MMS OFFSHORE GULF OF MEXICO

ORAL HISTORY PROJECT

Interviewee: MINER LONG

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Bio

Miner Long working for Shell shortly after he graduate with a degree in physics from Oberlin College in 1948. He took Shell's geology training course and ended up on a seismic crew. He went back to school and did work at John Hopkins in geology, and after graduating in 1953 he rejoined Shell as an exploration geologist. He served as in Denver, Tulsa, Pittsburgh, Houston, Lafayette, and New Orleans during his career. He joined the offshore division in New Orleans in and took part in the 1967 lease sale. Shell tapped him to be chief geologist for offshore in 1968, and he served in that post ion until 1976. He became Shell's fist general manager of geology in 1977 and served in that capacity until his retirement in 1984.

Summary

This interview covered geologic trends and trap analysis for the offshore Gulf. He spent an interesting period in Pittsburgh experimenting with recovery techniques. Discussed the 1967 lease sale and its failures and the 1968 Texas Sale. Long also described the role of geology, bright spots, and the Arco affect in the 1970 sale.

Side 1

TP: This is an interview with Miner Long at his home in Houston, TX. The date is July 16, 2003. The interviewer is Tyler Priest. Let's just start with a little personal information.

ML: Well, I would say my technical background began . . . I was a physics major at Oberlin College and I graduated in 1948. I was originally in the class of 1945, but I spent three years in the Army. I had a minor in geology in Oberlin and got an offer from Shell Oil Company when I graduated to join their geophysical training program in Houston. Because I was also interested in geology, I thought that sounded like a pretty interesting assignment. So, I went to work for Shell at . . . My first assignment was on a seismic crew in Shreveport, Louisiana, which lasted for about three months. Then I was sent to Houston where I took instrument course from Joe Niles at the laboratory on seismic instruments as well as theory and practice of seismology under principally [F.A.] Van Melle, who taught that course. Then, I was assigned to head office geophysics.

At that time, Shell was divided east of the Rockies, west of the Rockies, and this was east of the Rockies.

TP: Who was the chief geophysicist at the time?

ML: Goldstone. Goldstone is the one who hired me. He was chief geophysicist. At any rate, I went to work there for . . . I was assigned, first of all, to take a course in theory of seismology. It was a book that a Shell geophysicist had written and I cannot remember his name. I will think of it in a little bit. It was a very good book on the geometry of refraction seismology, reflection seismology. He had the interesting characteristic of writing down the proposition and then saying, 'it can be derived at so and so as the result.' This year, before they had the geophysical trainees go to the University of Texas to take a course in the theory of seismology. It was taught to them. They decided that four of us: Scaife and myself, and two others who are no longer with the company, took that course. We were put in a room, the four of us, with lots of efficiency pads and pencils and calculators. We were told to work through that book in, I think, two or three months or something like that. It was extremely difficult. But, by the time we got through with that book, we knew quite a bit about the geometry of refraction seismology. It was an interesting book. And then, I was assigned to gravity in that regional office under . . . I cannot think of . . .

TP: Was it in research?

ML: No, I am now in Houston. Shreveport was only about three months. Then, I came to Houston and took the geophysics course. They sent me downtown after I got through with that geophysical course to, I guess you would call it the regional office

then, or East of the Rockies. What was that guy's name? At any rate, I worked in gravity for about six months, which was kind of the armpit of geophysics! I mean, gravity had been a powerful tool but now with seismology . . .

TP: I understand even during this time offshore, you could not get good reflections so all you had . . .

ML: Right. All you had was gravity. But gravity was important. The leading thing right then was seismology. But, at any rate, I was in gravity for about six months. And then, I was assigned, at the end of my first year as a trainee, I was assigned to a seismic crew in Edinburg, Texas, and moved to Edinburg, Texas. Two weeks later, the crew was moved to Harlingen. We were there about six months and moved to Corpus Christi. And then a crew was moved to Beaumont. That was all in my second year.

By that time, I had decided that . . . I was mapping. I was taking geophysical records from the crew in South Texas mainly, and the mapping growth faults . . . did not have that . . . growth faults were not known at that time. I did not know what they were. They called them at that time faults with reverse throw because the beds dipped into the fault. And I knew that there was a geologic problem here that I could not understand. So I decided I had a lot of GI Bill, and I went back and decided I would go to school. A leading structural geologist was Ernest Close at

Hopkins, and I had known of him. So I went to Hopkins with the idea of one year of graduate work, but I got "hornswaggled" into staying. I could not get out! But in the course of that, although the problem I was assigned appeared to be a structural problem, I got into it and it was a problem of folded carbonated rocks – Upper Cambrian carbonated rocks. I decided, after one year of field work, that the only way I could understand the structural problem which involved the history of the rock was to understand how the rock was actually created. So, I dug into the origin of this carbonated rock. It became a very important early paper. It was never published, but it was a very important early paper on the origin of carbonated rocks.

It was interesting . . . at that time, Ginsburg, whose name you probably have heard . . .

TP: Yes, the carbonate specialist for Shell.

ML: Ginsburg was with Shell, and had opened a laboratory down in Florida. I was a student of [Francis] Pettijohn's who had just come to Hopkins from University of Chicago. Ginsburg was a Ph.D. candidate at the same time under Pettijohn from the University of Chicago. And, of course, Nanz had been a Ph.D. candidate under Pettijohn before that. So, Pettijohn made me aware of what Ginsburg was doing and gave me a draft of his dissertation to look at and comment on. Well heck, he had pictures down in the recent carbonated rocks of Florida and the Bahamas that were

very much like mine in the Upper Cambrian, which is four or five hundred million years old. So, that was an important marriage in my mind and, of course, in many minds. That is why I think that the research that Shell was doing at that time in BRC was really groundbreaking and important in training our geologists in how to examine those sedimentary rocks and how to predict what they might do.

