OFFSHORE ENERGY CENTER

ORAL HISTORY PROJECT

Interviewee: Bramlette McClelland

Date: October 6, 2001

Place: Houston, Texas

Interviewer: Tyler Priest

Side A

TP: Why don't we just start off with a little bit about your background and how you came to this industry.

BM: All right. Well, I came to Houston to start with, to work with the City of Houston on a design of the dam that eventually created Lake Houston. This was right at the end of World War II. One of my first employees in that project was a driller. He had worked with Shell Oil Company as a geophysical driller.

TP: Who was that?

BM: His name was Ray Hurst. I put him to work in the taking of samples at the site for that dam. We also developed a small shaft to test soil. Eventually, I had a little bit of engineering assistance, but it wasn't long until we finished that project. I looked around for something to do. Inevitably, I had had conversations with these men that I worked with about staying together - it was a good bunch.

The upshot of that was the formation of a consulting engineering firm, Greer and McClelland. The Greer was a Corps of Engineers employee in Galveston at that time.

Ray Hurst, as I mentioned, was a driller. This turns out to be rather fortuitous in terms of our subsequent work offshore because, in contrast to Greer and myself, Hurst had his hands on tools, understood, "turning a bit to the right," and making the hole in the ground. That happened to fit my very short engineering experience and also Greer's because the Corps of Engineers had a policy of doing site investigation; that is, drilling and sampling, as part of its engineering activity.

This is rather unique, as a matter of fact, and soil mechanics was for a long time, elsewhere in the country. This particular branch of the profession was practiced largely by office people and professors, and they left the sampling of soil to somebody else. It was sort of a secondary task. We made it part in parcel of our service. For at least a couple of years, we were primarily involved in conventional construction. A lot of it was industrial, refineries, buildings, some dams, highways, that sort of thing. But what really changed our life was an inquiry from the California Company. California Company was a subsidiary of standard of California, ultimately Chevron. They wanted two or three, three sites I think, investigated in about 20 feet of water off the coast of Plaquemines Parish in Louisiana. They simply asked us for a proposal to go do

it. No support, no implementation of any kind available for them- it was strictly up to us. Well, it caught our fancy and generated a good bit of excitement, the prospect.

We put together a proposal that is astonishing to look back on. It involved assembling, designing, fabricating, and then assembling a 20 foot square platform to be erected at the site, using bolted connections between 3 inch and 4 inch pipes, a platform that was to be about 8 feet to 10 feet out of water. We assembled it first in our backyard in Houston, hired a small barge and a small tug, and went to sea with it. Thank God it was August. The Gulf of Mexico was quiet. We put the platform together as intended, skidded our little rig up on top, made the first boring, disassembled it, and moved to the next location and repeated that process three times. Squalls at one point almost knocked us over and might have ended our offshore career, but it didn't. We completed that operation successfully. We just beat out a hurricane that was coming in, without much advanced warning in those days - this was in 1947. After that, the California Company took a much more active interest in the implementation of this sort of thing and we went along with that.

TP: Who were you working with at the California Company? Who were the . . .

BM: Paul Besse led that operation. Jerry Osborn was his close assistant. Paul Besse continued in that capacity for a long time afterwards. They adopted, after that, a practice of using a prefabricated template substructure, sort of a miniature of what became the early standard in the offshore structure design. This created, when it was put on site, a pile supported platform about 25 feet square, maybe 15-20 feet out of the water. They hired, primarily McDermott, at that time, with a derrick barge to put this on site to transfer our equipment to it, to stand by while we made a boring and did tests at the site, and generated the data that subsequently was used in engineering analysis of the pile foundations. When we got through, they took it down and subsequently reset it, pretty much on a customized basis whenever the need arose. They changed the height of it by cutting off a part and sticking it back on as required. That continued quite successfully, still in moderately shallow water, I would say, that probably continued, the whole time the water depths never exceeded 60 feet or so.

We were on one such platform when we had a very singular experience.

