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People, Institutions and Discovery

Transcript of the Videotape-Recorded Interview with
DOUGLAS INMAN

Conducted at
Scripps Institution of Oceanography
The University of California San Diego
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Interviewer: Ronald Rainger

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DOUGLAS INMAN

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Ronald Rainger,
interviewer

Ronald Rainger: This interview is being conducted by Ron Rainger, with Dr. Douglas Inman, at the Scripps Institution of Oceanology, on February 16, the year 2000.

I'd like to ask you, just to begin with, if you could tell us how you originally got interested in science, even in your very early years, before you came to Scripps and went to college.

Douglas Inman: Well, I think I think I've been very fortunate that I've had outstanding teachers all my life, and it even begins in kindergarten through eighth, when my mother taught me half the time, because my father was in the Marine Corps and we were going two years overseas, two years in the States, and so all of the time overseas, she was our teacher, and she had been formerly a teacher, and so I had a good teacher there.

At that time, it wasn't science in particular that interested me, but as I went through high school, I had outstanding people there, and I went to Grossmont High School in the back country here. Carl Quicksall, the principal, was an outstanding person, and I'll never forget.

Then I went into college, and at that time, I had had no geology, but I took Baylor Brooks' first course in geology, and that's the first time I knew what I wanted to do. Having been in

Baylor Brooks' class, I knew I wanted to do something in earth science. At that time, I hadn't specified or been specific to oceanography, because it didn't exist as such. So I was blessed by very good people there.

Also, at that time I belonged to a group in geology at that time, and we had Roger Revelle give us a talk in 1940, and it was following his first expeditions to the Gulf of California, or second one, I believe. This was so fascinating. This was my first inkling that, hey, there was something out there that involves the ocean and coastlines and so forth, and I had this in mind when I came to Scripps.

At the time I got out of the service, I didn't know that there were classes that you could formally apply to at Scripps, and so I had a fellowship at Caltech in graduate school, and had gone up there. In fact, I was all checked out, had an office, an advisor, Buwalda, and was reasonably satisfied this is what I was going to do, when a friend of mine phoned up and said, "Did you know they're starting graduate courses in oceanography at Scripps?"

I said, "They are?" I came down the next day, I applied, I was admitted, and I had to go up and explain to them. They thought I was crazy giving up a Caltech education for this unknown Scripps, but it was certainly the wisest thing I ever did.

RR: When you referred to Brooks, what institution were you at as an undergraduate in oceanography?

Inman: I was at San Diego State University. It wasn't even called a university then; it was called a college. We had the most outstanding people you can imagine. It's quite different than some places are today. In fact, Baylor Brooks was a Rhodes scholar and a very learned person, and dynamic, probably the best teacher I've ever had.

RR: I know that you spoke last week somewhat about your experiences during World War II, before coming to Scripps, and I wonder if you could tell us a little bit about that, in terms of whether or not it had any impact on your interest in coastal studies, or if that really comes later. But if you could comment on your World War II activities.

Inman: Well, World War II, as I've related previously, was a difficult time in this country, but most of us that were involved, of course, were involved for the duration. I was in college at the time and was supposed to graduate with a bachelor's degree in physics and geology, in 1942. Well, Pearl Harbor changed all that, because it had been apparent to many of us that probably we were going to be involved in a war. In my case, I decided if one was to be involved, you should rather have some knowledge and choose your service. I had been associated with the Marine Corps reserve and we'd gone to platoon leaders' class, which was taught down here in San Diego, and I went there two summers.

So shortly after Pearl Harbor, it was very uncertain what would happen, but most people who had been arguing with us in college--and college is, if nothing else, a place where young

people argue the pro and con--whether we should or should not be in this war. Many of them were planning to be draft dodgers and so forth. The day after Pearl Harbor, they all enlisted. It was a very remarkable unifying experience. I, having been associated with the Marine Corps, knew I'd be called up, but since I hadn't been yet and they weren't prepared for us, why, I started my final semester at State, and was called up after the first month and went into the service.

But they did something that was very unique to that time. They called me in and gave me my final exams, and I assure you that I wasn't an outstanding performer in final exams for courses I hadn't taken, or at least had only just started. Nevertheless, they gave me a degree, in absentia, and this made a marked difference in my life, because when I went back to the Marine Corps, I was to be a platoon leader, and I took my officers' training. Those of us who were at Quantico, Virginia, in various fields, I was going to be in the Signal Corps, but infantry.

Many of my classmates were called in and ended up on Guadalcanal right away, a very serious battle, our first serious battle in the Pacific. I was tapped and said, "Well, you're a physics major, so you can go to Harvard and study radar," which was then called electronic engineering. So I had the first taste of how education can change your direction, and, in this case, in a major way.

I then went from there to some of the practicing radar schools from the Army in Florida, and then set up the Marine Corps' radar schools at Camp Lejeune and ran them for several years before I went overseas. In fact, my going overseas was at my wish, because I had a commanding officer that I was not able to get along with, and I requested a transfer because I knew people in

Washington, simply because we were working up training films, and the film crew was from Washington, so I was able to get up there and request a transfer.

So I went overseas as a radar officer with the 7th Antiaircraft Artillery Battalion. We had a very, not difficult compared to the other groups. It was on the Peleliu invasion, and, as you know, that was a minor disaster in terms of casualties, or a major disaster. But we landed on the island just to the south, which was not as heavily fortified, and our task was the early warning and fire control of the entire island group once the battle group left, with all their radar and so forth. So I spent a year there setting up the radar and running this, and making an amphibious landing and so forth.

RR: So, some early experience with amphibious landings, even though at that point it was more with the radar team and not--

Inman: How do you get these big radar vans through the surf zone. We had actually worked out some very good systems. We had eyebolts right through these big top-heavy vans, and connected with ropes to either side, so when we went through the surf and up onto the beach, we could control them with just manpower on either side to get them up.

RR: That's difficult. When you come back from World War II, and then you are at Caltech briefly and then you come to Scripps, I wonder if you could tell me a bit about what your

training was like in oceanography here at Scripps. Who were your major teachers? If you can comment on that sort of thing.

Inman: When I came to Scripps, of course, the director was Harald Sverdrup, whom I didn't know before that time. Roger Revelle, whom I had met previously, was still in Washington, although he was in and out at Scripps, but mostly in Washington. The geology group was headed by Dr. Francis Shepard, and I then became his student. He was my advisor.

For many of us, this was a very tough introduction. I could backtrack and say that having spent four and a half to five years at war, I knew that if I came back to graduate school, I would have to do some considerable brushing up, so Baylor Brooks arranged for me to come back to San Diego State and teach courses in all the basic sciences for one semester, and I did that, and I think it helped.

When we came here, we were at further disadvantage, because about half of the class had already had up to three months of oceanography when they came through Scripps. They were weather and wave and swell forecasters, so these guys had an up on us, and we had to look to them, who had already at least started oceanography, and it was a brand-new subject to most of us, but fascinating.

At that time, Shepard's book was just being written. He was writing it and he handed out mimeographed chapters, and we would go through and use those, criticize them. I should point out that this was a very different graduate class than one would expect to have these days,

because almost all of us were officers during the war. We'd had field commands overseas, we were really a very much more mature group in terms of life decisions that you have to make, than the average graduate student would be today. So it was an exhilarating but very trying time, because most of us had been away for a while and had been away from our studies.

RR: I wonder if you could give me some of your impressions about Sverdrup. I assume you took probably physical oceanography from Sverdrup. Or just about him more generally, as the director.