Anyway, I finished Hopkins in 1953 and wrote back to my former boss at Shell to see if I could come back, and Goldstone wrote me and said, 'sure.' So, they rehired me and I went to work on the geologic training program in Fort Collins, Colorado, surface mapping for about three months there.

I finally got through the training program, much of which was taking courses at the laboratory. I wound up in Wichita Falls as an exploration geologist. I was there five years. Our two children were born in Wichita Falls. I was transferred to Oklahoma City, and I became division geologist in Oklahoma City for the Anadarko Basin which, at that time . . . was a pretty big organization. I was transferred in 1959.

In 1962, I had a bad interview and presentation before McAdams! It was not uncommon to have bad interviews. I had a bad interview with McAdams. Bookout was the regional manager in Denver at that time. Thanks to Bookout, he took me up to Denver on his staff there and kind of hid me for a while. [laughter]

TP: I understood he did the same thing to Tom Hart.

ML: Yes, well, Tom Hart was up there, too, being hidden! [laughter] That is where I really got acquainted with Hart for the first time. Casper Arbenz was assigned, I think, to check out my credentials, but, at any rate, I had a fine time in Denver.

TP: I know a number of people who had bad interviews with McAdams.

ML: Oh, yeah!

TP: Got crossed up for who knows what reason?

ML: Well, that is right. It was very complex but at any rate . . .

TP: So, you ended up in Denver?

ML: Yes, I ended up in Denver and had a wonderful time because I spent . . . I was essentially given free reign. I made regional maps, geologic maps of the Rocky Mountains, got acquainted with Rocky Mountain Oil and Gas. It was great. And, at the end of less than one year, I was transferred to Pittsburgh.

TP: Pittsburg? I did not know Shell had an office in Pittsburgh.

ML: Yes, we had an office in Pittsburgh. It had been recently opened, about 1962, probably open about one year before I got there, 1-1/2 years. Pittsburgh, the reason for that office was if you found gas in Oklahoma, you have got 25 cents a thousand cubic feet. If you found gas up in Pennsylvania or New York State, you would get 50 cents. The problem is there is probably more gas in Oklahoma than there is [laughter] . . . It is a little easier to find than up in New York State. But, at any rate, we went up there with the idea, because there is gas up there. So, we opened an office up there and I was sent up there, as still part, I think, of Bookout hiding me!

I had a fine time up there for 2-1/2 years because I got acquainted with the whole early history of the oil business. It started in Titusville, just north of Pittsburgh. They were really great years, those when I was being hidden because nobody much cared what I did! [laughter] I spent my time making maps of oil and gas distributions and geology and so forth and so on. I got very well acquainted with that world. I guess you want all this, is that correct?

TP: Yes. I am interested in all the geologic work that Shell was doing.

ML: What happened was that we decided, the manager up there, that perhaps those old oil fields still contained a lot of oil, and they did. They were discovered in the 1800s and early 1900s. Water flooding came in and got the remaining oil, but by the 1950s

or after the war, there were other methods for extracting. There were tertiary methods for getting more oil and gas out – the CO2 floods and all kinds of new techniques. So, the problem was to find out if that would work; which he wanted to find out whether it worked without anybody else knowing it so you could buy up the oil fields. You did not want to buy the oil fields and then find out if it would work. So, because I was in Siberia, more or less, I was picked to be the geologist for a drilling rig that was going to drill nine wells up there in old oil fields under another name. It was called Twin Exploration Company. It was a drilling rig that was equipped to drill with air, which nobody had seen up there. Shell's name was not attached to it at all. I went under the name of Barton Long. Everything I did was cash. I paid cash for my rent car. I paid cash for everything. The land man that was associated with it, he was pretty much under cover too, was John Marshall. And then, it was an outside contractor that was hired that had the rig that we used.

We drilled nine wells up there from roughly spring of . . . 1963 . . . Kennedy was shot in November of 1963, so it was the spring of 1963. My last day in the field I was eating in a restaurant in Franklin, Pennsylvania when word came that Kennedy was shot. So, that kind of brackets it. But after extensive analysis of those cores and so forth and so on, they did not quite meet the economic criteria.

TP: They were trying to do secret studies on steam ejection in California at the same time.

ML: I spent six months in northwestern Pennsylvania. I am a geologist. I like to be outside. One of those wells, right in Titusville, right next to Titusville . . . part of the thing. It was an extremely interesting assignment to . . . I could go back in those woods and there were just old driller's logs and stuff, old sheds and all this old stuff. It was an interesting assignment. And then . . .

TP: But the problem was that the fields were not capable of . . .

ML: The porosity and permeability parameters, which were key, were not quite high enough to justify these tertiary methods. They just were not quite high enough. Of course, you knew it was marginal going in, or would be sitting there but it was a good shot. It did not cost much money. I mean, people were coming for miles to see that rig. Nobody had ever seen anybody drill with air. Most of those wells have all been drilled with cable tools 100 years before. As a matter of fact, a TV station came out there. It scared the wits out of me! [laughter] You know, I just had to leave! The last thing I wanted to do was be on TV!

At any rate, it was an interesting assignment. Then, the next thing I knew I was transferred in 1965 to Lafayette, Louisiana . . . Lafayette was an onshore Gulf Coast division. I knew nothing about Gulf Coast geology, absolutely nothing! I had been in the mid-continent, Rockies, East Coast, Pittsburgh, but Gulf of Mexico was a

different animal, a very different animal. And with trepidation because I had a family, son and daughter, and we were particularly concerned about schools going down to southern Louisiana. You know, that is your first thought. Well, we got down to Lafayette and by golly, both kids got into very satisfactory arrangements. My daughter wound up in an Episcopal school, a private Episcopal school, but it was excellent. My son had a fine deal. So that worked out.

