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Joseph H. Engel Interview (MORS)

Engel, Joseph H.

Military Operations Research Society (MORS) Oral History Project Interview of Dr. Joseph H. Engel

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MILITARY OPS RESEARCH HERITAGE ARTICLE

INTRODUCTION

Oral histories represent the recollections and opinions of the person interviewed, and not the official position of MORS. Omissions and errors in fact are corrected when possible, but every effort is made to present the interviewee's own words.

Dr. Joseph H. Engel was a Fellow of the Institute for Operations Research and the Management Sciences (INFORMS). In 1968 he was President of the Operations Research Society of America (ORSA), which later merged with the Institute of Management Sciences (TIMS) to form INFORMS. He won the INFORMS Military Applications Society (MAS) J. Steinhardt Prize in 1986, and in 1992 was awarded the INFORMS George E. Kimball Medal. Joe was on the MORS Board of Directors in the late 1960s. He was an analyst in the Operations Evaluation Group (OEG) and was Director at the Center for Naval Analyses (CNA). Dr. Engel died on May 29, 2011. This interview was conducted in Fairfax, Virginia on December 21, 2004; January 25, 2005; January 29, 2005; April 27, 2005; and May 2, 2005.

MORS ORAL HISTORY

Bob Sheldon: We're here to interview Joe Engel. First of all, please state your name and where you were born and raised.

Joe Engel: Joseph Henry Engel and you can call me Joe. I was born in 1922 in the Bronx, New York City, New York State, the United States of America, the world. *(Laughing)*

Bob Sheldon: Who are your parents and can you tell me a little about them?

Joe Engel: My mother's maiden name was Jennie Gotthilf. My father's name was Arthur Engel.

I know that my father had an education in the United States up through high school. I do not know whether or not he had any further formal education beyond that level, although I doubt it. My mother, similarly, received all her education through high school in the United States and probably in New York City.

Bob Sheldon: What did your dad do for a living?

Joe Engel: Most of the time that I was aware of what he did I did not know what he did for a living. When I was very small I didn't pay too much attention to what my father did for a living, but I did see him and I remember seeing him and knowing about him. It wasn't until I was around seven or eight years old, that I first had some exposure to work that he did. I had great fun the first time he took me for a drive with him as he drove a laundry wagon with a horse in front of it. He was the driver and laundry man who picked up people's laundry and delivered it. I sat up high on the seat and watched the road and saw my father very professionally pulling on the reins and had the fun of feeding sugar to the horse.

The other experience that I had with my father's employment occurred about a year later. This time he was driving a laundry truck. He had been promoted and moved with the times into a more modern technology. He probably got more money for it, but it wasn't great for me because I couldn't feed the horse and play with it. That's all that I know about my father's employment, because he was out of the house for a long time.

When I was around 10 years old in 1932, my mother informed me that she and my father were going to divorce. I later learned that they had been legally separated for many years. There were never any quarrels when my parents were together to lead me to feel that they were having some kind of problem that would lead to a divorce. It came as a surprise, and from the time the divorce took place I only saw my father briefly, for a year after the divorce.

Bob Sheldon: Tell me about your early schooling.

Joe Engel: I started kindergarten at a lovely New York City free elementary public school, right across the street from the apartment in which we lived. It was not difficult to get to school at all, even though I was only 4 1/2 at the time. I knew how to cross the street, on a little quiet side street. I discovered many years later that even at the age of 4 1/2 I had already developed a competitive spirit and was very interested in doing well. I was into engineering and science, because one of the things that I obviously became aware of was the importance of the time it took to do something. I also learned how to count. So right there in kindergarten, whether I knew it or not, I was being prepared for the careers that I subsequently followed.

In elementary school, I discovered that I loved to learn and I loved to read. I recall, very distinctly, going to the New York public library very near my house on the first day of school, after I had finished kindergarten and moved into the first grade. It was half a dozen blocks or so to get to the library. I went up to the desk of the librarian and she said, ''What can I do for you young man?'' And I said, ''I would like to have a library card.'' She said, ''What grade are you in school?'' I said, ''Today was my first day in 1A at Public School 33.'' She said, ''Well, we are not allowed to admit you in 1A. Come back in the second half of the first year, namely when you are in 1B.''

I came back on the first day of 1B and asked for a library card and she gave me one. From then on I read as many books as I was allowed to take out. They were picture books for the most part, very little in the way of words in them. As soon as I possibly could, which was probably in the second grade, I was able to get a full-fledged library card. Then I would take out the maximum every day and usually bring them back the next day. I was *never* overdue. I just loved to read and I loved to go to school. In addition to English and English composition, we learned mathematics—that is to say, arithmetic. And we learned something about the world. We took courses in geography and history. There wasn't a subject that I took in elementary school that I didn't find interesting. That was the beginning of it.

This progression continued. On the first day of class when I entered the second grade, my teacher, Mrs. Buckley, said, ''Who would like to be the class reporter for our school magazine?'' *The Broadcaster* was a lovely magazine printed on slick paper, with a nice cover on it. I put my hand up immediately. She said, ''Very well, you're the class reporter Joseph,'' and that was that. I continued to be the class reporter through the sixth grade. I did a certain amount of writing myself and I chose pieces that the other students had written and submitted. I learned about layout and deadlines—not much—but the beginnings. I became interested in writing and using it for a purpose, not merely to educate, but also to entertain and instruct whoever would be reading. Also I learned how to get these from other people besides myself.

I was entering the field of journalism and by the time the sixth grade had come along, I had pretty much decided that I was going to be an English major and become a writer or journalist, or both. All that changed in my first semester of junior high school where I was in a rapid advance junior high, where you would go through three years of junior high in two years. In that first semester, I was introduced to elementary algebra. Up until that point I had solved word problems using strictly arithmetic. But in algebra we learned about the concept of a variable whose value is not known until you solved an equation. We learned first-degree equations, and then we learned second-degree equations, and we learned how to solve them and how to use them in various interesting applied ways. I was absolutely entranced.

I got encouragement from two people whom I remember very much for the help that they gave me. I decided, right then and there, that mathematics was going to be my other career. The first person who got me started was my maternal grandfather who was living with us at the time. He didn't speak English particularly well, but he did speak some English as opposed to my grandmother who did not. He spoke Hebrew and Yiddish and English. He spoke a pretty good grade of English for someone who was not taught English formally in an English-speaking country. When I'd come home from school around three o'clock, he would say to me, ''Joseph, what did you learn in school today?'' And I would tell him. One day I came home, and I was all excited. I said, ''Today, Grandfather, we learned how to sum a series.'' And he said, ''What does that mean? What is a series?'' I explained to him that there were many different kinds of series and the first ones we learned were arithmetic series and there were also geometric series. I told him the arithmetic series was a bunch of numbers, each of which was the same amount more or less than the previous one.

