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Interviewee: Efthymiou, Michael
Interview Date: September 21, 2002

OFFSHORE ENERGY CENTER

ORAL HISTORY PROJECT

Interviewee: MICHAEL EFTHYMIOU

Date: September 21, 2002

Place: Houston, TX

Interviewer: Dr. Joseph Pratt

Side A

JP: This is an OEC Hall of Fame interview. Today is September 21, 2002. The interviewer is Joe Pratt. The interviewee is Michael Efthymiou. Michael, I will start by asking you a basic question: tell us about your background, how you got into the oil industry, and then how you moved offshore to work.

ME: I started civil engineering in Manchester and then I decided to stay on and do a master's degree and then a Ph.D. The topic of my Ph.D. was loading on offshore pipelines. I became interested in applying it, and even when I came from Cypress to England to study my thoughts were always to study and then to return and apply whatever I learn in Cypress. But, things went differently.

I really wanted to work in the offshore industry. I found it a big challenge. The oil and gas industry in the North Sea was just picking up at the time. So, I applied for a job with Shell because I knew that they were doing rather serious work in the research and development of offshore engineering.

I started in 1981 with Shell. The first topic that I worked on was stress concentrations in tubular joints. Fatigue, which is the degradation of a structure over time with repeat cycles, was not such an important problem in the Gulf of Mexico where the industry began. However, when we started putting structures in the North Sea where the environment, especially the waves, is much more severe, much more of an issue.

I worked with one specific aspect of the problem, which was to establish the level of stressors very local to the connections which make up the offshore platform. Until I started, there was some knowledge from people who worked the problem before but it was not comprehensive and it was not covering all of the joint types that you would find on an offshore platform. Therefore, the designer had to make some leaps of faith and make up numbers as he went along. I think experienced designers, by and large, could do that, but there was always a difficulty.

In the course of a project, you had to stop and go back and do testing because there was no other way of deriving the data that you needed. So, the essence of my work was that we did whatever needed to be done; essentially analysis of line from the major project so that the designer would have it available and be able to apply it as needed. We comprehensively covered all of the joint types that you find on a platform. One major aspect was with all the geometric parameters that corresponded to our range. The other major aspect was that the level of stress concentration depended not only on the geometry of the joint but it also depended on the type of loading.

Prior to my work, people were making assumptions about the loading, whereas I brought it into the equations explicitly. It has now been introduced in software for analyzing and designing of platforms. So, most of the commercially available software now can have both the geometric aspect and the loading aspect.

JP: Did your work and your Ph.D. on pipelines give you an advantage in seeing the need to do this?

ME: I think it gave me an advantage in understanding the loading. Because it was the loading that I started for my Ph.D., it was easier to understand how a certain pipe or a certain connection gets loaded. So, it was useful in that sense.

JP: Is it essentially true that people had not had to worry about this in the Gulf because it was not going to be a problem?

ME: It was less of a problem in the Gulf, yes.

JP: They did not know that it might be a problem?

ME: Right, whereas in the North Sea we saw loading situations for instance direct loading when a connector got framed, where it becomes really important. So that work took the best part of about five years - from 1980 to 1985. When I completed it, the first step was to implement it within Shell.

Then I quickly moved to another topic because there was a mini panic situation with respect to the integrity of one of our major installations in the North Sea. It was actually the biggest installation that they had at the time. There was a real problem with respect to the integrity of the platform.

We had used up all of the available slots for conductors and we needed to put more loading on the top sides, which is actually not uncommon. What was different here was that we needed to put quite a lot. The reservoir was quite prolific; it was producing more than we had anticipated and when we reanalyzed the platform and used all the latest technology, we were finding it to be overstressed.

JP: So, you started studying it to just see the implications of new load and you realized that there might be a problem?

ME: Right. We started analyzing it to prove that it was fit for purpose, so we could go ahead and put the loads on. We were talking about maybe adding another 1,000 tons on the top sides.

JP: Which platform?

ME: It was the North Cormolon platform, and at that time it was in about 165 meters of water and was the deepest in the North Sea. Since then, they have put a few more in somewhat deeper water. The perception of the loading increased since the time of the design of the platform . . . we designed it in 1978, with certain parameters on the load, and those parameters increased by about 15% over that period of five or six years . . . and because we wanted to apply more load beyond what was emphasized at the design stage, it was looking overstressed.

