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

Transcript of the Videotape-Recorded Interview with  
EDWARD FRIEMAN

Conducted at  
Scripps Institution of Oceanography  
The University of California San Diego  
La Jolla, California  
February 17, 2000

Interviewers: Naomi Oreskes and Ron Rainger

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interviewers

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

**Naomi Oreskes:** Today is February 17, 2000. Dr. Frieman, I'd like to start by asking a little bit about your background and your early career. You received your undergraduate degree in engineering, and then you served in the Navy, and one of your early experiences in the Navy was working at Bikini.

**Edward Frieman:** Right.

**NO:** So can you tell us a little bit about that?

**ER:** Well, actually, I was at Columbia as a Navy trainee in the Navy program, and at the end of that time, all of us, I think, as I mentioned, whose names ran from E to H, were assigned to become deep-sea divers, although at the time I didn't know that I had been assigned to become a deep-sea diver. I was ordered to report to Pier--I've forgotten what the pier in New York City was. It was the pier next to the one where the *Normandy* burned and sank during the war.

I arrived and was told that that was my training school, so I had to dive down into this burned-out hulk and learn how to become a diver, in the Hudson River, which was filthy and cold. We had one of these big helmets which cut into your shoulders, so my shoulders were bleeding, and on top of that, the suit leaked, so as I was down in the water, I could feel the water coming up, up, up, up, up. And I was utterly panic-stricken, and I remain that way to this day.

So anyhow, after that, we went to Hawaii for further training. I was assigned to a ship which was called an ARS, auxiliary repair and salvage, and off we went. Got a little bit more training in Hawaii and then ended up at Bikini, and saw all three of the tests, which, I think, to this day, still give me nightmares. The one test, underwater test, where the lagoon was full of Japanese ships that had been captured and others that the Navy no longer wanted, and seeing something that appeared to me the size of the Empire State Building, standing on end with tons of roiling mud. So it was quite a sight, and I never--I guess I never forgot it.

After the tests--there were supposed to be three. I think there were two. They called the third one off. I never knew, of course, for many years that the people here at Scripps [Institution of Oceanography] were also involved, although I did meet Admiral Solberg, who played some major role in that expedition, but I was a lowly ensign and he, you know, seemed like God at that time to me. As I said, it wasn't until many years later that I realized the involvement of Scripps and the oceanographic community.

I guess at the end of that time, I was suddenly told by the captain of the ship that I could leave the Navy. I think it was, as I remember it, late in August, and I thought I could get back into graduate school. I had about three days. I flew across the Pacific and I got on planes and I hitched my way across the country and I went running to all the graduate schools in New York City. I said, "Please let me in." And Columbia said, "Well, you were here as a student but you'll have to wait a year." And I said, "I can't wait a year." Polytechnic Institute said, "Come on in," and so that's where I went to school.

**NO:** So when you were in the Navy then, how did you think about what you were going to do after your time in the Navy? Did you always have in mind that you'd be going back to graduate school in physics?

**Frieman:** Yes. I always--from the time I was eight, I knew I wanted to be a scientist.

**NO:** So when you saw the bomb test, though, I mean, did you make a connection in your own mind between the physics that you might be doing in graduate school and the atomic weaponry?

**Frieman:** None whatsoever.

**NO:** They seemed like different things?

**Frieman:** That was a different world that had nothing to do with anything at that time that made sense to me in terms of research and so on. I remember at that time of the--I guess the first notice we knew publicly of what happened, and then reading the Smythe Report shortly after that, when it was released, and understanding the physics of what was behind it, but the connection only came to me later. I remember having, at Princeton, many discussions with Harry Smythe about the report and so on.

In any case, I went to graduate school at a time when quantum electrodynamics was the thing to do, and I did a thesis on quantum electrodynamics on radiative corrections to the Kline-Nishima formula, and was just about ready to turn it in when I was told that a student of [Richard P.] Feynman's had just published that. So that was the start.

The head of the department then was a man by the name of Paul Ewald, who was, I think, Hans Bethe's father-in-law. So I met Hans through Ewald. Hans told Paul Ewald that this had occurred, and there was apparently some discussion in the department, but Ewald was new to the scene, was trying to make something of this post-war world, and decided that I had to do something else.

So I searched around for a different topic and ended up working with Lloyd Mote of Columbia on proton reactions in the sun. It was the first paper ever published, and it was even right.

**NO:** [Laughter] That's a first. The last few interviews people have told us all the things they've published that were wrong.

**Frieman:** So after that time, I guess the Korean War started. I'd never left the Navy. I had to stay in the naval reserve, so I kept on getting promoted, even though I didn't do anything. Once in a while they'd send me a paper, "You've been promoted." So I think by the time the war--at that time, I don't know, I was a commander or lieutenant commander, something, and they said, "Well, you've got to come to the Navy and go on active duty and go to Korea."

I said, "Well, I have a Ph.D. in physics. Maybe I could be of more help to the country in that mode." I talked to somebody at that time, a few people, and one person who worked for the Atomic Energy Commission in New York, I finally got to him and he said, "Yes, you're right. We should do that." So he picked up two phones and he said, "Do you want to go to Princeton or Brookhaven?" [Laughter]

**NO:** And you said Princeton.

**Frieman:** So I said--well, I thought about it for a microsecond. I said, "Princeton." So they put me on a train to go to Princeton, and I was interviewed by Johnny Wheeler. Johnny said, "I can't tell you what you're going to work on, but it's very interesting."

So I said, "Okay, I'll come work with you." I had heard of Johnny Wheeler, who was a great man in physics, and I thought this was a reasonable thing to do.

I was living in New York and I commuted to Princeton every day. My wife then was going to Sarah Lawrence. And I sat there for, I think, eight or nine months while I was getting cleared. I had not the slightest idea of what I was going to work on, although Johnny Wheeler came around one day, he said, "Think of two elements. One is called gravium and the other one is

called levium." So I thought of gravium and levium. And he said, "Think about the problem of how you could calculate the surface tension between gravium and levium." So I spent eight months working on that gravium and levium problem, and I was finally cleared and found out what I was supposed to do, and I was sort of taken aback.

**NO:** Which was what?

**Frieman:** Design of hydrogen weapons.

**NO:** Okay, just wanted to make sure. So how did you do it? [Interruption for microphone adjustment.]

**Frieman:** That situation at Bikini was sort of hair-raising. I can go back to that in a moment. I was on this ship that was five miles away from the blast, and just after it was over, we were ordered back into the lagoon. We had a long bamboo pole in the bow of the ship. At the end of the pole was a Geiger counter, so you listened to the clicks, and if they got too loud, you went somewhere else.