The simplest example of an arithmetic series is one plus two plus three plus four plus five. He said, ''What's so hard about that? You just add them up.'' I said, ''Yes, but suppose you have to add up a hundred of them? That takes a long time and we were taught a short way using algebra to add up any large number

of numbers in an arithmetic series.'' He said, ''Tell me, how do you do it?'' I told him the formula was you take the first number in the series, you take the last number in the series, you add them up, you multiply by the number of terms in the series, and you then divide it by 2, and that's the answer. I said, ''With that, I can add up the first hundred numbers and give you the answer very rapidly and you couldn't do it as fast as I do adding them up one at a time.'' He said, ''You're right. So tell me, what's the answer?'' I got out my pencil and paper and I did exactly what I told him. I wrote down one and I wrote down one hundred, I added them up and got 101, multiplied by the number of numbers that I had (100) and divided by two. So the answer is 5,050 and I said, ''And that's it.'' He looked at me and said, ''That's right, Joseph.'' *(Laughing)*

It gave me great respect for him. I loved him already before that, but that also added a great deal of respect because while he was quizzing me he knew what I was talking about. He knew it very well, enough so that long before I could have written down that formula, he had already worked it out in his head, which of course I could do now with perfect ease.

Bob Sheldon: What country did your grandfather come from?

Joe Engel: He came from Russia. He and his family lived near what was then known as St. Petersburg, then later Petrograd, and later Leningrad. They had to flee from Russia when my mother was quite small because there were pogroms at the beginning of the 20th century and they were quite terrible. Many people emigrated from Russia and other countries as well, near the beginning of the 20th century. Many of them came to the United States and others went to other countries, including the area that became Palestine. That's how my mother's family, including my mother, came to America. She was five years old when she got here and enrolled in kindergarten almost immediately. She spoke typical American, unaccented English. As far as I could tell, she was a typical young American woman. When she had me in 1922, I was her first child; she had been married about a year. She graduated from high school when she was 18 and she was a real flapper. I've seen some pictures of her. She looks just like anybody else in the United States, a great mom.

Bob Sheldon: Did your grandfather have a math background?

Joe Engel: I don't know. I do know that he had some Hebrew school training in the old country. He obviously had received enough formal instruction that he could keep ahead of a junior high school student.

Bob Sheldon: Did you continue your interest in math through high school?

Joe Engel: Absolutely. I was very fortunate. The junior high that I went to was very special. Not only was I able to learn mathematics there, I was able to study the other conventional courses. I also continued my journalism career in junior high. I became an associate editor of a junior high school newspaper, which was printed on slick paper. Every year, we won the New York City-wide competition for the best newspaper on the junior high school level. It wasn't because of me. It was because of the guy who endowed that junior high school. In return his name was placed on the school, in addition to its number, which was Junior High School 98. They had a journalism department and the faculty advisor was a former associate of the man who endowed the school. This man's name was Herman Ridder, so this was Herman Ridder Junior High School. Herman Ridder was the founder of the Ridder newspaper, which later merged with Knight and became Knight Ridder.

In high school the trend continued. When I came to join the high school newspaper, it was in my first year in high school. I was in the class that would graduate in 1938, but that class had started six months earlier. I was put into the second class rather than the first class because I had been in a rapid advance class in junior high school. The kids who got there during the first class had already snagged all the good positions on the staff of the newspaper.

When I came in, I was accepted as a reporter. I recall going into the office one day shortly after school had started. I was about 14 years old at the time. This other kid was 15. He was sitting there with his legs up, on the chair or the desk in front of him, smoking a cigar. He beckoned me to come over to him. He said, ''What are you doing here, young man?'' I explained to him that I had just joined the staff and I was a reporter. And he said, ''I'm one of the editors and

I want you to go down to the corner store and get me a box of cigars and something to drink.'' I said, ''No thank you, that's not what I came here for,'' and I walked out. *(Laughing)* That was the end, at that time, of my formal association with journalism.

But I did continue studying English as well as reading. I also continued to take whatever courses were offered in the curriculum. I went to a very extraordinary high school—Townsend Harris. You had to pass an examination and you had to be recommended to enter it. I was able to do that because I had a good record in junior high. Our high school curriculum was three years. Once we were admitted they gave us a good curriculum and as we approached our senior year, some of us were already taking college-level courses. For example, after elementary algebra I took advanced algebra, and then both plane geometry and solid geometry. I studied biology, chemistry, and physics. I took calculus in my senior year. When I left high school, I was very well equipped.

Bob Sheldon: You graduated from high school in 1938. Were you affected by the Depression?

Joe Engel: Very much so. In fact, looking back, it may well be that the reason that I didn't see my father around the house very often in the years when my parents were still married was that he was out looking for work. That's what many breadwinners did. I know that my mother was taking care of her children as a single person from 1932 on. Although I did see my father for approximately one year, I didn't see him after that. During that time he paid child support and we were required to visit him once a month. I was keenly aware of what the Depression did to us. I was aware of it on the economic, political, and social levels.

I observed that, generally speaking, the people we mingled with were people who were roughly in the same class that we were; charitably described as lower middle class. Later on, when my mother's career as an independent employee improved, we could say we were middle class.

Bob Sheldon: What was her career?

Joe Engel: The first job that she got was as a bookkeeper. Her oldest brother, who was 10 or 12 years older than she was, was a very successful accountant, although he was not certified. He was sufficiently successful that he was one of New York Governor Lehman's financial advisers. That's how I learned that money is an important part of politics. He also became the sponsor of every one of my male cousins who, with only two exceptions, all became accountants. This included his two sons and the sons of all my other aunts and uncles as well. He must have influenced, just in the family alone, about a dozen people to become accountants just like him, because it was a good living and it was honorable work. He wanted me and my younger brother to become accountants too. However, I was going to be a mathematician. He said, ''That's ridiculous. You'll never make any money as a math teacher or anything like that.''