We set up a program whereby we would strengthen the platform if needed and, on the other hand, we were really anxious to see if there was any reserve capacity on the platform - how strong, really, is the platform? That is when we applied, for the first time, methods of nonlinear analysis that can tell you how strong the platform really is. We went beyond code allowables and beyond normal conventional design and we were able to demonstrate, after a study of about 18 months, that the structure actually could take the additional loads. So, we went ahead and we put the loads on. For me, that was the start of recognizing the importance of the reliability of offshore platforms.

When we were tackling this problem, we only felt comfortable addressing the strength side of it - how strong the structure really is. But, the question of how safe the structure is has two aspects: what is it that is loading it and how strong is the structure? So, after we gave North Cormolon a clean bill of health and they could go ahead and continue to operate, then we started addressing the bigger problem of the reliability of offshore platforms. It took us about 7 or 8 years to complete. We did build on work that was done previously by Peter Marshall and Bob Bee in the 1970s when they first had to address this problem in connection with the Gulf of Mexico.

The history of the Gulf of Mexico platforms is quite different. Before the hurricane comes, the Gulf of Mexico platforms are evacuated. Hence, the level of safety and the consequence of a failure are quite different than for the North Sea where it is not practical to evacuate and the installations are so much bigger. They may be 10-15 times bigger in terms of the cost . . . and the manning level, we are talking about hundreds of people.

Around 1985-86, as we were tackling the problem of North Cormolon, there was another major platform which was being designed at the time, the turn platform. We decided to put instrumentation on the platform and that is a unique system - unique because it measures the total load that the structure sees acting on it from waves, currents, and winds. So, we instrumented the turn platform. The instrumention alone cost about two million dollars. The analysis of the data was an exercise that took us from 1988, when the platform started operating, until 1993; we were gathering and analyzing data. The intent of that was to establish the magnitude of the environmental loading and how that magnitude varies over a storm.

It was a major exercise. We involved the rest of the industry in it by making it a joint industry effort.

JP: Had that been done before? Did people use such instrumentation to try to get real measurements?

ME: Prior to that there was one instrumentation exercise, but it was much smaller in scale. It was done, I believe, in the mid 1970s. It was the OTS platform - ocean test structure. But it was so much smaller that it was actually a test structure. Only a couple of members were instrumented, you could only infer loads in a member. We were able to measure the total load by placing strain gauges on the base of the platform, on the main legs and on the main diagonals near the base. We had to place 68 strain gauges and between them we could work out the total load on the platform. That was unique.

Because of that, there was a lot of interest in the industry, so a joint industry effort was well funded by the major oil companies. The main designers took part and as the data was being collected, it was analyzed and presented to the joint industry effort. Separately, we were doing analysis within Shell and our partner, Exxon, was doing analysis here in Houston. And we were comparing the analyzed results from Exxon, our analyzed results, and those of the joint industry effort.

It was an eye-opener for many of us. We designed the platform to withstand about 100 megaton, which is about 10,000 tons of lateral loading. We designed it to withstand 10,000 tons as a design load. So, if it sees a design event which is an event with a return period of 100 years, then we thought we would design it to withstand 10,000 tons. We were quite lucky that on the first of January of 1992 we got the biggest storm ever recorded over the last 50 years. It was close to the design event, or at least the notion of design event . . . at least in terms of the wave height, it was close.

What we measured was actually, if I remember correctly, a little bit less than 4,000 tons. So, there was a huge difference, a difference of about a factor of 2, between what we designed the platform for and the actual load that was seen by the structure. There is one main reason for that. Prior to that work, at least in Europe and Norway, it was common practice to apply the 100 year wave and the 100 year load and the 100 year wind and assume that they would occur at the same time. We were adding them, we were assuming that they would occur at the same time and that they would occur from the same direction. What became clear throughout those four years of measurements, and was emphasized by the big storm, is that the big wave does not occur at the same time as the big current and it does not occur at the same time as the big wind. Also, they do not arrive from the same direction. These effects were quite significant and the monitoring system enabled us to demonstrate that and to use data to quantify it. Of course, there was another set of data. So, that is the monitoring system and its value.

There is a second value that is also very significant. It was very useful in telling us the level of uncertainty or variability in the loading. You would take a given storm, look at the variability of the loads that occurred on the platform during that storm, and compare those with what you would predict the variability to be. That exercise was extremely useful; it quantified those and it validated our models. So, there is the variability of loading on a platform, the noncoherence of waves of winds and currents - we got those elements from the turn monitoring system.

