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ORAL HISTORY DEPARTMENT
TEXAS A&M UNIVERSITY

INTERVIEWEE: Gustaf O. Arrhenius

INTERVIEWER: Robert A. Calvert

DATE: July 10, 1976

TIME: 3:00 PM

PLACE: Scripps Institution of Oceanography, La Jolla, California

RC: Start again with the Officers Candidate School. Explain why you were in Officers Candidate School from '40 to '42.

GA: Well, it was because I was performing my normal military service which is a one-and-a-half-year period in Sweden normally during the war. It became longer. I was in for about three years, and it seemed to me to make the best offer at that time. It proved most interesting to go to officer school. Particularly, I was interested in the coast artillary, which combined the physical interest of ballistics and mathematics and also the contact with the sea.

RC: The first contact with the sea, then, that you experienced was really from your home and not from the military.

GA: Oh, yes. I grew up on the ocean, on the beautiful Bay of the Baltic, which had very interesting natural features and, therefore, attracted my interest very early.

RC: What degrees did you receive from University of Stockholm prior to the Doctoral Degree in Science?

GA: The way you work it in Scandinavia: first you take your Bachelor's Degree; then, after that, work your way towards what corresponds more or less to a Doctor's Degree here, which is called Liesinthiot Degree. For a thesis in that connection, I worked on the problem I found fascinating, namely, studies of sedimentation in the past by using the type of deposit that occurs in the glacial strata around Scandinavia, where you can distinguish every single year; therefore, establish a chronology and study processes as they...on an absolute time basis.

RC: How do you distinguish every year? Does it fall in layers, you mean, like a tree reversed?

GA: Exactly like tree rings essentially, because in spring you get a flood with the melting. And that gives rise to a specific type of a more coarse grain deposit. And, in fall and winter, things slow down and the chemical character of the sediments is different. So, therefore, you get the kind of tree ring-like effect. And you can measure the thicknesses of these rings and therefore reconstruct exactly the different hydrologic conditions and meteorologic conditions during each year and also use the sequence as a timekeeper and measure things on an absolute time basis.

RC: Is that what began the Scandinavian's interest in deep-sea drilling?

GA: I wouldn't say so. No, the deposits of this kind were formed in the... around the polar ice that covered Scandinavia 10,000 to...or during the Ice Ages--there were several Ice Ages. It's the basis for establishing the whole chronology for the Ice Ages,and it was of an exponent interest in putting geological phenomenon in absolute quantitative basis. With that background, it became very interesting to go to the deep sea where one knew that sedimentation was much, much smaller. Instead of having a fraction of an inch for each year, the same thickness of sediment corresponded to many thousands years. So, you have a chance to extend much, much further back in time. That's what fascinated me first with deep-sea geology.

RC: When was your first expedition that began to explore the sea in these terms, in terms of deep-sea drilling?

GA: The first expeditions with that specific aim in mind were a series of preliminary test expeditions in the North Sea and Skagerrak that were taken by the Oceanographic Institute in Jetabori. I was lucky enough to participate in these and also in the final shakedown cruise to the Mediterranean, preceding the culmination of these efforts in the form of the Swedish deep-sea expedition with the "Albatross," 1947-1948. So the first major effort preceding that was then the Skagerrak Expedition to the Mediterranean, called the Skagerrak Expedition because the ship's name was "Skagerrak" in this case, in 1946.

RC: And then you were on the "Albatross" in '48?

GA: '47 to '48, fifteen months.

RC: Then, when you come back from the "Albatross," ~~then,~~ you work with the Swedish National Research Council in compiling your data?

GA: The National Research Council provided, essentially, the fellowship support and research grant for the working up of the material collected from the East Pacific during the Albatross Expedition, which I had been given the opportunity to use as a basis for my Doctor of Science thesis work.

RC: And when do you first make contact with Americans in terms of oceanographic work?

GA: It's hard to pinpoint the first time. It must have been in connection, probably, with the Skagerrak Expedition, the preliminaries leading to the Albatross Expedition. I developed correspondence with many people in this country who were interested in similar problems, particularly the people at Woods Hole and Lamont Geological Observatory (Maurice Ewing there), and several of the geologists in the country that were not necessarily associated with oceanography but who were fascinated by these new possibilities, and, finally and foremost, with the staff here at Scripps Institution of Oceanography (Roger Revelle and some of the other people working here).

RC: How did you manage to come to Scripps?

GA: It was an actual choice for what I thought would be maybe half a year or a year post doctoral work. After spending four years of working up the material from the East Pacific, which showed some very interesting, previously unknown features, it was fascinating enough so I wanted to at least devote a few more years to that problem. The logical place, the center in the world for Pacific oceanographic studies, was Scripps, so that was the obvious choice.

RC: Whom did you contact here?

GA: One of the things that I found most inspiring in my thesis work was the thesis by Roger Revelle that had just then come out--quite strongly delayed by the war. It worked on material collected by the American "Carnegie" Expedition much earlier, but the material had not been worked up until he started investigating it. His monograph on this is a very impressive piece of work, plus the fact that he was the man who inspired and controlled this place, at the time, led me right to him. Well, I didn't hear anything for...must have been half a year. I think I wrote to him around Christmas time, 1951. I spent the Christmas together with my wife's family in Copenhagen and at the War Institute, and Neils Borgn further encouraged me to take that step and to go for a post-doctor period here--he had also heard about Scripps. Sometime in May the following year I got a letter from a man whom I didn't know at that time but who, since then, has become a great friend of mine, John D. Isaacs. He explained to me in his letter, which was rather brief, that that day a whole pile of papers had fallen down from the director's desk (he was assistant to the director); and when he picked up the papers, he had found my letter among them and felt he should answer some of the letters and picked mine. And his answer was,"Yes, why don't you come?" That was all. I took that as an expression of the informality and directness of approach to science in the United States and embarked on the trip over.

RC: Obviously you stayed here. Why the change from research to professor? Was there any....?

GA: At the time when I came here, Scripps Institution of Oceanography was a kind of exceptional institution in the university and had very few faculty positions. They were very difficult to get. So Roger Revelle, who wanted to attract new scientists to the institution at a much higher rate than he could possibly get faculty positions for, used research staff positions for this purpose. And he offered such a position to me.

