MMS OFFSHORE GULF OF MEXICO
ORAL HISTORY PROJECT

Interviewee: HARRY HASENPTLUG

Date: August 21, 2002

Place: Sugar Land, Texas

Interviewer: Tyler Priest

Side A

TP: This is an interview with Harry HassenpPTlug. Today is August 21, 2002. We are at Harry's home in Sugar Land, Texas. Let us start off having you talk a little bit about yourself and your background, Harry.

HH: Where do you want me to start? School?

TP: Yes, where you grew up and where you went to school.

HH: O.K. I grew up in St. Louis, Missouri. I went to a Jesuit high school, St. Louis University High School, followed by St. Louis University where I studied geophysics and geophysical engineering. i have a B.S. and an M. S. in geophysics. I graduated in 1954 and went to work for Shell in September of 1954. I also got married in September, 1954.

TP: Who was your first boss at Shell?

HH: My first boss at Shell was . . . I guess I would have to say Van Millie, because that is when were in the training program. But before we got to the BRC, I spent time on various parties: 22 Fort Stockton, DC Smith. I cannot remember the name right now, but it was up in Plains,

Texas - a beautiful little town of 480 people which I got into at night! 480 - that is all we saw in the headlights!

Anyway, then I went to work in Midland, Texas with the Shell bunch in Midland. Later, I went to Houston for the training program. After that, I got out with the wonderful title of junior geophysicist, which I do not think they use anymore. Actually, I was a junior geophysicist when I was in Port Stockton, Plains, Texas, and Midland. And then I graduated, so to speak, from the training program and ended up as seismologist. Then I went to party 31, St. Joseph, Louisiana, and I thought that was going to be real neat because it is right on the Mississippi River.

TP: Was this still in about 1954—55?

HH: This was 1955. I thought, gee, that would be nice, coming from St. Louis and right on the Mississippi, you would go up and climb the levy and look. Well, I went up and climbed the levy with my wife and could not see anything but cornfield, you know! The levy is here and way out there somewhere was the meander belt of the Mississippi. We were sitting on a point bar.

In 1958, I went to the New Orleans area. And that is when I worked under Bob O'Connor for a while. He got me into working on the velocity determinations using long distance refraction; some 60,000, some 100,000 feet profiles, where they shot offshore. And you had reverse profiles, too, because, at that time we were using two instrument boats to record refraction and one shooting boat. Incidentally, those boats happened to be the old mine sweepers — 140, some odd foot wooden mine sweepers. The shooting boat actually was an 85 foot craft. They called it a crash-type boat; it was fast. Well, it was fast when they had two gasoline engines in them. Of course, with us, they did not want gasoline engines, so they put in diesel engines and we would go about 8 to 10 knots.

Anyway, with these refraction profiles, we tried to fit it with a double type of velocity function. For example, these may not be right but say, 5,000+ plus .6, and after you got out a little ways you had to transform it into another one which was maybe 6250 or 7000 plus maybe one half, just some of the numbers that stick with me. The interesting thing, though, was that all this was done on a Freden calculator where you are taking square roots! There were no digital, hand-held calculators at that time.

If I could backtrack a little bit, when I was in the training program here in Houston in early 1955, early, before our first child was born, I worked with a guy by the name of Slim Rolfs. He was using digital computers to calculate gravity data and to work up gravity data. And that was my first introduction to any digital computers. But they were the old IBM type that you had to wire, believe it or not! So anyway, I worked with Bob O'connor and I actually started doing some salt dome interpretation because, I was not party chief at that time, but the crew was also shooting profiles across salt domes. And to interpret that tuff involved using the velocity distributions that we had, making up hand drawn wavefront charts, and using the arrival times from the refraction data. Then we put that onto the profiles and determined the salt outline as best we could. In those days, I guess we might have been going down as deep as 7,000 or 8,000 feet. That was about the best we could do with the refraction type stuff there.

TP: Refraction was still the preferred means for finding salt domes?

HH: Well, for detailing the salt domes. Earlier on, Before

I even got there, they used to do fan shooting. You might be familiar with that. They would set up an instrument boat and do a series of shots in a fan in an area where they thought there might be a salt dome. Maybe they found out by gravity work, I do not know. They recorded at one place and by the arrival time changing, speeding up as it went through the salt, they could spot the salt domes. Well, they did a lot of that. But more recently . . . this is 1958 in New Orleans under O'connor 1958, 1959 - we were doing the salt dome refraction to determine the salt dome. Of course, we also had the regular seismic data, single coverage of course, where you would make the maps based on radial lines across the salt dome and tie lines around the bottom. So, we were doing both refraction and reflection at that time.

