SHELL OIL COMPANY

ORAL HISTORY PROJECT

Interviewee: JOHN REDMOND

Date: November 12, 1999

Place: Houston, Texas

Interviewer: Tyler Priest

Code: SOC023

Keywords: Prod, Shell, Mgmt

Bio

After graduating from Tulsa University with a Masters degree in Petroleum and Geological Engineering he started at Shell as a Chemist in 1936. In 1941 he helped fully establish the Bellaire Research lab. He spent 1945-1948 he served in West Texas, Midland, and East Texas. He transferred to Houston as Chief Exploitation Engineer in 1947-1948. He served in that capacity until moving to New York as Chief Exploitation Engineer in 1954-1955. From 1961-1965 he served with Shell Canada, and in 1971 he became Shell Oil's Executive Vice President for Exploration and Production.

Summary

This interview begins with a discussion of the Bellaire Research Lab and some of the experiences at the Emoryville Lab. There is commentary on Shell Canada with asides on offshore Canada on both coasts. Great information on Cognac and the bidding associated with that project. Additional information on the financial side of the business including the evolution of present value budgeting in Shell.

Tape #1, Side A

TP: This is an interview with John Redmond on November 12, 1999, at One Shell Plaza. Like we discussed, I thought we would start with the early postwar period. Tell us what position you were in at Shell and what was happening, from your perspective.

JR: I went to work in 1936 as a chemist in Tulsa. At that time, in the very first work we had done on the analysis was on properties of the reservoirs -- porosity and permeability. These are things that are very common now but, at that time, had really not been done in Shell: fluid content of the samples, core samples and reservoir samples. I was in the laboratory there for five years overall. During that time, I was at Emeryville for about two years. There were two of us: one from West of the Rockies and from the East Rockies. There were then separate Shell entities, in a way I was the first one to go to Shell Development as an engineer from East of the Rockies for training and to work. It was the beginning of creating a liaison between research . . .

TP: This is before the geophysical research lab was set up in Houston?

JR: The geophysical lab was well-established. In the early days here in Houston, it was by itself. It was the first building on the Bellaire property. We added to the Bellaire property to build the laboratory that is there today. But the seismic work

was going on, and that was all the research that was being done in E&P until about 1933 or 1934 at Shell Development, which started in 1928. They created the exploration, production and research group. It was under Dr. Alfred Loomis. So they were staffing that with people whose names are well-known in E&P. But they also selected a candidate for training the East and West Rockies organizations. It was really the beginning of the liaison between research and applied work. I just happened to be the one from East of the Rockies. James Murphy from the West. So we worked there about two years.

During that time, I designed and built the production laboratories at Long Beach, Ventura and Bakersfield. Then I came back to Shell Oil in 1938 in the Tulsa area. You have a letter from me that I wrote to Jack Little.

TP: This is the Toni van Everdingen group?

JR: Yes. He was there when I went to work in 1936, but then was there for several more years. I then moved to Houston, which then became the focus of East of the Rockies. Toni put together an applied research group, and I came down to join that group after I got back from Emeryville. Gus Harpatee (sp?) was here and the names that I mentioned in [the letter]. I came down in 1941. In 1945, I left that group and went to West Texas as an exploitation engineer. The fellows were still at war. They were very shorthanded. A couple of us were handling all the

drilling rigs in West Texas. I think we had 16 rigs going at a time but it was great fun. It was encapsulating an experience in a few years that you couldn't have had in a career. We were doing it all from the surveying, the location, to so-called "staking it," to working with the contractors. We had some Shell rigs there, one or two, but most of them were contractors.

Right through the building of the well, the evaluation of the formations, production and setting the tubing, and all the things that normally would have been done by several people -- we were doing that at the time -- simply because there was nobody else to do it. You had an opportunity that you couldn't create, even if you tried, under normal conditions.

Then I went into the division offices in exploitation engineering in Midland for about a year. Then I went to the east Texas division as Division Exploitation Engineer. I was there maybe two years, and I went to Houston as Chief Exploitation Engineer.

TP: And this would have been about what year you came to Houston?

JR: I guess it was in 1947, 1948.

TP: That is when they set up the Bellaire research center?

JR: During that time. I was a part of that group that established . . . what we did here and what the program would be.

TP: That was unique within the industry, wasn't it? All the other companies were setting up their own research functions, but it was a pretty ambitious project.

JR: It was ambitious. Probably the most ambitious part of it was when we separated it out from Shell Development at Emeryville. Exploration and production were being treated as a separate research group. They decided to have it here in Houston and to build this laboratory. Dr. Harold Gershinowitz, who was really the first . . . We all talked to him about what we thought ought to be done, what should comprise the research program and what is involved. Quite an array of things. We went beyond the geophysical. We then started working in a more complete way: the geology, geological properties and the tools that the geologists used in making their assessments.

TP: You mentioned in your early interview a little bit about establishing the interface between research and operations, especially on the exploration side -- how the exploration people were integral to pushing research forward. Was it difficult to iron out problems and areas of jurisdiction between what people were doing in the field and then in the research center?

JR: It really wasn't, and the reason was we were still a small group, and we all knew each other. We were accustomed to working with each other in the field because a number of the people that staffed research came from the field. Not all, but some. So it was really quite fortuitous, but it was the beginning of a real effort within Shell to remove any kind of barrier between research and applied research. That really started with Toni van Everdingen in our group of applied research. So Bellaire just became another facet of it, a very important facet, because we then extended the work that we were doing to a research program. I don't know if you want to get into what all we were doing, but it was as broad as we could envision as to what our problems were in production as well as exploration.

TP: This is when you really started getting into developing techniques for secondary recovery and water floods?

JR: No.

TP: Was that a little bit later in the 1950s?

JR: Actually, that was started in Tulsa. This Tulsa laboratory that I started was the first real effort on secondary recovery. The secondary recovery properties were in northeastern Oklahoma and southeastern Kansas. Later on they got into Illinois.

But that was all done in the Tulsa area.

TP: I didn't realize that.

JR: So when Bellaire was started . . . in Emeryville, we were working on certain facets of that.

TP: Was this prior to World War II or during World War II?

JR: Prior to. That is why Toni van Everdingen went to Tulsa and why we started the production lab there, which was the first production laboratory in Shell. I did start it, although Henry Kepplinger was there for a few months before we had our lab in the petroleum engineering building at the University of Tulsa. Then we moved out of that and had our own laboratory. But, no, the secondary recovery work was started in the Tulsa area because that is where the properties were that we were working on. Then, of course, certain research was done on how to stimulate the recovery of oil and displace it with water and all this. It was part of the Emeryville program. Of course, it was continued here and expanded when the Bellaire laboratory was started. But it began in Tulsa.