**NO:** This is a very high-tech solution to the problem of radiation safety. [Laughter]

**Frieman:** It's really high-tech measurements, but I actually--as I described to you, these ships were part of the blast, and one of them, and as we came into the lagoon, some of the ships were still sinking as a result, and so I saved the Secretary of the Navy's life because he was on a little, like a whaler, through, this was [Secretary of the Navy James] Forrestal, and his whaler--the seaman, coxswain, or whoever, didn't realize that the major battleship that he was going under was going to come down and slice him in half. And I was absolutely petrified. I saw this

happening, and I didn't know what to say, so I said, "Hey, SecNav! Get the hell out of there!"  
[Laughter] Then I thought, "Oh, my god, I'm going to get shot." But the coxswain sort of steered off and this--I don't know remember what it was, Japanese battleship, came down.

**Ronald Rainger:** You should have been promoted immediately.

**NO:** Did Forrestal ever know who it was who saved his life?

**Frieman:** I don't think so. I tried to disappear into the woodwork at that time.

**NO:** You never met him later on, did you?

**Frieman:** Not really. You know, we talked, and then, of course, it came to such a tragic end. The center, of course, where I worked, was the Forrestal Center.

**NO:** Let me just ask you one more question about Bikini, because you mentioned the Geiger counter at the end of the pole. Were you monitored for radiation exposure at all?

**Frieman:** Very little.

**NO:** Did you worry about that?

**Frieman:** Oh, yes. I said, during the talk I gave last week, that I thought I was going to turn green at night for the rest of my life, but, nevertheless, nobody understood what I was saying.

**NO:** Okay, so let's go back to Princeton. You were talking about when Wheeler tells you now, you get your clearance and he tells what you're going to work on.

**Frieman:** I was suddenly injected into a very different world. The buildings we were in no longer exist. It was a rabbit hutch from the old Rockefeller Medical Research Foundation, and within a short period of time, Robert Oppenheimer was there, and [Enrico] Fermi and [Edward] Teller and Bethe and [John] von Neumann. It was suddenly a very, very different world. We got involved in the Robert Oppenheimer issues because long before it hit the newspapers, we were told his clearance was revoked, and we all protested internally.

**NO:** Protested to whom?

**Frieman:** Up the chain, to the Atomic Energy Commission. Didn't do us much good, but--

**NO:** Did [unclear]?

**Frieman:** Probably not. I mean, we all felt strongly that somehow there was an injustice being done. I can't say at the time that we were sophisticated enough to see all the ins and outs.

The issue I worked on was r\_\_\_\_\_ instabilities in a weapon, and nonlinear r\_\_\_\_\_ instabilities, so I got to work with Fermi at Los Alamos during the summer. It was an astounding experience. So we lived through those times, we lived through developing numerical codes for these weapons, and went to Los Alamos by train with Johnny Wheeler, who, I guess, didn't believe in airplanes. So it was a very intense time. It was exciting in one sense and disturbing in another.

**NO:** Disturbing because of what you were working on, or disturbing because of the political--



**Frieman:** The whole political climate that we didn't, of course, know at the time, what the struggle was really about, although we knew that--we had been given briefings about what was going on in the Soviet Union at the time. It was a difficult, very difficult time.

**NO:** Can I just take you back for a minute, when you said you first found out what the project was [unclear] nuclear weapons. How did you react to that? Were you concerned about working on that? Was that really a shock? I mean, you had no idea that that's what the project was?

**Frieman:** It's clear, since I had to wait for a clearance, that I was going to be involved in something involving the military, and, of course, we were in a war at that time, so all of that fit together. I did not know that the U.S. was basically trying to develop a new kind of weapon, although going back to the gravium and levium story from Johnny Wheeler, it became clear that something of that sort must have been happening. But I decided I wasn't going to try to penetrate the thing; I would just wait.

So it came as a surprise, and since we were in the middle of a, as I say, a very tense and difficult situation, one's concern for your country is there. Your concerns for humanity are a different issue. So we worked and hoped that nobody would ever use this, but got involved in many of the concerns that the people earlier on had had. We knew that Robert Oppenheimer's position, we knew--

**NO:** So you knew that his clearance problems were partly to do with his opposition to the hydrogen bomb?

**Frieman:** Well, before that, we knew that he was opposed to that development.

**NO:** Did you talk about that?

**Frieman:** We talked amongst ourselves, yes, certainly. I should say, the opposite camp, if you will, was Johnny Wheeler, Eugene Wigner, and Edward Teller.

**NO:** I was going to ask you about that. So you had, in a sense, both sets of people right there with you.

**Frieman:** Yes. And it was sort of startling for us sort of youngsters, who were just barely out of graduate school, mixing with these folks who were sort of controlling our destiny, and seeing the disparate nature of what went on. The fascinating part of it, of course, was listening to them discuss physics, because they were just incredible. I guess, looking back on it, the Wheeler-Wigner-Teller-von Neumann axis, I mean, they were so right wing, so far over, it's hard to credit it.

But we heard, of course, about the debates at the use of the bomb on Japan, and the fact that there were discussions about not using it, but having demonstrations, and the fact that Edward Teller was one of the leaders in the group that wanted the demonstrations, it was hard to make that fit.

Nevertheless, that project came to an end for obvious reasons, and at that time, Lyman Spitzer, who was directing the other part of the project, asked me to stay. Edward Teller said, "Come to Livermore." This one said, "Come to Los Alamos." You know, on and on and on. But I decided that it would be a better long-range use of my talents if I got out of the weapons business, which I did, and stayed at Princeton for quite a while, worked very hard on controlled fusion, once believed that it might be accomplished in my lifetime.

**NO:** And that was controlled fusion for power?

**Frieman:** For peaceful purposes.

**NO:** For peaceful purposes, yes. And that was the project that this lab was mostly working on at that time?

**Frieman:** After the Wheeler project was over, he was gone, and all of the weapons stuff was left.

**NO:** Wheeler went to Livermore?

**Frieman:** Wheeler is at Princeton. He's a professor.

**NO:** Oh, but you said after he was gone.

**Frieman:** After the project, after Wheeler's project was gone, then Johnny stayed. You know, all of these people stayed plugged in as advisors and so on and so forth. Every once in a while, I would see them over the years and they would talk about nuclear weapons in a way that I think the rest of us found terrifying.

**NO:** That's interesting. One of the things I read in your biographical file, they mentioned that you also had some encounters with Albert Einstein at Princeton. Is that true or is that exaggerated?

**Frieman:** No, it's true. This was after the weapons era was over. He lived on Mercer Street, and he would come over to Palmer Physics Laboratory every once in a while. People felt they had to protect him, because wherever he went, reporters would follow him or little kids would follow him and ask him to do their homework for them. [Laughter] He was a lovely man. Every once in

a while we'd go to meetings at the UJA, and he would turn up. Come in and take his hat off, and his hair would stand up.

