AN INTERVIEW WITH DR. GEORGE AND BETTY SHOR

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Today is March 16th, 1995. This is David van Keuren. I'm at the home of George and Betty Shor in La Jolla, California. I'm talking with them about their reminiscences of the Mohole project.

van Keuren: When did you first come across this particular project?

George Shor: When did I first hear about the project?

van Keuren: When did you first hear of it?

George Shor: Probably within a few days after the suggestion was made, whenever that was. People were asking me for information about how deep the Moho [Mohorovicic discontinuity] was, where, with the idea of proposing the drilling. I wish I knew when that really was. It must have been sometime in the 50s -- '57, 56? Because I was intimately involved. I did my doctoral thesis up at Cal Tech mapping the Moho in Southern California, using refraction/reflection work, on a shoestring. I think Cal Tech gave me thirty dollars worth of supplies.

van Keuren: Who was your professor?

George Shor: Charlie Richter. Richter was sort of vaguely interested in what I was doing, but it sure wasn't his project. He wanted me to look at earthquake records to determine Moho depth. That's just a little too crude, not precise enough, and I was doing long refraction lines in California using quarry blasts as sources, and a little bit of reflection work, again using quarry blasts. I got two reflections from the Moho and a lot of refraction data and wrote a thesis. I used all the earthquake data that happened to be around and wrote a paper on crustal structure in Southern California and depths to the Moho.

Betty Shor: His dissertation was in 1953.

George Shor: That's what I was doing. Before I was back at Caltech for a Ph.D. I had been working for a geophysical contracting company -- Texas, New Mexico, and Louisiana -- mapping much shallower stuff. Not deep stuff. Twelve thousand feet. So, I knew pretty well how reflection seismics worked, and I knew from textbooks how to do refraction work. Turned out that nobody else knew very much more than I did, anyhow. I had been doing that. Russ Raitt had started doing refraction work at sea right after World War II, at the Marine Physical Laboratory. There was a meeting here at Scripps, an Institute of Geophysics meeting, in 1951. A whole bunch of graduate students from Caltech came on down to the meeting. We heard about it. They were talking about the refraction work that had begun at sea from here, among

a lot of other things. We came down for the talks. I met Russ Raitt, heard about what he'd been doing. Actually, I was invited to come along on the following year's sea trip, the 1952 CAPRICORN Expedition. Almost went, too. There was only one problem. They never mentioned they might pay me to do it. I thought about it long and hard too. Without realizing I might actually be paid. I finally decided I couldn't afford it. I was back in college on the GI bill, and had a wife and a child, and couldn't take off four months. I was fascinated by the work because it was the sort of work I had been trying to do on land, but obviously an unmapped, untouched area, and a lot easier methods of doing it.

I did a bit more talking with Russ Raitt, and in `53 I came down here on the staff of the Marine Physical Lab. I thought I was to be his assistant, but it turned out Russ never told me what to do. He said do what you want, and I started doing refraction work at sea. From there on, he and I were out, basically, as he said, ``Moho hunting.'' We were really doing refraction work all over the ocean, anywhere we could get the ship time to try to map the structure of the Pacific basin. It wasn't very long before we had a very good set of numbers on the layering in the sea floor, the depth, the sediment thickness, and found remarkably uniform layering in the ocean and the depth to mantle. These conclusively showed that the mantle was as shallow as you would predict from isostatic computations and not way down deep as many geologists kept believing, and some very well known

geophysicists were sure that the mantle was still deep under the oceans. Just that there was some difference in the density of the crust. So, it was shallow.

We started to map it and see if there was any real variation in it. I did the first refraction line on the continental shelf that showed that under the shelf the mantle was as deep as it was under the continent. It wasn't really a very abrupt change from continental thickness to oceanic crustal thickness.

van Keuren: The date for this project?

George Shor: That particular trip where I did my first continental shelf line off Guatemala was November or December of 1954. Off Champerico, Guatemala. From there on I did a lot of continental shelf and slope work, and Russ stayed pretty much with the deep sea work. Anyhow, we worked together; sometimes together at sea on the same ship, sometimes a two-ship operation on different ships. Sometimes alternating. Sometimes I'd go out and take a month or two of a long cruise, and then I'd get off and Russ would get on and do another stretch of it.

We were mapping. It became not just cross sections; it became statistics. What did we have at each depth? How much did it vary? How does it vary? How does it vary underneath the trenches, and the outer rises, and so on. That was the situation we were in when the idea of drilling came up. So he and I very promptly got involved in selecting a site. In fact, the Site

Selection Committee consisted of Harry Hess, Brackett Hersey, Russ Raitt, and myself.

van Keuren: We've jumped ahead to about `59 now. Right?

George Shor: Somewhere in there.

Betty Shor: I'm going to interject a comment. Everybody at Scripps knew each other very, very closely. It was a small community when we came here in `53, and it was still small in `59. Walter had started his Institute of Geophysics and Planetary Physics, I think, by that time. Walter Munk was very much involved in the origins of Mohole. O.k. His work was not associated at all with the Marine Physical Laboratory. He was also a geophysicist. George and Russ didn't work closely with Walter.

Betty Shor: So, when actually George first learned about the Mohole is hard to say.

George Shor: About the drilling. Pretty damn promptly though because, I say, after one decides, "well, we should propose to do it," now where should we do it?

van Keuren: Do you recall when you became a part of the Site Selection Committee. I can look that up in the records but ...

George Shor: I was trying to look back in my correspondence files a little while back, and I found a letter I sent back to the committee members at one point after I was out on the Western Geophysical site survey. I was on it from the beginning. There was never a change of personnel on that committee. We and the people of Lamont and Woods Hole were all competing to go find the best site. The final site chosen was the one I proposed.

van Keuren: The one off of Hawaii, off of Mauai. I get the impression from looking at the records at the National Academy that there was a lot of inter-institutional competition.

George Shor: Of course there was.

van Keuren: That Lamont wanted it in the Atlantic. That Woods Hole would have preferred it in the Atlantic or Caribbean. That Scripps was pushing for the Pacific. How did this work its way out?

