TEXAS A&M UNIVERSITY
ORAL HISTORY COLLECTION

INTERVIEWEE: Marston C. Sargent

INTERVIEWER: Robert A. Calvert

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PLACE: 2556 Grandview St., San Diego, California 92110

RC: Dr. Sargent, did you do work in marine biology at Harvard?

MS: No. I worked on photosynthesis of freshwater algae at Harvard.

RC: What took you to Harvard?

MS: I lived near it. I was born and raised within about a mile of Harvard. So it was only natural that like other graduates from the same high school, I turned my thought to Harvard and was admitted, graduated, had one year of graduate work, and then went to Cal Tech.

RC: Was there any particular direction that led you to oceanography?

MS: It was an accident. I got my Ph.D. in 1934 in the midst of the Depression, and Cal Tech kindly found a little money and employed me for three more years as a research assistant while I looked for a permanent job. And then through a friend I heard there was a vacancy at Scripps, and I said, "That's great!" (One hundred dollars a month was almost unimaginable). So I came down here, and I was accepted. And I discovered within a year or two that this was the branch of science I had been looking for and didn't know existed.

RC: With whom did you have contact at Scripps?

MS: The man who gave me this opportunity as his research assistant was Dr. Denis Fox. The following year he went on a year's sabbatical, and I took over guidance of his graduate student. Then the Director, Dr. H. U. Sverdrup, appointed me as an instructor for a couple of years. And then along came World War II.

RC: What sort of work did you do at Scripps?

MS: I started work with the marine micro-algae, the diatoms, dinoflagellates, and so on, working with photosynthesis and the production of organic matter by these organisms.

RC: Now I have you at Scripps through 1942. Did you go into the Navy in 1942?

MS: That's correct.

RC: And exactly what was your role in the Navy in oceanography?

MS: Well, the Navy didn't know what to assign an oceanographer to, when they gave me a commission in the Naval Reserve. They had offered me a commission; then having given it to me, they were in a quandary. However, Dr. Roger Revelle had already been on active duty in the Navy, not as an oceanographer initially, but as an instructor in radar, because at first the Navy hadn't known what to do with him either. But by the time I got my commission, he was wearing two hats in Washington, assigned to Naval Operations and also to the Bureau of Ships working in oceanography chiefly in connection with the use of underwater sound equipment. And when he heard that I'd received a commission and was batting around in uniform looking for a job where I might be useful, he got me transferred to Washington where I worked with him all through the war in the Sonar section of the Bureau of Ships. Our main problem at all times was this business of what facets, what characteristics, of the ocean affect the use of underwater sound for detection, identification, localization, and so on, of submarines or other craft. King Couper, a telephone engineer, had preceded us in the Bureau; and Gordon Lill, another engineer, arrived at about the same time I did. Lill had been in Norfolk, the headquarters of the Service Force, Atlantic Fleet, and put to work on underwater sound. He made inquiries and found out that the center of this work was in Washington. So he came up and talked to Roger Revelle, who got him transferred to the Bureau. Dr. Mary Sears of Woods Hole, with a commission in the WAVES, was ordered to the Hydrographic Office of the Navy at least partly through Roger's influence. She soon was head of an oceanographic section of that Office, which a few years later became the Oceanographic Office of the Navy.

RC: Had you known Roger Revelle prior to your entry in the Navy?

MS: Yes. The year I arrived at Scripps, he was in Norway; and so I didn't know him until the following year, 1938. But from then on, I knew him quite well.

RC: Did there seem to be a number of people here at Scripps who moved into the Navy and moved into such things as underwater research for sound and . . .?

MS: Well, besides Roger and John Lyman, who was called up before he'd gotten his degree, Dick Fleming went to work for the University of California Division of War Research; Gene LaFond went to work for the Navy Electronics Laboratory; Walter Munk, after a short time in the Army, was transferred and spent the rest of the war here with Harald Sverdrup, working on prediction of surf for amphibious operatons. Martin Johnson identified a sound that had been mystifying and disturbing sonarmen and electronics engineers, as the love song of snapping shrimp. He and Russel Raitt were in the group that identified the deep scattering layer as a biological phenomenon.

RC: The point of this is, do you think that the exchange of ideas created by the Navy in this period of, let's say, '42, when you went in, '42, '43, and '44 when oceanography seemed to blossom in the Navy, would you consider that sort of the launching pad, as they say in modern terms, for oceanography?

MS: Well, yes. Up until that time, oceanography had been a little byway of science; the only institutions were small and poorly supported. Scripps, Woods Hole, and the University of Washington were principal ones from the beginning. Woods Holes, under Columbus Iselin, was the most important. With the arrival of Harald Sverdrup, Scripps started to develop very rapidly. He'd come here from Norway, with a very good overall idea of what an oceanographic insitution should be, and he injected a lot of new ideas. Besides, he had a thorough formal training in physical science and in physical oceanography and immediately started classes. Scripps was not the first to have a ship of consequence; Woods Hole had had the Atlantis since 1931; under Tommy Thompson, Washington had built the Catalyst, a much smaller vessel, but still they managed to work in the Gulf of Alaska with it. That was chiefly because they were Vikings, not because the vessel was so luxurious. They just grinned and bored it.

So oceanography took off at this point. The first oceanographers commissioned were recruited partly because of their background of seamanship and only partly because they were scientists. Columbus Iselin was commissioned sometime in the '30's. And Roger got his commission before we were actually in the war--I think in 1940.