TP : Is this where you claimed the distinction of bringing in the Bay Marchand Field?

BM: Yes. We were asked to carry out this similar operation in what was already known as the Bay Marchand Area where Calco had leases, but they had not yet drilled their first wildcat well. At that time, bear in mind, all of the exploratory drilling was done from platforms; there was no mobile drilling. So they had not drilled their first well. They were approaching the design of the first platform for that purpose. We were called upon to make three boorings at different locations in that field and studied the conditions there. We were bringing the last sample out from the . I think it was the last boring, I don't know - about 200 feet below sea level, a very dense sand sample.

I was on the deck with my partner, Ray Hurst, and with three other men. One of them happened to be Bob Perkins who is also being recognized this week. As we were withdrawing that last sample from the hole, we noticed that water in the casing started pouring out which was startling, to say the least. Shortly after that, discharges of gas. Finally, sand, pebbles . . What really scared our mule was seeing the drill pipe which, at that time, was sitting in slips at the top of the hole

on the deck. We saw those slips unfold and the pipes started coming out of the hole - the function of the fast movement of that gas rushing out of the hole. Somewhat fool-heartedly, we started to crank up a little centrifugal pump to get more water in our mud pit where we could mix additional mud, thinking that we could pump it in the hole and restore some control. But on the first crank of that little gasoline engine, a spark jumped and caught the thing on fire. Immediately, we were in deep trouble. The wind was blowing pretty good, toward me and toward the guy that cranked the pump. He and I went over the side. The other guys were able to climb down and got away all right. The man who was standing right next to me was put in the hospital for about a month. It just singed the back of my hair - that was the only trouble I had.

That dadgum thing burned for two weeks before Halliburton, who was engaged by California Company to pump mud into it, finally got it to quit. So it was a very substantial gas. Whether it was natural at that level or escaped gas from deeper drilling somewhere nearby, we never knew, probably the latter. But it completely consumed that platform and the drilling rig, and that was a total loss. At any rate, that's what led to that rather jocular statement - we claimed the credit

for bringing in the Bay Marchand Field which, of course, became a very important field.

TP: I think, a year later, Shell had lost a platform from a hurricane that came through, in 1949, or they had severe damage in Bay Marchand.

BM: Oh, is that right? In that same area?

TP: Yes. Not an auspicious beginning for the industry, but got over it. So, from 1947 on, you began to develop methods for soil boorings and understanding the lateral loads and .

BM: What we encountered is what everyone else encountered, and that was the profound effect of deeper water. Everything changed as water depth became greater, and it increased just as fast as we could keep up with it. It caused almost every activity out there to be stressed to the utmost. And our situation fit right into that pattern. As I mentioned, in those early boorings, we made from this little prefabricate platform which worked fairly good until the water got beyond 60 feet, and it just got unmanageable. We had watched the activity of derrick barges, huge things by comparison to our equipment, anchored on site, and doing heavy

construction. Even in fairly rough waters, their movement was very ponderous, very slow, frequently, almost negligible. And it occurred to us that it should not be too difficult to drill from such a barge, or a large barge, if we could just make a few adjustments in terms of setting casing and circulating fluid, while that motion was taking place.

We designed some fairly simple ways of doing that. We started operating with our drilling rig on a little deck over the side of a large barge, anchored on site. As long as we were in relatively shallow water, we could drive casing deep enough that you could get circulation drilling fluid and do, more or less, a conventional drilling operation where you had full control. once the water depth, however, got beyond 100 feet or maybe 140 feet, the process simply didn't lend itself to getting the casing set deep enough to get full return of drilling fluid. so, we experimented with the procedure of just letting the drilling fluid escape in providing a continuous resupply of fluid, enough to keep the hole open where we could continue drilling, continue sampling, and so on. And it worked. We used drilling mud for the most part, but it meant a continuous resupply of mud. But, as a way of getting the job, it was perfectly acceptable. Here, again, I mention the fact that I

started in partnership with a driller who was a remarkable guy, in terms of his ability to adapt and adjust to evolution of that sort. And it had a rather profound, long-term effect on our engineering practice, up to a point.

