SHELL OIL COMPANY

ORAL HISTORY PROJECT

Interviewee: GENE BANKSTON

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Bio

Gene Bankston grew up in Southeast Oklahoma. After serving in World War II as a B-17 gunner he studied petroleum engineering at the University of Oklahoma, earning his degree in 1949. He began working for Shell that same year and quickly rose through Shell's ranks. He worked towards economically evaluating prospects in order to maximize profits for the company. After working in New York in 1957, Shell assigned him to the Hague to better understand the "big picture" at the company. Between 1957 and 1963 he worked in various capacities for the company including division exploitation engineer for Southwest Louisiana and assistant to Shell's manager of exploitation in New York. He was the key liaison between development and E&P for Shell. He became manager of the economics department in 1964, and in 1965 he worked in London as head of the North American Division for Shell International Petroleum. In 1966 he became the Vice President of E&P for the Houston area. He served in that capacity for 6 years until becoming Production VP. He worked in that position until his retirement in 1980.

Summary

This interview covers the impact of the economics department on exploration. He has interesting information on Production Economics. This included a thorough discussion of the impact various economic models had on offshore lease analysis. Some discussion of technology including bright spots and recovery methods. Short discussion on reorganization and research.

Side A

TP: This is an interview with Gene Bankston. The date is December 3, 1999. We are at his home in Houston. The interviewer is Tyler Priest. Like I just mentioned, Gene, why don't we start off and have you talk a little bit about your background and how you came to Shell?

GB: All right. Well, after World War II, I got a degree in petroleum engineering from the University of Oklahoma, and right out of school, I went to work for Shell in January of 1949. Actually, it was February of 1949 that I went to work for Shell. And after a training program, where I went to various places in Texas, Oklahoma, Colorado, Illinois, I was assigned to Elk City, Oklahoma, which was a major field for Shell.

After being there for two years, I was moved into the division office in Oklahoma City, which had responsibility at that time for the state of Oklahoma. Later, it was expanded to include Kansas as well. That is where I worked most closely with John Redmond. I was a reservoir engineer first, then later, I was the division reservoir engineer, responsible for all reservoir engineering. And I got acquainted with John, and he was later sent to New York, and he requested that I come up there to help out with the formation of an economics department. A.J. Galloway - and you had an interview with A.J. Galloway I understand - he had a concern about the ability to allocate resources, both manpower and capital, between the

various areas, one region versus another, and I will get into that a little bit more later. But he had brought Toni van Everdingen, who was our head reservoir engineer in Houston, up to New York to form the nucleus of that effort.

TP: Did this happen officially before the E&P economics department, because I know that van Everdingen was part of a production economics . . .

GB: That is right. Production economics. Before the E&P economics department was handled. And I will get back to that later, but from there, I went to The Hague for a six-month assignment, and then came back to Louisiana as a division exploitation engineer for a year and a half. Or, division petroleum engineers, they now call it. And then, back to New York again. I was personnel coordinator for all petroleum engineers and liaison between the research organization here at Shell Development and Bellaire, and the operating areas and divisions. And from that, I went to . . .

TP: Now, what year would that have been?

GB: That would have been in 1962. I was first there in 1957. That is before I went to The Hague, 1957 to 1960. The Hague for six months. Lafayette, Louisiana for a year and a half. Back to New York, and I worked about one year in that job of Personnel Coordinator, and they put me back in the Economics Department as

manager of Production Economics. And then subsequently, I was made manager of Economics Department.

TP: And you followed John Redmond in that?

GB: No, Jack Doyle was the manager at that time. I replaced him. And then, I moved from there to London as head of the North American division for one year. And back from there to Houston as a regional vice president in charge of what was then called the Houston Area, which later expanded to be the western region. I don't know if there is still a western region, because I haven't kept up with the organization lately, but that was the job. Then, after head office had been moved here, I moved in to head office as production vice president. Six years as regional vice president, eight years head office production vice president. And that pretty well covers the . . .

TP: And you retired when?

GB: In 1980, but to get back to the Economics Department, Galloway wanted a more logical and semi-quantitative method of allocating resources. We had to get better ways of measuring the economic value of any particular opportunity, and that is why he brought Toni in there. When he formed the Economics Department at Galloway's request, Redmond provided the practical side of the problem, Toni

provided the mathematical side, and they needed someone that could communicate the two sides. And that was my job, though later, I did a lot of the work myself.

TP: Now, was Kemball-Cook involved in setting this up?

GB: Well, he was, because Galloway retired and Kemball-Cook replaced him. So, Kemball-Cook was very . . . I would say, if anything, Kemball-Cook was more inclined to the economic analysis than Galloway. But I think Galloway was the one, I know Galloway was the one, that originated the idea. We had also to forecast . . . previous to that time, when we did analysis of profitability of a field development program or exploration program, we assumed that prices and costs were held constant forever, at whatever level they were at the time the analysis was made. No attempt was made to forecast them separately. So, they felt the need . . . we all felt the need to do something about that, and we did.

I don't really know how to explain this. I have here a report that I got out that I wrote based on my time in New York. Almost everything in there was presented in various meetings and stuff in New York. Mathematics is a language. That is one of the reasons there was a problem communicating between Toni and the management, because he spoke in mathematical terms and no one, including John Redmond, want to spend the time required to do it.