TP: Well, it is interesting to know the roots of research development on the production side because people look back to the creation of the Bellaire Research Center. It

is interesting to see how it had been implanted in various parts of the Shell organization.

JR: That's right. As far as the secondary recovery was concerned, it was really a Tulsa area problem.

TP: The Group was working on similar things at the same time?

JR: They were but nothing that really added to our problem here. If they were, I am not all that aware of it. Van Everdingen ran everything over here. And putting him in Tulsa was to create a group that would work on secondary recovery. That is how I got involved in building the first laboratory.

The first effort to determine the oil and water content of core samples was in Tulsa. I designed the equipment to do it. We had gotten porosity equipment from the Bureau of Mines, which was done in Bartlesville and was working on this kind of problem. The Bureau of Mines was quite active in this sort of thing in those years. We had purchased/ordered permeability equipment for measuring the permeability of rocks from Penn State. So, the first permeabilities were run in Tulsa, as well as development of the saturation equipment. And then we got the PBT -- pressure barrier temperature equipment. We built it there and I designed the first of that for Shell, for measuring and taking bottom hole samples or surface

samples -- if they are oil and gas, then recombining them, and to determine their characteristics.

When I came to Houston in 1941, I would continue that work. I was really concentrated on the PBT side and that design at PBT Laboratories for Los Angeles, The Hague and Maracaibo in Venezuela. We didn't build it here, but there was another one built in the Malaysia area. I never was out there but I did have to do with the other ones. It is cooperation between the two.

It started very easily because it was a small group. The research group was comprised of operating people, too. We carried that on with a real determination that we would never let a curtain develop between research and operations. As a matter of fact, as far as the responsibility is concerned, we made it very clear to the operating people that it was their responsibility to pry whatever knowledge that they needed to do the job they had to do -- whether it was from Shell Research or whether it was from the "oil and gas drill," not saying it wasn't by other companies. So by putting the responsibility on the operating people for applying research really helped this, because the research people were just doing work in isolation and hoping the operations people would take it up. We stopped that by changing . . . I personally, if I may say so, had something to do with that sort of thing.

We in Shell were given credit by other companies right through my whole career of having something that the rest of them didn't have -- a cooperative spirit between research and operations.

TP: There was also a decentralization in the late 1940s in E&P, right? Field operators were given more authority.

JR: This continued through the years. My responsibilities were delegated and pushed out. That is very true, more so, I think than in other companies. But there was always this underlying responsibility, and it was up to that operator to apply whatever knowledge was available, wherever it came from. So the research people didn't have to set up a sales organization. They did, of course, because they had to put it in a form that the operating people could put it to work. But it was a cooperative thing back and forth.

TP: Was this the technical services group?

JR: Yes.

TP: Kind of the liaison . . .

JR: We were sort of the in-between of that sort of thing. Another thing that we did to

try to continually take people out of the field and put them into the research for a year or two, like Tim Murphy and I had done in the beginning. We also put research people out into the field. We also said that anybody running the research had to have field experience and that any of these super managers in operations had to have some responsible experience in research. And every one of us did, Jack Little included, right back on through. This was an overall effort throughout the years to maintain that kind of a relationship between research, and I think it was a real strength.

TP: Can you talk a little bit about the offshore design group? I guess that is more the exploration side.

JR: No.

TP: From the production side, what were you doing when you were looking outward to the really deepwater in the mid-1950s, and Shell was gearing up to try to explore these depths that were unfathomable at the time?

JR: I wanted to tell you something about the sharing of responsibilities or accountabilities in E&P. We were different than any of the other companies in that exploration had the job of -- including geophysics and whatever tools they needed -- to go out and find oil. If a project culminated to the point that they

could actually justify drilling a well, the responsibility for drilling that well and its evaluation of what they found was turned over to the production department because Shell production department always had a geological arm. We had production geologists that had to do with the geology of developing the fields. So the exploration responsibilities ended at that time. It ended as far as accountability is concerned. As far as working together, we always worked together. But the responsibility was clearly marked at that level.

From then on, if further development was merited of the reservoir, the responsibility, including the geology, was the responsibility of the production department. So it was the production department's job to determine if we find oil, what are we going to do with it? Or if it is at a water depth, how do we drill wherever it is? So the offshore design group was always in the production side and never in the exploration side. In 1963 or 1964 -- I was in Canada at the time -- we were looking at exploring offshore Nova Scotia and the west side -- Vancouver Island, up in the States and so on. Here we were getting into depths that exceeded the depths that Shell Oil was presently working in the Gulf. So naturally, we had to determine how to handle any oil we found out there. How were we going to drill the exploration well? How deep could we build platforms? That sort of thing. At that time, it was beyond our ability to build a fixed-leg platform. How would we work on the ocean floor? So Shell Canada made a contract with Lockheed at that time to develop the kind of equipment where we

could have a man working under one atmospheric conditions on the ocean floor. We tried to get Shell Oil interested in sharing that project with us up there because it purely was a development project. They chose not to because they weren't off that far yet, and they weren't posed with a problem of that depth. They could still build fixed-leg platforms. They had much on their plate so they did not go along with us. So, we carried that forward in Shell Canada poorly.

TP: The Shell Lockheed system really originates with Shell Canada?

JR: With Shell Canada.

TP: I didn't realize that.

JR: I signed that original contract. I belong to a group of aerospace and oceans people, the Sea-Space Symposium, we call it. We had a meeting in Washington just last weekend. One of our newest members was an engineer on that first project. It was very interesting. His name was Phil Newtley (sp?) and he runs the same company up there.

TP: Shell Oil wasn't working on subsea technology?

JR: No.

TP: When were they working on the MOBOT system?

JR: That's the MOBOT.

TP: The MOBOT was remote?

JR: I wouldn't venture to answer that question exactly because I really don't know. But they definitely were not interested in participating with us. Whether it was the idea of how we were going to do it, or whether they thought that it would all have to be remote, I don't know. But we were also considering remote at the time, and they were looking at every possibility that we might be able to accomplish this job.

TP: Just to clarify something for me: when did Shell Oil divest its interest in Shell Canada?

JR: 1963.

TP: And you went to Shell Canada after . . .

JR: I was there in 1961. From 1961-1965, I was in Calgary with the exploration and

production, and when we took the leases off Nova Scotia and Vancouver Island.

TP: Why did Shell Oil get out of Shell Canada at that time? Is that worth talking about?

JR: Sure. Shell Canada is an older company in North America than Shell Oil Company, about a year or so. But it was purely a MTM – manufacturing, transportation, marketing operation. They purchased all their crude oil, and some of it was coming up from Venezuela and so on. Shell Oil, in the meantime and before, was exploring on the Rocky Mountain front. As we moved up through and into Montana -- geology doesn't know about the 49th parallel -- our interest extended into southern Saskatchewan and southern Alberta. Mostly Saskatchewan at the time -- Midale, Weyburn, and so on.