[PAUSE]

We were doing refraction and reflection in the marine division. In June of 1960 I was made party chief for the offshore crew.

TP: And that was 88?

HH: Party 88, right. By that time they had gotten rid of the mine sweepers. They still had the small 85 foot wooden

craft, but we then had The Tulsa and The Ranger; they were roughly 100 and 105 feet steel boats. one was a shooting boat and one was the recording boat. In those days, you had about a 2,400 feet cable. I do not know if you are interested in this kind of detail.

TP: Oh, yes.

HH: A 2,400 feet cable and the shots generally were shot in the middle of the cable but certainly offset so you did not blow it apart! That went on for a long time. They were still shooting refraction at times, too, around salt domes. Then somebody got the bright idea, well, you know, we have got multiples out here. Why don't we try to do something to discriminate against the multiples mainly by using longer offsets?

At that time, we got a third boat, The Walker, and we used two shooting boats — one out in front, one behind he cable — and the instrument boat towing the cable. We did this kind of shooting. Near distances were like 3600 feet and the far distance on a 2,400 cable was 36,000. You might wonder how you would put something like that together. If you want to turn that off, I will show you a little diagram.

[PAUSE]

With the continuous recording, when we were using the 2,400 feet cable and with the one shooting boat, shooting center spreads. And they just towed the cable and they would shoot as they went along. Of course, from navigation, you would use offshore _____ A good friend of mine, Joe Delano, was president of the offshore and unbelievably, a fellow that was one of the chief operators, his name was Dick Shell! TP: Continuous shooting did not come in until some time in the late 1950s, isn't that right? HH: To my knowledge they were shooting those center spreads with continuous shooting, locating by _____ where they had to be - both the shooter boat and the instrument boat. So, do you mean the term continuous profiling?

TP: I think so.

HH: Well, yes, you could take those records and you could paste them up side-by-side. And if you wanted to take a photograph of them and pick the records you could.

TP: O.K., that is what I was thinking.

HH: So, following in a rear shooting boat with a cable in between, and with appropriate _____ information to know where they were supposed to be, we were able to shoot this way and put together a profile whose short distances were 3,600 feet and long distances were 6,000, with a 2,400 feet cable. It was quite interesting with party 88. I should have mentioned that A.B. Cunningham used to have party 88, and I took over for him. A.B. must have been there for, God, I do not know — forever! He was not quite sure of a young whippersnapper like me, who was coming down with his crew and with all these guys, how those were going to work out. Well, one of the first early things we did after a couple of months was institute this forward reverse shooting.

TP: Up until that point they were feeding forward shooting?

HH: The center spread-type shooting. I had it all drawn up much bigger than this on a piece of paper and I said, “Fellows, this is what we want to do. We want one of the shooting boats in front, one behind, and the instrument boat in the middle.” Went through the whole story of how this was going to work because if we did have a multiple, hopefully it would have more move out across the record than a primary. Now, I will get to

That in a minute. But, we had all our location positions laid out. We said, what if we have a bad charge that did not go off? Well, I said, the lead boat had better call the instrument boat and say, 'Hey, make a circle because we do not want to run over it!'

So anyway, between two skippers, three radist men and the operator, we start down the line with these three boats and the cable dragging behind. We did not get very far when things went to hell in a hand basket.

TP: It was the first time out?

HH: The first time out. There was much yacking back and forth on the radio between the captain and the shooters and the instrument man, fled Mayer was his name. But we turned around and went down the line again, and they did it right the second time all the way. I think that really said something for those guys out there on thoseboats.

Now, what do you do with this kind of data? Well, there is such a thing called a Robinson machine. I do not know if anybody has ever mentioned that. Of course, we were recording on paper records; there was no tape. None. Tape did not come in then. These were all paper records.

They were taken into the office and they would make transparencies of them. Well, it did not take long for us to figure out what we need to be doing is shooting on film records, which we did. Well, it was a Robinson machine with a can that illustrated the velocity distribution, and a bunch of mirrors, 24 of them, for all 24 channels. You would put this record in, you would run it down, the thing would follow a can in whatever direction. I never could work with it myself, but we would delta T correct as best we could with the velocity distribution we had. You could see the primaries go through. If there were multiples, we would see them going through this way. And that is one way we could discriminate against multiples in the early days. I would have to say that would have been, I guess, late 1960 or early 1961. And that is how we did the shooting out there at that time. Later on, we did get FM tape. But, this was an interesting thing; it was the first time that we were doing that kind of work. We had done that for a number of years.