RC: And then you worked on research, and then from there you moved on into teaching. When?

GA: Well, actually there's no border between teaching and research here at Scripps. I was teaching then, in so far as we had contact with students around here. I'm doing as much research after the change in title that you refer to, which didn't mean anything much--in fact, just more of an administrative arrangement that you were called professor instead of researcher.

RC: What I'm interested in is what appears to be bio-geochemistry.

GA: Well, that thing stemmed from, partly, the fact that in much of my work in the ocean sediments, I had become to depend much on the record provided by the fossils of organisms. They provided a very rich, both chemical and paleontological, evidence to tell you where you were and what had happened in the past. That is a characteristic feature of deep-sea sediments: that they have large proportion of tiny skeletons of microscopic animals and plants. So, part of my geochemical work involves biological questions. When Roger Revelle kindly wanted to arrange for a professorship for me, he had to look for the best strategic way of doing it. And he had just succeeded in obtaining a million dollar grant from the Rockefeller Foundation for the rejuvenation of biology in Scripps. Here, he saw me as a biologist suddenly; and, in order to be able to incorporate this work I was doing in the general scheme of things, he therefore called the professorship Professor of Bio-geochemistry.

RC: When one reads the way you've approached work, it seems as if you begin as a generalist, and then you become a specialist, and then you broaden out to be a generalist again. Is that a fair description of your career?

GA: I'm afraid that's the case, yes.

RC: Do you prefer to be a generalist? Is that the way you conceive of the role of a scientist?

GA: I think a scientist must renew himself all the time. And to renew yourself, you have to approach problems, new problems, and see problems in a different perspective. You must learn something about other branches of science. There's a great danger that you spread yourself too thin. But the interesting thing is to learn new things, and I'm afraid if you get too specialized, then you might very well become an authority. But life will eventually become boring.

RC: Is oceanography specially suited to the sort of generalist-specialist-generalist approach as an academic discipline?

GA: Yes, it is. And I would say earth science is, in general, because there's no barrier between oceanography and ~~from~~ earth sciences, in general. There's no barrier between that and space science and the question of the laws that rule our universe or our solar system. So these diffused orders make a very wide field, but it also makes it necessary to learn a large number of things if you want to be able to work in it.

RC: Let me offer an explanation before I ask the question. Many American oceanographers, if that's fair to say, began as meteorologists and moved into oceanography through submarine warfare training, in one way, or through meteorological work in the Air Force, the other.

GA: Whom do you think of specifically?

RC: I'm thinking of people like Spilhaus, for example, who is only an American in terms of being naturalized, and those people who trained with the Air Force with him; or, in the other way, I'm thinking about people who moved out of oceanography and into space like yourself. Does there seem to be a blending together of atmosphere, space, and the ocean?

GA: That's certainly true, particularly for the people who deal with the physics of the ocean, the motion of ocean currents. There's no particular principal difference between the motion of the fluid that forms the atmosphere and the motion of the fluid that forms the ocean. If you can treat one, you have the tools for treating the other. So there's often a blend of atmospheric and oceanic sciences among these people: Rossby, Walter Munk, John Isaacs, you see, many of them.

RC: Is this what led you to turn your attention towards space? What I'm trying to find out is what specifically led you from the oceans to space?

GA: Oh, I'd always been interested in that, and again it came much from the work of,particularly back in the 1870's, one of my assistors who was an Arctic explorer but also very interested in the question of interaction of the earth with space—the question of how the earth had formed, the dust that it had excreted from, and what meteorites could tell us about the origin of the solar system. I was always fascinated by his work. In the deep-sea sediments, you also find traces of the influx of cosmic materials onto the earth in the form of fine cosmic dust that is not so diluted in the deep sea as it is on land. The work with that then led me on still further into these questions that I had already been interested in. We succeeded in attracting to Scripps one of the great authorities in this field, namely, Professor Harold Uri. Everybody around Harold Uri becomes enthusiastic about what he's doing. That further catalized my interest in this field.

RC: And so, you seem to think it was natural then to—if I may coin a word—sort of relate the sciences of the galaxy with the sciences of the ocean?

GA: Or I would say the science of the solar system with that of the ocean. The galaxy, as such, is a step I haven't taken yet.

RC: Then what is the Space Research Laboratory, because that implies galaxy to me?

GA: You can divide space into near space and the solar system, which is essentially our bound system, and then, as you say, the galaxy, which is a much larger concept, much more enormous. I've limited myself so far to the questions of our solar system, particularly because, with the techniques that I'm using and the material that I'm using, one has access essentially by observation mostly to the solar system. What was your question?

RC: What is the Space Research Laboratory?

GA: The Space Research Laboratory again was a convenience of the time. There were several people at the time on campus--the campus was growing now under Roger Revelle's direction--and we started to accumulate on purpose many physicists and chemists who were interested in not only the ocean, but the earth as a whole, and the solar system and the universe: people like Harold Uri, first; Professor James Arnold; the Burbiges, Jeffrey and Margaret Burbige; and after a while, Professor McIlvane, who has done a lot of work on the Van Allen belts and the radiation belts around the earth. It became practical, at least as a matter to try to pull all these things, all these people, together into some kind of organization that was very loose and perhaps more consisted --in a word--in the Space Research Institute, which then served as an organization to deal with the outside world, particularly the sponsoring agency that was NASA.

RC: What were your actions as director? That's what I'd like to find out. What kind of direction did you try to give to the Space Institute?

GA: Practically nothing. At that time, scientists were very individualistic, and there was no great attempt to force everybody to do the same thing. So, the idea was ~~that~~ for only one, Herb York, who was chancellor at the time, to create an umbrella for all of these people. And what I had to do was a minimum of integration of results, to present them to the space agency, to convince them that our campus was a worthy recipient of further funding and particularly student support.

RC: What is the Institute for the Sutdy of Matter?