So that exploration work was done, and the production was done by Shell Oil Company out of Los Angeles. The Los Angeles area did that. Casper, Wyoming had a bit to do with it, but Casper was then a division of the Los Angeles area. So it was really operated out of Shell Oil Company. Before I went to Canada in 1961, while we were in New York, we were concerned with everything that they were doing. It was our operation -- the budget and everything. So it gave us a chance down here to be familiar with that. After all, we were in another country's territory. But the corporate Shell Canada really had nothing to do with that at all.

TP: They didn't have the E&P capabilities?

JR: Completely separate organizationally. They were two truly separate companies. It was operated by Shell Oil Company. We had a close relationship with Shell Canada. But while they were separate, the accountabilities were separate and all. But more and more, it was obvious that that couldn't exist forever and there had to be a melding there. So in 1963, the company decided to spin off Shell Oil properties in Canada and they went into Shell Canada. Shell Canada then became an integrated oil company and the responsibility moved wholly to Shell Canada. Then I became a member of the board, for example, of Shell Canada and so on. But we always had a close relationship with Shell Oil, not only because it was the wise thing to do, business-wise, but we all knew each other. However, we did start to go off on separate worries because Canada was not afraid of the Arctic, and Shell Oil, I think to be fair, was. I say, they were "afraid" simply because it looked like sort of a formidable problem being under those conditions. But with the Canadians, being already there and working in the north and running wild, you didn't have that. I remember clearly, when I went to Calgary and started to look at how they viewed operating in the north, I really became a part of their team. I realized that Shell Oil had been really wrong in not extending their interests more fully to the north and to Alaska. Shell Canada, starting in the 1950s, had started exploring in the McKenzie Delta, so they were already up

there. It was a bit of a pity, if I may say so, that Alaska wasn't under them at the time because they would have taken that in stride, whereas Shell Oil Company kept it at arm's length. I had a lot to learn!

But that is how and when the separation occurred. At that time, Shell Oil got interested in Shell Canada's offshore. And here, we are talking deeper water, by 100 feet or so, not much -- certainly nothing like what is going on in the Gulf. The sea conditions were more severe than in the Gulf. I think anybody might say the Gulf of Alaska, for example, or off of Nova Scotia . . .

[PAUSE]

JR: That is when we started to figure how to do this. One concern was, because we built the platforms, whether to operate on the bottom or to work by remote. So we were paying attention to the remote, but our first order of interest was having a capsule in the bottom over the wellhead so you could lower a man into it and he could do work. Today it is a far-out idea, but back then we didn't really know how we were going to do it. Shell Oil chose not to get into that. I don't know whether you want to get into what the results were finally of Nova Scotia. We really never found . . .

TP: How did the Shell Lockheed relationship evolve from there?

JR: We went ahead with that project and developed it. We actually put one down in the water in the Gulf of Mexico. This was a little after I retired that they actually did that. I did some consulting work with Lockheed after I left Shell and this sort of thing. So actually, a capsule was put down in the Gulf, and it worked. We put men down there and so on. So there wasn't any question that the technique was applicable. We were designing to one thousand feet or that sort of thing, but there was no reason why you couldn't do it at greater depths, greater pressures, working pressures if you really had to. But we didn't go that far with it.

Back to Shell Canada's exploration on the West Coast. We drilled 14 wells off the west coast of Vancouver Island and up in Queen Charlotte Sound. The seismic information which we had out there was very promising. There was just one small area on Vancouver Island that would give us any kind of surface indication of what lie out in the ocean. We couldn't do surface geology and extend it because it was a different geological setup entirely. But there were large structures out there. What we found, instead of being salt, was shale diapirs. There really wasn't the sand out there, for development. So it never did . . .

TP: That is what I heard. It looked so promising first with the geology. Then you drilled and there was nothing there.

JR: But it even looked better on the east coast of Nova Scotia. They really did find in almost every well, if not every well, shows of oil and gas. And in a rough way now. We had the structures there, the domes and so on, as we had in the Gulf coast. But the sand layer content in the section was greater than in the Gulf. And so, when you had the shifting in the faults which would normally create a trap in the Gulf, we still had sand-to-sand. So we had no trapping, very little, until the reservoirs that we found -- if you could call them reservoirs at all -- were so small that they were described as shows, in a way. We weren't out there as Mobil and other companies were out there, then they found accumulations of gas. We had 32,000 acres -- all of the coast of Nova Scotia for miles covered by our leases.

We went up to talk to the Minister of Mines in Nova Scotia and we had a $50,000 check which we handed to him. That is what it cost us to get the initial hold of some 30,000 acres of offshore lands in Nova Scotia. So it was at that time we say that we had failed. Everybody who was familiar with the seismic information that we had there thought that we really had another Gulf Coast. It was really sad that we didn't, but that is the way it turned out.

So we didn't have any reason to, or any place to apply our underwater capsule and so on. It was at that time that I came back then, in 1971, to Shell Oil. And the exploration people were already out in deeper water. But here again the question was, how were we going to develop what we found if we moved into deeper

water? One of the first meetings that I had with the production people after coming back was in New Orleans. And "Cognac" was . . .

TP: What position did you hold when you came back from Shell Canada?

JR: Executive Vice-President of Exploration and Production. I took Ed Christianson's place. He had resigned. Harry Bridges had come down from Canada. I had worked with him in Canada. He came down to really become Shell's next president. And when Ed Christianson resigned -- and that is another story -- I was getting ready to retire. Harry asked me if I would come and join him in Shell Oil. In other words, he had never worked in the U.S. before.

TP: You and Harry worked closely together in Shell Canada?

JR: Very. So I came down and I worked here in those three years. John Bookout then came and took my place up in Canada. Actually, I worked with John for about a year. That was a part of it, because he was new to Canada and he became Shell Canada's . . . I was in the retirement stage all this time. Then I came back to Shell Oil for these three years. Then John Bookout came back from Shell Canada and took my place. I retired and now we have John Bookout's history, and you know all that.

TP: Yes.

JR: When I came back, the first thing I had to think about was this offshore business of going out into deeper waters. We had a meeting in New Orleans and Sam Paine was at that meeting. He was part of the staff over there. We talked to the design people on the platform. And they weren't ready to say that they could build a platform but they were close to making it work.

TP: In 1,000 foot depths?