My mother said, ''If my son wants to be a mathematician, that's what he is going to be.'' She supported me in that endeavor and I'm so grateful to her for having had the wisdom to allow me to pursue this wonderful newfound interest that I had. A similar thing happened with my younger brother. He wanted my brother to go to a technical school because he didn't seem to have the same strong mathematical inkling that I had and he thought that if he at least went to a technical school he would learn something practical. My mother said, ''Absolutely not. He goes to the same kind of liberal arts school that Joseph goes to and that will be that.'' I learned the importance of money and how you can work with it or without it.

Bob Sheldon: How did you choose your college?

Joe Engel: In order to be admitted to the City College of New York at that time, you had to have 90 percent or higher on each of five different Regents exams. I had to take exams in English, French, mathematics, and two other subjects. At Townsend Harris, we were guaranteed admission to City College if we merely completed the curriculum at Townsend Harris, regardless of what grades we got on the Regents. Although we took the Regents, no one who graduated from Townsend Harris ever got less than a 92.

I decided that I would go to City College in 1938, because there wasn't any place else that I could go to, and that when I got there, I would immediately sign up for ROTC, Reserve Officers Training Corps.

Bob Sheldon: Which service of ROTC did you choose?

Joe Engel: I went into the Army. The idea was that when I finished the four-year course, I would be commissioned a reserve officer at the rank of Second Lieutenant in the Army.

I tried to get into West Point during my first year at City College, but was not able to get a political appointment, and there was no other way of getting in. I couldn't have gone to West Point, and I soon discovered this and stopped trying, because it was useless. I was perfectly happy to stay at City College and I got my education that way.

Bob Sheldon: Did you take a lot of math courses at City College?

Joe Engel: I certainly did. We started off with differential and integral calculus. I had to repeat the calculus course on the college level, and that was a breeze, because they weren't teaching me anything that I didn't already know.

They may have broadened my knowledge somewhat, just as at some point in college, I learned that there were other forms of geometry than Euclidian geometry. What I learned at City College was a deepening and a broadening of what I had already learned; nothing completely new, not even in the field of ROTC. There were things that I had already learned that were being elaborated upon in ROTC.

The fact that people went to war was not news to me. The fact that infantry was the way ground soldiers fought wars and the general kind of information about the weapons that they had and the vehicles that they used and so on, I already knew about it.

I knew that the cavalry rode on horses and I knew that they had begun to use armored, tracked, and wheeled vehicles to a limited degree, but they still had a lot of horse cavalry and they still had a hell of a lot of fighting that had to be done on foot, by foot soldiers with rifles or pistols and bayonets.

I learned in greater detail what it was like to be an officer in command of ground troops. Ground troops generally fought with handheld weapons and marched long distances, sometimes under the cover of darkness, a forced (paced) march. This would get you to an objective at a point in time where you might be able to take the enemy by surprise.

Bob Sheldon: You did that when you were in ROTC?

Joe Engel: Absolutely, and we studied a lot. We read about the Revolutionary War and the Civil War. The Civil War was somewhat more modern than the Revolutionary War, but not completely different. But in the Civil War and in some of the other wars that we had been fighting, we were always learning a few new things.

The model of warfare before the United States came into existence consisted mainly of what Lanchester called ancient warfare, and that's strictly hand-to-hand combat or with limited range weapons. It was similar to ship fighting because once you got near another ship, you'd try to board it and engage in hand-to-hand combat to subdue the enemy crew and, if they were sailing ships, destroy their rigging. You could also try to ram another ship if you had a good, strong point on the bow of your ship.

I already knew from just general knowledge that there were many different things that you had to worry about in warfare, whether on land or sea or, for that matter, in the air. I knew that there were biplanes and monoplanes and that sometimes, these guys shot at each other with machine guns. Sometimes, the aircraft dropped whatever size or type bombs they had on board on various land targets. I mean, these are things that everyone knew from reading the newspapers.

When I took ROTC, I was learning the principles of warfare, both in the ancient sense, because we still were fighting a lot of ancient-style wars, and also in a more modern sense. The term ''modern warfare,'' which was used by Lanchester, had a specific definition. This was warfare in which you had weapons that could destroy at a greater distance and in which you could see all your targets. That is, you could aim, fire, and cause destruction at a distance.

Another type of warfare was guerilla warfare, in which one side or the other could hide and fire from concealed positions. Lanchester came up with four different kinds of warfare: ancient warfare; modern warfare, in which both sides were exposed; guerilla warfare, in which one side was exposed and the other not; and warfare in which both sides were hidden. The equations for each of these were different, and although I didn't learn about Lanchester

warfare at City College, I learned about it soon after, along with the *Theory of Games and Economic Behavior* by John von Neumann and Oskar Morgenstern.

One of the things that I learned at City College, which has a very direct bearing on military operations research (OR), was how to develop an estimate of the situation. It came up in my experience in a context of infantry operations and it was a form of developing tactics for fighting based on particular kinds of battles.

Suppose, for example, you've been marching down a road and it's a nice, cleared field. At the end of this field is fairly heavy woodland. Your original plan was to walk down to the end of this field and make a right turn because that's where the road goes.

Then you make a left turn another mile later, and you go down another road and eventually, you get to a place where you are to arrive just as it gets dark. You're going to enter a bivouac for the night, and you're going to attack the enemy who is entrenched just a little ways further in the next woods down. The troops that had already been fighting there will have to be relieved and brought out. You will go to their location at night, under cover of darkness, to relieve them.

But while you're walking down this road on this sunny summer day, you've been chatting with your troops and seeing how they're doing. Then you're walking along at the head of your platoon and you're talking to your platoon sergeant and suddenly, you're fired at by machine guns from the woods just 50 yards in front of you.

What do you do? I actually was tested this way in a two-sided maneuver. After I had been commissioned, I was doing this with real troops and the Army knew that you'd all benefit greatly from the experience of two-sided maneuvers. They had referees to see what we were doing. What we had to learn while we were in training was how to deal with situations like this.

When this actually happened to me in training, I flunked the first part because the enemy opened fire on us with machine guns, and here I was, at the head of the platoon, which consisted of two squads in the rifle section and two in the mortar or heavy section.

The men in the front section fired rifles and they had bayonets that they could use; some of them had Browning Automatic Rifles (BARs). In the heavy section, the weapons platoon, they had light air-cooled machine guns and 60-millimeter mortars. These were too heavy to be carried by one man, and they had to be put together and mounted to be fired.

What do you think my platoon sergeant did when we were fired at by these machine guns?

Bob Sheldon: Probably jumped in the ditch.

