SHELL OIL COMPANY

ORAL HISTORY PROJECT

Interviewee: BILL BROMAN

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Bio

Mr. Broman got his B.S. and M.S. from Michigan Technological University. He joined Shell Development Company in 1953 working mainly in the Houston E&P area. He began working as a geophysicist for in 1959 and became Senior Geophysicist in 1965. He continued to serve in various capacities including Acting Director of Exploration Research. He then transferred to Shell Oil in 1969 becoming Division Exploration for the Onshore Divisions, Southeastern E&P Regions. Shell Oil made him manager of Geophysical Operations in 1973, and in 1977 he became Division Exploration Manager, Offshore Division for the Southern E&P Division. In 1980 he became General Manager, Rocky Mountain Division for Western E&P operations. In 1983 he was named General Manager for Exploration for Shell Offshore. He retired in 1990.

Summary

This interview dealt mainly with Seismic processing technology. Comments on the early development of seismic including stacking. The evolution of digital and various computer programs. Good information on the discovery and implementation of bright spots and the use of 3-D technology toward deep water.

Side A

TP: Today is December 15, 1999. This is an interview with Bill Broman. The interviewer is Tyler Priest. I guess I will just turn it over to you. We have your biographical information, so start off with what you think is appropriate.

BB: All right. I would like to start off giving a couple of benchmark dates for some of the early developments in the geophysical industry. The first one I will mention is the mid-1930s, and a fellow by the name of, I believe his first name is Frank, Rieber, who developed a device for processing seismic records. It is called a sonograph, and it is based on technology that was used in the early days of motion pictures to make talking motion pictures. This is an early step to having reproducible seismic records that could be taken to the office for further processing. Much greater advances were recognized in industry in the mid-1940s by patents. There were two patents: one issued in 1946 and the other in 1947. One patent was the person, J.A. Sharpe, and an associate. I believe the date on that is 1946. And Rieber had another patent for magnetic recording dated 1947. These patents explicitly talk about correcting data before they are composited, or added together, to enhance signal-to-noise ratios. Very important steps. From these early steps, by the 1950s, recording on magnetic tape media was fairly common in industry, and a man by the name of Harry Mayne, who was employed by Petty Geophysical at that time, was granted a watershed patent in the seismic stacking method, the compositing method.

TP: Could you explain what "stacking" is?

BB: The simple way to look at seismic recording is that you will have an acoustic source of some sort. In the old days, there were mostly dynamite charges detonated in the ground. Today, it is compressed air guns, things of this nature primarily. These send out a seismic shock-wave traveling through the earth. And at every depth that the shock wave encounters a change in the acoustic properties of the earth, part of the signal is reflected back to the surface. And then, these reflected signals can be recorded with instruments called seismometers. That is generally a moving coil, a coil mounted on a spring that is free to move in a magnetic field. And that generates an electric pulse that can be recorded on whatever recording medium you use.

Now, the real trick comes in figuring out a way to record at many spacings on the surface. If we think in terms of a single sound source putting energy in the ground, but then with a multiplicity of receivers spread all over the ground, maybe not only in one line of profile, but in a complete aerial array, as the reflected waves come back up, to determine whether the energy you are seeing is a reflection or not, you have to make that determination based on the coherence of the signals between the different receivers. And this is where stacking comes in because we take a multiplicity of both sources and also receivers that will give rise to data that are reflected at various reflecting points in the subsurface. And those can be sorted out such that they can be added together in phase if proper corrections are made first so that we get a pristine signal with any background noises diminished. This is called stacking and filtering. Is that enough for you?

TP: Yes, that's good enough.

BB: The term that we normally use, incidentally, for these reflection points . . . we say we have a reflection point in the subsurface. It is actually a small area. But then, we illuminate that same area from a variety of different locations on the surface of the sound source and the receiver. So, they all have different travel times depending on where the source and receiver are located. And the essence of processing and what we were really trying to get to the bottom of in the 1960s, was a way to correct all of those data so that we could bring that multiplicity of data together, add them together in the stack, but add them so that the signals were in perfect registry. But the noise spread out, therefore reinforcing the signal relative to the noise. As a byproduct of doing this processing in the proper way, we obtained very good information about the speed with which the sound wave traveled through the subsurface. And obviously, if one of your objectives in seismic profiling is to end up with a picture of the depth to these various reflecting layers, all you are measuring at the surface, to begin with, is the strength of the signal and the time the signal comes in. If you know the velocity, then you can convert that time to a depth. So, you get that.

O.K., now we can change focus and talk about one of the great contributions that Gerry Pirsig was the primary author of. Around 1960, Gerry was working for Shell Canada, at which time the western pinnacle reef play was going on in Alberta, and so forth. And these pinnacle reefs are relatively small in aerial extent but relatively fixed so that they can represent sizeable oil fields.

TP: These are reefs of glacial till? Is that right?

BB: No, these are coral reefs.

TP: Coral reefs.

