



ORAL HISTORY COLLECTION
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Archives

OCEANOGRAPHY PROJECT

WILLIAM AARON NIERENBERG

Project Coordinator: Robert A. Calvert
July 1976

WILLIAM AARON NIERENBERG

Following a B.S. in physics at City College of New York in 1939, William Nierenberg continued in the same field to receive a M.A. in 1942 from Columbia University. However, he first appeared in the history of science when he became a participant in the Manhattan Project (1942-1945). Here he worked with the diffusion membranes and cascades of the atomic bomb. He also developed a higher level of management for science.

In 1947, the Navy called him into service. Working as a nuclear physicist under Rear Admiral William Maxwell, Nierenberg pursued the study of the extent of radioactivity of the naval vessels and the means to decontaminate them.

However, 1950 was the turning point of his career. At this time, he decided to direct his efforts to some area other than nuclear physics. This turnabout eventually led to the establishment, with Eugene Booth, of the Hudson Laboratories at Dobbs Ferry, New York, of which Nierenberg later became director. In the laboratories, he researched acoustic anti-submarine warfare systems.

This oceanographer can boast also of numerous involvements with the United States government. Several of these attachments include: consultant for the President's Science Advisory Panel on Anti-Submarine Warfare (1958-1960), consultant for the State Department (1959 and 1968 to present), United States delegate to NATO (1960-1962), member of the President's Science Advisory Panel on Foreign Affairs (1962-1964), member of the U.S. National Commission for UNESCO (1963-1970), member of the President's Science Advisory Panel on Naval Warfare (1964-1966), member of the White House Task Force of Oceanography (1969 and 1970), and member of the National Science Board as organizer and first chairman of the National Advisory Committee for Oceans and Atmosphere (1971 to present).

When he took over the directorship of Scripps Institute of Oceanography in 1965, the institute was divided. It simply consisted of a collection of individual departments. Since that time, Nierenberg has strived to unite the branches into a single entity and, consequently, has been successful.

Nierenberg sees the role of the scientist as having a place in politics and is involved in the issue of ocean energy as a possible alternative to solve the energy crisis. To him the oceans are "important in commerce, in climate, in our understanding the earth and the atmosphere...."

ORAL HISTORY DEPARTMENT
TEXAS A&M UNIVERSITY

INTERVIEWEE: William Aaron Nierenberg
INTERVIEWER: Robert A. Calvert
DATE: Wednesday, July 7, 1976
TIME: 5:00 PM
PLACE: Scripps Institution of Oceanography

WN: You know, oceanography isn't a science. You know, it's a common field of endeavor. You're working in the oceans. Now, did you ever hear of landology?

RC: No.

WN: Okay, so it's the same thing. Landology isn't a science either. There's a difference, though, in that people live on land, most parts of the land, so you work in a very normal way on land without any special organization. But the oceans are so difficult and so hostile, so very large, and people don't live on it, that you have to have some kind of an organized effort. What you do is you use every science you can think of, every discipline you can think of, and you use every technology you can think of, including the social sciences and history, archeology, anything. And you put them together in a very highly interdisciplinary institution; and, if you're lucky, it works. Now, you know, interdisciplinary things rarely work; but at Scripps it worked, partly because, I suppose, we started small and grew with it for 75 years. So, I mean, what you have here is a collection of, for the most part, just chemists, biologists, physicists, engineers, mathematicians, and so on. I happened to have been a nuclear scientist for a good part of my life. Newton's Laws of Motion don't change, you know, between nuclear physics and the oceans.

RC: Now, the first time, or shall I say the first time that you become important to us in the history of science, you're involved in the Manhattan Project.

WN: That's right.

RC: Okay.

WN: I was in it from pretty much the beginning.

RC: Can you describe, as much as you can, the impetus and resulting organization actions in the formation of the Manhattan Project?

WN: Oh, God, it would take hours. It was very exciting. The big stuff, all the big-shot stuff, you will find in Harry Snites' book; but that doesn't really tell what happened. You see, I was teaching--just out of college, starting graduate school. Just speaking for myself, it's true of a lot of guys--just started to be a teaching assistant at Columbia^a And you knew the war was coming. But, you see, even while I was teaching... I was teaching a course called Science Survey which is taught to freshmen. It's not even physics, but a smattering of the things that hadn't yet gotten in the science courses. And I was describing to my students how that atom bomb would work. Well, you know, there weren't that many physicists in the world. I was very low on the totem pole. But I could talk to Fairling, you know, and other people, and I attended the colloquia. Of course, that talk soon stopped. Now that was in the fall of 1941, and I realized that one way or the other I was... in my case, that I wasn't going to be able to last out the academic year^{XA} and so I asked my chairman to let me resign so I could be a Navy pilot. Well, in the process of doing it, I got persuaded to join this new working group--it wasn't called the Manhattan Project--and then all of a sudden became very serious. I was maybe the tenth person hired, you know. We just grew. It just started from there, and just developed and developed and developed with great speed. And we were quite successful. We developed a whole sort of new organization, organizational methods in big science and little science and so on. And you know the end result⁺: we built the K-25 plan, the diffusion plan. I ended up as

a section leader, actually a leader of two sections.