TP: I remember John Redmond said that he often heard from people that they just didn't want to hear the words "net present value."

GB: Yes. Right. And while the present value concept was there, the idea of internal rate of return or true rate of return or whatever it may be called, had not really been used in exploration and production, and in Shell Oil, for that matter.

TP: Was Shell an early convert to this type of analysis in the oil industry or, I guess, even in other industries? How would you compare Shell, the timing of what Shell was doing . . .

GB: I would say we were about average, I would guess. We didn't communicate much on this. We didn't publish, and others didn't either for a long time. So, when they actually did it, we don't know. But my impression is, and then based on things like the Harvard Business Review and other business publications, I would say that we weren't the leader necessarily, but we certainly weren't far behind, and no where when I wrote this book, this paper, had anyone done this sort of thing, published it. I don't know what they may have done unpublished.

But I did want to mention in here . . . I did want to show you in here a list of references because this gives you some ideas of the people that did some work

that was worthwhile. You may not have ever heard of Henry Lefkowitz, but they did some early pioneering work. They are both Ph.D. mathematicians, Lefkowitz and Kanner, and it was really heavy going for most people. So, it really needed some interpretation to get by it. But John Redmond and I wrote the second one together. We spent night and day together for about two weeks writing this in preparation for a trip to The Hague where the explanation was going to be made about what our investment philosophy was. John wrote an additional paper, one that I didn't help him with. It is largely taken out of this, but it had some extra stuff. And then, van Everdingen's study of what the profitability of exploration and production had been in terms of true rate of return, which was measured, was deemed to be better than other measures. I think it still is. Our present value, I mean, they are not the same thing, but they are very closely related.

Jeff Croes, a Dutchmen in The Hague worked on this, and it was a very good effort. And I didn't agree with everything in it, and I said so in here. But it was a very fine piece of work. Jimmy Lyons, who worked for John in Canada, he was a very great contributor also. And then, of course, one public source, Joel Dean, from the Harvard Business Review. So, at that time, and I would say that is better than my memory.

TP: This was published in December of 1961. It is called “Selection of Investments for Optimum Economic Performance.”

GB: And I am sure it is in the archives somewhere. I have a copy of it that I was allowed to take when I left Shell.

It went on from this to, we did the first quantitative type analysis of what to bid for leases offshore, from this effort. When we talk about a lease, let's say, there are 2,500 and some acres in a typical block offered, and we would have some part of a potential oil field would underlie that, and we would have to look at the probability of certain amounts of oil or gas or both. And then, we had to provide a development scheme that showed how they would be developed and produced and calculated, a value we could afford to bid, with the proper discounting for risk. Many people worked on that later . . .

TP: Was this done in time for the big 1962 federal lease sales? This was published in December of 1961, so did you have time to get it approved and this methodology in place?

GB: That methodology isn't all in here, but we did, we put Don Russell . . . in fact, I picked Don myself, to work over there with Billy Flowers and . . .

TP: I know Don talked about it in terms of the 1967 lease sale.

GB: No, but he did . . .

TP: He was also in 1962 . . .

GB: Yes. That is when we really started to . . . well, I'll take it back. My memory may not be perfect on that. Let me think. The one that we did first was for a 1960 sale, that's before I ever even went to The Hague, and it was off Texas. And so, I am not clear on the 1962 sale. I don't remember what was done, but we worked gradually into it in a way that we used various methods, starting with just simple recovery per acre, assuming geologic closure for certain types of structure, and that can vary all over the map, to finally when we were using bright spots, we could almost be certain of whether it was oil or gas or neither. And we could do a much more quantitative interpretation than we were able to do when I first started working on it.

TP: Now, was one of the objectives in setting up the economics department to look at the offshore? I mean, was it set up with an eye to trying to evaluating the offshore, or was it put in place to look at all . . .

GB: It was put in place to actually look at it all. It was a natural progression offshore because the offshore became profitable only when all of the similar prospects had been used up onshore. Offshore was more costly to develop. So, a field that

might be economic to develop onshore, may not be offshore. But the progression was started already by that time. But we were still doing a lot of exploration work on shore. So, it wasn't specifically for offshore, but it was an opportune time to use it there.

TP: Because, then, people were thinking about offshore . . .

GB: Sure. They were. They were already doing geophysical work. Seismic surveys offshore and some development in shallow water. So, it was very useful for that purpose, but it was not the only reason. We needed it anyway, whether we had offshore or not.

TP: How did it help, say, in evaluating things on the production side? There was a study in the early to mid-1960s, once the *Blue Water 1* was tested, where you had to begin to evaluate how you were going to produce oil from these really deep water leases, whether it was going to be subsea or platform, those kinds of issues.

GB: I was very heavily involved in that. In fact, I did a study, or participated in a study . . . I shouldn't say I did it, when I was in New York, in 1960 or before, because I left there in the early 1960, in which we looked at the cost of developing with platforms and out to a certain depth. And we concluded that if we got into more than 150 feet of water, the cost would favor underwater

completions. As the study was redone by others, the crossover point kept moving out. It got more practical and less costly to build the platforms, because we had designs that allowed us to do it cheaper, and the cost of underwater, it wasn't as simple as we thought, and so, you raise that cost and together, you move the intersecting point out. At the time we developed Cognac, if my memory serves me right, we thought the crossover point was near 1,000 feet. We thought that was very close to . . . well, obviously, they have gone farther than that. But now we are out where it is impractical to build a bottom-supported structure.