We thoroughly enjoyed . . . we were in the motel when Betsy had . . . so I just stayed another night in the motel while that thing hit. So, that was September of whatever. They announced the closing of the office on December 11. By that time, both my wife and I had fallen in love with Lafayette. We thought that was the . . . We had a lovely house there. Everything was just great. So, we were transferred to Houston.

The reason for the transfer was they were going to consolidate the onshore exploration, Gulf Coast with the onshore that was already in Houston. There was another onshore division there. They just consolidated that gently in Houston. So, I spent a few months there in Lafayette. But, it was an important part in my deal because I spent my time . . . And again, I was still a little bit in Siberia. The division managers, everywhere I went, had kind of . . . 'do whatever I wanted to do.' I was functioning kind of as a division geologist a little bit but not . . . just because, at that time, I was somewhat senior – senior in the sense that I had, at least, been around.

The important thing I did there was I met Urban Allen, and the reason for it being important was that I had to understand what it is you had to do to find oil and gas on the Gulf Coast. Clearly, I knew something about it elsewhere, but how hydrocarbons are trapped, what is a good trap, what is not, what is a good prospect, what is not – how do you do that? Well, it was extremely murky.

TP: Yes, even by 1965, after all the . . .

ML: Oh, yes. It was extremely murky. You mentioned right up here that an important part of the thesis was that these were deltaic sediments, and the source rocks were embedded with the sediments so that you have to worry about source rocks. And you would have to worry about migration because it was all there. Traps, though, were very difficult to find . . . There were various myths about what a good prospect was versus . . . I had enormous difficulty trying to sort that out.

TP: Because the traps from salt domes were difficult to pinpoint and they all were faulted . . .

ML: Well, yes, they were faulted traps, for the most part. And some apparent fault closures were valid and some were not. Urban Allen was, I thought, one of the brightest geologists I have ever known. He was very low-key and very soft-spoken person. But he knew a lot, and he knew there was a problem. The first thing you

have to know: 'Is there a problem?' Many geologists down there did not really realize there was a problem. You mapped your faults and your prospects, and some worked out and some did not.

TP: He was in Lafayette when you came?

ML: He was in Lafayette. We were both transferred to New Orleans. But it was not long after that . . . I would say I transferred in January and I would say, June, I was transferred to offshore division.

TP: June of 1966?

ML: 1966. Maybe it was late summer. I cannot remember. It was somewhere in the summer of 1966. By that time, Hart was essentially chief geologist for . . . no, Hart was not. Tom Connally – you have gotten his name, right?

TP: Yes.

ML: He became division manager . . . for offshore division about the time I went there, or just before. And Connally I knew from – he was in Tulsa when I was in Oklahoma City and Wichita Falls. So I knew Connally, and I think basically Connally asked me to get over there. Hart was probably still in Denver at that time, I think. But at

any rate, I came over to the offshore division to work on the 1967 sale. I was the new kid on that God-damned block, I will tell you! [laughter] I really did not know what you did with . . . how you identified good prospects as opposed to poor ones.

Urban and I worked hard on it. I think we basically went into this sale . . . We felt that there was some value in early structure. In other words, if there was evidence that the structure was there early, it was much better than if the structure was late. In other words, in this hypothesis, it was there to collect oil and gas for a longer period of time than a late structure. And you could map the timing of those structures and the closures. And that was the principal criteria we went into the sale with. I would say that is right.

The other aspect . . . There had been a massive statistical study of oil fields in the Gulf of Mexico under the leadership of Claude McMichael. Have you run into that study before?

TP: Was that in Shell?

ML: Yes.

TP: Well, I know that for the 1962 sale, Ronnie Knecht and Jerry O'Brien . . . is that the study you are talking about?

ML: No, that was an earlier one. Then there was a later one that built on that but it was much more extensive.

TP: I have not talked to anyone about that.

ML: I have got to think of where Jerry O'Brien was when the 1967 sale . . .

TP: He might have been over in the North Sea.

ML: Yes, I think he was. This study was under Claude McMichael in the offshore division under Billy Flowers.

TP: Billy might have mentioned it in his interview but I cannot remember. It has been a while since I looked at it.

ML: It was a massive statistical study. It compared salt domes and this, that, and the other thing. He developed a regression analysis, if you will, of . . . I could not reproduce it, but J.T. Smith can give you a lot of information about that because that study was the key analysis as of late 1966. I think Connally had very, very big reservations about that because you gave so many points to . . . You weighted the source rock, you weighted this, you weighted that and so forth and so on, and you

got a number out on the prospect based on this statistical analysis of all these oil fields. There is a famous letter by the chief geophysicist of New Orleans at that time, and he pointed out very carefully, just very succinctly, that the problem with oil and gas is if any one of those parameters go to zero, you do not have an oil field; whereas, that zero would not nullify anything in this equation. It was all added in. But, at any rate, Connally . . . I think, principally J.T. and I and Urban Allen were working on how to go about evaluating the sale and the statistical analysis was thrown out. There was a lot of good information in there but as far as making a judgment about a bid, it would be dangerous.

Well, at any rate, we went into the lease sale. Incidentally, Jim Hohler was the regional manager at that time, and a powerful figure. That early structure was very important in his whole way of looking at oil and gas as well. So, he was a powerful proponent of that. We went into the lease sale in 1967, and, as you point out, we had some successes but our biggest bid was that Prospect 370. My recollection is like $33 million for those tracks.