RC: Right. And how did you direct research as a section leader? Did you have any coordination or....

WN: Well, not bad. I mean, I had a reasonably well-defined job. I was mostly to develop systems for testing membranes and their performance and some very clever ideas (some of them original). And we would develop these, and we would develop methods for measuring performance of membranes. And that was a very important thing to do. That was in one of the sections. I was also in another section where I did theoretical work on the design of cascades, for example.

RC: What was the general spirit of those working in that project? Was it enthusiastic? Did you feel that you were working under pressure?

WN: Well, no. There was a war on. It's like a lot of things. Most of us worked like hell, you know, from 8:30 to 6:30, six days a week. A few guys goofed off. Some guys worked very hard and didn't contribute very much; some guys didn't work so hard and contributed a lot--very variable. There were other companies, of course, working in parallel--Kellogg's Corporation and so on. Union Carbide later took over. So there were all kinds of people; but, you know, there was a great deal of hard work, effort, that went on for four or five years.

RC: Did you notice anybody afflicted, let's say, with moral hesitation in the working on the Manhattan?

WN: Not in my area. You see, our group was a little remote. A few of us were; I wasn't because I was in the theoretical group. In fact, it's a statement I can make if I ever get my notebook home (my papers are still impounded in Oak Ridge). But, you see, we were very compartmentalized; and the people working on the diffusion plans were producing material. Really, on the whole.... Well, first of all, many people didn't know what they were doing. It was really only a limited group of us that knew what we were doing.

And, of those, they didn't have any sense of production schedules or when things would happen or when the bomb might go off or anything else. So while you would talk about it in a vague way, you know, since it was more remote than the people working at Los Alamos involving testing, the question didn't come up too much except--it's funny that you asked--it came up with me. And I ^{be}ame deeply concerned, and I didn't know how to handle this subject. But I called a couple of my friends together because I knew something about the production figures, ^a And I began to get a sense of when enough material would be piled up to make an effective weapon, you see. So, when I felt this was impending, I wrote something in my notebook--this was stupid, but because that was our ^f habit for patent reasons: people wanted us to write any ideas that we got and have it witnessed--that I thought it would be a very big mistake to use the bomb on an inhabited area, that there should be a demonstration first. Well, maybe other people did it. Nobody paid any attention to me, however. I just wasn't working at the level. And the President of the United States didn't ask me.

RC: What did you do for the Navy in Washington in the summer of 1947?

WN: Oh, that was very simple. That was my first exposure to the Navy. I ~~g~~ got drafted. They had a real problem. They had the first explosions out at Bikini, you remember; and the damn ships were radioactive and they didn't know what to do with them. And they didn't have enough trained people and everything else. So there was a captain, who retired as rear admiral--his name was Maxwell, William Maxwell ~~X~~ ^H he was sort of put in charge, I guess, to produce ^f ships in the Office of Naval Research. I knew very little about those things then. And, in a real hurry to try to do something about these damn vessels, I mean, what do you do with them? We didn't know how to work, decontaminate and so on. So one of the things was, they set up laboratory at Hunter's Point, at that point. And then he suddenly realized he had no

technical help in Washington whatsoever to work with. He got a hold of Dean Peterman of Columbia University, and Dean Peterman persuaded me to spend the summer and help this man out as a nuclear physicist ~~X~~^A and I did just that. And, in fact, he needed help so desperately that he even was helpful.

RC: Now, but did you go to the Bikini atoll?

WN: No, no, no, the ships were already, you see, on their way back or back. And they had to have a place where they could study the extent of the radioactivity, the decontamination. You see what I'm driving at? In other words, they needed a laboratory where they could develop, simply, techniques for cleaning up the whole business. This captain said that he thinks he was chosen because he was the _____ on engineering duty only, he was involved in power, and he was the world's expert in scaling boilers. So they figured if he could scale boilers, he could probably clean the ships. But, in all seriousness, you know, the laboratory lasted-- what was that--20, 23 years. It's no longer in existence.

RC: Now, why the one year break from Berkeley and the join of Hudson Labs?

