MMS OFFSHORE GULF OF MEXICO

ORAL HISTORY PROJECT

Interviewee: JIM LAMPTON

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Interviewer: Tyler Priest

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Bio

Jim Lampton graduated from LSU with a degree in geology and joined Shell in 1951. He had worked part time while in school in a lab, and Shell hired him as a paleontologist. After the 1962 federal sale, Lampton moved to research. Later in his career he spent time in Michigan, South and Central America and Indonesia. He finally retired in 1981 after 30 years of service.

Summary

Interview covers basics of paleontology and its role in production. Discussion of paleo's role in the 1962 sale including the study group on salt domes. He told the "lead pipe cinch" story. Talked about deep water and turbidites, the Michigan basin and early seismic.

Side 1

TP: Today is July 2, 2003. This is an interview with Mr. Jim Lampton at his home in Cold Spring, Texas. The interviewer is Tyler Priest. Why don't we just pick up where we left off before I turned on the tape?

JL: Right. Three microfossils that are used in paleo in the subsurface. One of them is Foraminifera, which is a single cell, amoeba-like organism which secretes a shell, like a little seashell. And there is a whole family and genre and species of these things which are differentiated in time. The second group is pollen – the science of pollenology. Pollen also is obtainable from the drill cuttings, and it also has some variations as you go through geologic time, which are useful. The third is a group called nanofossils, and these are very, very small creatures with different generic names. They are extremely small, and they are most useful up in the youngest sediments like the Pleistocene. Each of these types requires a different processing technique in the lab. They all are derived from the drill cuttings that come up as the well is being drilled so there is no . . . all paleo is a low budget operation, as distinct from seismic and production work. Someone once told me I should spend more money – I would get more attention! But I could not think of a way to do it.

Another thing I would like to mention is the stratigraphy of the Gulf Basin. Well, it is somewhat unique. Its uniqueness is what made the paleo work as well as it did.

There is only one other basin that I can think of right offhand that has the same general stratigraphic setup and that is in Nigeria.

TP: Yes, there are a lot of similarities I have heard between offshore Nigeria and the Gulf of Mexico.

JL: And something that made the Gulf more usable, the paleo in the Gulf so usable, is its simplicity, really. On a regional scale, it is a very simple basin. It is all sands and shales. The topology is simple. It is a subsiding basin. Overall, there has been no upthrust, overthrust. Everything is what you would call normal faulting. And everybody knows this, so you tune those other models out of your mind.

The other thing is the basin consists of a series of regressions where the shoreline, the rivers, overland to the ocean. They push their margins farther and farther into the ocean or into the Caribbean, farther south. So, this made a break in the marine environment over the shelf. And then, the subsidence would take over and overrun the amount of material that was being deposited. This lets that sea come back in and cover that package of sands that had been deposited by either beaches or by the river. So, in its best form, you get the marine condition here, and then, a regression occurs. The shoreline is pushed farther and farther south. There are no fossils in that, nothing that is worth anything anyhow, and that can be a package of a few hundred feet to a few thousand feet. And then, the subsidence takes over. The supply of plastics is diverted to somewhere else. And the sea comes riding in over

that sand package with its own distinct group of microfossils – either Forams or whatever. And then, it is broken up again. So, you get a series of these transgressions/regressions, transgressions/regressions. And each transgression can be mapped with a microfossil assemblage. Some of the microfossils in those transgressions are time dependent. They only live up to a certain point and then they are gone. Those are wonderful ones called marker fossils. The number of marker fossils per thousand feet of section becomes less and less as you get younger and younger, because there is less time to evolve.

TP: So, like Miocene and older sediments are more . . .

JL: Are easier to subdivide than as you get young. And then, when you get into the Pleistocene, you are in a different environment structurally and stratigraphically. You do not have the same type of fluctuations and transgressions/regressions that we had earlier because of the lower sea level by ice. It has changed some. Now, I do not know squat about the Pleistocene.

So, that kind of sets the stage for the Gulf Coast.

TP: Just give me a little background on yourself – how you came into this field, how you came to Shell.

JL: Sure. I have a B.S. from LSU in geology. I liked paleontology. We had a semester

course in micropaleo. Good teacher, and interesting work.

TP: At LSU?

JL: At LSU, yes.

TP: Who was your teacher? Do you remember?

JL: Dr. Anderson. He has passed on, too. And Dr. Howell. He was the leading light for a long time. So, at the time, Shell had an office in Baton Rouge where LSU is, and I started working there part-time in the lab washing samples. I was doing just work in the lab to make some money to get me through college the last year. I was on the GI Bill, but it ran out. So anyhow, I was working there, and I got to know the division people, all the people here and there. So, I applied for a job and they accepted me as a paleontologist. So, I started working there. I got my early training in the Miocene there.

I went to Jackson, Mississippi for about 1-1/2 years. It has a stratigraphy which is different. It just dealt with rocks. I did not like that, so I got transferred back to Baton Rouge, and I stayed there for a while.

TP: You joined Shell in 1951?

JL: 1951. Stayed there the whole way until 1981. I had 30 years when I retired. One of the paleontologists was moved down to New Orleans when the Marine Division was set up. He stayed there, one year, 1-1/2 years, something like that. He decided to be a consultant because work was booming. He could make a lot more money. So, he went out with someone else to consult, and they moved me down as Marine Paleontologist, or something like that. I was like a division paleontologist. I had a staff of six or eight people plus the lab. We were involved in the first big lease sale offshore in the 1960s.

TP: 1960 and then 1962?

JL: 1962. That was the first one I was involved in.

TP: Let me just back up one second. Where was the industry's state of understanding of the Gulf Basin when you arrived at Shell?

JL: It depended on the company. Some companies had a better understanding than others because unless you can correlate from one well to the next over a fairly large area to where you can put together a map . . . say, the sand percentages, production in that particular segment of the thing . . . it would be like if I gave you a map of Houston and told you to map out the ethnic groups in Houston. And I told you that the ethnic groups were Greek, Armenian, Mexican, blah, blah, blah, but I did not tell you where they were. Unless you went out and defined where those groups were

located and marked on a map what streets . . .

TP: You mean companies with the most well data?