George Shor: It didn't. In those days there was a great deal more institutional loyalty than there is now. Take a look at who is going out on whose ship when. NSF's system of allocating ship time and the pressure they gave that forced the creation of UNOLS has made everybody's ship ....

van Keuren: What was the last part?

George Shor: UNOLS, the University National Oceanographic Laboratory System, which I helped found. It was actually a defense against an attempt by a lady at NSF, Mary Johrde, to take total control of scheduling the American oceanographic ships. She wanted to set up the National Oceanographic Lab System, by which NSF would provide the ships but also schedule. Say who could use what ship when.

van Keuren: Her name again was?

George Shor: Mary Johrde. She was a good friend of mine, too, but I sure fought her on that one.

Betty Shor: Give a time period.

George Shor: Good lord. It was in the sixties. Before that time, hardly ever did a Scripps person set foot on a Lamont ship and vice versa. John Ewing at Lamont was a good friend of mine. John and I once at a meeting sat down and tried to work out how we could set up a two ship operation. At that time, refraction work at sea required two ships. Getting two ships together was difficult, and doing it in a remote area was darn near impossible, and Lamont would do refraction work usually with some Argentine ship or a Japanese ship, anything they could scrounge up and jury rig to do it, plus their own, the Vema. We were luckier. We could send two ships out at once if we could make the

schedules work. John and I were talking about Indian Ocean work and suddenly realized that we had a ship going to the Indian Ocean, and they had a ship going to the Indian Ocean in the same year and we cooked up the plan that we would have the Vema and the Argo, (maybe it was the Horizon,) meet in the Indian Ocean to do two-ship refraction work. We talked about it at the meeting, and I wrote up a little bit about what I thought we could do when, and then nothing much happened. Finally, I called John to find out what were the prospects of doing it. His answer was very simple: Doc says no.

Betty Shor: His older brother, Maurice.

George Shor: His older brother, Maurice. "Doc says no." I realized then all of these two ship operations that Lamont had done, it was the senior partner and the very junior partner, and he was not about to do anything that was going to be a shared operation. Actually, Lamont was sort of the last hold-out: "We work for Lamont. We don't work jointly with other people." Now, Lamont people can join things too, but in those days there was a very strong institutional rivalry and loyalty. There were the big four: Scripps, Lamont, Woods Hole, and Miami.

Betty Shor: Five.

George Shor: Who's the fifth?

Betty Shor: University of Washington.

George Shor: University of Washington? No. They were in another level, along with Texas A&M and others.

van Keuren: Was the University of Miami in the same league as Scripps, WHOI, and Lamont?

Betty Shor: No.

George Shor: It considered itself to be. It was somewhat accepted and was realized when JOIDES was started, the JOIDES original planning committee. That was the Scripps, Lamont, Woods Hole, Miami one. That was partly due to [Cesare] Emiliani and [Fritz] Koczy. Not due to their head, Walton Smith. But, Miami was trying hard and coming up fast.

Betty Shor: But Emiliani got the drilling ....

Geroge Shor: Emiliani accomplished a hell of a lot. As a matter of fact, I think that Emiliani's only problem was simply that he couldn't resist making puns and jokes. He set up an organization for long coring called LOCO. That was a very bad acronym. It would never fly in Congress. Cesare just does this. But anyhow, he has also written some wonderful phony papers which some people had trouble distinguishing from the serious ones.

Ok, let's get back to Mohole. As I say, Russ and I were doing the refraction work here. Maurice Ewing was doing a tremendous amount at Lamont, along with an infinite number of colleagues and students. I used to look at the papers as they came out. The first paper would be Ewing, and so on, and so on, and so on, and so on. And then Ewing's name would move on down the list of five authors till finally it was at the tag-end because his only participation was having started the project. But, there was refraction work going on from the Vema all the time, everywhere.

So I say, Russ and I managed to put in about four months of refraction work a year. That's a lot of ship time. At Woods Hole, Brackett Hersey, who had, of course, been one of Ewing's early colleagues from the Lehigh days and knew how to do it, and did it whenever he got a chance, but he was spending far more of his time on navy underwater sound work. I took a sabbatical one year and went back and spent the winter at Woods Hole working under Hersey in his lab there learning how to do his side of the classified sound work. Well, anyway. It was a small group.

At the time of the first International Oceanographic Congress in New York, which was 1959, we had in one hotel room talking almost everybody in the world who did refraction work at sea. We had the people from Lamont, Woods Hole, and here, and a couple of Russians and the Japanese. There they were. So, it was a very small fraternity, and we all were competing like hell. The only person who wasn't competing hard was Hersey because he was

really spending more of his time in other things.

Now the real intellectual leader of this, I would say, was Harry Hess from the word go. Of course, Harry came from a very neutral location. We used to joke about the fact that the rest of us all went off and gathered the data, and Harry wrote the important papers about it. He did. I was always spending my time getting logistics going for the next trip, processing data from the last trip, and Harry, as soon as he gobbled the data, would think about the implications of it and start writing papers about tectonics. The rest of us were like that too. I was a lot more so because I was very much more involved in the logistics of the work at sea. But Harry Hess, as far as I'm concerned, was the real central person in all of this. Harry Ladd came very close. He was interested in drilling in general, not just the Mohole. Hedberg was also .... Hedberg's connection with Princeton at the time I never understood totally. I think he was half-time Princeton and half-time Gulf Oil in Pittsburgh. They were very good people to present a case in the higher scientific level. They were all Princeton.

How did I get involved? I got involved because I knew how deep the Moho was where. I also had a reasonable idea of how inaccurate our data were. Geologists had a tendency to think that we knew things within a few percent. You know that Brown and Root assumed that they would know when they got to the Moho. That if we told them that it was so many meters deep that when they got there in another meter they would hit it. They

didn't understand the inaccuracies at all. I had to spend a lot of time telling people that when we say it is 5.32 kilometers down, that might be plus or minus a half kilometer. Refraction work is very imprecise stuff. It was then; it still is. The wavelengths you're dealing with are just not sharp enough.

van Keuren: Tell me about the Site Selection Committee.

George Shor: Ok.

van Keuren: How well did it work? You had people from all the different institutions working together, and they were ordinarily competitive.