WN: Oh, well, that was really earlier, you know, in 1950 ~~X~~. See, I was always a professor in research of nuclear physics; but, you know, I had gone through a war. (I always felt I had to do something on the side to help the government, one way or the other.) Until 1950, most of my external help were nuclear affairs, something like this business with the Navy, you know. In 1950, I decided that was foolish. I spent all my professional life with nuclear physics--teaching it, researching it. I ought to work in a different field in my outside life. And so, about 1950, I got involved. I had friends in ONR and fellow physicists, and I got involved with ONR. And Columbia University was asked by the federal government to build a major laboratory--it was called Project Michael, the initial study; Professor

Raby was the head of it--because during the war, of course, the deep sound channel was discovered. We were beginning to try to capitalize on it by Bell Laboratories building a social system. I think it was felt that what was needed is a big civilian laboratory that could do basic research related to this long range, acoustic anti-submarine warfare. Well, Gene Boothe, who was at Columbia, took a leave of absence to help this lab and build it up. It was in Dobbs Ferry. And he drafted me--we were old friends--to come and spend a summer or two helping him and some other guys as well. And we built the laboratory. And then finally, when he left in 1953 or 1954, I became director but only stayed one year. But that also got me a lot of experience in underwater work of one kind or another, you see.

RC: Now then, was this experience in underwater work ^W that directs you towards Scripps Institution?

WN: In a way. It didn't direct me; I ^a ~~man~~, I had other offers in other things. But, you see, I kept doing my work at the university, but then I got to know people. I made a lot of friends in the field so whenever there was an underwater meeting, I would show up. I'd write a few little papers in the secret literature, so I was pretty well known, you know. And I had already made a reputation in my own field. And, after all, when they were looking for a new director of Scripps, you know, they were looking for a lot of things: they wanted somebody that was reasonably academic; I guess, somebody with a reasonable reputation in science, and whatever the other requirements are; but also somebody who obviously had some experience in the oceans as well, you know, and so on. I didn't uniquely fill the bill; but I did because it wasn't enough, they felt, I guess, to have somebody who was just an ocean worker. They wanted somebody that understood student problems, academic problems as well, too. It's a very big part of our business, teaching, you know, and so on. And, of course, I've done a lot of that. I've had 16 of my own students, or so, over the years.

RC: Well, I'm struck with how you become so intimately involved with the United States government over the years. Is there any explanation for that as a scientist?

WN: You know, I don't know myself. And I don't seem to control it either. And my involvement isn't.... You saw my other things--my membership in NACOA, my National Science Board, now the President Baker Rainbow Committee, my consulting for Peace Act, and a whole host of other things, you know, and so on. And I don't understand myself. I've only got one explanation: I also speak Turkish. I'm also a flight instructor. People ask me how do you do it all. I finally figured it out. I don't watch TV, and I don't go to movies. I have all that extra time that I can use for everything else.

RC: I want you to make some comments, if you would, on some of these things you've been involved in ~~X~~^T The President's Science Advisory Panel on Anti-Submarine Warfare, '58-'60.

WN: Oh, wait a ^U minute now. What? Oh, no, no, I'll tell you the precise title. That was the Panel of the President's Science Advisory Committee on Anti-Submarine WARfare. There were three different panels. You see ^{''''} let's see, when was Sputnik? That was '58-'60.

RC: Sputnik was '57.

WN: But then they made two other panels, and I was on those later. They did more or less the same thing. Well, it was exactly that-- just what it says. We had the White House yacht. Usually we did exhaustive studies on the state of readiness and R&D and related matters in anti-submarine warfare for our Navy. And they were very effective, and they were very good groups.

RC: Okay, so then the President's Science Advisory Panel on Foreign Affairs, '62 to '64?

WN: Now that one was a little bit--I have to be very honest about it--was more

or less ex officio. And the reason it was, was I had already been Assistant Secretary General of NATO for two years, and I knew all the people. So naturally when I came back, I was a natural for their foreign affairs panel in science, you see. Ditle Font was the head of that. But, you see, before that I spent two years as the Assistant Secretary General of NATO.

RC: Alright, now I had some questions that I wanted to ask. X.

WN: And that's very important, I mean. We have very heavy involvement in international science, you know--all sorts of treaties and arrangements in so many countries. And that's the sort of thing that gets reviewed, you see, and gone over.

RC: And the President's Science Advisory Panel on Naval Warfare. That's '64 to '66.

WN: Well, that was similar to the first one. It was a new one. That's right. The emphasis there was a little more general; it was naval, not just anti-submarine warfare. That's correct.

RC: Okay. So in each of these, you're called on to bring expertise in, one in warfare, and is the other one in the judging of international treaties and international cooperation?

WN: No, but usually judging the effectiveness of programs, suggesting new types of programs, helping the State Department, you see, review certain programs, you know, like, is the one with Japan effective? Could it be improved? The one with Russia.... You know, this sort of thing. That's the sort of thing you treated in committees like that. You try to put some expert feel into the value of these programs. Those are more tricky because, you know, they have to be meshed with non-scientific policy issues of the State Department, because it's a little tricky.