JL: The best well data and the best paleo to help them correlate that well data . . . Because in the early stages, seismic was not adequate to go from well to well necessarily. When I first started, seismic sections were hand plotted on a big piece of graph paper on a wall. And the representation of a reflection was a pencil line between two points. That is all you had. There was no digital seismic data at all. It was all computed by hand and plotted by hand. So, you were friggin' blind most of the time, really. And all you looked for on that type of data was north dip because regional was south dip. And if there was any reversal of that, that meant there had to be a trap somewhere in that area.

TP: And they were also using it to refine salt domes? That did not work that well.

JL: It did not work that well in those days. They tried to use gravity a lot in those days to find salt domes.

TP: In the early 1950s?

JL: Yes.

TP: There were problems with reflections in the shallower parts of the Gulf, is that right?

JL: Offshore, there are. Onshore not so much. But you did not have the ability . . . Since the data was not digitized, you could not manipulate it. So, all of the modifications, the type of reflections that you were going to get, had to be made by the field program. You had to change the shooting parameters and things like that. So, it was pretty damned primitive. It really was. It is amazing how far they have come.

TP: I know this is not your specialty, but the reflection seismic worked better onshore than it did offshore in the marine environment, right? It was still unreliable.

JL: They had reached the digitized format, I think, before they got offshore. Once they were digitized and had that type of presentation . . . you have seen the sections, I am sure, with the reflection, and so on and so forth. That was a different ball game. Then you could see stuff, most stuff. And they did have a hell of a lot of trouble with the salt interface, salted interface. We knew of three wells of one offshore prospect on a dome. And the first well, we saw we were close to the salt. We were 1-1/2 miles away. So, we moved in. So, we thought it was a giant step. We move halfway in. We thought we would be okay. And we were half a mile from the salt. And finally, the third well was in the proper position and tagged the salt. So, that was after the 1962 sale.

TP: All through the 1950s, you have these huge flotillas of seismic crews out in the Gulf and it still was not that reliable.

JL: Well, but it was the only thing you had. The only way you could make a location was with seismic. You could not make a location with bugs, with Forams or anything else. You could not make a location with gravity. Gravity helped define a general area of a salt dome, for instance, but the only way to get anywhere close to where you would like to drill, was seismic. That is why they kind of ruled the roost . . .

TP: So, it worked but it was primitive?

JL: Yes, that is right. It was all you had. You used what you had. So, that was what you had – you used it.

TP: Did you know anything about which companies did the best paleo work?

JL: Well, I think Shell was number one, and Chevron at that time, or California Company, at least in the offshore, was number two. And then, maybe Exxon/Mobil or Exxon at that time, or Esso, I guess, at that time, maybe next.

TP: Humble.

JL: Yes, Humble. They got off to a bad start onshore and it did not help them any offshore. Texaco was the bottom of the barrel paleontologically. They were not too good. And then, there was a group of consultants that did some good work. There are different levels of paleo. There are different levels of usage for paleo. There are local correlations that surround one structure, one dome, where you go from this well to this well and that is just local. That may cover an area of 10 or 20 square miles. Then, there is semi-regional, which would go, say, 40 miles square or something like that. And then, there is regional which will go from one side of the offshore to the other side of the offshore, which would be several hundred miles across there. There are more correlations from paleo available on the local scale than there are on the regional scale. But the regional scale was the one that was important in the 1962 sale because that is what we used to define the packages of sands. What was productive? What could be productive? Where there would not be any sands? So, there are areas in the offshore in a given package where there are little or no sands and no objectives. You can drill the hell out of it but you will not get anything because there is no reservoir.

So, in the 1962 sale, for instance, people like Jerry O'Brien and Joe Broussard, who has passed away, and Cos van Wijnan. . . have you heard of Cos? He is a Dutchman.

TP: I think I might have. I have not really heard much.

JL: He was on loan from The Hague, from the group. He was a brilliant man – a geophysicist. He had no preconceived biases. Some of the disciplines have a preconceived bias against using anybody else's data but their own. I am sure historians do the same thing.

TP: Oh, yes!

JL: You understand it then.

TP: Right. There are historians who are more inclined to political science techniques, sociological techniques.

JL: Right. Find what sells.

TP: Yes, right. I am very interested in the 1962 sale because I know Jerry talked about it. It was a landmark.

JL: It was.

TP: I mean, especially for Shell. Some people in other companies did not fare that well in it, but it seems to have been a step level change in the understanding of offshore . . .

JL: Well, that is when the leases were still cheap. If you got the right leases, you made a lot of money. If you got the wrong leases, you spent a lot of money.

TP: They were definitely cheap in 1962 because they offered everything that was nominated.

JL: Right, and the terms were cheaper, too. So, an outfit like Humble or Esso, for instance, they bid on large tracks of acreage in West Cameron, East Cameron, off to the western side of Louisiana offshore, which had great structures. And where there were low-level features, there were salt domes, things like this. But we said there were no sands there, or very few sands. It was going to be pressured. It was going to be reservoir poor. So, we avoided those areas when we went to the sale, and they did not. Once we got past the sale and started drilling, they had spent a lot of money over there for no return. We did not spend any money over there. We did not drill any well in which we had predicted there would be reservoirs and there were not reservoirs. There was not always production but we never screwed up the stratigraphy once. Not once.

TP: Do you know other companies that went over in the west? I think Mobil went. Would you consider that ship shoal area or are you talking further west?

JL: Further west than the Camerons. Esso is one that sticks out at me because they had gone whole hog. We were so surprised at that. We really were. Did Jerry mention

some of the statistical work we tried, that the group tried to do to set up a drilling program – a little group that they formed to study salt domes and all those . . .

TP: Yes.

JL: All that work was used in defining where we were going to buy and where you wanted to miss.

TP: Can you talk about how the paleo work fit into . . . was that part of that second group?

JL: Paleo provided correlation information so you could develop packages of reservoirs. You always dealt in packages, as small as possible because that gave you better definition. It also provided facies or environmental information: This is new shore. This is delta area. This is open ocean. This is the base of sands. We provided that information which they plugged into what seismic was good and what well correlations were usable to develop maps where you wanted to lease and where you did not want to lease. And they were going to use Chuck Stewart's geopressure drilling technique to reduce the cost of the well. Really, the very initial concept was that on any given salt dome, for instance, prospect, we would drill one or more expendable wells just to find out if we had picked a good prospect. We would not necessarily expect any production from it. If we got it, that was fine. But we were going to test to see if there really was an incentive because we did not really know.