George Shor: Well, basically, we went out and competed. We looked over existing data, all of our existing data, and tried to see a little bit of pattern in them and tried to figure out from that where we could find a site that met a bunch of criteria that weren't just geophysical. One thing which seemed sort of obvious to us was that the shallowest Moho depths we had found were on the outer rises outside of trenches. We came up with a little bit of theorization as to why, but this was a pattern. I don't know how many cases we had, probably three, but it did seem as if that might be a clue to finding an unusually shallow spot. All you really needed to do was find something that was a little shallower than this oceanic average.

We had a set of very conflicting requirements. First of all, it had to be typical crust and mantle. Then it had to be a whole bunch of things that were not typical. It had to be shallower than normal. It had to be within easy reach of a port for logistics. It had to be in an area of reasonably good weather. Didn't have to be perfect, but it had to be somewhere where there wasn't going to be a humungous storm come through while the ship was on station. So the trade winds were acceptable. What else did we have as a constraint? It couldn't be within the territorial waters of some unfriendly country.

Betty Shor: What about currents?

George Shor: Ok. I said it in 1962. The weather must not be too bad; your currents not too strong; where the discontinuity is shallow, and seismic velocity suggested average conditions, and high heat flow would also have been a problem. Ok. But otherwise I said don't drill on what became later known as a spreading center. Because actually the shallowest mantle we had yet measured at that time was under the East Pacific Rise, on the crest of the rise. My next best location, on the basis of existing data, was right off Ketchikan, Alaska. Not the world's greatest place to keep a ship for a year on station. Going by those same things, well, the Hawaiian Swell, which is not really the outer ridge outside a trench, has similar topography and might be the same sort of an effect. You might have a situation

where the sea floor is shallower there, and it might well be that the mantle was shallower too because it wasn't isostatically compensated. It was an elastic bowing down of the crust under the weight of the island arch, which indeed it turned out to be. So, the Hawaiian Arch was the one that I suggested we go out and look at, and we got a grant and went and did it. We made several repeat trips there. Somebody, I gather it was Lamont, came up with the Barracuda Ridge, which is down near Puerto Rico. I don't recall if there were any other spots that were really selected for detailed examination. We looked at the data for a whole lot of existing stations, but I think that those were the only two spots that got a detailed look-see.

Now, go back earlier. By the way, the experimental Mohole trips up here. Again, that was our data. The drilling down by Guadalupe Island. That was a spot where we had shot a refraction line not too long before, in an area where now it would be impossible to get permission to do it, of course. The Mexican economic zone, fishing area, and so on. But we, without permission from anybody, had gone through and shot some refraction lines and had found this fairly thin sediment layer and a nice hard reflector below it, and that would give a chance to go down and see what the "layer 2" of the sediments was really like. Of course, that was not typical. What was in there at Guadalupe is not standard oceanic crust. That was a lava flow and not too old a one. But that's all right. At least it gave a nice sharp reflection marker. Something to drill to, and they hit

within a very small error from what we said it was. It gave everybody a lot of confidence, both in the ship, in the drilling process, and in the geophysical data.

van Keuren: How important was the phase one of Guadalupe for later deep sea drilling? Both for Mohole and for DSDP?

George Shor: It proved out the maneuvering system. That's what was really vital there. Bascom, and Walter [Munk], and Ed Horton. There's another real Munk connection with Mohole, which was that his brother-in-law Ed, Judy's brother, was in the Bascom team, and he was the one who did the work on the maneuvering system.

van Keuren: That was Munk's brother-in-law.

Betty Shor: Edward Horton.

George Shor: So, I'm sure he [Munk] must have heard of the progress on the project rather regularly, about what was happening. Anyhow, that system, dynamic positioning, which was all the sort of thing that if one was in a think tank working up a way to do it, [was] what they might come up with. But it hadn't been done! The oil companies, which are very conservative, had done drilling on the continental shelf with taut wire, anchored rigs and taut wire moorings. Many in those days really thought on the basis of, well, let us make a platform with longer stilts,

things like that. It was a pretty drastic step to cut loose the tie to the bottom and leave that drill rig moving around up there when it might go off and bust the drill pipe. The dynamic positioning system was the real leap forward, and the fact that people had faith in it. After that, experimental Mohole was over. People believed in that system.

van Keuren: That's the major contribution?

George Shor: That's the major contribution.

van Keuren: When I talked to William Nierenberg this morning, in his opinion one of the great failures of the Mohole project, Phase 2, is that they put too much effort, too much money, into trying to create too new technologies. He thought that what they really needed to do was to do what the DSDP was comparatively successful in: in taking technologies off the shelf and letting the oil companies develop the technology rather than spending money to try to develop them. Do you agree with this?

George Shor: I think there were two reasons that Brown and Root Mohole failed. One reason is, yes, they were putting all their eggs in one basket. I'm not sure how much of what they were proposing to do was unproven. The biggest problem was Brown and Root because Brown and Root didn't understand what it was they were supposed to be trying to do. Honest to God, they didn't. I

would go off to meetings with Brown and Root down in Houston gritting my teeth and saying I'm going to cooperate with those guys and get this thing moving, and I would come home from those meetings swearing I'm never to go back there again. Those people are idiots.

van Keuren: This is very interesting. Why didn't they know what they were supposed to do? What was the problem?

Betty Shor: Politics.

George Shor: No, it wasn't politics. No. The reason they got the contract was politics, but the problem was they were incompetent. Just that simple. They were absolutely incompetent to do this kind of a project.

Betty Shor: They should never have been appointed.

George Shor: Their feeling was [hits table] 'we can go out and hire enough people to do this.' So they had to hurry up and put together a team.

Betty Shor: And they fired Bascom.

George Shor: They wouldn't even let the people who had worked thus far be hired as subcontractors or anything. They were under

the impression that it was to be a one-hole thing. They didn't realize that there was any interest in the sampling above the Mohole. They thought somebody would be able to recognize a chunk of rock when it came up the pipe; that they would know immediately when they hit the Moho, and that's when they're going to stop. They didn't realize .... Well, let's put it this way. I worked with oil company people both before and after that on surveys. I worked with oil industry and since then I ran an industrial associates program here, and they knew what the geophysics could do and what the purpose was. Brown and Root had never, never, as far as I know, been involved in hunting for and finding oil. They built hardware for ports and things like that. I don't know if they were ever in the oil industry. They didn't recognize the imprecision of the measurement, the need to feel your way and experiment as you go along. I just never understood that bunch. But anyhow, ...

van Keuren: Getting back to this, we were talking here about why the project went to Brown and Root.