BB: Coral that typically have built up from the sea bottom, so they are standing up in the water. And then later, sediments will come in and fill in around them. The sediments that fill in around the little pinnacle reefs are normally quite reflective, so you get good reflections there. When you cross the reef, no reflection. Then, on the other side of the reef, you pick it up again. It is a simple criterion for picking out these types of structures. But they are very subtle features. To use stacking techniques to enhance your data, it is critically important that you make precise corrections for the highly variable travel times you have on the near surface. We call this the weathered layer. That is much slower, normally, for the passage of seismic energy compared to the consolidated sediments at greater depth in the earth. And, again, using the stacking geometries properly laid out in the recording, you can get a multiplicity of travel times beneath each recording instrument and also beneath the sound source. And Gerry developed the techniques for analyzing the raw data, the total travel times, and then sorting them out, devising proper filters where we could determine our best estimate of the corrections to make for the near surface travel time beneath each receiver, and if there was any residual time underneath each shot. And this brought all of the data into better registry to stack them for higher signal fidelity. It also gave us better precision of the way to measure the changes in the velocity, propagation velocity, down to the reflectors all along the profile path of the seismic profile. So, what started out to look like the Achilles heel for the stacking method; namely, that the data have too much jitter in the time due to our inability to correct precisely for the variants in travel time and the weathered layer immediately beneath each receiver, primarily, and also for maybe not having quite enough accurate velocity information to correct for the travel times in the reflection path to depth. Gerry's filters, that he devised, gave us an optimum solution for all of those parameters and one calculation. It was a brilliant move and took away the Achilles heel and turned it into an advantage rather than a disadvantage. And from there on, stacking went full bore and with Shell Oil, this technique had its final flowering in the reef play in lower Michigan in the late 1960s. Here, the glacial till is very thick. There are many bogs . . .

TP: This would be the surface weather . . .

BB: The surface weathered layer. There are many bogs up there that are filled with peat and spaghum moss and things like this that are just atrociously slow propagators of seismic energy. So, this introduces large corrections that have to be applied. And the play in Michigan, this was a critical element in our ability to prosecute that play as well as we did.

TP: I didn't realize that Pirsig had done work on pinnacle reefs in Canada before the Michigan play. I didn't have the sequence right.

BB: This is my understanding, yes, that Gerry was general manager of exploration in what we called our Denver area in the mid-1960s . . . no, that was in the later 1960s after he had served as chief geophysicist in New York. And that was when the Michigan play was finally kicked off.

TP: How would you look at describing Shell's abilities of stacking with two of the larger industries? How did it evolve at Shell compared to what other companies were doing? . . . Or maybe if you could just provide some context on what was happening in the industry at the same time. Where was Shell relative to other companies in this geophysical technology?

BB: I would say, in the early 1960s, say, in 1960, Shell was as good as any of the companies, any company, in the theory of seismic work, things like this, the ability to interpret conventional photographic records. We were good at that. We were not in what I would consider to be in a leadership position, and the ability to acquire new data, things of this nature, but with Gerry Pirsig's transfer from Canada, probably about 1960 or 1961, to New York to be head office chief geophysicist, the pace picked up real fast. McAdams had a dying commitment to geophysics, geophysical excellence, and Gerry spent an enormous amount of time traveling to the various operating areas preaching the gospel of improving our geophysical capabilities. We had many things that were in the process of coming forward. Our ability to process seismic data with the magnetic tape medium was excellent. We had some wonderful machines there. We used to call them seven drum machines. We could put magnetic tapes, FM tapes, on six of the drums, correct the data as the drums rotated together, and could either sort the data off onto the seventh drum or add the data together and stack and things like this. So, that was how we were processing data. But a group was formed in the Houston operating area at that time. It was headed by

Bob O'Connor and Paul Terrasson. They were the two key players, and they saw, along with Gerry and other people, that the real route to travel was to move from the magnetic format to the digital format. And they were able to acquire time on the IBM and finance processing machines that we had there, and they developed a set of digital processing programs. And we wanted to keep them . . .

TP: Use them for converting analog to digital?

BB: No, this was after having things in the digital format. Yes, we did have what we called A to D converters, but then, converted from the analog magnetic tapes to the digital . . .

TP: To the digital recording . . .

BB: Yes. And it is interesting. We had code names for the programs that they wrote to process the data. Rather than calling it the program stack, we called it "Sac." Then, we had a program for velocity determination of the data, I explained that earlier, determining the velocity of the travel times through it. We called that "Vac." We had another program, a very important program, and this was, if the reflecting surface is not perfectly horizontal beneath the source and receiver but is dipping, when we plot our first estimate of where the reflection point is, the only estimate we can make is that it will be halfway between the source and the receiver, in the horizontal dimension. That is not where the true reflection point is, so the data have to be moved along the profile. So, we called this process migration. And having the velocities and so forth done well enough, we can make a fairly accurate migration.

We called that "Mac." So, those were old . . .

TP: Sac, Vac and Mac?

BB: Yes, those were the old key programs. And then, a couple of years after that, Woody Nestvold at Bellaire Research, came in with the first of our higher powered programs to sharpen the signal shape itself, and that was called "Sprint." And we were off and running.

I think that we should point out here that, I think I have already called attention to the competitive advantage we had in these programs to prosecute plays like the Michigan pinnacle reef trend, things of this nature.