TP: What were some of the things that made, well, you said, better designs in terms of platforms, but what were some of the problems with subsea or underwater completions that Shell was having at the time?

GB: I think you had better talk to guys like Ron Geer. Ron, particularly. And Bruce Collip, rather than me on that subject. It just was more complicated than we thought it was. I'll tell you, with any new engineering development, the first look always leaves out some things. It is not because you intentionally do it, you just don't think of everything. So, as a consequence, your cost estimate, even with some contingency allowance, is always too low. I think that is the nature of it. We always knew it was complicated, but there were complications that we didn't foresee. And each one of those added to the cost. But I really think someone else can . . .

TP: Yes, we have talked to people about . . . I am trying to get different perspectives. So, you stayed in E&P Economics until what year again?

GB: It was 1965. Then I went to London.

TP: Now, 1965 seems to me a kind of pivotal point in terms of the offshore, in that some people say, well, you know, you got the big leases in 1962 and you were moving on to deeper water. And in 1965, you begin to really start to emphasize the seismic technology, and maybe even begin to think about real profound depths. What was going on in this period, in thinking about all this?

GB: That is when the bright spots technology came around. Mike Forrest was the first one I ever knew that recognized bright spots. It wasn't that he necessarily knew exactly why, but he observed it first, and others contributed to understanding it, but Mike was very important in the early stages. The first I remember hearing about bright spots was after I came to Houston in 1966, in September of 1966, and Mike was working in one of the divisions in the Houston area at that time. If you had a geological structure, and the bright spots tended to indicate hydrocarbons that coincided with the structures, so the limits could be at the water level, he believed he could believe in them. And you could. Not always it turned out, but you could. But later, they got very quantitative in evaluating these things,

and that played a tremendous role in our ability to acquire leases and know what to bid on because, at first, when we were talking about bidding on leases, you had two problems: 1) what is the maximum you would give for it if you just had to lay out a price? And that meant estimating some potential, discounting for risk, and establishing an amount you could pay as a bonus; 2) Evaluating what would be required to get it. That meant, try to evaluate what other people would bid for it. At first, it used to be the bid was a lot less than what you thought the maximum was worth. So, the big problem was how much it would take to get it. You wanted to pay as little as possible and still get it. But as time went on, it got to the point where you almost had to bid the maximum to be successful. You didn't always, and particularly, we didn't if we thought we had some information that others didn't. But it was very important in Shell's . . . well, the analysis had to be done some way. And I don't know what Shell does now. I have no idea. I have been gone so long that they may have abandoned everything that I am familiar with, but I doubt it. I'll bet the basic principles are still the same.

TP: Can you talk a little bit about how, as I mentioned before, the merging between geophysical research, and geological research, and Shell's exploration strategies, in developing their exploration capabilities? Can you talk about when and how this emerged? Was it primarily seismic technology, or were other things pushing this?

GB: Well, geology and geophysics always went hand-in-hand, but I would say there was a great effort in that time frame, in the 1960s, to cause them to work more closely together. But I don't see a great turning point. I think it just gradually became practical to do that. I am sure that some leaders in the exploration pushed that. I mean, McAdams, for example. Did you interview McAdams?

TP: We never got a chance to. A lot of people have talked about him.

GB: I am sure! McAdams, this is kind of an aside, but when we were working back around 1960, when we were working on that sale there, and I had said that there was a possibility we would have to inject water to recover the oil; you know, that there were some places where the fault pattern was such that I didn't feel the water drive could reach the oil. McAdams jumped up and he said, "Damn it, Gene, you can be sure of three things: death, taxes, and water drive in the Gulf Coast." He wouldn't hear of any need for artificial water drive. And when I went back to New York . . . I left New York shortly after that, and when I went back to New York in 1962, I met McAdams by the elevator, and he greeted me, and he was glad I was coming back, and he said, "You know, I was down in Louisiana, New Orleans the other day, and talked to John Pittman," who was production manager, and he said, "They are getting ready to put some water drive injection facilities into" . . . I've forgotten which fields it was. And he said, "You just can be sure about death and taxes." So, he remembered very well the exchange. Mac was a

very volatile and difficult guy in many ways, but he and I always got along pretty well together. I mean, as anyone does, you have your differences, but it is really too bad you couldn't interview him.

TP: Yes, well . . . Mike Forrest talked about him. Just missed . . . he was going to meet with McAdams at some point, it was just before he died. It is unfortunate that there are some people who we will never get a chance to talk to.

GB: I am glad you got a chance to interview Redmond because someone of his age and with his mind still as clear as it is, it is very opportune to interview him now.

TP: Yes. So, you came back to the Houston area, which became Southwestern Region . . .

GB: And then Western.

TP: . . . in the mid to late 1960s, and then Western Region. Can you talk about what kinds of things you were involved in during that period?

GB: Well, one of the things we were involved in were studies that led to the reorganizations.

TP: That happened in 1968, is that right?

GB: Well, that is the first one. That is the when we took the Southwest . . .