The chief geologist at that time . . . By the time of the sale, Glen Robertson was chief geologist. Glen had just bought a Mustang for $3,300. Every time we were discussing bid, he would always translate everything into so many Mustangs! I think, if I got the numbers right, $33 million is 10,000 Mustangs or 100,000 Mustangs. All the bids were translated, as far as he was concerned, into how many

Mustangs! But we bid $33 million for that sucker and it was noncommercial. That was a major trauma in exploration.

TP: That was over on the east side?

ML: That was on the east side.

TP: East side of the Delta?

ML: East side of the Delta.

TP: What area was that? It is not Main Pass . . .

ML: No.

TP: It was further north than that?

ML: By Breton Sound. It is east of . . . it is in that area. I do not think it is Main Pass. I am fuzzy about that.

TP: We can look at that. We have maps to figure it out. What was the failing in the analysis?

ML: The failing in the analysis . . . You mentioned in here that Mike Forrest saw shallow bright spots there later. I remember that somewhat later. That was one of his observations of bright spots. But it was a folded anticline, and it was a sandy section without much closure. It was not much overall. Overall, there was a lot of closure but when we looked at it later on we became very conscious that faulted sandy sections were very dangerous. I go into that a little bit here.

One of the fallouts of that sale was this faulted sandy section problem.

TP: It was very traumatic for the . . .

ML: Very traumatic. A lot of soul searching. Hohler basically was transferred to head office and wound up . . . he was there for a year or two, but left for Mobil and Sohio, and so forth. Jim Wilson was vice-president and he had written a letter on the eve of the sale. Did Billy ever mention that?

TP: Is this the "lead pipe cinch" story?

ML: The led pipe cinch. That was horrid. Well, Jim Wilson had written a letter on the eve of the sale in which he expressed a lot of reservations about that prospect, and his logic was extremely good. Nobody paid any attention to him . . . He went to

Denver as vice-president after that . . . I have got to think about that. Do you remember who followed Wilson as vice-president?

TP: I have it down somewhere. I cannot remember.

ML: It was Bookout because the Cox well blowout and Bay Marchand were all under Bookout. Yes, Bookout came down from Denver. I could mention that by this time, people were starting to say that the New Orleans region was taking on a "Tulsoid" look. Connally, Hart, Bookout – all came out of the old Tulsa area, Oklahoma City, as opposed to the Dutchmen who ran New Orleans before.

TP: Dykstra . . .

ML: Yes. So, by that time, Mac has finally gotten New Orleans under his hand. So, the management changed quite a bit in that period. Well, so did my fortunes. After the 1967 sale, I was made division geologist for the offshore division. That was with McAdams' approval. After four years in Siberia, I got a raise! [laughter] You know, I was never bitter. Everywhere I went, I had great learning assignments, great assignments – just what I wanted to do. So, at any rate, I became division geologist.

How do I go from here? Well, we did a lot of examining of post 370 of how to look at prospects in the Gulf of Mexico. Importantly, the next big step was Mac had to

prepare for the 1968 Texas sale. I do not think you mention that in here.

TP: Well, I only mention it in the sense that is where Mike Forrest really started to catch on to the bright spot phenomenon. I think 1968 Texas is where he said he first saw . . .

ML: Is that right?

TP: Yes. He mentioned that in 1967 but the 1968 Texas sale is a gap in what I have written so far.

ML: Well, I tried to cover some of that here. Let me try this. Well, what Mac did was, in the early spring of 1968, he had me assigned to the Texas offshore division temporarily. I was division geologist in the offshore division which helped them in preparation, and my success at Prospect 370 must have . . . at any rate, be that as it may, I was sent over to Bob Chuoke's division, offshore division, Texas, as was Mike Forrest and J.T. Smith. So, we went over there to help out Chuoke who really, I am sure, thought he did not need all that help. But, at any rate, we went over and helped him. I got acquainted with another part of the world.

Texas was different. It had large faulted structures on the Texas offshore. Up until then, it had been lightly explored. There was one important thing that came out of

that Texas sale. I brought Urban Allen over because, by that time, I knew if we were going to get any insights, there was a good chance it could come from him. And so, I got Urban Allen over there. His major contribution, and it had a lot to do with some future lease sales, was he built an assumption. He said, 'O.K., suppose I assume that fault traps that have an impervious shale across from them will seal, but if there is a reservoir rock across from this fault, it will not seal. He constructed this hypothesis and went around testing it, and we wound up deciding that was the important problem at Prospect 370. I had rounded up a group in offshore division because I was concerned with it over there as well. I had gotten a group together to test that under Leighton Steward, and to get a collection of oil fields and so forth. So, it was early and probably not tested.

TP: So, the hypothesis was if there is a . . . can you say that again? There is a reservoir rock across from the . . . .

ML: Do you understand the configuration of a fault trap?

TP: Yes.