George Shor: All I got was first hand, second hand, third hand, fourth hand stuff, but the story I always heard from everybody was that the political pressure on [Alan T.] Waterman was such they broke that man. He selected them and then retired. A distinguished scientist who sold out and then couldn't live with himself for having done it. If anybody came up with `Waterman

thought they were wonderful,' well, they didn't have a chance to ask him, did they?

Anyhow, it was infuriating to find out that I was working with people who didn't understand what the project was. Brown and Root's engineers were so far behind the people from Mobil and Shell and the other major oil companies that it was pitiful. Anyhow, that's not the point. I did not go to Washington to make presentations. I was not involved in the proposal writing. I was involved really in looking for sites for it. That was my end of it, and, I say, the work off Guadalupe was based on a line that I had shot the year before, or before that, and I went out and did other random lines looking for it and finally wound up settling down to survey the Hawaiian Arch, in which we got an awful lot of very good data that would have been useful for other purposes.

van Keuren: The information that you brought to bear in terms of site selection, was it a combination of looking at old readings that you had done for other purposes and readings you did specifically for the Mohole, or were most of the sites you were looking at done specifically for the Mohole?

George Shor: Most of them were there. Just going back to our own data, and testing data, but we were continually doing refraction on sea trips through that period. So, as far as I can recall, on every trip I had in the back of my mind what kind of a spot might be for the Mohole site. But, the only survey that we did that was

specifically a Mohole site was Hawaii, and that was because we had a couple of refraction stations out there that suggested that something may be reasonable. The first station I had that made me think that the Hawaiian Arch might look like a good spot was actually way west of the state of Hawaii. It was Gardner Pinnacles, and boy, you wouldn't base any project out of Gardner Pinnacles.

van Keuren: Why?

George Shor: There's no port! Nothing. It's half way between Hawaii and Midway. There's a little piece of rock that sticks out there. I don't know if anybody has ever stepped foot on it even. We had done a line of refraction stations there. Just as part of it we were doing a cross section all the way down from the Aleutians to Hawaii and then heading on in to Honolulu. As sort of a final major effort to use up the explosives, we did leap frog refraction lines, two ships, one after another, day and night, from what we considered the normal north Pacific crust across the Hawaiian Arch, the deep, and right up the slope going to the Gardner Pinnacles, and across the flat there. So, while it was not the kind of place one would pick for precise data because deep slopes make refraction data go to hell, it was still about as a good a spot as we could find to do a transition from deep ocean to an oceanic ridge.

Anyhow, that one did indeed show some shallowing of the Moho

on the bulge that was to the north of Gardner Pinnacles. It goes like this: A shallow lobe and up a little bit and then finally out in normal crust. It bowed up a little bit. So I figured, ok, let's look at the same relative structural position down closer to Honolulu. We had to go up the the north side, and there isn't a very prominent outer arch on the south side of the Hawaiian chain. There is too much other stuff, seamounts and so on down there. The north side is pretty uniform. So, we took the north side and decided to do it off of Maui. It kept us out of the way of shipping traffic and a few other things. We did a set of lines in there, between Maui and the big island of Hawaii, and that was very encouraging. So, we got together a rather major survey, and I put that one together with people from four institutions. Now there, there we were, but no one from Lamont or Woods Hole. Operation SHOW. People from Scripps, Hawaii, Oregon State. Four institutions. And Wisconsin. Wisconsin had the buoys.

van Keuren: It was called?

George Shor: SHOW.

van Keuren: SHOW.

George Shor: *S*cripps, *H*awaii, *O*regon State and *W*isconsin.

van Keuren: What were the dates for this? Do you recall?

George Shor: Nope. I'll have to take a look in old reports. Early 60s.

Betty Shor: Probably `61 because when Sandy was eleven, and I got a trip to Hawaii out of it.

George Shor: And the whole project was unravelling at that point. By the time we had the data worked up, there wasn't any project for it to be for. There's somewhere in that interval that Western Geohysical did a reflection survey across the Pacific.

Betty Shor: The HILO expedition was in 1962. That was to survey locations between California and Hawaii.

George Shor: But they also did a little bit more on the Hawaiian Arch then.

Betty Shor: SHOW was 1966. Early in `65 the Site Selection Committee endorsed that location, northeast of Maui, Hawaii, for the Mohole. The next year Scripps participated in the multi-institution SHOW exposition to carry out a detailed survey of the area.

George Shor: Where in all that did the Western Geophysical survey come in, which I went out on for a week? I think it was after SHOW. It might have been before. Western Geophysical was

contracted to spend a month there trying to do detail work on the sites. I found the letter that I sent after that down in my office this morning, but it doesn't give the dates. That was my part in all of this. Strictly on the sites, and the accuracy of the data, and what we might find.

van Keuren: Tell me about how the decision was reached between the Barracuda Ridge and the Hawaiian Arch. The Barracuda Ridge data came out of Lamont. You and your colleagues came up with information out of the Hawaiian Arch. How was the choice made?

George Shor: Let me see. Actually, Western Geophysical did the detailed surveys in both places. They went back to Barracuda Arch too.

Betty Shor: Nafe, from Lamont, was a member of your committee. Your committee was you, and Russ [Raitt], Brackett Hersey, and Jack Nafe, of Lamont. And Harry Hess, who was chairman. The second echelon of people at these institutions got along just fine with each other. Politely, the directors spoke to each other, but the directors had more feeling of competition than the people who were doing the work. Nafe was reasonable to work with, wasn't he?

George Shor: Reasonable, but he was a loyal Lamont person.

Betty Shor: He would have been out-voted. Two at Scripps and one from Woods Hole, and Harry Hess.

George Shor: Chuck Officer basically left Lamont because he cooperated too much.

van Keuren: Chuck who?

George Shor: Officer.

Van Keuren: OFFICER.