Inman: I took two physical oceanography courses from Sverdrup, and he was an outstanding teacher. Not only that, but he was a real scholar. Of all the people at Scripps that I've later been associated with, I think Sverdrup and Carl Eckart were the two real scholars, and Sverdrup, of course, was one of these, and I had a fairly close association with him because we were a small group and he was interested in what we were doing. I was interested in physical oceanography, being a geologist and a physicist in background, and he was very free and giving of his time.

RR: What about Fran Shepard? I gather he's clearly your major professor.

Inman: Fran Shepard was my major professor and he was just writing the book he called *Submarine Geology*. And as I say, many of us had inputs. I believe I even supplied a

photograph of the Palau Islands for his book. He was certainly *the* geologist in marine geology in the country, and I think he's now known as the father of marine geology, or that's one of his titles. He was an early descriptive geologist. Fran was not as interested in how things got there, but what was there. And in every science, it's necessary to go through a very intense and important descriptive phase before you can go into the more quantitative phase. So I would say that Fran Shepard was the descriptive geologist of that era. If he said it was mud at that point on the shelf, then there was mud there.

RR: I don't know if it's really this period or somewhat later, he and Revelle don't get along very well in somewhat later years. Do you have any insights into that?

Inman: Some. They are certainly total opposites in character and personality, and where Roger is dynamic and outgoing and visionary, I think Fran was somewhat less, certainly less so in all those respects. On the other hand, Shepard was the older geologist there and Revelle was a young upstart, so there was bound to be some difference and friction, and there was.

RR: One of the things you talked about in your talk last week, is the importance of interdisciplinary work in oceanography and what I think what many people, Sverdrup and others, sort of refer to as a balanced approach in oceanography. I wonder if you could comment

on that in terms of your training as an oceanographer and maybe in ways in which it comes into your own work.

Inman: I think that geologists in general tend to be a little more interdisciplinary than some other groups, for example, physicists and biologists, to some extent. So that, in this case, it seemed to me that Sverdrup was a remarkable person, because basically he was a physical oceanographer, and yet he was the one who brought all these disciplines together. He really put oceanography as an interrelated science group. He's the one who did that, no one else.

I think throughout his time here, he had to spend time getting people together. It would have been interesting to have insights when he and Martin Johnson and [Richard] Fleming were writing *The Oceans*, because I suspect that he had to bring a certain amount of coercion to get this interdisciplinary activity started.

RR: Can you comment about it in your own work? In what ways does your work represent that interdisciplinary kind of approach and background?

Inman: Well, I think that my work, which is really the quantification of coastal studies, uses it extensively. There's no way that the physical oceanography, in terms of generating waves, currents, is not a fundamental driving force along the coast, and geology, the land masses that

these forces are acting upon, are in another discipline, so that I think it was very important in the way I approach my science.

RR: It sounds like it's somewhat different than Shepard's. I hadn't really thought of this until now, because I don't really see Shepard's work--I see it, as you say, as sort of more descriptive, submarine geology, maybe not as much concerned with waves, mixing, currents. Are you and your generation sort of the first ones to really bring that physical oceanography to bear on the geological questions, or am I giving disservice to Shepard?

Inman: No, I think you're quite right. We are the first, and this is perhaps why at Scripps my principal mentors would be Sverdrup and Revelle, because they were the people also with that same visionary approach, and more so than Shepard. I think an example would be the case that in his second edition of *Submarine Geology*, he had me and Ed [Edward] Goldberg write chapters, sort of bringing the mechanics of sedimentation and waves and currents, in my case, up to date, two chapters, and Goldberg bringing some of the chemistry and chemistry of sediments up to date, and out of the descriptive realm and into the quantitative realm. This stood for several years, but as Shepard got older, he didn't like this situation of having chapters in his book that he didn't really understand, and so in a later edition, he took them out.

RR: That's very interesting. That is quite unfortunate.

Inman: Well, what he did, rather than take them out, was he rewrote them, no equations.

RR: Physics for poets kind of thing.

Inman: And I think they lost some of the--after all, mathematics in terms of statements of this kind is simply a shorthand of covering more completely a subject, so that it lost some of the things that it had before.

RR: Some people have said that particularly by the 1950s and later, to some extent, that Scripps kind of loses that balanced approach or that interdisciplinarity, and I wonder if you could comment on that. What's your view on that?

Inman: Well, that's exactly right. I think it's a combination of the factors that, as groups grow, and I mean, faculty, Scripps--I mean, after all, Scripps is doubling every ten years. In fact, our class of '46 more than doubled the academics at Scripps, and it's been going that way ever since. So the interdisciplinary or the interrelatedness of all science that Sverdrup had pioneered, and certainly has stated in *The Oceans* and elsewhere, was much more easily maintained when Sverdrup was here and when the groups were small.

After all, every Wednesday, I believe it was, we had a group meeting that included everyone at Scripps, in one lecture hall, and usually a very interesting speaker. We would all enter from all disciplines, so that everyone was subjected to the same situation, we knew each other, and it's a very different atmosphere than we had as the curricula and faculty grew and Scripps grew, then the interrelatedness of science seems to get further and further.

I know there was one time--I would have to look up the date--we have always had in Scripps the four interdisciplinary courses, as a starting point, and the earth science of that day, scientists of that day, decided, and the geochemists, that there was not sufficient geochemistry required of their students, and so they attempted to put through a situation where they took entrances directly from chemistry on the upper campus or other schools, and no more basic courses. This was a severe direct collision of the interrelatedness of science versus the disciplinary approach, and we had quite a faculty uproar about this. I remember that the three of us who finally won over the faculty were myself, John Isaacs, and Fager, Bill Fager. We were the three interdisciplinarians, and we did persevere, but it was only just.

RR: I've read a little bit from biologists like Carl Hubbs, and, to some extent, Claude Zobell, that when Revelle returns in the late forties with mostly an emphasis on physical, geological oceanography, that the biologists feel that they are sort of on the margins in some ways. You're not in the biological oceanographic group, but I wonder if that has some bearing, at least for them, in terms of the move away from this earlier balanced approach.

Inman: I think, again, it's related to the large groups, as we had a large faculty in biology, and I should point out that part of the faculty in biology, and you've mentioned some of them--Hubbs and Zobell and so forth--were not part of this interdisciplinary teaching to begin with, and so as time went on, they were leaders in trying to break it up.

RR: Would Martin Johnson or any--were there any biologists participating in the interdisciplinary--

Inman: Martin Johnson, and he remained interdisciplinary.

RR: That's very interesting.

Inman: Also there was another aspect to this, besides the fact that Scripps grew, we had larger groups. When Sverdrup was here, we did not have a fleet, so he was more or less around and things were under his supervision. When Roger came here, we were world explorers, he was gone a great deal of the time, and it's very difficult to have any direct control of a faculty that's back home somewhere and you don't see very often.

RR: Yes, he was gone a great deal on many different things, as we know. I want to shift a little bit and ask a couple of questions about your very early work in the late forties. I know that some of your work is on Scripps Canyon, on Point Magu. Can you describe some of what kind of work you were doing in that period of time? What were the main emphases?

Inman: I think the thing that I did was to try to, as I say, quantify coastal geology in general, and near-shore processes. This began by the fact that early on, there was no direct known relation between waves, currents, and sediment transport, for example. And yet we had finally mastered at least the early rudiments of wave forecasting, and there was now good wave hindcast and wave data, and so I began working on this problem of the interrelatedness, in this case, of waves and currents and sediment transport in the coastal areas, and particularly along beaches and in shallow water.

