MMS OFFSHORE GULF OF MEXICO

ORAL HISTORY PROJECT

Interviewee: CHUCK EDWARDS

Date: April 30, 2003

Place: Houston, Texas

Interviewer: Tyler Priest

Code: MMS023

Keywords: Exp, Chevron, Mgmt, Socal

Bio

Mr. Edwards served in the Pacific during World War II and received his degree in geological engineering in 1949 from the University of Tulsa. He went to work for Standard Oil of Texas (Socal) in Midland, TX and worked on seismic crews in West Texas through 1956. He became a supervisor in Houston and helped start the company's Seismic Integration Group. He was also involved in the geophysical research arm of Socal. He was assistant chief geophysicist at Sotex until 1962 and then assistant chief geophysicist for Socal. In 1964, he became chief geophysicist for Chevron West in Denver. In 1969 he went back to Sotex as chief geophysicist. In 1970, when Sotex was shut down by Chevron, he became chief geophysicist for the parent company, a position he stayed in for fifteen years until he retired in 1985 after the merger with Gulf Oil. Mr. Edwards now works as a consultant.

Summary

Interview begins with Mr. Edwards' educational background and early years working as a geologist looking for reefs in southeastern New Mexico for Sotex. Talks about working with geophysical contractors. Gives credit to his boss, Julian Pawley, for forcing him to learn geophysics without academic training in the field. Work with the research arm of Chevron -- not as controlled by research as Shell or Exxon. Integration of geology and geophysics. Discusses move from analog recording to digital recording and processing. Story about use of "sausage powder" source shooting in the Powder River Basin. Problems with getting reflection signals in the sand-shale sequence in the Gulf Coast. Discussion of bright spots. Claims Mobil found it in Nigeria before Shell Oil was onto it. Discussion of 3-D seismic and recent developments.

Side 1

TP: This is an interview with Mr. Chuck Edwards at his office in Houston. Today is April 30, 2003. The interviewer is Tyler Priest. Can you start off with some background on your career?

CE: After I came out of the service, I had, prior to that time, been at LSU in chemical engineering when the war was over, World War II.

TP: Were you in Europe?

CE: I was in the Pacific. At the time of the war, when it ended, I happened to be on the island of Hawaii, on the island of Oahu, and had an opportunity to go to the University of Hawaii because of an opportunity that you drew names out of a hat. There were 12 of us out of a regiment and a half that got to attend a semester at the University of Hawaii after the war had ended.

I tried to continue my field of study and found that the only course that was offered in science that I could take was a course in geology which I would never have considered doing before, because I was looking for chemical engineering as a career. I took that course along with some math and some other things and was so taken by geology that I talked to my professor and told him that I thought I might want to

change my career path. He was from OU and he suggested I come back to OU. So, I started back to OU.

I got as far as Tulsa . . . I had lost my father during the war and his only brother was in Tulsa. It was really a visit to Tulsa to see them. I was sidetracked in that he insisted I go out to the University of Tulsa and check what curriculum they had there. I met with the head of the department and several others, and I found that there was a course called geological engineering. It was a five year course like the professional ones that at one time had been at Mines and I think maybe one or two other schools that had these courses.

TP: How many did OU have?

CE: 170. I do not think OU had one at that time, but about 178 hours of academic work. But you did not have a thesis requirement. So, at any rate, the reason that it appealed to me so was that it used all the chemistry I had already had, plus a lot more. In fact, when I graduated, there were only two courses in organic chemistry that I would have needed to take to have a degree in chemistry as well. So, it really was a great mesh for me. But I have never taken any geophysics. I want you to understand that. All the way along, it had been geological or engineering of some sort.

TP: There were not many geophysics courses offered?

CE: We had a geophysical department there at that time. It had just started, actually, about that time. But I graduated in 1949 and had worked the previous summer for Standard Oil Company of Texas which is one of the affiliates of Chevron, as you probably know. It was at that time, at any rate.

So, in the summer of 1948, I worked in New Mexico out of Midland and Albuquerque doing geological field work.

I came back to school, finished in 1949, got married during dead week. The minute I got the sheepskin in hand after the graduation ceremony, we both headed for Midland, Texas.

Well, this is not of importance but I had sold my car to my mother in order to get enough money to take a honeymoon. I had $300 in my pocket, had a job at $315 a month which was as good as anybody was getting. It probably was higher than most people were getting out of school. I say this to bring all of you young people up to date on what it was like during those days!

Anyway, I stayed there, worked in geology . . .

TP: You were hired on as a . . .

CE: As a geologist. I worked in geology there with a gentleman who I think is also still alive maybe – Hugh Frensel. And Hugh was my mentor. I worked for him in southeastern New Mexico primarily. This was during the first reef trend where Standard of Texas found the scurry reef after it had been drilled through by Exxon. I do not now whether you were aware of that. That was about during that same time in 1948. So, we were looking for reefs. In fact, Hugh and I did a lot of field work as well as through field trips showing on the Capitan reef which was related in many ways to the reefs that had been found in the subsurface. So, reef was really a big word during those years.

After about 18 months, almost 2 years there, I had the opportunity to go out on a seismic crew. At that time, Chevron or SOTEX, Standard Oil Company of Texas, was drilling a lot of wells in West Texas, but they were also running a lot of seismic crews. In fact, from years 1950 to, oh, I would guess 1956 or so, we ran as many as 50 and more seismic crews during several of those years. So, it was a very active seismic time.

Standard of Texas was in a growth period at that time, trying to become one of the players with the majors. Although we had been in there as Standard Oil Company of California all the time and were involved with Yates Field out there early on with

some of our earlier people, the company was confined to Texas, New Mexico and Oklahoma, parts of Oklahoma.

TP: SOTEX was totally owned by SOCAL? Is that right?

CE: Yes, a wholly owned subsidiary, SOCAL. Just like Calco, if you know what Calco was at that time. And the Canadians were also a wholly owned subsidiary.

I moved 23 times in two years on seismic crews. The more I learned from the contractors about seismic work, the better I liked it.

TP: This was all reflection?

CE: Yes. Well, we did some refraction work all, but it was primarily reflection work. And we worked very closely with those same contractors that you mentioned earlier. The people with GSI, in particular. Graebner was a very close friend from very early on, and the people at Western that . . . Boothe Strange was a supervisor at that time. In fact,if you have already gone through the history of those two contractors in particular, all of those guys were . . . we were rubbing shoulders all the time. In fact, we ran as many as six or seven crews at one time with each of those, really, I guess. Each of those contractors, they were our major contractors although we used others as well.