GA: Well, that is yet another interesting creation in the early days of the expanded campus here. And, due to the arrival of the great solid state physicists, Professor Bent Mateas, who is one of the foremost pioneers in the field of material science in general and particularly super conductivity.... And he was one of our great catches. Again, he's a very inspiring and brilliant man who enthuses everybody around him. He established, again for the purpose of generating an umbrella for all activities in the field, an institute called the Institute for the Study of Matter. I felt that I would like to branch a little into that fundamental field to learn more about the atomic and molecular structure of the solids that I depended on in my work, so I joined in his effort in those early days.

RC: Your publications have concentrated on the geology, chemistry, and physics of outer space, interspersed with occasional articles on marine geochemistry, if I understand it correctly. Would you agree with that?

GA: And also material science.

RC: Material science, okay, right, I'd agree with that certainly. Now, is space, then, a key to oceans for you or is space by itself for you, in terms of research?

GA: I see it all as an interrelated system. The oceans are a very interesting special phenomenon generated on some of the planets, due to their separation from the primordial matter of water on the surface of the planet. And I like to understand the ocean, partly as it exists today and the processes it shows today, and partly as a recorder of the most immediate past history of the earth--that means the last, something like, 100 million years, the ~~very~~ most recent pages in the history of the earth. But space science has opened up for us the questions that refer to the very earliest history of the earth that are no longer recorded in any existing sediments because they've been wiped out. We can learn from what we see on other planets and in material that has not yet formed any planets. From that we can learn where the ocean originally came from and how it originally formed.

RC: What was the American Chemical Society PRF Award presented in honor of, in 1961?

GA: Oh, I guess it must have been in response, or in some recognition, of the work that started with the work of the Deep Sea Expedition and continued here at Scripps afterward, trying to clear up some of the questions of the chemical metabolism of the ocean and the relationship between the ocean sediments,or the use of the ocean sediments, as a record of the past history of the earth.

RC: Is it fair to say, then, that you treat both space and oceans as a frontier of time?

GA: How do you mean "frontier of time"?

RC: Well, the phrase I wrote down, when I looked at the things you had written about, was, in effect, trying to trace time or trace the impact of time through what amounted to places where time had not been disturbed.

GA: Or rather where the record has not been disturbed.

RC: Yes, I'm sorry, where the record has not been disturbed.

GA: Yes, that is true in a sense. What we've been trying to do in our group is to establish phenomena of functional time, the record of the past history.

RC: What are some of the things you have discovered, then, which have modified beliefs about, let's say, the origin of the oceans?

GA: Well, anything that we've done that has a bearing on that subject is most closely associated with a question that lies much further back in time—the question of how matter originally formed, how salted matter originally formed in the solar system. Those questions decide, essentially, where the water eventually came from that later on formed the ocean. So, in that field, it's very hard to make any really breakthroughs or major discoveries. You work with little details in the record that are very scanty and so limited that it's open for many different interpretations. What you can do is to essentially see how this record fits with the actual observations of reality today. The field is very full of speculation. There are many people very interested in the question of the earliest history of the solar system. What characterizes much of this work is that it's split up between people in different fields--chemists, physicists, geologists--who work kind of separately on these things; and each one of them has a theory for the origin of the solar system--one for chemists, another one for physicists, a third one for geologists. Strangely enough, there is little contact between them. It's very rare that these different theories are confronted with each other, and people find that where geologists thought some things were possible, the physical laws of how matter behaves in space prohibits that and negates such a theory. What I've been interested in essentially is to work together with, to try to join information of the type that I'm particularly interested in with, the information that comes from other fields. The collaboration that I've particularly treasured and learned much from is with the outstanding physicist Professor Hanz Anwen. Together we have tried to develop some--I wouldn't even call it theories--framework for the permitted range of interpretations of the past history of the solar system, on the basis of the boundaries of the limits that are imposed by physical and chemical laws and such things. That is essentially the contribution, this way as I see it, in that particular field.

RC: Exactly what is the framework around which you've sketched, let's say, the origin of the universe?

GA: That is a very interesting philosophical question. There again there are very different departures taken by different people through time. One is that, very commonly, you have a brilliant idea of some kind; you're fascinated by some specific phenomenon. And you say, "Wouldn't it be interesting to see if you can use that (a specific one) to explain things in the past?" I think what one finds generally is that human knowledge and human rationalization is very, very limited. And, if you base your approach to the history of the past on speculations without contact with reality, then, even if you are very smart, after a few steps, the chances that you will go wrong are very, very high. This is the basis of existentialism, essentially; and I am an existentialist. Therefore, I think that scientists, as well as other human beings, need very badly to be guided by reality. Reality, in this case, is the situation that you're interested in, the conditions in space in the past. And you're to ask yourself what are the processes going on in space today, what are the laws that govern the motion and state of matter in space today? And there, of course, a whole new...that we didn't know very much at all twenty years ago; all kinds of beliefs then prevailed that are completely obsolete today. People thought that the interstellar space was a void, near vacuum, whereas now one knows it's a thin medium that is strongly electrified like Northern Lights roughly -- full of a network of currents and magnetic fields. In the same way, the chemical ideas about interplanet or interstellar space in the olden days were one of how such a nothingness was in between clouds of gas. And these gases were believed to be behaviors like gases in the laboratory on earth that one is commonly used to. This is where the space exploration opened up a completely new experimental field and demonstrated how completely wrong people were. Instead of neutral gases that dominate the picture, you find that just very small degrees of ionization in these gases cause them to behave fundamently differently from the ordinary gas dynamics laws; and that's the law of plasma physics and the law of magnetohydrodynamics that govern behavior of matter in space. This is where I felt that collaboration with Professor Anwen was particularly informative for me, because he is one of the founders of the modern science of magnetohydrodynamics and one of the pioneers in understanding on how things actually behave in space, not in a way that is based on speculation, but in a way that's based on observation.

RC: Is U.S. spending enough money on it's space program?