RC: And then seamen and scientists take the Navy into oceanography through the issue of underwater sound, the scattering layer and these sorts of things?

MS: That's correct; better understanding of the behavior of underwater sound was the principal contribution of oceanography. This involved knowledge of the shape and texture of the seabed; the vertical distribution of water density; the effects of surface waves; the organisms of the scattering layers; the large sea mammals who make a variety of noises and also reflect sound; and other matters. Oceanography and meteorology involved in prediction waves and surf; in estimating the movements of drifting objects; and understanding the heat exchange at the sea surface as involved in the formation of fog and related phenomena.

RC: When did you first hear mention in the Navy of creation of something like the Office of Naval Research to continue after the war?

MS: I think the first I heard of it was after the capitulation of Germany but before the capitulation of Japan, sometime in the summer of 1945.

RC: And what seemed to be the general consensus, that research in oceanography needed to be carried on jointly with the Navy?

MS: I want to emphasize that the Office of Naval Research was established to enable the Navy to support at universities or elsewhere basic reserach projects in any branch of science ranging from psychology to radio astronomy with no special emphasis on oceanography. The selection of projects to support from among those proposed was put in the hands of committees including representatives of the major branches of science with advice from specialists as needed. However, oceanography took an increasing share of ONR funds partly because other agencies began to support other kinds of research. It is also true, as you suggest, that the ocean is important to the Navy in more different ways than are of concern to any

other large scale organization.

Also I think that the results of oceanographic reserach came as an unexpected gift to the Navy. Before the War the Navy had made little use of accumulated knowledge about the oceans. For example, there were essentially no charts of oceanic conditions published or used by the Navy. The only ones I can remember were the Pilot Charts which showed (among other things) the average surface currents. This phenomenon had been called to the attention of mariners by Benjamin Franklin, and much of the data had been collected by Navy Lt. Matthew F. Maury before 1859, so it is perhaps not surprising that the Navy had at least caught up with this body of knowledge in 1941.

In December of that year, the Navy was confronted with new kinds of warfare, especially submarine warfare. Who knew what was below the surface of the sea? The oceanographers. For more than half a century they had been recording conditions--temperature, salinity, density, illumination, organisms--abundantly in the upper several hundred meters of the sea and occasionally at great depths, deeper than submarines operated during the whole of World War II. You might say that the oceanographers had been accumulating this intellectual money in the bank and that in about 1940, they opened a joint account on which the Navy could begin writing large checks.

In addition, no great investment of manpower was needed to make use of this knowledge. Oceanographers were poor, and had no large industrial counterparts. Their instruments were simple and cheap, and had been extensively tested in service. A handful of scientists, including some brilliant physicists and astronomers, were sufficient to get out the first editions of instructions on the practical application of oceanography to the problems of submarine and anitsubmarine warfare. By contrast, development and production of (for example) radar, effective tactical aircraft, and especially nuclear weapons, required huge investments.

While these facts were confirming a favorable view of oceanography among the top ranks of the Navy, many oceanographers, especially Roger Revelle, Columbus Iselin, and Maurice Ewing, were making an impression as individuals. Simultaneously, oceanographers became conscious that the Navy could supply ships, vast experience in operations at sea, trained observers, and of course, money. Both parties could therefore anticipate advantages in cooperation. My personal knowledge of the people and steps involved in establishing the Office of Naval Research is slight. At that time I was at sea during OPERATION CROSSROADS, and after that re-entering civilian life.

RC: Now, you stayed in the Navy, until 1946.

MS: Well, after the capitulation of Japan, people, of course, began to be demobilized in hordes. And the Navy very shortly, within a couple of weeks, offered each of us oceanographers in uniform inducements to sign up for another six months to at least help roll up the program without just dropping it. And, potentially, some of us might stay in the Navy as officers specializing in oceanography, at least until the next selection board met. So Roger and I both accepted this offer. I don't remember about Gordon Lill at that time. We signed up for another six months.

And this involved us in OPERATION CROSSROADS, the first test of nuclear weapons at sea.

RC: What was your task at OPERATION CROSSROADS?

MS: Revelle was on the staff of the commander of OPERATION CROSSROADS and took part in overall planning. He also had special responsibility for oceanography and related matters, with groups under him on two ships, about 50 people in all. Our group of 20 to 25 (It changed from week to week as specialists arrived and left.) was under Cliff Barnes of the University of Washington and the Coast Guard. With him were Mel Traylor of the Field Museum and the Marine Corps, Jack Marr of the Bureau of Commercial Fisheries, and I. Cliff and I were the seniors and had general charge. We had, then, 15 or 20 other people, nearly all civilians and scientists, but including three professional deep sea fishermen from Monterey who were great additions to the party. We had several physical oceanographers measuring the water currents in the lagoon and surrounding ocean. We had biologists collecting and identifying the marine life and the rather scanty land life. We had the fishermen, seining, trolling, and long-lining in the ocean. We had geologists analyzing the sediments and charting the topography of the sea floor between and around the northern Marshall Islands, among which were the three atolls: Bikini, Rongelap and Eniwetok, where we were working.