Along the Gulf Coast, out in the water, the thing that we felt was bedeviling us the most in the early 1960s was multiple reflections. We were trying to make accurate maps deeper in the subsurface, and, typically, the velocity of the sediments that your seismic wave passes through speeds up with depth. In fact, well-known relationships that are used for velocity depth determinations. And it works out that a lot of the strongest reflectors that you have are in the shallow layers. So, quite often, what we were seeing were multiple seismic bounces reflecting more than one reflection point. The wave would go down, get reflected back up, see another reflector, get reflected down again, and then emerge to the surface. They would be reverberating in the near surface and then come up. It turns out that for the Gulf Coast particularly, with the reflection speeds we had out there, that stacking, if we went through long enough spreads between the source and receiver, say, out to about distances equal to the depth that we want to be looking at in the subsurface, we could distinguish quite well whether that was a primary reflection with one reflection point, or whether it had multiple reflection points. It would have a different apparent velocity that we could measure, and then you can begin to devise strategies, filtering strategies, to discriminate one against the other. So, stacking was very powerful for us in that time. So, we were getting better reflections. And then, in the late 1960s, probably about 1967 or so, Mike Forrest made the brilliant observation that the seismic in some oil fields were showing unusually strong reflection strengths at the pay levels.

TP: Now, when you are talking about being able to sort out the near surface reflections, is this what you would call true amplitude recovery, or am I confusing that with something else?

BB: No, it is not sorted out on amplitude. It is mainly sorted out on apparent velocity. See, if the wave stays in the near surface, it is traveling always in sediments that are slower. Maybe 40% slower than a wave that goes to a greater depth and comes back up. Say they arrive at the receiver at precisely the same time, or at two receivers where you can correlate from one to the other, you will notice that the difference in travel time between receiver one and receiver two for the multiple will be greater than it will be for the primary reflection. And that is the basis for designing the way that you add your data together, to emphasize the primary reflection and de-emphasize the multiples that are clouding and giving you false dip information and things like that. Where true amplitude recovery really came to the fore was with the bright spots, because now . . .

TP: I only said that because Mike Forrest mentioned with true amplitude recovery, it was able to recognize the . . .

BB: Yes, this is now where reflection strength, not just the shape of the reflection, things like that, but the strength of it, was going to be analyzed to make an estimate of the fluid content of the rocks. I don't know whether someone explained to you or not, but let's stay largely with the Gulf Coast, where the sediments are quite young relatively. A lot of the sand grains are not cemented together. They are in a relatively loose matrix. There is some cement but relatively loose. The acoustic properties of a rock can be described, estimated as being the product of the density of the rock times its velocity. If everything else stays the same in a reservoir sandstone, but you replace the water in there with hydrocarbon, the hydrocarbon is compressible. Water is basically incompressible. The hydrocarbon is compressible, so it makes that rock appear to the seismic wave to be much softer. So, one has to know that. One has to know the acoustic properties of the reservoir sandstone relative to the encasing shales. Are the encasing shales harder or softer than the reservoir rock? And knowing that, then you can basically see the probability that you should see in amplitude effect if there is oil or gas in the rock rather than water. And typically, gas will give a little bigger effect than oil does. They both give a bigger effect than water. A bigger effect in softening that reservoir rock. So, Mike Forrest and Billy Flowers, the exploration manager, at the time, got together about six examples of this, and they were beautifully . . .

TP: Well logs that you correlated with the seismic . . .

BB: The well logs. The six case histories, Billy brought them to the lab. Yes, they were well logs. A little geological history with the knowledge we had and, of course, the map and things of that nature. They asked the lab to give the theoretical support to the observation. And so, I gave those examples to Dr. Aaron Seriff who was our senior scientist, and in an extremely short time, a matter of days, he had examined all the data, looked at the theory, and had the equations that described the effect in a quantitative sense, and we were off and running.

TP: Now, Mike first mentioned that there was a group at the lab that had published or written a paper that said, right before he made his observations and came up with the six-well test, that it wasn't possible to directly detect hydrocarbons with seismic, but that once they got the well logs, then they had the quantitative data . . .

BB: I don't know what he is . . . I'll tell you what. That would be a heck of a thing to put in the report because in hindsight now, that would not put Shell in good light for the following reasons: in hindsight, we could see there was a report written by a fellow fairly early in the game with Gulf, Winchell, that alluded to this, and there was a report back in the 1950s written by a fellow by the name of Hicks, with Mobil, that had to do with velocity logging in well and that talked in terms of using the softness of the sediments with hydrocarbon in them to prove that there were hydrocarbons there. So, there were things that were in the literature . . .

TP: But no one had really acted on it . . .

BB: Well . . .

TP: In the way that Forrest did?

BB: Well, the thing that is crucial here is that for the majority of rocks, the method will not work. The rocks are too heavily cemented. The Gulf Coast deltaic rocks are the ones that are most amenable to this technique, and it works marvelously. Now, in cemented rocks, it works . . . sometimes you can detect porosity or something in a carbonate or something with that. There are variants on this, but to see the hydrocarbon directly, the Gulf Coast was the unique situation.

TP: So, bright spot didn't work as well in other provinces?

BB: Oh, in many, there was no effect, or no measurable effect. Yes. Things have to be going for you. You have to have good record quality. Good signal quality. And rather discrete layered geology enough, things like that. But most importantly, not heavily cemented rocks. Partially unconsolidated and relatively high porosity.