Well, that process continued for the next several years. An interesting thing happened in the early 1960s. I don't recall why, but this was a lull in activity. Maybe you as a historian can identify it. But things were rather quiet at that time.

TP: Well, the late 1950's, 1958 especially, was a deep recession and a real fall-off in business. I think it probably . . • The industry was trying to think about how to drill in much deeper waters, and Shell had its Blue Water task. I think there was sort of a lull from the late 1950s to the early 1960s.

BM: When this matter arose between ourselves and conoco, I can't be sure. The work that I am going to describe took place in 1962, however. It, undoubtedly, was under discussion for quite some time. Conoco at that time and I think, as you have correctly stated all the other players, was interested in what they were going to do next, what they were going to do in deeper water. So,

they had an engineering study underway as to what construction methods, what were the design problems, and what were the conditions that had to be met in deeper water. So, they approached us - could we do a series of boorings out on the edge of the Continental Shelf. That was a jolt. The deepest water for construction at that time was about 240 feet or thereabouts.

TP: Did you have your own drill ships that you . . .

BM: No. Again, we were working on barges up to that point, and we would rent the barges. There was a long period of time .

TP: Submersible barges mainly or . . .

BM: No. Floating barges, anchored on site. For a long time, we had a cooperative venture with Brown & Root where they dedicated a barge to this operation and kept the anchoring equipment and housing and the rather minimal requirements that we had on-board. In between projects, they were willing to tie it up and hold it in reserve until we went out again. That continued for a period of some years.

Brown & Root Marine Operators, headed at that time by Hal

Lindsey. And his assistant at that time was Noland Stewart. Noland worked very closely with us through that period of time. Again, that barge drilling method worked great, but when Conoco faced us with this question of 500 feet of water, that didn't set very well at all. The anchor line links and so forth were just not for the kind of operation we had or wanted, and were just too, too severe.

I don't know quite where the flash of . Well, I don't know what you would call it - not brilliance or intuition or what took place, but we began to consider the possibility of drilling from a boat. The more we thought about it, the more we thought it was feasible. We talked to a boat operator and asked if they would be willing to put a well through the center of the hull, case it off, of course, to maintain integrity of the boat, put anchors on it, and put our rig more or less in the middle of the cargo area, and occupy a site long enough to drill a hole. They didn't bat an eye. It didn't seem to be a problem at all because they knew that it could be done and the boat maintained intact, and you could reverse the procedure at the end of the operation. So, we . I don't remember at this time what boat company we worked with.

Over the years, we worked with quite a good many, State Built Company and Tidewater Marine, and quite a number of others. I remember, I think it was Horace House with Conoco, saying, as we were negotiating a contract "but what if you . " His people allowed him a certain number of days that they would go along with this. This was our estimate as to what we thought would be adequate for us to do these holes if we didn't run into too difficult a problem. And I remember him asking the question, "But what if you can't get even the first boring done within that period of time?" and I said, "All we can do is try".

We got every one and it worked slicker than a whistle. The intriguing thing about it, what really made it a complete shift, is that, up till that time, every sample that was taken was with a tube at the end of a string of drill pipe. You'd run the drill pipe down, force it into the ground pushing or hammering it, take it out, take the joints apart just like any round trip of a drill string, drill a hole down a little bit deeper, and repeat the process to take the next sample and so on. The prospects of that out in 500 feet of water just weren't attractive at all. So, we cast about far away that we could use a wireline operation. The upshot of that was that we designed a small diameter sampler. We arranged to get

somewhat larger diameter pipe - I'm talking about inside diameter, a flush joint pipe (internally flush), and design a small diameter sampler that would run through it and a hammering mechanism that would let us drive it into the bottom of the hole using a wire line, so that we were able then to drill down to sample depth with a full hole bit that didn't restrict the opening at the bottom at all, run that sampler, pick a drill pipe up a little bit off the bottom, and then run that sampler in and operate it with wirelines that drive the tool into the ground, and to retrieve the sample.