The delta T corrected film of each record were laid out and they were photographed and worked into long seismic profiles, where you did your interpretation. It worked quite nicely.

TP: But it took time, right?

HH: It took time.

HH: It was very time consuming.

HH: Yes. I cannot recall exactly how many shots a day we had. I want to say, at one time, using 50 pound charges, and I hope I am going to say this right . . . I think one day we got as many as 400 shots off. That is 400 records. My boss was B.B. Houston at that time. I said, "B.B., we got 400 shots off today." He said, "Wow, Harry, that is 200,000 pounds of dynamite!" I said, "No, B.B., it is 20,000 pounds of dynamite."

Another interesting thing, as a sideline, is that since we were always in Louisiana, we always had a Louisiana person from ish & Wildlife. And he would be with us

. . .

TP: On every trip?

HH: Every trip. Generally, it was something like 7 or 8 days on and the rest off. And, of course, that little boat which we still had, the 85 footer, they used to have to load it up with dynamite to bring it out to the bigger

boats.

TP: A delicate operation. Well, I mean, this is nitro carbon nitrate. It is said although I had never seen it done, that you could fire a 22 into these canisters and they would not blow up. As a matter of fact, it is real good fertilizer, tool

TP: To check and make sure you were not killing off marine life?

HH: That is right. If we even saw some fish float to the surface, that man from the Wildlife and Fisheries would say, "O.K., let's go around and pick them up." He did not want them floating around out there, because in those days we were only, at best in 50, 100, 200 feet of water.

TP: How frequently did you see fish floating in the water?

HH: Once or twice every couple of shifts. That is about all. So, it was not all that much.

TP: There was a human cry over it and then the move to no dynamite source.

HH: Yes. Am I going too slow?

TP: No, not at all. You have to go slow for me to try to understand this!

HH: That is the way we shot. We were still doing the refraction data, too.

I have to refer to my notes now. That was in Morgan city, Louisiana where I was party chief and we had a little house down there that we rented. I was looking or a house and I saw this house that was for sale. And, of course, it was unheard of for anybody on a field crew 1961 to buy a house. Well, I did not buy it, but I found out who owned it. I went to the bank in Morgan city and asked him if he would consider renting it. He said, "Well, yes." I said, "I'll tell you what. If you rent it, I'll put in a good loan for you," and we rented this little house June 1960, when I was made party chief, to October 1961, roughly 18 months or so. We lived in Morgan city. That is where the third child was born.

There is another little anecdote which probably will never find its way into the book . . . have you ever been to Morgan city?

TP: ` No, not really.

HH: It is a pretty small town. Especially then. There were only two buyers in town to speak of; the other ones, I would not even waste my time on. One was Maurice's Lounge and the other one was The Hub Cub. Cunningham liked the Hub Club and most of the rest of us liked the other one! But my wife was feeling sad and was pregnant and she said, "This is kind of not a really nice town." I said, "Honey, why don't we take a ride?" It happened to be a clear forecast day. So, we got in the car and I took her for a ride down to Grand Isle. I said, "Honey, this is the end of the earth!" She was a little bit happier in Morgan City after that.

TP: The guys on the boats were all Cajuns …

HH: More or less. Some of the captains were Norwegian. Leif Liestad. Verdic was another one. Gosh, I would have to look up the notes somewhere to find the rest of them. Are they on here? Yes. Ben champion. He was not a Norwegian. Or were they Scandinavians? I better be careful there! A.G. Hyde. He was another skipper. Verdic, I mentioned. The captain of the small craft, Charlie Boudreaux. Jim Ryan was captain of the small craft. Oscar Howell, boat engineer. And Thibodeaux, the

other boat engineer. Well, they were not all Cajun, but a good number of them were.

By this time, I was in New Orleans because I was still party chief. They had moved me back to ew Orleans in October 1961. Pete Treole who was the original operator on party 31 when they shot this stacking experiment onshore with one of the crews that I was on before, they got to the offshore. He came on and they installed the 24 channel FM tape units. We decided we needed longer delta T, so they put in 50 foot dead sections between each 100 feet section of this 2,400 feet cable. So that made it, what, 3600, I guess? So, you had a 3,600 feet cable. That is still not very long.