ML: The beds dipping up into it? And there is maybe half an anticline cut off by a fault. So, the hydrocarbons go up to the crest of that anticline, and if there is enough, fill down to a spill point. Typically, the spill point has always been the syncline on the

down thrown side of that fault. Ervin was proposing that the spill point could well be because there is sand stone across the fault and that the fault leaks. And basically, the working hypothesis for post prospecting was that faults are seals. If you have got a fault and you have got a trap against the fault, then that is what you are looking for. And what Ervin was saying . . . well you know, it depends upon what is across the fault. It was a hypothesis. But the more we looked at it, the more we thought, we had better pay attention to that. It was clearly a problem at 370. It was a very sandy section. We had always been worried about having a seal above the reservoir rock in the anticline in the fold, but we never worried about having a seal across the fault. So, this was a flexible deal. So, between Ervin and myself, we pretty much convinced Birch Kennedy and Bankston . . . Birch Kennedy was exploration manager. Bankston was vice-president. That was a way to look at those structures, and those structures were big faulted anticlines. You know, you could draw a big oil field on those things. But if you looked at it, it was a sandy section. If you looked at it with all those faults, all of a sudden, you got very spooky about it. So, what we did was we drew very eliminative traps there because we knew enough about the stratigraphy to kind of predict more or less where the sands were. So, we went into that sale with drawing limited accumulations compared to the size of the structure. The second important thing in that sale was, of course, J.T. Smith was over there. Parallel to our work, J.T. was working on source and migration, and he concluded that the source rocks were so deep and hot that there was a very high probability that hydrocarbons would be gas. And gas was not worth very much. And so, in that

sale, we went in and were outbid by Texaco and Exxon by 7 to 1. They clearly were betting on big fields, big preserve, big structures, and oil not gas. So, that came before that 1973 sale you mentioned.

TP: That is interesting.

ML: I remember the division manager was Bob Chuoke. He was just shattered by that sale because, you know, we had been outbid by so much. But it turned out there were just small non-commercial accumulations out there. So, we lucked out. So, we came out all right. I would say that fall out . . .

TP: So, your stock further rose with McAdams after that?

ML: Yes, it probably did not hurt! But the key to it was Urban Allen. The key was Urban Allen. It was his mind that did all of that. I did a lot of spade work there but he was the key . . .

End of Side 1

Side 2

TP: Well, that is good. I had not gotten that story.

ML: Well, Mike would kid about the division . . . 'Miner would put the area of trap down with . . . He would put a quarter down and put a circle around it!' [laughter] They were small. But, at any rate, we learned a lot out of that sale. We learned a lot out of Prospect 370. I mean, I do not know whether it was worth 33 million dollars, but we got something out of it.

TP: Well, it probably boosted your confidence for the key sales in 70 and 72.

ML: Well it did. Right. The name for what Urban did we called trap analysis, and trap analysis became extremely important in lease sale preparation essentially from then on because trap analysis . . . With bright spots, you just could not go just on bright spots. There were bright spots and there were bright spots. I think you pointed out some of that. In most sales, we had some understanding of the stratigraphy. We insisted on the geologist performing detailed fault plane cross-sections of all their prospects with the bright spots . . . In other words, we wanted to validate that the bright spots made sense. And furthermore, you could look for traps that bright spots would not get or would not see, that were hidden by shallow bright spots and so forth and so on. So, trap analysis, I think, has . . . I do not know if any subsequent

important analysis of entrapment in the Gulf of Mexico since 1968.

TP: Was Shell way ahead in this game?

ML: In my opinion, it was because Exxon and Texaco were not looking that way.

TP: Do you think they figured it out after . . .

ML: I have no idea. One of the problems was bright spots in the Gulf of Mexico. Bright spots became pretty damned important. You could do fairly well with good geophysics on those alone. I think, without any doubt, trap analysis was important in many of our judgments. We insisted that the geologists make fault plane sections to show us just exactly what they thought was across the fault line. It worked both ways. The iteration between the bright spots and the stratigraphy would give you greater confidence in what you were saying was an oil field.

O.K., that was 1968 Texas . . .

TP: Then there was a planned sale for 1969.

ML: That was canceled.

TP: And then, the next sale in Louisiana. Did you work that sale, too?

ML: Yes. Of course, I was temporarily in Texas until the 1968 sale. I came back from my job as division geologist in New Orleans and there was an acting division geologist sitting there who would not give me my office! [laughter] It was not quite that bad but nobody seemed to . . . I was given an office, and I was clearly waiting. What was happening is I wound up in a few weeks becoming chief geologist in New Orleans, replacing Ray Thomasson who was chief geologist there. Have you talked to Ray, by the way?

TP: No, I have not.

ML: Well, you ought to talk to Ray.

TP: Is he in Houston?

ML: No, he is in Denver.

TP: I am going to have to go to Denver for another business soon so I will look him up.

ML: Look him up. He is well worth talking to about that period. But at any rate, I wound up as chief geologist in New Orleans. By that time, of course, Bookout was vice-

president. Hart was division manager for the onshore division. Connally was offshore division. They made Connally stay and drill up Prospect 370. So Connally was slugging away there. But New Orleans had become quite "Tulsoid." At any rate, I was chief geologist from 1968 until I transferred to the western region in 1976.

TP: Those early 1970 sales were really important. Do you have any stories about those? I guess that was the first time that you applied bright spots.

ML: Yes, the 1970 sale was. Incidentally, I might mention, the first I knew about bright spots . . .

TP: Was from the lab?

ML: No. The first I knew about it, I would have to say it was a drainage sale for Bay Marchand. Now, do you have the date on that? I think it was 1968.

TP: 1968, yes. The first sale in Louisiana since 1962 was the drainage sale of 1968, yes.

ML: No, the 1967 sale was the first . . . There was the drainage sale in 1968. I cannot remember what month that was in but I was in New Orleans. I was in the offshore division. Mike Forest was the geophysicist working on that drainage sale. I

remember him coming down the hall to my office . . . 'Miner, come here. I want to show you something.' He had this section, 'Do you see that? That is the pay.' That was the pay at Bay Marchand. You know, it was just a slice like that but, you know, that little slice like that was many million barrels of oil.

TP: There was a well log study that he did.

ML: Yes, right. Well, this was before the well log study.

TP: It was?