In just a matter of minutes, we had a sample and were drilling down to the next level and sampling again. That, again, had not been done. We, again, were, of course, forced to use a continuous resupply of drilling fluid, but that was already familiar to us - we had been doing it anyway. So, this worked and enabled us to get each of those sites investigated in a fairly nominal period of time that they had allowed for this. I don't remember what it was, but it was not too tight.

Bob Perkins, incidentally, was in charge of that operation. He was a key player in making it work. Well that completely changed the course of soil exploration and foundation investigation at sea. There was work

being done by others elsewhere in the world. There was a little bit of . Well, for example, the Heerema Company was operating a drill boat to a limited extent in the North Sea and off of West Africa. They were using very crude drilling procedures. We were retained as engineers by one of the oil companies to go on-board their vessel off of West Africa. It was just an impossible situation, so we introduced them to this drilling technique. From then on, it became their standard as well. In fact, it became an international standard and has been followed ever since that time, with a lot of variations, of course, in terms of tools, sampling methods, in situ testing devices, all kinds of things in the way of improved technology, and were combined with that drilling procedure over the next 40 years. It was just short of the 1940s, 1939 . . .

TP: So you continued a contract with drill ship companies and went from that point on?

BM: Yes, that is correct. There were periods there where we would have one or two boats under a long-term charter for long periods of time, and . . .

TP: Was most of your work at this time, you mentioned Heerema in West Africa, but was most of your work still in the

Gulf of Mexico or did you move over to the North Sea in the mid 1960s maybe?

BM: Yes. My memory doesn't quite allow me to put this in very close sequence, but most of our very earliest overseas work was in the Arabian Gulf for Aramco. And then we had a fair a bit of work in Southeast Asia, working out of Singapore. We had very infrequent work in the North Sea for a good while. We worked with Phillips on the Ecofisk Platform.

TP: I talked with Mr. Focht about that.

BM: Did you? Yeah. That was a very interesting new development in terms of design problems. Again, he was a key player in that. We were very fortunate in having, I think this sort of three person team that filled different niches in this whole picture, but Focht was a key player in the design analysis of new structural systems and new foundation problems.

Signal Oil Company, Chester Rabickey, was one of our North Sea clients. Signal I think, or their platform at least, may have eventually been taken over by BP -I am not sure. At any rate, we eventually established an office in London and had had a continuous North Sea

operation for a very long period of time after that. I can't give you a timetable particularly for that - almost from the beginning of that Ecofisk project and later from there.

TP: You mentioned in situ testing the oil from 1966. Can you elaborate on that?

BM: In situ testing really started in the late 1950s. We designed a tool called the "remote vein". It didn't come into very widespread use until the 1960s, but it became a significant device tool in getting in the in situ strength of clays. It was initially developed with a commission from Shell Research.

TP: Really? In the late 1950s?

BM: Yes, it would be the late 1950s. We retained a company called Slope Indicator Company in Seattle, Washington to work with us on the initial design. Wayne Ingram at that time was with Shell Development and was involved in that. The first tool that they designed in principal worked very well. This was a four-bladed vein designed to go in the end of the drill pipe, with an attached electric motor geared to the device. We, of course, had a conductor providing current to it from the surface. This

would be jacked into the soil below the bottom of the hole and then caused to rotate. We had string measuring devices built into it that enabled us to measure the torque. And then the torque could be converted to shear strength of the soil around the surface that was sheared. The device that the instrument company made for us worked the first time, and was enough to give us encouragement that the system, the concept, was on target and was going to do what we wanted it to do. It simply didn't have oil field huskiness to it.

We went to the marketplace in the Houston area and found people who were accustomed to designing down-hole tools and tools that required a much more rigorous environment in which to operate. We completely redesigned it. From that time on, it functioned very well and is still being used today in a variety of forms. This also was eventually converted to our line tool that could be lowered to the bottom of the hole where it was engaged with the drill pipe. With its own power system and own memory system, the test could operate and be completed. We could send an overshot down the hole, pick it up, and bring it to the surface, and avoid the problem with many hundreds of feet of electric conductor which, as we got into deeper waterand deeper holes, got to be a significant problem with that tool. But it continued to

be very useful to us.