Betty Shor: Charles Officer.

van Keuren: So, a decision was made on the site selection committee and they out-voted Lamont.

George Shor: Not really. Not really. I think it merely became an obvious solution at the end. The Barracuda fault area was a very small, shallow area. There wasn't much room to maneuver there, to choose a detailed site, where the Hawaiian Arch was a very big area that was potentially shallow.

Betty Shor: So you couldn't miss?

George Shor: There was not much chance of happening to hit on

the wrong side of a fault or something like that. Basically, we had a big, fairly uniform, shallow spot. Now, Gerald Morris, of course, did his thesis on the delay time mapping to look at the fine scale variation in depth across there. There wasn't terribly much variation.

Betty Shor: Off shore of Hawaii.

George Shor: Where the Barracuda Fault was a rather small feature. I don't recall if there was ever an up-down vote of the Site Selection Committee. My recollection is that we, as a committee, recommended the Hawaiian Arch. The fact that it was U.S. waters might have made some difference, too. I think it was fairly obvious at that point. It wouldn't have made a tremendous difference because it wasn't that much shallower under Hawaii than the average oceanic depth, but they felt that every foot counted. It sure was nice predictable weather there. That's another thing. Ah, Atlantic hurricanes were another problem in the Barracuda Fault. There aren't any hurricanes out on the Hawaiian Arch; there are just trade winds. Nasty weather all the time. Unpleasant weather all the time, but never truly nasty. You know. So, the winds are twenty knots. They just keep blowing.

van Keuren: You can allow for that.

George Shor: You can allow for that. A place that is flat calm

with a hurricane coming through is much worse for the drill rig, as distinguished from a cruise ship, which can go away.

van Keuren: There was a big debate within the American Miscellaneous Society and within the people associated with Mohole about drilling: one ship versus two ships, drilling straight to the Mohole as opposed to sedimentary drilling. Hedberg was a great proponent of doing an intermediate step, which included a lot of coring for sediments. Do you know much about this controversy?

George Shor: Sure.

van Keuren: Can you tell me about it?

George Shor: I was not really on either side of that controversy. I was very frankly only interested in drilling to the mantle. Well, and the crust. I wanted to see what the oceanic layer was. The same as what we call the second layer. I thought I knew pretty well what it was, but I wasn't sure. I wasn't right, but that's alright. I guessed layer two alright. It was basalt flows with pillow salts. Layer three, I wasn't all that sure. I thought it was probably gabbro. Well, maybe. I wasn't really interested in the sediment coring. It struck me that a lot of people had taken a lot of cores in the oceans. So, what's new? Well, it turned out that the sediment coring was a very important

thing to do and showed up a lot of interesting things.

Hedberg was much more interested in the sediments than igneous rocks. I think there's the real difference. My feeling was we've seen all those things, they're up on land. Up-raised sediments. So, what's new? He wanted to see the history of the earth in the sediments, and so did a lot of the geologists. I think that the difference was between the geophysists who wanted to know what the stuff was down there that we'd never seen, we thought, on the surface, and the geologists who simply saw this as a chance to get a good complete sedimentary section in a lot of places in the ocean. Now, it turns out that tactically, yes, drilling the sediments first would have been a much better move.

Side 2

Betty Shor: There was not unanimity in the geological profession about drilling to the Mohorovicic Discontinuity. My father ....

George Shor: Oh, my argument with him was wonderful, yes.

Betty Shor: My father was a mining geologist, James A. Noble. He was a land mining geologist.

George Shor: He was what you call a metalliferous geologist. Somebody wanted to know "does that mean he carried bits of metal around in his pockets."

Betty Shor: He was at Caltech then. He had been with Homestake Mine prior to that for some years. Dad didn't see much point in drilling all the way down to the Moho. He didn't really care what was there.

George Shor: He used to tell me "I can take you to it on the surface. Why do you want to drill a hole to get it."

Betty Shor: He said, "How are you going to recognize it when you get there?" This is an awful lot of money to pour into one project that could be more usefully spent in other ways within geology. I think, to a certain extent, he represented some of the land geologists of the U.S. at that time. There wasn't much interest in marine geology by those people. They cut off their interest in the wave zone.

George Shor: The other part of it was he had more faith in what the geophysicists wrote than the geophysicists had. He told me if you want to see an example of the mantle I'll show you. As a matter of fact I've been there too. Duke Island, Alaska. You walk around on peridotite, and next to it are some gabbros. My vision of what the sequence is like I could see right there on Duke Island, which is one of a large number of ultramafic intrusions on the coast of Alaska. I set refraction lines all around the island, that's why I happened to set foot on it. His added feeling was the mantle is peridotite, all the textbooks say so.

Here it is, sticking out right there on the coast of Alaska.

Betty Shor: He may be right. He may have been right. He's no longer living.

George Shor: But anyhow. Sometimes when you sell an idea you sell it too well. You tell people here is the crustal structure, and we think this is thus and thus and thus, and they think you have the answer. Then you suddenly discover that they don't want to support any more research because you've already got the answer. My arguments with him were very unproductive because he thought we'd already solved the problem.

van Keuren: Do you think then there was a divergence of opinion between the geologists and the geophysicists, and the geophysicists were interested in the mantle, geologists were interested in the sedimentary cores?

George Shor: In very crude terms, yes. For instance, Bill Riedel was out as the chief scientist on the experimental Mohole because sediments were his thing. He was very much involved in the Deep Sea Drilling Project. I was involved only in the establishment of the project. The planning for it. Of getting things organized and up and going. I got out of there because I wasn't interested in looking at sediment cores. Riedel and Van Andel took over on that because they were the sediment types.

van Keuren: Very interesting.

George Shor: It was quite a while in the Deep Sea Drilling Project before the hard rock people and the geophysical logging people got back and involved. John Orcutt got into it in order to put seismometers down the drill holes. That's a useful thing -- a quiet place to put a seismometer. Anyhow, it turned out, of course, that the drilling problems in hard rock were much more difficult than anybody thought in the beginning. They still have problems in hard rock. They've been redrilling this one hole east of Galapagos now. How many times have they reentered it? I don't know. It's half a dozen at least. May make a few hundred meters more on a two-month drill leg, and they twist off the pipe, or they have some problem or another, or their time runs out, and they pull out and go away and do other things. The next time they're in the eastern Pacific they go back and do another chunk on it. At that rate, the Mohole would have taken them a decade. In the between time on ODP they work on better drill bits and better sampling methods. I don't know what their probabilities still are, or whether it's just a matter of the fact that you have to keep making discoveries to keep the project going, and therefore you can't spend a decade on one spot waiting for the great day when a piece of rock comes up.

van Keuren: Bill Bascom's group. His technical group separated under unhappy terms from Brown and Root in `62.