TP: Yes, but if you could talk about the reorganization and what were the motives behind it, and how you worked it out.

GB: Well, it was not a reorganization that I instituted alone. It was an E&P-wide reorganization. But the idea was that we felt that, up to then, the division exploration manager reported to the area exploration manager, the production manager reported to the production manager, the land manager reported to the land manager. And we felt that they needed to work together more closely. Just like geologists and geophysicists need to work on the technical level, these managers needed someone to resolve the differences closer to where they were. We would have disputes between the exploration and production, or land and exploration, or land and production, and it would come up to the area office and they would get together and bump heads and go back down, still never getting solved. So finally, of course, it would come to the vice-president, and it would get solved. But they were reluctant to bring it there and admit they couldn't work it out. So, the theory was, and I believe it was correct, that by putting division general managers in, at that particular point in time, it enabled them to resolve their differences quicker and more closely. You still had regional managers to be

worried about what was going on here but we had the day-to-day operating decisions made at a lower level.

TP: That was kind of a decentralization. It was maybe just pushing further down for certain kinds of decisions.

GB: It was. We did push more authority down to the division level because we had a general manager there to do it. And it was a decentralization in a way, but it mainly just was a more efficient method. And, you know, you can argue centralization, decentralization. I mean, there are times when you need centralization, and Shell has gone back and forth, and so have all other companies. I really think that there are just conditions and times and situations when one extreme or the other is needed. Right now may be a good time when Shell needs to centralize, and I suspect that is what they are doing.

We were still doing a lot of important exploration work then -- offshore California, Alaska. We were still looking in Alaska. We had the northern Michigan Pinnacle Reef play, which was very important. And there was a case where the geophysics were very important in delineating the pinnacle reefs and cutting down the dry . . . it is not that we couldn't have found them, some of them by just drilling, it would not have been an economic enterprise if we hadn't had the geophysical delineation.

TP: Yes, there were very few dry holes . . .

GB: Yes, considering the size of the individual pinnacles, it is really a miracle almost. I can't think of anything . . . we had the Uinta field in Utah which was a fairly big discovery. That was made . . . the discovery was drilled just prior to the formation of the Western Region, while it was still part of the Pacific Coast area.

Gerry Burton, did you interview Gerry?

TP: I haven't. No.

GB: Gerry was a geophysicist. He also was vice-president in charge of . . . he was head geophysicist in New Orleans, and I guess he was exploration manager in New Orleans. After that, he was vice-president in California. And then after we merged the Pacific Coast area into the Western Region, Gerry was in charge of International Ventures. But one of the reasons I mentioned Gerry is that during my term in the Economics Department in New York, and I can't remember whether I was manager of the Economics Department or of the Production Economics section, but Gerry worked under my supervision to do a study on Alaska. This was before Prudhoe Bay was discovered. And we had a model in which we, if I remember correctly, we figured if we found as little as a billion

barrels of oil, but it had to be . . . I mean, it could be scattered fields, but the fields had to be at least 100,000,000, and we thought that was possible. We didn't visualize the giant that they found, but he probably can answer better than anyone else our situation with regard to how we missed Prudhoe Bay. I would bet on him. McAdams would have been a good source for that, too.

TP: Yes, I have heard various stories. I mean, no one really knew that the geology was going to be what it was like, where the big field was. I don't know if you agree with this or know anything about it, but someone said Shell had a choice between exploring on the West Coast off places like Oregon and Washington, or really focusing on Alaska, and really chose to use the *Blue Water II* to drill off the Pacific Coast.

GB: Well, I don't know that we made that choice. I don't know that we didn't either, but I feel like we could have done both. We did explore in Alaska, actually. I mean, we even had discoveries, particularly in southern Alaska. And we had acreage up on the North Slope. It wasn't in the right place! But I don't know. You know, you make those decisions. I was not the one that made the decision. I mean, I wasn't at the decision-making point at that point in time.

TP: Did the failure to get in on Prudhoe Bay have an effect on Shell's commitment to the offshore Gulf of Mexico? In other words, "we missed on this one, and we

have had success here. This is where we are going to have to concentrate"?

GB: Well, I don't recall that anyone ever openly said that, but I am confident that it was a factor in our push for offshore.

TP: I guess in 1969 and 1970, there were big lease sales offshore and you began using your bright spots there.

GB: Yes.

TP: So, you came back to head office in 1972, is that right?

GB: Yes, 1972.

TP: Can you talk about that period at all? It was a pretty interesting period for the oil industry,, the whole embargo era. What kind of strategies was Shell forming at that point, from your perspective, in production?

GB: Well, we certainly were putting a lot of emphasis and research on methods of drilling and producing offshore. That was a big part of it. But also, we were putting increased significance on supplemental recovery. Water flooding started back about the time I joined Shell. And it had come a long way by then. But we

hadn't done much in other areas. But we did increase our efforts to discover new methods of recovery, such as chemical flooding, as we called it, and steam drive and thermal recovery.

TP: When did Shell really begin its work on CO2 flooding? The 1960s was mainly the emphasis was on thermal, is that right? In California?