The old rule of thumb was that below a depth of 10,000 feet in the Gulf of Mexico, you were on your own for making a hydrocarbon call based on bright spots below that depth, for several years. And by the time we got to deepwater, where those sediments out there are so young, so porous, so unconsolidated, we were making bright spot calls at a depth of almost 20,000 feet. So, right here in the Gulf of Mexico, we can see variance. You can go to some of the onshore rocks at depths of 10,000 feet and you won't see much of any affect at all. So, you have to know where your . . .

TP: What other areas besides the Gulf of Mexico were you able to use bright spots effectively?

BB: Oh, gee. Go to the Deltas again. Niger Delta.

TP: But not in the United States? There aren't many others in the United States?

BB: Oh, now, I am not going to say that. They have been tried in Alaska, California, but, you know . . .

TP: No other areas around the world?

BB: To the extent that they really worked like gang-busters, it is the Gulf of Mexico. But I think the important thing is this technology, this technology that was developed in the Gulf, has other nuances that had great applicability in other places, tremendous applicability. O.K.?

TP: Such as? Can you tell me what those were?

BB: Sure. True amplitude recovery. I described a few of them already. To see stratigraphic variations, to see where reservoir bed perhaps pinches out requires a better signal strength. These sorts of things are there. And any place you have a well log penetration where you can calibrate your seismic right there and walk away from the well log to extrapolate nearby, you can bring all those technologies to bear in a calibrated sense. But it was the Gulf of Mexico that really got us, in a production sense, to start doing calibrated geophysics that ended up percolating all over the country.

TP: Now, there were some problems at first with bright spots. There were things that still had to be worked out, such as phony bright spots . . .

BB: Oh, yes.

TP: Can you talk about what kind of things the lab did?

BB: Aaron knew this right off the bat, and I think this may be getting around to some of the things of a publication that would say it would or would not work. No, it was well-known from the beginning that one only needed a small saturation of gas to give the signal strength effect. Twenty percent saturation of gas. There could still be 80% water. Now, of course, that is not a gas field. That is just a pain in the neck! But, you know, the people in New Orleans developed other criteria very soon to help them with that. I am sure they talked to you about this: the geological calibration, the greatest proof that you have a true bright spot is after you get the amplitude effect. Particularly if it is a fat sand, you might see a stronger effect at the shallowest part of the sand that would be gas, then an oil effect, and then a drop off to the water. And the part way you dropped to the water amplitudes fits the structural contour. Then you know that you have a structurally controlled water level. And then, you can get much more certain about it. Or if you can walk away from a nearby well log that does not have hydrocarbon in it and see going up structure and increase in amplitude. There are all varieties like that, that help you. Very often, you see the termination of, or sometimes, you see the termination of the event down dip, you can literally see the flat level there as you pinch out on the water way. You just see it. So, these are all variants that come in to play. So, it requires geological calibration. O.K.?

TP: Yes, it is an interesting story.

BB: This might be a good time to ask any other questions.

TP: About bright spots?

BB: Yes. What I still plan to come to is going through the 3-D work, and then how we carried it out into the deepwater.

TP: One thing I am wondering . . . two things, I guess. The first is, can you maybe just generally or briefly talk about the evolution of computer technology that enabled you to get to process the amounts of data that you needed to . . .

BB: O.K., through the 1960s and 1970s, computer capacity was always the limiting factor. Our theory was well ahead of our ability to fit the data in the computers. And I mentioned earlier that we started out with digital processing in the Houston area, borrowing time on the IBM machines. By the later 1960s, we had purchased by that time, UNIVAC machines, and the data processing center was located there at the lab, in Bellaire lab.

TP: They had the lab first?

BB: Yes, it was run by a fellow by the name of Fred McBride who . . .

TP: I was confused because I know that they had the information data systems . . .

BB: That came later.

TP: . . . that was built at the Astrodome, but that was later.

BB: That decision to move to the Astrodome was about 1970, something like that. O.K. at that time, it looked like the UNIVAC computers were the best suited for specially dedicated computers for doing processing. And, as I said, the complexity was increasing very rapidly, particularly the number of channels that we were trying to. . . . the amount of data we were trying to feed through there. One of the things that we did at the lab that, for the time, was pretty darned advanced: We saw that we could speed up the effort quite a bit by, if we would build a special purpose processor that would interface to the UNIVAC, it could grab many traces at one and make these corrections that I talked about, the preprocessing corrections. We built a twelve-function processor, preprocessor, called an array processor, interfaced to the UNIVAC, one of the real early, what could be called parallel processors that we were built in the industry. We built it for UNIVAC and we were using it there at the lab.

So, to show you how rapidly technology was moving forward, on the development side, when I went to the lab in 1967, Gerry Pirsig outlined a challenge for the lab that we were to devise a seismic system capable of handling 1000 receivers. You know, here back in the 1950s, typically we had 24 receivers and one individual sound source. By 1970, we were already recording with up to 100 channels, recording channels, and Jerry wanted us to look at what we would have to do to go to one thousand. So, that work was done in a small research group we had at the lab called exploratory science. Basic research. It was headed by Mike Papadapoulos. He was the director.