We had our hands full keeping peace between the scientists, who were all individualists, not used to regulations and chains of command, and the ship's officers who were constrained to operate the ship only in accordance with orders from above. A major source of friction was the use of small boats to get to other ships (there were well over a hundred moored in the lagoon, in addition to the target ships which were unmanned) and to Bikini Island where the officers' and mens' clubs were, stocked with tankerloads of delicious cold beer. In fact, small boats were the only way to get to any of the islands around the lagoon. The people on the ship waiting for a boat were like people coming out of a theater and trying to flag a taxi. The professionals, that is the ship's officers, felt they had priority while the scientists felt they had been invited in order to accomplish certain tasks within a very short time, and should have priority. Of course some of the scientists were adaptable and cheerfully put up with working conditions, including boat service, and the definitely confining life. And some of the officers were positively helpful, even taking part in the scientific work to the extent that the captain would permit. One aspect of the problem was that during the hostilities just ended, the Navy had not assigned the best officers to survey ships. While we were on board, several times when a very good officer was ordered to the USS Bowditch, after a week or two he would get orders to a much better billet in a combat vessel. A few scientists found the conditions of work very unsatisfactory. Tactfully (we hoped) we suggested sympathetically that if they were unhappy we could get them on a plane for the States within two or three days. They usually demurred politely, but as soon as we saw them beginning to pack their gear we made them a plane reservation two or three days in advance. (There were many flights a day, so no prolonged wait.)

We also had some sterling characters. A burly young conchologist needed two or three days to work over one island. We would put him ashore with his gear for collecting and packing shells. (Remember that many tropical shellfish have extremely fancy shells.) He also collected bird skins as a hobby and carried a homemade small carbine with a bird shot, cutting and skinning tools, a frame for drying bird skins, and pickling alcohol. Ashore, he would collect all day, skinning his birds toward sunset, cook the eviscerated carcass (or carcasses) of selected specimens, drink the alcohol, and wake up fresh at dawn.

We had a prominent and very persuasive geologist. He noted that many of the ships in the task force were lying at mooring, day after day. These were transports that had brought equipment or men for the construction of test facilities, installation of instruments, and other accessories to the upcoming tests. They were waiting, fully crewed, to carry back the men and some of the equipment after the tests. So our ingratiating geologist, over a few beers at the Up-n-Atom officer's club, talked one of the captains into getting permission to make a few test runs outside the atoll to shake down his ship after some ostensible repair work. The captain and the crew were delighted with the idea of getting away from the mooring for awhile and willingly made some minor repairs every two weeks. When the ship cast off, who was happier than our geologist, ready to spend 24 hours or more conning the ship around the atoll or between atolls, making thousands of echo soundings. After six months of this he had the data for a detailed topographic map of the seabed covering hundreds of square miles.

We all saw the first atomic test from a safe distance of 25 miles. Roger Revelle had taken care that we had enough welder's goggles to go around, so we could watch the event while others watched the insides of their eyelids, buried in their arms. It was scary. Finally the six months was over. Planes got us to California in 48 hours compared with the 22 days it took to get to Bikini.

San Diego! Demobilization! Wife and daughter!

RC: Did anything in the test amaze you? Had you expected, for example, more damage to be done to marine life?

MS: We had very little to go by. The three previous bombs had all been used on dry land in New Mexico and the two cities in Japan. We knew there would be a big shock wave, and a series of big waves generated that would splash up on shore and wash away a good deal of it. We had no idea what would be the effect of all kinds of toxic materials, either radioactive or not, injected into the water through the explosion. We had only the vaguest ideas about how fast the lagoon was going to flush itself through the daily rise and fall of the tides and the unidirectional force of the trade wind nearly always blowing from the east in that locality. So we put quite a lot of work into the hydrography of the lagoon in an effort to measure the rate of flushing. It turned out that, even after a month, we would expect a considerable fraction of the material injected by the explosion still to be in the lagoon, because while the surface was continually flowing downwind and out the leeward passages, there was a back circulation underneath returning some of the material to the point of origin. Since Bikini lagoon wasn't designed neatly like a heat exchanger, the problem was complex and we didn't solve it in any general or accurate way, but we got some ideas about it by this. We thought of a model test. We thought of getting a whole transport full of, let's say, superphosphate--

a common enough fertilizer so that you can get a ship full in short order--bringing it out there, and then dumping it as fast as we could into the lagoon. We gave it up partly on the grounds of expense, time, effort, wasted phosphate. More important, we couldn't imagine how, without non-existent equipment, we could unload the ship during one tidal period. Meanwhile the lagoon would be flushing, freeing itself of contaminants at a sufficient rate to affect the accuracy of the experiment.

We did realize after the first couple of months that whatever sampling of the life we did would probably by inadequate. This is a problem biologists face in general: no matter how much we feel we know about the ecology of a locality, when someone comes up with a specific question, we frequently don't know near enough to give a specific answer. So this is the situation we found ourselves in; we should have started studies 25 years before. Then by 1946, we would have known at least how to start looking for answers to some of these questions.

RC: Now, after "CROSSROADS" then, you returned to Scripps in '46.

MS: Right.

RC: And were put back on the faculty at Scripps.

MS: Right.

RC: Had that always been your goal? I know you turned down a commission in the regular Navy. Was that what you had in mind, to do research at Scripps?

MS: Yes, it had always been my goal. When I accepted a commission, it was with a promise from the University that after the emergency I would be reinstated. This was common at that time. Everybody, including universities and corporations, was willing, if not always happy, to loan people to the armed forces for the duration. And the organizations gave assurances that those who served could come back to something equivalent to or better than what they left, even though they knew that the war would change some peoples' lives in ways that would make it difficult to return to former occupations. This was true even for people who went to work for the University of California Division of War Research; that is, they didn't serve in uniforms; they went to work as civilian employees, but they had the same assurances from their parent organizations.