Southern Geophysical was one that had been a friend of ours. Hardy Long was, at that time, chief geophysicist, and he had had a relationship with Southern over the years. Dick . . . who had Empire Geophysical at that time. He was one of my first friends and all of a sudden, my mind has gone blank. At any rate, those were all contractors in the geophysical business. We were doing, in West Texas, all these nine-arm star, big array type of recording in order to try to get data in the Delaware Basin and in the Valverde Basin.

I came to East Texas from New Mexico and West Texas where I had started, in about 1950, I guess. I moved a seismic crew into the little town of Anahuac, which was the first time. It had 300 people in it. It was the first time anybody had ever moved a crew into Anahuac. We worked there on an auction concession and drilled actually the first discovery well that I could claim any credit for. Although you had some anomalies and they had been drilled out in West Texas. This one was one that we drilled right after we finished it.

In fact, I have a story about . . . I do not want you to print this, but about my friend, Mr. Pawley, whom you have already interviewed. Julian was so rough on all of us, trying to bring us along as geophysicists, that I think he would come out each week and visit the crew as a supervisor. He always had a geophysical item which he had boned up on before he came, to tell us that it was all wrong the way we were doing

this thing. I had another crew that was pretty close to me and I will not mention those names but he would always call me on a Tuesday night to find out what Julian had sprung on us this particular time. And then, he would try to bone up on it so that he would be ready for him. But Julian was a real taskmaster and I am very grateful to him for having done that, although there were times that I was ready to check out. He really kept my feet to the fire and it caused me to learn geophysics, really, without having had academic work in geophysics. Although I had had all the math and physics, I just had not had geophysics. So, I learned still the hard way with the help of all these great contractors who later in life as you probably know, took over from some of the majors the responsibility of research. They actually picked it up and had to run . . .

TP: What access did SOTEX have . . . did you have access to the research being done at SoCal or did you have your own . . .

CE: Oh, yes. In fact, very early on, I was allowed to be a representative from Standard of Texas for the meetings that were held at Chevron Oilfield Research. So, I had a close relationship almost from the beginning, after I got into geophysics, with that group. We also had, in our company, a geophysical technology group that had a meeting annually, and that group related to the lab. But it also related all of our operations, so that there were papers presented at that one week conference every year that came from both the laboratory as well as the field operation.

Our company really did not until later on when we finally had a geophysical arm in SoCal . . . Prior to that time, we relied very heavily on the contractors in order to keep teaching all of our young men. If you had any kind of recognized ability, you got your time on a seismic crew. Those positions were called geological observers. And so, you went out as a geologist and a bird dog in order to bird dog the crews. But the important thing about it I think was that that was really our classroom for most of us. Even though we still called ourselves geologists, the interpretations that were being made, at any rate, were always geologically oriented, but they were geophysical because of the constraints that geophysics put on it. I think that it was a good way for a company to train their people. And this was probably in contrast, in particular, to Shell and maybe some of the others who hired geophysicists and physicists and mathematicians and ran with those people and made geologists out of them, sort of, in a way.

TP: Yes, they did. They did have their own geophysicists . . .

CE: Some of them had their own crews, too, you see, and that was a big difference. We did not have our own crews until Chevron Geophysical came along in . . . I do not know what the years were but that would have been about the late 1950s, I think, when . . . But prior to that time and even after that time, those of us that had these good relationships with GSI and with Western, relied very heavily on the technology

and the know-how that those people had. We kept bringing it back and rubbing Chevron Geosciences or Chevron Geophysical's nose in it because they would say, 'O.K., these guys can do so and so,' and we would come back to the lab and say, 'Hey, why are we doing this?' or 'Why are we looking at this particular area of research?' 'Because so and so over here is doing it.' Well, this gave us, I think, a very objective view of what was going on in the industry because we had all of these tentacles out that we were able to talk to . . .

TP: Plus you had so much of the innovation and learning being done in the field.

CE: It all came that way. That is right. It came that way. So, in contrast to, I think, Shell, in my view at any rate and maybe Exxon to some extent and some of the other majors, we were not as closely - I almost want to say controlled, and I think that may be the right word – controlled by the research element. We kept coming back at the research element to make sure that they were up to date and that . . .

TP: Well, I know at least through the 1950s, research set the agenda . . .

CE: Absolutely. In the 1960s though, they sort of began to lose . . . it was one that we . . . boy, we watched Shell, just every move they made, if we could, to make sure that we were trying to keep along with them. And fairly early on, even before I became corporate in nature, I still had that role with the company of trying to keep

the research people aware of what was going on with the contractors and other competitors. And I was helped immeasurably by the Bob Graebners and all of the rest of the people.

Western was a little more careful with their technology, but early on, Cecil Green had set up this relationship with MIT. And I was one of the fortunate ones that was invited to all of those to speak for the industry in those organizations. Every summer, they would hire a large group and then we would give them an orientation and I was involved in that orientation from, I do not know how early, but 1952 maybe, 1953 – somewhere along in there. If I am off by 10 years on any of these dates, then please correct me somewhere along the way.

But, at any rate, and because of that, I got involved in some of their technology early. And I could share those things that I felt I could share with the contractors to tell them I thought we needed it this way because, rather than getting credit or trying to say that this was a technology that we wanted to hold very close to our breasts, and we did. Many times, those people kept some of the things that were outside secret. You know, contractors in the geophysical industry for years have been some of the highest, in my opinion, some of the highest and most ethical people that there have ever been in existence. I had shaken hands with a Bob Graebner on a contract that was a five million dollar contract, and we may never see the contract until the job was already done and it finally went through all of the legal people. And the

same way with Western and all of our contractors. During those days, a handshake was a handshake that meant something. And because of that, we shared many times pieces of information that might have been held in confidence somewhere else that we felt like it was beneficial.

Rudy Prince, once he split from GSI and started up Digicon, was a very close ally of our company. And he and his people were brilliant. When he left GSI, they, of course, had some problems in that GSI felt like they had taken some of their secrets. But they were very intimately involved in the digital era when digits came along.

I hired Rudy's first crew and put him to work. Digicon had . . . all the time, even up many years after he left Digicon and before he died, he was always a very close friend of mine. If he came up with a new technology, he would share it with me, and if I would come up with one, we would share it back and forth. In fact, we even had some opportunities later in his career where both of us worked with the same guys in sort of a coordinated and cooperative way.

TP: It is in the contractor's interest to keep . . . the oil companies up to speed on . . .

CE: Absolutely. It was. But, I think that . . . I hesitate to put it this way because I do not want it to sound like I am a privileged character, but I really felt like . . . my relationship was something beyond what you would normally expect, I think, with

contractors. In fact, another thing that today has probably been a nemesis for the contractors, is that we and GSI, while I was chief geophysicist in Denver for Chevron, probably did the first turnkey work that was ever done in geophysics. And I really regret it now in a lot of ways. It was a cooperative thing at that time. We were doing digital recording. Our company, the geophysical portion of our company, had not really bought into digits completely. They felt like they could get everything from analog that they were getting from digits, so they drug their feet a little bit.