This required extensive measurements of both suspended sediment, of waves in the surf zone, special wave arrays, a digital approach to data acquisition, and I relied heavily on the physical oceanographers, Walter Munk, for one, who was working extensively with waves and wave measurements. Through this procedure we were able to relate the wave energy flux as it comes into coastlines, and how much sediment is transported along the coastline by that flux, and demonstrate, for the first time, that there was a direct cause and sequence between the forcing functions of waves and currents, and the sediment that was transported.

By having done that, then there was the problem of, okay, how do you extend this to a general understanding of what's happening along coasts. I developed a concept of a littoral cell, which would involve the transport of sediment into the area from streams and rivers, coastal erosion, the driving forces of waves and currents impinging on this area, how much material is taken along the coast and where it goes. This is where the submarines canyons in our coast come in.

So if you have a littoral cell that establishes the metes and bounds of this problem, and you can begin to look for a balance of sediment, in terms of how much comes in versus how much moves, versus how much goes out, there has to be some kind of balance here, a balance of the energy fluxes involved and so forth. So I think this is an important step in understanding coastal processes.

We actually put some of these concepts in a training film that the *Encyclopedia Britannica* put out. It's called *The Beach: A River of Sand*, and I'm happy to say I was the technical director of that. We did it in the mid-sixties and it's still just as applicable today as it was then.

Then we went further than that. We relied heavily on the concept of plate tectonics, and I looked at the problem of why are there different kinds of coastlines around the world. There have been all sorts of coastal classifications, but none of them seemed adequate. So we came up with a classification based on the plate tectonics, looking at these areas such as the California coast and the coast of South America, where the plates have collided and caused coastal mountain ranges, versus the trailing edge of the continental plate, such as the east coast of the continents,

where you have totally different kinds of morphology and coastal topography, and wider shelves, and rivers play a different role and so forth.

By dividing the world into these kinds of coastal classifications, what I'd call those are along collision coasts, those are along trailing-edge coasts, those that are in marginal seas with large deltas, like the Gulf of Mexico, the Mediterranean, and so forth, coral reef, yet a different classification, and arctic coast, you could subdivide the world's coastlines into things that you now apply littoral cells to, and make quantitative sense out of the driving forces and sediment transport in all these areas.

RR: That's quite an accomplishment, in many ways. [Interruption.]

I wonder if you could comment on if you had any connections with Murrough O'Brien in the engineering group, doing coastal engineering out of Berkeley.

Inman: I think there was a fairly extensive contact. It wasn't, by the older groups here, always as pleasant as it might have been, but in my case, I got along with these people quite well. The background, of course, is that during World War II and before, Mike O'Brien's group at Berkeley was working with waves and so forth and forecasting from the engineering standpoint, and working with it throughout the war. In fact, they did a lot of experiments as to what size waves DUKWs could actually surf on and not capsize.

At the same time, Walter Munk and Harald Sverdrup were doing wave forecasting and surf zone work down here, so there was a certain amount of competition between these two groups. My main contact with them, at that time, we didn't have a hydraulics laboratory, which later I had some input for our own, but I would go up to Berkeley and calibrate our sensors and things like that in the Berkeley labs, and so I got to know Mike O'Brien and Joe Johnson and Bob Weigel quite well.

RR: I thought, for a while, in the late forties, Johnson had something down here at Scripps, too. Maybe that's not correct.

Inman: Well, Joe Johnson, of course, was interested in coastlines anywhere, and he may very well--I don't recall that he did.

RR: Let me sort of switch a little bit here and talk about patronage in some ways. In those early years, and certainly on into the fifties, much of the support for your studies and for many others, but in coastal and near-shore studies, come from ONR [Office of Naval Research] and from the Beach Erosion Board. Last week you referred to Roger's aphorisms about ONR, but I wonder if you could comment a little bit on why ONR would have been funding that sort of work.

Inman: Well, in the first place, ONR, early on, was well aware of the importance of amphibious operations to World War II. After all, the wave forecasting came through Scripps, came through ONR. Secondly, under their early considerations, they supported any basic science. They were the basic science supporters in this country.

If you look at who supported basic science in this country after World War II, it was ONR. Now, others did, too, but by and large, it was ONR, and it didn't make any difference whether it was physics, the oceans, chemistry, sedimentation, biology, or what it is, ONR funded it. They even allowed the University of California to establish the CALCOFI Program by providing the ships and salaries to get it started. Couldn't have happened without ONR.

So ONR was a very broad-based funding group early on. Now, on the same hand, the Beach Erosion Board, when it was at 5202 Little Falls Road, Northwest, in Washington, D.C., was also a very small and broad-minded funding group, particularly under the directorship of Martin Mason. Martin Mason was as interested in all aspects of coastal problems as anyone else, and things progressed very well here and everywhere at that point.

Now, following that, all these areas grew, times changed, the Beach Erosion Board became the Coastal Engineering Research Center, it moved on down to Fort Belvoir, and it took on a much more military, at least in terms of military and coastal engineering aspects, than it had formerly.

ONR, on the other hand, had a problem in that following World War II, beaches seemed to become less important to them, and with the onset of the Cold War, the big thing was the deep blue oceanography and submarines, so their emphasis changed slightly, too.

So all of these things entered, and I know that there was a period when we in coastal problems and coastal studies had more problems. I can point out to you how bad it can get. We had a contract with what was then the Coastal Engineering Research Center, and the contract stated clearly in it that I was studying the interrelation of the driving force of waves and the profiles of beaches, the formation of a beach and how steep the beach face would be and so forth, and I had stated that clearly in the proposal and it was granted.

But then having looked at it, they decided that waves came under a different section and beach profiles under yet a different section, and, sure, we could go do this, but we had to have two separate reports and report to two separate groups, and so forth. The net result was that we had to perform the research they sponsored, submit the reports, and then come back on our own and put it together. This shows you how bad it can get under the bureaucratic approach of the Coastal Engineering Research Center in later years.

RR: I want to bring you back a little bit to ONR for a second. You commented--and this is a well-known sort of point about ONR--that they're very supportive of basic research, fundamental research, but I wonder what your view is on the relationship between basic research

and sort of mission-driven research, in relationship to ONR. Because clearly they're interested in landings, trafficability, those kinds of things, even mine warfare, that come into coastal studies.

Inman: Those kinds of things, of course, change emphasis, and as ONR became older, several things happened. One is, they got further away from their basic general science support. Secondly, the National Science Foundation was formed, NSF, was supposed to take over more of that, and Congress then got in the act as well and said, "Hey, you're a Navy group, why aren't you doing Navy relevance here instead of general background science?"

And so all of these forces caused a change in the ONR approach, and it was very obvious, throughout the Cold War, the approach was not particularly on the coastal activities and so forth. It was not until the almost recent times in the Gulf War when, with a bang, they came back to the fact that coastal oceanography is a very important part of what we should look at.

RR: Revelle was very well known for his aphorisms in some ways, and the point that you made last week where one of his comments was, in dealing with ONR funding, that Navy relevance was always secondary, at least that's what he would claim. Both are involved. There's clearly basic research, but there's also Navy relevance for most of ONR kinds of things, and I wonder why Revelle particularly minimizes the sort of Navy connection or the Navy relevance.

Inman: I think Revelle, following the Sverdrup doctrine of the interrelatedness of science, was well aware of the fact that in relevance you tend to channel your efforts in a very direct, straight, narrow path, and that the ocean is a great big multidisciplinary place with lots of interaction on all scales and at all times. It seems to me that they were well aware of this, and if you let relevance creep into this to that extent, then you're going to overlook something that may be very important to the problem.