Joe Engel: He did better than that. He yelled, ''Down!'' and we all jumped into the ditch, and that's what I should have done. But I had to figure it out and it took me two seconds longer. Had he not done it and had we been standing there for another two seconds, it's quite possible that more of my men would have been wounded and killed.

I flunked that part of it, but what do you do when you're pinned down by enemy fire like this almost in the open? That's where the estimate of the situation comes in.

You first have to know your position and the enemy's position and the terrain and how it could influence the battle that has just begun. In this particular instance, which is an experience that we actually went through, as soon as you're fired upon, you've got to take cover. I didn't do it fast enough.

More specifically, the situation I had to deal with is the situation we had studied in school. The enemy troops firing at us were on the right side of the road as we were approaching it.

This was a straight country road with occasional right angles with it around the various quarter sections, with two slight ditches on both sides. There's earth on the sides and grass along the middle of the top of the road, and ruts between the two sides of the grass.

And here we were, being fired at by a small force consisting of two machine guns. We've got about 20 rifles and four BARs. We've got some pistols, but they're not much good at a distance.

Our men have bayonets, but we're not ready to use them yet. It wouldn't happen until you get into close combat, if you've got time. We also have our light machine guns and 60-millimeter mortars. What are you going to do now to drive these men away and keep them from impeding

your forward progress so that you can get to your objective at the assigned time?

You have to do an estimate of the situation, which, if you're lucky, you had studied when you were a kid in school, and you know what to do. You lay out, in horizontal rows, your options and you lay out, in the vertical columns, the enemy's options.

Frankly, I don't think the enemy had much in the way of options, at least not the unit that was opposing us. He could stay exactly where he was and keep firing until he's destroyed or he could instruct the second one to withdraw while the first one is still firing. In other words, that would be a holding action on their part, because they knew they had been met by a superior force, although, if they had heavy machine guns or the equivalent, they might not necessarily have been an inferior force. Or, they might decide, when this one machine gun withdraws, maybe he could pull a flanking movement of some sort under cover and come out on our left flank, because that's where the cover was in the woods, on the left side. And then, while we're busy firing at the guy who's in front of us and slightly to our right, the flanker suddenly starts firing at us from our left and slightly to our rear. We have to take these three things into account.

Similarly, we have things to decide. What are we going to do? I might get my weapons platoon into position somewhere. I might bring the mortars up to where they have a clear field of fire that can come down on those machine guns. I could stay in concealment and the enemy can't do anything. On the other hand, I could direct the machine guns to get forward on the left flank without revealing their positions and set up and start firing from the left flank.

I would then have three options for us, three options for them, and I'd have nine boxes that show the interactions. I would write down, in those boxes, what was going to happen. And I would then choose my tactic that would be the best if the enemy used the tactic in that column, and the same for each column.

I would then pick as my strategy the one that gives me the maximum attack, regardless of what the enemy does, in each of the different positions. In other words, if he's fighting in the way it says in column one, I have to figure out which of these three strategies is going to be best for me.

Then I look at those three maximums and I choose the minimum of those three maximums. If I do that, I know I will be doing that well or better. So that's the strategy I choose.

The min-max is the conservative solution, and if the enemy does the same, he chooses a certain max-min for him. If I choose the max-min and he chooses the min-max, it's a zero-sum game.

That's what we were taught in school and it's exactly the way von Neumann and Morgenstern saw these problems of zero-sum two-person games with perfect information.

Bob Sheldon: Did you take ROTC all four years of college?

Joe Engel: Yes, I did. At the conclusion of college, I had not only finished my four years of ROTC, I had finished my four years of everything else and I had been given a good education in all of these fields.

I didn't do much singing in those days, but I took two years of public speaking. This was a required course for people who were taking a major in secondary school education or a minor in it. I passed it, so that I was a good public speaker, at least in theory.

Also, in ROTC, one of the things we had to do was learn how to give oral commands that could be heard in conditions of combat and confusion. One of the things they used to do to us was to line us up across the width of a football field. The field is 100 yards long and approximately 50 yards wide. They would line up about a half-dozen 12-man squads. They changed our weapons from Springfield rifles to Garand rifles, the primary rifle that our troops used in WW II, so we were getting up-to-date training.

We were squad leaders and had to drill our squads. In the first two years, we were enlisted men, and we had simulated ranks of corporal. We had to drill them in school of the soldier. School of the soldier is the term for how you march in formation and follow all of the orders. You also had to know how to carry and move your weapon. You had to know how to do ''forward march'' and you had to know how to say ''forward'' slightly drawn out, because that's the preparatory command, and ''march'' is the command of execution.

They had six squads lined up and six of us as officers on the other side of the field, each of us 50 yards away from our troops, who had to hear and recognize us as the ones who were giving the command—not the officer next to us.

We had to be sufficiently distinctive that they could hear our voices, and we had to be able to get them to do those maneuvers promptly. It was right shoulder arms, left shoulder arms, present arms, fix bayonets. You had to give them the commands and they had to obey them without question and rapidly.

In the second year, we studied history as well as tactics and strategy, using models from history, including the Civil War, where there wasn't yet much in the way of guerilla warfare.

These movements of units into fixed combat, which is the way the Romans fought and the Greeks fought and the British fought in all of these wars, was changing very slowly. That in itself was an education, seeing how the face of warfare was changing rapidly in WW I to begin with, and in WW II even more so.

In ROTC, we mimicked the military promotions. Generally, promotions were associated with your years in ROTC, so that most people became corporals their second year, and we all became commissioned officers in our third year and got a promotion in our fourth year. If your service was exceptional, you might become a sergeant in the second year, and you might rise to first lieutenant or captain by the time the four years were up.

These promotions meant higher responsibility, so I was increasing my managerial experience. I remember being a corporal, and a second and a first lieutenant, with increased responsibilities and larger numbers of people to lead and manage. The same thing happened when WW II started, and I was called to active duty.

I started out as a second lieutenant in the Infantry, and my first duty station was Fort Benning, Georgia, where they put me through the same kind of 90-day course that they used for people who had no previous training. With us, they called it a refresher course. I cannot say they taught me anything that I hadn't already been exposed to, but I got more practice doing those things. Thereafter, I was put into an active Army unit.

