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Interviewee: Focht, John A.
Interview Date: October 6, 2001

OFFSHORE ENERGY CENTER

ORAL HISTORY PROJECT

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Date: October 6, 2001

Place: Houston, Texas

Interviewer: Tyler Priest

Side A

TP: I thought we would start off today by having you give us your background, where you are from, your education, and how you got into the industry.

JAF: O.K. I am a Texan by birth, and grew up in Austin. I got my BS in Civil Engineering at the University of Texas in 1994. I served two years in the Army; then went back to graduate school at Harvard on the GI Bill. I was there from 1946 to 1947 and got a Master's Degree. At that time, Harvard was the premier school for soil mechanics and foundation engineering.

TP: How did you get interested in that area of study?

JAF: Just by virtue of the exposure in senior classes at Texas. I had a professor there who had brought an outstanding engineer - his name was Karl Terzaghi - to lecture at Texas several times, and that got me interested. From Harvard, I went to work for the U. S. Army Corps of Engineers at the Waterways Experiment Station in Vicksburg, Mississippi. In that position, I was involved in the design of foundations for flood control structures all along the Mississippi River.

One of my major significant projects was the design of pile foundations for the combined Morganza Floodway Control structure, which is a major structure on the river north of Baton Rouge. In 1953, I joined Greer and McClelland here in Houston. They were involved in soil and foundation investigations throughout the Texas Gulf Coast, and had already been the pioneer in beginning offshore investigations of foundation conditions at locations where a platform was to be constructed.

Two of the other honorees can tell you a little bit more about those very first few years. I quickly became involved in the analytical, design, and engineering aspects, particularly the design of the long piles to support the structures being built in the Gulf of Mexico. That was much of my effort for a number of years, but I also became involved with mobile jack-up rigs and evaluation of their foundation stability as they were being installed.

TP: I know jack-up rigs had a lot of trouble in the mid to late 1950s. There was a lot of capsizing.

JAF: There were no foundation failures. The problems with jack-up rigs were all related to other aspects, and therewere no foundation failures.

TP: . . . at the water level or . . .

JAF: Yes, or something really going wrong from an operational standpoint.

TP: What did you think of this industry as you got into it? It was sort of new and exotic. Did you think, "Wow, can this really fly?"

JAF: No. I never really thought about it from that standpoint because the progress was gradual. We began in water depths that were out to 100 feet. Pretty soon, it was 200 and then 400 and then 1,000. You didn't really notice that it was extreme, except that I recognized very quickly that what we had done was to take onshore foundation practice and just extend it on to a slightly different, more hostile environment.

So, in the beginning, the technology flow was from onshore to offshore. But as we got on to the very large and very long piles, then that information began to feed from offshore back to onshore practice. In the Gulf of Mexico, the soils, to a very large extent, are relatively soft clay materials. The predominance of early oil production activity was off of the mouth of the Mississippi River where the Mississippi River Delta materials had been brought down to the Gulf of Mexico for a very long time.

TP: So, what were the main challenges that you had in extending this technology from onshore to offshore? Were they that great?

JAF: They were reasonable extensions, but then there was always an uncertainty when you would go beyond the empirical database. Actually, as a result, we encountered a variety of things that began to create changes from what had been done onshore. Ultimately, that led to modifications of the general technical criteria that we were using. I should mention that there are three of us from McClelland Engineers who are being honored today, and each one of us had a slightly different role.

Bram McClelland was the owner and principal executive of the firm, and was responsible for overall operations. Bob Perkins was the one who developed most of the innovations in terms of ways of investigating the foundation conditions that exist at a proposed site location. My contribution was more that of developing the analytical and predictive techniques for the design of pile foundations, mobile rig foundations, and later gravity structures. Some of the procedures that we documented for inclusion in our reports to offshore clients were criteria of how to predict pile capacity, how to predict performance of laterally loaded piles, other aspects of foundation performance. Those criteria were ultimately adopted by the API.

TP: In their recommended practice . . .

JAF: RP2A criteria. The work that we did in the late 1950s and 1960s and on up into the 1970s led to the criteria that are now being utilized.

There is one story I might tell that came to mind as it relates to gravity structures. The first major one of these was to be the Ekofisk Tank for Phillips in the North Sea. It was to be built by Doris for water about 270 feet deep. Phillips had already built a number of pile supported platforms, but they wanted to be able to store the oil as it was being produced, and then transfer it later to tankers to be transported into shore. This structure was to be a concrete tank about 300 feet in diameter and 300 feet high. Its exterior wall was perforated in order to absorb the energy of the waves. This tank concept was developed by Doris of France.