TP: So, this would have been the early 1960s?

CE: That was early 1964. 1963 or 1964. Of course, these crews, once you had a single crew on Western 21 or Western 65 or GSI's crew, all of them sort of become your crew once you have worked with them and they have been your contract for many years. So, when we got up . . . Mel Carter, in particular, was involved in some of the early digital work that Digicon was doing. Mel and our company did a lot of things that were different up there at that time but one of the things that we did was that we set up a situation that would take care of, we thought, both parties – the contractor as well as ourselves. And the way this was going to be done was that we thought we could build an incentive for those people. That was the crew and they could do their very best job and as long as we kept it within the quality that was necessary it would be beneficial to us. Because we could get more for our money and they would do a

better job because of this incentive. So, it worked very well for us, for GSI and ourselves, for two or three years up there.

When they would get into trouble because they found an area that the drilling was more difficult, they would come to me and tell me and we would adjust the rate or the price or whatever.

TP: What do you mean by turnkey?

CE: Turnkey means that we pay them so much per shot or we pay them so much per day of work or something like that, rather than paying on a monthly contractual basis. It was all adjusted on either so much per mile or so much per shot, or something of that kind. It worked very well except that when the downturn came and some of these people had been working on that, they did not give. In other words, if they got into a weather problem or something like that, the clients were too hard . . . they were looking . . . in fact, we even had some supervisors of our own that were awful hard on the contractors, that they would just hold their feet to the fire when a situation occurred like that because they were looking after their own dollars, I guess, more than anything else. It is not a one-way street when you are in that kind of relationship. You have got to make sure that they make money and that you get the best product for the least amount of money. And we worked very well. In fact, I have still done that for years and years. I think that none of the contractors feel like I

ever took advantage of them.

I owe a great deal of my success to the people that ever worked . . . I say, success – my partners in the company, at any rate. I owe my success to the people that worked as contractors and were closely related to me as well as those that worked with me who sort of carried me along.

Actually, I guess I had better go ahead to this historical thing. I have sort of gone astray. I started out with the company in Midland. I moved at least 23 times across Oklahoma, New Mexico and Texas. I became a supervisor and moved to Houston in 1952. I supervised these 50 odd crews that we were talking about over the Standard Oil Company of Texas territories. Then, Mr. Pawley started our group called Seismic Integration Group that was supposed to just catch any kind of overflow from what happened out on crews. These would be specialty jobs where we were trying to either merge with a company or pick up a company acreage of some kind. We would review seismic data that Amoco might have in their shop, and I would run off and spend 10 days to 2 weeks and make maps on areas that Amoco had decided they were going to farm out or were going to have some kind of relationship with. I did all of those. We also integrated all kinds of geology and geophysics together. These were things that established some kinds of guidelines for things that were to be done out on the crews.

During that time, at Mr. Pawley's urging, we probably picked up the first computer that had ever been used in seismic work. We went from analog to digits early on as far as activity was concerned. That was in about 1953, I guess, something like that. In fact, I knew very little about what a computer was, and he sent me to . . .

TP: You were converting analog data to digital data?

CE: We had theoretical seismograms that had been built at the laboratory through electronics, and we had all of these monstrous things that would build us a theoretical seismogram by inputting the density and velocity information to it. Those kinds of things were the kinds of analog things that we were doing. Also in those early years, that was the early years of tape recording. We started doing tape recording in the early 1950s but it was not digital. It was analog tape. And we would bring those back and sometimes convert them, A to D conversion. But it was obvious that Julian had seen where we were going to go with computers one of these days. So, as I said, he introduced me to computers by sending me to Wayne University for a short course to determine which computer we should buy in order to be the first and a leader in digital processing instead of analog processing.

TP: This seems pretty early.

CE: It was early.

TP: It didn't come until the late 1950s . . .

CE: That is right. I can say that Julian and Standard of Texas had the first digital processing of seismic data, I think, without any question. But, at any rate, I went there. I picked a computer just because everybody else was picking computers but nobody else was trying to use the same kind of application that we were. They were still looking at computers that were adding and subtracting and doing the kinds of things that you would do in order to keep up with the accounting of your company rather than the technology of it. But, again, I give Julian a great deal of credit for having a vision of where they were going and what he could do.

So, at any rate, we got into the digital area that way while I was working here in Houston with the Seismic Integration unit. Then, we reorganized and decided that we were going to put . . . we had been centralized up to that time in that we did not have a division geophysicist in each of our divisions. Instead, we had all of these individual people that were bouncing around everywhere, and then we had supervisors that were supervising over them. My first experience downtown with the company was on the chief geophysicist staff as a supervisor. That would have been about 1951 or 1952, something like that. It was not distributed out to the divisions. The divisions all look back to the central line. Well then, in about the late 1950s or early 1960s, they actually had then a division geophysicist who had the

responsibility.

TP: Was this SoCal similar to the way it was organized in SoTex or were you just really different organizations?

CE: Yes. In fact, they had gone to the divisions and this probably was before SoTex did. In this industry or in any industry, I guess, you go through these cycles of centralization. That's right. This was one of the decentralization periods that we went through.

In fact, I was one of the trials there. They set up two division geophysicists. One of them was Lee Lawyer, incidentally. Lee was in Amarillo, and I was in Houston. We did things a little differently, but Lee and I laugh about how, at that time, each of us was sort of competing with the other to show that it would work and how it would work. But I then went from division geophysicist of this eastern division back downtown again to be assistant chief geophysicist until about 1962. In about 1962, I went to San Francisco, became, again, assistant chief geophysicist out there for SoCal. That was, at that time, what they called WOI which was Western Operations for SoCal. That is the way each of these companies; subsidiaries, were listed.

There had been some thought that maybe they needed to be a mixture of blood lines, if you want to call it that. They were sort of . . . Since they were at the mother

company's location, they were sort of held under their wing and maybe there were some things they were developing in other parts of the company that needed to be there. So they sent me and Harry Stommell, who was chief geophysicist at that time, out to San Francisco. Incidentally it was the first time that I worked with Larry Funkhauser.

We made some changes. That is when digits first came in.

TP: 1962 was really the sort of . . .

CE: We picked up GSI's first marine crew on the west coast. Now, our relationship, partially because of Julian . . . The digital end of the business was one in which I had this very close relationship and I do not mean that others did not too. But we had a very close relationship with the contractors. And when the digits came along, they came to us immediately from Dallas, and asked us to be part of that first consortium that worked on digits. I cannot even tell you who they were.