GA: That is very hard to say. I think the space program consists and has consisted of two things: science and public relations in the international arena. For example, the lunar programs, the manned landing on the moon, were entirely.... I think the political justification for it were not the advancement of science. Science got a free ride there on the publicity stunt undertaken on a very grandiose scale by the United States to impress the world with what is possible with modern technology. In that program, science, in the beginning, was perhaps not thought of as the major purpose. It became quite well funded during the great era when this was playing an important role in the eyes of the world. The problem with such a background for scientific support is that, when the publicity interest has dropped--people's attention span is very short--then it tends also to be dropping interests in political support for scientific work in the field. And this is where, at the present time, there is a need to carry on a sustained exploration of space around us, not just for the publicity, or not only for philosophical reasons--I may express it in a more favorable way--but also for understanding of nature and the processes that ultimately have to do with the everyday life on earth.

RC: Do you judge NASA to be too mission-oriented?

GA: NASA has to be very mission-oriented because the great breakthroughs NASA has made are due to missions. So, not at all. No, I think that the lifeblood of modern space science is actual expeditions into space. They don't need to be manned, and some of the more exciting explorations will probably be undertaken in the near future by unmanned spacecrafts: their orbiting space station around the moon; the polar orbitor that is planned and hoped for; and imperatively, I would hope, the exploration of comets and asteroids, the small bodies in the solar system which hold the secret to the origin of the solar system, in contrast to the planets and the moon that can't give any information about that.

RC: Why do comets hold the secret to the solar system?

GA: Well, I think it is generally agreed on that the solar system was formed from gas and dust. What, perhaps, hasn't dawned on everybody yet is the state that this gas and dust was in and what specific laws it followed in that form, but the general understanding is there. When such gas and dust clump up together to form clumps of larger and larger size, they grow and eventually form bodies as large as planets. The gravitation is so strong and the forces become so strong in these bodies that they destroy their original record. The earth has destroyed totally the record of the structure and the composition of original material it was made from; it has remelted itself and is continuing to remelt its crust at least and spew out remelted rocks as volcanic eruptions. The same on any big planets like Venus, Mars is kind of halfway through that process. It is more youthful maybe than the earth, but all of the original material is certainly totally destroyed. Any time you land some little chunk of original material on such big bodies, just because it smashes into the surface so hard, it is destroyed unless you have an atmosphere like the earth that makes it possible for meteorites to have soft landings. So, the bodies, then, that haven't grown big enough to destroy this material are the small planets, the minor planets, the so-called astroids, that cruise around mostly in orbit, in space mostly between Mars and Jupiter. And also, the comets that have come from way out in the outer reaches of the solar system have come in close to the sun and go back out again. There is quite a lot of evidence that they consist of material that has been very little altered from its original state.

RC: What is information science, by the way?

GA: Information science is everything that has to do with modern computers, data, data processing.

RC: Okay. Well, what relationship does information science have to do with your work?

GA: It's only a tool in all modern science where you, or in much of modern science, are aided very much by computers and by the whole science that is behind computers. It's not only the everyday aspects of computers; it's a question of signal processing--the way you convey the information in an electro-mechanical sense and also in the widest sense of information between, by communication between, people.

RC: What have you tried to do as the associate director of the Institute for Pure and Applied Physical Sciences?

GA: That institute is essentially an outgrowth of the original Institute for the Study of Matter under Professor Mateas' direction. By merging with some other similar-oriented organizations essentially this Institute for Pure and Applied Physical Sciences,originally a creation by Professor Keith Bruckner (one of our great physicists on this campus and of international fame), joined together with the Institute for the Study of Matter and formed this new structure. What I've been doing there is partly to try to draw together the different types of research efforts that were going on in that field on this campus and to assist, essentially, in the effort by

Professor Mateas.

RC: When you say draw together the different kinds of research going on on this campus, do you mean draw together the different kinds of research on this campus and apply it to space?

GA: Well, not so much in space sciences. In this case, it is mostly in material science. It is a material science effort that has happened by that institute.

RC: Is it possible to, let's say, harness that enthusiasm that appeared for the original space program and turn it towards the oceans?

GA: How do you mean? In Washington?

RC: In Washington and with the general public.

GA: I don't see why one should. It's like saying you should.... Well, to turn the legitimate profound interest in one field of science into the equally legitimate and interesting work in another field of science.... There's no reason why one should be converted in another. They're both justified on their own and tie in with each other.

RC: There's only so much of a pool, let's say, of research money available to the academic world. Do you feel as if, as a man who operates in both fields--may I put you on the spot here--do you feel as if we should continue with space as a major priority or should the major priority become oceans or should we have no major priority?

GA: Oh, I think we are far from the point where the amount of funds going into science, fundamental and applied science, in this country are limited by the resources of this nation. To compare the magnitudes, the amount that went into the space program during its "hay days" was really costing money because of the tremendous engineering effort. That amount is less than people spend on smoking in the United States and less than is spent on cosmetics every year. It's nothing compared to the real Gross National Product, compared to what is spent on armament and weapons. It's a very, very small fraction. And that, yet, was the largest effort in science at that time. The other branches of science, oceanography and so on, are a small fraction of that again. So, this country is devoting a very small fraction of its gross national product to creation of new knowledge which is essential for the future of the nation.

RC: That leads me to another series of questions which I'd like you to make invidious comparisons, if you don't mind.

GA: I'm happy to make invidious comparisons.

RC: Vis-a-vis, let's say, your original home of Scandinavia, um, how would you rank American scientific efforts? Does one receive a better or a worse education in Scandinavia in terms of the sciences, as a professional?

GA: That's a very broad.... It's like saying are earthlings better or worse than Martians? I think it's easy to speak about the education you receive or scientific training you receive in Sweden or in Norway or in Denmark, because these are small homogeneous societies which correspond in size, in geographic size, to California, for example, in Sweden, and where you have, therefore, rather unified structures in education. In the United States, that's a continent! You have an enormous variety of standards and of institutions ranking from completely worthless to the most outstanding in the world. The great institutions in this country are unparalleled anywhere in the world.

RC: What about work and activity in, let's say, oceanography, first? Would you estimate that the United States is on par with, let's say, the oceanographic institutions of the other affluent countries in terms of science: let's say, Germany, the Soviet Union, and the Scandinavian countries?