JR: Yes, just out beyond 600-700 feet. But "Cognac" was close to 1,000. We had one atmospheric capsule worked down and to a point, and we knew that we could use it. We were really proud of it because it was a difficult procedure and there were certain risks involved with it. But we knew that we could do it, and that is the reason that we put the test unit down in the Gulf coast. We built one and tried it out. But the platform design people were close enough to succeeding in designing a platform for those depths, and we had the attitude that there was no reason why we shouldn't go ahead and do the exploration. So the decision was made to continue on out and to bid on these deep leases. "Cognac" was the first one. We bid $108 million for it. But at the time that we did that bidding, we really didn't know which method we used. If we would have had to make a decision that day -- and I am repeating myself a little bit -- it would have been the

capsule. By the time that the exploration work was done and the drilling evaluation of "Cognac" was accomplished, which took several years, a design group had clearly established a design that could be used in that depth. The problem was how to get it out there. We had no way of floating it out there. So being the clever and good people that they were, they cut it in three and put it down in sections. Then we had a platform. With no struggling, we could have a platform out there that could withstand the conditions over a period of 25 years. But we didn't know how to get it out there.

TP: It was an astounding project.

JR: It was astounding. But with Honeywell and some of the people with the Sonar experts and all, we put the lower section down and directed the midsection getting into the right place. This midsection was submerged 300 feet. To get it down and locked to the bottom, we tacked the bottom one down first because it had to be put down and the piles driven to tack it to the ocean floor. We had it positioned so that you could mate it with a float, then deballast it down and carefully . . .

TP: The precision required was amazing.

JR: To a fraction of an inch! Honeywell worked with us closely on that. One of the people who was involved in that, Mike Bonseigneur. He is now the chairman of

Honeywell. He put the allied signal together. He happens to be one of my closest friends. We did this together. We went to Colorado this summer and we were together in Washington last week. That group of Honeywell and Lockheed people comprised the principals working on this thing as well as the design group. They were successful at putting the "Cognac" platform down. And so now, we were in business.

TP: Did the oil crisis and the price rise give greater emphasis to the platform design as opposed to what you were talking about -- considering subsea and then the capsule option?

JR: In all probability, it did have some influence on our thinking, but remember that the planning, particularly the exploration and design work that was going on, predated the embargo. We were still using the normal anticipation of the trend in oil prices and so on. So some real decisions were made. When it happened and we saw a brighter picture for the price of crude oil -- which, of course, didn't last -- that no doubt made us feel better about what we were doing. But it wasn't in the preliminaries.

TP: I guess the lead time was so long . . .

JR: Exactly.

TP: . . . short-term changes did not really affect your decision-making.

JR: We were putting our minds together on this in the late 1960s and around 1970. I would say that that had really nothing to do with it. When they came to Bullwinkle, we had stepped out further. By this time, not only could they build the platform structurally -- it was a different kind of construction -- but we had something to float it out on, with this big barge that was built in South Korea. So we put it out and positioned it in one piece.

TP: Now going back to the lease on "Cognac", you told me about an anecdote over the phone. I can't seem to remember it. It was something interesting about the process of obtaining the lease on "Cognac". Do you remember the story you told me?

JR: Do you mean on bidding for it?

TP: Yes.

JR: Maybe this is what you are thinking about. We had the lease sale. Of course, we carefully evaluated the prospects that we knew out there. As a matter of fact, the leases that were up for auction had been named by industry not just Shell. We

might have had our favorites out there. But from then on, the security of the evaluation was the highest. . . because big sums of money were being spent for these leases on the front end. In the organization, security was kept as well as possibly could be, and only a few people were really involved, vice-presidents and heads of exploration. It wasn't just the top people, but there was a certain group. It finally came to the point where we asked how much can we afford to pay on it for what we have seen down there? What kind of risk assessment do we have? We did have a way of approaching that. It was going to be in the $100 million range. You talk to your board about that. You don't really mention dollars except in a very broad way. The president would know . . .

End of Side A

Tape #1, Side B

JR: . . . and we would give our board a ballpark figure, a principal. I don't mean to take this lightly. We really told them what we thought was out there and so on. They didn't know the location of it. Code names were used so nobody could take what we said and say it's in this particular lot. We kept that as sanitary as you possibly could. You end up at bid time, and you get a feel of who else is bidding for it, let your previous experience in bidding. But we in Shell -- and I am not saying that we are altogether different than other companies -- were probably as strict down this line. We bid what that lease meant to Shell. We didn't bid

according to what we knew Exxon, for example, was bidding and what their bidding history was. We didn't try to outbid anybody. We only bid on what we, in our experience of risk evaluation, thought it was worth to us. If we left money on the table, too bad, but well and good. That wasn't going to be a deterrent at all, even though we might say they always didn't have much of an interest in it. We didn't take a chance. We bid what we thought it was worth to Shell. It got to setting the exact amount. Finally, it got up the pyramid to the executive vice-president of E&P, and the others backed away. I am not even sure I told Harry Bridges what the final number was. We were close but the final sense set that way. You conveyed that to your land person who was going to be representing you at the lease. Is that what you were talking about?

TP: Yes.

JR: That is the way it was done.

TP: It is such an interesting process running the risk analyses, and deciding what you are going to bid. Who knows how much it is going to be? It is almost a mysterious process.

JR: It is, but everybody supported it. It was fun. It was an exciting thing that we were doing. I think there was complete respect within the organization among all

of us. We were doing our job, and it didn't bother anybody. We had partners in some of these bids. There was quite a bit of pressure put on us at the time by the federal government because the smaller operators were being left out of the offshore because of the money involved. Florida Power and Light was one of the . . . we had several that went in with us. They had no idea as to where the lease was or how much money we were going to bid. They were never brought into the picture at all. They had to have in their offices confidence that Shell was a good operator and that they felt comfortable in remaining with us. They were a small percentage but at least they were represented. And we treated them with respect. We just explained to them that there was no way that we could involve them in this. They didn't participate at all in the technical work that was being done at all. They were just outside people, small operators.

TP: And they didn't know the ballpark dollar figures?

JR: No, no way! Or where it was.

TP: You said the government was putting pressure on you. Were they threatening antitrust if you didn't bring any people in . . .

JR: No, I don't think there was anything like that. It wasn't just our government; it was the smaller operators, too, and an industry problem. I certainly wouldn't say

that there were any coercive type things. The pressure was there. It made sense to us to acknowledge it, and that is the way we handled it.

TP: Partnering in the offshore wasn't unprecedented, but Shell hadn't done much of it, right?

JR: That is right.

TP: Until this point.

JR: Yes. There were a number of joint bids out there, too, but mostly the ones that I was involved in, we were pretty much on our own. But even with some of the major operators, we wouldn't share information. There was too much involved because there was offsetting acreage that might not be up for sale. We weren't about to divulge any of our particular knowledge about that, so we handled it as close to our chest as was possible to still get the job done.

TP: And the other majors probably did the same, right?

JR: Yes.