TP: Texaco, Mobil?

CE: Texaco, Mobil, Exxon, I guess were the three. They came to us immediately. In fact, they came to us first. Julian did not want to buy into that. He said, "Let them develop it." I am not sure that it was he. It may be that we just did not have the

budget at that time. There was some influence from other circles. But, at any rate, we did not join them although we told them that we wanted to follow as closely as we could everything they were doing. So, again, because of our relationship with GSI, which had been long-term, they came to us first. They said, 'O.K., we are ready to put out a crew now for somebody other than the developmental research work that has been done. Would you like to put out the crews?' So, we did. On the West Coast, we did the first offshore digital work with them while I was out there. Then when I came back . . .

TP: That was in Santa Barbara?

CE: We did some in Santa Barbara, yes. We also went up the coast during that same period to offshore Washington and Oregon. A lot of that depended at that time on what the weather was like in the Pacific because it got to be pretty hairy sometimes, and we could not work in some of that water. So, part of it had to do with the time of the year.

I was in San Francisco from 1962 to 1964. In 1964, I went to the chief geophysicist in Denver for, at that time, what was called Chevron West. I was in Denver for five years actually, as chief geophysicist there. That was a new and broadening experience, too, because I had really never worked the thrust belts. I will take that back. I had seen Oklahoma's thrust belt but I had never gotten into the Rocky

Mountain area at all. We had maps on in Alaska during the time that I was out on the West Coast. In fact, during that time is when Shell and ourselves were on the North Slope doing work. This was before the discovery. We were up there doing gravity work which I treasure very much. I was on the Slope almost as early as anybody was overseeing the crews up there. I even was on my way to Alaska when the Alaska quake occurred. But we had an office in Anchorage and were working the Slope. And, not only the slope but Susitna and all of the bay and offshore work that was done there.

When I went to Denver, as I said, that was my first experience with the Rockies and that kind of thing. I really enjoyed it because it sort of gave me an opportunity to lay out how I would like to run a company. I had a lot of freedom to do that and had a great deal of help. I had a brilliant guy who actually, when I left, made chief geophysicist. Joe Spencer, who was there at the time, was the most practical researcher that I think I have ever run across. Just about. Maybe Graebner is in that same vein, too, but nevertheless, he was able to bring to my attention what I did not know technically in a manner that I could understand. And through the use of Joe and Mel Carter and Graebner, all those people helped mold me into what I finally could call myself a geophysicist. Prior that time, I had never called myself a geophysicist until I got over . . . when they made me a chief geophysicist, I sort of had to be a chief geophysicist then. So, I could at least wear that title. I am not sure whether I ever justified it but nevertheless, I finally felt like I could call myself a

geophysicist because I was beginning to learn. That was during the early digital years when I was having to fight battles with some real knowledgeable research-oriented people in my company that were still hanging on to the analog. They had developed the analog to its utmost usage, I think, and they had done a fine job of that. But some of those people just were not ready for change. That was a significant change when you finally went to digits from analog.

TP: I remember I spoke to Graebner about how difficult it was to sell that to some companies.

CE: You had to prove it. They were very helpful to me. In fact, I came up against another guy that I have great admiration for, Roger Judson, who was a real technical genius in our company. He was with Chevron Geosciences at that time, and Chevron Geosciences were the people that were dragging their feet to go to digits. They just did not want to turn loose of all of their processing. They had all the processing facilities and everything that were analog as well as all their crews. I remember one meeting where I had to get in front of the vice-president of exploration for the corporation up against Roger Judson, of all people, and explain to him why digits were better than analog. Had it not been for Graebner, Mel and Joe Spencer carrying me through this, I could never have stood up, or never would have tried. And it was not that I was trying to be as smart as he was. I was just trying to make sure that our company realized the advantages that we were going to get with

digits. Graebner was right; it was hard to bring people around to digits from analog. There were just too many people. About that same time . . .

TP: They would say, 'Well, it is too expensive," but then you have cost curves and learning curves . . .

CE: It all came to be. But one of the things that also set it back, and I do not know whether Graebener told you this or not, was CDP. All of a sudden, in East Texas, Western, and I think Western probably had grabbed it from somebody else all right, but nevertheless, Western put out crews and was doing CDP work. And we were beginning to see the apparent improvement with CDP over just conventional . . . well, the work that we did with digits up in . . . this was all happening about the same time in the 1964 to 1968-1969 range, something like that. The digits that we were using up with GSI, we were only shooting three-fold. This business of shooting 20 and 30 fold, we were not using, and the main reason why . . . and thanks to GSI, I still feel that same way about it. Smearing, that is the only way I know to put it, even though you try to put it together the very best way you can, the multi-fold coverage is most beneficial, if you can keep it separate and utilize each one instead of ever having to put it together because you lose a lot of resolution. Well, they were really losing a lot of resolution in East Texas because we were getting down to dominant frequencies of no higher than about 30 or 35 hertz. Well, up with GSI and my digits up there, we were getting 80 and 90 hertz data.

Joe Spencer, for instance, the way you were doing this is . . . we were using what was called sausage powder. We had tried several different kinds of things to get rid of ghosting and the ghosting . . . Actually the notch that was in the ghosting, depending on where you were shooting from, and there were two ghost surfaces. One was the actual surface of the ground. The other was a weathered layer. Those things set up all of this reverberative tail on the wavelet, such that you had a smeared wavelet, per se. And even though deconvolution was coming along with digits, deconvolution did not take care of that. So, again, Mel Carter and Graebner became to my rescue because I would never have been able to do this. But we decided . . . Maybe even beyond that, it probably was Joe Spencer that decided that we could do something with that ghosting that would be equivalent to sort of a deconvolution. During that time, GSI was proposing, and this was Backus, was proposing that we shoot three shots in the hole and then we would use those to deconvolve. So we would be able to find out what the . . . Well, it really never worked very well because you did not know exactly where those shots were and sometimes they were bigger, and sometimes they were less. But knowing that wherever you put that shot that they were going to be in . . .

End of Side A

Side B

CE: . . . the burning of the source with the velocity of the medium to which it was going. In other words, the near surface velocity and we would match it. Actually, we undermatched it with the speed of firing. And we did what they call broomstick charges, which I am sure you never heard of, but you would wrap primer cord around a stick like a broomstick, such that you could either wrap it close together or you could wrap it far apart and as it burned, it would have whatever that velocity was. There would be a charge that would be triggered by the cap at the top, and then it would burn according to whatever that wrapping was with that particular speed until it hit the bottom. And that would be the actual source that went out then, would be always downward. In other words, anything that wanted to come back was offset with this downward energy because you were doing it at the same rate as the velocity in the hole.