I was assigned to the 95th Infantry Division, which was part of the Third Army Corps. At first, I was an ordinary rifle platoon leader. I had approximately three dozen men under my command; three squads of twelve men each, plus a platoon sergeant, and a couple of section sergeants. I had to supervise and manage this platoon. I had to lecture to these guys and teach them left from right. I had to teach them school of the soldier, which were the elementary movements for close-order drill.

Later in the war I flew as a B-29 navigator, flying 39 missions over the Pacific and Japan.

Bob Sheldon: WW II started during your senior year in college?

Joe Engel: We had not yet entered WW II while I was in my senior year, but the war was definitely going on and the news was bad. Paris fell in 1940. In my junior and senior years, things were looking very bad for the Allies. Roosevelt had initiated Lend-Lease by that time and we had been supporting the Allied war effort which, in the transatlantic part of the battle, consisted primarily of providing air cover for destroyers and for lending the British our destroyers to protect their convoys that were carrying the supplies that they needed.

As I learned later when I went to work for the Operations Evaluation Group (OEG), the OEG did a lot of very important work in that regard.

Bob Sheldon: How did you get recruited to OEG?

Joe Engel: The personnel manager for OEG gave me a spiel about what went on in the Navy, and it sounded fascinating. I accepted the OEG offer in 1949 because I was married at the time and the salary they were offering me was the best of all the offers, including prestigious educational institutions like Yale. This was my first real professional job, outside of work I had done in graduate school at the University of Wisconsin as a teaching assistant.

Bob Sheldon: Can you elaborate about OEG's role in the protection of Atlantic shipping?

Joe Engel: What I can tell you about our protection of shipping during WW II in the Atlantic is what I learned in our OEG indoctrination and in conversations with people in OEG, and what subsequently we used to do comparable work in the postwar years.

When we did antisubmarine work, we worked on how destroyers would protect and escort convoys crossing the Atlantic to bring needed supplies, troops, and logistics. Also, we worked on aircraft in support of these convoys and our own submarines in support of them.

We had to combine lots of different kinds of operations. When you screen a convoy with destroyers, you have to know how fast the convoy is going, whether it's going to be making any turns along its route, how big the convoy is, and so on. You have to know the capabilities of your own equipment, which would be primarily those available in the earlier days of WW II, some kind of sonar, which could listen for submarines.

Based on that, you knew how far from your destroyer you could detect an approaching submarine or surface ship. You have to know how to space your destroyers so that you can see far enough to your flanks and to the sides of the convoy itself that you can detect approaching enemy craft in time to prevent them from doing harm to your convoy. This means you have to have a curved screen of ships in the front and to some distance around the flanks. One can work out speed vectors to figure out what those angles ought to be and you have to get your spacing so that you don't have gaps in the line.

One of the concepts that they came up with early in WW II was the concept of a nonuniform sweep width, the idea being that when you use something like sonar, it may be pretty strong directly in front of your ship along the direction that you're going, but less strong to the sides.

When you know how good your sonar is, you can have a pretty good feeling, based on data, experiments, and prior experience. You will have a good estimate of what the probability is that an enemy target of some particular kind will be detected if it's going to come in, let's say, two nautical miles to the left or right of the actual track of the particular ship that is doing this sonar work.

If you take that probability and you integrate the probability curve along the line perpendicular to your direction, you get a result that tells you the average distance at which you will detect your convoy. That tells you if you had a broom that detected everything at that width and you had the spacing that you do, the proportion of ships that you get would be the sweep width divided by the distance between two adjacent ships. So that's the concept of a sweep width.

Bob Sheldon: Who taught you about antisubmarine warfare (ASW)?

Joe Engel: We learned this initially in indoctrination courses, which were held for newcomers in the OEG, and this was a recapitulation of what these guys had learned on the job during WW II. They had some very bright, sophisticated people doing this work.

Bob Sheldon: Do you remember the names of any of those folks?

Joe Engel: The first peacetime director of the OEG was Jacinto (Jay) Steinhardt, and he had been a member of the wartime ASWORG, Antisubmarine Warfare Operations Research Group, which is what the Navy called that group. He was personally involved in this kind of work, especially in the portion of the Atlantic closest to South America, because there were convoys going to and from South America.

He saw to it that we were taught knowledge based on the accumulated experience developed during WW II, and in which we got initial guidance and ideas from the British, who had been doing these things before we started to do them.

Bob Sheldon: What other kinds of training did you get at OEG?

Joe Engel: We were trained in air warfare and surface warfare. We were taught not only how to screen convoys in protecting convoys, but we were taught how to look for a lost target—a target that had been detected, maybe damaged and was sinking, or was just plain lost. You were not in contact with them and you had to conduct some sort of search pattern to find them. You did this with whatever you were using to search—surface ships, helicopters, aircraft, could be anything.

Bob Sheldon: Did Koopman teach that?

Joe Engel: Bernie Koopman taught search.

Bob Sheldon: Were you in a classroom with others?

Joe Engel: Yes. Depending on how big the newcomers in that particular year were, there might have been anywhere from 10 to 20.

For example, when searching with aircraft, the primary detection instrument is radar, and

radar was similar to sonar in the sense that it enables you to get a good picture of where things are at a distance.

It's using light waves rather than sound waves, and light waves travel a lot faster and they travel in the medium of the atmosphere, rather than the water. The refraction problems are not as great, but certainly, they're different. What you are seeing with your radar is some solid object in the atmosphere that is on the surface or above the surface of the ocean.

With good radar, you actually get a good map of where you're flying, in addition to little bright blips on the screen. Say you are over open water, so there is nothing else bouncing back; then you can detect a flying object like an aircraft, or you can detect a little rock sticking up out of the ocean, which might be dense enough to give you a reflection on your radar, and which you might confuse with a reflection from a ship.

You have to worry about errors of the first and second kind. An error of the first kind is a failure to detect a target when it's present. An error of the second kind is a detection of something that is not a target or that may not even have been present.

There sometimes were instances of anomalous propagation, which is very similar to what happens when you see a mirage in the desert. Refraction brings into range something that might have been actually below the horizon, because of the curvature of the light wave.

When you look at a mirage and you see a beautiful city floating in the sky above you, which could be a real city over the horizon brought to you through anomalous propagation. You have to be sufficiently well-trained to recognize these symptoms.

We also learned a lot of theoretical things. Mentioning Professor Bernard Koopman reminded me of some of these. He was very well-versed in the general mathematics of predictive situations and specifically about detection of small targets using radar or sonar.

At one point, we had what was known as a double blip hypothesis. You couldn't be sure that you had actually detected a target unless you saw the blip at the same place on two successive rotations of the radar beam around full circumference and returned to that spot.