RC: That they could return?

MS: Yes.

RC: When you returned to Scripps, didn't Revelle come in then, too, as director, or assistant director and then director?

MS: Not quite. Dr. Sverdrup remained as Director unti 1948. Roger was occupied until 1950 as first head of the Earth Sciences Division of the Office of Naval Research and then returned to Scripps.

When Dr. Sverdrup returned to Norway in 1948, Dr. Carl Eckart became Director until 1950, when Roger became Director.

RC: What did you do at Scripps between '46 and '51? Did you continue to work on kelp?

MS: Yes. And this was because I began to have doubts about my ability to do useful work with the microalgae; I kind of shot my bolt. All the things I had tried, the measurements I had made, led to grave uncertainties. Measurements of organic productivity and the fate of organic matter produced have been a major problem in oceanography for a good hundred years, and there've been some very good people struggling with it. I was making measurements that I had said, when I was first at Scripps, would yield quantitative data on the organic productivity of the microalgae; but when I began to list the assuptions I was making, I found too many possible sources of error to leave me with any dependable conclusions. Even today, 25 years later, I am not entirely satisfied with results reported from time to time by newer investigators. So I finally gave up and worked for awhile with the major algae. This is something that had started just before I went into the war, when a Chinese scientist, Dr. C.K. Tseng. turned up at Scripps. He had been doing post doctoral work at the University of Michigan. He was going back to his university, the Tsinghua National University, and got stopped by the war; he couldn't get back to China. So he turned up on the Pacific coast and visited Scripps to see if there was any possibility of work. Dr. Sverdrup found him a little money, enough to last about a year; by this time, I had my commission and went away. And so, with the common consent of all three of us, he moved into my laboratory, was paid from my salary and used my research funds during the whole war.

Tseng turned his attention to seaweeds, for several reasons. First, they are available; you don't need a ship to gather seaweeds. Second, in China they are an important part of the diet of many people in interior areas who would suffer from iodine deficiency without them. Third, some seaweeds are the only source of agar, a gelatinous material used in medical loboratories around the world. At the beginning of the war it was recognized that there might be a shortage of this material which the United States imported from Asia. There are also other marine algae widely used in countries with sea coasts as condiments or delicacies. C.K. Tseng felt that working in this field he could benefit this country immediately, and his own country when he returned. I think he succeeded to some extent in both goals.

When I got back, we worked together for several months until he completed arrangements for his return to China. I continued the work on seaweeds and published one fairly good paper. Eventually I was discouraged. I was one of the people whose life was changed by the war experience.

Quite unexpectedly, I was offered a job at the Naval Electronics Laboratory as Director of Training for the engineers, mathematicians, physicists and oceanographers at the laboratory; and as oceanographer on the staff of the chief scientist.

RC: And so you move over in '51 to the Naval Electronics Lab and serve there from '51 to '55.

MS: That's correct.

RC: And what was your primary job there as an oceanographer?

MS: As an oceanographer, I worked on several projects: underwater sound, atmospheric exchange, planning oceanographic surveys for use with tests of nuclear weapons.

In 1954 I was assigned to a joint Navy - Atomic Energy Commission task force being organized to conduct operation WIGWAM, a test with a deeply submerged nuclear weapon. At that time the Commission had their own separate requirements for clearing people to have access to AEC classified matter. Their investigation produced a short list (less than a page) of circumstances which the intelligence people represented as casting doubt on my honesty and loyalty. For a full year a good deal of my attention was diverted to answering interrogatories and defending myself before two security boards.

It was a melancholy and upsetting experience, but much less so than the experiences of tens of thousands of my contemporaries in those dark McCarthy years. In the end I received a letter stating that the Secretary of the Navy had approved the opinion of the security boards that "Your continued retention in the naval service is clearly consistent with the interests of national security" and that "The Chief of Naval Personnel has therefore directed that your case be closed without prejudice."

Meanwhile the task force conducted the operation, and disbanded. Only a week after the letter just mentioned was written, in October 1955, the former commanding officer at NEL, who had become the commanding officer of the Office of Naval Research Branch Office at Pasadena, offered me a job up there. This was the third time I had been offered a job with ONR, and turned the others down. But this time he was very persuasive. Since he had known me for three years, and knew all about the investigation, and so couldn't be accused of making a mistake, I happily accepted. And I worked for the Office of Naval Research until retiring in 1970.

RC: Now, what was your role in the Office of Naval Research?

MS: I was an oceanographer stationed at Scripps, a marvelous situation if there ever was one. My job was to keep track of oceanography in progress at Pacific Coast institutions. In later years, ONR had a man in San Francisco who covered the Northwest and Hawaii. But for a number of years, I was the only oceangrapher ONR had on this coast. They also had one at Woods Hole and later one at Columbia.

RC: When you say your job was to keep tabs on what was going on in oceanography, what does that mean?

MS: Remember that oceanographic operations are expensive and complex. Also, note that the Navy in many respects is split into two commands, an Atlantic command and a Pacific command. Well ONR doesn't have anything resembling a co-equal Pacific office. There is a branch office in Pasadena with about 60 employees, including one Resident Representative (a contract administrator) each, in Seattle, San Francisco, Berkeley, Stanford, Tucson and La Jolla. There are about 12 scientists in Pasadena, three or four in San Francisco and one in La Jolla, each having a specialty in one of the broad spectrum of research fields supported by ONR. In the '60's there were four major Navy laboratories

south of Santa Barbara and one in San Francisco whose work was concerned largely with the marine environment. A number of major corporations in southern California were engaged in development (and even a little research) related to equipment and craft for Navy use. Finally there were large Navy establishments all up and down the coast and in Hawaii supporting the ocean and airborne Navy fleets and forces.