The New Orleans area, being somewhat different from the Houston area, had a boat also and they did shooting in the offshore Texas area. And we were doing the shooting which, by this time, started to be stacking, literally. Not 3D or anything. I did not draw it out but you could just picture it. It went something like this [gestures and draws an example], but would make four passes on the line where we could take short cables of 3,600 feet, then couple those with another pass on a line of 3,600 feet cable where the shooting boat was farther away, and then you turned around and did it the other way. So, what we

ended up with was C8 stacks. One set of C8s was shot and recorded such that you had all shooting in the same direction, but put together properly, you had a short cable, a long cable, a short cable and a long cable, a short cable and a long cable from the FM tapes. And those, of course, obviously, were taken into Houston and the machines over here would delta T correct that stuff and put it out on profiles.

Also, we had the ability to make long singles, as they called it, where we had just single coverage to spot multiples. Also, we had put together again, since you did still have forward/backward shooting, something like that; and something like the two directional shooting, but still single coverage. Our final stacks were a composite of two C4s — one shot in one direction, the other set of C4 shot in the other direction, making that a composite of 8.

TP: Is the stacking related to common depth point?

HH: Yes.

TP: Suppressing . .

HH: Multiples, yes. About the same time, we decided that we

needed a better way to know which of these cables went together because, very often, if you had a cable here and a cable here, when you made the second pass, they did not always line up. So maybe you had to shift over and add trace 3 to trace 1 of a similar subsurface coverage. We developed, on the IBM 1410, a program where we put in the latest information. We put in the distances as we would read them from the first water breaks. I did not mention that before. It would print out on a profile -- two, three, four, five, six, seven, eight of them, exactly where they should be, so that the people in Houston could put them together properly. That is the way we sent the data to Houston.

There was one other thing that they did in Houston when I was still in New Orleans . . . In order to delta T correct them, they used what they called a shot point ratio and a total spread ratio which allowed Houston to dial in only two numbers that would delta T to correct this 24 channel record.

I mentioned water breaks. We wanted accurate distances from shot to receiver. You could use the first breaks off the record if you wanted to, but we also had four channels up on the top where we would read the direct wave through the water to the first detector. And it was

filtered in such a way that all we got were the high frequency water breaks, which were quite high frequency, and used those to determine distances. And, of course, we are still using _____ to position on the thing.

At the same time, I was doing interpretation. Ronnie Knecht was

TP: Division manager, right?

HH: Yes. As a matter of fact, I actually worked the detailed refraction across the dome and the radial reflection blinds. This was before stacking. I am going back to the single coverage. I made the refraction and I tried to show the shape of the dome and also the stratigraphic information that we had from our profiles and put that together. Well, we drilled that thing. Every day I would go down to one of the geologists - Roger Cai, we used to call him "The Sarge" and I would say, "Roger, how are things going?" He would say, "Well, we are still going around • We are getting down there."

Finally, one day, I went down there and I said, "Roger, how are things going?" He said, "Well, they logged." I said, "Well?" He said, "I can't tell you!" I said, "What do you mean, Roger, you can't tell me?" I knew he

could not tell me. So, I went next door to one of the other geologists, I think it was Jerry O'Brien. Do you know Jerry?

TP: Yes.

NH: So I went to Jerry and I said, "Jerry, what is going on out there?" He said, "I can't tell you." So, with my tail between my legs, I walked down the hall to the coffee room and I pulled a cup of coffee. It was time for a coffee break anyway. Everybody seemed to congregate in there. And Ronnie Knecht walks in. I said, "Hi, Ronnie. How are things going?" He said, "Well, have you heard about our success?" I said, "No, Ronnie. Nobody would tell me." He said, "We had 240 feet!" So, I was quite happy about that!

TP: They kept a pretty tight lid on information, even from within the division?

HH: Yes. Which lease sale did you mention?

TP 62. Was this 62?

HH: Yes. As a matter of fact, another little thing we instituted out there with our three boats is that we made

up a little form and asked the skippers if they saw another crew shooting would they jot down the radius coordinates where we were, take a bearing, and see if they could estimate the distance. If we were shooting we could not use our radar, but if we were ot shooting we could use our radar and find out where they were. And we would bring that back in and turn it in.

TP: Could you find out which crew it was?

HH: Well, usually, yes, although the radus people would not usually tell us because they were on the other boats, too. So, they would not tell us and I cannot blame them. But, every Monday, the offshore scouts would meet with Shell and other companies and have a scouting report. And the scouting report would tell where their crews were shooting.

TP: The companies would get together and figure out where each other was shooting?

HH: Yes, I mean, it was a common thing to do.

TP: It was advantageous to know where other people were shooting so you would not end up in the same place at the same time, right?

HH: Right. The scout report would tell you where the crew was in terms of which offshore area. I am not sure they even told us where in that area they were but we would know which crew it was and who they were shooting for. So, with the additional information of where they were, spotted, our people in the office could figure out who was there.