These were projections.

TP: Well, I know their expendable well approach was fairly new. Did Chuck Stewart's drilling process allow you to say "okay, we can drill these expendable wells because the cost for each well has gone down."

JL: These were standard-sized holes. These were not slim holes. There is a type of well called an expendable that is a smaller hole, and even if you get production in it, you could not do anything with it. So, this was a standard rig, standard-sized holes, standard drilling operation the whole way. This did not last long. Upper management could not stand it. The idea was really to drill one on the top of what we thought was a dome, to see where the salt really was. From that point, once you had a soft point known from drilling, then you could use refraction shooting which is a different . . . you have heard of refraction?

TP: Yes.

JL: O.K., you could shoot refraction from that hole. You could define the salt face much better than any other way at that time that was available. So the first well was kind of felt to be a test to the top of the salt. Find that, shoot refraction, and then start drilling the subsequent wells. Then, you test that model for where the salt shale or salt sediment was. Was the refraction good? If it was good, then you could start developing the flanks with some knowledge instead of just hope! Those

expenditures for those dry holes wore on management. They do not like to hear dry hole, dry hole, dry hole. So, that was kind of curbed pretty quickly. So, we went to trying to pick the best shot we could pick with the data at hand.

TP: I remembered from a conversation with Jerry that, it seemed to be, and from what I have read in other trade journals, that there was a period in the 1950s where companies were not so interested in salt domes anymore in the offshore, and there was sort of return to . . . I remember Jerry's story about Freddie Oudt saying, 'Don't give us another White Castle.' And so, was there this rediscovery of salt domes in the Gulf with the 1962 sale or was it . . .

JL: A lot of that, I think, came out of that special study group that went back and actually looked at domes. Some of the stuff that Freddie Oudt thought he knew, he did not know. Nobody ever really told him how good a salt dome was. I mean, it is the ultimate trap. There is nothing better. I mean, it is huge, it seals, and you just cannot beat it. He had a lot of strange ideas.

One of Freddie's problems was he was an old, old-time geophysicist and his wife had property down here in Chambers County on the discovery salt dome down there. What is the name of it? High Island. She was rich and he was rich enough through her. So he got to enjoy himself. He was not a very technical person. I am sure he was a good businessman. Most Dutchmen are good business people. And he liked to spend a dollar today and get you a dollar tomorrow. So, the simpler the

trap was, like . . . You are familiar with lower level structures?

TP: Yes.

JL: Those are simple traps. There is nothing very fancy about them and seismic, at that time, defined them fairly well. They were defined as where you got to the reversal of the dip, and you got dip in both directions away from that. They were simple and cheap to drill. So, he liked them, I guess. But the big fields are salt domes. The special studies group went through a lot of data. God, those guys went through a lot of data. Jerry was in that group, right? Yes. And Don Daley. He is dead, too. He was smart, a smart little guy. Who headed up the damned group? I cannot remember now.

Anyhow, they came up with some numbers that showed that certain quadrants of salt domes are more productive than other quadrants of salt domes. So if you have a salt dome with no production and you would like to know where to drill; from the study, you would pick this quadrant over that quadrant. They came up, of course, with volumes of how much a salt dome could produce and blah, blah, blah, blah. And those were the big simple things that you could see offshore. The salt dome just stood up there like a mountain. So, that study later on was pooh-pooh'd by some other management. It was a political knife job. I will not go into that.

TP: After the 1962 . . .

JL: Yes, well after. There was a turnover in management. As in most turnovers in management, whether you are in an oil company or the oatmeal company or something else, the new manager or the new vice-president brings in his people because he knows he can communicate with his people. They will do more or less what he thinks ought to be done. Whereas, if you are using some of the older people, they might be a little smarter than he is and know more than he does. He does not like that. So, they were all moved out. That is how I got moved to research. I got moved to research to get me out of the way. There was no reason to send me to research except to get rid of me. And it happened to a whole group of us that were in the 1962 sale because we knew too much.

TP: So, when were you moved to research?

JL: Oh, I do not know. 1969? I think before 1970. And then, I went out from research. I went out to Midland and worked the Michigan Basin. And then from there, I went to what the local U.S. Shell called Pecten which was their international group. I was sent there and I worked South America and Central America. And then, they sent me overseas to Indonesia. I worked overseas for a couple of years and came back.

TP: Let's go back to the 1962 sale. So, Jerry and his group put together this package on the salt domes.

JL: Right, and that was used in making the decisions and making the presentations to the upper management to do the program. And one of the elements of the program was . . .

TP: Ronnie Knecht, was he the one who put together this group?

JL: He was the one who O.K.'d the organization. I do not remember who pitched the idea to him, but he said, 'That is great stuff. Let's do it.'

Let me finish this off: One of the elements of drilling all the wells, as far as that goes. In the past, a well was drilled and you reached some total depth or you had a blowout or something, and it was about time to quit. And it took days sometimes to get authorization from management to leave the location and do something else. Well, when those days were going by, he was still paying for that rig, and it was costing him money. So, the plan was when the well reached its total depth, or at some point, we would have all that data together. This included all the paleo and our best interpretation with seismic with the new well data built in to it. Then, the next location was picked. The next location picked. So, we would say, okay, let's abandon this location today and move over to this location tomorrow. And that saved you the travel time. It saved you the circulating time waiting for somebody to make up their mind. So, that was a money saver that was built into the plan. It worked pretty well for a while, at least, because it meant some people getting up in the middle of the night or early morning to make some decisions which sometimes

did not sit well with some people. But I spent a lot of my time on the phone with people on the rig.

Paleo also sent people on the rig.

TP: To collect the cuttings?

JL: To look at the cuttings and to wash and to prepare them, process them, to look for the markers: Have we reached such and such a level? This was before the log was run. Were we approaching a high-pressure zone? There were some places where you could foretell that from some marker group in your cuttings coming up. You would say, 'we had better start setting our pipe or drill more slowly and wait for that first kick' – things like that. So, it was a warning. And we did work like that for production a lot. And we did work like that also, of course, for exploration but more for production. That work was done more for production than for exploration. Of course, we were not on a production budget so they got that for a freebie.