We were working for Phillips in helping review the foundation design and reach a conclusion that what had been proposed would be satisfactory. The foundation conditions there are different than they are in the Gulf of Mexico.

TP: A bolder clay?

JAF: No. They actually are very dense sand. As we went along with the design, the final point that became of major concern was potential liquefaction of the sand under the dynamic loads of the waves, in a manner not unlike that which happens occasionally in an earthquake. When an earthquake shakes sand and the sand can, under certain conditions of density and earthquake intensity, just turn to a liquid, and anything supported on it just slumps down into it. The Norwegian Government became concerned and called for a conference to be held in Oslo with the Norwegian Geotechnical Institute, a semi-governmental geotechnical agency, who was their consultant.

Since I really didn't know anything about earthquakes, I called on a good friend by the name of Kenneth Lee who was at UCLA and was one of the top three earthquake geotechnical experts in the U.S. He happened to be working in New York that summer. I made a deal with him to go with me to Oslo. So, I met him in New York and we got on a 747 with seats together in the lounge upstairs. We worked on the problem all night flying over. What we were doing was exchanging what he knew about liquefaction and what I knew about the soil conditions as they existed. As a result of that all night effort going over, we reached the judgement that there was no problem. That turned out to be not quite adequate for the Norwegians. While they did not put a hold on the progress of the structure, they went ahead and assigned NGI the problem of evaluating it further with laboratory tests and a whole lot of sophisticated analyses. About two years later, Lauritz Blerrum, who was the Director of NGI, was in a meeting in Canada and met with Ralph Peck who, then and now, was the recognized Geotechnical Expert in the U.S., and who was aware of the problem because I had discussed it with him. Ralph just kind of asked Lauritz, "How did the problem on the Ekofisk tank come out?" And Bierrum's comment was, "Well, after a couple of years, we figured that John Focht and Ken Lee were right." So, a lot of times what we were doing was largely based on judgment and intuition, but I think we did a pretty good job.

TP: Well, that's good. I know a lot of the pile supported platforms in the area had subsidence problems, but I guess that was largely due to the oil . . .

JAF: That was not anything to do with the structures themselves. I think there's one other interesting aspect of how things progressed that really came out of the Ekofisk complex. That was the first development over in the Norwegian waters. The oceanographers came up with a "design storm". Well, the first three winters all produced design storms greater than that design storm and the previous storms.

TP: That happened a lot in the 1960s.

JAF: This is a matter really of the learning and the development went all the way through, I think, all of the offshore.

TP: How closely did you work with oceanographers and meteorologists who were studying design, wave height, or with geologists and geophysicists? Because I know from my work on Shell, there was increasingly greater interaction between the exploration people and the production people as you were facing larger and larger challenges.

JAF: Most of the time, there was not very much of a direct interaction with anybody except the geologists. With the geologists, there was much closer one there; but not with the oil company geologists, more with the folks who were aware of the near surface geology, not the petroleum structure. So, for example, the geologic understanding of the Mississippi River Delta increased very substantially by our interaction with Harold Fisk who was at LSU and later was a consultant with Shell.

It turned out that some of the engineering properties that we were able to measure helped define the geologic classification of various portions of the overall delta. For example, one of the problems that came up as they moved into the main delta area itself is that there is a thickness of extremely soft clay, anywhere from a few feet thick to well over 100 feet thick, that actually has been deposited so recently that it is not yet in equilibrium under its own weight. And there was one of the major storms . . .

TP: Was this Hurricane Camille? Because I know Shell lost a couple of platforms.

JAF: They lost several structures in the South Pass area because of the instability of that near surface material. Well, that hazard had not originally been a part of any analysis of the structure stability. Very soon, bottom instability became a part of the overall evaluation of the region, not only at the site of the structure, but uphill of the structure because that material might flow and move downhill on to a structure. our interaction with geologists using some of our data, I think, helped the geologists better understand the formations of the delta.

TP: I was curious about this story because it was a big deal. I think Shell measured an increase in water depth of 40 or 50 feet in some places after that.

JAF: There were marked changes. So, as a result, bottom stability is now a problem to be studied not only for structures but also for pipelines. There probably is not a simple solution; you simply have to deal with what is there.

TP: Did you work with Robert Bea from Shell?

JAF: I did not personally get involved in the bottom stability questions, but Bob Bea was one of the leaders on this problem.