TP: And exactly where they were shooting?

HH: Yes. Which prospect even, which we might have shot the day before

TP: That is interesting. That would be valuable information when trying to develop a bid strategy.

HH: Well, I was party chief until June of 1962.

TP: I am sorry to interrupt . . . going back to the discovery that Ronnie Knecht told you about, do you remember which field that was or what block that was?

HH: Yes. I want to say, it was South Pass 62, but we called it prospect 69. It was right close to the edge of the shelf

TP: South Pass 62 was a significant field.

HH: I believe that is correct. I do not have my notes in here anymore.

TP: That was something that was drilled long after you had the lease, right?

HH: I think you are right. Well, actually, we drilled very soon . . . I stayed on the marine division until 1965, 50 I was there several years.

TP: South Pass 62, O.K.

HH: I think it was South Pass 62. I remember the two numbers. Block 62 and prospect 69, we had numbers for the prospects.

Another interesting thing . . . when B.B. Houston, our division geophysicist . . . we had actually shot that thing when I was still in Morgan city. Well, of course, I was still party chief. He always would send information out as to start here at this lock, 1,000 feet this way, in four or five radial lines and all that — a detailed explanation of where you wanted to shoot. Well, it turned out the first time we shot it,

nobody could find the salt dome. B.B. called me. He said, "Harry, I can't find the salt dome. Are you guys sure you shot in the right place?" I pulled my sheet of paper out. I said, "Yes, from the southeast corner, 1,000 feet to the east." He said, "Let me get mine." He got his. He said, "Harry, I am sorry. You have to shoot it again. It should have been 5,000 feet to the west." So, we replotted out and reshot it again the next shift. And lo and behold, when they shot it they finally ended up calling that prospect 'crow foot' because we had refraction lines and reflection lines like so; they looked like crow feet.

TP: Overlapping.

HH: Overlapping coverages. So anyway, that is just another aside.

TP: So, in talking about the lease sales, there was a flurry of activity. You were shooting as often as you could?

HH: Right. And about that time, I was moved back into New Orleans. By the way, another fellow that took over for me is on party 88. I guess it would have been June of 1962 . . . Willard Vandeveder, W.S. Vandeveder, he took over for me as party chief of 88.

TP: Have you seen the little history of Shell offshore seismic parties?

HH: Yes.

TP: I think that is where I remember seeing his name.

HH: It is about that thick. I have got it upstairs. Who the heck wrote that?

TP: It is pretty good.

HH: Yes, it is. They started off way back with Cunningham. It is a good reference.

TP: I think it starts in 1946.

HH: It probably does. They were using a bottom cable and a drag along the bottom.

TP: That is right. Did Shell shoot all its own seismic at this time? Was Shell using any of the contractors?

HH: We shot all of our own seismic. Well, in more recent years, they probably bought some, But you recall that the Shell American and _____, those were ships that

Shell built specifically for that purpose and that was after 1966.

TP: They were doing some coverage, but most of it was not done that way.

HH: Yes, and of course, by the time they had those two boats, they were doing stacking offshore with long, long cables. And also with air guns and probably touring as many as three or four cables. I was talking to a guy the other day who was working offshore in the western part of Africa. He is not with Shell. He said that on that boat that they were pulling 15 cables with air guns.

TP: All 3D?

HH: Yes.

End of Side 1

side 2

TP: Was this at BRC or the Houston area?

HH: Yes, that is at BRC. It was housed at the BRC but it was the seismic processing division.

TP: So, you are starting to accumulate a lot of computing power?

HH: Yes, at that time, we were getting our seismic data systems, SDS 9300 and an SDS 930. I do not know who bought them. I almost went to work with them about that time, but I did not. I sure ran up and down the beach in Santa Monica because they sent me out there for a two week course to learn either the SOS 9030 or the 92 unit which was the digital unit that eventually went on the boats. SOS had a course out there and I was out there for two weeks. I took a week off and took then another one week course. Fortunately, we were able to take the whole family out there - all three boys and Janie — and we had a nice place on the beach, a motel. It was an enjoyable experience. We drove out. I got permission to drive my own car which you had to do on those days. You probably still do.

I was going to mention also, on that refraction shooting, you had to know what the water velocity was. One way to get the water velocity was to take an empty gallon can, tie it to a rope, tie the rope with a weight on it, and lower it over the side of the ship. And when you got it down to where you wanted it, you pulled it a couple of times and it would flip up. You would get water and bring it up to the surface and measure temperature and salinity from which you could calculate the velocity of the water. Of course, we all know that if you go down 50—100 feet, the pressure is about one-half pound per foot, so you have got 25 psi of additional pressure on the can which is open, which would tend to fill up with some water.