They did a lot of well sitting, onshore and offshore. A lot of the old fields had high-pressure zones that they wanted to avoid. One of the simplest ways was to have a man on the well looking for the marker that they wanted. So, it was used there, too. That was really just routine grunt work. There was not much thinking involved in that. You had to stay awake. You had to be able to see through the microscope. Other than that, you knew what you were looking for. There were no development

zones. You were looking for known quantities, and that was about it. Whereas, in the offshore, we were developing the zones. There were no zones out there known to us at the time. So we had to develop the correlation zones as we went along. Out there in particular, we used a combination of Foraminifera and pollen. Pollen was very important. But it only had two zones in the whole Pliocene, but they help to reinforce anything that we came up with. In other words, you had two independent lines of investigation reaching the same point that say, "O.K., this is so and so." And that made everybody feel a lot happier.

TP: So, from the work that you did for the 1962 sale, do you remember the areas that Shell concentrated on, and what your final objectives were?

JL: Yes. Eugene Island. That was one of the first wells we drilled early on. And then over to Grand Isle. And then, to the Delta area. Not the Eastern Delta but the West Delta. Not on the east side of the Mississippi River. Pretty much, that was out. The west side was good.

TP: Well, I mean, that seemed to be where . . . you had East Bay which was still on the left side of the Delta, and the whole South Pass area. It was logical then, to focus on the areas closer in.

JL: Oh, yes. Absolutely. But there were a lot of structures east of the Delta. And in the 1967 sale, after I was gone, the area exploration manager, James Hohler . . . I do not

know if you have run across him.

TP: Yes, I have run across Jim.

JL: He did not believe what he should have believed. He had a huge structure sitting out there east of the Delta. I cannot remember the . . .

TP: The Main Pass area?

JL: No, it was further up and over.

TP: Breton Sound.

JL: The Breton Sound. Just south of Breton Sound. Maybe that is Main Pass. That is not Main Pass. Anyhow, he had this huge structure, and when he went to the sale with his crew, some of the people asked him about that area. You know, 'are there going to be any sands there?' And he made the mistake of saying, 'Oh, yes. This is a lead pipe cinch. We are going to get plenty of sands on this huge structure.' He even had a volume estimate for it he was going to produce. They drilled the well and out of the Pleistocene which is right at the surface there. It only goes down about 1,500 feet. It was all shale. So, somebody got a small lead pipe, tied a string around it and suggested he wear that around his neck.

TP: So, this would have been after the 1967 sale?

JL: Probably somewhere in there, yes. It was a total disaster. Those were the types of things you had to avoid if you were going to make any money.

TP: In the 1962 sale, you had studied that . . . you knew that were not . . .

JL: There were wells drilled out there, so you had some cuttings, for instance. You could look at the fauna or the Forams in those cuttings and it was all marine. There were no major regressions in there. It was all major transgressions, and the logs of the wells that those cuttings came from were by primarily shale. So, it was kind of . . .

TP: So, they did not rely on the paleo work?

JL: That is it. They had their bias. One of the things that is difficult for a manager who did not grow up as a working geologist, who did not grow up with paleo, was the fact that the manager had to take my word. I could not show him something and say, 'This is why it is like I say it is.' Whereas the geophysicist pulls out a section and he shows it and says, 'we are here.' And you can see all the structures, all the seismic sections. 'We should drill there.' The manager says, 'Oh, yeah, that looks great.' And, you know, he can see it. There it is. It is on that section. Whereas, with me, I could not show him anything. Take him to a microscope, show him a

bug? A Foram. There was not anything you could show. You had to have the background of the whole regional setting, and the managers who migrated into the Gulf from say, the Mid-continent or other types of basins somewhere else in the U.S. had a hard time grasping this. That they had to take somebody's word that this is the way it is. I was told one time by a division manager, he said, 'If you could just give us some pictures to look at or something. Take some pictures of the Forams and show them to us.' I said, "Go to hell! That is silly!" But that has been a drawback. It is only after you have worked with it as a working geologist or geophysicist that you see that what the paleo indicates turns out to be right between two wells that you are trying to correlate. You all of a sudden can correlate them because you take these markers and put them on your e-logs. You say, "By George, yes, I could not do that before I had this information, so that is good information. I believe it. I can see it." The same thing with a geophysicist who is trying to maybe jump across a large fault. In the old days, it was very difficult to get across these down to the south growth faults, you run into growth faults. So, the transgressions or the markers are at different places now, and the set of reflections is sometimes hard to carry across these faults because the stratigraphy has changed. Once you plug in the correlations that the paleo suggest, all of a sudden, you can see the faults. You can see that these seismic blips of _____ where you add this information. So, it is additional information for those two disciplines that makes it easier and more reliable to do what you are trying to do at your desk.

I remember one time, I was working with an old gentleman, Stanley Say. He had

been in Mexico for Royal Dutch Shell. He had fallen off a mule doing field work, injured his back and he was crippled. But he was a very good geologist. He was in his 50s at the time. I worked with him in Baton Rouge. And Meacham, I guess it was, drilled a very, very deep test too deep test, I guess, at Lake Washington. It was a huge dome. And they found sands all the way down to something like 20,000 feet or 18,000 feet. The old gentleman could not correlate this expanded section with a section upthrown on a fault. So, we worked the well up and I gave him the data. He started slipping his logs. All of a sudden, the shales that carry the bugs usually carry a distinctive electrical character. They have a distinctive look on the e-log. And all of a sudden, he could see some of what he was looking for when he plugged my data in. And from that point on, he never questioned me. Before that, he was a little shy. He was a grand old man. He did a lot of the regional thinking and framework for the offshore. He could not go to New York because of his crippled condition. He did not want to travel. He could not travel. So, James Hohler was his leg man. James was tall, he was blonde, he came from Miami, Ohio. He made good presentations. So, James would take all of Stanley's work, take it up to New York and present it. On the basis of that, Hohler's career got started. And that is how he ended up manager of the offshore in the late 1960s. That is how he began his work.

At the time Stanley did his work, most of the control was right on the shore line. There was not anything drilled out probably over 100 feet of water, but he played a role. He certainly did.

TP: Shell got a lot of leases in the 1962 sale.

JL: A lot, yes.

TP: That must have really helped with all the well data you got from drilling all the leases to develop your understanding of the whole offshore basin.