At the same time, we spent some effort developing methods which would tell us how strong a structure is; the ultimate strength of the platform. Those are numerical methods and we did some testing to validate those. Bringing these elements together led to a complete and holistic definition of the reliability problem.

When we did that, it enabled us to answer questions that we were not able to answer five years before; for instance, is the North Cormolon platform safe enough? Now, we could say with confidence that it was extremely safe. We could express that in terms of probabilities of failure; the probability of failure is extremely low. So, when we got to that stage we felt that the next step was to implement those methodologies first within Shell. If you change the design methodology in such a big way, you do need to discuss it with regulatory bodies, classification societies, and so on. So, that is when we started discussing it with, for instance, the HSC in the U.K. We discussed it, of course, with other oil companies and eventually that led to revising the design practice to take these effects into account. What you see is that the international standard for the design of offshore platforms has that methodology embedded in it.

The reliability methodology was actually developed prior to us with pioneering work from Peter Marshall and Bob Bee. Later in the 1980s, there was a development within API of the so-called load and resistance factor design. So we built on it by using models which are more reliable and more accurate.

JP: Are computers crucial to the development from theory to more knowledge? Is computer monitoring a big part of that?

ME: Yes. In fact, some of the problems could not have been handled if we did not have significant improvement in computing power. The biggest element there is the use of hindcast models. I have not talked a lot about those. Once you recognize what is going on using, say, the full scale monitoring of the turn platform, then you go back to hindcast models; those were in existence for major offshore areas like the Gulf of Mexico. There were hindcast models built already.

Hindcasting is where you go back in time and you generate tracers of winds, waves and currents, and what they looked like 50 years ago. You do it over the entire time from, say, 1900 until today. And if you are able to generate those records, it is actually a remarkable achievement. But without having any instruments, say, at a certain location in the Gulf of Mexico, you are nevertheless able to tell in 1905 what the current and wind and wave environment was like. That is possible because the pressures in the atmosphere were always known and were always measured. You start from pressures and you generate wind speeds. Then, from wind speeds, you generate waves and you generate patterns. All of that is very intensive numerically, so you need very powerful computers to do that.

JP: You are going way beyond just the theory of what 50 and 125 year old waves; you are saying that is what they actually were. . .

ME: This is what they actually were . . .

JP: This is from 75 years of data.

ME: Right. And from those 75 years of data you are able to predict with accuracy what the next 100 years will look like, at least statistically. Then you are able to distinguish between the wave and the current and wind combinations, so you are no longer forced to apply the maximum at the same time. That is really the value of that work.

JP: You say development of OCF factors, equations for fatigue of offshore structures, is your most significant accomplishment, and then the implementation of these load and resistance factor designs in Shell's installation. Does that follow then or does that come out of the work?

ME: Right. There was actually a real need to apply the load resistance factor design in areas where we had many structures being designed and built every year. And, for us, those areas were in Malaysia, Brunei, and to a lesser extent in the North Sea. So, we revised the design methodology for those three areas and we did it quickly. The result of that was: a) you know safety much better, so you design them to treat a determined level of safety and that level of safety is extremely high; b) you do save money in the process. It is not immediately obvious how you can both save money and make structures safer, but it is possible because those areas where you take steel from are areas where you do not need it. If there are areas where you place some steel, you do that and still you save quite a lot of money overall.

In particular, in Brunei and ______, because we have many installations . . . generally, that population of platforms were safe enough to take on more load. So, instead of designing new structures in order to drill in between the existing wellhead platforms, we tended, over the last five or six years, to utilize those existent platforms more and more. You will find many platforms now in those areas where we have added conductors over and above the design stage. Because there were plans to put in place many new structures. . . after our methodology it was no longer needed to place those structures in and you can imagine that it had a big impact in terms of the capital expenditure of those components.

It is very well recognized that in those areas we made some significant savings from the time when I implemented it, around 1994-95, until now. I would like to recognize the efforts of a colleague of mine, Jan (William) VandGraaf, who has worked passionately in implementing the reliability-based methodology in those countries. He is still head of offshore structures in Brunei. We developed the methodology together and then he got transferred to Brunei to implement it. It is a remarkable story of how you can move from research and development to implementation and realize very significant benefits in the process.