RR: I wondered, in some ways, too, if he might be trying to sell the scientists to take ONR contracts, in the sense that after the war, people are more interested in getting back to their fundamental research and their basic research, and it's the Navy that's the main funder. Do you think that he wanted to kind of sell the Navy to the science community?

Inman: I don't think so, at least not in oceanography. Perhaps somewhere this would be a facet of that problem, but it seems to me that those of us in oceanography who came through the war and then went to Scripps under the Sverdrup doctrine and so forth, that I don't think that would have ever been a problem.

RR: You didn't need to be sold on this.

Inman: No. Also, anyone wants money for their research, and to some extent, you take it where it is.

RR: You get more than money, too, right? You get ships, you get material, instruments.

Inman: All those things, right.

RR: All those kinds of things. You mentioned the Cold War, and the early Cold War. What impact did the Korean War have on your work in coastal oceanography?

Inman: I think the Korean War, in terms of general impact, was relatively small, considering the various wars we've been involved in. I mean, Vietnam was quite different and had a very much more pronounced impact. I personally had a very brief association with the Korean War, in that I was back in Washington's Hydrographic Office when they were planning the Inchon landing, and although I didn't know that at the time and it was all highly secret, I had been back there giving a series of lectures on coastlines and tidal estuaries and so forth, so that I was brought into this highly classified area and asked my opinion about this. I must say I thought it was a great risk, by its very nature. The great risk paid off, and it was a very successful landing. But I didn't even know about where it was until sometime later. It was very highly classified. But in terms

of general oceanography and oceanographic funding, I don't think the Korean War had nearly the impact that the other involvements have had.

RR: Could you comment a little further about the other ones, then? Vietnam.

Inman: Well, Vietnam was, of course, I mean, it was a national tragedy. We had a lot of contact, Scripps did, and I personally had a great deal of contact with the Vietnam War. In the first place, going back well before it, many of us, including some of my former classmates, conducted a UNESCO-held Marine school in Natrong in Vietnam in 1959. This also coincided with the International Geophysical Year, and so we were not only conducting a class there in what had been formerly the French oceanographic laboratory at Natrong, but we also had wheels, which was very important. I got to drive around most of Vietnam before the war.

[Begin Tape 1, Side 2]

Inman: Vietnam, we were there before the war and many of us then had had an opportunity to really look at the country and know something about what was happening. Therefore, when the war came along, we were in a position to know a great deal about the country already. My own personal approach had been the fact that I certainly was against what we were doing in Vietnam,

but on the other hand, not sufficiently so to deny what expertise I had to the country to facilitate what our guys were doing over there.

So I sighted the harbors that were used in the Vietnam War, did it from small craft, I mean airplanes and helicopters, under very hazardous, trying conditions. So later on, I had an extensive involvement in a series of reports, and these had a great deal of impact on my science, because it was some of the first quantitative science about processes in Vietnam. We had generated wave data for the entire coast. So to an extent, Vietnam became a testing point for some of our concepts and theories, and this, in retrospect, was a very useful and well-funded approach to science.

RR: A couple of sort of follow-up questions. Were you part of the NAGA expedition then, which goes over from Scripps in, what, '59?

Inman: '59. I was a land-based end of NAGA, yes.

RR: And NAGA continues into the early sixties?

Inman: Yes.

RR: During the time when the Vietnam protests come around, the anti-Vietnam protests come around, how did you feel about those?

Inman: Well, as I say, basically I was against our involvement, but as a person associated with invasions and people fighting over there, I couldn't deny what expertise I had to do this cause. Also, I mean, in the long background, had we done the proper thing by Ho Chi Minh in the first place, this would never have happened. After all, we had people in there advising and helping Ho Chi Minh, and he would probably have rather gone U.S. than Russian if it hadn't been for the fact that we withdrew all of that and left him sitting there. So he still wanted his independence for his people, and this is what came of it. So it was politically a very bad situation all the way around.

But in terms of here at Scripps, they held campus meetings and so forth, and I tried once, this business of talking to the students. It's no go. I mean, they don't want to hear what you have to say. They simply want to shout you down. And it was a very trying time.

Walter Munk and I and others were called part of the military industrial complex in all the papers, there were threatening notes and etc., and I think the thing that really let us off with as little problem as we had was the fact that the upper campus and Scripps do have a geographic separation and distance, and several times when the upper campus students attempted to come down to Scripps, the distance was such that their ranks were depleted before they got here, and so nothing ever much came of this. It's really very wise and fortunate that nothing did come of it, because we had many technicians and other people who were very much involved with what we

were doing and what was going on, and they weren't about to have these guys from the upper campus come down and destroy Scripps. So I'm sure it would have been a bloody clash had this occurred.

RR: I know they went down to NEL [Navy Electronics Laboratory] at one point. I didn't know if they ever tried to come down to Scripps.

Inman: Oh, several times.

RR: I want to take you back a little bit to the earlier period and ask you sort of a related question in some ways. I know that recently, in some of your recent publications you've written about work that was actually done in the early fifties and couldn't be published in the early fifties--the Korean, early Cold War period--on mines, the paper on the chronology of mines. I gather that this is because of classification.

Inman: Absolutely.

RR: I wonder if you could comment on the role of classification.

Inman: Of course. In the early fifties, in sort of preparation for Korea and other things, there was a great deal of interest in mines. We also were studying the near shore. We had three amphibious DUKWs, and they were in many ways remarkable amphibious vehicles. They were simply a big floating bathtub with a very strong motor and lots of water pumps, and as long as the motor ran and the pumps worked, why, they would pump water out about as fast as it came in, so they didn't sink. But if you killed the motor, that was it.

At any rate, they were useful for our survey techniques, and we mounted a fathometer on them and would survey the beaches and so forth, so these were very valuable. Also, we at Scripps, particularly in Fran Shepard's group, did the first diving at Scripps. This is because Fran Shepard and Jacques Cousteau were friends and Cousteau was trying to sell his aqualungs, so he came over with the first aqualungs, and left, I believe, three of them with us. This was in many ways a hazard, because we didn't know how to use them. [Laughter] That's another tale, and a long one, but we did start using them.

It turned out that later on, when we had developed a diving procedure, then all divers were required to take one or two or three dives a month in order to keep their card, just like flyers and so forth. The big problem at Scripps is how do you dive and get through the surf zone. So whenever we went out for our own work, we always had many volunteers who would go along and get their dive in and also perform our work. So I had a team of people out there often.

The navy was interested in mine scour, so I told them, "If you put these mines out, we'll drop them for you. In fact, if you give us the dummy mines, we'll put them out and we'll

measure the change in bottom and scour around mines. We'll photograph them and give you all the information."

Initially, it was simply that kind of observation on our part, so we put four mines out at different depths along the Scripps shelf. We went out and measured them probably several times a week for a number of years, and we began to see a lot that was going on about how mines scour and bury. In general, we found that a mine sits there on the sand bed, the vortex over the mine caused by the waves scours the material, and eventually it pushes the mine into that scour hole and so forth. There are several mechanisms involved in mine scour and burial.

We had some three to four years of good data, detailed data, and then the Navy decided that this was a classified project. So I did work with them to give them some of their classified write-ups of mines and so forth, but it went where most classified things do, which is in some classified file, and nobody ever sees it. [Laughter] So that was the end of that. This was in 1954.

Then came, of course, the problems of the Cold War, which were less directly shallow-water mines and more directly deep blue and submarine, so there was a long hiatus.