JL: Oh, absolutely. Yes, it was critical, for seismic, for e-log work and paleo. A lot of stuff was drilled with a tight hole and a secret hole. You were not supposed to show the log to anybody or share any information, and things like that. But I had some friends in other companies and we would surreptitiously share samples because you could not tell too much. Some people could not tell too much about the section involved. You certainly could not tell anything about any objectives of production, gas, oil or whatever, in the hole, but you could develop the correlations from the paleo. So, we did a lot of swapping under the table, and everybody benefited from it.

TP: You were not undermining your own company, you were helping the whole industry understand each other.

JL: We were helping each other understand, yes. And we had some guys that would do that and some guys that would not. The Chevron guy, California Company, was one of the ones. James Dorman, he was always good about that, and we would trade

samples off. We had not much talked though about what we were doing with the samples. We did not talk about the correlations we had developed. We just swapped samples. He did his thing on my samples and I did my thing on his samples. So, we never really swapped data on what we were using. That is what we did not do. But it provided a lot of information which you could plug into some seismic where it was running across country. You had no well control. You would drop in some markers from the samples that you got, and it helped you to define your stratigraphy better.

TP: Do you recall other companies that did well in the 1962 sale and then companies that did not? I know that Mobil did not do well in the 1962 sale. You had a couple, like Forest Oil, bought a bunch of leases.

JL: I cannot remember.

TP: I am talking about these companies that were not traditionally involved in Southern Louisiana or marine environments.

JL: Right. I really cannot . . .

TP: I know that Esso was way off west . . .

JL: Well, they blew a lot of money. They got some stuff elsewhere also that was good.

Do not get me wrong. They were not wiped out. It was just that they overexpended their funds for this no-good property. Texaco did not do well. Texaco was not much of an exploration company. They got their start with Huey P. Long.

TP: In the State Lease 340?

JL: Yes, the five island trend. That is five big salt domes. And they got almost all of them in that State Lease 340.

TP: From the state leases. How did that work? It was originally leased to a company that Huey had interest in? Is that right?

JL: I do not know. All I know is that they paid Huey off and they got the lease. I mean, that is the story. I do not know it personally. I think money had to change hands.

TP: Then they transferred it to Texaco.

JL: Probably. It sounds like a legal dodge of some sort, yes.

TP: A similar type thing was happening in Plaquemines Parish with Leander Perez.

JL: Yes. They made their money. So, Texaco was primarily a production company. They were working their ass off on those salt domes.

TP: Well, yes, then they got the LL &E leases, too. I do not think that involved Huey. That was early. That was the 28 . . .

JL: I think that was a straight-up deal there. I do not think there was anything there. So, they were never really adept in exploration because they had so much work and were making so much money from the stuff they had. So, they put their money where their effort was.

TP: And Shell was fairly lease poor onshore in Louisiana.

JL: Yes, we had some salt domes. I cannot remember . . .

TP: Weeks Island.

JL: Weeks Island. Thank you. That was a big salt dome there.

TP: In working on the Shell history, it always comes back to the question that everyone wants to know when I tell them where I am from: What was the secret of Shell's success? Why were they so successful in the offshore? Why did they emphasize it? You could say, why did they put so much into the paleo? Why did Shell and not other companies?

JL: Well, Shell was always a technology-oriented company; regardless of where they were, whether you were on the West Coast or the Gulf Coast or God knows where.

TP: Back to the Royal Dutch Shell.

JL: It goes back there, to the types of people that worked for Royal Dutch Shell, now even. I guess they are all doctorates. They all have doctorates, pretty much, I think. And they all tend to think scientifically. I mean, that is the way their mind works. When I was overseas with them in Jakarta, they had a paleo section there run by an Austrian paleontologist. That type of operation is particularly different. They had one paleontologist in a room full of natives picking. They would go through and they would sort out stuff from the samples. The paleontologist would give them a suite of bugs, a suite of Forams and he would tell them to look for these shapes. That is all they knew. They were looking for shapes. And they would go through the samples and they would isolate those shapes onto some sort of little format and pass those on to the paleontologist who would sit there and do the taxonomy. That is a completely different type of operation because everybody that worked for Shell had, at least, a bachelor's degree in geology.

TP: I think someone said that Shell hired Ph.D.s without prejudice.

JL: At one point, they hired by preference. At one point, they would not hire anybody but a Ph.D. Now, paleo work inside Louisiana offshore anywhere, it is not the type of thing that a Ph.D. wants to sit down and do because it is a lot of grunt work. To do the work, you have got to sit at the microscope and you yourself have to look at the samples. And you cannot just look for some . . .

End of Side A

Side B

JL: . . . and the seemingly hum-drum existence. They did not get to make any grand conclusions after the first six months. Actually, being a paleontologist was not the staircase to upper management. I left paleo and was just regarded as a geologist. That is how I started moving around the country. If you remained a paleontologist like Ed Picou . . . He has stayed a paleontologist all the way through, and he stayed in the South Louisiana the whole time, which is fine, you know, if that suits your pistol. If he is happy, then he is happy. _____ switched over, or they switched him over, I guess, to geology, and he started working . . . . he did some work, I think, on the Atlantic Coast a little bit and offshore Florida, and what have you.

Ronnie Knecht actually started out as a paleontologist. He worked for several years as a paleontologist. That is one reason he understood the stratigraphy so well because he had worked with it down and dirty, you know. He would look at the logs, he would look at the samples and he combined those two in his concept of the section.

TP: What happened to Ronnie Knecht after the 1962 sale? He did not rise to high management but every one I have talked to credits him for Shell's success, I guess in 1960 and 1962, and in really getting the company offshore.

JL: He moved up the ladder a little way. After I went to research, I kind of lost track of Ronnie's career. He went to New York for a while. Oh, he went to London, to The Hague for a while. And usually, these are grooming steps. You are going to get a whole overview of the whole operation. And then you come back. Sometimes you go into economics, economics for the whole U.S. operation. And then, the next time I run into Ronnie, I am transferred from research out to Midland, Texas. And Ronnie is now VP for that huge area which was the whole mid-continent.