There was also an ''operator factor.'' You could compare what you actually observe under operational conditions with the degradation between the ideal performance and the actual performance under controlled conditions. This operator factor was a probability that the operator would detect a target if the target was presented.

Bob Sheldon: The classes you took at OEG from Professor Koopman and others, did they give you homework and exams?

Joe Engel: We did do a certain amount of homework. I don't think we got exams. It wasn't a question of having to pass a course, it was a question of really learning and we all wanted to learn.

There had been people who did not survive the first year at OEG. I was told by one of my colleagues that I did not do well in my first year (I was told this 10 or 15 years later, by the way). I should have known this because I didn't get a raise that year. I was sufficiently new. I was hired in 1949, which made me second generation at OEG. I was definitely junior to the wartime group and didn't have the same viewpoints they did. At the time, I was fat, dumb, and happy, and I did my job and I wasn't overly upset that I didn't get a raise the first year. I would have liked one, but I didn't even know whether I was supposed to expect to get one.

Bob Sheldon: After you finished your training, what was the first project?

Joe Engel: I was working initially on air warfare. I was given some data to analyze that had been collected by my team leader, who was off on one of these exercises at which data was being collected.

Bob Sheldon: Who was the team leader?

Joe Engel: His name was Douglas Brooks. He gave me some data and I enjoyed working on it. I was able to figure out the probability of detection as a function of distance. I was able to determine the detection curve, and I did something very interesting with that data over and above getting the detection curve. The data were not quite as extensive as a classical statistician might like. We had maybe 50 trials in all and they covered more than one set of circumstances.

When you study statistics in school, you learn about confidence limits. This enables you

to draw one curve above the mean curve, which shows a higher probability at a given range, and another curve below, which shows a lower probability at that range.

You can then state that there is a 95 percent chance that the true mean will fall between these two curves. That enables you to make an optimistic prediction against a pessimistic condition. We put out an OEG study that analyzed a particular radar and an aircraft detecting a specific kind of target, and it had these confidence limits on it.

For me, it was a first. I had never applied these confidence limits to anything in the real world before, but I recognized that it was appropriate to do it here, because the data were less than copious. That's often the case, because we always have limited resources to work with.

I've always been interested in learning practical limitations on the validity of our findings, and this is a very important tenet of OR. To be honest and true to your client, you have to let your client know any reservations you have, and you have to let him know why you have these reservations so that he can understand just what the circumstances are that led you to the conclusions that you drew and the recommendations that you made.

He may have sufficient other experience that enables him to say, ''Given those circumstances, I will accept your recommendations based on what else I know; or I won't.'' He has to have that prerogative, and it's up to you to give him the best possible information you can about any reservations you may have concerning your findings.

Bob Sheldon: After looking at air detection, what was your next major project?

Joe Engel: I did one or two things while I was on that air defense team, then I went down to Key West to work with VX-1, the Navy's test squadron. In between, I also worked as a liaison with someone who did subcontracting for us. I went to the University of Michigan, where one of our people was stationed, and he and a team did work for OEG. We had to go there from time to time to review the work and bring them a new assignment that we wanted worked on, and we needed someone there to make sure that they got exactly what it was that we had in mind. I went there maybe once a month or every other month and just spent a few days on any given occasion.

At the same time, we did air ASW work, and I was attached to a squadron known as ZX-11, based in Lakehurst, New Jersey. What did the Z stand for? Zeppelin. This was the Navy blimp squadron attached to the OPDEVFOR, which stands for Operational Development Force that later changed to Operational Test and Evaluation Force (OPTEVFOR), to test the equipment that might be put to use by blimps.

If a helicopter could dunk a sonar in the water, so could a blimp. At least, that was the reasoning. Blimps could also drop mines in the water and maybe with great accuracy, because they didn't have a terrific forward motion and more silently, because they could come drifting in without their engines being on. If they came in by night with their lights off and their propellers not going, they might be very hard for an enemy to detect. They could drop their mines and leave surreptitiously and the enemy wouldn't even know they'd been there.

This was very interesting for me, because of the operational and analytical problems associated with it. There were some big ones that were, by then, real zeppelins; the difference between a blimp and a zeppelin is the blimp did not have a rigid framework. It was more like a balloon. The zeppelin had a rigid framework, like the Hindenburg. The Hindenburg was the one that blew up.

Our zeppelins had that same possibility, but by then we had shifted from hydrogen, which was the very explosive fuel that blew up the Hindenburg, to helium, which is an inert gas and you don't have the danger of explosion. I had the fun of traveling around in blimps and balloons.

Bob Sheldon: After you took the class, did you go out on ships to verify what was going on?

Joe Engel: I did get involved in this kind of ASW, but with a slight difference, in the sense that I seldom was involved directly with fleet units that were doing antisubmarine work, although occasionally I was.

In 1952, I was attached to an antisubmarine organization in Key West called VX-1, VX meaning ''aircraft experimental'' and ''1'' was the number of the squadron. The man that I relieved

was Howard Kreiner. It was an Air Development Squadron for antisubmarine purposes. They were stationed in Boca Chica, Florida, which is about three miles up the causeway toward Miami from Key West. They were functioning as a part of OPDEVFOR.

OPDEVFOR has counterparts in the other military services. A lot of these people knew each other. Ellis Johnson, for example, who was the director of the Operations Research Organization of the US Army, knew his counterparts, Leroy Brothers in the Air Force's organization and Jay Steinhardt in the Navy's organization. They were friends and professional colleagues. During WW II, Ellis Johnson ran the US mine warfare program that designed the way these naval mining programs would be taken care of.

When I flew as a navigator in WW II, I took part in the campaign that Ellis Johnson had waged, a campaign called Operation Starvation. Its objective was to blockade the Japanese, so they couldn't get sufficient supplies coming into Japan to maintain their armed forces, and so they could not send sufficient supplies out to their troops who were waging combat on other fronts outside of the Japanese islands proper.

It turned out that my experience in the military was germane to what was going on in the Navy's mine warfare efforts that were current and projected for the future. And it was relevant to my ASW test work.

The purpose of OPDEVFOR is to test and evaluate systems of weapons or detection systems that are being used for some specific purpose, and which the Navy is considering ordering in sufficiently large quantities that the fleet can use it. This is clearly a very important function, and unless you have ways of checking performance against the specifications for which the equipment is being built, you have no really good way of determining which, of perhaps several different systems, you want to acquire.