ML: Oh, I am sure.

TP: This was still sort of theorizing?

ML: Yes. That is the first I ever heard of it. I never heard the term before that sale. I swear he called it a bright spot. Now you refer to the fact that somebody else made up the name bright spots . . . Maybe that is true but . . .

TP: Well, that was Mike's story about bright spots . . . It is not clear who actually coined the term and Mike does not himself remember . . . Maybe he did it.

ML: Of course, you remember all these things but I always remember him . . . 'Come on and look at these.' But yes, that 1970 sale was . . .

TP: So, were you convinced when he showed you that or were you really still . . . Were you skeptical?

ML: No, I was not skeptical.

TP: But it had to be verified probably.

ML: I do not know that I did any . . . I did not do anything with it. That was the curious thing. And at that point, I do not think he did anything with it. There is a period where you observe something and you know where it is, but you think there is a correlation there.

TP: Too good to be true!

ML: But I can still see that damned record. It really blossomed there. That is all right.

O.K., we go to 1967. The sale of 1969 was delayed. The 1970 sale was Posy, wasn't it?

TP: Yes.

ML: The one thing I remember, and, I think I am right about it being the 1970 sale, is that a couple of Arco people wrote a paper, in like the *Oil and Gas Journal* in which they analyzed bidding in offshore lease sales. I have forgotten the logic of the thing completely except that the conclusion was we bid too much. It became known as the "Arco Effect." The Arco Effect.

TP: The bids are too high?

ML: The bids are too high. I think I am right about this. This time, Charlie Blackburn was vice-president in 1970. That was his first sale. Charlie became very conscious of this Arco thing. I cannot remember details about those prospects or bids. We had a pretty elaborate rigormoral that we went through presale meetings in which the prospect geologists, and geophysicists would present their prospects. It was detailed. I and the chief geophysicist would beat down the divisions and, prospect by prospect, walk through those suckers before they were presented to the vice-president. So we had asked all our filthy questions before it was presented to the vice-president. There were pretty substantial quality control meetings. That went on through all the sales. I think they had a lot to do with our maintaining a high quality . . .

TP: The people who were pushing the prospects wanted more money . . . I could tell you stories about Hart and Flowers. Billy always said 'You have got to put more money on them.' Hart would reply, 'Well, how much do you want to put on it?' 'Put another million on," Billy would say.

ML: Right. I functioned as the leveler in all that. I was, 'On the other hand, here is the problem.'

The Arco Effect. I can tell you this little story on Charlie just because I think it is kind of interesting. We went through all the bid meetings and so forth and so on. Finally, the vice-president and the division manager all kind of agreed on a bid level. On the 1970 sale, Charlie brought me up to his office, and I brought my slide rule. What he wanted to do was examine the Arco Effect on these bids because they were all finished. What in effect the Arco thing said . . . well, you know, you ought to discount it so much. I have forgotten what it was. It was six-tenths, seven-tenths or something like that for the Arco Effect. So I am sitting up there with Charlie running the slide rule and we are going down there prospect by prospect. [laughter] And some prospects, I would take the Arco effect, give him the number and like that. But some prospects, he would not accept the Arco Effect! [laughter] I thought that was kind of interesting. The Arco Effect. The Arco Effect. Some prospects he had a good feeling about. We did not cut that bid by 20% or whatever it was.

TP: Were those the ones with bright spot on it?

ML: I could not tell you now, and I never made an analysis to see what the effect was. I should have done that. I do remember sitting up there in that office with that slide rule. That was right at the end of the slide rule. It was that year or next that the HP35 came out and wiped out slide rules all over the world. But I still was running that slide rule for Charlie.

We did, of course, the 1972 sale . . .

TP: To go back to 1970, I think Mike told me that Shell used bright spots, but you did not push it as far as you could.

ML: Well yes that is right. One of the problems was this: The problem with gas versus oil and the problem of the response of a poor reservoir versus a good reservoir. Let me see if I can put this together . . . Gas was much brighter that oil, so that you had an oil/gas ambiguity. We are in the early stages of understanding what we are looking at. Then would come the question: 'O.K., if you have got bright spots here and no bright spots there, should you react that way?' There was a lot of ambiguity.

TP: You were being misled?

ML: That is right. There were a lot of ambiguities at that stage. As our geophysical measurements got better and better, which they did, a lot depended on our understanding of the physical properties of the rocks themselves. We got much better information over a period of time on the rocks offshore. So that the whole thing became much better, and we should have bet more heavily in the 1970 sale than we did in particular. And I forgot what the problem was on Posy

TP: That was the Eugene Island 330 field that Pennzoil got.

ML: Yes, POGO. I think we were concerned about gas. That was the key question there: gas or oil? We missed it by just a little bit. I do not remember what our bid was but compared to POGO . . . but it was close. It was close. It was just we should have . . . that was a tough one. That was a tough one.

Boy, my mind, I just do not . . .

TP: That was an interesting sale, the 1970 sale, because it seemed like a lot of smaller independents really . . .

ML: That is where POGO was created and where POGO came in there with price premises that were substantially higher than the rest of us for the most part.

TP: I guess it was maybe the 1967 sale to 1968 Texas but, I mean, up until then, the majors – the Shells and the Chevrons, California Company – had really sort of taken over offshore Gulf of Mexico, but in the late 1960s and early 1970s, you had these . . .

ML: That is right, of which POGO was clearly the biggest.

TP: Yes, they took a jump.

ML: They did an excellent job of moving in, and it was not their territory.

TP: Yes, they did not know much about it. They did a crash course on Gulf Coast geology.

ML: That is right. They did.

TP: That sale first had an objective in what they called the Pleio-Pleistocene? Is that right?