TP: So, this was created in about the mid-1960s? 1965? This exploratory science . . .

BB: It was the creation of Tom Baron, in 1965-1966. By 1967, we got serious about this challenge. We ended up doing some things. One of the profound things . . . it didn't help us directly in seismic of the day but . . . we had an electronics scientist there, his name was Alton Christensen, who took out, I believe it was 26 patents for Shell in a new technology called integrated circuits. MOSFET. Do you have any of that from Bob Nanz or anyone?

TP: Well, it is in this lab history.

BB: O.K. The interesting sidelight of this: We had people like Stanford Research Associates, I forget who else, evaluate the technology for us and our position, and they basically said, look, you, Shell, have two choices: 1) Do you want to get deeply involved in developing computer technology? The kind of technology you are developing here, we literally underwrote a company in California, a small start-up company, to build some small random access memory chips. One possibility was . . . to begin to become a computer manufacturer. The other possibility was to license the technology. And a decision was made in 1969 that we were going to put all of our horses in the oil businesses at that time. It is interesting how . . .

TP: It was one of the most profitable licenses for Shell Development that they ever had.

BB: It was very, very profitable. But the potential was perhaps staggering.

TP: I guess, in hindsight, it is easy to say, but did you regret not having moved in that direction?

BB: Well, this is what I call "beer hall" talk now. Or as Jim Mora, the former football coach at the New Orleans Saints, in answer to a question like that from a reporter after the Saints lost. After he got done cussing at him, he used the phrase, "woulda, coulda, shoulda."

<u>End of Side A</u>

<u>Side B</u>

BB: O.K., let's pick up the story again in the late 1960s. I would like to mention one other person that I felt at that time had a very important role in developing the jump forward we were making in geophysics, and that is Bill Scaife, W.P. Scaife. Bill had taken sort of a special assignment at the laboratory. In about 1967 or 1968, Gerry Pirsig asked him to move to . . . or slightly earlier than that . . . maybe 1966 . . . had asked Bill to come to the laboratory and bring together all of our development work and seismic processing, things like this, to really make it a package, a more useful package for all our operating areas, and to get the word out, to get our entire staff, a great leap forward in getting the entire staff educated in . . .

TP: People in the operating areas?

BB: People in the operating areas everywhere . . . educated in this new information technology. I could say the people in the earlier 1960s that made the preliminary rounds in doing this education work was a man from the lab, Frank Hallisbart, and he gave lectures. One younger geophysicist who heard those lectures either in the Houston area or the New Orleans area, I forget which one, was Harry Hasenpflug, who took very careful notes of all that Frank was putting on the blackboard, all the equations, things like this, and those notes were xeroxed. They were handwritten notes that were xeroxed and distributed throughout Shell. Many informal meetings were held by Shell staff to digest those notes and learn the fundamentals of information processing technology that was behind the new seismic digital techniques that were coming in in the 1960s. But, by the late 1960s, this had to be formalized, and Bill Scaife was given that task. When the package of bright spot materials came to the lab, I just took two people in the . . .

TP: Were you the director of exploration and research at this time?

BB: Yes, I took two people into confidence about this. One was Aaron Seriff who did the theoretical work, and the other was Bill Scaife, who immediately got into the work of how all of our . . . the various technology that we had could be brought to bear to enhance the processing of data for bright spot technology. So, that was the ground work that carried us in our geophysical activities through the 1970s. Those were the people from the laboratory that . . . in fact, Bill Scaife . . .

TP: So, did you have conferences and seminars for people to help disseminate . . .

BB: Well, there was a training group over there, a major training group . . . people would come over for a week or a month to study depending on the level they needed. But Bill Scaife transferred to New Orleans right after that to manage the geophysics effort over there, to bring it really to its full fruition.

So, O.K., also back at that time, in the 1968 period or so, we knew that the way we were migrating seismic data at that time, that was the program that was really choking the computers, absolutely choking them . . . had some very limiting approximations in it with the way that we were correcting data. We were stacking the data first and then migrating the stack of the data. And we knew from the beginning that the preferable term would be to migrate the data -- preferable in that you could be more precise that way and, in the process, get better velocity determinations also – and then stack the data after you migrate. So, it was impractical to do, but we fussed around with it and came up with, I think, the most efficient technique there was to do the pre-stack migration with better velocity determination in it and things like that, in the late 1960s. And we had some migrations then that just all . . . we just made it so apparent that that was the way to go. It was much cleaner, and, of course, so many oil fields only occur where there are structural complications, and that is right where the migration was falling down. So, they cleaned that up quite a bit. And the byproduct of the way geometrically we structured the calculation for, the way we fed the data into the computer just led itself directly into going from two-dimensional to three-dimensional migration. So, we had the theory and the practical application of three-dimensional seismic at the laboratory in 1969, and it was about 1975 before Bill Schneider with Texas Instruments published on that in the industry. But again, it was not a practical technique because we didn't have the computer capacity. We had to wait for the supercomputers. Of course, as computer capacity increased, we could slowly begin to build to 3D migration.

O.K., by the late 1970s, let's see, I got transferred over to New Orleans in, I think, if I have the date right, 1977, to head the offshore division, Offshore Exploration Division.