When a manufacturer prepares a prototype prior to the contract to actually acquire them in mass quantities, he does his best to produce something that meets the specifications that they're bidding on. He has his best scientists and experienced technicians working on developing the equipment and then actually using it. They are skilled professionals, one hopes, and they should know a lot about getting the best they can out of the equipment.

It might well be the case that if the equipment is submitted to whatever trials the Navy wanted, and these professional personnel used it under fairly idealized conditions, the equipment might well perform according to specifications. On the other hand, if this equipment is designed to be used at sea, and it is to be used by seamen and officers who may not be quite as expert as the scientists and engineers who developed the equipment in the first place, and the technicians who helped put it together who were very well versed in all the ins and outs of how to use this equipment, and they were using it under ideal conditions, the professionals might do better than a typical Naval team, which didn't have the opportunity to get quite this extensive training.

You need to observe the equipment under operational conditions being used by people who would be using it in combat when it's trying to do its job in the real world. That's what we did at Key West in VX-1.

They were an Air Development Squadron for undersea warfare. At the time that I was there, they were evaluating several aircraft and helicopter-mounted equipment for this purpose. They had patrol aircraft, which had various kinds of radars that could look for surface shipping, submarines on the surface, or even their periscopes.

Howard told me what was going on, some of which I would have to take over because his tour of duty was up. One of the things had to do with the evaluation of active sonars; the vessel that was going to carry them was a helicopter. The helicopter was going to take a sonar dome, known as a transponder, which sent out sound and received echoes of sound back. We would go out to the location, lower the sonar dome into the water while hovering over the water, with the idea being that the sound the helicopter made in the atmosphere would not travel through the water. So an enemy submarine that was listening for other ships or looking for other aircraft would not be very likely to spot the helicopter low over the surface of the sea when they were looking at a couple of thousand feet up. Consequently, we could look for submarines without ourselves being discovered. They

would turn on the sonar, send out their pings of sound, and hope that they bounce off something that might be a target—although it might just be rocks on the bottom. If there's a little echo that has come back, the sound reflects back to the dome, which also has a receiver in it, and that sound is received and it goes through the electronic circuitry and shows as a blip on a screen.

Howard told me I would have to learn how to analyze and develop tactics for such a thing. The tactics for dipping sonar are going to be very different from the tactics that are conducted by a ship or an aircraft that moves continuously. He said that what you're going to have to do is somehow represent what happens when the dome is down here; and then you pick it up and you have to carry it to another location and lower it there. Completely different from anything that we've ever done before, because these are just spot looks. I thought that was going to be dull, however, when I got into it and I started drawing these circles and got into the swing of things, I discovered there were lots of fascinating problems in it. You had to analyze what amounted to continuous motion. You had to be able to pick the sonar dome up, then move far enough along that if you're screening a convoy you don't get overtaken by the convoy. You don't get too far ahead and you lower the dome and you stay motionless there while the convoy is speeding up. If you get too close to it, you're not doing your job because you're not finding the enemy soon enough. There are lots of side issues in this. I had something that was very repetitive, but required a lot of thought. It was fascinating for me, and I would invent some tactics. I would draw diagrams on a chart, a simulated place where this was going on, under certain conditions, and I would consider how this particular circle would act during the period when the dome was down. Then they'd pick it up—they'd accelerate from zero speed forward to carry it ahead of where it was before, fast enough to exceed the speed of the convoy, stop, move to a hover position, go down, listen again to the dome, pick up, discover perhaps that the convoy is turning so they've got to turn also. You have to work out all these details, and I developed some visual aids in which I had a circle representing the region within which there were three zones.

From that, if you conduct enough experiments, you learn lots of things about the sonar. You learn the distribution of distances, both forward and on the side, that a target of a given type will be detected.

They would run experiments in which submarines would try to make a passage in a given direction and they would come from far enough out that they would initially be out of range, so that it would be necessary for the sonar to detect something. Then they could measure the distance and direction at which that detection took place and they could track it and they could get data under operational conditions.

Bob Sheldon: The objects they were trying to detect, did they use Red subs or our own submarines for the test?

Joe Engel: We were working on a variety of subs. We may have had some old German submarines that we had captured or rehabilitated after WW II and used those. We had other kinds of submarines of our own. We had standard, conventional submarines. We had GUPPY (Greater Underwater Propulsion Power Program) submarines, which is what we called the snorkeling submarines. Eventually, we had nuclear submarines.

The ships of various sizes and capabilities and speeds and the information that you get from these kinds of ships are comparable to ships of that kind, regardless of who has them. Unless one ship in the Navy of one country might have some equipment that quiets the ship, this is one of the things one has to worry about and develop ways of accommodating it. But generally speaking, the spectrum of kinds of submersible targets that we could provide were quite comparable to what some putative, hostile type might put up against us. So it was definitely relevant.

Bob Sheldon: Did you test under various environmental conditions, e.g., the currents and the ambient temperatures, to wash that out as a factor?

Joe Engel: Yes, the currents and the ambient temperatures were always very important because sound waves are strongly refracted within the water. And if the temperature differs near the surface from what it is below, you can get tremendous curvature of an upward or downward nature, which will severely affect the ability to detect targets beyond certain ranges.

There is a phenomenon in large bodies of deep water known as a thermocline. The thermocline is a boundary between layers of hot and cold water, the cold being below. So on a warm summer day, there will be a sharp difference between the warm surface water and the cooler water below.

That may easily be down to a depth of 100 meters as the divide. The sound wave tends to be bent upward, so if you are sending your sound waves out and they hit the thermocline and they bend upward, that limits their range and they won't be able to get deep targets out as far as they would if the conditions were more favorable. They definitely worked under a variety of conditions so they could take these factors into account.

Bob Sheldon: Who did you report that data to?

Joe Engel: We reported that to the commanding officer of VX-1 and his staff. There were several engineers on his staff who were responsible for conducting these experiments and data collection trials. We worked very closely with them, observing the actual trials, discussing what their plans were, and giving them designs to test a particular piece of equipment.

When they conducted these trials, we went along on some of the naval units that were involved in the trials. I made many flights in the helicopters that were dipping their sonars down in the water.