ML: Right.

TP: The geology is not much different than the . . .

ML: That is right, we were just moving out. I was rid of the story in the 1970s and the 1980s. We just kept moving out. I am sorry I missed all the 1980s . . . I retired in 1984. All that really deep water . . . that is fascinating geology out there, I know.

TP: The turbidite sands?

ML: Yes, right. That is all after my time.

TP: That was after your time? They were not working on it in the 1970s?

ML: No.

TP: I mean, I know Shell had done some reconnaissance work with the *Eureka* back in the 1960s but that was not . . .

ML: I do not think the *Eureka* wound up having any impact on that deepwater exploration as such. I have got to think a little bit about the *Eureka*. One of the things – they did discover oil and gas seeps out there which was pretty significant. But there are oil and gas seeps all over the Gulf of Mexico. We knew about a lot of those. I could not articulate right now what the consequences of the *Eureka* were. Leighton Steward was on it. Bob Chuoke – well, he is dead, but Leighton Steward

was on the thing.

TP: I have been meaning to try to get to him.

ML: He could fill you in.

TP: He went to LL&E, right?

ML: Right.

TP: And Burlington Resources.

ML: He was involved in those 1970 sales.

TP: 1972 is when you really got a leap in the price of bids.

ML: Oh, yes. And there again, a lot on the bid price was based on how the company viewed price premises. Price premises were a terrible distortion that was going through the whole bidding process because how you valued – a sale opportunity had been very much on how you looked at the price premises. It was tough. Clearly oil versus gas, of course, was a big thing in the Texas 1968 sale, and that was the difference in price between the two. In the 1970s, particularly in the late 1970s,

price premises went up to $40, $50, $60 a barrel. I mean, I think a 10-year forecast was like $100 a barrel oil. I mean, it got very crazy in the late 1970s. Well, all that got quieted down pretty quickly. OPEC took care of that.

TP: So, you were in New Orleans until 1976?

ML: Until 1976, right. During that period, we had a couple of sales in offshore eastern Gulf of Mexico.

TP: Was that the Destin Dome?

ML: Yes, Destin Dome and so forth. I believe Leighton Steward was probably the sale manager for the first sale. Then, we had the sales in the East Coast. They were unsuccessful as far as finding hydrocarbons was concerned. So it was kind of the maturing of the shelf of the Gulf of Mexico, and everybody was working with bright spots. By that time, an enormous group of seismic surveys were available. So, there was a lot of geophysical coverage. A lot of data. It was a maturing of that period and some steps out into the deeper water as far as getting data is concerned.

I do not have any stories to tell about that period. It was moving pretty fast in that period. The lease sale at the lease station . . .

TP: I guess the challenge was building the platforms.

ML: Right. You bet it was. They did a marvelous job in backing us up in prospects that were in crazy places - deep water, and so forth and so on.

In 1976, I was transferred from New Orleans to the western region as chief geologist. I guess I was transferred some time around March or April of that year. I was a late participant in the Alaskan sale. I mean, I got in too late to influence it at all. Jim Jackson was exploration area manager. Then, there was the West Santa Barbara sale that came in about that time. But in January of 1977, Ray Thomasson, who was chief geologist, resigned, and Hart brought me over as chief geologist in the head office from the western region. So, I was just in the western region less than one year. So, I came over as chief geologist and one year later, I became general manager of geology which was the first general manager of geology they had ever had. I always maintained there was no way you could manage geology. And Gerry Pirsig was chief geophysicist at the same time. So, he and I were partners there for several years, a number of years.

TP: He was quite a whiz from what I understand.

ML: He was quite a guy. A very unusual guy.

TP: How unusual?

ML: Oh, Hart called him "The Buddha," and he was kind of like a Buddha. He was a very big man, a big man like the Buddha and smoked like a chimney. Of course, Hart and Blackburn did, too. I was in conference room after conference room with those three birds smoking like a chimney. They were sending out for cigarettes and getting more cigarettes. I am always sitting next to Pirsig or between Pirsig and Hart. So at any rate, that was my life.

Pirsig and I got along very fine. He was an extremely good guy. Very sharp. I have been very lucky. I have been surrounded by extremely bright guys.

TP: Especially Blackburn and Hart and Bookout. What a group!

ML: Spend your days with that gang, and, you know, your little head starts to swim. All that was extremely stimulating. That was one of the best things about my whole career was I was almost always associated in some way or other with very stimulating, bright people.

Billy Flowers was a wonderful man in all of his positions. I first met him as offshore division manager when he was chief geophysicist and so forth and so on. But an extremely delightful person. Well, you have met him.

TP: Yes. If there was anyone who deserved credit for the geophysical work that Shell was doing in the Gulf, it was Billy Flowers.

ML: Right. I have covered the Prospect 370. The other thing I wrote up briefly here, and I think you probably have already covered that, was what I thought might have been left out from . . . the major advances in the 1940s and 1950s – understanding origin, deposition and sediment of rocks. I wrote a little bit there because I was afraid you had left it out, but clearly you have covered that. I had never been assigned to the laboratory. So I was never inside that. John Smith spent his time in the laboratory, of course, as an observer of the geology group but as a chemist and geochemist.

TP: Who were the key figures . . . you mentioned Ginsburg.