Ronnie's greatest strength was not in his ability to put people at ease, because people did not know him. So, when he was at Midland out there . . . but Midland had been very quiet, kind of backwater. They did not do a hell of a lot. The staff was kind of beaten down, let's put it that way. They were not very aggressive. The individuals were not very aggressive. And Ronnie is, if nothing else, aggressive. He may be overly aggressive. That may be what got him in trouble. But when we would come in for a presentation from the Midland staff, he would scare them to death. He came on with a very strong, booming voice. He was not mad at you; that was just the way he spoke. He asked very pointed questions. They were not used to any of this. They were used to the management just kind of sitting there, letting them go through their song and dance, and then get out and go back to work. Well, he does not operate that way. He wants to know why.

Sometimes, he likes to see some details of something you would show him. This upset the whole staff out there pretty badly. They were scared of him. Just flat scared. So, I was sent out there, not because he was there but I was just sent out there to get me in research.

I had been in head office for a little while, too. But then they sent me out there and set me up initially as division stratigrapher. Well, I did not really want to be division stratigrapher, so I ended up being what they called a project leader for exploration. So, I ran the exploration program for the Michigan Basin at that time. I helped program where the seismic was going to be, not how they did it, but when they did it. And where they should do it, and to better understand the presence and the distribution of the little reefs that were making all the money for people. They were small targets. They grew in a particular environment in the Silurian and in that age block. And one of my aims, one of my endeavors, was to locate that same environment in other parts of the Michigan Basin. We knew where it was in the north because they had already found them. So, the idea was to go find them somewhere else before anybody else found them somewhere else. And we made a couple of stabs at it. I was not successful.

TP: That was quite a play, the pinnacle reef.

JL: Oh, a hell of a play. A strange play because the targets were so friggin' small,

you know? And yet, they produced . . .

TP: Once you figured it out, you did not drill any dry holes.

JL: No. The seismic zeroed in on those. They had a processing technique, and one geophysicist who could close one eye, look down one second and tell you where a reef was.

TP: Was it Pirsig?

JL: No, it was not Pirsig. He was a fat Italian. I cannot remember his name right now. But he would actually take his glasses, and he would put them down on an angle like this, and he would look through it that way at the top of the section. And he would see a disconformity in the signals, and he would bet his ass that that was a reef. And he was right almost all the time! That was strange. But they had already processed the data a certain way and it was very lucrative. It was cheap. Drilling was cheap. Shallow. No pressures to speak of, as I recall.

TP: That is interesting.

JL: But the reason he did not grow up to be the head of exploration for the U.S. . . . I think he just got on people's nerves because of his aggressiveness. He did not

have a lot of small talk. He was not a golfer. He did not hang out at the bar with you – that type of thing. He was all business. All business. I think that kind of made it bad on some people. Maybe they were afraid if he got too far up, he would really take over. So, they did not give him the due that he deserved. In fact, his last years with Shell, they really put him in a pocket. He was back on shore in the old Miocene trend, in a separate office from the rest of the crew, and they gave him a group of old folks to help him try to dig up some prospects in the old Miocene.

I had left by then, and I consulted for him in that small group.

TP: So, the paleo work was most useful for the Gulf geology. In the hard rock areas, on the other hand . . .

JL: The tide involved in the hard rock areas allows the rock packages to be pretty discreet and different, one from the other. You had the limestone and the shale, and this extends over hundreds of miles. So, the paleo is not worth a lot.

TP: One thing I am trying to figure out: Does the geology change much from, say, onshore southern Louisiana, in the marsh areas as you move further offshore into the open water? Is there a change in the geology that people would have seen? Do the sediments thicken? Does that make any difference for your objectives or

for your understanding?

JL: The mechanics of the basin are the same all the way out to the edge of the shelf. The subsidence the loading, the shifting of the shoreline, the transgressions, regressions, that's the mechanics. That stays the same. It is different age rocks. You moved from Miocene to Pliocene, which is just a term. You can call it anything you like because as far as the rocks know, they do not know it is Pliocene. And we used Pliocene because we just needed a new term. I do not know that it is . . . I have never had anything dated somewhere, a radiocarbon or something like that, that would say that is Pliocene. That is just a term that we used. And nothing changes until you hit the Pleistocene. And then, things change. Now, one of the changes also in the search for objectives that has changed in that shelf area was the salt overhang concept. Are you familiar with that?

TP: No. Explain that to me.

JL: A salt dome comes up during differential loading and gravity difference between the sediment and the salt. Salt lighter. Coming up like a balloon. And, at some point, the salt may actually reach the surface, and it starts to spread laterally. They see this happen in the Middle East where the salt hits the surface, and it forms sheets. Maybe that is what happened. But anyhow, some of these domes now,

they found that there are sheets of salt sticking out away from the domal structure itself. And underneath this sheet are sediments . . .

TP: That is why they call it the subsalt?

JL: The subsalt, right. And, of course, the salt being an absolute perfect seal, any sediments that are trapped underneath that shelf or wing should be great. You have got the trap and you should have hydrocarbons. You could not see this until the seismic got good enough. You could not penetrate that wing. You would hit the wing, it would bounce back, and you could not see anything below it. But now, they can. In the past, in the 1962, 1967, 1968, whatever sales, we knew in the stratigraphic arrangement that once you went through all these sands which, starting at the surface, you were at the surface, you were now marine . . . as you go down . . . the marine transgressions in general show there is deeper and deeper water. You can go through a certain amount of water depth on these indicated by these transgressions and break back into another regressive package. Well, at one point, you reach what we termed in those days, a slope facies, which was a really distinctive suite of fossils. There was no missing it. And from the little control that we had from wells drilled hither and yon across the basin, there were no signs encountered once you got into that facies. Once you got into the slope facies, you could not get out because the regression had carried. The regressive package had never gotten to that geographic location. There had always been a sea there.

There had always been a sea at that location from some time, from the Cretaceous on up, I guess. And it was not until some younger time that through regressive package, this delta had been able to push that far south. So, we said, whoa, when you hit that, you stop. That was a paleo decision. That was one reason you sent people on the rig. So when you hit that facies, you could just slow down. Because you were going to hit even more pressures, and if anything, high pressure gas from small stringers of silts and sands. They were called turbidites or something of that nature – deep water deposits. So, in the 1962 sale and I guess from then on, for a long time, that was the basement. That was the economic basement. There was no reason to drill any deeper to make any money. Now, with this new seismic and with these great traps on these salt wings, they have revised that now somewhat, and they are now looking for turbidites. So, as you go through this deep water facies, which is still deep, but now you have turbidite deposits. If you carried sand out into the deep water, you did not regress the shoreline out. You just funneled out sands from the shelf into the deep water. So, you can go through this deep water stuff, go through the salt wing and the deep water stuff, and find turbidite.