TP: So, we have been brought up to the mid 1960s. I talked with Mr. Focht about Hurricane camille and the mud slide problem. When I wrote the history of Shell, they lost a couple of platforms in the South Past area, a real huge displacement. Were you involved in any of this at all? But you were more in the drilling side.

BM: Well, we were very much involved in the efforts to quantify what the conditions were out there. What were the strengths of the soil and how did they relate to what happened, and how could they put a structure out there the next time around with the hope of it staying? Yeah, we were very much involved in that. The vein, itself, as a matter of fact, was an important tool in investigating the conditions of the soil out there. It was a very interesting geologic problem, too, to understand why that area was so severe and how extensive was it, and how could one anticipate where you would encounter it again or where you could get away from it. So, this also was one of my keen interests from the very beginning. We had a very avid interest in the geology of the Continental Shelf and worked closely with the geologic research people.

The one that stands out, just absolute giant, was Hall Fisk who was head of Geologic Research for Humble Oil Company which, of course, became Exxon. He had taught geology at Louisiana state University, and one of his students was Rufus LeBlanc. Rufus ended up with Shell and Fisk ended up with Humble. Both of them were very good friends of ours and collaborators, and had a great deal to do with helping us to understand the origin of the deposits that we were working in. They were delighted at the same time to have quantitative data that we could get and feed them. Their resources were an entirely different sort. They got quite excited at the opportunity to get physical data and very specific information on the bedding of formations at individual sites across the Continental Shelf. They were more than happy to raise our level of understanding of how these materials got there. So, this is one of my particular areas of interest.

I coauthored an article with . . . It was a professional journal, Geological Society of America, and I coauthored it with Harold Fisk which was just a remarkable opportunity for me because I was just very junior to him and that whole area was sort of outside my field, but it caused quite a bit of stir among those professionals around the world, as a matter of fact, that had an

interest in this same sort of thing. There was a conference at the University of Illinois not too long after that, that brought people from all over the world who were concerned with this area we were just talking about, the conditions of the kind and distribution of materials in the Continental Shelf, their physical properties, how they related to construction, and so on. That worked, what Fisk and I did, and the publication that we wrote seemed to have almost a key note relationship to the discussions at that conference.

TP: What was the content of your paper? Just demonstrating greater geological reconstruction of

BM: Well, the title of the paper was "Geology of the Continental Shelf off Louisiana: Its Influence on Offshore Foundation Design" and that was literally what it . . . It was a combination of both - it recited the geologic history of how those materials got there and what happened to them as changes occurred, and how that established the physical properties that then became so important to the structures that were put out there.

I had a similar opportunity . I gave a key note address to the International Conference on Calcarious Sediments, I guess it's Woodside Berma sponsored in

Australia, in the late 1980s or mid 1980s. We had repeated experience with very difficult foundation problems involving carbonate sediments all over the world - the Arabian Gulf, Brazil, and Australia. It, again, was a case where collaboration between geologists and carbonate sedimentologists on one hand and engineers and construction people on the other hand was absolutely necessary to find some way of dealing with those difficult materials. Woodside suffered a hell of an economic loss by erecting their first platform off of Western Australia and finding that their foundations were totally inadequate. To their credit, they didn't sweep it under the rug - they did just the opposite. They called this . Well, in the first place, they let a large number of research contracts with almost every university in Australia and several in this country and in England, and hired all kinds of studies to be made, and then put on this international conference in, not Sydney, but Perth, I guess. It was a remarkable event. But, again, I think it's an example of this very close tie-in between the work that we do and the geologic sciences.