TP: Do you need to talk about your time in head office at all?

BB: No. I think that has been worked in the Michigan play and that stuff. It was apparent that the shallower water shelf play at the shallower depths where bright spots could be observed had pretty well run its course. Industry was extremely competitive, and the big discoveries had been made. And there was a lot of emphasis in Shell Exploration to put a significant portion of our effort into looking for large scope projects that would be higher risk but if they came in, you know, could significantly affect the company. And we knew that we were going to continue to work the conventional Gulf of Mexico, the so-called "shelf" water depths of less than 600 feet and things like that. We were going to work it first, going for the ultra-deep horizons where normal bright spot activity effects are no longer observable, but we were looking for gas plays there. But we also began looking out into the deeper water. And we laid out some new seismic reconnaissance probes out into the deeper water . . .

TP: You are talking about this is still the late 1970s? 1978? 1979? Around there?

BB: Yes, I am saying 1978-1979, this timeframe. And it turned out that we already had inklings from a little earlier work around our "Bullwinkle" discovery, there might be a nice oil field there. But we got a well-oriented line over that area and with the new seismic, it showed the three oil pays there just as clean as could be. And we saw other effects but, you know, "Bullwinkle" was in 1,350 feet of water. So, industry as a whole was not interested in those water depths yet, and, as a consequence, we could not get the Department of Interior to put any of the acreage in those water depths up for sale. So, Billy Flowers and I and Lloyd Otteman, who was production manager at the time, put together a road show where we had several concerns about where the . . .

TP: Were other hands involved in this, too?

BB: No. Where the offshore play was going to go in the Gulf of Mexico and places like that, along the East Coast and so forth. So, we gave talks . . . I had a packet of probably about 40 view graphs or something like this, of examples and things like this, that get tailored for whatever group we were talking to. We gave talks . . .

TP: Like specific groups?

BB: Oh, no, this was to government officials. We talked to the highest levels in the Department of Interior, the geological survey and so forth, in Reston or New York or Washington. We made several approaches at them. Of course, we were a little restrictive in what we showed, but we didn't want to overstate the potential out there, or anything like that, but we didn't show everything either. But I had to make a view graph of this line across the "Bullwinkle" prospect for a trip that we made to Reston, and showed that to Bill Menard privately, you know, and just said, "Bill, look. This is one possible oil field. It is obvious it is in water depths greater than 1200 feet," or something . . . I said greater than one thousand feet . . . "but, you know, through the last couple of cycles of nominations, we nominated it and you people haven't put it up." I had several other examples. I said, "Look at other seismic we have across things that you have put up," and it just paled by comparison. And I think, in a long way, that rang a bell a little bit, but by the early 1980s, of course, with the Reagan Administration coming in and so forth, that got opened up and we were able to bid.

Now, we had to have a little courage to go out into the deeper water. There were publications in the industry, in the trade magazines and so forth, about concepts for floating platform technologies that were just coming on stream. We knew we would have to count on a lot of technological growth and cost reduction and things like that to make it work, but if the plums were big enough, we could make it. Here is a place where some of the earlier geological work that had been done in the Gulf of Mexico and also in California with turbidite reservoirs . . . [PAUSE]

I would like to talk now for a minute about the role that some of the earlier geological research played in this decision to go to the deepwater. As I mentioned, research work was done in California, in what is called an active continental margin . . . a big mountain building forces right near shore and things like this. And then there was the Gulf of Mexico, which was a passive margin, very settled normal deltaic setting, gave us some confidence that we could interpret the seismic well enough out in the deep water to make the play. So, we began to focus some of our seismic at shooting not over the crest of the salt structures that were out there but shooting on the flanks of the structures, because turbidites are going to tend to avoid more of the structural crests than they do up in the deltaic setting. So, we put probes out there that were more in basin centers and things like that. Not right in little mini basin centers but near the centers and, lo and behold, we started seeing reflection amplitudes. But these were occurring, some of them were occurring at depths greater than our existing theory told us we should expect to see amplitude effects.

TP: Not water depths but . . .

BB: In the subsurface. Down to depths of 20,000 feet, for instance, and well beyond the horizon of conventional thinking. So, if you look at it through those sets of eyes, the decision to go to the deepwater was a very courageous decision on Shell's part. But it was driven by our confidence in our technology that those amplitude effects were real, indeed real.

TP: What was the greatest depth up to that point that you thought that they were real? That you could be sure of?

BB: 10,000 feet, maybe, with a stretch to 11,000, something like that. You know, we had glimmers deeper but when you are talking about . . . when you make a hydrocarbon call with Shell, you are actually measuring the amplitudes of the reflection events. You are doing it quantitatively. And you are measuring how much the reflection swells over the background outside of the hydrocarbon zone and, generally, we said you had to see about a 40 or more percent increase in amplitude for it to be considered a real effect. Otherwise, it could be just normal stratigraphic variation of things like this. So, we were down in that range. So, we had to be very sure of what we had and quite often, these down dip amplitude fits that I said to a structural contour are corrupted out in the turbidites because you have a strong stratigraphic overprint on your reservoirs and things like this. So, we really had to believe in that technology, and we did, and that is what brought the play on.