George Shor: That was a shotgun marriage in the first place.

van Keuren: If it had not separated, would it have made a difference? Was Bascom difficult to work with?

George Shor: I don't know that he was difficult to work with. I haven't tried to work with him particularly. Just a few minor things around the campus. I never heard that he was a difficult person to work with, and he sure put together a very loyal, hard working group there, which was very innovative, very productive.

van Keuren: Harry Hess said that he was a very difficult person to get along with in his papers. I came across that comment in a couple of sets of papers from the time. All agreed he was very competent but not an easy person.

George Shor: I'm not sure how well he would have worked out as a subcontractor to Brown and Root. If Brown and Root had been a subcontractor to him, it think it would have worked. They might have reorganized in the process.

Betty Shor: Somebody who was with him on the Capricorn Expedition told me recently that his nickname on Capricorn was Bad Bill. I was left with the impression that he was difficult to work with.

George Shor: I don't know. I've known him only as somebody who was .... He has been peripherally associated with Scripps ever since then. Sometimes as an adjunct professor, sometimes not. An excellent lecturer and a perfectly reasonable person to get along with, but, again, there's nothing really to fight about with him. Now, if he'd been well within the institution, and I was arguing with him regarding space or policy, it might be a different matter. I don't know.

van Keuren: Where did the project go wrong if you had to give me one or two or three major reasons? We've talked about the choice of Brown and Root as being a major shortcoming. Did that doom the project right there? Are there other things that went wrong or would have gone wrong? In your opinion.

George Shor: After the fact, I decided that was the kiss of death. When Texas politics got into it that was the end. I don't know. The real question at that point was who was in charge? Who was setting the goals? I'm not sure I ever really knew who, if anybody, was telling Brown and Root what they should be doing at that point. Who were the real project managers at that point? What have you been told?

van Keuren: I can tell you what's in the literature of the National Academy of Sciences. That was a major debate point between the AMSOC committee, particularly when it was headed by

Hedberg, and the NSF committee that was supposed to be running it, run by Benson. A point of major contention.

George Shor: Bill Benson did not make that kind of decision ever as NSF program manager. NSF program managers, in general, are reactive not active. A few of them are different. When Gordon Lill was .... Was Gordon Lill ever NSF or was he strictly ONR? Doesn't matter. Gordon Lill and Art Maxwell made decisions.

van Keuren: He was ONR.

George Shor: ONR had a different way of doing things. Bill Benson was a very good program manager. People came in with unsolicited proposals which he got properly reviewed, and he made some decisions on his own hook. The occasional NSF program officer could. If he had confidence in a proposal and it had got mediocre reviews, he could fund it. But, he was not a leader of science.

van Keuren: Yet, of them all, he was the most qualified. Of the NSF team. He's the only one who had a science background and knew what was going on, as I understand it. Do you see it this way?

George Shor: I don't remember who the others were. I have the feeling there was nobody in NSF that was .... There was no scientist in charge of the project. Brown and Root had been given an engineering task to do in their minds, and they were going

ahead and do that engineering task as they understood it. We on the committees were there to advise them. They didn't have to take our advice, and they didn't.

van Keuren: In 1963 Gordon Lill was brought back by NSF to be a scientific manager. Did it make a difference at all, or was it too late?

George Shor: I don't know. I was doing other things by that time anyway.

Betty Shor: Benson was very unhappy about the whole thing, but I only learned that from him later, and I don't know the details. Is he still around?

van Keuren: I don't know.

Betty Shor: He was very unhappy with the way the whole thing developed, but I don't know why. Actually, I think the death of that Texas congressman and the foisting of Johnson into the presidency, wasn't it about that time?

George Shor: Kennedy was shot later.

van Keuren: Kennedy was shot in 1963.

Betty Shor: See, you're right in the middle of that. Vice President Johnson had a certain amount to do with putting Brown and Root in there.

George Shor: That was the story. He never told us that, but on the other hand Brown and Root turned out to have been almost his entire financial backer in all of his political ....

Betty Shor: Who was the senator from Texas?

George Shor: It was a congressman from Texas who was chairman of the Appropriations Committee.

van Keuren: He came from Houston. I am forgetting his name, but he was the big supporter.

Geore Shor: He died, and when he died ....

van Keuren: `65 wasn't it? Or '66.

George Shor: You have it in your book [speaking to Betty Shor]. No. You have it in the article about Mohole. You have his name in there. But when he died, the rest of the committee just deleted it. It was a pork barrel item in their opinion. It was pork. Who died before the Super Collider? Was there any similar thing about the Super Collider that there was some Texas strong supporter? I

wonder whether if there was an election or a death that affected that or not?

Betty Shor: Schism within the geological/geophysical community made it possible for the whole thing to collapse once Brown and Root came in. There were geologists who didn't like that, geophysicists who didn't like it, and it took away a lot of the total enthusiasm and support. What is this big company in Texas that's never done anything like this? It made it possible for the geologists to say, ``Let's not bother.''

George Shor: Most Americans hate oil companies. Most geologists like oil companies. The oil companies have good research labs. You have people in the oil companies that you can talk to.

Betty Shor: Creighton Burk was not a good choice, was he?

George Shor: Nope.

Betty Shor: He became the AMSOC scientific officer in '62. I'm sorry, I'd have to reread this. It's been years since I looked at it.

George Shor: Well, Creighton Burk was good, but he was not a senior, widely known scientist. He was very bright. I guess he was like three years after his [Ph.D.] degree. He was not like

one of these people like Hess, and Hedberg, and so on who was known all over the country.

van Keuren: He was a Hedberg student, wasn't he, Creighton Burk? Didn't he come from Princeton?