There were some other areas where the application of the methodology shows you that there are some areas where structures are not sufficiently safe. One example is the available air gap in places like offshore western Australia. The environment there, of tropical cycles, is such that you need a higher air gap. We recognized that by applying the methodology and then we implemented it. Now, new structures in that area have a bigger air gap than their predecessors. That is a small additional cost and it leads to a significant improvement in safety; it makes it sufficiently safe.

JP: Is it hard to convince a company to do these things? Are the managers going to be skeptical of the researchers? In Shell, were they willing to listen to you?

ME: Within Shell, it was no problem at all. In fact, it was always done with a lot of management commitment. Since the mid 1980s when we instrumented the turn platform, there was a big commitment to spend two or three million dollars. Woodside is actually not a Shell company; it is the main operating company in Australia. We have about a 25% share in it. But generally, we have been working very closely with them. When we make recommendations which are clear, they do generally follow them.

JP: You talked about learning from Peter Marshall and others. Was there a lot of interchange between the group and Shell USA?

ME: Our relationship with Shell USA is a peculiar one, but it has evolved tremendously over time. In the 1980s, we were not actually allowed to talk to the U.S. If there were telexes going across, they had to be sent at a very high level. So, I was not allowed to just send a telex across and communicate with people like Peter Marshall, even though I have always respected him greatly. I think it had to do with corporate ownership - that Shell Oil was not a wholly-owned operating company. But, they were trying to make it. I think that is part of the reason. But, even after it became wholly-owned, the philosophy was to let Shell Oil operate as before. Perhaps there was an agreement that Royal Dutch would not interfere with the activities of Shell Oil.

All of that was reversed around 1996-97, and now we work very closely. In fact, we have what we call global teams; global means that we work across from Holland to Houston in single teams or single organizations. We even charge to the same accounts. We have common systems. That way: a) we communicate so much better; and b) we are able to utilize the best people wherever they are sitting to solve a certain problem. So, we are actually using the people from here to design platforms in areas where, in the past, only Royal Dutch Shell would touch. It is also happening the other way around for problems in the Gulf of Mexico, they use expertise from The Hague. In fact, I have got some friends who are now working on installations in the offshore Gulf of Mexico, and a couple who work in New Orleans. There was a remarkable change around 1996-97, and we are benefiting from it now.

JP: That sounds like the equivalent of the BP/Amoco merger . . . creating a truly international company out of parts. Shell USA has been incredibly independent. I guess one way to have a global company and have efficiency on a global scale is to really start to use people.

ME: Yes, I think that is what it is. When the rest of the world is moving, not only in the oil industry - just everywhere is globalizing - you cannot ignore those changes; otherwise, you lose efficiency.

JP: Here you say that a topic of special interest would be deep water challenges. Does this research apply directly to the deep water? Is it just taking it to a different level?

ME: All the technology that we have talked about so far relates primarily to the conventional platforms in less than, say, 300 meters of water. Deep water technology, as you are aware, is different than dealing with floating platforms. The titles may look the same when you move to deep water; for instance, reliability of a floating platform is still a major issue, but the actual mechanics of solving them are different. Actually, there is also a big difference in the consequence sense, which makes the reliability of fixed platforms a little bit more important, and that is when that load exceeds the capacity of the structure, a fixed platform will collapse. I need to qualify that: if you talk about the hull itself, the way that it works is quite different from the fixed platform. So, what we started doing is looking at the reliability of what anchors the floating platform to the sea bed; that would be the moorings if we talk about the spar or a ship or a tanker, or it would be the tendons if we are talking about the tension leg platform. Those elements, in terms of cost, are relatively small in relation to the total cost of installation. Hence, it is possible to make those more reliable than for the fixed platform. So, the equations are different and the picture is different, but the problem of safety of the deep water installations is, of course, as important. The topic of reliability of deep water platforms may not receive as much attention in the near future as the reliability of the fixed platforms did in the past.

JP: Once all that is clear analytically, it would also be another cost advantage for the TLPs and the floating production.