"Bullwinkle," that was in 1,300 feet of water. I don't know what the production depth is there, but it is not horribly deep. But the one that probably really broke it was when we drilled "Auger" . . . had some intermediate depth bright spots that were quite strong at 11,000-13,000 feet, but then the well was taken down to 18,000 or 19,000 feet in front of the deep pays. That was when the scope of the play suddenly got quite a bit bigger.

TP: Then you started realizing the kind of production rates you could get?

BB: No, that wasn't realized yet.

TP: It wasn't realized yet.

BB: No. I am talking now about the people . . . the questions that were asked at the highest levels in the company when we were getting to the discussions of whether to develop, say, "Auger" for instance . . .

TP: Not before you developed "Auger" but once the platform was there . . .

BB: Once the "Auger" platform was in, and we saw that those deep sands, 18,000, 19,000 feet, were still capable of producing at extremely high rates, that made a sea-level shift in our production strategy. Then after this, our development strategy would be to come in with larger bore holes to begin with that could accommodate bigger production tubular goods and get the production rates up. But I mentioned just in passing earlier that we knew from the beginning we had to count on technological progress, a learning curve, if you would. The first thing that we began seeing in the early drilling was that our drilling people were drilling the wells faster.

We saw at "Bullwinkle," a little side line . . . about one week or two weeks before the "Bullwinkle" platform was scheduled to be launched at "Bullwinkle," one of our geophysicists, Dave Johnson, recommended that we go in and shoot two special 3D surveys over the "Bullwinkle" prospect: one where all of our survey lines were oriented in the north/south direction, and then come back and shoot the same survey that the survey lines oriented in the east/west direction. So, we would have two independent surveys we could lay on top of each other. The reason for doing that is "Bullwinkle" lies in a little mini-basin surrounded by intrusive salt structures all the way around, and we were shooting with relatively long offset cable. At that time, I would guess we were towing a three-mile cable behind our seismic ships. So, part of a lot of those ray paths are corrupted by travel through the salt which just tears them out. So, we could take pieces of a survey shot in the north/south direction and then other pieces shot in the east/west to avoid the salt, and made the composite interpretation of "Bullwinkle." The "Bullwinkle" platform itself was designed to accommodate some 60 well bores. And after seeing, with the predevelopment drilling that we did to calibrate our work, we had several penetrations in the pays, getting that exquisite seismic, that whole field was developed with about 30 well bores. In the process, we recognized there were more hydrocarbons there and that we had a little more, 20% or so more, than we had predicted before, and we could cut the number of well bores way back and were going to get the high recovery factor. So, putting all the data together, we were just able to design a much more optimum program. And, of course, that extra boring capacity that the platform has is now being utilized not only as a producing platform but as a transfer platform to off-load production from the deeper water things, it has processing equipment on that. So, it was a blessing in disguise. So, it all worked together.

O.K., you asked a little earlier about the *Shell America*. That was a beautiful ship. Again, we were maybe the last major oil company that operated any of our own seismic vessels. The reason we wanted to do that was to have absolute control over data quality, from beginning to end. And that pays its dividends in the true amplitude recorder, true amplitude recovery process, the whole thing. We do not use automatic volume controls or anything like that. It is all true amplitude and gives us a little better facility to make quantitative rather than qualitative measurements in our data. We knew from years ago that it is always a good criterion to have your longest receiver spread on the surface, your receiver spread out, to at least the depth that you want to explore in the earth. So, if we were looking for depths of 20,000-22,000 feet, we would want a cable about that long. And we started out with three- mile long cables and got up to four-mile cables. And then, got up to towing more than one cable at a time and things like that, and having more than one source at a time. So, it was a multiple-source, multiple-cable operation, and the people on the *Shell America* were able to catch all those data in the computers and get them preprocessed to go to the data centers. So, it was an integral part of the effort at that time.

We had other vessels. They were all equipped about to the extent that they could carry equipment. That part worked real well, real well.

TP: You mentioned, you got into a little bit of 3D seismic when you were talking about migration. Could you talk a little bit more about the development? When you needed them, when you did not need them? Because it gets expensive, and so, you weren't always using 3D, even after the technology was there. Is that right?

BB: By 1987, we were shooting large proprietary 3D surveys in the shallower water shelf area for what we felt would be the last round of exploration work through that belt that had some of the early bright spot discoveries -- "Pine," "Cognac," "Posy," that dip position, say. And for the deeper water part at that time, this was early reconnaissance exploration, so the work was largely 2D. The deep discovery at "Auger" was made in 1987, and the decision was made soon after that to develop immediately other drillable prospects. And we had "Mars" pretty well in the bag, and we knew that "Ursa" was over there but we had to do land trades to put the land together to get that. So, we had a few there that we could see them before us, for the things looked like they had a lot of heft. But, I'll tell you, when we drilled "Mars," we were very concerned that we would have enough pay to make the field economic. It was not a lead-pipe cinch. We felt quite confident that there was some hydrocarbon down there and we were drilling after one particular zone that we felt good about. And, of course, we found more than half a dozen zones at depth, and, again, real confirmation of the play concept that the hydrocarbons are not all nestled high on the structure. It is well off the structural crest, the sand section bloomed as you would expect a turbidite to expand, and the hydrocarbons had a strong stratigraphic trapping component to them, too. So, that gave us the indication that the tool would be working. It doesn't take a mental giant to go from that point to thinking through several spots in the world where other situations ought to be there.