George Shor: Came from Princeton. I know that.

Betty Shor: Then completing his Ph.D. at Princeton in 1962.

George Shor: I know he was very much involved with the Washington scene while he was a grad student, but still he didn't have the clout. You know what happened to Creighton Burk at the end? Creighton Burk became the head of the Institute of Geophysics at the University of Texas after Maurice Ewing, and not very long after he took over that job he died of a -- what do you call it when a blood vessel in your brain breaks?

van Keuren: An aneurism.

George Shor: Aneurism. He didn't die of it. It happened to him when he was riding in a car, and he just went into a coma, and he stayed in the hospital completely out of it until he died. Just all of a sudden this young guy going `poof' -- had a blow out. That was a real shock to everybody because he was really a very energetic, very bright young man.

Betty Shor: Of course, Waterman stepped down as Director of NSF and Haworth came in. I'm sure Waterman left because of Mohole.

George Shor: That's what everybody thought.

Betty Shor: But, I'm still trying to find out the ....

George Shor: The congressman mixed in there somewhere.

van Keuren: The consequences of the failure of Mohole ....

Betty Shor: Albert Thomas, Chairman Albert Thomas, who had championed the project, had died February 1966.

van Keuren: What were the consequences for the failure of Mohole? Did it affect federal funding for geophysics/geology, marine geology? Did it have any influence on deep sea drilling? What do you see as the after effects, if any?

George Shor: It provided a lot of good lessons to the people who were putting the drilling project together, that's for sure. We tried very hard to avoid the problems of Mohole. Now, with respect to when Mohole ended, when was the launching of Glomar Challenger?

Betty Shor: '66. [William A.] Nierenberg was already here [as

director of Scripps].

George Shor: The timing was pretty close. There was an overlap there.

Betty Shor: I have a parallel in this article because there was an overlap, so I have a double ending to the article.

George Shor: But we definitely avoided some of the traps.

Betty Shor: Emiliani started out with Global Marine Submarex and drilled ....

George Shor: That was Emiliani on his own. Basically, none of the rest of us were involved on that. He caught the Submarex on a transit from one coast to the other, and he managed to get a quicky grant and do it. But the Caldrill.

Betty Shor: The Caldrill was 1965, and that was the first JOIDES drilling project. Lamont did it April to May of 1965, one year before the Mohole appropriation faltered in the House of Representatives. So, I tied it together there. The Glomar Challenger was built for JOIDES in 1968 and promptly began work for the Deep Sea Drilling Project. Most of the dates are in here [referring to her article from which she has been reading.]

George Shor: I got a phone call from a friend up in San Francisco, I guess with Chevron, about the Caldrill, who wondered if we might have any use for time on the Caldrill because it was going around from the West coast to Nova Scotia. I got on the phone real fast and put together that project, and Lamont volunteered to manage it. This was a matter of ....

van Keuren: You created that, did you?

George Shor: I was the person who got the phone call and relayed the news. I got lots of phone calls in those days, but the fact was I knew a little bit about the Caldrill, and they knew my name. So, this fed into the system real fast. I don't know how much lead time we had there. A month or two, that's all. We couldn't do that nowadays, but we managed to put it together and basically pick up the use of the ship for the Blake Plateau drilling.

Betty Shor: For very little cost.

George Shor: Yes, because they were willing to get it to there and get it on, and the only cost would be time there. You'd have to have a long transit, which was hellish expensive. So, they started to put it together in a real hurry. The Caldrill was not the ship for a deep sea drilling project, but we had to hunt very quickly for an interesting place to drill that was somewhere

along their route that was within the length of their drill pipe. The Blake Plateau was a perfect fit. But anyhow, in putting together the drilling project, and Nierenberg was quite right about this, we avoided any reliance on technology that did not already exist. It was to be 'You do this, and then you work on improvement, and you do that.' That's all.

Betty Shor: You had the global positioning though, which was the key. That had already been proven.

George Shor: Well, dynamic positioning. Yeah.

Betty Shor: Dynamic positioning.

George Shor: But we did not have the global positioning system. We didn't have decent navigation yet, at that point. But he was right. There was nothing in the early part of deep sea drilling that we didn't already know existed and had been tested. So, cruise leg number one was a success, and we went on from there.

van Keuren: Do you know much about the organization of the LOCO project, the Long Cores?

George Shor: I was on that committee, too. Emiliani came up with the LOCO, and we had some very nice parties in Miami [laughing]. He was really quite serious about the whole thing, but as I say

he could never resist making a joke.

Betty Shor: Did you skim over this? There is a certain amount of description of LOCO in here, but you can take that. I think George gave you a copy.

George Shor: The real problem with [Emiliani] Cesare was that people just wouldn't take him seriously.

van Keuren: That hurt his funding with NSF and hurt his chances of cooperating with the big three?

Betty Shor: The big three didn't really take him seriously. They did not consider the University of Miami a significant oceanographic institution. He pushed his way through and accomplished something, and they had to take him seriously.

George Shor: Anyhow, I used to go down to LOCO meetings in Miami with Cesare, and we tried to work on how to put together a program, but it just didn't seem to fly. Besides which, at that point, we had not set up the political infrastructure to make sure that nobody would make an end-run, and end-runs were made.

van Keuren: What do you mean an end-run?

George Shor: Submit a proposal to NSF to do the project. There

was the one Maurice Ewing put in, and there was the one Roger Revelle put in. Ewing did his, and so Revelle got us together and we put together a Scripps one.

Betty Shor: But Emiliani was the first.

George Shor: Yup.

Betty Shor: And he got funded by NSF.

George Shor: For a small scale project. We sensed he was going on testing out drilling in deep water. What we had to do, to the point of being obvious, was get Scripps, Lamont, Woods Hole, and Miami together with an iron clad agreement that would say nobody can make an end-run. That was the original steering committee, which was Fritz Koczy, Brackett Hersey, once again, and me and Chuck Officer.

Betty Shor: Charles Officer.