So, there is another way of getting some good water velocity information on these long refraction lines. You take the radus information, plot it, and then take the time for the water breaks at each shot and plot them. And if it was not dead flat zero, you knew that your water velocity was either faster or slower . . . I cannot remember if it was generally faster or slower. But at any rate, this turned up something else. When there are low refraction lines in 200 feet of water, the ship would go to the position where they wanted to drop the instruments onto the bottom and they would dump them over

the side of the ship; hopefully, they would go where they wanted them to go. Well, by looking at this plot of water break distances versus radius distances, the intercept could tell you how close the instruments actually to the XY coordinates from radus. And we could adjust it that way, too. It is just another aside as a fine tuning, but again you would not want that.

TP: There are a lot of variables that you have to account for.

HH: The January of 1966 seismic processing division; that was when we had the SDS 9300s doing the seismic processing. The other thing I wanted to mention . . . that other course I mentioned in Santa Monica, that was on an SOS92. That was the onshore computer that drilled everything and recorded. I had that when I was still in New Orleans. But anyway, by this time in 1966, I was in the seismic processing division. We were getting several digital programs for processing and I was in charge of putting together documentation for our people in the field. So, we put that together. Man, what I could have done with a word processing unit then! They did not exist, nor did PCs. But we wrote it out by hand, handed it over to the

steno pool and the stenographers would type it up on this form which was then keypunched. And we had boxes of keypunched cards with the documentation on them, which you could print out and do whatever you wanted to with them.

I did that for a while. Then, I was with a geophysical evaluation group in June of 1966, that was with Ed Hubbard. This is where we were still doing documentation. I was then transferred to basic measurements and theory in Shell development, working under Aaron Serif f. Those were the days when we were trying to make synthetic seismograms from acoustic well logs look like the seismic data. It was not too easy! I finally wrote a memo to Aaron, I said, "Aaron, look, the stuff that we get off of their well logs is all very, very high frequency stuff and we do not have anything near that in the seismic." In those days, we "deconvolution" was the big word, so I said, "why don't we try to deconvolve as best we can the seismic data up to a point and then band limit the sonic logs back to that same bandwidth and get a better correlation?" Well, I do not think that was too accepted, but at any rate that is what I started doing.

From February of 1970 to May of 1970, I was transferred,

not transferred but got a suitcase job as we called them, from the BRC, Bela Research Center, over to New Orleans, to do more seismic interpretation for later lease sale. When I got back off of that, I was transferred over to the so—called head office experimental data processing and interpretation which was also at BRC. That was under Paul Terrason. There we worked on better putting together the seismic and the sonic logs from wells and trying to match them. I was made staff geophysicist in 1971 and continued to work in there with Paul Terrason. About that time, Terrason said, "Harry, you know if you take that sonic log," which was simply individual reflection coefficients basically, "and if you integrate that thing, you'll end up with a velocity log." So, we did that. We also matched it up with the seismic where we tried to deconvolve that using computer technology as best we could. We had pretty good success with it.

TP: Matching the seismic . .

HH: Matching the seismic to the well logs . . . as long as we were dealing with a velocity log rather than a travel time log from the sonics. The sonics only measure travel time from here to here, two feet, five feet, maybe. When

you integrated that thing, you ended up with a velocity log which was what we converted our seismic to. With Terrason, I got interested in doing a little better job of deconvolving the seismic and matching it to the well logs.

TP: Can you explain deconvolution in simple terms?

HH: Yes. The seismic data is, of course, band limited because of the way it is shot, to some extent, where you have a high cut of somewhere around 100 cycles, and maybe little or no low cut in the offshore. Deconvolution, in its simplest form, would be to find out what the outgoing signal looks like from the shot and then remove that from the seismic data. We tried that and it worked fairly well. But, when we integrated both that deconvolved seismic data and compared it to the integrated well logs, the velocity logs, there was a much better comparison. Deconvolution - let me put it this way. This guy is standing in front of a cliff and he shouts, “Help!” Well, H-E-L-P comes out of his mouth, goes to the cliff, gets reflected, comes back inverted . . . I do not mean inverted this way, but it comes back with a P flipped, L flipped, E flipped and an H is the last thing that comes in. Well, if you know it is HELP that is coming back to

you, it is s very simple to take the HELP out of that signal and check the time. And you know how far it is using the speed of sound to know how far that cliff is away. It is just basically what deconvolution is. It is removing the outgoing signature, the pulse, from the seismic data. With Terrason's bunch we did some deconvolution that way and were pretty successful with it, starting at a well log and comparing it to the seismic. And then, trying to determine what the level is in the seismic by comparing it to the well log and then removing that seismic signature from the seismic, leaving behind what should look something like the velocity log.