**NO:** Did people think that he was just anomalous or were people influenced by [unclear]?

**Frieman:** We knew that he had encouraged President [Franklin D.] Roosevelt to develop this weapon, and then after the war he clearly then changed his mind about it, and we all understood that. He was a great humanist and had a very warm feeling about the world, in a sense, and its peoples.

My interactions with him were not that great. I would go to these seminars that he would give. In particular, once a year he would answer questions that came from the graduate students in the physics department, over at the Palmer Laboratory, and that was sort of a "by invitation only" seminar. The graduate students usually wrote out the questions for him and they gave it to him and then he would respond.

The one I remember was that he came shuffling in, in his t-shirt or sweater, etc., etc., and the graduate students in physics at that point could not understand his concerns about the foundations of quantum theory that the familiar quotation, "The Lord God does not throw dice," so the questions always, many of them were focused on that issue, and it was always marvelous for us to say, "How is he going to deal with that issue?"

Anyhow, the last time, I remember he looked at the question and he smiled. He stood there for a minute and he said, "Well, I'm looking at you and now I'm going to look out the window and I'm going to look up at the moon, and there's a little mouse on the moon. I don't believe that mouse is going to jump into a new quantum state." [Laughter] So all the graduate students laughed, but that was the answer he gave.

At the end of that--I was sitting in the second row--after the questioning was over, he came and sat right in front of me and Val Bargmann was next to him. Val was a professor at Princeton

at that time and was a former student, and he said to him in German, "This is my last examination," and he knew he was ill. He died a month later.

So, as I said, he lived down the street from us and we'd see each other and nod. I saw him once or twice at the Institute, but I never worked with him. The people that I knew, did. But that was the extent of it.

**NO:** So tell me a little bit more about the Plasma Physics Lab and when you were working on controlled fusion, and you said just a minute ago that was when people still thought that it could be done. So did you imagine--I mean, I know, in the 1950s people said things about fission power, that it would be too cheap to meter, those kinds of things. Did you imagine that controlled fusion would happen soon, I mean within a decade, or by the year 2000, or in your lifetime? How did you think about what you were doing?

**Frieman:** Well, early on, we were, of course, completely naive. We were developing a new kind of physics, the [unclear] state of matter, and so it was extremely exciting doing the physics. The fundamental physics was a challenge, and somehow we were all buoyed up by the fact that we're developing all of this new information and we can now somehow capture this and do something good for the world. We all convinced ourselves that fusion, because of its intrinsic nature, had many advantages over fission, which I won't go into now, and that the world would need this and that we could somehow do it.

As we got into it further and further, it became clearly more and more difficult and more complex. Some of the issues that you've raised in your writing, which I admire, reflect on those models and so on and so forth. We were terribly naive, we were young, we felt we could do it, and it has so far, in a sense, escaped us. In one sense, in a scientific sense, it's almost there. In a societal sense, it's not.

**NO:** Not even close.

**Frieman:** Well, I mean, who wants it? Who needs it? Do you need it to have a sustainable world? Well, probably by 2100 you will. So the time scales have changed, our understanding of the world around us has changed. In any case, I published many papers, I enjoyed my time there, many colleagues who I remember fondly. I've gone back on their advisory board. So I can't let it go, but, on the other hand, it became clear to me that at some point I had to move on.

[Interruption.]

**NO:** So you said you came to a point where you'd been doing that work long enough, and that's the point which you then moved to the government circles?

**Frieman:** Yes. Somebody called, I can't remember, for--it was during the [Jimmy] Carter administration, about a year and a half into the administration, and somebody had gotten to Jim Schlesinger and recommended that I be for the job, the Director of Energy Research. John Deutsch had had the job before me, and he moved over to become the Assistant Secretary for Energy technology, and I started the process of coming into the government, and had an interview with Jim Schlesinger and a few of the officials at DOE [Department of Energy].

Then about halfway into the process, Jimmy Carter fired Schlesinger. Remember, he went off to the mountains somewhere to invent another Ten Commandments or whatever. It's like the old story, you know, there were really fifteen and five of them broke off. Anyhow, so Jim Schlesinger was gone and they appointed a new Secretary, Charles Duncan, who had been the Deputy Secretary of Defense. So I said, "Well, wait a minute. Duncan--Charles didn't ask me to be at this job. Maybe we ought to have a talk."

So I flew to Washington and I met Charles, and said, "You didn't choose me, so if you want to toss me overboard, it's fine, no hard feelings, I'll go home."

And so, I don't know, he tiptoed around, talked to Deutsch, and this and that one. He said, "No, come on." It took almost a year to go through Senate confirmation and so on and so forth, and my babysitter was [Senator] Bill Bradley.

**NO:** Your babysitter?

**Frieman:** In the Senate, because I came from New Jersey. He was the senator. You need somebody to sort of say, "He's a good kid," and blah, blah, blah. So that's when I got to know Bill Bradley. Finally got to Washington, got into the office, and I think the second day I was there, I was talking to John Deutsch all through this, who was giving me lessons, and he said, "You need a tough SOB deputy so that you can be the nice guy and the deputy will kill everybody, so, you know, it's your own private mafia."

So I learned how Washington worked, and I had a great deputy, Doug Hewitt, who had come over from OMB [Office of Management and Budget], on and on and on. So we had a meeting the first day and I said, "Well, let's talk about future plans," and everybody's sitting around the table, I had my feet up, so we can discuss the plans. The door closed, and all of a sudden I see everybody running around and rushing around. I said, "What are you guys doing?"

They said, "Well, didn't you just say we should have these plans?"

I said, "No, no, no. That's not the way it works in a university. You have to discuss it for eight months." I mean, these guys were off doing everything. So I learned, you know, it's a different world.

But all of the DOE laboratories reported to me, except for the weapons labs. It was an exciting time on the one hand. It was a discouraging time on the other, because we were in the midst of the oil crisis and trying to figure out the role of energy. I mean, the Department of Energy has never made a barrel of oil or produced anything, which remains true to this day.

**NO:** So is that what--I mean, when you went to DOE, I'm just interested in like, what did you hope to accomplish when you took that position, or did you feel like if the government asks, then it's--

**Frieman:** No, what I had hoped to accomplish was, since it was clear that fusion was running into trouble, I had hoped to somehow restructure the fusion program and get it on a more rational basis, and I had hoped to set in motion various programs that made some sense, in terms of energy production, energy technology, and I had hoped to kill some programs that were absolutely dumb.

**NO:** Like synfuels?

**Frieman:** No, synfuels wasn't a research issue. If you were going to build a billion-dollar plant, you're not going to do the research the Wednesday before you build it. No, there was a solar-powered satellite, and I said, "This is ridiculous. Suppose the beam's a little bit off, and you fry New York." To kill programs was worth your life.

The other issue that I felt strongly about, John felt strongly about, was the enormous amount of money that the DOE laboratories soaked up versus the universities. So we tried to shove the budgets around so that more would go to universities. That's enough to get you strung up from a lamppost.