We finally ended up with what was called sausage powder. This was a long tube for like, well, what they pack sausages into. But they would pack then . . . I think it was ammonium nitrate probably that was compressed. They would be packed in and I am not sure what that . . .. It was a slower burning powder is all I can tell you and I cannot remember exactly what the explosive was. But at any rate, you would get the sausages such that we would lengthen them to a wavelength and length that would offset anything that was going to come back. As I remember, these were about 90 foot length sausages that we would use at our source.

The very first thing, and this was . . . some of these things you may or may not have run into with other people. But this was in an era when they were looking at auto-correlations and cross-correlations with digits in order to be able to tell you what kind of wave that you had. This was way back when. At any rate, the fact that we were using these . . . What we would do is that we would take a charge out in Wyoming somewhere, and we would drill a 90 foot hole. And we would put the sausage down it. And then, we would shoot. This is partly what I was talking about a minute ago when I said that during those years, I really was able to sort of do some of the things I wanted to do. There were not any real constraints on us so we would try a lot of things. And GSI was very helpful with it, too. But we found that this sausage powder actually gave us an auto-correlation that was so sharp, it was just like doing deconvolution. You did not have to do deconvolution at all because you had already taken care of all of that lengthening of the wavelet until finally, you would get a really sharp wavelet. And it was always consistent. It never changed from shot to shot to shot, because, even if the hole changed and it was a different upper reflector. You were still able to overcome that because you were always pushing the signal downward. So, from hole to hole to hole, boy, we were getting beautiful sources.

Even today, if I had to do something that was really very refined somewhere, I would probably go to something like that because you need to get every element correct in order to get the broadest band of data that you could get. Well, as I said,

we were getting the 80 and 90 hertz data on our seismic or I can say it was coherent data, which we do again by cross-correlation and auto-correlation to determine whether it was. And we were shooting three-fold data. The data were so good in the Powder River Basin that we were correcting sonic logs with our data. We could correlate our theoretical seismogram that was made up with our digits that we were taking from the sonic log. And during those days, the sonic log had arrows in it because its source and receiver were such that they were very close together. And they also had a lot of loss when you had hole rugosity and things of that nature. There was always a big problem in making a match. But we actually made corrections because our theoretical seismograms were so much like the actual data. I mean, our records were so much like the theoretical seismograms when we got through because we had already taken care of all these ghosting and lengthening of everything. We actually made correlations and we would adjust them. You could find out where it was because you would go back and look at the sonic log and you would see that there were washouts there. And therefore, there had been a delay in that signal getting back to you. So, we were actually making corrections to the actual correlations on the sonic log. which is some of the finest work that I have ever been associated with in trying to do resolution. Again, I give all credit to those three or four people that . . . and I may have overlooked somebody along that line, too, but they were really very helpful. That is a technology that, after I left Denver, somewhere, a lot of that got lost. Even some of the tests that we had run were never recoverable. I do not know exactly why. I know that it works and it can. I was very

gratified with some of that. In fact, it really kind of turned things around in Denver because it was not long after that until they finally found some oil and gas that had been sort of slow incoming in the early years.

I came from there in 1969, back to Standard Oil Company of Texas here in Houston as chief geophysicist. Chief geophysicist here until it was closed down which was 1970. I was offered a job in San Francisco as chief geophysicist and my wife had had a pretty rough time moving from San Francisco to Houston the first time, so I was not going to put her through that again. I chose not to take that job. They were kind enough to listen. In fact, Mr. Funkhauser was one of those who was kind enough to listen. They sent me over to New Orleans then to help out Jack Harris who was, at that time, chief geophysicist in New Orleans. I was sort of a bump on a log over there!

TP: With CalCo?

CE: Yes, with CalCo then. You might be interested . . . Bob Sheriff was working for us at that time, too. Bob worked with me over there. That was the first time I had ever worked very closely with Bob.

TP: Sure.

CE: Have you run into Bob?

TP: I have not, no.

CE: Well, you need to interview him, too. He is sort of a unique individual, and I will tell you about him a little later if I may.

We were not doing things over there in the same way that I had been doing them in either Standard Oil Company of Texas or in the Chevron western region. That part of the world over there, for a long time, did not have to have the technology because they could drill . . .

TP: Bay Marchand?

CE: Yes, well, Bay Marchand is an example, but you could drill on any one of them and as long as you have got structure . . . you never knew exactly where the reflections were coming from anyway because of the signal to noise ratio of a sand and shale over there. If you are looking for the reflectivity, at any rate, from a sand and shale, it is about 0.025 or something like that. It is not really enough to give you a reflection problem on conventional. I am not talking about bright spot technology, and I will go back into that one in a minute. But before bright spot technology, and where bright spot is not related, the reflectivity of the sand and shale sequence in the

Gulf Coast is just almost nothing. And it is only there because you build up these things that seem to . . . Some of them are detrimental, some cancel each other, but some are close enough space in the wavelet. You can get a long wavelet and a low frequency out of it. But nevertheless, it is not anything that you can tie down and say very much about until you get into the signal to noise ratios that are due to the gas or the fluid content.

TP: It surprised me that people were still using gravity methods and looking for salt domes long after the development of reflection seismology.

MM: We started doing . . . I was involved with that, too, in the very early stages of the downhole gravimeter. I was a representative for the company and the work that was done there. It again was a consortium type of thing. But, at any rate, I went over there and we tried to make some changes and got them started on the fact that even though I have some written material . . . and I will not tell you who had written it to me. In two or three of the locations, in Canada as well as in the Gulf Coast, who were still fighting the same battle of not making a change, and they did not think they needed analog. They did not think they needed anything above 30 or 40 hertz in the data that they were doing but they had not reached the point of exploration. Because over here, all you had to do was find a dome or find a big fault that had a closed end to it. If you could get structural closure somewhere, then you have probably got production somewhere because there were several of these objectives

not identified before you drilled the well. But they could be identified. So, that sort of slowed down, in my opinion, the development of the technology, and some of these areas that were easier to find oil and gas in.

TP: The reflection seismic mapping was still useful in some places along the coast . . .

CE: Always useful but not as useful as it could have been. We could not tie down a reflection specifically to a particular bed or an interface because that bed or interface was sometimes too small. That interface just did not have enough reflectivity for you to be able to say this is that. You could do it with the carbonates when you got into the Cretaceous carbonates or something like that. Those were all identified. But as long as you were looking at a sand-shale sequence in the Gulf Coast that was unconsolidated, it was very difficult to do anything with. Until, all of a sudden comes along reflectivity that is 10 to 20 to 100 times as great as the normal reflectivity caused by the gas content in a sand versus water . . .

TP: The bright spot.

CE: And the bright spot.

TP: So, you came over to Louisiana and CalCo right at about the time that . . .