TP: "Cognac" was the last major project that you were involved in before you retired?

JR: In the offshore. Yes.

TP: When you came back to be Executive Vice-President, what other major things were you involved in?

JR: We were getting into Michigan in a big way. We had some thoughts about California with the heavy oil out there. That happened after John Bookout came back. They were producing 275,000 barrels a day out there. They told me last night that they are Mobil and Shell properties now, the two together, but a really remarkable thing, that is going well, apparently.

TP: I think everyone had said after the price collapse in the mid-1980s in hindsight, it was a really bad idea. But they are still producing it.

JR: But as mentioned right there, it is technology. It is this horizontal drilling. They know that they will create a greater exposure to the wall of the sand face through horizontal drilling. It makes a tremendous difference, and you can control patterns better that way. So it is the technology that has really batted along, and the technology really wasn't in existence at the time they brought it. If the technology hadn't changed, I doubt if it would be the same . . .

TP: Shell has always had faith in the promise of technology to make anything viable.

JR: That's right. It all stemmed from that early creation of the applied research group and the cooperative effort. I've commented that people always felt that they were working together, and we had respect for one another.

TP: In talking to people, the subject always comes up about the origins of Shell's faith in technology. Was it something that transferred from the Group? Was it something that Shell Oil developed internally?

JR: It isn't internal. I am not taking the Group out at all because we had a good relationship with the Group. But what we are talking about now is part of Shell Oil Company. It started in that group that I outlined in that letter. We worked together in bringing Bellaire here, and so on.

I want to show you something. You can turn this thing off for a minute.

[PAUSE]

TP: We were talking about many of the things that are going on today dating back to the . . .

JR: Data systems, and the things that I mentioned, all started within this group -- not in the finance group, not anywhere else in the company. If you want to get into the data systems, I can tell you a little bit about that.

TP: Yes, please do.

JR: When I was an exploitation engineer in Midland after I came out of the field, right after the war, Robert Dunham was one of the engineers there. I was Assistant Division Engineer. Computers were just starting to be used, punch cards, that sort of thing. The exploitation engineers had within their responsibility to determine the reserves and create the reserve report for the company. This always had to be tied in to other records in the company including financial records and evaluation reserves. The treasury was using, for purely accounting purposes, the punch cards. Robert really started this and we all got involved in it. I certainly supported it. We started putting our reserve reports on the computer. And the idea then grew out of that.

TP: This was during the World War II period you are talking about?

JR: It was just a little after. Now, we are talking about 1945-1946. The idea arose as to how can we create the barrel and the dollar sign together and use computers to do it? In other words, how to create a data system that will not only work for us

out there in Midland but company-wide, because every division had the same job to do. It was all done by slide rule and pencil. Other than plain calculators, we didn't really have any system as a database that we put on computers. So that was the start of it. We started working within the production department putting our records on the computer and creating a data system. Immediately when you do that, you involve other departments. You cross departmental lines, and immediately a barrier goes up. They don't want to change their bookkeeping records. They don't want to change their way of doing business because it was all really pretty well designed to the location. You were working in Texas under Texas regulations and laws. In California, it is another ballgame, and so on and so forth. So to create a common database is really difficult. You have to involve the whole company or otherwise, you just cannot do it. We started to try to do that and immediately had to shut down, too, because we couldn't get across into the Land Department and so on. No criticism there. I mean, it was just the problem of the day. So it sort of died away. We in production started doing it in other locations but just to get our records on the computer. In the meantime, I went back to Houston as Chief Engineer. Then after that, I went to Tulsa as Chief Engineer. After that, I went to Oklahoma City as Division Production Manager. The Houston office corporate rise E&P had moved to New York and Alan Gallaway was the Executive Vice-President. They voted to create a staff up there and expand that to include mechanical engineer. We were always mechanical and exploitation engineering. Those were the two engineering entities. By the way,

to be perfectly clear this design group for the platforms and all was on the mechanical engineer side, not the exploitation side. Exploitation was reservoir and geological engineers.

One night, I was going to New York, and then I was to become Chief Exploitation Engineer. They were going to establish a mechanical engineering group and an exploitation engineering group in New York under Galloway. Reed Bond was the production manager. This was a typical organizational set-up. I was involved with all the areas . . .

TP: What year was it when you went back to New York?

JR: 1954, 1955, right in there. So this was the first time that, although I had worked in all these places, it was an office that all the areas now reported to. Gene Bankston, Jimmy Lyon, and so on were with me. It was really a great day because I remember Perry Bristol called me from Tulsa to Oklahoma City and said could I come over and talk to him that afternoon. It was an hour drive. I was going to be transferred. When I was driving down the toll road there to Tulsa, I wondered where I could be going. It could be New York. I almost drove off the road. Nobody particularly wanted to go to New York in those days. Then I thought maybe I could find whoever started this 100% profit and two-year payout criteria. That was the criteria for all practically anything you did; if paid out in

two years and you made 100% undeferred profit, you had to go. That was a difficulty, yes or no. It just wasn't the right answer. The answer was what we used. It was strict and there were very few exceptions to it. And I thought maybe I'll find it at last! Who is it who created this graph for probability criteria. I couldn't find him. I am making sort of fun out of this but it didn't exist. It was something that had been age old in the company.

[INAUDIBLE AT THIS POINT] . . . we were then privately evaluating our sales in New York, and there were some areas that we went over working with. The engineering group was taking our projects and working with present value profit. I'll use that word to describe the whole thing. We for a year or two, created completely separate budgets -- one on one, and one on the other. And when we talked to our management . . . In fact, I hate to say this but Reed Bond and Dick McCurdy said they didn't want to hear the word "present value." They didn't want it used in their offices. It was just friendly conversation and all, but what they were saying is that they didn't want to present the budget. They were thinking of the budget and a new way of looking at it.

I was going with Galloway to the Hague and London with the budget. Galloway saw what we were trying to do and he was quite supportive. But we still, in The Hague, talked about it in terms of 100 percent two-year pay out. But after the second year, Galloway agreed, in the budget discussions over there . . . I could

talk about both . . . Jan Brouwer, who was then the Managing Director in charge of Exploration and Production, was who we were talking to. Well, Jan Brouwer is a Dutch engineer. Do you know the name? I am sure you have heard of Jan Brouwer.

TP: I have come across it.