**NO:** So were you successful at doing that?

**Frieman:** One percent.

**NO:** One percent. So was that the biggest frustration of your job, finding out how difficult it was to make change?



**Frieman:** I wouldn't say it's the biggest frustration. [Laughter]

**NO:** What was the biggest frustration of government service?

**Frieman:** That's a good question. You would see things being done, decisions being made, which were purely political, and had, in some cases, very little driving it in the way of informed science debate. Probably that exists today, and it has gotten worse, clearly, because the--I mean, people like me no longer work for the government. Look around and ask. Very few.

**NO:** There aren't high-level Cabinet people or high-level representatives with strong scientific backgrounds.

**Frieman:** Very few.

**NO:** Why is that? What's changed?

**Frieman:** The world has changed. The governments don't really control all that much anymore. You know, it's the old story of John F. Kennedy. He has the wheel, but it's not connected to anything. And more and more that's happening. Look at R&D at the moment. The growth in R&D is in the private sector and it's pulling away from the government sector, so we're in a time when the influence of the government is becoming less. The influence of NGOs is increasing, the influence of ordinary people is increasing, the influence of multinationals is increasing. So probably there's a conservation law of influence, so if something goes down, the others come up. In any case, we're going far afield.

**NO:** So what was good about being in government? There must have been something that was satisfying.

**Frieman:** I learned an enormous amount about all sorts of new things. I went to China with Frank Press and we sat at Deng Xiaoping's feet for four hours. That's a very interesting experience, when he was lecturing to us about the whole flow of history from Jesus on, as he saw it. So there were many such occasions where my horizons were expanded. So from just a purely personal point of view, it was a rewarding experience, and I made many friends that I would not have made otherwise. President Carter still is in touch. He's a very interesting man.

**NO:** Did you feel that there were some areas where you did make a difference [unclear]?

**Frieman:** In the sweep of history--

**NO:** Well, not the sweep of history, [unclear].

**Frieman:** Well, I mean, you're historians.

**NO:** [unclear].

**Frieman:** I mean, you're an historian. I answer history questions, not all this other stuff.

**NO:** Okay. Well, let me rephrase that.

**Frieman:** I understand your question. Possibly, some small things. Maybe I've prevented it from getting worse.

**NO:** Do you feel like it's hard to [unclear]?

**Frieman:** I mean, some of the things I put in place are still there.

**NO:** Some of the programs. I was just thinking about the comparison between, say, your thesis advisors and the people you worked with at Princeton, I mean, people like Wheeler and Oppenheimer--especially, I think, Oppenheimer--were so optimistic at one point about what they thought scientists could do in government. Were they a role model for you at all, or did you feel like the world had changed already so much by the time you came to Washington?

**Frieman:** I think that's probably right. At the time I was there, science and technology were still regarded highly, but the political aspects of it were becoming much greater. Let me give you an example where we fought and fought and won, and then we lost, and that's ethanol.

**NO:** Bill Nierenberg was talking about that the other day, too.

**Frieman:** John Deutsch and I fought the ethanol battle. John went back to his office, I went--

[Begin Tape 1, Side 2]

**Frieman:** On the ethanol issue, during those times we looked at hundreds of energy production schemes, I mean anything from yo-yos on. Ethanol, by our calculation, was not an energy producer, but an energy user, when you took the total cycle into account. So therefore, from a scientific perspective, it made no sense. We were roundly attacked. I mean really, seriously

attacked, under Federal Advisory Committee rules. I mean, you name it. People going through my records. It was really slimy.

The only way that ethanol could have made any sense was by huge subsidies to farmers to grow the corn. So from a scientific, technological point of view, it didn't make any sense. Can you buy ethanol today, from the gas you put in your gas tank? Yes. So it's where science and politics met.

I guess one of the issues that became clear to me, that this problem was there, it was getting worse, and it's a problem that from standing up and sort of looking at it from a larger societal perspective we're facing today, I mean, nuclear power is in trouble. GMO is in trouble. Gene therapy is in trouble. We go through these waves where we think that, as scientists, we've produced something new and glorious and it hits, and it's all turned to mush, something.

So there's an ethic and a politic and a structure to these debates which I think we haven't learned as a society how to manage yet. I saw that then. It was the first time I really sort of ran into it sort of smack head-on. Trying to push fusion forward, I ran into the nuclear lobby, so it was one after the other, of these issues, and I suspect we will continue in that mode.

**NO:** Just a little more follow-up on this then. So based on your experience, do you think that there needs to be more science education? Do you think scientists should be more involved in these kinds of conversations? Do you think it's hopeless?

**Frieman:** No, I don't think it's hopeless. I think that considering the state of our society, scientists can no longer believe and operate as if they were in ivory towers. Society doesn't value that any longer. There was a time in Greece and Babylon or even to a few centuries ago where the production of new knowledge was an end in and of itself, which had value, and it's no longer valued that way. We, I think, in the university community and the science community, have not

really truly appreciated the fact that the world has changed so significantly and that our roles in this new world are different.

So I am not pessimistic about it. What I'm pessimistic about is some of my colleagues who refuse to accept that this new world is upon us, but I must say that the younger people, perhaps your generation, understand that a hell of a lot better than my older colleagues do, and respond to it and do things about it.

**NO:** Some historians, I'm thinking of [unclear] and some other people, would say that one of the reasons why people don't think of knowledge as being good in and of itself is because of the nuclear atomic age, and because of what knowledge led to with respect to weaponry. Do you buy that at all?

**Frieman:** No, I don't.

**NO:** So what do you think has happened to make people less respectful of scientific knowledge?

**Frieman:** Well, we flunked. [Laughter] I just told you five or eight or ten. I mean, why blame it all on nuclear power?

**NO:** Right. It's just one of many.

**Frieman:** It's just one of many.

**NO:** So then how does that make you think about future scientific knowledge? Would you say that we need to be more modest in our goals or more humble in our claims for what technology [unclear]?

**Frieman:** No, we have to try to put ourselves in a situation where we think through the issue as a total system. Human beings are involved, our planet is involved, our society is involved. So I can't just simply go do "X" and not take account of its impacts. I don't agree with those people who say that science has no truth, but it's a function of the particular political day and age. That's ridiculous. But I do think that there is a structure in which we live, and science is one piece of that structure, and so which has to take its place along with other considerations as we do these things. We'll learn, I hope.

**NO:** I hope so, too. Let's shift gears a little bit and talk about--so then in 1981, you left the federal government and you went to SAIC.

**Frieman:** Right.

**NO:** So tell us a bit about that. That was an interesting move in your life, because now you've been in academia, government, and now into private industry. First of all, what is SAIC? What kinds of things does SAIC do?