CE: In our company, the bright spot started while I was in San Francisco actually.

TP: But just when people started getting enough confidence in it to . . .

CE: In our company, I had seen . . . while I was in CalCo at that time, I had seen an area, block 140, where we had reflections that tied to production.

TP: Block 140 was?

CE: It was CalCo. It is offshore . . .

TP: In the Gulf of Mexico?

CE: Yes, known as Vermilion. No, it is not Vermilion. I do not . . . but at any rate, we had seen some of this, and I got really excited about it. I said, 'Boy, you are seeing the production here.'

TP: What year would this have been?

CE: Well, when I first saw it was before I ever convinced anybody of it. I saw it over there and they had admitted to having seen it 10 years prior to that time in the same field! I said, "For crying out loud, it looks to me like you are getting a reflection."

They said, "Yes, but look over here. We drill this well. There is the bright spot. And we drill this well and it is not productive."

Well, it did not dawn on me and it was later. Another person I need to mention is Hilmi Sagochi. Hilmi Sagoci was with Standard Oil Company of Texas. He was the guru of technology for our company. Now, if Hilmi . . . you may be able to get hold of him. He is here in Houston. He is very elderly now but there might be some things that he could pass on to you what would be beneficial. He was slow to move into CVP and he was a little slow to move into some of the other things. But boy, he always had reasons for these kinds of things. And in bright spots, he made us go through all the things to prove why there was a bright spot which took us a little bit longer than some other people to do it but we learned about . . .

TP: Some companies jumped on it but found they had phony bright spots . . .

CE: Well, we knew early on . . . we were about as early as anybody was except Shell. Now, we went back and looked while we were trying to research bright spots. Shell had come up with something in their research organization about 10 years prior to the time that it actually hit here. And Shell had shown, and that is the reason that I am not sure that Exxon was the first, which is the thing that we have been discussing with Lee. But Shell had shown that there was this kind of relationship and had made some research into it to determine that if you changed the fluids in the rock and the

compressibility and all of these things, that they could account for it.

TP: The research organization in . . .

CE: Yes, it was in the 1950s.

TP: It was not until 1968, I think, that the geophysicists and the operations got hold of it and convinced the management to make bids on it.

CE: That is right. In fact, we made one of the first bids with Mobil in the Gulf after we had . . . Well, there is a discussion whether it was Mobil or Shell. Well, we went into that thing and Mobil had found it over in Nigeria.

TP: Oh, really?

CE: After we had found out about it, we made one of the first bids in the Gulf with Mobil. And, in fact, at that time, you could put two majors together. And Mobil had then been with us. And all of a sudden, we found out about this thing. So, we said, 'Boy, it is going to be some negotiation when we get ready to bid on this because we are going to come up with these figures and Mobil is going to say, "Why do you want to bid that?" There is no way in the world you want to bid that much money for this block." Well, we went into that thing and Mobil had found it over in

Nigeria. They had found it in Nigeria and we were all on board together.

TP: What year would this have been? About 1970?

CE: It was 1970. Yes, it was 1970, I think. We had seen it before and we had been working on it but . . .

TP: Did you know that Mobil was working on this? Do you remember any names?

CE: Well, my son could probably tell us. I am not sure that I can because all I saw at that time . . . Mobil was the one that we never had a very close relationship with. In fact, because my son during those years was working for Mobil, we did not talk. I avoided Mobil and anything that had to do with technology during those years. He finally became chief geophysicist about five years before I became chief geophysicist corporate-wide with Chevron. He was chief geophysicist with Mobil. It was after I had become, but, I mean, as far as tenure was concerned, he was way ahead of me.

At any rate, we had bid on that particular one and we just could not imagine why. All of our bids were alike. We thought we had a secret. And they thought they had a secret, and it was not until after that, and I think it would have been about 1971, that some people split off from Mobil, and they came out touting this technology. It

was out in the industry then. That is when it broke in the industry as to the fact that it was there. Well, I am sure that Shell and ourselves and maybe even Gulf were thinking about it. But I know that we bid that one. I will never forget that because we bid with Mobil on it. And come to find out later, I found out where they had originated it. And it is so obvious in Nigeria. It was just a lot clearer than it was down here in the Gulf. But we had seen it probably six or eight years before and had discounted it because we were drilling these dry holes. Let me tell you why we were drilling dry holes. It had to do with Fresnel zone; the fact that when we get a reflection back, depending on what the velocities are and the densities and everything, it does not come from a point. It comes from a series of additives as it comes back to us. Well, the Fresnel zone was such that if we shot . . . well, the reflectivity, first of all . . . the Fresnel zone was such that it does not come from a plant source. Where we were doing 2D work during that time. That was a long before the 3D came of age. We were doing 2D work, which I said came of age. We were doing 2D work and here would be . . . let's say that that is a structural closure that has gas in it. If you shoot across it here and you drill a well there on that bright spot, then you have got a well. However, the Fresnel zone being a size like this, if you shoot off of that closure, because the reflectivity is so strong on the gas. You get it? As I said, it is 10 to 100 times as strong a reflection as you would normally get in the Gulf Coast which is a very small reflection on that sand shale sequence. So, what was happening is that we were drilling out here on the edge thinking that we were okay. In fact, we drilled . . . I will bet you CalCo, in drilling offshore,

before they finally came to grips with the thing, would drill on what they thought was the edge of an anomaly here, and they would end up with a dry hole about half the time. Because the anomaly was actually back here but it was showing up on the 2D as being extended way beyond. And you do not get really the Fresnel zone unless you do corrections and migration correctly. And if you can do 3D migration, you can take care of her Fresnel Zone. But you cannot take care of it with 2D because things were coming from all different directions. You think they are below you but they are not. If you are on dip, then you can take care of more of it, if your line is a dip line. But nevertheless, the only way you can ever correct it is to get a 3D migration on that.

TP: Were there ways you could improve before 3D migration, so that you could say you were a little more accurate.

CE: You probably would shoot dip lines if you could. But, you know, the ones in the Gulf Coast many times are very low relief. Extremely low relief. And I do not know how you would correct that with 2D. It would be very difficult. You always migrated. We migrated from the time I first got into geophysics. We were probably one of the first companies to ever migrate data. Even though there might not be reason to migrate it. The reason that we migrated it was for faulting and for other kinds of defractions that we thought were causing problems, and we would try to migrate the data. And, of course, defraction has one velocity because it is a one way

thing. But nevertheless, we learned a lot. This was part of the era that I would run in from Julian about certain of these things.