GA: I would say in each of these groups, and particularly the wealthy nations that are so large that they can afford massive effort, Scandinavian countries don't belong in there. Their effort in the expensive fields must necessarily be limited and spotty, limited to certain periods of time when there's a giant effort like in the 1880's or in the 1940's during the great expeditions that were undertaken then. In the United States and in the USSR, Germany, France, there can be sustained efforts; and the sustained effort in the United States is undergoing a very strong change right now. In the USSR, it is continuing in a traditional way. In the USSR, everything is very structured. Their effort that has gone into oceanography has been very extensive. Their ships are just perfect, excellent ships. If not designed for the purpose, ships are selected for the purpose (that are very expensive to run, but serve the purpose very well) that make it possible for people to stay at sea for a very long time. The Russian ships are roaming all the oceans to the sea, and people stay out for very long times. In the United States, I would say, the effort is probably much more imaginative; but, from the point of your ship's facilities, one has depended on the Navy essentially during the initial era, during the '40's and '50's and '60's. And the ships have characteristically either been local donations by some millionaire who wanted to get rid of a yacht and, in the better cases, ships placed at disposal by the U.S. Navy but that have to be on call all the time, that could not be modified for their original use--tugs, rescue vessels, things like that. We also saw.... That has marred the efficiency of the U.S. Oceanographic Fleet. Then there's been a period during which ships, some of the ships, were designed actually for oceanographic purposes. But my regard for most of these designers is not very high. The results are not really vessels that are characterized by the utmost in modern engineering and skill but some of them rather amateurishly done and become rather rapidly outdated. Many of them have low speeds; and, therefore, you can't achieve so much in a given time. And also I think that the puritan aspect of the American society prevents the creation on board of comfortable quarters for the scientific crew and for the sailors and officers. It somehow goes against the grain that people should be comfortable onboard. In contrast, for example, on the Russian ship, people live in great, perhaps not in great luxury, but in great comfort. As a result of that, you can never keep people aboard an American ship for a long time. As a result, you have to fly them out to wherever, to change crew all the time, every six weeks or so. And this becomes very, very costly to exchange crews. And also to keep morale high onboard is very difficult because of the rather limited comfort, and that is deplorable in a way.

RC: How many oceanographic voyages have you made?

GA: Oh, I've lost track of them. Now days I go out there rarely, but the major expeditions lasting for some considerable time--half a dozen or so.

RC: Now, I think where we ended rather abruptly yesterday was that I was asking you to make some comparison, if you could, between Scripps when you came here and Scripps as it is now.

GA: Well, at that time, it was quite small. I think the number of faculty at the time was 15. It was all a tightly-knit group and under very direct collaboration and contact in research and thinking and development with Roger Revelle, who was the director at the time; whereas now, of course, it has grown tremendously. It is a huge organization, and there is no longer the closeness and concerted overall institute action that was possible at the time.

RC: Did you think there was a--that's a poor choice of words possibly--but did you think there was more leeway or more freedom in research then than there is now?

GA: Not due to institutional restrictions at all. No, complete freedom then, complete freedom now.

RC: I was not thinking so much in instututional restrictions as I was thinking in ability to sort of move from project to project, possibly.

GA: Well, that is always limited by the availability of funds. Probably there are more funds going into science research today than in those days; but science has become, first of all, much more expensive to do. Instrumentation and cost of doing everything has risen sky high. And, furthermore, the individual freedom in seeking research projects of fundamental interest has been very ~~much~~ strongly curtailed these days in the country, as a whole, than they were then.

RC: How's that?

GA: Well, I think it's a long story, that essentially the era in the '50's and the '60's was a great development of science in the United States, after the war. The country realized from the war experience what a fundamental role that knowledge has in making the nation strong. The sponsoring by ONR of science.... ONR sponsored essentially all oceanographic science in those days.

RC: So, I'm to take it then that you feel like ~~that~~ a decrease in funding by ONR has, in effect, limited one's individual research projects.

GA: Well, it's not that simple; but in those days, essentially, the nation saw fit ~~what~~ through ONR, for example, first, and later on as the National Science Foundation was founded a couple of years later, to dramatically increase the volume of scientific research in the country. Of course, that growth could not go on forever. And what happened was, of course.... In the very useful growth there is also a wild flora of less useful developments. And what made crises occur, essentially, crises in the attitude of the citizens of the country towards science and scientist, was when their illusions were broken, essentially during the conflict of the Vietnam War. And that is when the illusions that people have about ethical stands and the objectivity of scientists and, in addition, their responsibilities as human beings were seriously being questioned. People saw, particularly, that military support of scientific research was not entirely a blessing, that there were strings attatched, in some cases, even though fundamental research was often involved, and that the scientists essentially went where the money was and didn't necessarily follow what they were compelled to feel ethically. Essentially, that whole crisis then led to a strongly decreased confidence in that scientists can be trusted with the leadership of the country, that they were.... People thought of them before as some kind of omniscience perfected, idealistic human beings; and many of them didn't come out that way. Furthermore, obviously, growth must stop sometime; and, as a result, essentially there were some years of terrible turmoil where everything was reorganized. And as a result, it's very difficult to be creative in carrying out some of the scientific research. In the reorganization essentially that emerged from that.... The project research is what has risen. I can understand that: the individual scientist is no longer trusted with the judgment of what is good or bad for the nation. Some scientists are very good, and yet others are not; everybody is to carry the blame for the bad ones. Instead, then, the emphasis is on large projects where any single individual does not carry so much weight but which are also then becoming often less imaginative, tremendously burdened down with enormous bureaucratic apparatus. And, although therefore much, much more money goes into science now (that may be dollars) than in the 1950's or even the '60's, the effective input, I think, is much smaller today.

RC: You mean a combination of bureaucratic costs plus rising inflation.

GA: Rising inflation, bureaucratic costs, the loss of imaginative approach that you have when everything is molded into huge programs that are run from some kind of centralized bureaucracy. Some of these projects are very good and done in an imaginative way; others become characterized by tremendous inertia and preservation mainly of "business as usual." There is, of course, a whole spectrum of judgments possible, but I think that, on the average, is the correct situation.