ML: Rufus Leblanc. Nanz. Barney Wilson. What I refer to is there were two projects, two kinds of projects: regional sediments like Ginsburg and Florida; Bernard and Leblanc on the particular alluvial and deltaic deposits, clastic deposits, of the Gulf of Mexico. Onshore Gulf of Mexico. Recent. Then there were parallel projects in the ancient rocks. Nanz was working on ancient rocks as was Barney Wilson. He was working on the sandstones. Let's see – who else was working on ancient rocks? Essentially, what they were doing were mapping many places out west where you could see, I am going to say, alluvial sandstones, deltaic sandstones, and look at their

characteristics on outcrop and compare them with what Bernard and Leblanc were seeing with their shovels digging in the Brazos River banks, you know, to see what the crossbends looked like and what the recent sediments looked like. Then they put that together so that geologists in exploration could have an appreciation for those environments and understand how to recognize them, which none of us had in school. It was not part of the curriculum then, even by the time I got out of Hopkins in 1953, I took courses in sedimentary rock . . . none of that.

TP: So, these guys were like professors?

ML: Yes, right.

TP: College work in Shell.

ML: Oh, yes. Almost all of them had Ph.D.s, but they were doing high level academic research. The reason they could do what they could do was because they had money. Shell supported field trips and supported these field expeditions to find out the answer to how those rocks were deposited. And few academic institutes could afford to set up a research lab in Florida and the Bahamas and study those things or hire helicopters and hire boats and so forth and so on. That was an expensive operation.

TP: Well, you had to have commitment from somewhere to spend the money on basic research. Not all companies were doing that.

ML: No, and this began, to my knowledge, around 1948. I do not know of anything before that. But, at the same time, as I had mentioned, in geophysics, I mean, we had a substantial commitment in geophysics. I mean, we did our own thing in geophysics – going way back. And, as we have already mentioned, built our own seismic instruments and built our own seismometers.

TP: Had your own crews.

ML: Had our own crews. There is only one way to do it: That is our way! [laughter] It was quality control. After a while, years later, we went to contract companies because by that time, they had advanced far enough so that they were meeting our quality control.

TP: And innovating.

ML: Yes.

TP: In ways the company could not do.

ML: They way we really could not do, that is right. But at the time, we made major advances in geophysics.

I came back in 1953 after three years of Hopkins. Before that, I had been laying out seismic records on my living room floor sometimes, correlating those things. I came back in 1953. Wichita Falls and the geophysicists there in Wichita Falls knew I had been in the business. We had very secret variable area records. It was a very secret process in a secret trailer. And they developed the photographic process for laying those records down side by side and printing the record with about 12 or 15 records in one sheet. It was time corrected and everything. So this is all done by a complex photographic process in a secret trailer behind the office. They brought me in there and showed me. It was highly secret. Has anybody ever mentioned that to you?

TP: No.

ML: In geophysics, that was a major . . . That was the start of geophysical processing as we know it today. I mean, it was really the start of it. Then, we went to . . . After that it was analog, but after that, it went to a digital processing. When digital came in, you could do all that in . . .

TP: Common depth point.

ML: That is right. But just putting those records together, time corrected, the geophysicists could look down that record . . . Whereas, I would spread those damned things out on the floor, with a pencil . . . It made a major difference.

TP: One thing that seems to be a key: it seems like Shell did this as well as anyone, and you are an example of this, that is the work and the cooperation between the geologists and the geophysicists, and the combination of the two. It is something that the contractors did not have in geology.

ML: That was a very important relationship in exploration, and became more and more important. When I first went on a seismic crew in Edinburg and Harlingen, the seismic crews always had offices separate from the division office of the geologists. So, the seismologists never saw a geologist. He would correlate the records and make a profile and make maps and send them into the office; but he never had any contact with geologists. When we moved to Corpus, the crew moved to Corpus, the office moved to . . . took offices in the same building as the division office where the geologists were. So I was there and the crew was out in Kennedy, Texas. I had to go up there almost every day to see the crew which was not very good. We had a separate entrance from the division and for all practical purposes, I never saw or met the geologists. The party manager and I reported to the division geophysicists. But, at that point, it was still quite separate. That all changed by the time I came back from Hopkins. That all changed. So that in Wichita Falls now, the geophysicists are

right there with the geologists.

TP: I have heard that Burt Bally was a key figure in forcing the geologists at Shell to learn geophysics and understand it and apply it. And I do not know who else might have taken initiative up in that area, too. It just maybe became apparent to everybody that they had to.

ML: Oh, I am sure Mac, before Bally . . . Bally would have still been in Canada . . . I have forgotten when Bally went to work. I am talking about a period in the middle to late 1950s. And clearly, by the late 1950s . . . Of course, with Bally and others, it became increasingly important that they be married. So, by the time we went to a lease sale, the geologists and geophysicists were sitting there side-by-side presenting the prospect.

TP: There is one other thing. I do not know if you mentioned it to me or not but I thought you might have – the geochemical work. Was that Ted Philippi who . . .

ML: Yes, he was the one that . . . he did all that. He did a lot of oil classifications. That is what I remember. Trying to identify the origin of oils. They clearly came from different kinds of source rocks. I think that was what he was particularly interested in. But that was the early work.

TP: So, you would include that in some of the pioneering stuff that Bellaire was . . .

ML: Oh, yes. And you did, too. You mentioned Phillipi. I just wrote that little thing because I did not know that you had already covered something, but I think it is important that in the history of exploration, that that period . . . Nanz was right there. He is the guy that would be able to tell the best story. I was not there. I observed it, as I told you.

TP: I did not have this stuff, and there is certainly a more detailed story about Prospect 370.

ML: Yes, he is a key figure in the Gulf of Mexico.

TP: So, are there any other things you can think of? Do you have any other Tom Hart stories?

ML: I have got some Tom Hart stories . . .

TP: We cannot tell . . .

ML: I am a little worn out right now. We ought to break.

TP: This has been very helpful. I can turn off the tape.

THE END