RC: Okay now, I'd like to ask you three questions on those as a scientist. First of all, do you seem to feel any conflict between your obligations as a

scientist and your intimate involvement with the military?

WN: No, ~~why~~^Y should I?

RC: Well, a present younger group of scientists seem to feel this sort of conflict. Do you?

WN: I don't see it. You give me the statistics[;] I can show you. We have ~~some~~^{some} statistics in this matter, and I don't think you'll find that it's a majority. You'll find some very vocal ones.

RC: Oh, yes. I'm trying to intimate that it's a majority. I was just wondering....

WN: Now there are narrow issues. They say that should we do classified research in Scripps, in this main campus of Scripps, where we gave up classified research years ago and we used to do tremendous amounts, you see.... And everybody agrees on that, but that's not the same thing as saying^{ing} you don't want to be involved with the military. And I don't see any majority of young scientists not wanting to be involved with the military or concerned about the state of readiness of military in this country.

RC: Okay.

WN: I'd like to see the statistics. I mean, I hear people saying this, but you ask them where their numbers are. And, in fact, a few cases where there are numbers, that doesn't show it.

RC: Very good. Okay. A second thing is, or second point I'd like to make is, do you see grants, in effect, from the military more mission-oriented than pure science-oriented?

WN: I'll come back to that. Let me go back to your other question, you know, about the military. It would be almost immoral otherwise. I don't care what these people say, unless you have a particular religion like a Quaker. I mean, even the United Nations' charter, the most universal charter we have, is almost.... The Article One--or ⁵ it Two or Three--is the right of a nation to defend itself. Now how the hell is it going to defend itself? It's

got to have something to do it with. And ^Who's going to do it--its citizens. I mean, unless the Congress of the United States or the Supreme Court outlaws the military forces, you see, the Department of Defense, are just as much of a department of government as any other.

RC: And you feel as if, then, science certainly has a vital obligation as scientist to play a role in that.

WN: Just as much as they do or they can do in Welfare or in the Department of Transportation or a lot of other areas.

RC: Okay now, this leads me to a question up in the future--I intend to ask later on. You become very intimately involved in a political campaign.

WN: Which one?

RC: With the Humphrey-Johnson campaign.

WN: I sure did. Yes, I was, I was. I almost regretted, in an oddball way, for several reasons. One of them is because it's the first time in my life I ever got involved in politics. I never was in very much deep, and it didn't quite turn out the way a lot of people felt it would. I wasn't so surprised. And it's now hard to keep out, you see. I'm not that really keen on being involved politically. Well, anyway, the reason I got involved, though, is very simple; because I got a call from, I guess it was, Jerry Reasoner, who was at MIT then and, you know, had been President Kennedy's Science Advisor and was deeply concerned. And he and several other people were organizing this because they were scared of Goldwater at the time. And so I got very deeply involved in it, and I guess I was ^{the} Northern California Co-chairman ~~was~~ my ~~xxx~~ exact title. It was a very interesting experience, I must say. I learned a lot.

RC: What I was really aimed at was, do you philosophically feel as if a scientist ⁺ outhy to be involved in these kinds of things: defense, politics....

WN: More than ever before. Defense we always were, but in politics I think more

than ever before. I don't think politics should be left^e to lawyers any-
more. If you look at the problems that are besetting us, you see, they're
judgement problems that involve science. You know, they're social problems
alright. But, I mean, all the problems in energy and everything else, you
see, requires somebody to exercise judgement based on some experience.
And it would be hard to beat a scientist's experience there. And for a
lawyer, all he gets is conflicting evidence, and I don't know how he's going
to rule.

RC: Okay.

WN: So I think more than ever a scientist should be involved in politics.

But not me!

RC: You're retiring then. Well, let's take energy, for example. You're on an
oil spill panel.

WN: That was the President's Oil Spill Panel, yes.

RC: In '69, right?

WN: That's right.

RC: Okay. And, if I'm not mistaken, you're intimately involved in the issue of
ocean energy, are you not?

WN: Yes, reasonably so. Yes, ocean energy, that's right.

RC: What do you feel as if a scientist's obligation is, in terms of promoting
energy from the oceans and oil spills?

WN: Well, it's a little complicated. Well, his obligation is to try to put the
true facts, as best he sees them, forward to the public, no matter where the
chips fall. It's difficult enough to make a judgement because, you know,
nothing is without risks. And you want to do it in some form so that the
public can decide where their benefit balance lies. Right now the technolo-
gy of drilling for offshore oil is really very good, if the companies can be
held to stand this. As an example, you don't really have to worry about an
oil spill like the Santa Barbara one again. They really held pretty well.