RC: Do you think that the bringing together or the making of institutions to grow in size--that's very poorly put--but the increase in size of research institutions has also created this concept of one major project or two or three major projects with a carefully designed goal?

GA: I don't think so at all. For example, Scripps was already quite large when this development happened, and it has always been quite an anarchic institution of a number of different scientists with different ideas. During the time when Roger Revelle was director, it was all kept together by one spirit. Later they were carrying on on their own and mostly in quite a good fashion. But it could be done; it was quite possible to carry out, in that situation, individual research projects of different size, some quite small, one-man projects, others much larger, and many of them depending on the unique facilities here--the ships and other things. So, the institutional structure has not forced this reorganization. It is, essentially, the nation's way of thinking about what research is, what it's good for, and what scientists are, and what responsibilities they should be charged with. That is what has changed, and it's for the good and for the bad.

RC: Do you see an expanding non-goal-oriented funds for, let's say, space and oceanography in the near future?

GA: Well, in space they must be goal-oriented because to go out into space and work, you really need.... There project orientation is an absolute necessity. Everybody can't go out in his own space ship and play around. So, there profound organization has to characterize the whole thing. In oceanography, the only organization you need is the institutional one. The institution holds the available ships and all the facilities. It doesn't need any national, huge bureaucracy to do that. So, in space that must continue;

in oceanography you see more and more nationwide projects. Some of them are very useful because they bring together people in the same field, like the GEOSECS Program, for example, and the NORPAC Program; but in others it hampers the development--this occupation requirement that you must, for some reason, bring together intergeneous groups from all over the place.

RC: Do you see an increase or a decrease in international cooperation in oceanography?

GA: Oh, I think that international cooperation is probably gradually increasing, particularly through projects such as IPOD, the deep-sea drilling internationalized project. And there are several other ones, I think, where at least there's some resemblance to international collaboration. And in space, too, of course, there is some at least theatrical attention paid to the same thing, not, I think, in the truly scientific way but in some kind of public relations way.

RC: Do you feel as if the developing third world countries and their demands for 200 mile limits and their distrust of what amounts to attitudes and goals of more highly developed nations, do you feel as if that will begin to limit your research?

GA: Well, that's an interesting question. First of all, the third country nations have always feared,where Scripps has always played a great role in receiving scientists from these emerging nations and training them here or giving them additional training into what they've obtained elsewhere, so that they can go back and contribute to the growing organizations of their home country. Mexico, the People's Republic of China, Japan.... Many people have come here to learn and others have come here for collaboration. Then with regard to the regulations you mentioned, or the 200 mile limit, obviously that throws in another bureaucratic complication into the previously free roaming possibilities of roaming around free over the ocean.

But that's deplorable, I think. Any increased bureaucracy is just terrible. But I can also understand these countries. It is very clear to them that there's no clear-cut limit between scientific research and what this scientific research is used for. I've seen numerous examples of that. When, in particular, you've seen the nefarious interaction or secretive reaction of the U.S. in internal affairs of various countries, it's understandable that they're afraid that discoveries of resources in their territorial waters, for example, will bring them into the limelight as an interesting place, where certain things will not be allowed to happen, where, if they were less important in the past, they would be allowed to happen. So, I can understand why this paranoia is growing up. And the best thing we could do as a nation, it seems to me, is to mitigate the situation by increasing the trust and feeling of responsibility from the side of our nation, that these people feel. If they trust us, there will be no difficulty, or not so much difficulty, at least, in getting the necessary permit to operate in their waters or even to make permanent arrangements.

RC: Now, in terms of space, then -- away from the oceans and into space for a second -- do you see an increase in international cooperation occurring there?

GA: In space?

RC: In space.

GA: Certainly at the level of international discussions and conferences, mutual participation in research. But there is not much more that you could do beyond that because, again, the specialized nature of the expeditions. There could be USSR members of, say, a cosmonaut team; but there's less and less space research done with cosmonauts. It's done with unmanned probes so there is not much need, essentially, not much improvement that would be obtained by making hybrid instrumentation packages. In some cases it could be; and there is such collaboration, for example, between the German space agency and the U.S. space agency in the exploration of comets, for example, where the Germans have advanced far and the U.S. is less far advanced. And the possibility, then, for U.S. investigators to place instrumentation on the German space craft and vice versa has been quite fruitful. I don't know if it will increase much in that respect, with regard to the USSR. I think there is still quite a lot of mistrust between the nations. Also the interlocking, the close relationship between military intelligence, or military applications of space science, and the information you derive from space experiments, some space experiments, will add a touch of touchiness to those questions also in the future.

RC: Has there been, in your opinion, much uneasiness concerning the close relationship of the military and oceanography, and the military and space among scientists?

GA: Well, in space, first of all, that development took place so late that the country has drawn a lesson and kept rather watertight separation between the civilian applications of space science and the military applications and the intelligence applications. So, with regard to execution of the programs, there is no interaction at all. The justification of the civilian space program had some overtones of the attending military advantages, so, like I say, it was a pity that it wasn't possible to man a scientific research program without having that aspect contributing. But that's not so bad, it seems to me. Something like that could be helpful. In the case of the interaction of the Navy, for example, with the oceanographic program, I think that's always been a very, very constructive interaction. And the tradition in the Navy, not only in this country but in other countries, has always been one of the most research-minded individual organizations in research for its own sake. So, on the average, I don't think there have been particularly bad repercussions there. Of course, when one thinks about the thing in more subtle details, it has effects. One could always ask how is one's independent judgement of political-military issues colored by the fact that you receive, for example, all of your support from the military organization. I think subconsciously it is affecting many people directly so that their unbiased thinking is not so unbiased. That is, of course, the negative aspect. It must be said that the Navy Office of Naval Research takes the credit for beginning and carrying the only scientific rearmament in the civilian sense, in the fundamental scientific sense, of U.S. oceanography. In the present situation, the ONR has decreased its role substantially. It is now.... As you know, all the armed forces have been forced by legislation to support only mission-oriented.... We will prove that research that you do with support from the armed forces is clearly mission-oriented. And that's fine. They need it for security and defense and attack--if this is what's called defense, then what's attack?--specific information. And it seems to me that that's one of the purposes of the institutions of the country: to provide what the government legitimately needs. But it's important to keep that, I think, separate from scientific activities. And again at Scripps that has been done, in so far as all these mission-oriented activities are kept in a separate organization and separate from the campus. This was not done on an absolutely voluntary basis. Although the separation has almost been partially done, it was largely catalyzed by the student protest during the '60's.