**Frieman:** Well, it's so big, so I guess I should say what it doesn't do. It doesn't make diapers.

**NO:** That's all? But that's the highest technology of all.

**Frieman:** It doesn't make anything, actually. It makes knowledge, systems integration, works in energy, the environment, information technology, defense. You know, in a number of these vertical areas. It's sort of a five-point-something billion dollars. Started from nothing in Bob Beyster's basement or something thirty years ago, and it's employee-owned. There's something,

the essence of something valuable about employee ownership, because people look at those individuals around here who have become enormously wealthy in the past few years, whereas the thousands of people who work for them are working for them. What has happened here is quite different. That is, there are a number of people who are working and reaping the rewards of that. Anyhow, that was one of the appealing things about it.

At the end of the administration, when--I should back up a little bit. When the Office of Energy Research was put together, Pete Panofsky and a few others were advisors to the administration, and they deliberately took my job, although it was executive-level job, presidential appointee, and didn't call it an assistant secretary. They called it "director." And the object was to make it look or feel or smell like the director of NSF [National Science Foundation]. In other words, to try to take that job out of the political arena. The notion was, since it was basic research, it should be apolitical.

So at the end of the administration, a number of colleagues, Panofsky and others, said, "Hang on and see whether we can do the transition," to see whether it would work or not. So I said, "Well, okay, I'll try it," and it didn't work, so I got fired. But it was interesting, because the Republican transition team was in the office next door to mine. This was after the election. The guy who was heading the transition team--he was a nice guy, a lawyer--came running in. He said, "Ed, what should we do?"

And I said, "What do you mean, what should we do? You're the transition team."

They said, "Well, we don't know what to do. Tell us."

So I said, "Well, okay, sit down." So I gave them a list of twenty-seven things, and they came rushing back next door, they're all talking to each other.

He comes back the next afternoon, he said, "Well, we did all those things. What should we do now?" It was ludicrous.

**NO:** So did you have to suggest your own successor as well?

**Frieman:** Yes, as a matter of fact. So the new Secretary came in, who was a dentist, of course. That means he's qualified to be the Secretary of Energy. He's going to pull teeth out of a barrel, I don't know. In any case, so I met him and he said, "Hold on, Ed." My wife was then teaching in Washington, so I was just as happy to sit there for a while. I don't know, a lot of these high-level Republican kitchen cabinet folks came around and said, "Ed, good God, stay."

So I stayed for a little while, but it became clear, and I was told later that they took it up to [Ronald] Reagan, and Reagan said, "No holdovers." This was just fine with me.

I had, I don't know, ten job offers at that point. Go back to Princeton, become a this, go to Maryland and become a that. And I decided my world had changed and I would find it difficult going backwards.

**NO:** You said that SAIC produces knowledge. Isn't that the word you used?

**Frieman:** Yes.

**NO:** So how does that differ from a research university, which probably thinks of itself as also having a role in producing knowledge?

**Frieman:** In a certain sense, it's a for-profit university, or at least it was. It's changed considerably, of course. You can't have a five-billion-dollar university. At least I haven't found a way to do it. If I did, I would make one. In any case, I was there for five years. I built programs in energy and environment, things having to do with the Navy, which I talked about a little.

One day I got a call, I think it was from Walter Munk, saying, "Ed, would you be interested in being the director of Scripps?" And I was a little startled. So I went home and talked to Joy. She said, "Well, let's think about it." I had, in Washington, gotten to know Bill Menard,



who was the director of USGS [U.S. Geological Survey] when I was at Energy, and so all of us R&D-niks used to get together and talk, Dick Atkins, Frank Press. Dick was then the director of NSF, and we shared R&D information. So as I said, I had gotten to know Bill Menard. I never knew him before, and he was a very sterling character. But he died of cancer and there was a memorial service in Sumner, here, and his students got up to speak. Amongst them, Marcia McNutt, and there was a little quartet playing, people who were here, and suddenly Joy and I looked at each other, and I said, "This is where I belong." It was a strange response, but somehow it was like coming home to the university and things I cared about. So that's what I did.

**NO:** I want to ask you about Scripps, but before we get to that, can I ask you a few more about cold war science and the Navy, since you mentioned that?

**Frieman:** Yes. [Interruption.]

**NO:** So before we get to Scripps, I just wanted to ask you a bit more about cold war science and some of the things you talked about last week. One of the things you mentioned last week in your talk, you spoke about how the cold war anxieties really shaped scientific priorities during the Reagan years, and you mentioned arctic research, was one example that you gave of that. I thought that was an interesting example, because that could be viewed in at least two ways. I mean, one way you could view it is that it's a bad thing, because anxiety about Soviet submarines is driving science and distorting scientific priorities, or you could view it as a good thing, that it leads to more funding for arctic research. So how do you think about the whole question of how cold war priorities are a driving mission to drive scientific research?

**Frieman:** Well, you have to separate DOD [Department of Defense] R&D from the rest of R&D, because, I mean, during the cold war, a lot has been written about the subject of somehow that the

cold war reflected back into research and development in the civilian agencies and shaped it, and that's just not true. I never saw it happen. There are mission agencies and there are non-mission agencies, and the mission agencies which have a mission will respond to what the needs of the mission are. NSF doesn't have a mission in that sense. NASA's mission is to do science. NOAA's [National Oceanic and Atmospheric Administration] mission is, you know, and on and on and on and on.

And so this sort of blanket indictment of the cold war as somehow shaping the whole science and technology endeavor of the country is just false. It never did. Other societal forces shaped it. If you remember, there was a time when NSF had research applied to national needs. The national needs were never defense needs, they were other societal endeavors, and that program collapsed.

So I think to try to answer your question, a mission agency which is involved in one sense or another with the Department of Defense, its programs will be shaped, were shaped. What I didn't emphasize, however, during the talk, because it's a more complex subject, and that is that from where I sat, looking at these issues and seeing ONR's [Office of Naval Research] response, there was a lag time.

**NO:** A lag time, you mean, for ONR to take on--

**Frieman:** Right. I mean, you start up with the National Security Council and then the Secretary of Defense and the Joint Chiefs [of Staff], and by the time it got down to the guys in ONR, that war was gone and they were on to the next one. Okay? So that's what I mean by a lag time.

**NO:** Well, let me ask you about ONR now, because this has been--I mean, I've got at least one ONR question. [Laughter] Also, this has been a theme with several people we've spoken to that, a common theme is that people have said that what was great about ONR was that they funded

basic research. And yet, you know, to an outsider, looking at a lot of what they were funding-- well, two things. First of all, ONR is obviously, as you say, an agency with a mission. It's the Navy and the Navy has certain needs and priorities.

**Frieman:** Yes, but the mission was changed as a function of time.

**NO:** Sure, sure, that's okay. But I'll ask you about that in a minute. Well, why don't I ask you now. So how did the mission change with time, then?