But there's other energy, and that's more complicated, like ocean thermal energy and wave energy, that could be very valuable. There the problem is how much should we invest in R&D, if at all, in an area like that. Congress has got very severe problems where ERDA is concerned, because there are so many directions they could put their money in.

RC: And do you see wave energy and ocean energy as a possible solution to the energy crises?

WN: Yes, as possibilities.

RC: As possibilities. Are you optimistic about it?

WN: No. But I'm not as optimistic about a lot of things.

RC: Okay.

WN: That doesn't necessarily mean it's bad.

RC: Well, may I ask you another question? One of the major interests that have evolved in the ocean in the last several years is tapping the ocean in terms of mariculture, in terms of....

WN: Well, that's a long story, you know. We wouldn't have time. The mariculture.... We are already getting an incredible amount of food out of the ocean. It's near the limit, and mariculture is in large amounts. I mean, if you want to grow oysters and lobsters and so on, that's another matter. You're talking about getting appreciable increase in world fishery, common fishery catches. Mariculture has a very long way to go, a very difficult road to go.

RC: And so, in terms of energy and the ocean as sort of the new frontier being tapped, now, you're not convinced about that either.

WN: No, I think the big thing the ocean is going to give us is more than we have today of things that don't extract her values. I think we will learn to use the oceans to predict short-range climate. I think we will be able to use the oceans to control devastating hurricanes and monsoons, and so on and so on. And I think we'll learn to save our beaches all over the world, and things like that. And those are very valuable, or very great value. Fisheries

are near the maximum. The trick is just to keep the conservation so we can keep getting fish in the quantity that we get right now.

RC: Alright, the White House Task Force of Oceanography in '69 and '70.

WN: That was a very special thing. That was not very successful, and I was very irritated. I was off in England on a three months leave trying to write a paper, and they threw together this under Dr. Wakelin. There were a lot of tasks force like this in all kinds of areas. I was on the oceans. We produced a paper, but it really wasn't very good. That was not one of our more successful operations.

RC: Okay, and you're still involved in the National Advisory Committee for Oceans and Atmosphere.

WN: I was its first chairman, and organized ^{er of} it.

RC: Yes. Exactly what are you doing with that?

WN: Well, our most important thing is to write an annual report to the Congress and the President, criticizing and describing the state of our affairs in both areas. And they generally take our advice. We've written four reports now, I guess, or five; I guess we're writing a sixth this coming year. And then we do other white papers in between and do a variety of things.

RC: Okay. What is "Project White Oak"?

WN: Oh, my god, X Oh, that's such a long time ago. I've almost forgotten. That was before, I guess, or during the time ~~we~~ ^{we} can't remember now. What date was it? Do you have the date?

RC: I don't have a date for "Project White Oak."

WN: I didn't do all that much on it. Jean Boothe, the guy that started Hudson Labs, and I were good friends. And "Project White Oak" was one that had to do, I think, with defense against torpedoes, as I remember.

RC: Okay. That's where it would fit in perfect in what we are doing. And what type of work did you do in ocean surveillance in 1971?

- WN: Well, I do that all the time so I don't.... It was a specific?
- RC: Yes. Really the specific question is, in terms of ocean surveillance and counteractivity against torpedoes and so forth, what do you think of the present state of U.S. defense forces?
- WN: Well, they're as good as they can be, I think, given the state of technology. That's a very difficult job; that's very complex. It's an uphill battle. It's one you just work on year after year. You do a little better, the enemy does a little better, you do a little better--you know, it's tough, it's tough.
- RC: Do you see the major role of oceanography in defense?
- WN: No.
- RC: Not at all. Okay.
- WN: It's a very important thing, and the law makes it very clear. I mean, but it's in commerce, in climate, in our understanding the earth and the atmosphere, in minerals from the bottom of the ocean, in the food and fisheries problems, all of these things, our beaches, our enjoyment of the oceans. It's a whole mixed bag of things. And the military is an important part, but it is probably, in effect, as a science and even as a technology, it is no longer the major...what it was. Fifteen years ago the Office of Naval Research was the principal supporter. That's no longer true.
- RC: Would you make some comments about UNESCO? Your activities in UNESCO?
- WN: I never had too many activities with respect to UNESCO myself. I was more in NATO.
- RC: Okay.
- WN: And we still have a very effective program in oceans, among other things, in the NATO Science Committee. We had an ocean science committee that was very effective for many years and had expeditions or joint vessels with many different nations. And that work still goes on. They're very effective.

In UNESCO, Warren Wooster, who used to be here, was more involved in that aspect. I never had much to do with UNESCO.

RC: Do you see the changing boundaries, the 200 mile limits, and these sorts of things affecting the work of oceanographers?