Let me go back to Ekofisk for just a minute. A few months ago, I wrote a paper with a subtitle, "You Can't Do It Alone." You always are dealing with others, either with the oil company or other consultants, that add immensely to the solution to the problems. One of the people with Phillips that I have to give a lot of the credit is Bill Bowles. He was out of their office up in Oklahoma. He and others like Bob Bea, Pat Dunn of Shell, and an immense number of other fellows with the various oil companies who contributed very significantly to my understanding of the geotechnical aspects of structures offshore.

TP: We are finding that in every area. The industry has to move out together, at least in terms of their production technology, and it has really become a fraternity of interaction. That is one of the unique things about this industry, I think.

JAF: Then you slip over and pick up a few others, and then there are some that come in from the construction side: Griff Lee with McDermott and Jay Weidler over at Brown & Root. There was another fellow who had Jay's job just before he did and can't recall his name. So, as a geotechnical or as a soils engineer, there was always interaction with, in essence, the civil engineer and the structural engineer for the oil company, the civil engineer and the structural engineer for the structural designer, and the contractor, with us and with the geologists. So that interaction played a very predominant part.

TP: On your bio here, I have some information about you developing the criteria for the axial load design on long cylindrical piles. Is there anyway you can explain what this was?

JAF: The object of this process is to take the results of laboratory tests on samples of soil that are recovered from the site or else in situ tests and convert that data to predictions of the skin friction on a long pile. One in situ test that has proven to be quite valuable is kind of a vane device - a four-bladed vane on a shaft, and you insert it into the soil below the bottom of a boring. Then, with an appropriate mechanism, you have to latch to the side of the bore hole and you twist the vane until it actually causes the soil to fail, and then you can determine from the force that was mobilized on that cylindrical surface and convert it into units of pounds per square foot, which is the soil strength. The design criteria consists of taking those strength measurements and converting them into the design adhesion or skinfriction on the side of the pile. Early on, mobilized it all the way down at the full value.

We later got results from some tests that were done on behest of the oil companies at a site near Venice, Louisiana, and we began to find that there were indications that, on very long piles, you did not mobilize that maximum strength at each location simultaneously all the way down the pile. So, we had to make some sort of a reduction in the measured shear strength to convert it to the mobilized adhesion at the time of failure. In addition, there had been experience onshore that if you happened to be in very stiff clay right near the ground surface, you did not mobilize all of that strength. So, it becomes a question of where do you begin to mobilize the full strength.

But the offshore foundation soils are not all clays. So, you must consider the other predominant type - sand. Then, it's a question of the density of the sand, its grain size, and the hardness of the grains. On the grain size, there is a concern as you move into the intermediate zone between clay on one hand and sand on the other, called "silt," which may still be a very fine material, but does not contain much cohesion, or stickiness, but it's not granular enough to act like a sand. In that zone, there still are not really good criteria. But, fortunately, there are not many of those materials offshore.

TP: Is it much different when you get out to the Continental Slope off the edge of the Shelf. Are the soils much different? Does the incline. . . I guess it's still fairly gradual.

JAF: It's still is not steep from the standpoint of the pile support for the structure. The problem is you get further out onto the slope is the slope instability itself, and the mud slides.

Now, one other thing that we have not touched on at all is when a platform is subjected to a storm. There are the lateral loads that have to be transmitted from the structure into the foundation. So, it's a question then of mobilizing all you can at the surface, but that's not enough. So, as you go on down, you develop rather substantial bending stresses in the pile, so the pile has to be designed structurally to resist those stresses that occur when a pile is laterally loaded.

TP: I know that in the North Sea, there were all sorts of new metal fatigue issues when the industry first went there.

JAF: Right. But this one is not really just a fatigue problem; this is actually an actual strength problem. There was a test program that was conducted in the mid-50s by Texas A & M for the California Company that created the initial work for the prediction of the response of piles to lateral loads. Bram McClelland and I wrote a paper on that and came up with a concept that has subsequently been called the "P/Y Criteria" and is a technique that is used for predicting the performance of laterally loaded piles.

TP: The "P/Y" stands for . . .

JAF: "PY" . . . "P" means the pressure on the pile in pounds per inch of pile length, and "Y" is the deflection of the pile in inches. So, it just means it's a load-deflection curve. There was very substantial work done later by Lymon Reese and Hudson Matlock at the University of Texas. We were usually involved in a peripheral way on those research projects but all of them precipitated from the original work that we did in the mid-50s.

I guess one of the other interesting things to me is that we, like so many others who started working for the oil companies in the Gulf of Mexico went on their coattails all around the world.