JR: Well, he was in the Dutch army. Actually, he was in Sumatra when the Japanese came in. He and his wife escaped into the backwoods with the natives and lived with them. They had about a half million dollars of Shell's cash with them. And they actually had notches on their guns for the number of Japanese they killed. There were little groups of natives that went out and harassed the Japanese. Jan and Mia both did that. They both escaped to Australia. And Jan immediately went into the Dutch army and was a Dutch liaison officer with our U.S. Pacific forces. When they were released, of course, most of those people came to the United States because at that time, the office in The Hague was nonexistent. There was no place for them in London and so on. Until they got things back together over there, these men, great guys, all came and worked with us at Shell Oil Company. Galloway was then in Houston, and I was Chief Engineer there. He called me up one day and he said, "We have a Dutch geologist here. Would you have room for him on your group down there?" And I said "For sure." It was Jan Brouwer. So, Jan and I became really fast personal friends over the years. He

died several years ago and I am still in touch regularly with his wife, Mia.

He was the coordinator at the time. So I started talking and presenting these budget items. At that time, we were even discussing in The Hague on per barrel basis, if you can believe it. It was in that detail. If you would take a field in Midale and take a 40-acre tract, or 160, and so many wells allotted to that, it's amazing, really. But that is where we were at the time. And Jan asked me the question, because it was a new thought to all of them, "John, using this criteria, do you do more work to increase your investment, or do you do less?" And I said, "More." And he said, "Go ahead." From that day on, present value profit became part of our budgeting system.

TP: The old 100 percent two-year payout excluded too much, right? It was too rigid?

JR: It just didn't represent the problem; couldn't fit the project. A perfectly viable project will return a good profit to you. It might be paid out in four years or it might be paid out in six months. It was the present value profit. It makes the difference. Admittedly, we were farming out properties on that old basis that we should have never farmed out.

TP: In the mid-1950s, this criteria was accepted?

JR: That's right and we had no help out of the financial people at all. They were purely doing their accounting work. They didn't enter into the investment evaluation of the company at all -- not in the sense that I am talking about, not on projects at all. Maybe one would come to the banking part of it. So we didn't get any help out of them, with due respect to them. They just weren't tuned that way. It wasn't their responsibility at all.

TP: When did you perceive or grab hold of the net present value idea, evaluating profitability was this when you were in London?

JR: No, it was in New York. Jimmy Lyon, was then Reservoir Engineer in Calgary. Calgary was treated then as an area of the United States when we made the budgets, Jimmy Lyon, I had known quite a long time. He was a young reservoir engineer. He was involved, and so was Gene Bankston, certainly. There were others, too. And Art Gurnsey. We worked on the reserve reports and evaluations, evaluating other companies and all. That is where the work was done within this group. When we looked at another oil company, in evaluating reserves, that is where it was done within Shell, in the Reservoir Engineering group with Production. So, the liability was attaching a dollar sign to a barrel of oil, but how do you do that and make sense? We worked purely on discounted dollars.

TP: Was this the predecessor to the E&P economics organization?

JR: It was out of that that they felt the need to have the E&P economics department, and I became its first manager. That is how it started, exactly on a plane between . . . Where were we going?

TP: Bob Ferris gave me a cartoon of the depicting the various heads of E&P economics as football players. Art Gurnsey is the coach. You are on there and so is John Bookout.

JR: Is it that group? Yes, I am familiar with it. That is where the job originated of evaluating not only our own investment opportunities but buying leases from others, small oil company or something. So these people were the only ones in the company whose business it was to attach a dollar sign to a barrel, who knew how to do it and have it make sense in a responsible way. So that is where it started.

Gene Bankston and I . . . Art was in New York at the time and Jimmy Lyon. And we used to argue this back and forth. We finally worked out a system that would be something we could communicate to everybody. But that is how it started.

Dick McCurdy was president of Shell Oil Company at that time. He was another guy that I knew because he started with Gus Archie. He was the West Coast petrophysics engineer, as we called them then. Petrophysicists. And Gus Archie

was from East of the Rockies.

TP: McCurdy wasn't president in the mid-1950s?

JR: No. Burns, then Monty Spaght, and then Dick McCurdy.

TP: Right.

JR: Monty really didn't involve himself much in this sort of thing, but Dick did. You asked me a question to put those guys . . .

TP: You didn't want to hear about it?

JR: This took several years to develop, but Galloway saw it and he is the one that . . . I'll tell you the real breakthrough was when Denis Kemball-Cook came back from Venezuela and became the executive vice-president. He took Galloway's place when he retired as executive E&P vice-president. Then we had a guy that was trained in economics and so on. He saw right away what we were getting at and gave us full support. It was him whom I accompanied on several trips to The Hague. He is the one who said, "Let's try it out." And that is where Jan Brouwer got into it. And now, once Jan said that, the Group was with us. Then it became pervasive and we were able to do it.

TP: Did the group adopt this criteria at this point?

JR: I don't know a whole lot about . . . I can't really answer that question the way it ought to be answered. The answer, I can say, is yes, but to the extent they did it and just how they handled it, I really don't know. It wouldn't be right for me to comment on it.

TP: Looking at the early 1960s, you had started off talking about the evolution of Shell's data systems. Can you talk about how things changed? This was the real breakthrough in computer technology in Shell . . .

JR: And finding the computers to create a data system. I loosely say attaching a dollar sign to a barrel of oil throughout the whole company. That means many things.

When I got to New York, again, I started this. I moved Bull Durham in from west Texas. (Robert Dunham is Bull Durham.) He was a student of Tulsa University and worked in that laboratory there. Because he had had early thoughts of it then, I asked that he be brought into New York on my staff and to start out on this project. And he did. We set him up to start the basics of it. But we really got nowhere. We just simply couldn't break down the financial organization on this

thing. They had never been given the job of helping us on the investment pattern of Shell. That is why it really fell back to those of us in this group because it simply wasn't done anyplace else. And we had no help from here. Shell is a big company. There are many entities involved. The history of West of the Rockies and East of the Rockies makes a difference, because they kept their books different.

TP: Two separate organizations.

JR: You bet! And the departments within those groups varied. It was a formidable job to even start this thing. You learned that it can't be done from the bottom up. It just simply can't be done. You have to, from the top down, say this is what we are going to do with Shell. Then you've got a force that will work. So we started it in New York and Bob got very disenchanted with it, really frustrated. I went to Canada -- a smaller company -- and I was in charge of Exploration and Production now. Although we were operating in different provinces, we didn't have the complexity that Shell Oil did. This is fair to say. These are not critical words. But we had a much simpler opportunity in Canada. Paul Karsky (sp?), who was president, was very much for it. The financial people weren't.

Jimmy Lyon was up there at the time, and we started working on it there in Calgary. We, in E&P, went some of the distance. Still, we had the whole

company - MTM - make it company-wide. Paul and I talked about it at length, as to how to get this thing going. I went to Toronto as Administrative Vice-President and Deputy President and the financial organizations reported to me. That is how we started. The vice president there at that time was so reluctant to get involved. We actually had to find another assignment for him. I became -- if there was ever a duck out of water -- the financial vice-president of Shell Chemical.