But at any rate, I think that many people for a long time did not recognize until 3-D really came into its own. We did not recognize the problem of the fact that we were drilling so many dry holes on the edges of . . . In fact, I think the first thing they came out with was that it was probably just a bubble phenomena. In other words, there was not enough gas out there. In order to . . . it made a reflection, but it did not make a commercial discovery. Which, there is something to that as well. But I do not think that was the reason. It was primarily because of the Fresnel zone that we had missed it. I am sure it set us back at least 10 years. And we did not do anything about it because we found it the first time about 10 years before, but we would never admit to having seen it.

TP: So, it continued before 1970?

CE: Yes. The first time we saw it, but we did not ever recognize it . . .

TP: Did you ever drill?

CE: Only drilled dry holes.

TP: Oh, so you did drill based on . . .

CE: Oh yes, we did because we thought this was production and therefore, we would drill that way. But when we would drill on a bright spot, we would get a dry hole. Well, that did not go over at all with the corporate entity at that time. So we did not do very many of them. In this particular field I am talking about. I am not sure that we did it anywhere else. I am not sure that we would even recognize those bright spots in other places, and correlated them with production.

TP: The quality of data was so much better in 1970.

CE: It was doing much better, but it was the fact that we finally recognized that it should cause a reflection. We had not gone through that. That is the thing that Shell had done early. And, in fact, we knew back then and read Shell's papers that had been presented. And how we got hold of some of them, I am not sure.

TP: Didn't Carl Savit write a paper on it in about 1960?

CE: With all due respect, Carl took that thing and said other people had been doing it. He was the first to write on it, I think. But we were very careful not to write on things like this.

TP: Mike Forest at Shell said he also read about it in some Russian geophysical abstracts.

CE: That is right. I have heard that. But I did not realize it was in Russian at all. But I can tell you that Shell and even . . . I am not sure that Mike even went back to the early research work in Shell.

TP: I do not think he did.

CE: But it was there very early on. We recognized it. I imagine that somewhere in the files, we can find that paper that Shell had written. I think it was internal all right, but somebody had gotten their hands on it. We used that as substantiation of the research that we were doing at that time.

TP: It would be papers presented at . . .

CE: I do not think this one was presented. I think it was an internal paper that some of our researchers had or got hold of or something.

TP: That is interesting.

CE: So, in spite of the fact that the general gave credit to Exxon for having done the first

3D, I certainly will give Shell credit for having been involved in the bright spot technology. They were the very first, no doubt about it. Some of the rest of us fell into it about the same time but nevertheless . . .

Now, as far as 3D is concerned in my discussion with Lee, is that . . . well certainly prior to 1969 because I came back to Chevron in 1969, but prior to that time. In the early years of the 1960s, probably in 1959, 1960, something like that, we were shooting in Oklahoma along the Tishomengo Uplift and along the Wichita Mountain front. We had decided . . . as you know, in Oklahoma, or maybe you don't . . . the township and range situation since they came in a lot later with some of their surveying, so that there are actual section lines on sections. Often, there are roads that run on all four sides. In fact, nearly all of southeastern Oklahoma is set up that way.

So, we were shooting seismic lines everywhere that normally would have been considered a 2D seismic line. But we knew the data was coming from outside the planar section many, many times. And we decided that we had to figure out some way in order to resolve the place that that data was coming from. So, we shot with the layout along a section line. If that was here, we would shoot with . . . at any rate. We would shoot it on a section line but then we would lay out in both the inline direction and a cross line at each of those intersections that occurred on the section corner. This meant that as we shot from here all the way across and recorded that

line which would be like a 2D line, we were also recording a cross line at that position. Well, we were pretty proud of our technology and weathered layers and how we determined weather in corrections and surface corrections at that time. So, we thought that we were probably the only people . . . You can see that if you shoot here and record there, you are going to get a point out here somewhere. If you shoot here and in record there, then you are going to get a point out here. So what happens is around the intersection of those two lines, you get a set of data that all of a sudden begins to fit in. These areas out here. Well that, to me, is 3D.

I have been around 3D a long time. Chevron was one of the very first people to make application of 3D in spite of all of these other things. We were using it first, I think, economically. And partially because of some technology that occurred at the lab that allowed us to look at cubes early on. You recall, I am sure, having talked to Graebner and the rest of them that they were using slices. Well, we were using slices also but we were doing it electronically a long time before. And we were looking at slices. This way. We could slice in any direction. The only proof, I guess, that I have of this is that I was an Aramco representative, and we presented this to Aramco as a technology that they needed to use to start doing 3D work very early on. In fact, we presented it finally in Norway at one of the EAEG [European Association of Exploration Geophysicists] meetings up there. In fact, had Landmark who had just started talking about 3D at that time, had Landmark people come around trying to hire our people from us, from that very presentation up there. And

incidentally, Royce Nelson is a close friend of mine as well, and I served on Landmark's board for 9 years after I left Chevron. So, I have a great deal of respect for where we were there. I was trying to bring them along to where Chevron was in the beginning.

TP: You presented this to Aramco would have been about when?

CE: Early 1970s. I am not sure. I am not sure what date. No, maybe not early 1970s. I think the one in Norway was maybe as late as 1978. We had been doing this for a while so it was not early 1970s, I do not think. But, at any rate, I was really excited about where we had gotten at that point, and I still feel that 3D, certainly has come of age now with 3D and 3C recording. It is exciting stuff still. I am sorry, I still think in terms of 20 years ahead as to where we are going to be and that is the way I function, but I haven't got 20 years ahead. Nevertheless, I am still looking at what I think it is going to be like 20 years from now, and it is going to be exciting. It is already exciting right now.

TP: Time lapse and . . .

CE: If people were just . . . And, in fact, right now, you are talking about time lapse. I am working full-time now for a small company. I am trying my best to get a project together that is sort of an integration of four or five different developments in the

industry that now allow us to do things that we could never do before. If we bring them together. And one of them is passive seismicity. And they use passive seismicity and these three component phones that AIWA has come out with out with now. And also, I guess, there is another set of them out here somewhere which was that thing right there. That is a three component phone.

TP: A three component geophone?

CE: That is right. If you use those in patches, I want to call it, over a surface of maybe 2,000 of them or more that you have laid out. Those do not require a raise because the signal to noise ratio is so good. All you have to do is put one out there. And it gives you . . . it orients itself vertically. It orients itself because you have got orthogonal directions on the sheer, you can determine what that orientation is after the fact. On the basis of the arrival, you can determine what it is. So, you can correct every one of them down the line after the fact. So, you know you have got all of the sheer modes as well as the B modes.

