felt a lot better, I tell you. I felt a lot better.

But to give you an idea how bad things got to be, it really was so badly mishandled. It would have been a valuable project. You know the Russians had the same problem on land.

NO: [unclear]

Nierenberg: Yes. Political problem. Then they restarted. Then also they ran into wonderful results, and it's had no effect on the geology. They found water at much greater depths, you know.

NO: [unclear]

Nierenberg: Oh, yes. They found water there. In other words, the cracks were still there.

NO: [unclear]

Nierenberg: It's not the water, but it was the cracks, which is weird.

NO: [unclear]

Nierenberg: Well, there are so many things. Speaking of the institution under my reign, I'm proud of the students we get and what they do. I think they're fabulous. I've heard some criticism formerly about our teaching here. You know, I don't work that way. I look at the bottom line. You see, you put in money at the top, you get the research out at the bottom. That's the deep-sea drilling thing. We were in the budget. Look what's coming out. The students, we're putting in the students, look what we're turning out. They're fabulous. Even when I became director, what impressed me--it isn't so much today--wherever I went in the world, to an oceanographic institution, there was somebody who either studied here or got his Ph.D. here. I go to Canada, there's Ron Cameron. He was the big-shot in oceanography. Wherever I went, you know. There were a couple of exceptions. One of them was, of course, Lamont [Lamont-Doherty Geological Observatory], and the other one was Woods Hole, but Woods Hole was built by Scripps.

NO: [unclear]

Nierenberg: No, but it's true. The record is there. Actually, it was the Rockefeller Foundation. They did twice.

NO: [unclear]

Nierenberg: Well, the first one--there's three or four in its history. The first one was--I don't understand the first one. You had these professors that came down from Berkeley to get to semitropical water, and that's why they're named biological [unclear]. I resuscitated. They started with the Coronado boathouse, you see, then they moved up here because the bay was polluted, and worked here. The people in town saw the work. It's quite clear that the power structure then, a lot better than now, really realized that this was very important. They couldn't visualize climate or anything else, you know, and they put a lot behind it. You know the story of how they bought the property. You broke your arm if you bit on it. All seriously, the word went out. Nothing has changed much, I might add. But I'm saying that was the first step, and that's inexplicable. That's absolutely inexplicable.

Then the next thing, it sort of went in stages, you know. I don't understand another one. Even while I was here, the first physicist ever hired--his name is McEwen--I remember when we dedicated the [unclear] Scripps, he was there. Jeff Frauchy introduced me to him, long retired. I don't know when he was hired, thirteen, fifteen, or whatever. You know what he came on board for, why he was brought in? They felt that the oceans was the prime source of climate, and his job was to do something about it. His papers are still here. How could you accomplish much then? But, you see, this institution was always this way, but it just sort of lucked out.

Then the really big thing, it started with, as far as the modern-day goes, started with Sverdrup. No question. He brought a degree of serious science here. You see, oceanography then was kind of a mixture of yachtsmen, you know, rich-boy playthings, [unclear], this and that.

NO: [unclear]

Nierenberg: When you brought somebody like Sverdrup in, he not only was a good scientist--and the others were, too--but he had central vision. I had to cut it out of my talk, one of his many legacies, of course, was his book, *The Oceans*, you know, that he did with the other guys [Martin Johnson and Richard Fleming]. In his obituary, the one you read about, you may not have noticed--I always forget the chapter, I think it's chapter sixteen, is on ocean currents. You know, if you want to know about the ocean currents of the world as a whole, even to this day, fifty years later, the only place you'll find them described is there. But the big thing also I mentioned is his broad application of physics to hydrodynamics, the use of the curl as the main thing, and his generally good judgment in many, many areas, that, of course, I got a better appreciation of in the writing.

Then the post-war era. People here, like Eckart, you see, his book was very widely used, incidentally, but the level of his work, you see. And then Roger--you have to be careful--I don't want to sound negative. See, there's a strange thing about Roger. The good thing he did, the incredible thing, was the wonderful people he brought here in Scripps and outside, whether you're talking about Arrhenius. He brought tremendous vision, still does, you see. Or the ones I mentioned. I knew all the other people. I knew them all. Maria [Goeppert] Mayer--I'll tell you

a story about her afterwards, a woman's story. Maria Mayer. I knew them all. I didn't mention a lot that I did know. I didn't know so many of the chemists, but I knew some of them and so on.

Roger was just kind of the personality, it was almost irresistible. The only reason I suppose [Kenneth] Watson and I didn't come is we were both lazy. The idea of packing and traveling is probably what stopped Kenneth and me, you know, and so on. But that had a profound influence on Scripps. The one I was talking about before, you know, is an example.

There was another person that was--see, these are accidents, but these people are remarkable. I didn't mention Murrough P. O'Brien, Mike O'Brien. Mike O'Brien's spirit is behind--did Doug Inman mention him, incidentally?

RR: [unclear]

Nierenberg: I mean, his talk.

NO: [unclear]

Nierenberg: He should have. You see, there's a whole bunch of people like Isaacs and Doug. The whole business of in-shore oceanography all came out of Mike O'Brien's operation during the war. A whole lot of people. It's a very big effect. He was in touch, I'm sure, with Scripps in those years with--of course, when I knew Mike O'Brien originally, I didn't know he was connected with Scripps. I knew him in another way. That's still another story, you know. But I'm saying that Scripps was just plain lucky in that sense, but there was a certain vision all the

time. It's the darnedest thing, starting with people in this small community. It was a nothing community, you see, and they had that vision that this was somehow very important, and they would put whatever money they could scrub together.

The trouble with some of the histories is they don't give you the feeling of people and motives; they just give you dates and times. I don't know who got the idea that it was important to hook up with the University of California in a formal way, you see. That was a very important step. They tell the story about it, they say how the paper was in the hands of the treasurer who traveled around the world for eighteen months. You know the stories. But they don't tell you somebody had a very good idea there, you see, and you don't know who it is or what. So that was a very important step, you see, as an example. That connection with the university was a very important one.

NO: [unclear]

Nierenberg: No. I didn't have any success in my nominees, so I can't tell you.

NO: [unclear]

Nierenberg: And from a few of the nominees in recent years that they've made, I don't think much of them. They've made some very poor ones that are--what's the word--more politically correct nominees, but they're not very good science. Don't misunderstand, you know, but there's

at least two or three that I know that really wouldn't be recognized by anybody scientifically. I was shocked at that. So I haven't bothered to put in any more names.

I put in a very good mathematician, really impressed by a mathematician at Berkeley. A very funny story. [Laughter] You people are too young. He's part of JASON. He joined JASON about three years ago. He was introduced to me. I forget his first name. His name is Woodin. It's funny about one's memory. That took me back to Roosevelt's second administration or something. I said, "You wouldn't by any chance be related to [William H.] Woodin, who was the Secretary of Treasury?" He said he is. In fact, the family still has his chair. He was only for one year. He said I was only the second person he ever met that made that connection. I said I'm surprised. You know why I made the connection? Why would I remember that? Because there was a big joke at the time, Roosevelt wouldn't nominate him because everybody would say, "He isn't worth a wooden nickel." [Laughter] And that's the only reason I remembered it, and I couldn't understand why nobody else could remember it.

I nominated him, you know, and it turned out, I looked up the records, he had been nominated by somebody before me. He's one of the world's foremost logicians, something I don't understand at all, logic, like the kind of crazy thing that Girtle [phonetic] did, sort of thing, in that area, which I find hopeless.

NO: [unclear]

Nierenberg: Well, I'll tell you one. I don't know if that's why my--

[End of recording]