Between these and the oceanographers supported by ONR at universities in Hawaii, Alaska, Washington, Oregon and California there was constant interchange of information, support, equipment, and even manpower.

The scientific liasion officer at Scripps was a center of communication between all of these and between any of them and ONR Washington. The typical questions were:

> Who knows about....? Who is doing....?
> Where can I get: 1) money 2)surplus Navy equipment
> 3) a berth on a Navy or institution ship and time and facilities to make (certain observations) in (certain areas) 4) advice on writing a contract proposal to ONR 5) an introduction 6) an invitation

The questions came by phone, mail, or personal visit. My duty was to be sure that the inquirer and I agreed on the essentials. Did he understand the limits or constraints on what anybody could do for him? Did I fully realize the value of his ideas and the implications in terms of work, time, expense? The answers were based on personal knowledge, or were obtained by canvassing likely sources, and in some cases boiled down to referring the inquirer to someone else who could inquire more thoroughly or make a decision.

The questions from Washington were more often "We have a letter from....", "We don't fully understand....", "Might be worth following up. Will you get a hold of him and get as much out of him as you can?". The questions about money usually but not always took the form of asking advice on writing a contract proposal. Usually my first reaction was to find out if he knew what he was talking about. In one case, very typical, he wanted a million dollars to build a revolutionary new submarine like the sketch he had on a piece of typewriter paper. It was soon clear he had no idea of the problem. My problem was to discourage him firmly but not unkindly. Another case was a very young man who wanted support for research on the effects of very high pressures (such as are found at the bottom of the sea) on enzymes. I couldn't find anyone to refer him to but I was bailed out by a colleague in the Pasadena office, a first class physical chemist who introduced him to the right man in ONR Washinton who in due time gave him a contract.

The prime requisite for my work was to know a great many oceanographers, what work they were doing, and how they were doing their work; what methods, what equipment, what requirements for work at sea. I did this by visiting widely, going to conferences, reading reports, seeing how the different kinds of work were done, especially at sea on short cruises for testing equipment or methods.

Another duty that fell on me was to give talks: formal instruction at Navy training centers; less formal orientation for visiting groups of mixed interests, such as Navy Reserve units; public relations talks to civic groups; and what was sheer fun, talks to busloads of young people

who somtimes produced very shart comments.

RC: What did you do in London in '58 - '60 in the ONR?

MS: Well, it was not unlike what I've been telling you, except that I was dealing with people in other countries.

RC: And so you were liaison, then, to the British Navy? Is that fair?

MS: I had no connection with the British Navy. The American Navy assigned officers to liaison duty with the British Navy. One reason was that the two navies were already operating practically as one and were closely coordinated in many activities, including research and development. I worked practically only with the civilian laboratories and universitites in the western European countries. I went there and talked with everybody I could. Mostly they talked English, or at least one person did and could translate for me. This last was true in Yugoslavia, for instance. In Germany, Scandinavia and Holland, most scientists talked English fluently; less true in France, Italy, Greece, and Turkey. I had very little difficulty. My little German and French were helpful, not only in their homeland but also in Yugoslavia and Turkey. The Office of Naval Research in London did quite a lot of business in facilitating the visits back and forth. If an American wanted to visit a European laboratory--an American who was employed by ONR either directly or as a contractor--it was customary for him to get in touch with us and ask us to pave the way a bit, because we were known individually to people in a lot of places, and our clerical staff could advise on routing and accomodations. On the other hand, there were European people who had been asked by ONR or some other government agency to come to the United States, who had difficulties with consuls and so on--the kind of difficulty that scientists at Bikini had; consuls weren't used to dealing with scientists in general. So we had quite a lot of owrk to do, facilitating these exchanges. We were occasionally asked to help some naval attache somewhere. I remember doing this in Denmark and Yugoslavia; they had some specific questions they wanted us to look into. I did have direct connection with a foreign navy in Turkey and France. There was a general in Turkey who had a dream that the Turkish navy needed a lot of oceanography done. The trouble was, as a general, he didn't really know what was going on in the Turkish Navy. The Turkish Hydrographic Office had, for several years, been sending officers to the University of Washington to study oceanography; and they now had four or five, maybe more, officers with excellent training, who had already charted a good deal of the hydrography of the Bosporus.

So I went to Ankara where this general talked to me. But he had the sense to get the Hydrographer of the Turkish navy there, and so, quietly, the Hydrographer and I arranged that I would visit their station right across the Bosporous from Istanbul. While I was in Istanbul, I visited the hydro-biological laboratory of the University and the fisheries laboratory. I formed a favorable opinion of what was going on in oceanography in Turkey. As far as the Navy went, I turned in a report that the Turkish Navy oceanographers seemed competent and industrious. Their biggest problem, which is common to countries lacking an extensive industrial infrastructure, was in obtaining and maintaining imported equipment, that was expensive, and in finding skilled maintenance and repair service. They

couldn't call up an electronic technician downtown and have him come out with his voltmeter and make tests; there wasn't one. That was the condition Turkey was in at that time. Otherwise, they were doing very well with good officers and good ships. One of the ships was Turkish built. A couple of them were American, small ships, mine layers or mine sweepers.