TP: These were all beyond the continental shelf?

BB: Yes.

TP: And then you started looking at regions beyond the continental shelf?

BB: Well, yes. There is something about being out off of the shelf, you know, again, the turbidite geology . . . if you have the good geological models, it helps you make the interpretations of your seismic data before you get a lot of well control out there. And indeed, we saw that the early drilling by industry out in those deeper waters, there were a few wells that were drilled out there, courageous deepwater wells that were drilled, but they were drilled up on the structural crests. And they were basically sand poor.

TP: And these are salt domes still?

BB: Yes. You get off the crests and you see quite a section of expansion. The seismic was just working perfectly. I have often thought back to the talks that Ron McAdams used to give us earlier about: we've got this seismic that is so good now . . . where are the other stratigraphic traps that we could see alone? Mac's vision from the 1960s came into play out there in the deepwater.

TP: Do you think this had any affect on Shell going international in the 1970s? You know, you had this great technology . . . you felt like you could apply it to other regions of the world? Was that a factor in all of that?

[PAUSE]

I guess the thing I want to just confirm is, going back to the 3D seismic, that it is most useful in already developed fields? Is that right?

BB: No. You used a phrase in the way you asked the question earlier . . . you said, "3D seismic is very expensive." And the standard reply to that is, yes, it is very expensive, but it is truly cost effective. And that is why I gave you that example of the development program at "Bullwinkle." That was done with very good production department's geological engineering based on 3D seismic.

TP: So, did you use seismic in the frontier exploration areas?

BB: It is getting to the point now that it is used quite a bit, yes. The processing costs have been driven down low enough and I think it is not much of an exaggeration to say that it won't be long before probably the vast majority of the continental shelves of the world will be covered with 3D seismic of high quality. So much of it is done in group combines where many people share the cost. Shell opted, back in the 1980s, to do more of it for a proprietary nature because we were interested in land acquisition then and things like that. So, it is a question of the timing and things like that.

TP: I know you mentioned that you had the capability to do 3D seismic, at least the knowledge, in the late 1960s, early 1970s . . .

BB: We literally had the computer programs that were capable of doing it then, yes.

TP: So, you had better capacity.

BB: The computers just couldn't take it, yes.

TP: Can you talk about what contributions has Shell made to the development of 3D over the years? How does Shell's . . . I guess 3D is a pretty standard technology that is used by everyone now or . . .

BB: Oh, yes.

TP: . . . is it possible to see, you know, some companies doing it or using it better than others?

BB: Well, the way to view industry is a term that I learned from Bob Nanz years ago. Multiple working hypotheses. You know, that is the environment we work in. And Shell had . . . well, I'll give you an example of Shell leadership that is not 3D *per se* but is an early vintage, very research-oriented seismic vessel. In the late 1960s, 1967-1968, our laboratory in Houston had a hand in outfitting a worldwide ranging seismic vessel for the Royal Dutch group. It was called the *Lady Glorita*. And, if memory serves me correctly, all of these instruments were on it: It had a seismic system on it. It had a marine magnetometer on it. It had a marine gravity meter on it. Of course, it had a sea-bottom profiler on it. It had some capability for sampling hydrocarbon concentrations in the water that they traversed through. And it had one of the early global positioning system navigation systems on it, where at that time, there were just a couple of satellites that were available to the nonmilitary users, and you might only get a fix every hour or two but you had to know where your seismic vessel was, or your vessel was, for the measurements to have any use at all. So, we could use other dead reckoning techniques between the satellite fixes. We put that on back in the 1960s. So, it was one of the early integrated systems that had just many sensors on it to do a complete suite of . . .

TP: Had Shell launched the three modern seismic vessels around the same time?

BB: Well, they build the *Phaedra*, *Artemis*, and the *Niobe*. Yes, and those were in use up until the 1980.

TP: They weren't equipped with this suite of instrumentation that you are talking about.

BB: They didn't have all those instruments on, but they had quit a few, yes. But when you work in the Gulf of Mexico, your positioning is determined by radio location; you know, Radist or Loran or some of those. But if you are going to go on a worldwide cruise, you have to use satellite or something like that because you are not going to set up Radist stations all over. So, we were early in the development of that technology. We were developing . . . we were early in the development of just radiolocation technology. Right after World War II, that was developed by the Americans, British, and Germans immediately beforehand during World War II. We used that, so we had radio-location stuff. I think our contribution to industry as a whole has largely been utilization of these technologies in just about every exploration theater and the knowledge of what works and doesn't work permeates all through industry. Whether we want it to or not, it is there. And yes, we have been a major player. We have utilized an awful lot of contract services and things like that. So, that is . . .

TP: Well, I think we have covered quite a bit of ground here. I don't have any more questions.

BB: O.K., well, I'd be happy if, as you write this, if you have any questions, I could try to clarify any that you would want. I would be happy to do that for you.

TP: This has been very helpful. I will shut the tape off.

THE END