ME: There, by applying reliability methods similar to those that we apply for fixed platforms, I think the benefits will not be as significant because the cost of whatever anchors the platform to this event is small in relation to the overall cost. There are other challenges with deep water in general because one of the challenges I see is as we go deeper is that we need to do more and more subsea. That means that we need to place more and more of the equipment on the sea floor. If you manage to do that completely, you eliminate the need for floating platforms; but, of course, to do it completely we need to make one big step forward and I think it is one of the biggest challenges to do with processing on the sea bed. It is a big challenge but it is also a recognized challenge. There are people working on it now and there will be, I am sure and I hope, improvements over the coming five years in that area. They can then revolutionize the subsea and deep water.

I see another challenge in deep water which goes in the opposite direction, and that is for oil companies to recognize the value of direct systems; to say it more accurately, to recognize the value of having direct access to the wells. There is a big value in achieving that and to illustrate it I will go back to areas where we do have access to the wells. If you look, for instance, at installations that they put in place 25 years ago, the design life was expected to be 20-25; for example, the Brent field.

The Og field, in particular, was meant to be abandoned in 1990. It is still producing and it will continue to produce for another 10 or 15 years. The reason for that is that we are able to recover more of the field than we thought in 1970 because of enhanced recovery techniques. From the existing wells, we can sidetrack and reach pockets within the reservoir that we now know exist, but we did not know of their existence 10 or 15 years ago. Because of that, the ultimate recovery from a field is actually much higher. It could be 1-1/2 times higher than what was thought of or considered at the design stage.

Statfjord is another good example. It may currently be producing 200,000 barrels a day; whereas, if it was designed differently, it would now be completely abandoned. What I am saying is if we go for direct access production systems, then we are much more likely to recover more of the reservoir than we would otherwise. That is very significant for the future.

JP: Yes, because you can apply new technology as it becomes available.

ME: Yes.

JP: In subsea, it would be very difficult to go back and redo that.

ME: Yes, it is very difficult to go back and redo things once it is subsea. There are many reasons for that. The main reason is that in order to go and access a subsea well, you need to take a mobile offshore drilling unit and the cost of that is typically $200,000 a day. You need to mobilize it, demobilize it and do the job. You are already talking about tens of millions. Whereas, if you have access to the well, you just do it and it does not cost you anything. If you are not certain of the outcome of, say, drilling a small side track, if you have that access you will do it and the benefit may be that you have another well flowing, say, 10,000 barrels a day; whereas, if it was subsea, you would not do it and that is what you would lose.

You need foresight to go for that because sometimes when you compare an option for developing a hypothetical field in the water with a subsea scale and a direct vertical access scale, you may find that the capital expenditure of subsea is in a hypothetical case, lower. Okay, so then you take the decision to go for subsea. But, in fact, in the long run you would benefit more if you had gone for the other option.

JP: In talking about Bullwinkle and what they thought they were building there and what it has become . . .

ME: Yes, it is actually an excellent example of the same thing.

JP: So, finances come out completely different . . .

ME: Completely different, yes. Now, if you had developed Bullwinkle as a subsea tieback to one of the Shell Oil platforms, you would have lost all of the benefit.

JP: And probably never even would have thought of doing it. There would not be any reason to think about it if you knew you could not do it - you lose the option to be innovative.

ME: Yes, you do. Exactly. And I think that, to a large extent, the same applies to most of the TLPs that we have in the Gulf of Mexico. We have five or six and they are direct vertical access systems. Now, all of them are producing more than what we thought and we have many more small subsea fields tied back to them. That benefit is huge, but you cannot take that benefit into account at the design stage because you do not know the future improvements in technology and you do not know the future discoveries.

JP: You do know how much it costs to put one of those structures in place, though!

ME: You do know that, yes.

JP: Are there other things that you would like to talk about? We have covered a lot. We have not really talked about the deep water risers and you said that is an area of specialized expertise. Have you been involved in that?

ME: I am actually involved with the water risers at the moment which is perhaps why I put it down. It is of special interest to me but there is nothing special to say there.

JP: Is there anything else, in terms of people or general reflections of the offshore industry, that you would like to put on the tape?

ME: I am delighted to have been given the opportunity to work in this industry over the last 20 years. I have worked on some great teams. I should say that most of the work I have described has been the result of team work rather than the work of just one individual. I am grateful to Shell for giving me that opportunity, and I am grateful to the people that I have worked with over the last 15 or 20 years. I have mentioned working with Peter Marshall and Jan William VandGraaf.

End of Side A

Side B

ME: I should also mention Peter Trumons, who has done a lot of good work on the loading side of the variability models.

THE END