**Frieman:** Because after the end of the war, ONR was NSF.

**NO:** At the end of World War II.

**Frieman:** Right. So before NSF was created, ONR was the agency supporting basic research.

**NO:** Okay, so you see it as it became more mission-driven with time, after NSF was created?

**Frieman:** That's right. I mean, then you look at the process by which budgets are developed in OMB. The OMB examiners won't let ONR do that any longer. They say, "Well, wait a minute. That money belongs over there with that agency." And then DOE was created and NASA was created and NOAA was created, and so there's a diffusion of things that used to be ONR's responsibility, or at least it was understood that it was, and that changed as a function of time.

**NO:** Well, that makes sense. That makes plenty of sense to me, but, nevertheless, people who have spoken to me have said things that even in the fifties, even after this had been completed, that their perception is that ONR was funding basic research. But to an outsider listening to it, when I

hear about spectrum analysis or acoustics, I mean, it sounds awfully applied, to an outsider, and some of it even sounds like engineering.

**Frieman:** Well, okay. If I look at the thing from the point of view of where I started as a fundamental physicist, all of oceanography is applied science. Newton's laws have been written, and I'm not going to rediscover it, and that, you know, etc., the quantum mechanics. So, I can say, well, all of oceanography, well, here's the equations, guys. Go home and float your ships or do whatever you want to do.

**NO:** But it's not just having equations or not having equations. I mean, stratigraphy isn't necessarily a--it doesn't have equations, but it's not necessarily applied science.

**Frieman:** I understand, but I'm saying that earth sciences and the kinds of things we're talking about in complex systems now, in a certain sense, are applied. Or from the point of view of somebody in the physics department, he looks at the earth science department and says, "Oh, you guys are plumbers."

**NO:** Yes, that's applied physics, as opposed to applied in the sense of mission-driven.

**Frieman:** That's a different issue.

**NO:** Right. So how do you think about that, though? So when you hear the word "applied science," I mean, or "basic science," what does it mean to you to talk about basic science? Where would you draw the line between what constitutes basic science and what constitutes applied science?

**Frieman:** Well, I think what we've learned in the past few years is that there is no line. I mean, that's the lesson.

**NO:** Well, I talk to someone like Fred Spiess and he tells me about FLIP, and ONR is supporting that. I mean, would you consider that basic science?

**Frieman:** No.

**NO:** No, okay. I was curious. And what about all the acoustic work? I mean, lots of the people here who worked on acoustics will tell you that they're doing basic science.

**Frieman:** Well, if you--I mean, there's a certain amount of confusion about this. Some people say they do basic science when it's not directed. In other words, they dreamed up, said, "I would like to work on this," and so they get some money and go work on this. So is that basic science just because it's not directed? But that confusion often exists.

**NO:** That makes sense to me. And I guess the other question that comes about ONR is the question of secrecy and security clearance. And again, most of the people we've talked to will tell us that they did basic science and ONR paid for basic science, and yet they'll also tell us that they did have to have security clearance to do much of this work.

**Frieman:** Very few.

**NO:** Well, it depends when, of course, we're talking about.

**Frieman:** During the time I was director, there were maybe five or eight people.

**NO:** Yes, but I mean earlier, back in the fifties.

**Frieman:** Yes, well, back in the fifties, it was probably different. I don't know that time.

**NO:** Maybe I can just ask you more generally about the whole issue of secrecy, because, I mean, most people outside of the scientific community have been sort of--well, let me rephrase it. There's a kind of standard model of science that's been written about a lot, in which one of the essential features of science is a kind of openness, open exchange of ideas, conversations, conferences, peer-reviewed journals, and lots of sociologists of science have written about that as being really essential to the creation of robust knowledge, that something about openness and something about the processes are actually essential for creating reliable knowledge, of finding out something about the way the world really is.

**Frieman:** Fine. I have no disagreement with that.

**NO:** Okay. So then how do you science if you can't talk about it? I mean, lots of people have worked on a lot of things that they couldn't talk about.

**Frieman:** But that's applied science. There was nothing basic about that.

**NO:** So you think you can do applied science in secret, but you can't do basic science in secret?

**Frieman:** Well, is making a nuclear weapon applied science? It's not basic. You're doing it in secrecy.

**NO:** And yet when you were working on the nuclear weapons, presumably you had to bring it back to fundamental physics. No? No, you weren't. So you wouldn't say that was fundamental science? You would say that was applied science?

**Frieman:** I can barely think of a piece of classified research that's basic. You are taking knowledge that has been developed in the basic research arena and applying it in some way. Now, it may be clever, it may be whatever, but it's rarely anything that's classified.

**NO:** Okay, so it's not fundamental new knowledge?

**Frieman:** That's been my experience.

**RR:** I was going to ask about work on waves, work on currents, and those kinds of things, which, you know, clearly, in some sense, they are based in physics and based in meteorology, but isn't there new knowledge that's coming out from work on waves and on currents and on circulation--

**Frieman:** Sure.

**NO:** --that much of it is done under ONR auspices and a good deal of it was classified?

**Frieman:** Maybe it was years ago, but during all the years that I was involved in it, there's nothing classified about any of that.

**NO:** No, we were really thinking about the earlier period, so maybe we can just drop that.

**Frieman:** I can't speak to that.

**RR:** That's fine.

**Frieman:** My involvement with Navy issues goes back into the seventies, as I was saying, but it was all then concerned with trying to understand what was happening to us. But we used, as I tried to say in that talk, knowledge from the open-source research community, and I think what I was trying to do was give credit to that community for developing the kind of knowledge that could make the advances that we made.

**NO:** Let me just ask you one more thing about this security thing and then I'll drop it. One time I asked John Knauss this same question, about the fifties, about the time period when a lot of things were classified, about waves and currents, and John said to me, "Well, it wasn't a problem. We all had clearances."

**Frieman:** Fine. I mean, I just can't usefully comment on that.

**NO:** Okay. Well, let's talk about Scripps, then. So you became director in 1986, and you told us about how you met Bill Menard and stuff. But you were a physicist and you were actually one of three directors in a row who were physicists, so I'm curious how that worked, in terms of coming into an institution like this, an institution of oceanography, and in what way you think your perspectives were different, being a physicist, and how it affected [unclear].

**Frieman:** In many different ways. The physics community and the ocean community are extremely different. People here tend to think of themselves as--some think of themselves as arrogant. They don't know what arrogance is. [Laughter]



**NO:** You said it, I didn't. [Laughter]

**Frieman:** It's just different communities. The sociology is different, the way people interact is different, the way people work.

**NO:** Do you think people here are more collaborative or more friendly?

**Frieman:** No.

**NO:** Okay, so what are they? More what? Or less what?

**Frieman:** That's a good question. I find that, or at least I've found in physics, there was more discussion of ideas and really sort of struggling together to advance science, and I don't find that in the ocean community in the same sense.