One of the first places, I think, was the North Sea, then in addition, down to Venezuela in South America. Most of the time, the person who went was Bob Perkins, or one of our other field people. Then I was involved in the design or the interpretation of that data. Frequently, it was in conjunction with Brown & Root of McDermott, or other constructors. Of course, then they began to move and opened offices in London. Before long, we had one there and then in Singapore, and in Saudi Arabia to support offshore geotechnical activity in those parts of the world.

TP: Did you do any work on the Cook Inlet structures up in Alaska?

JAF: Yes, we were in Alaska. It introduced a new problem. In the Gulf of Mexico, personnel were always evacuated from the platform at the time of the maximum lateral and vertical loads. Early on, we had recommended, for that condition, you could use a factor safety of one-and-a-half instead of two which is more conventionally done onshore. But, when they went to the Cook Inlet, the personnel would not be evacuated when maximum loads would occur during ice loading. Should the foundation factor of safety be increased?

The oil companies really didn't want to switch to a higher factor of safety, and told us that was their choice and not ours. I would have frankly thought we ought to have used a slightly larger factor of safety, but there was enough conservatism in what we had done because, again, there were no failures and no problems. So, their optimism turned out to be appropriate.

TP: Those big tidal surges in the Cook Inlet and all the boulders on the sea floor . . .

JAF: They vary. We had had experience with exploration in large tidal variations in doing work for the Corps of Engineers up in Passamaquoddy Bay on the Canadian border because, early on, they were thinking about building dams across some of those inlets where there were high tidal surges as a mechanism for producing power. So, Bob Perkins came up with ways of being able to drill in a location where there was fairly significant tidal variation; the tidal ranges there are in the order of 15 feet or more. And how you do that with a fixed drill bit down in the bottom of the hole because you don't wantyour drill bit going up and down as you are drilling.

Bob came up with some clever techniques for doing that, so we were ready when we had to go to Cook Inlet.

TP: Are there any other memorable projects that you worked on in some of these other countries, or in the Gulf of Mexico, that posed unusual problems?

JAF: The other thing that we began to encounter was the problem of calcareous sands. In the Gulf of Mexico, we think of sands as being hard grain silica sand. There are sands in some other parts of the world that are calcareous, mostly fragments of shells and small organisms. Those fragments are relatively soft. Information came to light that the skin friction in those materials was markedly less than in silica sands, even though the piles might actually drive relatively hard.

The calcareous sands became real evident off of the Northwest Coast of Australia. While we were not directly involved in it, I thinksome of our early findings pointed out the problems and we have been involved in the evaluation of those kinds of materials in terms of their skin friction, particularly with respect to pullout loads more than compressive loads because you still have end bearing for compression. So, if a failure were to occur, it would probably be due to a tensile load as a result of overturning due to the lateral forces on the structure.

TP: How about West Africa? Are the soils very similar to the Gulf of Mexico?

JAF: Generally similar - either the soft clays or the slightly over-consolidated clays. There are some sands, but not of a major problem. The problems there are really more generally typical of those of the Gulf of Mexico. The same way with Venezuela - those are fundamentally soft clays and not markedly different than in the Gulf of Mexico.

TP: Are there other problems that occur once you move into the deeper waters of the Gulf of Mexico with the big platforms, say Cognac or Bullwinkle, these giant structures?

JAF: In some respects, not. In some respects, yes. It becomes much more difficult to get a high quality sample of soil because, as you bring the sample of soil up from say 4,000 or 5,000 feet, the pressures on it are released. If there is any dissolved gas in the water within the sample, it tends to come out of solution and expand, and can significantly affect the strength of the specimen so that there is somewhat more of an increasing intent. I can't tell you too much about what's happened in the last ten years.

TP: That happens in several thousand feet rather than a thousand.

JAF: Yes. It is really beyond 1,000 feet. So, there is increasing concern about what you do there, and there's more of an emphasis on in situ testing for the soft clays at substantial depths of water. Of course, you're not then dealing with the jacket structure with the piles coming all the way back to the surface or even the structure coming all the way back to the surface - you're moving into the other kinds of structures.

TP: You mentioned at the beginning of the interview about the movement of technology, incrementally, from onshore to offshore, but also, as you developed an understanding of soil mechanics offshore, you were able to apply it back onshore. Can you give us some examples of how this works?

JAF: It really begins to occur as, let's say on a bridge structure with piles that may begin to be very long, and so there needs to be a transfer of the understanding offshore to onshore, of the problems with very long piles. It is not a significant change, but it has had its effect and the people that are involved in current onshore things are aware of the developments that have come about offshore.