WN: Yes, it already has. It makes it more and more difficult to work in a 200 mile zone.

RC: And I assume that international cooperation that had developed before will not limit, then.

WN: I don't really know. There isn't much international cooperation in oceanography. I mean, in a real sense, there are organizations; but there are only a few states like England, France, Russia, United States, Germany, Canada, that do appreciable work in oceanography. Very few other countries do very much. Brazilians do a little, you know, and so on and so on. So it's hard to say that you cooperate on an equal basis. You certainly.... We have scientists of many countries on board our vessels at any time. We have a ship up the Amazon right now and we have ^{an} Brazilians on board, but they couldn't match an institution like this.

RC: For closing this off, I'd like to ask you a couple of questions, then. Were you always attracted to the oceans?

WN: Not exactly. But, you know, I grew up as a poor kid in New York City with very little guidance.

RC: Right.

WN: When I got out of college, I was considered quite brilliant, a very good physicist and mathematician. I did want to be a geophysicist. In fact, my thesis was going to be in geophysics in the physics department, and a man by the name of Professor Beo, who was a very good.... And, in fact, it was to be on anisotropic ^{an} earthquake propagation. That's really what I wanted to be. And the reason I didn't is the war came and I went in the Manhattan

Project ~~X~~ ^W When I got back to Columbia, Beo was gone because they no longer wanted to have a classical physicist. He went off to Brown. He had been working on wind flutter. So I just kept working in nuclear physics with Robby, and I got my degree and don't regret it. But I really wanted to be a geophysicist. Now that's pretty close. I just didn't know that there was a science called oceanography, but geophysics I did know something about. And I'd been exposed to it because Farning in my first year gave a course--it's not his big course, but he enjoys doing this--and he gave a course in geophysics, one that he had done in Italy and did it one more time just for fun. It was a fun course, and I enjoyed every bit of it. And I think that went a great deal to make up my mind that that's what I wanted to do. There was a little bit of oceanography in it, but oceanography as a whole thing to be taught was only done at Scripps. And I was so far away that I wouldn't have even heard about it.

RC: What I'd really like you to describe is what was Scripps Institution like when you took over here?

WN: Well, it's fair to say that I didn't come here with any preconceived notions of what it was like. So really, when I describe what it was like, it was mostly based on three or four or five years later when I thought back on what happened. Reason I came here.... It was probably, as I say, I was interested in a change of career, as I said last time anyway, and also I had great admiration for a certain number of people here like Walter Munk and John Isaacs, Russell Raitt, Victor Raquier, Fred Spiess--these are people I knew; I didn't know everybody. As was certain, I knew the biologists less. But, you know, an institution that has people like this is bound to be very, very good; and, in fact, essentially that reasoning was very correct. They have high personal standards and these were communicated to the people that were brought in. Well, because I think it would be only fair to say for the

record...no, I want to repeat to be very precise...I didn't understand all of this all at once. Like anything else, if you come to an institution that was about, at that time about 60 years old, it had been through a lot of history, a lot of personal disagreements, and through several wars and building a new university on top of it and soon. There are all kinds of problems and complications, and I didn't know them all at once. In fact, I didn't recognize many of them for quite a while, and it took a long time to sort them out. And I think it's even fair to say that 11 months later, right now, I'm still working on some of the inherited problems. Oh, I think a lot of them have been straightened out. But, well, the institution itself certainly was intimate in its field at the time, just like Woods Hole. But there were real problems, and to generalize them--the important problems...one was that it had been pretty badly demoralized by the building of a new campus on top of it. And I think part of the reason is that the people here, while very, very good, had, for the most part, led a very sheltered life in an academic sense. There really was only one person, the director, who had contact with the outside academic world, the University of California. It had been Roger Revelle for a number of years; of course, before that it had been Sverdrup and so on. Most of the other people in the institution, however, led very sheltered lives this way. So when a lot of people, and a lot of them very good, were brought from elsewhere to form a new physics department, chemistry department, and everything else, the turmoil was really pretty hard. The institution didn't know how to protect itself very well from the academic in-fighting. That was one feature. The second feature was, very frankly and very bad, that you never get the amount of money you want. And Roger Revelle was very anxious to build up this new campus. It went through several stages, you remember. His first idea was to build just a sort of graduate school of science and technology; then it moved down to an