**NO:** Why do think that is?

**Frieman:** Well, it may be peculiar to this institution, but there was a--people had the sense here that if--it's again, one of these conservation laws. You know, if somebody gets X, then I'm going to get 1-minus-X. There's a kind of mentality that--it's driven by many things.

I mean, one of the issues that I found very problematical is the criteria that are used for promotions. You look at papers and you ask how many of those are single-author papers. Well, if you're going to be promoted on the basis of single authorship, it says, "Don't form teams. Don't collaborate." I mean, that's a bad thing to do. And so the structure, the underlying structure

of the way this thing works and the way it's interpreted by various faculty committees legislate against some of these things happening.

**NO:** Where do you think physicists learn to work with people? I mean, do you think they learn from working on these projects?

**Frieman:** Well, look at the physical [unclear], where a paper is published with five hundred names on it.

**NO:** Right, but that wasn't always the case [unclear].

**Frieman:** It's been that way a hell of a long time. When you do particle physics, I mean, what are you going to do, throw the 499 other people overboard so you can publish a paper? I mean, the nature of the field demands collaboration. The nature of this field, in oceanography, demands collaboration also. The structure fights against that.

**NO:** That's interesting, because one of the things we talked about with Walter Munk was, he was saying that as you look back on the *Capricorn* expedition, that if you look at all the different bits of data that were there, in retrospect, you could put that together and have much of the evidence for plate tectonics, but that's actually [unclear] because one person was doing the sediments, somebody else was doing the magnetics, somebody else was doing gravity.

**Frieman:** I'll give you an example. You've heard Rom [phonetic] talk about Endoex. In order to make Endoex work, he had people from enormous numbers of countries, enormous numbers of institutions. I mean, everything was brought together in a team, and it worked superbly well. In

my mind, that is a model for how oceanography has to be done in the future, because that's where the science is leading us.

**NO:** So is that one of the things you tried to do as director, to make it more--

**Frieman:** Yes, I fought like hell to bring Rom here, etc., etc. and once he got here, I gave him piles of money. So I mean, that's the kind of thing a director can do.

**NO:** What about the tenure promotion decisions? Did you try to do things to persuade faculty to rethink the way they thought about--no, because you can't do it. [unclear].

**Frieman:** No, you see, that issue is a UC [University of California]-wide issue. There are a certain set of rules in the manuals and this is the way it's interpreted. I mean, I must say, coming here from Princeton, I was startled at the way things worked. Somebody showed me the book full of FTEs [full-time employees] 1.7, so I said, "I just own three-quarters of the person? Where's the other quarter?" You've got a tenth of an FTE. All I own is a thumb. [Laughter] You know, it was strange things, but that issue of the university regulations and how they are applied loomed large for me, and it does, as I say, create problems.

**NO:** One of the things that you're credited with while you were here is moving the institution in the direction of climate change and climate research. How did you decide that that was the most important thing or the next big thing that Scripps should be focused on?

**Frieman:** In point of fact, some of that started when I was in the government, and these issues of climate change and CO<sub>2</sub>, that actually was in energy research. So I began to understand that problem and saw it as--at that time, we knew much less about it, but it seemed to me that this was a

problem that had to get worse as time went on. And then I began to look around. I mean, all the time I was here, I was on one government committee and NRC [National Regulatory Commission]. I think I was half the time over Kansas or something. And it was clear to me that this was a field which was evolving and that aside from a few people like David Keeling, who was providing--

**NO:** [unclear].

**Frieman:** Yes, Dave. It's the same person. Sorry, I'm using his nickname. I mean, I can't tell you how many times I saved him. Even when I was in Washington, Bill Nierenberg would call me and say, "Ed, he's losing all his money. Help." And I finally told one of the Secretaries of Energy, when they were about to take all of Dave's money away, I said, "You're going to go up in Congress and show this curve and then it's going to stop, and you are going to have to tell the senators why it stopped." Bang! The money appeared. [Laughter]

**NO:** Well, that's good. [Laughter] So is that just because people didn't appreciate the significance of what he was doing?

**Frieman:** No. It's because NOAA wanted to take it over. I mean, you get into these battles. NOAA said, "Well, we have--the mission says we are responsible for monitoring blah, blah, blah, so stop it. He should retire or do something else." And of course, that issue is one that has come back to haunt us many, many times, is rather than the government being our friend, they're our enemy.

**NO:** So how did you respond to an agency that claims something that people here are doing as part of their mission? [unclear].

**Frieman:** You fight like hell. You go up the Hill. I hired Kathleen Ritzman, who is fantastic. I don't know how many times we walked through the halls of Congress on one issue after another. So the fact that this institution flourished in those times was largely due to just spending a hell of a lot of time just pouring money back into the place.

**NO:** So do you see that as one of the benefits, then, of your having been in government?

**Frieman:** Absolutely. Absolutely. See, I naively thought that when I left the government, a big gate would come down. Those guys are there and, you know. You find, actually, that you're part of the alumni club, and we all talk. Somebody who's there now will call and say, "Ed, remember the so-and-so and that problem? It's still here. What should I do?" And we trade.

**NO:** Let me ask you a little bit about, more about climate change, since you mentioned Bill Nierenberg, and Nierenberg is on record as being something of a skeptic about [unclear] global warming. But at Scripps now, climate change research is a large initiative and part of your work. So how do you respond to Bill's saying that it's all overblown and exaggerated?

**Frieman:** Look, I mean, Bill and I, long ago--let me say two things about Bill. Number one, when I became director, he moved out, he never bugged me, he never interfered, he behaved beautifully, and he went up the Hill when I needed him. If something happened that I didn't understand, that was left over from his time, he would help.

On two or three issues, he and I came out just poles apart. One of them was strategic defense, the other one was this. There are probably others. But on both of those, we just agreed to disagree, so he ignored me and I ignored him. He would go off and play with his right-wing guys, and I would go off and do something else.

But luckily, strategic defense didn't hit home here, necessarily, although the story I told of what happened, but there are lots of stories about that we can go into some other time. I got attacked by Edward Teller in that whole business, and that's all written up in the history books. And it was, in fact, David Packard who helped me with Reagan on that one.

But nevertheless, that was the way we did it. I just felt strongly that this was an issue for society and I believed that this institution could play a major role in dealing with it, so I encouraged that build-up. It wasn't easy because there were many here who, again, you know, in the conservation of money ideas, whatever, saw this as a diversion from their pet rocks. So that was a problem that I just struggled with.

**NO:** And now, in terms of climate change, do you see Scripps' role as being an important place for creating a factual knowledge about climate? How do you see Scripps' claims to policy questions or the policy going to be?

**Frieman:** That's a more difficult issue. Roger Revelle had tried for quite some time to convince me to set up a policy unit here at Scripps, because he felt that the policy implications of this kind of research were so important that we should do something about it, and it was clear that that was not something that I could do successfully.