But then following the more recent times in the Gulf War, why, mines are back and in as a study project, and it turns out that the data that we collected for the Navy--in that case, for free; we were doing it as an additional observation to our other studies--had a database that would have cost them many millions of dollars to start over and try to do now. So it put us in a position to use the database we'd collected in '54 to proof the model experiments that we were

doing and checking also with observational, but mostly model-type experiments here in the year 2000.

RR: It's a long time between the publications. I wonder if you could talk a little bit more about your views or your attitudes toward classification. Obviously, in that case it delayed publication on research for a long period of time. Do you see other sorts of problems or difficulties that are presented by classification?

Inman: None of us want to become in classified research if we can avoid it, just for that reason. You can't publish and you can't have freedom of discussion and contact with others, so it's not the desired situation that most of us would like. Of course, during the Cold War--this was not firsthand, although I did talk to the people involved--the Russians, as you may recall, we were having visits here and there between the Russians and our people, and they suddenly decided we couldn't enter through a certain port, I've forgotten which one now, so we decided they couldn't enter through San Diego. They had to go up to Long Beach instead. This was the typical Cold War back-and-forth tactic.

Also, we had a classification on all soundings. Well, this is the bread and butter of most research in my kind of studies, so this was not a good situation. This was finally broken by the Navy when we had a visit from the Russians, and the Russian guys on their research ship actually put in to Long Beach because they couldn't visit San Diego. Then we flew them down to

San Diego, where they could really get a good look at our installations. [Laughter] They visited Scripps for a number of days. But somehow they learned about this problem of classification of soundings, and there was someone in our group, I've forgotten who now, who really needed a good profile from here to Hawaii to publish, and so the Russians said they would send it to us. [Laughter] I think this went a long way towards breaking down that classification on soundings.

RR: That's very interesting. Did you ever have the feeling that when you were working on classified, doing classified work, or work that then gets classified, do you ever know what happens to it? Do you ever get a sense there's just this big black hole out there, or did anybody ever get back to you to say, "Thanks for this work"?

Inman: No, never. It's a big black hole, and by the time it's declassified, it's so many years later that you're really working on something else. The mine scour data was an exceptional case in which it's still valid and we can use it, but if you're on to some totally different situation, why, it's just a loss. It's lost time in the past.

RR: What kind of sense do you have about the impact that that might have on when you come up for tenure or promotion, or for just getting credit for your work?

Inman: Oh, I think the impact can be rather severe, and those who have done mostly classified are at an extreme disadvantage. I know of several of them here that have not been promoted at the same rate. I think that classification is frequently unnecessary, and that's the other problem with it. I'm certainly not against classification of highly important things like the atom bomb and other things, but I think that just routine classification for its sake is a waste of energy, a waste of science, and produces poor science. I think if we want to be tops in science, communication is the important situation, and if you can communicate and stay ahead of them, that's how it's going to have to be. You can't just classify it and hope they don't find out about it.

RR: I don't know if this is really classification or not insomuch as maybe just contract work, but I know in your case, you published a great deal, and yet in trying to track down your publications, a lot of them are more kind of contract studies for ONR or Beach Erosion Board. I wonder what your sense is on what kind of impact that has had.

Inman: Well, I think this also has a negative impact. Much of my early material that came out, for example, on the Beach Erosion Board, was, of course, not classified, but it was required that it be put there, and having once been put there, it isn't then available to be republished in some scientific journal, so that it puts your publication record in a very questionable situation, and for any young scientist trying to get along, that's bad.

RR: Did that have any bad consequences for you? I assume it didn't. You became a professor at Scripps.

Inman: Well, I did become a professor, but then it probably had some bad effects as well. Maybe I would have been one sooner. [Laughter]

RR: Well, possibly. I didn't realize that the Beach Erosion Board reports were really classified.

Inman: They weren't, but you were required to publish in them, and since you put it out, to many of your colleagues it was considered quasi-grade literature, because it didn't go through peer review.

RR: That makes more sense. I must not have heard you correctly the first time. [Interruption.]

We're going to come back for a second to some of the protests in the late sixties. Herbert Marcuse was here. Did he play a role?

Inman: Well, he played a role, of course, in that he was very sympathetic and, in fact, egging on the student revolts, so this was a major role, I would think, particularly on this campus. As I had mentioned before, there were several attempts to have marches, student marches, down to

Scripps, and our geographic separation prevented this. People dropped out and by the time they got here, they had lost some of their verve.

But we played this in a rather smart way, I think. We finally invited Marcuse down to give a lecture in Sverdrup Hall, and we made sure that the graduate-student population at Scripps filled up at least half of the audience, and then when Marcuse gave his talk and his followers would start to yell and holler and so forth, we had sufficient people in the audience that it was controllable. So he gave a talk which convinced--certainly didn't convince me, and I think hardly anyone here--and it lost all of its verve, so that it was a failure in terms of his expectations.

RR: That's interesting. Did you agree with any of the students' complaints or with Marcuse's complaints, or was it so polarized that there wasn't much common ground?

Inman: I think as I've indicated, it was so polarized, my own thought was, as I've told you, I had my own doubts, and I tried to communicate with student bodies in open forum, the open forums they had, but it turned out you couldn't, because they would shout you down. And that was the whole exercise, so how could you communicate?

RR: Thank you. [Interruption.]

We were talking a bit about classified research and about being supported by ONR and the Beach Erosion Board. How did Beach Erosion Board's support or their emphases differ from

ONR, or could you do essentially the same kinds of things for ONR that you were doing for the Beach Erosion Board, or did they have different objectives in mind? Was ONR as sort of interested in fundamental research in the same way? Was Beach Erosion Board interested in fundamental research?

Inman: I think it depends on the time, and I will speak now just for my own field of coastal processes. The ONR approach, originally, under the original guidelines, was basic science, and so that was the best of approaches for those of us who were doing science. The Beach Erosion Board, when it was small and under Martin Mason, also had a very similar approach, so there was not a vast difference between them.

I believe, in my opinion, the Beach Erosion Board, as it turned into the Coastal Engineering Center and got very much larger, progressively got more difficult and more oriented towards their specific divisional, departmental goals than ONR, so there was a time when ONR, even though it was less a basic science approach than early on, was at least a better funding agency, from my standpoint, than the Beach Erosion Board.

RR: I know I've seen some correspondence between Revelle and Mason, in which Revelle says that the Beach Erosion Board is essentially asking Scripps to do what he called "task work," and Revelle didn't like that and he said, "We want you to orient more toward fundamental research and we'll do that, but we won't really do task work." Do you think Revelle actually had some

impact on Mason, or was Revelle just--I mean, you're saying the Beach Erosion Board kind of was supporting fundamental work, and Revelle had some different views of that, I think.

Inman: I don't know the dates of the letters you're referring to, but I think that Revelle may very well have had a big impact on Martin Mason, because early on, now that you've mentioned it, I do recall some discussion about this matter, and Roger's entering, in perhaps the way you're suggesting, with letters and what have you. But then for several years, and under Martin Mason, the Beach Erosion Board was a very broad-minded supporter.

RR: I know you've also done a fair amount of work for the Atomic Energy Commission, both in the fifties and then again in the seventies. I wonder if you could talk about some of the work you did for that agency.

Inman: Sure. [Laughter]

RR: You can say no. [Laughter]

Inman: Actually, my participation was more in terms of what would happen if a radioactive missile was to fall in the near-shore waters, and in that sense, it was very good basic science. I know that we made studies involving the area around Vandenberg, because there was some

thought that if they might be putting material in orbit--there was some thought of putting some radioactivity to control these units once they were in orbit, and so we carried out extensive studies, with detail of what the material would be in terms of its density and so forth, and how it would interact with various kinds of sedimentary bottoms and currents and where these might go. So to that extent, it was very good basic science.