George Shor: Officer was the Lamont representative. We sat down and we hammered out an agreement. It was sort of like negotiating foreign affairs, like putting together a NATO agreement for Yugoslavia. A concession here, a change there, but what we were really all trying to do was tie up the hands of our bosses, and that is why there are two important committees for the drilling

project. First in JOIDES and now in JOI. There is the executive committee, and there is the planning committee. The planning committee makes the plans, and they submit them to the executive committee for approval. The only thing the executive committee can do is veto them or approve them. Yes. No. Up. Down. Vote. That's all. If they veto it, it goes back to the planning committee. But, the executive committee does not plan and make decisions; they merely approve what the planning committee has done. The people on the planning committee level were able to work together. No way could the four directors reach an agreement on anything. What time of day it was. So, the four of us reached the agreement and presented it to our bosses, and they approved. Then Officer went off to Dartmouth. I think that Ewing probably gave him hell for selling out Lamont. I don't know these things, but that was my impression. He did not have a very comfortable situation after that. That was the whole point of it. That was Revelle, not Nierenberg, that we were dealing with. Revelle was also inclined to go ahead and put in the Scripps proposal. Why should we share this?

van Keuren: Why this efflorescence of interest in sedimentary drilling in this short period of time? What caused it?

George Shor: The fact that it might be possible. Remember that people have been taking cores from oceanographic ships for a century.

Betty Shor: Short ones.

George Shor: No. Taking cores. Yes. Kullenburg managed to take thirty footers and so on, and that was great and wonderful. Lamont used Kullenburg corers all over the oceans to try to learn the older history. He's a Swede. The Kullenburg corer was the great leap forward after World War II. The piston corer. It was a way you could get a core ten meters long, or maybe longer. Well, Lamont managed to get hundred footers finally just by making it bigger, and heavier, and fancier. That was about as much as you could do with a corer dropped in there and not rotated. Yet, we all knew that rotary drilling could go much farther. Industry had been doing it for years on land. I have a friend out here in the Sossento Valley who still sells a gadget that you hang over the side of the ship, a little tripod and a little drill, but the idea of there being unlimited depth in the core meant that instead of looking at the most recent sediments you could look at a major part of the sedimentary section.

Betty Shor: I don't know, but do you think the fact that, for example, Raitt had found that the depth of sediments was much shallower than expected in the ocean aroused the interest of the geologists as to why, so that a longer core over a longer period ....

George Shor: No, because before that point there were still

people like [Phillip] Kuenin. Kuenin thought there were seven thousand meters of sediment in the oceans, and that this was the way one could find out the entire history of the earth. "The undisturbed sediments that had fallen to the bottom of the ocean from time immemorial".

Betty Shor: [Referring to her paper.] It is Berg. Kullenberg. Sorry.

George Shor: Betty and I argue about things like that. Every time a geologist gets into oceanography what they want to do is take cores. Some of them want to use dredges to take rock samples. Although you can only do that in a few places.

Betty Shor: Geologists have always been interested in the history of the earth and the techniques for learning the whole history.

George Shor: Yes, basically, taking cores at sea made it possible to get ``an undisturbed record.'' Well, it turned out it wasn't undisturbed, and that's all right. As time went on they found more interesting, more things to do with coring. That is what geological oceanographers do.

Betty Shor: And the field was growing.

George Shor: And the technology was obviously steadily improving. But this was a great breakthrough: that you could do it with rotary drilling from a ship. Now, there were other attempts from previous years. Mr. Piggott invented his core to use explosives, in the bottom, in the 1930's, to get longer cores.

Betty Shor: It's an approach to history of the earth.

George Shor: Yes. Basically, the assumption was that you'll find the history of the earth under the oceans, where you just find scrambled bits and pieces of it on land. That was indeed correct. It wasn't quite as simple as it seemed at the time.

van Keuren: You are arguing a confluence of available technology and intellectual interests.

George Shor: A continuing interest that had always been there, encouraged by small incremental improvements in the technology. Then all of a sudden the big improvement was in sight that you could use it and do it with rotary drilling from a ship, and there was no limit on how deep you could go. You could get the entire history of the earth.

van Keuren: What was this big jump that you are talking about?

George Shor: Well, dynamic positioning so that a ship with a drill pipe could ....

van Keuren: So it was the dynamic positioning that suddenly showed people that, well we can drill at sea. It wasn't so much the drilling that was done on the CUSS I, but rather the being able to hold the ship in position so you could do it.

George Shor: You could stay there long enough to get through two hundred meters, or whatever it was, of sediment, which we couldn't do any other way.

van Keuren: So, it really was phase one of Mohole that got the profession interested in doing this.

Other comments? What should I be asking you that I'm not?

George Shor: I don't know.

Betty Shor: It was an interesting time.

George Shor: Anyway, to start right back at the beginning, I would strongly suspect that everybody who was involved in geophysical work at sea, all dozen or so of us, must have, at some time or another, have thought: wouldn't it be nice if we could drill a hole and see what's really there? -- because none of us trusted our own data that precisely. If you work in the oil

company, you are always impressed with the point that the real test of how good you are, in your interpretation, comes from when the hole is drilled. I would go back and ask people from oil companies I've been contracted to, "Did they ever drill that prospect, and if so, what did they find?" Sooner or later the hole would be drilled and you would find out.

Betty Shor: Even on the experimental Mohole when they got down to the `second layer,' when they brought a piece of shiny basalt on board, they said, "There! That's what the second layer looks like.'' They were pretty sure what to expect, but until they had it in hand they didn't know for certain.

George Shor: I was suspicious even then.

Betty Shor: Who was the Life photographer who stole a piece of it? Then he decided he shouldn't, and he put it back, and the chief scientist gave him a piece of it.

van Keuren: Bascom says that was Steinbeck.

Betty Shor: That's right, that's right. I was thinking of the photographer. It was Steinbeck. Steinbeck stole a piece, and then he decided he shouldn't, and so he gave it back, and then he was given a piece of it anyway. Yeah. They had this rock in front of them and said, "There. That's the second layer."

George Shor: Well, we had quite an argument earlier, of course, about whether layer two of the refraction data was older sediments or volcanics. My friend, Ed Hamilton, published quite a few papers proving it was older sediments. He was wrong, but the proof was pretty good.

van Keuren: Ok. Any final comments.

George Shor: No.

van Keuren: Thank you very much for your time and for the very interesting discussion.