TP: And so what kind of reforms did you implement?

JR: I brought together a group of people from all the departments, a data systems group which we gave office space in Toronto. We brought in Bob Norton from Calgary, who had been working on it out there to head up that group in Toronto. We put in financial people. We brought people from all the organizations, financial included . . . Robert Winfield, who became the Finance Vice-President . . . and started to work on it seriously. Now, we had a company effort going from the top down. Shell Oil was still treading water down here. But we really made it work.

TP: What kind of things did this new data systems approach do?

JR: Everything. We really had a common database for the company.

TP: In all sorts of areas?

JR: In all areas. It took several years up there to do that. We had I don't think, 100 people but almost that many. But all the departments were represented and it was a corporate goal to establish the data system at Shell Canada. We got ahead of Shell Oil simply because it was simpler. We were going from the top down.

Shell Oil Company, in the meantime, with Denis Kemball-Cook, who saw what we were doing, created the leverage down here that it took to gradually bring Shell Oil . . .

TP: Would this be the creation of the I&CS?

JR: Yes. I was now back in Shell Oil at this time, and I was still pressing for it. And we brought Bob Norton . . .

TP: Is this when you came back as Executive VP?

JR: Yes. And I brought Bob Norton, who had headed this group in Canada, down to head the group in Shell Oil. He became the first manager . . .

TP: To transfer the Shell Canada experience to Shell Oil in Houston?

JR: That's right.

TP: That was a major undertaking.

JR: Oh, yes. It probably is still going on. I am sure it is not still going on, but a lot of things were happening. Computers were being upgraded and people were learning to be programmers. Up until this time, IBM was doing all the programming. They would give you a system and you applied it. There was virtually no systems worker programming work done by the users. Not just by us, but that was just the way . . .

TP: You could rent time on a mainframe or something.

JR: Yes. That is just the way they were. We bought the IBM 360, the next upgrade, but we had to put it to work more or less to their program; whereas, the companies had to write the systems for it. So that is what this group was doing and they were all learning because this was something that nobody had had any experience in. That is even a greater reason for it to be supported fully by your top management, because it really did create a different way of keeping records and doing the business in some of the departments. They would stretch them

right into Legal or Land, etc. To bring all this together in Shell Oil was a formidable job. As I say, I wouldn't be surprised if they are still working on it.

We did then create an information systems group down here. It was started in the way I described it. We brought Bob Norton down here to head it up. When we built the building down here, he was its first manager. But it started out in Calgary.

TP: I talked a little bit with Don Russell about this . . .

JR: Yes, he got involved with it later on. But he was a member of this same group. Don Russell came from the same group as all these other fellows. I think Don was a reservoir engineer.

TP: Yes.

JR: I think these are important things that really changed the company. Not to know how it got started and what it took to get it started . . . as I tried to put in here, it all emanated from this group -- Toni van Everdingen and all. I thought it was worthwhile and that is why I wrote it. When I read the history . . .

TP: Would you like to talk a little more about van Everdingen?

JR: In the late 1920s and early 1930s, the group was becoming a big oil company, and the oil industry was becoming a big industry: handling reservoirs, producing reservoirs, understanding their contents and how to produce them at an optimum rate, let alone the drilling. But the evaluation of the production was increasing. We had to know more about the characteristics of our reservoir rocks not only how much they contained but how they would conduct fluids through them. We found out there was a lot of water and oil. And how do these two fluids in their liquid and gas phases fill through the rocks? And how do they get to the well? What goes on? What is this physical system that goes on? And it is more than just physical; it is a chemical system, too. And how to produce these wells and how to drill them. How many wells do you need for drainage and all this were beginning to be the questions of the day. There was just simply no experience, no answers. So the Group developed the first reservoir engineering group for . . .

TP: The first real scientific approach . . .

JR: Absolutely.

TP: In the petroleum industry.

JR: Just as simple as that, the first scientific group. Van Everdingen was, I think, the

one that first, and he was the one that really saw the need for it.

TP: What was his background?

JR: He was a mining engineer. Most of them were mining engineers, the early petroleum people, people who dealt with geology underground. But mining engineers are hard rock geologists, and petroleum engineers are sedimentary rock. It is a different background.

TP: And Gus Archie joined the group in the late 1930s, or did he come a little bit later?

JR: Later. And petrophysics is really just another tool for evaluating reservoir rock.

TP: Was there a name for this group under van Everdingen?

JR: Not that I know of.

TP: It was an informal organization.

JR: We were all reservoir engineers. But the idea was that in this new company over in the United States, and West of the Rockies was different than East of the

Rockies at that time. Toni van Everdingen was sent to Tulsa. The reason he was sent to Tulsa was because of the secondary recovery thing. That is where it started. He was sent to Tulsa, or Houston. Tulsa was under Houston at the time, or St. Louis, a little earlier. But it started in Tulsa because of the water flooding. So Toni was one of the first reservoir engineers who you could formally say was a reservoir engineer.

I worked for my father in a natural gas company in Kansas. That is another story. I thought I was going to work with refining. That is really what I wanted to do. I was a chemical engineer. My father knew some of the Shell people because Dad was running the gas company in Kansas, one of the earliest ones. There was a contract between the two companies: if it was oil, Shell would take it; if it was gas, the gas company would take it. Every lease or well there was a contract between Shell and the old Kansas Pipeline Gas Company, and my Dad was an officer of the gas company. I worked for him through high school in the gasoline plants and so on, but I wanted to get into refining. Dad knew Doc Baker, who was with the Gasoline Department in Tulsa, and he introduced me to him. I got a job as a chemist, with the idea of going eventually into refining in Shell. Doc Baker met me at the door. I moved my little family to Tulsa. I was sent to establish the porosity laboratory. I had never used the word porosity or permeability in terms of the oil industry; they were completely foreign terms to me. But I had built this laboratory up in the gas company and so on. So I went

out there and built that lab. It probably was the luckiest thing that ever happened to me because it took me almost the rest of my career to have refining under my belt. Do you understand what I am saying?

TP: Yes.

JR: So I started in that laboratory and, at that time, chemical engineers were ineligible. They weren't thought of being a part of it. I had to work five years in that laboratory until I finally worked my way out of it, and I had to go back to school and got my master's degree at Tulsa University in petroleum and geological engineering to qualify to be an engineer in Shell. And that is where I started as an exploitation engineer. This is personal stuff. This is so interesting just how young I was. We were just doing things for the first time. But Shell always tried to keep sort of a niche. And with the wonderful support of the Dutch, that is where we had a leg up. If you want to say how the group ever impacted Shell Oil Company, then this narrative compared to the rest of the companies, you can put it to that.

End of Tape #1, Side B

Tape #2, Side A

TP: So, you were going to mention a little bit about Gus Archie.