TP: The turbidite is not all under salt?

JL: No, they are elsewhere, too. Absolutely. But before, you never had such an attractive seal.

TP: So, there is no paleo work being done in deep water?

JL: Aside from the facies definition. We have never had any zonations, although there are some zone formations possible. Because when you get into that environment, if it is a caustic environment, it only changes if you . . . The only changes that you should see, let me put it that way, would be time changes. If the environment remains the same, and if something in the Foraminifera suddenly changes, then it is because there is an extinction level and not because there is a facies change. Because Foraminifera are facies-dependent. Some bugs will not live in deep water. Some form in from a little shallow water. Others will only live in shallow water and not in any moderate depth water.

So, we can define, for instance, coastal bays and lagoons as a very distinctive fauna, whether it is today as it was millions of years ago. It is the same basic fauna in that environment. And the same thing is true as you go out into the shelf area itself. There are different species, but they have homologues or lookalikes in today's shelf. So, there was a lot of research done on today's Foraminiferal distribution on the shelf in the Gulf of Mexico. It was done by Shell Development's research group here in Bellaire. I went to it, stayed there for a while and learned some of that stuff, too. It was very interesting. Beautiful little shells.

TP: But until you get out, further out on the shelf, there is not much change in the geology?

JL: There is no change at all. It is just younger rocks. But as far as the mechanics of depositing those rocks and the interplay between the ocean and the shoreline, that is the same. There is no change. The growth faults act the same way. You get expanded section on the downturn side, blah, blah, blah. You get a rollover into the fault which forms a trap. There is no change at all.

TP: We see people in the late 1930s and into the 1940s who worked in South Louisiana said, 'there is no reason why there are not salt domes and production out there in the water. We just have to be able to' . . .

JL: Yes, go out there and get them. Absolutely. I imagine salt dome exploration now is a lot easier and cheaper, too, at least now that you can use directional drilling. Because when we were drilling, that really did not work except for a small kickout just to get around some problem in a few hundred feet. But now, you can go out so far. Of course, that is what makes some of that deep water drilling possible because you can drill from one spot and go out and reach all of your targets without having to move the rig.

TP: Right, and then tie them back from a subsea well.

JL: Right. There is a tremendous difference.

TP: It is amazing. Did you work the 1967 offshore sale, too, or had you left by then?

JL: I was either left or leaving. I cannot remember. Blake Taylor followed me in that job, and Ed Picou followed him in that job. I think I may have been gone. I am pretty sure I was gone. I think I went over to research. I am pretty sure Billy Flowers was division exploration manager. He was still in that slot when I left, and James Hohler was the area manager.

TP: We have talked to Billy. He remembers the sales and what the discoveries were in each sale.

JL: All that, yes.

TP: And the stories about actually going to the sales. Those are . . .

JL: I never went to one of the Gulf Coast sales. I went to a Michigan sale one time when they did it as an auction. It was on the floor bidding. You would raise your hand. Strange. It was not sealed bids. It was open voice bidding on the floor with

a bunch of people working the floor and with a man on the platform all fancy talking to get your blood running!

TP: I guess you do not leave a whole lot of money on the table that way like you did in a sealed auction?

JL: I guess not. There was a lot of money left in some of those Gulf sales. A tremendous amount, I was telling you about, that left a lot of money on the table. Boy! Whoa, did it! It made me kind of feel good! You always like to be justified.

TP: Yes, you were talking about nothing bothered management more than dry holes but money left on the table really got in people's craw, too.

JL: Yes. I was never involved in all of the economics of the sales. That got to be very sophisticated.

TP: I think the 1962 sale was the first time that they brought in some really specific, quantitative studies of bidding history patterns . . .

JL: Yes. There was a group of mathematicians at the lab.

TP: Don Russell, I talked to, who worked on . . .

JL: He was a bright man, brilliant man. And funny. He was a production geologist, a production engineer, to start with, but he was very, very bright.

TP: He lives over in Tyler. I have talked to him.

JL: Yes, they had some fuzzy-headed, wild-eyed mathematicians working on that stuff! I could not communicate with them very well!

TP: Bankston is another mathematician and John Redman. Both very competent, extremely brilliant men who did all that work out of the E&P economics. So, who else do you remember from your time in the offshore division? Were you there when Bouwe Dykstra was there?

JL: Oh, yes.

TP: I guess he retired not soon after . . . he retired in the early 1960s, I think.

JL: He was there for the 1962 sale. I remember that. Of course, Ray Ott. I cannot remember the area staff too well.

TP: Bibi Houston, I guess, was probably still there, wasn't he?

JL: Oh, yes. B.S. was there. And I mentioned Cos van Wijnan. Don DeSautel was in the division. Joe Broussard. Bob Collins. Ray Thomason. Those were all division people. We had a gravity man. I cannot remember his name.

TP: Tom Hart is on this organization chart.

JL: He was on the area staff.

TP: He was there but he was not probably known at that point.

JL: He was on the area staff. He came in with Hohler. He and Hohler and Thomason were a team, and they were on the area staff. Thomason had been in the offshore as a geologist earlier, and then he went off somewhere and came back with Hohler. One of those two, Thomason or Hart, was the area geologist. I cannot remember which was which.

TP: So, what did you do in research when you went to research?

JL: Fiddle-farted around! There is no place in research really for paleo, not applied paleo. I tried more or less to get paleo into the computer side of the business, so

some of this data could be more visible. You know, we could generate printouts that would show things to someone. I looked for a better way to store it because all our data was stored on paper. It was on paper logs about so wide and however long you need them at scale. And you examined samples every 30 feet and noted your findings. And I tried to set up a program, and I made the mistake of trying to set up a program that worked for everybody – the West Coast, the Gulf Coast and any other place, and that is an impossibility. But in those days, we were using mainframe and punch cards, and that was an impossibility. They brought in some programmers, some of which were talented and some of which were not.

TP: Well, at the time, they were trying to reorganize their whole information computer system and trying to convert to a centralized database. I know there were nightmares . . .