My other contact with a Navy was in France. Connie Limbaugh, the Scripps Institution's chief diver, vanished in an underwater cave on the Mediterranean coast. He was in the company of divers from the French navy and others working for Cousteau's commercial organization. They had all attended an international conference, and the local boys wanted to show Connie the wonders of this completely submerged cavern. Connie was well known as a pioneer who had established a school for SCUBA divers at Scripps. He had worked closely with U.S. Navy divers at the amphibious base in San Diego, and jointly with them, developed training curricula and safety standards that were widely adopted as models. The French navy promptly notified Washington and La Jolla. ONR, London, was immediately asked to send a representative to the site, and the finger pointed at me. Fortunately, Dr. W.H. Menard of the Scripps faculty, an experienced diver (one of Connie's alumni) was in England and we were able to talk with him. ONR made arrangements to fly us at once to Marseille. A French petty officer who spoke English fluently (he had spent two years at MIT) drove us to Toulon where an array of admirals expressed their shock and grief and offered every assistance in their power. We were then driven to the coastal site near Cassis. Eye witnesses gave us a full account of the incident (mostly in French) and described the round-the-clock search efforts. They also took us the the navy ship anchored as close as possible to the cave, and from there right up to the mouth of the cave, visible through the clear blue water. Cousteau's men were stringing lights up the main branch of the cavern, which was totally under water as far as it had ever been explored. The next day, Dr. Wheeler North of Scripps (another experienced diver) arrived with all his gear, sent by the San Diego organization of divers to take part in the continuing search.

All of us were fully conscious that after so many hours there was a very slim chance indeed of finding Connie alive. None of the many branches of the cave had ever been fully explored. The body was found several days later, with empty tanks, up a side branch.

Menard and I had talked with Cousteau and Revelle in Paris. North stopped in London on his way back. Our consensus was that there was no blame. An extensive limestone cavern, even above water, has branches where a man could disappear around a corner in an instant, and never find his way back.

A tentative contact with the Norwegian Navy fell through. But I was able to spend half a day at the Oslo museum, enchanted by the exhumed viking craft; and Nansen's ship FRAM which had successfully crossed the Arctic in 1893-1896, frozen in the drifting ice, as Nansen had planned.

RC: Did ONR fund projects in European universities?

MS: To a slight extent. Much the commonest arrangement was that ONR supported visits to the United States by experts in matters of special interest to the Navy. The visits might be for only a few days or might be for as long as several months.

RC: Between 1960 when you returned to the west coast and 1970 when you retired, did ONR support increase a great deal?

MS: Yes, indeed. During the early '60's, the Navy built their first ships designed for oceanographic work. They sought advice within and without the Navy from laboratories with experience in oceanographic work at sea. During design and construction, by agreement, the major laboratories assigned their own engineers to work beside those of the architects and shipyards. Each of the major laboratories received at least one of these ships with funds for operations and maintenanee. The National Science Foundation also began a similar program of design, construction and assignment of ships, and also provided funds for construction of shore facilities. In 1977, NSF has taken over nearly the whole function.

By the mid-sixties, oceanographic institutions were receiving funds by grant or contract from an array of Federal agencies including several offices of the Navy, the Army engineers, Air Force, National Aeronautical and Space Administration, Atomic Energy Commission, Geological Survey, and others.

RC: In this period didn't ONR seem to become more mission-oriented rather than, as previously, interested in pure research?

MS: Yes. These great expenditures for oceanography and the complex of channels by which they reached the institutions aroused a feeling among congressmen that they might be losing control of the distribution of a significant fraction of appropriated funds. To regain a closer control they inserted in appropriation acts a clause (the Mansfield Amendment) restricting each agency's expenditures for outside research to problems "relevant" to the mission of the agency. The agency least affected was the National Science Foundation because, stated inexactly but briefly, its mission was to support pure research. ONR had gradually to withdraw support in many fields which is had been encouraging since its establishment in 1946 when no other Federal support was available. Even within oceanography, support was diverted from some projects that were too "pure", in favor of continuing support for projects from which early development of practical applications was anticipated.

Within the Navy, the relevance of a good deal of oceanographic research was not easily apparent to many others in the department (hundreds of thousands of people, including civilians and those in uniform). The Navy is, in this respect, like large corporations. For example, ONR had for years supported studies of the movement of sediments in shallow water (to a depth of few hundred feet). The active processes moving sediments include transport to the sea and its estuaries due to currents, waves and tides. People who live close to salt water have, of course, known for millenia what movements occur at different seasons and during storms, in their own locality. The oceanographers were adding to this knowledge, an understanding of the general principles, applying the laws of physics to the turbulent, inhomogeneous, transient situation in the sea. By the early '60's, they had considerable useful ability to identify the principal forces at work in a locality or situation, and to predict the effects of changes in the seasons and weather, and of artificial changes, such as construction.

At this time the Navy needed in a hurry advice on the siting and improvement of harbors, selection of potential beach landing areas and related information for the coastal waters of Vietnam, for which there were only few charts, out of date, and inaccurate, and little historical record. They turned to the Army engineers; the Army engineers made contracts with corporations with wide experience in civil engineering applied to waterfront projects. Their engineers had no more information about Vietnam than the Navy, but some of them knew the oceanographers studying beaches and river mouths, whose major support was from ONR. So by hiring an ONR contractor's employee as consultant and sending him to Vietnam, they were able to get advice on the practical potentialities of several sites, and the probable effects good and bad of annual and tidal cycles and wind, waves and currents. These civil engineers and the consultant to a considerable extent spoke a common language, based on common experience, so knowledge of general principles governing the distribution and movements of coastal sediments got across, and could be applied on this unknown coast as well as anywhere. Up to this point large segments of the Navy had not seen the usefulness ("relevance") of these modest and inexpensive studies that ONR had been supporting.