GB: That is right. Well, we were doing research on several other methods, but we had an opportunity to apply thermal in California. CO2 was . . . before I retired, we were already doing some CO2, and we had bought those leases for CO2 production in New Mexico, and I forgot whether there are some in Colorado . . . I believe. Around the late 1970s was the time when it began to move forward. And it became much bigger later. But that was when the groundwork was laid. Steam flooding . . . you probably have heard this or seen the so called "huff-and-puff" method, where you inject steam and produce back the oil in from the same well. I was in The Hague, and I did some technical studies there at their request. They had a steam drive project in South America. This was before we had done anything . . . we had worked in 1989 on the concept, but we had no actual project. They had a steam drive project in which an injection well line broke. And it started flowing back oil immediately. Up to that time, we thought you inject steam in there for a while and if you open it back up, steam would come back, and eventually, oil would come, too. But you would first flow out steam. There

wasn't any delay. The oil came back right away. So, we had to get a new theory, and we did, of course. And they worked out models that explained it. But then, we had plenty of opportunities in California to apply it. We did both inject and produce through the same well, and then also the steam drive were injected in one well and produced from other wells. Unfortunately, we didn't have as much of that heavy oil acreage as others. Mobil, for example, used our knowledge to exploit there, so we didn't have as much production, but we were definitely a leader.

Now, we experimented with surfactants, all kinds of ways of increasing the percent of recovery. But apparently, the carbon dioxide, and thermal, those two are the most important supplemental . . . I don't know of any others that are . . .

TP: You were still doing water flooding . . .

GB: Oh, sure, water flooding, yes. That is correct.

TP: And John Redmond, I think, mentioned that the first experiments, the first work on this was done in Oklahoma. Is that right?

GB: Yes, that is right. I can't think of the name of the field but it was done in Oklahoma. That was before I joined the company. They already had, in Benton,

Illinois . . . I was in the Tulsa area then . . . they had a large water flood project going, and I went there on the training program and spent some time there studying that flood. But that was the first big one for Shell. But we have done many others, and they no doubt will still do them. But the question is could you do better? Like, for example, we water flooded the Wassan field. I am sure you heard of the Wassan field. We water flooded it, and they went in behind it with carbon dioxide flooding. And you've got to wonder . . . if you had a new field like that, would you start with carbon dioxide flood immediately? And I don't know the answer to that, but I suspect you would, if you had the chance to do it.

That brings to mind one other thing. The studies we did, and I am backtracking here a bit, but in the studies we did, we had to analyze the economics of acquisitions, and some of Wasson was acquired after it was a developed field. And we compared the profitability of exploring versus purchasing. Not that we thought we could ever totally substitute purchases for exploration, but we had to have some way of deciding whether to buy if an opportunity was provided to us. We concluded that the profitability, not surprisingly, of our own exploration and development was more profitable than buying some field someone else had found and knew of. Common sense would tell you that. But that wasn't easily accepted by some of the proponents of buying properties. They wanted to say they could do better buying than exploration. Of course, if your exploration is unsuccessful, it is riskier, so it stands to reason that, if successful, it would be more profitable,

and it was. I don't know whether it still is or not. I mean, in the United States, I think it may be approaching, outside of the deepwater stuff, it may be approaching the point which is exploration is getting uneconomic. And I gather from what I see has been going on that that has been concluded by some others.

TP: Well, you see a lot of the larger companies selling off properties to smaller firms. From their perspective, it is certainly not economic to explore.

As your exploration technologies improved, I guess it really started to reduce the risk in certain areas, in the basins that you were familiar with.

GB: Absolutely.

TP: Well, I don't have too many more questions. I guess the only other thing, and we have covered it in various ways, was Shell's beginning to contemplate the real deepwater in the 1960s, but we have talked to some other people about that. I was just curious about how early it was. When did you really start to think, well, 600 feet, we thought about it and figured out going to 600 feet, now let's start looking beyond that horizon?

GB: Well, I can't tell you when exactly it happened. I know it happened before I retired, but I can't tell you exactly when. The Royal Dutch Shell group started

looking at proposing the vault out into the ultra-deepwater earlier than did Shell Oil Company. And I don't know who was responsible for that.

TP: Really? I didn't know that.

GB: Yes. They made a decision to go into ultradeep water and gosh, what's that company, SEDCO, Southeastern Drilling Company, built this big . . . I have forgotten what it was, Number 445 was the number that was on it. But anyway, they built a rig to drill in, I believe, in at least 5,000 feet of water, and Shell signed a contract to use it, so that they didn't take any risk in building it. And Shell Oil Company's engineers went to Holland, worked on it, and it was a worldwide cooperation on that. But nobody here was proposing then, that I knew of. And I believe that I was . . .

TP: So, you are talking about the real deepwater? 5,000 feet . . .

GB: Yes. Well, I mean, even 3,000. Not that no one was thinking about it but the point is, you have so much money and so many people, and if you can find opportunities to do that in . . . to use the water thing in 1,000 feet of water, you don't go out to 4,000 yet. If somebody else is about to go out there, you are not going to be shut out of it but the timing is not right for it. So, I can't say who was thinking of it, but I guarantee you one thing: no one that I ever talked to was

thinking about wells that produce as much as the one . . .

End of Side A

Side B

TP: . . . the rate of production is astounding.

GB: Yes, it is astounding. And I can guarantee you that I did not visualize that.

TP: No one in the industry really had any idea, do you think?