JL: Oh, yes. We never really accomplished anything. The pollen people used the programs. The pollenologist was able to use the program and he is the only one. The West Coast had so many bells and whistles they wanted to put in the program, it just wrecked it. They had a different way of doing business out there because their basin was different. They have a much more complicated basin structurally and stratigraphically. They do a lot with turbidite deep water flows. And they do not have the nice clean transgression and regression story that the Gulf Basin has. But the paleo they developed was very useful out there. They

use it quite a bit. I am just glad I never got involved in it!

I was kind of the head office paleontologist for a while. I would go around to various divisions and find out what they were doing and how they were doing it and to come up with ways to do it better. And one of those ways I thought at the time, was the computer. Which, with the mainframe and the punch cards, just would not work. It could work better now with your PC sitting on your desk. And they put a lot of reports on the computer to where anybody in the office can pull the report up and not have to go fumbling through a file somewhere or go to some paleontologist and ask him, 'What have you got?' He can actually look up the report by himself. That is about as far as it went.

TP: So that probably led to your involvement with Pecten?

JL: No, I went from head office to Midland. And then from Midland, to Pecten. And while I was there, I was able to go overseas to Indonesia which was a real adventure.

TP: I am still doing the Shell history and Pecten is part of the chapter. I am interested in that whole story. So, you were mainly in Indonesia? Did you go anywhere else?

JL: Well, yes. When I came back from Indonesia, I guess it was . . . I worked Central America: Guatemala, Honduras, Costa Rica, Nicaragua, but not Mexico. And then, also some in South America: Bolivia, Peru, Argentina, Venezuela a little bit. It is all almost library work because you did not have any data. You would run around to whatever sources you could find to put together some sort of picture of a regional basis basically – very broad, brush type of stuff.

I remember I requested a trip to go to some of the geological surveys in some of these countries to try to get some data. I wanted to bring back some samples or see whatever they would show us. They are not allowed here, and I finally found out why – because the group did it. They went on a long whirlwind tour of South America and mined everything they could for them over there. Pecten was always . . .

TP: An uneasy relationship.

JL: Yes, a very uneasy relationship. I do not know why in the hell they started that crap. It was kind of silly.

TP: It was because of the lawsuit, the minority shareholder lawsuit.

JL: It really was silly, pretty silly.

TP: There were some areas, Cameroon, I guess, that did okay.

JL: Yes. Right.

TP: Syria. I mean, you were just getting a late start.

JL: Yes, and you are competing with everybody. You had to tell your parent company what the hell you were doing. They did not have to tell you. They did not have to tell us everything that they knew. We kind of had to tell them everything we knew. So, it was kind of a one-way street.

I know I was seconded or loaned to the group when I was in Indonesia. They sent three of us over, two geologists and a geophysicist. I was in a group office with a Swiss group manager and a general manager who was Dutch, and a potpourri of nationalities working for the group. They were Australians, Austrians, Dutch, Germans, and whatever. Their work ethic was altogether different from ours. But then again, they were working on a different scale. It was a large scale type of thing. And they would come up with some of the most obscenely stupid ideas for drilling a well I had ever seen.

They actually drilled a well in Kalimantan on the island of Borneo back in the

jungle. They had to air lift the rig in with a helicopter. And they were drilling on a type of structure that was evident on the other side of Borneo offshore. It was apparently not a good structure to drill on because I was involved with offshore Borneo on the north side, on the Sabah side. I was the Pecten man for that, and we did a lot of geology and geophysics, and a little bit of paleo, I guess. But they set up a huge camp in Bali Papa. They love to do that. They set up a big Shell camp. They had one on the other side of the island in Brunei. They set up a big camp, staffed it. They had a shooting program, they hire hundreds of natives, and subcontracted to a local boss to go out and cut line . . . I mean, it was a huge operation. Anyhow, they nailed that to the rig, and I do not think they got down more than maybe 1,000 feet or two and the whole thing blew up on them. I mean, the rig collapsed. One of the people got some sort of jungle disease. They had to send him home.

TP: They did works projects to stay in good with the political bosses.

JL: Well, there was a lot of that going on. There surely was. There was a lot of politics with the local Indonesian people. But to us Americans, it was just absurd some of the stuff they came up with. But that is the way they operated, and they have been pretty successful over the years.

TP: That is why they probably started operating Royal Dutch way back.

JL: That is exactly right. That is the type of thing they started with huge camps.

TP: In Indonesia?

JL: Yes. Everywhere. Everywhere they went overseas. I remember when I was in Oman visiting somebody, and they had a huge camp they had on the seashore. And the staff had a sailing boat to use, they had a motor boat to use, they had tennis courts, a golf course, nice bungalows, so many days paid home leave, and extra bonuses for working overseas. I mean, it was quite a thing. Quite a thing. But you are right. That is the way they started out was the huge investments and huge numbers of people doing things. It was really funny. Sometimes overlooking the obvious, it seemed like. You know, the simple stuff, they seemed to just whiz right by and dive right into the hard stuff.

TP: And they were always stronger . . . I guess the group as a whole has always been stronger downstream.

JL: Oh, yes. Absolutely.

TP: They bought a lot of their productive properties and they were really not as . . . there always seemed to be a stark contrast between Shell Oil USA and the Group.

Shell Oil was an exploration minded company.

JL: Absolutely. That is a huge difference, and in the mindset of the people in the two companies. When they come together, it is a difficult chasm to build a bridge across.

TP: They were explorationists, too, but they were just not as well known as say . . .

JL: Well, they worked at a different speed, a different level. Like I say, so many of them were Ph.D.s. They tended to write copious reports. They turned out more reports per person than any other Shell organization with lots of illustrations and a lot of drafting. God damn it, they had a huge drafting department in Indonesia – all locals. I ended up doing two reports: one on Java and one on Sumatra, Java and Sumatra. The Java Sea side of Java, which was very productive and Shell was not in it. It was Arco at that time. Arco was the big operator there. They were a failure.

They did not do squat. They liked to be there because, remember, it used to be a Dutch colony and a lot of the guys from Holland, a lot of the Dutch geologists, felt it still was. They really did. One guy would talk like that. At some instance that happened between say, he and the police, and he just took over. He was in charge. He was the Dutchman in charge. You could see it in his face, in his

expressions. And a lot of Indonesian languages use Dutch construction in words. So, the Dutch move in easily to an Indonesian lifestyle.

Let's break for lunch here.

THE END