TP: What is the point of trying to match up the seismic with the well log?

HH: The first real success, I guess, was on the so-called Posey prospect. The names may sound somewhat familiar to you. At the Posey Prospect, we had two wells – seismic stacked line - and we deconvolved at the one well, deconvolved at the other well, and used the outgoing seismic signal that we determined to deconvolve the whole line. It was a bright spot prospect to begin with; over here at this well you had bright spot. Over here at this

well, you had bright spot. But doing that, we were better able to delineate where the oil was. And Bob Nance, when we showed him that, said, "My God. Think how many exploratory wells we will save in not having to drill so many exploratory wells." Because we actually ended up with a profile for seismic that looked like a velocity log. So that was some of the very interesting stuff we did there.

TP: When you were working on verifying how a bright spot . . .

HH: Well, Mike Forrest is the bright spot man.

TP: Yes. Does it relate to what you were doing?

HH: Yes, it does. But, I should point out that when I was in experimental data processing and interpretation, Mike sent a profile over to us. We tried to get reflections beneath this bright spot. At the time we were not really convinced and even Mike, I do not think, was convinced that it was a bright spot at that time.

TP: What year are you talking about? Was this is after people believed that there was such a thing as a bright spot?

HH: No, I do not think I could say that. That was in 1966, in the geophysical evaluation group. We tried our damndest to find out what was below this strong reflection and we never could figure it out until we found out that it was fairly shallow gas and you did not get much energy through the shallow gas!

TP: I know the lab verified the bright spot theory. Were you there when all that was happening?

HH: Oh, yes. I was there at the lab until January of 1975.

TP: That must have been exciting.

HH: Oh, yes. I mean, everything was exciting then. We had a computer video screen, an adage computer, and we would take the seismic and filter it in such a way that we could break it into fairly narrow frequency bands. For example, like every 2-1/2 cycles per second, that would peak at 2—1/2 cycles per second, across the frequency band and do the same thing with the well log. It would take those individual things and the individual band limited sonic versus the band limited seismic and try to match up the shapes and amplitudes for each of those frequency bands. When you do that and you add them all up, you end up with a seismic signature.

When you take the amplitude and tore shifts of each of those discreet frequency bands, and if you apply them to the similar type frequency bands of a single spike, which is all frequencies, you end up with the seismic wave length. If you use the amplitude tone shifts on the seismic, it becomes looking more like the velocity log itself, which is displayed on the screen on this adage computer. We did an awful lot of that - showing people out in the divisions how to do it. That was under Terrason.

TP: The effect it had was it reduced the number of exploration wells you had to drill once you made a discovery.

HH: Yes, you did not even have to have a discovery for this; even a dry hole would work to get the seismic signature. The problem is, if you use the amplitude changes and the time shifts from this well, how far away could you walk along the profile and still have it be valid? So, there was a caveat there. But nevertheless, we tried to do it.

Let me go back to party 88 briefly. From June of 1961 through September of 1961, they moved party 88 up to the Atlantic Coast. C_____ L_____ was the division geophysicist and I think he went up there at the same

time. Bassett went up there. I am sure you have talked to him.

TP: I talked to him at the reunion. I was going to talk to him in New Orleans, but he has some health problems. So I have not been able to talk to him yet.

HH: Oh, he does?

TP: Yes.

HH: Well, he was at the meeting here.

TP: Yes. Right after the meeting.

HH: I did not know about that. I will have to give him a call.

TP: Yes, he is still someone I need to talk to.

HH: Anyway, they went up to the east coast and the Texas crew moved over to Morgan city. Here is another one of these funny things that come out in the wash • When I had party 88, my boss, B.B. Hughson, would say, "Harry, these guys over in Texas are shooing in 10 feet seas and you guys have to shut down when the seas are only 5 and 6 feet.

Why is that?" I said, "B.B., all I can do is go with our shooters because they are the ones that determine when we shut down and rough seas because of the safety angle." So, when party 218, the Texas crew, came over, a guy by the name of McMeeley was the seismic operator, I would sit there on the deck and we would look and I would say, "McMeeley, what kind of seas do we have out here?" By this time, I had made up my own mind that we had 2-3 feet seas. McMeele would say 4-5. And I do not care how big the seas were, they were always bigger than what I would have guessed based only on my recollection of what my crew people told me when I looked at the sea.