Number one, I didn't think we would be able to attract first-class policy wonks, and then, secondly, it would take resources away from research. So I actually had discussions with people like Resources for the Future and others, to try to work with them. It never went anywhere, because in order to have those discussions, you need sort of two-way communication, and it just wasn't going to exist. The people here, by and large, are not in that policy wonkism mode, so there's no point in trying to make it happen.

So what I see for the future is the evolution of some of the things you've heard about, and that is, moving toward global observing systems. That is a very difficult path. It has an

extraordinary set of difficulties associated with it, but, nevertheless, I think it's the right thing to do. And somehow to take this to global scales, and that's where we're headed.

I think one can see in the President's budget, as of a few weeks ago, that this is beginning to move, and I think it's the beginning of something very different, which I think we all hope for. But what we're learning is that climate is not isolated, that the climate scales, the El Niño scales, the intermediate scales, they're all related through some background soup of some sort, and so there's a complexity and richness to this which, from a research perspective, is extremely interesting. But at the same time, the societal impacts are also extraordinary.

So we're at the incipient beginnings of something new, and I think all I was able to do was take some baby steps in that direction. I talked to some people who were close to the White House and I said, "You know, when you're back there talking to your friend Bill, why don't you stir this up a bit?" So this lady went off to Washington, stirred it up--

[Begin Tape 2, Side 1]

**NO:** ...climate change, and what was your public policy and scientific information and public policy? What do you think about scientists like Steve Schneider or Jim Hanson, who have been very public and vocal about climate change?

**Frieman:** Both of them are accomplished scientists, and I think somehow we need a new compact with society, as I tried to say earlier. So individuals speaking out, as they have, it sort of doesn't go anywhere. In Jim Hanson's case, it actually produced a counterrevolution, in a sense, which was unfortunate, because Jim is really, as I say, very accomplished.

We need somehow to find different ways to do this, and I believe that the only way that it's going to come about is real involvement, not simply going up and giving testimony and then walking away, saying, "Well, I've done my piece." That kind of societal involvement we haven't

seen from our community. It takes time, it takes effort, it takes care, it takes thought, and many of the people say, "Well, if I do that, I'm not going to make tenure, so how can I do that?"

So it comes back to an issue that I raised earlier, that somehow our university-based promotion and tenure system really fights against some of these things, and if we're going to make changes in society that I think all of us in this room believe are appropriate ones, we've got to work on that issue with our so-called "masters." It's as simple as that in ways, because if people were encouraged to do that, and if I really believed what the motto, whatever it is, of the University of California, talking about teaching, research, and public service, public service is a third now, so are you really going to be credited with doing that as part of your next step or not? So we are not living up to really what's expected of us in this university system. That may be a function of the fact that we're a state university. I think things are different at a private.

**NO:** Do you think there's any more public service at Princeton than there is here?

**Frieman:** Well, I don't think at Princeton that you are judged as harshly by your colleagues in such a restrictive way. I mean, when I was at Princeton, all of the tenure and so on decisions-- there was something called the "Committee of Four" which had seven people on it, but, you know, that was the way it was done. It's clearly changed since then, but there is a great deal more leeway and more of an ability to take chances. Look at Princeton hiring Peter Singer and look what's happened to that with Steve Forbes, a trustee. Peter Singer would never be hired here.

**NO:** That's probably true. Let me ask you a totally different question, getting back to the whole issue of funding and how all science, obviously, depends on money. Every scientist has had some kind of project turned down, or something they wanted to do that they couldn't do or couldn't get funding for. Is there anything that you can think of, something that you wanted to do that you weren't able to do, or couldn't find the funds for?



**Frieman:** There are lots of things here that I wanted to do that I couldn't get done. I would have liked to hire some people that I wanted to hire and I couldn't.

**NO:** Couldn't because you couldn't get faculty support?

**Frieman:** Well, I had a certain number of FTEs, and so I was stuck. So I could take somebody out and shoot them and then hire somebody else, that's not a very productive way to do things. I wanted to bring--I mean, Bill Nierenberg really started this, much to his credit, in bringing this institution into space and remote sensing. I served on things involving NASA and the Vice President's Space Advisory Council and so on and so forth. It was clear to me that there was sort of a major change afoot with satellite information and what it could do for oceanography, so I wanted to take this institution strongly into that arena, and I couldn't.

**NO:** For lack of funding?

**Frieman:** For lack of funding, for lack of people, you know, the whole thing. I mean, basically, when I, early on, talked to Craig Dorman at Woods Hole, it was sort of like restraint of trade. We agreed he was going to do this and I was going to do that. There was no point in our colliding on those issues, so they were very strong in ocean engineering, Bob Ballard and all the rest of it, and they could do classified research in that arena. So I let ocean engineering languish here.

I think somehow that spirit, if you will, of coordination, has grown stronger under Charlie [Kennel]. He's really working very hard with the directors of other marine institutions to make sure that you somehow coordinate and work together, and I think that's fine, because there isn't enough money to go around. There's no point in our fighting over those things and let some people grow in specific areas. Of course, now with modern high-speed communications, there's

no reason why anybody has to be here. I could run a virtual Scripps with one person and a computer.

**NO:** Don't let the faculty hear that. [Laughter]

**Frieman:** Wipe that off the tape. [Laughter]

**NO:** Okay, we'll take that one away. We won't quote you on that. This is kind of a related question. Your life is interesting because you've had a kind of bird's-eye view of science. You've been in academia, you've been in high levels of government, you've been in industry. What would you say is the most important or significant science that has been done in America in, say, the last thirty or forty years?

**Frieman:** It changes with time. The era of physics, which was sort of the golden years, are gone. We are now in a completely new world. If I had to choose two of the most exciting fields at the moment, one of them is clearly the revolution that's going on in the biological sciences, and the second is what's going on in astrophysics and high-energy physics. From the most basic point of view, those are two of the hottest fields where there are things, very exciting things, to be done as we go forward.

I think the arena that this institution can work in, which it will find itself in, is really very large-scale attacks on trying to understand our planet, and I think that's extremely important, but it's not basic. It's how do we interact with each other in our world. I think it's crucial to a sustainable world, which is something I care about.

**NO:** What about missed opportunities? If you take a historical perspective, are there things that you see as significant missed opportunities in American science or American oceanography in the last two decades?

**Frieman:** That's a hard one. So what did we miss? If we missed it one year, you picked it up the next. I don't really see what we've missed in terms of scientific opportunities. I think the issue that we've reflected upon here about how we deal with it in societal terms, we've missed.

**NO:** We haven't really kept pace.

**Frieman:** We haven't learned our lessons there.

**NO:** What should I have asked you that I haven't asked?

**Frieman:** You've asked quite a bit. You've been very good.

**NO:** Thank you. Well, you've been great, too. Anything you want to say before we quit?

**Frieman:** No.

**NO:** Thank you so much. It's been great talking to you.

[End of interview]