Also, I should mention that we, early on, had had a very trying problem with sediment transport, and that is, sediment moves from here to here. How do you know how much of it really started here and ended up there? How do you tag it? That was a fundamental problem, and it's one that Ed Goldberg and I worked on, he from the chemical standpoint, and me from the sedimentological.

So what we did was to take some Scripps beach sand, a pail of it, and send it back to Oak Ridge, and they radiated it a certain time, and it came back to us. We had to take on very stringent procedures for using it. It turned out that there was a beta radiation in quartz that was associated probably with small apatitic [?] inclusions in the quartz, rather than the quartz itself, and that once these were radiated, they would give you an individual grain tracer for some length of time, and at a low enough level that it wasn't a problem. So we thought that we'd hit on just the procedure.

But when the Atomic Energy Commission found out about this, that it was something that we would then want to put on beaches and use, they were not so concerned about the radioactivity itself, since it was a very low level and would be widely dispersed, but rather about

the people handling it. So we had to undergo monthly physical checks, and the most ridiculous thing of all, monthly X-ray examinations. [Laughter] It didn't take a very bright person to figure out that they were subjecting us to more radiation by the testing than we could possibly get from this procedure. And so it died.

RR: Was this part of Project Plowshare?

Inman: Plowshare was different. That was mostly a John Isaacs project. Plowshare was the idea of using rather large explosions to change areas, perhaps Kamchatka and areas, and change the circulation in those areas. It was perhaps a grandiose idea, and there was considerable survey in the northern portions of Alaska. But I wasn't directly involved at that time in those, although I later on carried on a number, so that I don't know the details of that. That was a John Isaacs-type project.

RR: I know that in the early seventies you do some work that is studies for their work, in term of powerplants, nuclear powerplants, and water motion and water-sediment interaction.

Inman: I had totally forgotten that. The idea was at that time that nuclear energy would be a major source of power, and, of course, in some instances it has been, but not extensively in this country, for a number of reasons. But at that time the question was, where do you put these? I

think my principal involvement was studying offshore localities, particularly on the East Coast, how you would go about constructing an offshore, if you wish, a small island that would contain this well away from the mainland and so forth. Again, this was good basic engineering science, but nothing came of it, since the country turned against nuclear power.

RR: The one report that I read was from 1973, and in that report you're basically kind of telling the Atomic Energy Commission what they need to know in order to do these sightings, and you talk a lot about physical oceanography, sea-floor studies, that they need to incorporate those kinds of things. I wondered to what extent they did. How receptive were they to the kinds of studies or the kinds of points you were making?

Inman: In toto, we don't know, of course, but at the time, it seemed to me they were receptive. The point is, it was never done, so it's not clear whether they would have carried through or not.

RR: I don't know if you would have been involved in this or not, in the late sixties, early seventies, there's a lot of concern about powerplants and thermal fish kills, basically, from the waters associated with the powerplants and these massive sorts of fish kills. Was that in any way part of what they were involved in?

Inman: That was something that I became involved in, off and on, over a long period of time, was the amount of heating that came out in these coolants. In other words, there's a water intake and a warmer water output from the plants, and how much this would affect fish and kelp, but also to what extent there's simply an exchange of water. This also went in conjunction with just outfalls in general, from municipal sewers to other kinds of outfall. What does this do to the local environment?

I know that I chaired a citizens' committee back in the early days, when there was discussion of what to do with San Diego's outfall. This is not nuclear, this is simply the sewage outfall. We looked at the coastline and sighted the locality off Point Loma, that they now use. I'm afraid at that time we didn't dream of the amount of material that would later come through that outfall, but this was an early attempt to sight it in deep water, well enough away from the coast so that it would do a minimum of harm.

To some extent, my opinions have changed simply because of the amount of this. I think if you have one outfall like the San Diego outfall, using sewage exchange with water at the rate that it was designed for, that there's not a great deal of harm associated with it. When they extended it, and extended both the distance and the amount of material going out, and other outfalls happen up and down coasts, so it's almost no longer a point source, but a general background source, then I think it's a totally different type of problem.

RR: I'll ask you a follow-up to that in a second. I want to come back to an earlier point where you were talking about these offshore powerplants. The other day when we were interviewing Bill Nierenberg, he talked about some of his ideas in the mid- and late sixties, of these floating islands, and I wondered, is there any connection?

Inman: Well, we here at Scripps thought of what was going to be called "Scripps Island" for a short time. It was an idea to get people just offshore and instruments and so forth at the canyon head, and it was just before we started doing all--in the realm of when we were doing our saucer dives. I should mention that we here at Scripps brought the Cousteau diving saucer over from France and used it extensively up and down the coast, diving into these small places where the Cousteau diving saucer could get that no other could at that time. In fact, we had dubbed it as the only true flying saucer, since it was brought over in an Air Force cargo plane. [Laughter]

But at that time, there was also a thought that there would be perhaps more underwater activity in terms of research, and there actually has been as follows, and the Scripps pier was in quite shallow water and during heavy seas you can't use it, so there was a discussion of building something at the head of Scripps submarine canyon, that on one side of it had adjacent deep water.

This was a fun time. Nothing ever came of this, but actually, we contacted Harald Sverdrup's brother [Leif Sverdrup], who was a Corps of Engineers general, I believe, in World War II, and later a very important civil engineer. I think they put the underground system across

Chesapeake Bay and so forth, entrance. So he came out and gave us free advice about how we might go about this, and reminisced a bit about his brother and so forth. It was really a fun time.

RR: That's very interesting. You mentioned before, the point about the outfall, the sewage outfall. I know that you, in the sixties and ever since, have gotten very involved with issues concerning human intervention into the coastal environment. I wonder if you could talk about some of the major projects you've been involved with in that.

Inman: I think, early on, the first situation that I was involved with is the fact that if you build a harbor, for example, along one of these areas that's in the middle of a littoral cell, so that the source is upcoast from it and it has to travel past it on to downcoast, that then this is a situation where the structure has to be very carefully designed, or, indeed, it will interrupt the supply of materials, so that the sediment will be stopped like a dam at that point.

When that happens, since the demand for moving sediment goes on downcoast, then there's always downcoast erosion and upcoast accretion, which upsets the natural balance. I've been very active in extensive problems of this kind, and they still occur. Some of the points that I made in this motion picture, *The Beach: A River of Sand*, are on that point, and still hold true today.

RR: What are some of those points?

Inman: Well, the point is that if you do interrupt this river of sand, there will be an accretion on one side and an erosion and sometimes very serious erosion on the other, and then the common situation is, hey, if this is eroding, we'll build a structure further south and continue until, before you know it, the whole coastline is armored, which isn't exactly what many of us had in mind for a natural coastline.

RR: I'm sorry, what do you mean by "armored"?

Inman: Coastal structures, such as groins and sea walls, whatever. At one time these coastal harbors in the middle of littoral cells were called "white elephants." An example, I think, a good example, is the early one, which people seem to observe but not learned from, and that was Santa Barbara. Santa Barbara was on an area where there's a pronounced--since the coastline faces south, there's a pronounced transport from the west to the east, and the Santa Barbara harbor was constructed as an offshore breakwater to break the wave action and let ships enter and small craft anchor in their lee.

Well, this is fine, except the longshore transport of sand comes to the lee where there are no waves, so it deposits and drops out, so they had put yet another one there, and the final solution was to put a dredge at the entrance and dredge continually. This became a very expensive problem, but the erosion wave, which travels--it begins at the harbor and travels

downcoast, traveled about one mile a year. It took them about twelve years to figure this out, so there's twelve miles of erosion until they finally understood what they were to do, and started bypassing. The word is "call," but what they would simply do is dredge what's accumulated in the harbor and bypass it on down to where the wave regime can take it and transport it on downcoast.