RC: At the University of California in San Diego?

GA: Right.

RC: And I think that's when classified research stopped being done. Is that correct?

GA: Well, it stopped being done on campus. It's still being done to a quite large extent off campus.

RC: In these veins of political geography and political oceanography, do you feel uneasy ever about the facts that scientists serve as consultants for private enterprise and for military, as well as for the academic campus?

GA: No, I think, on the contrary, that consultation done by scientists is really one of the ways to have them keep in touch with the real world. Scientists, otherwise, tend very easily to become too theoretical or irresponsible. If you are allowed to build airplanes that never have to fly, then it's possible that the design is not so crucially thought about. If the airplane has to fly, then there are pressures on you to really solve problems in a realistic fashion.

RC: Okay, now, correct me if I'm summing this up incorrectly--that awful phrase again. You see Scripps Institution of Oceanography, then, you see their job as a combination of work in military, private enterprise, and private scientists as a, if you should like, public institution serving all three sectors of society.

GA: No, I think that Scripps, as it should,if it entirely does it, serves only the civilian and scientific purposes. Then, if the staff places their capabilities at the disposal of industry or to the government as consultants, they do that normally by.... You have to, by rule, take leave of absence or vacation while you do that or do it when you're not serving the university. So, that's very important, of course, that you don't deprive the taxpayers of the service that you have to give them and particularly since most people use procedes of consultation for their own personal income and more rarely for the benefit of the university.

RC: When you came here, Roger Revelle was the director. Is that correct?

GA: Yes.

RC: And he remained director until he actually went off to Harvard. He sort of worked two hats here for a while.

GA: That's true.

RC: Have you noticed any abrupt change between the direction of the institution under Revelle and then after his successor?

GA: No, no, everybody has tried to do the best they could in their own way, and each one of the directors have contributed very great things. For example, what gained Roger Revelle's greatness was particularly the tremendous inspiration that he gave to everybody and his ability to amalgamate and weld everybody together into a joint effort in fields of importance. In these efforts, perhaps he did not have time or so much understanding for the importance of the sophisticated scientific facilities. He had largely puritan instinct that science should be done in an inexpensive fashion, which is very commendable. But for many things in modern science, that's not so easy. Cyclotrons are hard to make out of old pipes and things like that. Director Nierenberg, for example, has done a tremendous job in two fields particularly that I can see.... Probably more, but the two that particularly stand out in my mind are his bringing Scripps into the computer era, into the proper handling of data. Previously, in the enthusiasm of doing things, lots of data were collected on shipboard and then forgotten. And the way they were handled and stored you could never retrieve them. Now, thanks to Professor Nierenberg, essentially, computers were installed onboard. All the information that we receive must which is so voluminous that it is bound to be lost unless you do something special be immediately retrievable. And that has been immensely advantageous. Also, the introduction of online computers in campus laboratories and on shipboard has been much stimulated by him. And also the creation in the institution of a very well organized analytical facility that has a wide range of sophisticated instrumentation that is difficult for each individual in his contract, or anything, to justify but which serves a whole community, including students, has been one of the really great developments that he's done. So each one has contributed in his own way.

RC: Alright. What sort of computers has he installed now?

GA: Medium size computers of various kinds that are onboard ships so that you can put in, first of all, all the navigation and you can relate the navigation and the position to the scientific data. That was something that, in old times, took many months to go through the ships' logs and find out where you were when you found such and such a thing exactly. Now it's all there right away.

RC: Had other institutions already utilized computers on shipboard?

GA: That was essentially the theme of the era and that happened simultaneously, I think, at other institutions. But I'm not sure that it has been so thoroughly carried out as here. But I wouldn't swear on it. I'd have to look at their facilities.

RC: Had other nations utilized computers on shipboard before the United States?

GA: Again, it came at the same time and particularly in France and Germany who have always been rather advanced in the field. The USSR has been lagging behind, but I think it is coming up again. But the U.S. has certainly been leading in this because of their leading role both in software and hardware development.

RC: It has been suggested in another interview that possibly the age of the great explorations of oceans is over. With the introduction of computers shipboard and so forth, more and more analyzing of data will have to be done back in labs and this sort of thing: that the old idea of the oceanographer at sea may be through. Would you...

GA: I don't think so, at all. No, I don't think so at all particularly with sophisticated ships. With large ships that are essentially floating laboratories, you can do more and more experimental work onboard. One of the pioneering efforts in that direction has been made by the great marine physiologist Professor Schlander, here at Scripps, who has demonstrated that the most difficult experiments can be carried out even on relatively small ships. He was the leading spirit in acquiring the ship "Alfa Helix," which is a floating laboratory, rather small, that goes all over the world then carries out, on the spot, particularly marine biochemical, biological investigations that could not be done back home because you need to look at things right then and there. If you pick up things and bring them back, it's just dead stuff. So that has set the fantastic example in that field. Also, in physical oceanography and geological aspects, there's quite a lot you can do on board. There, I think, the United States is somewhat lagging behind. That aspect is much more stressed by nations that have large ships that are comfortable and have laboratories that can operate even when the sea is a little rough, like the USSR. But, an exception from the general rule here is the deep-sea drilling. Deep-sea drilling is done from big platforms, and therefore they have good laboratories on board. Much of the important preliminary work is done on shipboard by scientists so that one knows when one comes back exactly what one has and one can do some planning for the next drill hole from what you get from the first, with some sophistication.

RC: What would you have judged to be, then, the more important developments that have occurred in oceanography over the last, let's say, since you began, in terms of analyzing data?