GB: Well, someone must have had an idea that it was possible because we all went out there. But I certainly did not, and I never heard anyone in Shell say that they visualized that situation. But what I did often say to people . . . you talk about size of field. They talk about what is the minimum size of the field we can develop out there, because they figured it had to be bigger. And I said, it depends on how it is packaged. If it is stacked, if you've got layers that are stacked one on top of the other, and you get 50,000,000 barrels in a small little area where you don't have to reach out all over in long lines, long direction of wells and all that, you can have a smaller field than if it is in one layer scattered over a wide area. But I never once thought of productivity being so big that we would need the few wells that we do to get the amount of production we get. It is astounding, and,

you know, I don't know what you have heard from others -- success has a thousand fathers, they always say, but I never heard anybody say that.

TP: What is it about the geology that makes it so productive?

GB: Well, they found thicker sands, oil saturated sands. That is the main thing, because the sands, generally on the Gulf Coast, the sands are very permeable, so they will produce a high rate of production. But if you have 10 feet of sand and you get 100 barrels in an hour, if you have 20 feet, you can get 200 barrels an hour if they are all exactly otherwise the same. They even had to use bigger well bores than we thought because of the larger volume. All kinds of things changed that had not been thought of when I left the company. But again, they were always looking out there, and they always said it might be something.

TP: It reminds me of another question relating to some of the advances in geological understanding back in the 1960s. I know one of the big breakthroughs that Shell had was in developing theories about oil origin and migration. How were you able to incorporate those kinds of factors into your economic analysis? Do you remember when you started doing that?

GB: No, but I would say that it was probably around the mid 1960s, I would say. That is where they talk about the temperature required to generate the oil, and you had

to have a path of migration, and you had to have an area in which the migration could occur.

TP: Was that very helpful to you offshore, or was it more of a . . .

GB: It was helpful everywhere. And if it is higher temperature, you get to the point where it is predominantly gas. Previously, we wouldn't be able to say whether it was going to be gas or oil, and we got so we could predict that pretty well. And I would say it was very helpful. Bally [Bert] played a large role in that, and there isn't anybody that can do better than he can do to tell you the time . . . I mean, I am assuming he remembers. I don't know that.

TP: Another thing I wanted to ask you, I guess, is about people. You referred a lot about McAdams and Galloway and a lot of the major players, going back now to the earlier period, but I haven't heard much about Ned Clark and, you know, from what I can tell, he was very important. Did you know him well? Can you talk about him?

GB: Yes, I did. As a matter of fact, I was very fortunate in working under him because Ned was the kind of guy that would be bothered by something, but he couldn't quite express it in a way . . . but I could detect what was bothering him and usually could come up with something that satisfied his need. Like, he felt we

should sell off a lot of properties that were nearing their economic depletion, and they took a disproportionate amount of our time, and he wanted to get the people to do it; they wouldn't do it because they were afraid that sure enough, they were going to sell this and someone was going to buy it and drill deeper and find the bonanza. So, I finally wrote a letter, and we wrote a report in which we studied these areas that were candidates, and we said we recognized, in the letter, that there was a risk that large discoveries might be made at deeper depths that we don't now see, but we are prepared to take that risk. Ned and Bouwe Dykstra were at odds over offshore. And Bouwe felt that it was going to be so costly . . . as I recall, he said, if you found South Pass Block 27 in 80 feet of water, you couldn't afford to develop it even if you could find it at no cost and no bonus cost. And I did a study on that. I personally did a study and other people helped me, with cost estimates, which showed that that wasn't so. I mean, we knew how right then to build things that would make it and plenty would be left over for exploration and development, if you could find a Block 27. I didn't know whether you could find it or not.

TP: You have had a couple more. A couple hundred more or so.

GB: Yes, right. But the thing was that McAdams didn't understand the mechanics of production and drilling and whatnot, but he thought there was a lot of oil out there, because, really, geology doesn't know where the present shoreline is now,

and he felt there was a lot of oil out there, and Ned supported him. Kemball-Cook was the executive vice president at that time. Kemball-Cook was neither engineer nor geologist. He was more of an economist. But Ned supported McAdams in that.

TP: He was able to back it up with these quantitative studies or economic analyses.

GB: Sure. Right. He was. But they had me do this study, and they took me to New Orleans with them. They said, we don't want you to get in this because it is going to be pretty hot and heavy. We want you to be outside in case we need to bring these studies in. Well, when I got there, Bouwe welcomed me, and he just got me on in. There was nowhere I could withdraw again! And so, here I was in this meeting as the young engineer. So they asked Bouwe, they said, "What is the situation?" And he said, "Well, you can't make a profit on it. Do you have any studies to prove that?" He said, "No." They said, "Well, we do. Gene, show him your studies." And I did. And Bouwe said, "Where did you get those costs?" I said, "Out of your capital budget!" And man, I thought I was going to be fired. But certainly, Ned was . . .

TP: Why was Dykstra so set against it?

GB: I don't . . . For the life of me . . . he was not that kind of . . .

TP: He was so instrumental in the early . . .

GB: In getting it started.

TP: Getting it started.

GB: It just baffles me why he suddenly became pessimistic about it. Maybe people tend to get more pessimistic as you age, but he still wasn't all that old, you know? He didn't retire for a number of years after that. But anyway, Ned was very supportive . . .