upper division thing, then finally just a general campus, a classical general campus. In the process, he didn't always get the resources he wanted so he used the resources of the Scripps Institution, you see, which were diverted, in effect, to build this new campus. And these resources ^{'''}Many of them have never come back. And that was recognized. And so one of the things, I'll give you an example, that happened really was with the director paying very little attention to the institution, worrying about a new campus. Well, it got so bad people--I call them the barons--like Munk, Isaacs, and others, simply took the areas of the institution they were responsible for and just ran them very well more or less independent of anything else. What I'm saying is, when I got here, the institution, in effect, was largely a collection of individual baron and baroncies, you see. And so a job was.... I didn't do it deliberately; I meant simply to work with them and work with everybody and try to bring the institution back as a unit. And we did; I think we've succeeded very well. You see, nobody did anything in the institution. It was sort of silly--the institution had been giving Ph.D.'s since 1920 and before; it was a teaching institution, but hadn't done anything. And, when they formed a new campus, they sort of, without thinking, formed three departments: one of them not even associated with Scripps in principle, of earth science; one in biology; one in oceanography. The oceanography department was barely formed. It only barely carried by one vote in the Senate, which is weird when you look on it now. But there was no school, so one of the first things I did was to ask the chancellor to--my title was director when I came--~~to~~ give me the title of dean. The reason for asking that was, by having the title of dean, Scripps could be recognized as a school and as a unit of the academic senate. It had been nothing up to then, and it was sort of fading away as an organization. That helped a good deal; in fact, it was very effective. Then there were

other problems, for instance, leaving that aside. The organization.... We had to work on that for many years, and I think it's been solved and we're in fine shape this way. There were some other problems. One of them was--now is true of oceanography as a whole; I'm not critical of Scripps--there was a lot of talent here, particularly in the Marine Physical Laboratory, in dealing with modern science and equipment for modern science. There was some in IGBP, not much elsewhere. I think the way to describe oceanography in this country and anywhere is as backwards, extraordinarily backwards, in the use of scientific instrumentation technology in actual application. There are a number of reasons. I think the principal one of which is like the farmer who is land-poor. Most of the money and effort went in just barely staying alive and running the ships; and the balance was very, very poor. And it's still going on to some degree in the whole country, and I think Washington is at fault in the amount of money you spend on instrumentation and so on. Now, I saw this very acutely because I had just spent 15 years of my life in Berkeley, and I had a laboratory in the radiation laboratory, which is exactly the opposite. In modern physics, people use the absolutely best and latest in technology, whether it's computing technology or photo cells or whatever. And that simply wasn't true in oceanography. So I set as one of the goals almost immediately--and I think with some success, looking back ten years later--to try to improve the position of oceanography in general, at Scripps in particular, in the area of balanced technology and had a measure of success. But the principal thing I did initially was to upgrade the computing capability in the institution, and we developed what is now considered a very good and very powerful system conceived on computer technology. And it's gone extremely well. I think the measure of that effort--of course, it wasn't me alone in anything; I mean, other people recognized this--is the fact that the institution, including a number of students, has been almost constant in size and number of people in the last 12 years. But the output has gone up a factor of five or ten. And I think that's

based on simply increased productivity due to better technology that is being used all around. Even the number of miles we sail at sea each year hasn't increased particularly, but the productivity has gone up. That's because of ~~the~~ instrumentation that works, and better instrumentation, as an example. So that's been a very big effect in these years. The third important one, not due to anybody just times, although it would naturally happen at Scripps, is the extraordinary increase in the quality of the students that we're getting. That just about started when I got here. It had nothing to do with me, but it's probably related to my action of leaving physics at the same time, too. But you can see, just before I came or just about, I think, the year I came, the department brought Merle Hendershad here as an assistant professor from Harvard. Merle Hendershad is very brilliant, and I see that as the third wave of appointments in the institution sort of since World War II or just before World War II. Between Merle Hendershad's appointment and our most recent appointment, we've probably made about 15 or 18 assistant professorship appointments. One of them was clearly a failure. In fact, we never were serious about that appointment--just a question of doing it right. One of them was a heartbreak; the man simply didn't make tenure. I consider almost, not quite, almost the rest, the remaining 12 or 13, of those I consider ten, about among the most brilliant, young--of course, they're not so young now; the ten years go by--each scientist in this entire country. And I give _____ some of these; I'd be very happy, all of them; and I mention Merle Hendershad. There is, just to pick them at random, Wolfgang Burger, who had some of the most extraordinary letters of recommendation I've ever read on any young man. He is apparently thoroughly competent in every possible area in oceanography, particularly those aspects that deal with marine geology, but other things. Then, of course, there is Jeffrey Baker, who is a typical

example, I mean, with his, certainly with his work in dating. And Falkner's work in chemical, the bio-chemical area, has been unbelievably productive. I could just go down through the list of these people. Epel _____, his work's been very productive in biology as an example, extremely prolific. His work has been well received. Then, of course, we hope, since this will be made available. We have some young people who particularly work in _____ area in physical oceanography, who are extraordinarily brilliant like Rick Samon. So one of the big phenomena over the last ten years, at least where Scripps is concerned and other institutions, is the ejection of some extremely brilliant young people, and the effect is being felt right now.