GA: Oh, as far as analyzing data goes, there are, of course, a large number of refined instruments that have been developed, very elegant instrumentation that permits much more accurate measurement. And for treatment of the data, there are obvious computer developments, but that is not the most important. I think the most important are ideas and completely new techniques that have opened up new realms for investigation. And there you have again deep-sea drilling that has permitted us to look at the ocean in the third dimension and that makes, I think, the statement wrong that exploration of the ocean is over. The first guess at what the very surface looks like of the ocean floor is, perhaps, done in many areas, not all; but it's opened up a complete new third dimension, the distribution in depth of all geological history of the ocean. And there is much left to do there. So that is one of the breakthroughs in new techniques that have opened up completely new vistas in the field.

Another very important development is the deep tow instrumentation, that is, the beast that this institution has developed, where it can look at the sea floor very closely, and, therefore, see detail and measure things that you could never even guess at when you tried to do it from the surface, as you did in the past. Other important tools, of course, are things like anchored buoys, big buoys that can collect and transmit data continually. That is a field where only the surface has been scratched. An, example of how the lack of measurements of many points at the same time limits our knowledge is the equatorial current system. There is, as you know, an undercurrent along the equator running under the surface about ~~fifty~~ 50 meters deep, which is fantastically swift. It's as fast as a gulf stream and carries a larger volume of water than the gulf stream. It is one of the most important flows of water in the world. That plays a crucial role for the whole productivity of the equator; that is a basis for the enormous fishes potential and has lots of ramifications. The whole mechanism, the dynamics of that system, is practically unknown and not very well understood, mainly due to the fact that there have never been observations at sufficient number of points at the same time that permit you to know where the thing is. Nobody knows if it's snaking through the ocean, if it separates into blobs, if it undulates like a snake up and down, what the periodicity is of these motions, and what are the dynamics of the whole system. To solve such a problem you have to have either something like a dozen ships operating at the same time or a large number of buoys or some similar things. So, that is just an example of how synoptic measurements carried out with measurements at a large number of points at the same time for a solution of some of the major problems of the ocean have yet to be done in the future.

RC: What do you see for your future?

GA: Oh, I'm an existentialist, as I told you, so I'm doing today what I see as the most important things; and I have very little idea where that will lead me tomorrow. But I'm almost dreaming of coming back sometime for a third take on the problems again associated with the very exciting equatorial circulation and its effects on sedimentation and on the record in the past, because it is one of the most spectacular global mechanisms where the pulse of the earth, essentially, is recorded and where you can get an integrated picture of what happened to the earth as a planet over a very long time and understand how the climatic mechanisms of the earth function. So that is, in oceanography, one of the most interesting things I look forward to. Another one that we are working on right now, which has somewhat of a social implication, is the mineral resources of the ocean and their importance for the world, the future world.

RC: That was something I wanted to ask you about, only I was going to ask you in terms of what you thought your major accomplishment was thus far. What is the mineral potential of the ocean? Would you hazard to guess?

GA: There are several different aspects that are very interesting to that question. Some phosphates, for example, deposited near a shore area are one of the major potential resources discussed today, which is at the focus of our interest. Also, from a scientific point of view, are the huge deposits of copper-nickle ore, the copper-nickle incorporated in the normally more innocuous manganese nodules in a specific narrow zone parallel to the equator, essentially, in the Pacific and perhaps also in the Indian Ocean: an ore that, in just one stroke, has doubled the world's small resources of copper and nickle. One thing that interests us much, too, is that associated--probably everybody realizes, from a resource point of view, that this is important.... The challenge to me lies in other associated uses of the deposits that these nodules lie in. They have very interesting characteristics. One of the things we have been playing with is to investigate or to develop an interesting type of ceramic from this sediment, which is extremely lightweight, porous, highly insulating, and relatively high in flexive strength so that it is an interesting building material for earthquake-proof buildings with a very high thermal insulation and energy conservation properties. I don't know if it will ever work out practically. You never know that with an early development, but it's a type of interesting secondary approach to new mineral resources on the seashore.

RC: Now, some sizable number of critics have maintained that the extraction of minerals from the ocean will be so expensive that this sort of resource is limited. I take it you disagree with them?

GA: Well, I wouldn't know because I'm not an economist; I'm not an engineer. And it's staggering for me to think of the difficulties in designing tools for retrieval of these ores at the necessary rate of something like a couple of million tons per year for each unit involved. But, I think the best testimony to the practical possibilities is that all of the major mining companies in the world, essentially, are involved in one of the major consortia that have been formed to explore these possibilities and that they, who are experts in the field of actual practical mining, judge the possibilities good enough so that they have invested many millions of dollars into this and are continuing to do it at an increased rate. I think, when I talked to these people, my friends among them, it is clear that the more conservative among them think that it might be touch and go, but that it's promising enough to pursue, for sure, at the present time.

RC: I take it, then, that you feel as if your career will now again--to use that awful word--drift back into the ocean rather than to float back out to space. Is that....

GA: No, I don't think so. It will remain in the ocean, as it always has been, without being limited to that. So I foresee to divide my time between problems in the ocean and the solar system as a whole and also in more fundamental questions in material science, which interests me very much.

RC: And exactly in terms of material science?

GA: Well, again, there I want to learn and understand better the fundamental properties of the materials that we study and use on the ocean floor and that we see forming in space. These are very complex processes in nature. And you have to look at the underlying, more simple processes in the laboratory, in order to understand them, and the theory that has developed from more simple things. This combination of three fields, I think, is to my mind an appealing and productive one.

RC: I just have one more question in terms of three fields. Do you think it will be possible in scientific training in the future for a scientist to switch or to move from field to field, let's say, as you've done, Roger Revelle has done, Isaacs has done? Do you think that will become less possible?

GA: I don't think so, no, if you acquire a good fundamental training. The characteristic of a good fundamental training is that it gives you the tools to both move in different fields of science and also to adapt to all the developments that take place in science. Unless you have such training and insight, then you are lost after 15 years, when developments have run away from you. You must keep in touch both in time and in space with the developments.

RC: So you see the role, then, continues as the scientist as generalist.

GA: As generalist with a broad base, sharpened to a point. Because it's dangerous.... If you are too general, then it's dangerous; you could become a dilettante. You have to have some things that you really are better in than anybody else.