RR: Besides Santa Barbara--well, I guess you weren't directly in on Santa Barbara in the beginning, because they did something you wouldn't have wanted them to do, but what are some of the major either coastal zoning or coastal planning activities that you've been engaged in?

Inman: Well, in the Los Angeles area, Santa Monica Bay, for example, is one of these situations that's very close to being totally armored. The first installation was the Santa Monica detached breakwater, which was placed off the Santa Monica pier, and the idea was that this would give them a quiet anchorage. Well, it did, but it also gave a deposition point for the sand, which they then had to pump. The situation I'm describing went right on downcoast, so the Santa Monica Bay and the beaches in Santa Monica Bay are not what one would call a natural situation. They're all armored. The Redondo harbor at the very end of them is a special harbor, using the submarine canyon as a deep-water entrance, which also shields the canyon from the sand that would otherwise get to it. So the whole situation now seems to be somewhat static, but it's only become that way because it's all armored.

RR: These things involve the scientist, then, as very much sort of part of a policy and a planning kind of process for communities. I wonder if you could comment on that, on how you view the role of the scientist in terms of those kinds of activities.

Inman: Well, I think the scientist's role, if he's interested in the natural environment, or interested in near-shore environments, his role is, and, in fact, responsibility, is to come in and inform people. I think early on, when oceanography was new, surprisingly enough, we were asked, and perhaps there was just a smaller group to ask from, but we were asked more frequently to come in and help plan.

We put out a number of planning--I know I helped put out something called "The Coast of San Diego," or "The Ocean Edge of San Diego," explaining what these problems were, and I think it helped immeasurably for a short period of time. I say "short period" because administrations change, people change, and the first thing you know, it's right back where it was, only it's much larger and harder to penetrate.

So I think where most coastal scientists feel that they have an obligation and would gladly help in any way they could, whether for free or not, rarely have the opportunity they would like to have because of the large bureaucratic structure and extensive federal, state, and city planning groups.

RR: So you felt some frustration in terms of seeing your work and its impact.

Inman: It's the same frustration I feel about the California Coastal Commission. The problem got very bad, so a number of us agitated to have the California Coastal Commission formed. We went up and talked at the state legislature and did all sorts of things to get it going. The idea originally in the California Coastal Commission is that it would be not just a regulatory body, but also a planning body. It would get ahead and plan what could be done and so forth.

The problem was that after it was formed, it became so involved with lot-by-lot decisions, up and down this thousand miles of coastline, that it couldn't possibly have any time to look ahead and plan properly. So its sheer size and the size of the problem defeated what we thought would initially be a sound approach to these problems.

RR: Have you had any that you feel were successful, of these kinds of activities, where there has been a serious recognition and incorporation of your assessments and ideas?

Inman: Oh, sure. We have lots of success, but compared to the number of the things going on, they seem somewhat puny.

RR: Let me switch gears to something else. We talked a little bit about Vietnam. I'm not going to pursue that, but there have been a number of other international kinds of scientific activities

that you've been involved in. I think I remember reading that in the 1960s and then again in the 1970s, you did a fair amount of coastal and near-shore work in both Egypt and in Israel. I wondered if you could comment on that work.

Inman: Oh, yes. I think that was certainly one that had some useful worldwide implications. I became interested in the Mediterranean and had begun work there, mostly in Italy. A student of mine here at Scripps was an Israeli, and when he went back to Israel, he saw some of the application that might be useful. So I was asked to go over and take a look at the problem, and at this time, this was in 1968, it was just after the Six-Day War when Israel had prevailed in the conflict with the neighboring Arab countries, and things were rather tight about visiting these two together. I was one of the people that had research contracts, in one case funded by ONR, to do some studies in Israel. I was one of the people who had contacts in both countries.

But to get back to the situation that first occurred, I was asked to look at the Bartowill [phonetic] Lagoon in then Israeli-occupied Sinai, before they gave it back to the Egyptians. The idea of the Israelis was to improve the fisheries so that the Arabs in the Sinai could live more adequately, by keeping the lagoons open. When they're closed, of course, then there's no fisheries available and so forth. I was working with the Israelis on that problem, and working under the Israeli oceanographic group, which was headed--

[Begin Tape 2, Side 1]

RR: I think we were talking about your work in Israel and Egypt.

Inman: Yes. I had had relatively little contact with Israel, per se, until I was asked to look at the Bartowill problem, following the Six-Day War, and I was most impressed. The one thing, I was under the wing of Johai Benun [phonetic], who was a most remarkable person. He was one of the seven heroes of Israel from the War of Independence in '48, and had been in the Navy, and actually saved Israel from the Egyptians because the cruiser *King Farouk* was on its way up when his small group encountered it, and all they had were small motor torpedo boats, and his motor torpedo boat wouldn't loosen its torpedo, so he dumped his crew well away and then took it in himself, just in time, and sank the *King Farouk.* So he was quite well known in Israel, so you might say, wherever we went, and I was with Johai, why, we were well received.

But in general, I was so impressed by the level of competence of the Israelis and what they were doing, their level of science. I had been initiated under ONR, initially funded some studies on waves along the Israeli coast, and we started a program of measuring waves and predicting wave transport along the coast, which took me there, off and on, for a number of years.

Subsequently, in the early seventies, I became involved in the Nile Delta erosion, and this was erosion caused by the Nile dam, the High Aswan Dam, rather, that took the normal sediment load that would have been placed on the Nile Delta and carried along the coast, placed it in the

dam, and so the coast eroded. Areas like Ras El Bar, that had been built right on the Nile Delta, were eroding back a block per year, so it was very serious erosion.

I remember visiting it one time and taking a picture from a lighthouse that was then right at the coast, and I came back several years later and took a picture of this lighthouse that's now several kilometers at sea. So there were very dramatic changes occurring.

So I was studying the coast of the Nile Delta and the Israeli coast, and it occurred to me that the sediment transport all along the Israeli coast is from the Nile Delta, and so you have a littoral cell that I've mentioned before that starts--the source is essentially the Nile River, which places the sediment on the Delta. Waves and currents take it from the Delta, along the coast, all the way up past Bartowill [phonetic] and up into Israel, and then it sinks in the canyon just near the Israeli-Lebanese border. So this is a littoral cell, and it ties these two areas together, and this has consequence later.

So I was working in both countries, and under considerable duress in terms of getting back and forth, because, after all, it's a ways over there, and I would want to do both things in the same trip. The Egyptians would not let us come in if we had an Israeli stamp on our passport, but the Israelis, of course, overcame that by giving us a spare leaf with their stamp, which we could remove when we went to Israel. But still, we had to fly to a neutral country and fly back again. There was considerable strain and stress about talking to one group versus the other group, so that it was a very tense situation, but very interesting in terms of its coastal problems.

This was going on over a period of years, and I was in a UNESCO-sponsored summer school in Erdamli [phonetic], Turkey, in 1979, when things came to a head. The background is, of course, that in '77, I believe it was, [Anwar] Sadat went to Israel, and that began to break this up, these two countries fighting back and forth. In '78 there were the Camp David Accords, and in '79, while I was over there in Turkey, they signed a peace agreement between Israel and Egypt, but this was a piece of paper. The people had just ceased fighting, there was still a lot of tension between these two countries as to what should happen.