TP: That's what amazes me about the industry is the collaboration, not only within companies between the exploration and the geology people and the production

people, but technical collaboration within the industry which is necessary to be able to surmount this incredible environment. Comparisons are always drawn between the space industry and the offshore industry, but it almost seems more of a challenge offshore because you're working with three mediums - the air, the water, and the soil. I've never really The first time I really encountered in studying this history the real issue of soil is with Shell's problems through Hurricane Camille, and then you look into it more and see that it's absolutely vital. And the role of universities, scientists at universities, especially Texas A & M more so in the design of structures, but it really is amazing.

BM: The University of Texas led for many, many years the research and the response of piles to storm loads and the intricacies of design of piles to meet that situation.

TP: What kind of problems were you faced with as the industry moved into ultra-deep water, 1,000 feet, beyond 1,000 feet, in terms of the work that you were doing on soil mechanics? Say, in the Gulf of Mexico, but also .

BM: Well, I think for us, it is more of an intensification of the problems of getting samples up from great depths and being able to run the instruments down through those deep

waters, and have them function satisfactorily and get reliable data. During the period that I was active, we continued to operate our own drilling rigs and work off a variety of different platforms in drilling boorings for this purpose out to water depths of 2,000 feet. Generally, from there on, it gets into heavier equipment. And while there may, I don't know, in the successor company to my company, whether they owned drilling equipment today that handles ultra-deep water, I can't say. But, by in large, we had to shift to using oil well drilling equipment itself. It could be made available temporarily for running our tools and samplers through their drill pipe to get the information that we needed. That's the big problem, operationally speaking, of going from a moderate water depth of 1,000 feet to the 4,000, 5,000, 6,000 feet depths that are . . .

TP: Who were your chief competitors?

BM: We had very little competition up through the 1960s and into the 1970s. The Dutch company, Fugro, began to work offshore after Shell had, I guess it was blowout damage in the North Sea, which almost completely destroyed one of their platforms. Fugro, at that time, had an entirely land-based practice, but were known in particular for operating a system called a cone penetrometer. They had

an electric cone penetrometer. They were hired by **Shell** to bring it on board and to push that device into the foundation where this

End of Side 1

Side 2

BM: . . . to help quantify the extent to which there had been disturbance, and help them decide what to do next. We were there at the same time, making boorings and doing other things for that purpose. That sort of started their involvement. The Heerema Company continued to be involved with boring, somewhat in competition with us. They finally stopped that, but **Fugro** then began to expand their operation in direct competition with us. We were running into them all over the world, in Southeast Asia, the Gulf of Mexico, and West Africa, pretty much anywhere there was a project to be done.

Our two companies would be involved in at least the proposal stage. They and ourselves, both, found things rather difficult in the 1980s, in that period between 1982 and 1986 when everything went to pot. This led us to get our heads together and it resulted in merging the two companies. That took place, I guess, in 1987. That was, of course, a way of trying to avoid the . . .

TP: Everyone consolidated then . . .

BM: Overstaffing and the overpopulating . . . And they have ----stayed with the company for three years, until

1990. In fact, I stayed on its board for another three years after that. But, there have been some small competitors. There are really no large competitors at the present time.

TP: So, you must have worked with everybody.

BM: Yes. In terms of clientele, we worked with almost every company that's in the . . .

TP: Well, this has been very enlightening. I don't have much else to add.

BM: All right. I don't think you've left out a lot, as far as I am concerned.

TP: Well, it's a distinguished career, and congratulations on your induction. We can probably stop here.

THE END

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Interview Date: October 6, 2001

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Index for Interview with Bramlette McClelland
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Forming Greer & McClelland Filling a	p. 2-3
Need with Soil Mechanics Involvement	p. 3-4
with The California Company The Bay	p. 4-5
Marchand Incident	p. 6-8
The Effect ofDeeper Water	p. 8-11
The Use of Ships and Barges	p. 11-15
Working in the North Sea	p. 16-17
In Situ Testing	p. 17-18
Work on the Continental Shelf	p. 19-22
Problems at 1000 Feet and Beyond	p. 23-24
Competition	p. 24-27