TP: And you worked on some of the onshore dams - Livingston and Conroe dams. Is that right?

JAF: That's correct. Actually, presently, my present consulting activities are more related to dams than they are to offshore structures.

TP: And you analyzed the foundation for many of the towers in Downtown Houston.

JAF: Probably about 90% of the ones in Downtown Houston - I was either the supervisor or the actual engineer that did them. The first real major one was for First City National Bank, and then it went on to the Humble Building, and I still call it the Humble Building.

TP: That must give you great satisfaction.

JAF: Oh, it does.

TP: It's such a wonderful skyline. Do you have any other interesting stories that you can tell that you remember from your experience offshore?

JAF: There is an interesting one and, unfortunately, I can't remember the name of the fellow at Brown & Root that was involved with me.

TP: I might be able to . . .

JAF: Shell had a blowout on a structure in the North Sea. This facility actually consisted of two platforms - there was a drilling platform and a production platform. They had a blowout in drilling one of the wells. It created a conical cavity that was fairly deep. They got the well shut in and they evacuated and shut down operations. But they wanted to get back into operation.

So, one of the fellows from Brown & Root and I flew over to London and reviewed all the data they had. They had surveyed it and the extent of the cone. We looked at the loads, looked at the pile capacity curves, looked again at the extent of the cone, and made some quick computations for what the piles capacities were for the conditions them. We reached a conclusion that, for small storms, it was okay as it was, but there were things that they could do to restore most of the last stability. It was in an area where there were some sand banks fairly close by. We recommended that they get a dredge and start trying to dredge material to fill back in that hole. We told them that we thought it was all right to put people back on the platform, so they said, "Okay, let's go out and have a look at it on site". We looked at each other and said, "They want to see if we really believe what we told them." We were willing to go out and spend some time on the platform. So, we always laughed about that.

You couldn't really just pump the sand back in. They had to dredge the sand up into the vessel and then to come up-current about a half a mile away from the structure and dump the sand there, because there was a fairly substantial current in the area. The sand would then drift down in and fill up the void underneath the platform.

It took, as I remember, a couple of weeks because I think they had to transport maybe eight or ten times the volume of sand it took in order to get it into that location because, as it drifted, of course, it widened out and not all of the sand came down just right at the structure location.

I guess the only other story that always struck me was back again in the 1960s. Shell, Humble and one of the other oil companies sponsored a pile load test on piles driven into sand. This was down on Padre Island. It was done on a "poor boy" budget. There were representatives there from the three oil companies participating in the test program. We ran these pullout tests on the piles with two jacks, one on either side of the pile, so we had to have a person to pump each jack. And then there was a person reading the dial gauges down underneath, and then there was one fellow writing all the data down. Of those four people, three were named "John". So, it was a real problem with forgetting that we had to call the "Johnss" by their last name and ask him to repeat that number or whatever.

The results of that test changed the procedure which had been used early on for installing piles into sand. We pretty well proved that, if you jetted a pile down and then drove it, you did not significantly redevelop the capacity in the jetted zone by driving further for just a few feet, in comparison to that which would have been developed if you had driven the pile all the way. So, it was immediately reflected in the installation techniques offshore in that if you jetted it, you had to jet it only to assist the driving, not that you just jetted it down and then drove it the last few feet.

TP: What year was this?

JAF: I can't tell you. I would have said it was in the early 1960s.

TP: Well, is there anything else you would like to add? Any comments about companies that you worked with?

JAF: Let me refer to my list of names that I wanted to acknowledge. For example, I worked with Jimmy steel of Bethlehem Steel in the original design of Mr. Gus. That was the predecessor to the Ocean Star. Other ones, as mentioned a while ago, were Griff Lee at McDermott, Jay Weidler of Brown & Root, Pat Dunn of Shell, Arthur Guy with Humble, Bob Bea with Shell, Paul Besse with Chevron, and others.

I would like to acknowledge my feeling of a successful career to those and others like them, but then, as well, to Bram McClelland, Bob Perkins, and Bill Emrich of McClelland Engineers because I think they contributed significantly to whatever contributions I was able to make, so they have to share it with me. Most of all, I want to acknowledge the encouragement and support that my wife, Edith, has given me for 50 plus years. I couldn't have done it without her.

TP: I think that's good. We can end here.

JAF: O.K.

TP: Thank you for your time, Mr. Focht.

THE END