Another factor in the Navy's declining support of oceanography was forgetfulness. As I mentioned before, at the beginning of World War II, the Navy had been presented with a large store or knowledge of the ocean that had accumulated over years. Twenty-five years later, many had forgotten that this was a unique event; they began to expect that the goose should lay a golden egg every day. Even as early as 1958, when I was about to leave for London, an officer in Naval Operations adjured me at length to keep a sharp watch during contacts in Europe for a discovery of any hitherto unknown force of nature which might be exploited for military purposes. I tried with no assurance of success to dissuade him from expecting too much.

As a matter of record it was not until 1973 when the oil-producing Arab nations began charging what the market would bear that a hitherto unrecognized force of nature - unbounded confidence of the industrial nations in an inexhaustible supply of cheap energy - was exploited by the Arabs in a new (and bloodless) form of warfare.

To get back from the long disgression on "relevance" and decreasing Navy support for "pure" research in oceanography, I must say that the oceanographers have not found themselves seriously hampered or in very straitened circumstances. To some extent they have had to forego luxuries. Perhaps the greatest source of unhappiness has been that candidates for appointment or promotion now have a somewhat smaller chance of acceptance.

However in the late '60's, I felt the definition of "relevance" was too narrow. Also I was dismayed with the inhumanity and futility of the military action in Vietnam. So I retired in 1970.

Before I had publicized my intention (although I had notified my superiors a year in advance) I was offered an opportunity to work with the fisheries people. This was unbelievable good fortune. The position was a part-time one as coordinator of the California Cooperative Oceanic Fisheries Investigations. I have known and respected many of the fisheries people for a long time. Part-time employment suited me exactly. I gladly accepted.

RC: What did you do with the fishereis people? Who are they?

MS: The California Cooperative Oceanic Fisheries Investigations, CALCOFI, established in 1947, was a joint effort of several public agencies, principally (using their present titles) the National Marine Fisheries Service, the California Department of Fish and Game, the California Academy of Sciences, the Hopkins Marine Station of Stanford University and the Scripps Institution. CALCOFI was a result of requests by several major cannery operators understandably alarmed by a catastrophic drop in the catch of sardines, centered in California, which for about twenty years had been one of the largest fisheries in the world. During those twenty years the average annual catch along the coast from southern California to British Columbia was close to 500,000 tons. Since 1951, in only a few years has it been as large as 50,000 tons. Since 1967, in spite of a moratorium imposed by the state of California, surveys have shown no indication that the population of sardines has increased to a level that could reliably support a profitable fishery.

The original request for help was made to Harald Sverdrup, the director of Scripps. He arranged meetings with Frances Clark, director of the state fisheries laboratory at Terminal Island, and Elton Sette, chief of the South Pacific Fishery Investigation of the U.S. Fish and Wildlife Service (South Pacific, to somebody at a desk in Washington, D.C. meant south of Seattle). These three laid out the plans for CALCOFI.

In the succeeding years, CALCOFI ships have travelled many thousand kilometers in a band of water about 300 kilometers wide extending from the Oregon-California border to Punta Eugenia half way down the length of Baja California. They have caught hundreds of thousands of fish and many millions, perhaps billions, of small animals (the zooplankton) and fish eggs. They have measured the temperature and salinity of the water and various chemical constituents repeatedly at several hundred stations at depths regularly to 1200 meters.

With this information the experts have plotted the seasonal changes in distribution of a number of kinds of fish (of present or potential commercial value, or because they are the food required by commercial fish).

One major result of this work is the identification of a population of anchovies much larger than that of sardines at any time during the last sixty years. These anchovies are closely related to those off Chile, Peru and Eucador, which have supported the large tuna population of those waters, and as fish meal for livestock feed, have been one of Peru's most valuable exports.

The samples and measurements have also yielded information on the relation of spawning to water temperature; on the relation of weather to water conditions and movements; on the distribution of plant nutrients and of food sources for fish (This last has caused John Isaacs to publish some very interesting general conclusions). Another interesting result has come from the study of sediment cores. Sediment in quiet deep ocean basins accumulates in thin layers which can often be counted like tree rings and can sometimes be dated by the occurence of unusual material at some level. For example, a layer rich in volcanic ash can be considered as following soon after a large eruption at a known time. Scales and bones of fish are common constituents and can be interpreted (with caution) as indicating the relative abundance of each of several species of fish in any one year or in different years. The core data strongly suggest that the abundance of sardines has been very variable in the last few hundred years with periods of plenty lasting a few years or decades, occurring between mostly longer periods of scarcity. The core data also suggest that anchovies have been much more abundant than sardines most of the time.

I have to admit that my efforts at coordination did not produce a clue to the reasons for the great decline of sardines. Within the last five years the great anchovy fishery of Peru has undergone a decline of similarly catastrophic magnitude and the reasons are equally unknown. I can't go into the

numerous, serious, complex and often subtle difficulties that interfere with studies of the dynamics of natural populations. Struggling with them was the duty of the scientists. The coordinator was bound to pay attention to other complexities in the operation of the fisheries and the conduct of the research.