TP: Maybe he just felt that he was assuming that his area would be assuming all the risk in this venture because it is in his backyard. And if it did fail . . . .

GB: I believe he genuinely believed it was going to fail, but I don't know why because it is not characteristic of his history for him to do that. But Bouwe was . . . it wasn't any fun for me! But I knew he was wrong. I mean, I didn't know that he was wrong about whether it would ultimately be economic or not, but I knew that his basis for it was wrong.

TP: So, he didn't produce any studies to try to prove the point? He didn't have any

people on it doing cost analysis?

GB: They made studies after it, but he couldn't support it. Even if they could, they weren't going to accept it. So, I suspect if he hadn't gone along with it eventually, that he would have left the company, because I don't think they would have . . . I mean, the top management in New York -- McAdams, Ned Clark, and Kemball-Cook, and Galloway before that -- believed that it was going to be economic, and it was. Of course, all this hindsight is 20/20. You don't know about that but . . . and Bouwe could have been right, but I haven't the foggiest notion why he took that attitude. It was uncharacteristic of him then, and actually, he didn't get in the way of going ahead with it afterwards. I don't believe he ever took credit for it. I doubt that he did. I mean, in the sense that he said, "I pushed for this."

TP: Well, this would have been about what year, the meeting that you had? That would have been about 1960, is that right?

GB: I think it is either 1959 or 1960, because I went to The Hague in February of 1960. So, it was before that. But anyway, Ned's contribution was . . . .

TP: How would you compare Ned Clark's style to McAdams' or some of the other people? What was his personality like?

GB: Well, he was much more reserved. McAdams was very volatile, strong feelings

about people. He had a division exploration manager in Houston, and he had just come here. He had been here about six weeks, I think. And we had a vice-presidents meeting, and Mac said, in this meeting, he said, "I just don't know about" so and so. And I said, "Well Mac, I don't know about him either, but in six weeks, we can't turn around in six weeks and decide we made a mistake." That is all I said at that time, and we didn't do anything at that time, but that is indicative of his ability to change. It could go both ways. It wasn't just that he would say this guy is good and then he is no good, and no good forever. He might become good again! But Ned wasn't that way. He was more even-tempered. He was an engineer. He wasn't a geologist. But he was knowledgeable in drilling and production operations, and he had a good business sense. I feel he was good at choosing people, and so was McAdams. I mean, I am not going to knock McAdams. He was very valuable to Shell. But he was very difficult to work for. Well, I didn't work for him ever. I mean, I did a lot of things for him but I wasn't under his supervision. But I had to work with him a lot, and I respected him.

TP: But he was known to make snap decisions?

GB: That is right. He certainly was. Ned was not that way, but neither would he waste a lot of time. He made the decisions when he needed to make them, and he was, of course, heavily involved in the decisions on all the drilling -- the semisubmersible rigs and that sort of thing.

TP: Can you talk about other notable personalities, like presidents of Shell that you worked under, say, beginning with, I guess, Monty Spaght. You probably didn't work closely under Burns.

GB: No, I only saw Burns once or twice, and then I had no experience with him. I thought Spaght was a fantastic executive. I can't imagine anyone better qualified than he was. He had an advanced degree in chemistry, but he got into management fairly early. And so, I would say that he was pretty stable. I worked closely with him because I worked in head office. I had contact with him there. One of the managing directors was in New York. I didn't know at the time, but he asked to have lunch with some young people that were comers, and Spaght invited me to that lunch, where there were only six of us at the lunch. And so, I got along well with Spaght and worked for him in London, directly under him in London. Well actually, I worked under the committee of managing directors, but Spaght was responsible for Shell Oil and Shell Oil matters. He had everything that I could imagine an executive having. He had knowledge, he had . . .

TP: What was your experience of London like? It was only a short period, right?

GB: Yes.

TP: It was kind of exposing you to the group and to the rest of the . . .

GB: Well actually, the job I was in . . . that is part of it, but the job I was in was supposed to be for about three years. What happened is: Ned Clark decided to retire as executive vice-president, and they wanted Christianson to replace him, and they brought me back to replace Christianson, so I came back earlier than I would have otherwise. I kind of hated to come back at that point, because I really enjoyed living in London and enjoyed the work.

McCurdy was different, he was a good president.

TP: Now, Spaght and McCurdy were the only two that I could think of in this period, presidents who didn't come from E&P, or was Kemball-Cook from outside of E&P, too?

GB: Well, Kemball-Cook was in E&P first, I think, but he wasn't a technical person so he, in a way, was an outsider, but McCurdy came from E&P originally. He was a petroleum engineer.

TP: I guess I just associate him with Shell Chemical.

GB: Yes, that is what he did for a long period, but he did come from E&P and, you

know, that is an unusual move, to go from what he did in petroleum engineering, to Shell Chemical. And then to run it for such a long time, then run the whole company. I never worked as closely with Dick, although I knew him, was around him a lot in New York and other places, but if you would have to rate people, I would probably rate Spaght above McCurdy, but McCurdy was . . . I have no criticism of any action, any decision he ever made that I had anything to do with.

TP: How was he a different kind of personality than Spaght? Spaght was very serious and calculating and hard-nosed in some ways, from what I understand.