The beautiful part about it is that the capacity of seismic work, or seismicity, I want to call it, works in earthquake determinations as to what the loci or what the point of origin is of the waves. It has never been used for high resolution in any way. Well, with passive seismicity, since you record it for a long period of time, when you do the cross correlations, you get rid of all of the noise but you still pick up the signals

wherever they are. Well, within an array like that already set up, all you had to do was beam steer that thing in a direction until you finally get the max. And you can locate that just like you would do it with earthquake seismology. It may be much, much less amplitude of source. Maybe of very late source, but because you have measured it for so long, if it continues out there, it builds up when all of the ambient noise goes away. So, I am excited about the possibility of . . . in particular, I am involved right now with coal-bed methane. Coal-bed methane is a very shallow objective most of the time. So in order to do a 3D survey . . . The first one that I asked somebody to give me a bid on came out $250,000 for a single mile because you have got to hold your sampling so close together. Because you are just down so short, you end up with no CVP is what happens. And if you enter an area that is pretty noisy, you are in trouble. Well, fortunately, coal has got great reflectivity. So, I have been working with AIWA and with Veritas and with Peter Duncan, who has a new company called Microseismic, to see if we can get all of these things together. We were trying to use tomography which is the other thing, and tomography has come a long way. If you know, now in a field, and they do this in the North Sea in particular and I think they are doing a lot of it in the Gulf, put detectors down hole and they just leave them there. And they will go into several wells around and put these down, and they monitor them. And they can monitor steam fronts, they can monitor water fronts, they can monitor just production itself. They can watch what happens to the reservoir as it is going because it will set up certain movements that become sources. So they are now doing tomography and

modeling fields for reservoir characteristics and reservoir modeling simply by this kind of measurement. This is a long-term measurement all right but nevertheless, it is microseismicity. If you can put all of these things together, then it seems to me that it will be able to go places and get data in the mountains, for instance, where it is really difficult, the terrain is difficult. If we can just get those little boogers out somewhere, you never have to worry about bringing in a drill or a vibrator or anything else as a source. The sources are there. All we have to do is identify them. Once we identify them, then we can model with a great deal of accuracy the tomography or the velocity field such that with all the iterations, we end up with a very fine model of the model of the velocity field and the density field. I think this is going to be another breakthrough that is going to be equivalent to 3D.

TP: Really?

CE: I did not mean to get off on my soap box here but nevertheless, that is what I am working on. If there is any way that I can get . . . it looks like I need a $100,000 . . .

TP: I talked to Dave Work also about the potential of shear-wave seismic technology.

CE: I think it is really there. And I do not know, this has all been bio up to this point? What kinds of questions do you want?

TP: How long did you stay with CalCo?

CE: I was with Chevron a total of 37-1/2 years.

TP: And then when you went to Louisiana, New Orleans in 1970, and how long did you stay there?

CE: Just about one year and a half. And I went back to the corporate then in San Francisco. The last 15 years, I was corporate chief geophysicist for Chevron working under Funkhauser, incidentally.

TP: In San Francisco?

CE: Well, actually, I had an office here and in San Francisco, but I traveled about 80% of the time so it really did not matter where I was. But I had dual offices. I had one out there and one here in Houston.

TP: So, you were chief geophysicist with . . .

CE: Corporation.

TP: So, you retired in?

CE: I retired as a result of the Gulf merger. At the time of the Gulf merger which was 1984, they gave packages to Gulf and they also gave the same packages to Chevron people. I took early retirement at that time but I stayed on for almost two years, 18 months or so, to help merge the two companies. I was on three different merger teams: the research merger team, operation merger team and an exploration merger team. Then I finally left the end of 1985. In December of 1985, I retired.

At that time, I really had intended to retire. I have a little ranch out north of town, northwest of town, and I was going to go kick the cow chips around. Lee Lawyer and a few others made some wagers with me that I would not stay out of the industry, and I paid off with some steak dinners. But nevertheless, they said six months and 5-1/2 months after that, I joined Digicon on their board and Landmark on their board as a consultant for exploration and as a board member. I stayed with Digicon about three years and in the meantime, had picked up some other clients. So I resigned from Digicon, but I stayed on with Landmark through their going public and some of the . . . in fact, about the time, I guess, that Halliburton took over, I left Landmark.

During that time, from 1986 until three years ago in December, I had built up a clientele of clients as a contractor. And I was sort of working the hours I wanted to work and when I wanted to work and where I wanted to work. One of my clients

was CDX, and I had been involved with them very early on as a consultant on some prospects that had been brought to them. I had helped keep them alive I think the first year of their being because they were short of money, and I loaned them a little bit of money. But, at any rate, had worked off and on for them during about a seven year period, seven year period, I guess. At which time, they said, "Can you give us more of your time?" They wanted to get more involved out of the coal-bed methane into the business, more involved in the conventional exploration. And I said, "Yes, I will give you 50% of my time. I will get rid of four of my clients and I will spend more time with you." Well, I found out that I was spending about 130% of my time for them, so I went back to them two months later. This was in August, I think. Two months later, I went back to them and I said, "Look, guys. You have got to pay me for 70% of my time at least because I am working for you for 130%." Well, sure enough, it stayed at 130 until December and in December, we picked up three major projects that were conventional projects. So, I gave up and said, "O.K., I will go to work for you full-time."

Since that time, I have learned a little more about coal-bed methane as well, and so I serve in the capacity of vice-president of exploration for them. It has been a real exciting time because the company has been on a rocket for the last 2 or 3 years. We are moving so rapidly that we cannot keep up with what is going on, I think. At any rate, that is the only reason I am working. You know, I have got no business working. First of all, probably my mind is not adequate. But aside from that, it is a

time when I ought to be doing some other things rather than working, but I surely am enjoying it. I do.

TP: You enjoy it and you are good at it. I would not say that your historical memory . . .

CE: Well, some of those dates may be a little off and we may have to go back . . .

TP: We can always get that. I have Lee Lawyer's book on the history of geophysics, *Geophysics in the Affairs of Mankind*, which has really good dates on a lot of this stuff.

CE: I even accused Lee once in a while of forgetting things that happened during the time that he was working with me.

TP: We have enough supporting materials that enable us to pin down dates and things like that.

CE: Incidentally, I hope I have not been derogatory towards any of these people because they have all been the cause for me being who I am today.

TP: Oh, I do not think so. So, when you were chief geophysicist at Chevron corporate, you were going all over the world?

CE: Six continents of the seven. I did not ever do any work in Antarctica, but all the rest of them . . . We did not have to get on Greenland, but we played around in Greenland. Nearly all the others, we had exploration activity in.

TP: Well, I do not want to take up too much of your time. I think we can probably end the interview.

CE: The only thing that I would like to pass on to you that I was very fortunate in my career in that I had a lot of exposure to a lot of different things. I think that has helped me be a little more objective than I would have been otherwise.

Another thing that Funkhauser did almost immediately after I went to New Orleans is that he pulled me out . . .

THE END