I noted earlier that CALCOFI scientists had identified a large unexploited population of anchovies capable of supporting a major fishery. This discovery attracted enough attention to stimulate the California Fish and Game Commission to issue regulations governing the taking of anchovies for reduction, that is, for such uses as stock feed, fertilizer, or inedible oil.

The sardine processors for years had been required to pack a stipulated minimum proportion of the landings for human consumption while remaining free to dispose of the rest for other uses. This regulation reflected a compromise between two politically significant points of view. The fishermen and processors felt that if there was any market, they should be allowed to catch all the fish the market would absorb. Other people felt that, in a country or world where some people went hungry, every edible natural resource should be used only for human food until all the hungry were fed. In 1965 it was clear to the Fish and Game Commission that any large fishery would have to be, for political reasons, governed in accordance with such a compromise, and through legislation imposed the regulation on the embryonic anchovy fishery.

Other factors put a brake on any possible rush into the anchovy business. Markets had to be developed before fishermen and processors would invest heavily in new equipment. Prospectively, the important market would be for stock feed, but it was immediately apparent that two strong competitors had developed during the long sardine famine: anchovy meal from Peru, and domestic soy bean meal.

Initially, the only processors who ventured were those who still had equipment (now somewhat obsolete) left over from the great sardine years. This restricted the number of tons of fish that could be accepted each day. Meanwhile, canned anchovies were not received with enthusiasm by consumers, partly because not all canners took pains to make the product attractive, so the small market for anchovy as food restricted the permissible production of fish meal and oil.

There have been other impediments. As with any other new fishery, it is taking time for the fishermen to develop efficient methods. Different kinds of fish have different habits, capabilities of evasion, favorite localities, etc., that the fishermen must cope with. There has been long standing conflict between commercial and sport fishermen, partly realistic. Naturally, both groups fish in the closest waters that afford a satisfactory catch. If the commercial anchovy fishermen fish the near shore waters heavily, not only they, but also the game fish, who depend on anchovies, will have to move offshore, and the sport fishermen will have to follow. At least this is a possibility much feared by the sport fishermen. With help and stimulation from the large numbers of people who provide goods and services for sportsmen (gear, clothing, boats, motels, etc.) and the editors of sports magazines (who dearly love a good crusade) the sports fishermen form a lobby that legislators as well as commissioners have to consider.

Other phenomena that temporarily delay development come out of our fast increasing concern with protecting our environment in this overcrowded world, where some of us are reckless in our uses of energy and all of us are careless about discarding waste. Fish processing, as now conducted, is a great producer of objectionable waste in harbors, fish offal and escapes of petroleum products.

At the same time, processors of fish, like other manufacturers, are very sensitive about publication of comments, opinions, or even evidence affirming that any food fish are or may be contaminated. I have even heard a public blast that, "A scientist should not be allowed to make any statement damaging to a business." But that was in the early days (1970) when some businessmen were suddenly realizing that a scientist is not necessarily Santa Claus. But these problems, if not solved, will be reduced to tolerable dimensions, and we shall be worrying about others.

From the Coordinator's point of view it was not always easy to get the various CALCOFI agencies to work together at high efficiency. In my time, they had lost a good deal of the early sense of mission, the apparently clearly defined goal of the first years. It is no longer possible to promise that research will bring back the sardine or even predict when, for whatever reason, the population will increase.

Like most of us, the people who work in CALCOFI are over-organized, over-regulated, overburdened with paperwork. The several agencies work with different legislative bodies or boards of regents, different charters, different procedures. Goals for the National Marine Fisheries Service are set in Washington; for the Department of Fish and Game, in Sacramento; for the people in universities and the Academy, in their own individual minds; and these goals interpreted narrowly are sometimes incompatible. However, like most of the rest of us who wish to cooperate, the CALCOFI scientists found ways around, under, through these entangling nets and cooperated anyway. Frequently they got substantial help from administrative, legal and fiscal people in their own agencies who knew their way around in the paperwork. And I keep telling myself that the Coordinator was helpful.

In spite of these doleful remarks about organizations, I must speak highly of the action of the California legislature, in the beginning, in establishing a Marine Research Committee to advise and oversee CALCOFI. By law this committee must include respresentatives of the fish processors, fishermens' unions, sport fishermen, and the public at large. Over the years, this committee of nine must have had as members more than fifty very substantial citizens appointed by the governor to represent a wide range of interests and abilities.

Twice a year or oftener, they have well publicized meetings at one or another agency to hear and discuss reports from the scientists to decide on questions of policy and budget and to hear from anyone who wishes to be heard. These meetings are well attended and many suggestions and complaints are heard and reviewed.

From the point of view of someone inside CALCOFI, the greatest benefits are, first that the numerous scientists who attend receive an education in the points of view of business leaders, and of various citizens and groups. (They already have good contacts with fishermen). And on the other hand, a group of individuals in the various parts of the fishery industry get insight into the operations of the scientists. Over the years, these meetings have produced considerable understanding between the scientists, the businessmen and the fishermen, concerning their diverse views of their own and each others' problems.

CALCOFI has been an eminent scientific success. It has accumulated a vast body of data on the waters off California and Baja California, which has been of use to many people outside CALCOFI. Its methods, developed chiefly by people already mentioned (Sverdrup, Sette, Clark, Marr) and by Elbert Ahlstrom, Warren Wooster, and the late Hans Klein, have been widely adopted. It has resulted in