JR: Yes. The Tulsa reservoir thing was the nerve in the engine and matured a bit -- the secondary recovery and, of course, there were a lot of other things. We had activity on the Gulf Coast and in Louisiana. So they decided to move Toni to Houston. Houston was the corporate office, let us say, of East of the Rockies, but this reservoir engineering started really in Tulsa. It got a little further along here, and the importance of this thing was recognized. And he was brought to Houston to set up what they called the Applied Research Group. He brought in several people, I was one of them, to do the reservoir engineering. Doc Wilhelm was the geologist. Have you ever run into him?

TP: It doesn't ring a bell.

JR: He has been dead for some years. He was the geological side of that group. They brought in Gus Archie to be the logging side. So there were the three of us under 20 who comprised the first reservoir engineering group for the Houston area. Gus was a field engineer in Kansas, because Kansas was under the Tulsa area. So we came here together in 1941. Gus was already here, I guess, but it was a matter of months. It was just within the year or so that the put this together. Galloway, who was the head, was the senior person involved East of the Rockies on E&P. Reed Bond was the production manager. And there was another individual, too,

that is long gone. He's just passsed in the last year or two. And then Alexander became the vice-president there. With all that started, we all realized the importance of developing our technology and these things. This had to be done. And it was during these years that the E&P sectioned out in Emeryville. It was recognized that we had to get involved in E&P research in a focused way, so we built the Bellaire laboratory. The fellow that I am staying with today was the architect that designed that building. I roomed with him in college.

TP: What is his name?

JR: Herbert Powell. He designed the Hyatt down here and so on and many of the principal buildings here in town. We see him every year here or there. That is when we all worked together.

And then, there was a fifth guy, William Hurst, who was the mathematician. Toni was a good mathematician, but he felt the need for more help. So he brought in Bill Hurst, who had worked for Exxon, the old Humble Company actually, in research. But he was strictly a mathematician. We were getting into doing scale models of reservoirs, and we didn't have computers yet. We used to have calculators and we had a room full of mostly women punching these calculators to do these endless calculations that could be done in an instant today for reservoir studies at Bellaire geophysical at the time only. I told you I designed this PVT

equipment, the pressure volume temperature. They were building it. He had a great machine shop out there. He was a very fine machinist and did wonderful work. This was all high pressure equipment and hadn't been done before, so it never evolved. There was a mathematician out there by the name of Henry Rainbow. They were all interested in what we were doing in The Hague, but they were working geophysics. I was describing one day what some of our problems were in mathematics. He was interested and said, "Have you ever looked at Heavyside's calculus?" I had never heard of Heavyside's calculus, but I told Toni about it. So he got Henry out there. Henry was a mathematician. This doesn't need to go on the tape, but his wife brought him to work and picked him up. He was a wonderful man, really fast. If you get a good mathematician that can take your physical thoughts and actually put it into mathematical language, then you really take a step forward. Henry and Bill Hurst had started to do that and Toni, up to a point. And I was behind them. Henry suggested Heavyside's calculus, and that was the first effort . . .

TP: Heavyside?

JR: He is a well-known English physicist. He has developed what is known as Heavyside's calculus, which is solving problems where you were dealing with approximations. In the first piece of the Heavyside calculus textbook, it says, "Shall I refuse my dinner because I do not understand the process of digestion?" I

am paraphrasing here a bit, but that is what it said. And that is what Toni put in our first report on reservoir behavior, which quoted Heavyside. The idea was, do I turn away an answer because I don't understand all the underlying principles involved?

We felt the need of this sort of thing in our research here. So, they got Dr. Harry Gershinowitz to put together the Bellaire laboratory. So E&P then had its laboratory, and we were all part of that. About Shell Development: I don't know whether it has come out in your discussions or how it is now. But in E&P program out there at that lab, while we had a president of Shell Development, the E&P program really had to be approved by operating. In other words, I as executive vice-president, had to direct input and approval of whatever they did exploration and production-wise. Of course, the president of Shell Development always had a big part of it, but his part was getting the job done. Yet the work, the programs, what they were working on were really set by the operating divisions. So that was another part of this business of . . .

TP: Form really came like in the late 1950s, right?

JR: Whenever Shell Development in Bellaire started. But that was a part trying to absolutely eliminate any barrier between operating and research. Anybody that became a senior operations manager had to have E&P experience and vice-versa,

if you could possibly do it. But that is the way we tried to keep that.

This was something that Gus and I had worked on from the beginning. They were starting it out there. Ted Nelson, who had my job at Mobil, said, "We always wondered why, at Shell, you didn't have the same problems as we had between research and operations," because the research kept them at arm's length. They had all kinds of trouble finding the technology that they really needed, which was never a problem with us. That is enough said!

TP: Is there anything else? Can you speak to the introduction of petrophysics and Gus Archie or is that not your area?

JR: I can tell you a little. Gus Archie and I shared an office for years together. We worked on the same problems, in a way, but we all had our responsibilities. Schlumberger, a French company, first came to the waterflooding area of eastern Kansas, and that is where Schlumberger started in the United States. Their engineer was Bill Cunningham, who became chairman of Schlumberger and was chairman when I retired from Shell. They were very secretive about anyone trying to find out what they were doing. They would give you an interpretation, but they wouldn't tell you how they interpreted it. For Shell, that was never good enough. We had to understand. Gus was one of the first one. We were all involved early on. We would measure the distance between those electrodes, to

try to figure out what they were doing and why they were giving us the answers that they did, because they wouldn't tell us. Gus really became the leader of that. That started in the Tulsa area, because that is where the first Schlumberger was. But to form this group of petrophysicists, logging engineers as we called them, was an obvious part of the total production, E&P effort, and research effort. So Gus was simply brought down to head up the group East of the Rockies. Dick McCurdy was given the same job in the West. And that measuring of what they were doing went on for quite a while.

TP: To try to figure out what Schlumberger was doing?

JR: Yes, exactly. And we didn't figure it out! We started presenting them with better arrangements for logging for this because they didn't really know anything about the geology and so on.

TP: And this was the Archie equation?

JR: No, that was correlating . . . Gus Archie could draw a straight line through the greatest mass of dots on the page that you ever saw, and have it turn out right! The boys used to kid him on that. He could have a splatter all over the records and draw a line through it, long before we were able to weigh them in any systematic way. He truly was a good man. But he was a practical man. He

probably helped Schlumberger as much as anybody ever did outside their company. He was very respected, as you know.

TP: Yes. Are there other personalities? You've mentioned quite a few. I don't want to keep you too long.

JR: I'm free. I don't want to take you too long. I will take you to lunch if you are interested. But, I don't know what time it is.

TP: I think we have covered a lot of ground, so I might just turn the tape off now.

THE END