GB: Yes, but I would say that McCurdy was hard-nosed, too. But Spaght was always making speeches, and he was excellent at it. McCurdy made speeches sometimes but he didn't . . . I think Spaght sought the opportunities, and he did well at it. In fact, there were three books . . .

TP: Yes, *The Bright-Key*, I guess, and a couple of others.

GB: You know, the interesting thing is, since I know Spaght so well, as I read those speeches, even though he didn't write the speeches, he always put his mark on it. I could tell almost what he had put in it, you know, where he went through it. Once when I was in London, Spaght was going to give a speech in Norway about the offshore oil industry. And so, we brought a young engineer over there who

worked under me, and we wrote his speech. And then, he put his mark on it. I mean, he really did. And he knew the subject and did an excellent job. Very few people can do that. Spaght was very gifted.

TP: Well, it was unusual, too, that he was tapped to be a managing director.

GB: He was the first American to be a managing director.

TP: I guess it is hard to speculate on why that was but I guess he had done such a good job turning around the company from . . . because there were a couple of years there where . . .

GB: Yes, that is right . . .

TP: . . . some rough times.

GB: It is hard to say, and I certainly wasn't in a position to have any knowledge of why they decided to. But later, the thought was that they would like to see more of that, and one reason that they hadn't was the fact that we had outside shareholders in Shell Oil. And now that they've got 100% of Shell Oil, they can move people any way they want to, and money.

TP: And you obviously spent a lot of time working with John Bookout, too.

GB: Oh, yes. I spent a lot of time with John starting in Oklahoma City when he was a geologist and I was an engineer. So, I know John very well. He, of course, went on one step beyond Spaght. He went on to the board of the parent company. Well now, actually, Spaght did, too. Spaght was on the board of Royal Dutch.

TP: How would you characterize Bookout's years, his style, approach?

GB: I would say that Bookout is a superb industrial politician. I mean, he really knows how to play the powers that be and to get the right people on his team. I don't know that he is unlike any successful executive, actually.

TP: I guess it was his greatest challenge, though, trying to take Shell Oil through the buyout period.

GB: Yes, it must have been, but I wasn't there then.

TP: Contentious . . . he was in the midst of a contentious battle on both sides . . .

GB: It was tough. And I would say that his contribution during that time had a lot to do with him becoming director of the Group. I would guess that was it, but it

would be speculation on my part. But that was a very difficult time.

There were troubles when I was in London. There were problems with . . . they were developing the North Sea, and Shell Oil had more of the technical people and know-how to do offshore development. And they wanted to have first call on those people. And since there were outside . . .

TP: This would have been about the mid-1970s, or somewhere in there, or even earlier?

GB: Well yes, it started then, but it started earlier than that because . . . it started in the mid-1960s. And the problem was that, theoretically, if you made a move with someone, it had to be to the benefit of Shell Oil Company shareholders. It had to benefit both sides. But the people there never quite understood. I am sure many people did understand at higher levels of management, but many people at fairly high positions didn't understand whey they couldn't just say, I want that guy to come here and do this. And the management here just couldn't say, yes, it is fine if they gutted their own organization. I even said at that time, the best solution would be for Royal Dutch to offer to trade shares in Royal Dutch or Shell TNT for shares in Shell Oil and having 100%. I didn't like the idea very much but I thought that would be the best way to solve it. It wasn't the way it happened. They actually paid cash, but it was the same sort of thing.

TP: I know Shell and the group had this research sharing agreement. Did Shell Oil share a lot of the technology that was developed offshore?

GB: Oh, yes.

TP: I have heard though that Shell tried to protect bright spot and not disseminate that, even within the group.

GB: Yes, I think maybe that may have happened, I don't know, but we did have a research sharing agreement. We balanced costs. They paid us. It started during the war years, World War II years, they couldn't do research there. All the research power was here in the U.S. And so, they agreed to pay part of it. That was necessary because of the outside minority shareholding. We did share it. As far as I know, I don't think anything . . . I can't say that something like bright spots might not have been withheld while we were getting ready for a specific sale or something, but I don't think there is any long-term withholding of even bright spots. I know of nothing else. I don't even know that bright spots were withheld but I think we shared both ways. I mean, I felt like that the relationship between the two companies was very effective, but they kept wanting to pay less of the research bill. That meant we either had to . . . we had to reduce research if they didn't pay more of it. And it was tough.

TP: Were they behind a lot of the . . . were they pushing Shell on offshore technology research, say, in the 1960s, or was that more of an independent initiative?

GB: I felt no push from them, but I would say that they certainly encouraged it. There was no discouragement. We used to have what they called a functional meeting in The Hague every year. We would go over there, and the exploration and production management would go over there and present their plans for both operations and research. They didn't approve it, because we supposedly had . . . but they did have hands-off management . . .

TP: "You will inform your major stockholder . . . "

GB: You bet! We had obligations to our minor shareholders, but we had an obligation to our major one as well. So, I feel that they certainly encouraged it, approved it, but I don't feel we were pushed in to it. I think the impetus came from this side, and they supported it.

TP: Well, is there anything else you can think of that we should go over?

GB: No, I really can't.

TP: We have covered a lot of ground. I think I have run out of questions.

GB: Well, I can't think of anything that I can add.

TP: I'll shut the tape recorder off.

THE END