RC: Do you think this has happened across the scientific community at large or just particularly Scripps?

WN: Well, I think it's happened at large. I mean, I think there's been a general improvement everywhere, but I think it's particularly because a place like this would naturally attract ~~some~~ some of the very best people first. But they've got others as an example, you know. Now, of course, one of the reasons here is.... You know, there's one very important difference between Scripps and many of the other institutions: Scripps is part of the University of California. One good thing about the University of California is that our merit and review system is a very tough, rigid one. And all of our appointments at the institution go through this system, even the ones that are not tenured; even the ones that are not faculty, you see, go through this same system. So the standards are pretty high. But that's really unrelated to the high quality of these very good, young people, the sort of dozen or so that I was talking about; and their effect and impact is already very great. And now there's a new group that just sort of come into our consciousness: ^{group of five} physical oceanographers, of whom one is Rick Samon, who strikes us as being very, very good and very excellent. And I'm looking for-

ward to tremendous things from this group of people who will, in a sense, you know, 10 or 15 years from now, be the power structure, of course, of the institution. So I think we'll even do greater than was done in the past. I just think the future looks terrific.

RC: Is there any particular future project or project you're working on now that you feel would give Scripps....

WN: Well, no, my own philosophy, you see.... You know, being with people as good as this, you don't second guess them. I mean, there's nothing I can tell these people about they can't read. I do my little bit, but they do their own and so on. But as director, I think, one of the contributions I try to make in this sense, in this area, is each year I try to help the institution start in what I consider an important new area. I mean, we had the deep-sea drilling project that was very early. We had the North Pacific project; that was one of them, and I could go through the list. I didn't do very much on it, but I certainly helped. I did least on the GEOSECS. Dan Craig and his colleagues were perfectly capable of developing by themselves, but they certainly had my support, but on a somewhat lower level. They didn't need much from me, but like guaranteeing the ship would be available and other things. But, as I say, I try to do sort of one major thing a year. The big thing last year, ^h the big effort, ~~was to~~ not so much a project in a normal sense, ~~it~~ was to upgrade by orders of magnitude-- we're still in the process of doing it--our seismic equipment on board ship, digitizing it. It's a three-year project, and it's under way now. And it meant cash flow, raising money, deciding on equipment, organizing people; and it's doing very well. I'm very pleased with it, and probably ^{it} will need very little more help from me. This year and last year also the big things to develop are climate capability in the institution. We have it, but, I mean, in the more formal sense. And so that's the big thing I'm working on

right now, as an example. Each year I try.... And usually when the thing gets under~~way~~, I sort of fade out and somebody is running it. I worry about the next thing. And I think that's probably an appropriate role for a director of an institution like this.

RC: As director of this institution, do you feel that your attachment to the University of California at San Diego is a good one?

WN: Well, I don't know about the University of California in San Diego, but I...to be very fair, but I think the attachment to the University of California is a good one. It's a good one in two or three respects. Certainly the most valuable part is that you have an independent scrutiny of your appointments and our various so-called budget duties. There, I think, is the most valuable connection--the standards, you see. On the financial aspect, in the management form, that fluctuates up and down. In one sense, we get a certain amount of cost support from the university. That's very helpful, you see, in riding out fluctuations; but, you see, when the cost support used to be half the support of the institution, say 20 years ago, it's only ten percent right now. And because these are problems maybe beyond us, ^{AB}but there are all kinds of problems related to the overhead question and the support from the university and the money it's gotten. And rather than getting better, it is actually getting worse. And I don't know how it is all going to end really.

RC: You're talking about overhead cost, contract overhead cost?

WN: No, the handling of the overhead and how it's used and the amount and so on, how it's fed back and involves the government and so on. It isn't in the sense a contentious question, in a strict sense of the word, where we're concerned, but decisions are made between the university and the government that are not always and very often adverse to Scripps and its operations for reasons that are very, not unfriendly reasons, but very

complex reasons that actually may end up that the university just not be able to afford the Scripps Institution of Oceanography.

RC: Now, in your future career, do you see your future role in science as director of Scripps Institution or do you feel as if you may move back into a....

WN: No, I see my role as director of Scripps Institution of Oceanography. Sure, I do. I mean, it's just a nice balance of what I like to do. I do some science; I help originate some science. I actually do a minimum amount of what is called administration. My administration is primarily scientific administration. I enjoy that very much, and it's just a lot of fun. I don't see myself moving back into a full-time science role, no. I don't even want to. I've done that. I've published a hundred odd papers, and I'm not looking forward to the next hundred.

RC: Okay.

WN: Even if I do some science, it will be like pulling teeth getting me to publish it. I guess that's it.