TP: So, they just had different measurements.

HH: Well, there is no way to measure it, really, unless you pull up next to a rig or something, a platform. So anyway, that was another aside. I was then moved out of Terrason's group. I made staff geophysicist in August of 1971. I was put to supervisor staff of exploration and research geology, in the geology department, stratographic prediction at BRC. This was under Pete Lucas, Tom Torque, and D_____ M_____ . I had a staff of several geophysicists and geologists and we were studying seismic response — kind of like the Exxon

people are doing, looking for turbidites and such.

TP: This was beginning in about 1975?

HH: This started in 1975, yes. We put together a little program where we could load seismic data into this adage computer — a portion of it, not a whole lot because an adage computer was not very big. Then we would show it on a map and make measurements off line in a digital program and bring those measurements back, load them into the adage computer, and post the measurements of amplitude or whatever on the adage screen and start making some intelligent guesses as to where a particular facies might stop and another one begin. A fellow by the name of John Andrew . . . he is no longer with Shell. As a matter of fact, I am not sure where he is, but John Andrew was the prime mover behind that along with Terrason, and he had the adage. So, I was in geology research working with geologists and geophysicists and we were working with Terrason's people to program this adage computer to do this. I guess that was probably one of the most exciting things wewtwere doing. Let's face it — that was the forerunner of the workstations. Not that we were the only ones working on it; other companies were, too. And, of course, what you can do now with work stations is fantastic.

TP: It must drive you crazy to think of what you could have done with the computing power that people have now.

HH: What people have now on a PC!

HH: On October of 1980, I was transferred over to the information center which is that big white building just north of the Astrodome. Are you familiar with it?

TP: Yes.

HH: They called me manager of technical coordination geophysics. My job and the job of the people who I worked with was to keep our people in the divisions happy, and to keep the people in the information center with the IBM . . . what did we have then? We even had some crey computers by this time. Well, not in 1980, but certainly in 1985-86 I think we had Cray computers. But there was never enough compute time to satisfy the people in the division. You had three or four damned divisions! It is kind of like walking a fence between the people at the information center and the people in the divisions dodging the bricks going back and forth! Well, I had nine good people with me. Five of them were geophysicists who were trying to smooth things out between the data center and the divisions.

TP: By that time you had had 3D seismic for quite a while.

HH: Oh, yes. We had a lot of compute power. And that brings me up to, I guess, the point where I retired, in August of 1989.

TP: Can you talk a little bit about some of the people you worked with like Paul Pearson? Did you know Jerry Persig?

HH: Sure.

TP: He is sort of legendary .

HH: I was writing these down when he knew you were coming. O'Connor, he was in New Orleans. B.B. Hughson, division geophysicist. G. Spate who took over from him. J.C. Van who has since died. Winnie Nesfault. Jim Robinson. Aaron Sariff, who is dead. Paul Terrason, who is dead. Fred McBride, who is dead. Ed Hubbard - I do not know where he is but I do not think he is dead. Three geologists by the names of Pete Lucas, Tom Turrick, and Dave Missman. I worked with them when I was supervisor staff of the stratigraphic prediction section in the geology department.

Then, when I was over at the information center, there was Hackert, Phillipson, and other people who were in and out very often. Well, Bob Nance, he was VP of exploration, as I recall. There was Billy Flowers. Bob Chook. Have you heard about him?

TP: Yes.

HH: He collapsed right outside my office. Jerry Persig. But two of the guys that really worked with us were Billy Flowers and Jerry Persig.

TP: "With us," meaning the researchers?

HH: Yes. Jack Threet. I was making a presentation when I was at BRC as a supervisor in the seismic stratigraphy, and put together what I thought was a real good presentation. Jack Threet, have you ever talked to him?

TP: Yes.

HH: Well, he sits there and if you are presenting something to him, he is going like this all the time. Yes, tell me more, tell me more. He is like a cheerleader out there. So I was real happy about the presentation. Jack came up and he said, "Harry, that is real good." This was at

BRC. We went around to another room where we had lunch set up. And, of course, in those days, we had ties on, I went over to the table where the food was and got myself some shrimp and some red sauce. I went to sit down and Jack was right behind me. He sits down next to me. I sit down. His tie goes out over that red sauce. It looked like I had been shot! All I could think to say was, "O.K., God, here I was, top of the world, and you really know how to bring a man down!" [Laughter) So, that was one of the funniest things I think that ever happened to me over at BRC.

TP: I am going to conclude the interview and turn the tape off.

THE END

HARRY HASENPFLUG

MSS Offshore Gulf of Mexico

Oral History Project

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