While I was in Turkey, I received a radio telegram to please stop by the American Embassy in Tel Aviv on my way back, and I also received a separate one from Johai Benun and another one from Bob Abel [phonetic] in the U.S. So I stopped by on my way back from Turkey, and learned of this.

What was happening was that they would like to see some kind of interrelation between the Egyptians and the Israelis, and wondered if there wasn't some basis that people like myself, who were working in both countries, could come up with which would relate these people and let them work on a scientific level to help overcome some of the old political problems. Of course, I was very interested in doing this.

This began a series of, literally, shuttle diplomacy between Israel and Egypt, where you couldn't go directly, as I mentioned before, and back and forth from the U.S. to Israel. What the State Department had in mind was a type of AID program that would be a scientific program of studying the coastlines. My principal entry there was to tie these two together and say, "Look,

you're all in the same littoral cell. You can't make good sense out of the Israeli side without doing the Egyptian, and you don't know what happens with the Egyptian without studying the Israeli, so this is a good place to work together." So my concept of what has since been called the "Nile littoral cell" was an interesting turning point in science.

By shuttling back and forth between these countries, we arranged for a conference that would then, for the first time, unite Israeli and Egyptian scientists in one room, one place. So the question was, how to do this and where. The Israelis, of course, were not under the same pressure as the Egyptians, because the Egyptians had factors in their country that were still very anti-Israeli, and so this was to be done in extreme secrecy.

So I hit upon the idea of, why not have it at Scripps. So in April of 1980, after considerable back and forth, for the first time, in extreme secrecy, we had a meeting between Israeli and Egyptian scientists here at Scripps for a week together. The security was very tight here. It ranged all the way from local campus to San Diego police, to state, to federal. [Laughter]

Of course, it was in secrecy. An example is the fact that in one of the meetings, some silly American got up with his camera and took a picture of this group, and one of the Egyptians went into hysterics, and would not stop until we had taken that camera, opened it up, and exposed the film. This was the amount of their concern about the possibility of assassination if it was known that they were participants in this Israeli meeting. So you can see it was a very tense situation.

However, it was the beginning of a very useful series for us and for them, and we ran something called the Middle East Cooperative Study, and it went from the early eighties through, I think, for twelve or fifteen years, into the nineties. Initially, it was very difficult to get these groups together, although we kept trying. Mostly we could get them together in some foreign country, but rarely direct from Egypt to Israel. So this went on.

In fact, it was difficult to get them to publish together. I remember I wrote a paper that involved the Nile littoral cell, and literally brought in people from each side and made them co-authors in order to get them published together.

But the interesting thing now is that, in retrospect, in looking back at this, the most lasting ties between Israelis and Egyptians are in the scientific groups that have been working in these studies over the last twelve years--no, twenty years now.

RR: That's very, very interesting. I wonder to what extent it went beyond the science, in terms of Israelis and Egyptians getting together. It sounds like it's mostly in the sciences, or did it have wider ramifications?

Inman: It had wider ramifications. You can't, particularly in Egypt, meet with the scientists and not be involved with the administration and so forth, because Egyptian science is much more structured than Israeli or U.S., and the situation is such that you're almost always initially dealing

with someone at the top in Egypt, whereas in Israel, you could be dealing with whoever's involved in the research, as you would in this country.

RR: Did you view this as science, as part of the peace process, as in some ways trying to not only to bring scientists together, but to mitigate some of the problems?

Inman: Well, of course. I think that all of us like to rise to this challenge. Can we help? I think this is a case where we did. Unfortunately, there's very little known about it, because it was held under such intense secrecy in '80, so that it was not in the newspapers. And all these years later, why, then there's no record of it.

RR: So the conferences are no longer taking place, I assume.

Inman: Well, the conferences now are involved in Israelis visiting Egypt and Egyptians visiting Israelis. There's almost a free flow.

RR: Among the scientists?

Inman: Yes. And to some extent among the higher people in Egypt who are involved with supervising these scientific groups.

RR: Did it help to bring back the Egyptian fisheries, or what effect did it have on the coastal and fisheries kinds of issues?

Inman: Well, the interesting thing, of course, is that when peace was declared, Israel gave the Sinai back to the Egyptians, and now studies we'd started under Israel were interrupted and have to be taken over under Egypt. So there was some progress made, but I think our major progress was in understanding the overall coastal problems, which was good science, and in uniting these two countries, which was also a good thing.

RR: I'm going to shift gears to a last couple of questions or two. One of the things that came out in the interview here, but also last week at the conference, is that there was a lot of support--well, in the fifties and the sixties, ONR largely sort of supported blue-water oceanography, for the most part. And now in the nineties, we're hearing a lot about support of the littoral from ONR and the Navy. Why do you think the Navy's gotten so much more engaged in littoral in the nineties?

Inman: Well, I think it's quite apparent, really. After all, look at the Gulf War. What were the big problems? They were certainly not deep-blue-sea problems. They were problems in the coastal zone. Where were the mines? They were in shallow water in the coastal zone. Our ships

that ended up on mines, they were involved with shallow water. So you don't have to be a genius to begin to see that this is an important situation.

In the background of all this, during the Russian--and our activity in the Cold War, we weren't as concerned, nor were they, that one was going to make a landing on another country, so there was little amphibious thought there. The big situation there was their submarines versus our submarines, and this is clearly a deep-blue problem. So that we had reason to concentrate at that time on that type of problem.

Later, it's clear that most of the problems in the world are now going to come from perhaps some of the underdeveloped countries, and they have access mostly to weapons, but they're also a shallow-water-type thing, smaller country, and they don't, as a rule, or very unlikely, to have a fleet of submarines.

RR: So, in a sense, you guys are more in the limelight now again for ONR, and I assume that means more money and support, but does it manifest itself in other ways, besides just more money?

Inman: Well, it manifests itself, me personally, in a lot of satisfaction that over all these years we've made full circle from one of the most important things in World War II, right back to where it's now the most important thing in war, as well as civilian. I think the important thing, to me, is that most of man's egress into the oceans and possible interaction with oceans is along the

coastline, whether it's fisheries or recreation or harbors or what they are, they're coastal problems. Therefore, this field has always been important in terms of coastal engineering. Shortly after World War II, we took the wave data techniques generated in World War II and applied them to civilian uses all along the coast. That's been a very big plus, but it's very interesting to me, in terms of supporting agencies, to see that now it is much easier to get this support for research in coastal sciences than it was before, but not all that much.

We at Scripps, at least, when I formed the Center for Coastal Studies, have always had adequate support, and in one way or another, mostly through the Navy, but also civilian, and other government support, from the USGS [U.S. Geological Survey] to Bureau of Land Management, and so forth.

RR: Kind of as a last question, what haven't I asked you that, or what would you like to comment on, or are there other areas that I should have asked you about? Maybe that's the way to put it, in terms of your career or in terms of your work.

Inman: I think that the main thing that I would like to comment on is simply that we have made this full cycle from importance of coastal areas in military as well as civilian, back to the civilian and military importance, and that there is a general awareness now that never existed before, and that fortunately I at least could see this in advance. I directed many of my energies towards forming groups such as our Center for Coastal Studies here at Scripps, bringing in people from all

types of discipline to look at this interdisciplinary problem, so that we've always been in a good position to progress from there. So, looking back, I can have a feeling of great satisfaction that we have indeed accomplished a lot, it was worth the go, and it's been a lot of fun.

RR: Wonderful. It certainly sounds like it's been a great career. Well, thank you very much. This has been a lot of fun for me, too.

Inman: It's been fun recounting it.

RR: Thank you.

[End of interview]