I found the experience of flying in the helicopter while they were doing these evaluations fascinating, because it was an early Sikorsky helicopter. In the very warm climate of Key West and off the shore area there in Southern Florida and the nearby Gulf of Mexico, the Sikorsky was underpowered and did not have much lift. They could barely carry a pilot, a copilot, and a sonar dome. Under normal operational conditions, the copilot not only would tell the pilot when it was time for them to make a change in position so the pilot could rest, the copilot could also function as the observer who monitored the sonar and could see whether or not it was detecting any targets. It could do the work, but if it carried somebody extra like an observer (me), then it was underpowered. To overcome that, they had to strip the helicopter of most of its skin to lighten it, except for the floorboards where the pilot was sitting and the windshield to protect him from wind blowing into his eyes. With that in mind, we had a nice breezy flight, because we were not protected on the sides. There was a breeze because when we were picking up the dome and going from one spot to another, we flew around 80 knots. That's pretty fast.

There were just a bunch of essential struts and framework, metal pipes linked together to hold the helicopter to its proper shape so that there was a little bit of a fuselage in the back and the tail rotor would be in the right place and this was all rigidly mounted so it would function correctly. With that, they had enough power that they were able to do their job.

The weather was glorious. We were flying somewhere between 20 and 50 feet over the surface, and we could look down into the lovely green waters of the Gulf of Mexico or the Atlantic, whichever side we were on, usually it was the Atlantic. We could see the marine life and we could see the coral. We could see the dolphins, the squid, the manta rays, and the barracudas. It was gorgeous, and it was as if you are in a low slung convertible and the breezes were blowing and you could see all this beauty beneath you. It was a wonderful experience! I wouldn't have missed that for the world.

Bob Sheldon: Do you recall in your experiments any results that surprised your customers?

Joe Engel: We didn't get surprised as much as informed because we only knew that the sonar was supposed to have certain detection capabilities, and it was our job to find out if it did. They never came up with more than we expected. That would have been a surprise. But they often came up with less, and that's what we were expecting. The fact that they came up with less did not mean that the equipment was no damn good, or that the vendor was trying to cheat the government.

It meant that we were using it under operational conditions in which we knew, from all the prior experience, which things didn't work as well in the heat of combat with your normal line personnel as opposed to the technical experts on shore who had built and developed and fine-tuned this stuff.

Bob Sheldon: When you worked in Florida, did you live down there?

Joe Engel: Yes. I was able to bring my family and we had housing on a naval base. The place was called Sigsbee Park by the officials who allowed us to rent a nice little one-story bungalow with two bedrooms, a living room, dining room, and kitchen. But we didn't call it Sigsbee Park; we called it Dredgers Key, because this housing development of around 100 homes was built around a huge circle. The land this housing was built on was called marl, a mud which contains clays and silt. Marl had been dredged up from the bottom of the Gulf of Mexico, which we were adjacent to on this side of the Key. They just tamped down in shallow water to build it up to get more land. It was on this land, at least partly dredged up land, that our homes were built. That was a lovely place. When we were on field assignments that were overseas for sufficient duration, and if peacetime conditions were prevailing at the time, we were allowed to bring our family with us.

This was in 1952. I had my wife, to whom I had been married nine years, and we had two small children. We didn't have our first child until I was in graduate school after the war. In the last semester of my third year, we had our first child, our daughter. We took her with us to Key West, along with my second child, my son. My daughter was two years old and my son was about six months old. The conditions were ideal for raising children. The weather was beautiful, not too hot, not too cold, because there was always a sea breeze.

My wife was there full-time to look after our children and she loved it. We had congenial neighbors, most of whom were naval officers who were attached to various units operating in the area. We were totally accepted by the naval society in the region.

We enjoyed it very much and it was a very smart thing to do, because people can work better when they know that their families are well taken care of and preferably, when their families are around and don't get in the way.

Bob Sheldon: You wrote an article in the 1950s on fitting Lanchester Equations ("A Verification of Lanchester's Law," *Journal of the Operations Research Society of America*, Vol. 2, No. 2 [May 1954], pp.163–171). Tell me about that.

Joe Engel: When I did this work, I was moderately junior in OEG, and I still had a vivid recollection of the various things we were taught during the indoctrination course that all the beginners had to go through. Part of that course covered the Lanchester Equations, which came in three varieties; ancient warfare, modern warfare, and guerrilla warfare. Naturally, we are in the age of modern warfare, because we have weapons that can inflict casualties at a distance, whereas in ancient warfare it was pretty much my sword against your shield, and your mace against my head, and your dagger in my stomach. Strictly hand-to-hand, nothing much at a distance. Later, that evolved into throwing spears and arrows, which could be construed as an early example of modern warfare because you could inflict casualties at a distance.

When I became interested in this, I was aware of the history of the development of Lanchester Equations, which came out in about 1916. Frederick Lanchester, had been an automotive engineer at that early stage of the game. He proclaimed himself also an aeronautical engineer, even before there were any motorized aircraft to speak of. He buttressed his arguments, which were primarily theoretical, along the lines we have just discussed, by reference to some ancient historical battles like the battle where Nelson defeated the French at Trafalgar. Nelson did it by crossing the T, which meant the British sailed their ships right through the middle of the French ships, and they were able to fire on both halves of the French ships as they penetrated, before and after. Consequently they were hitting more troops than the French were. The French were only firing at a very narrow target, and they only had short use of it.

During WW II it might be presumed that the Lanchester Equations were brought into some situations when the context was appropriate. Nevertheless, I don't know that they were used very much. Things tended to be much more tactically based on the immediate characteristics of the weapons and other systems being employed in a particular engagement. Nothing much generalized beyond that.

The time came when I had a little experience. I had been out to the field and I'd come back. I'd started to take on some management responsibilities. The associate director, Martin Ernst, obtained from classified naval sources the statistics on the casualties inflicted during the Battle

of Iwo Jima. He also had the statistics as to how many troops were put ashore by the US forces during each day of the battle. And he had knowledge of the size of the Japanese force at the time of the beginning of the engagement and at the end of the engagement. He had the daily casualty figures on the American side. This was almost all the data that we needed to make sense of the battle. If we had the casualty figures on a daily rate of casualties for the Japanese, then we would have a complete set of data. He also had data on where the front lines were. And he had a planimeter, a measuring instrument used to determine the area of an arbitrary two-dimensional shape. He could measure the length of the front line, and he could measure the area that was occupied by the US forces, and the area that was occupied by the Japanese forces. The Japanese were dug in; they had all sorts of caves and ditches and redoubts in the portion of the island of Iwo Jima that they still held. We had what was left of it, which was fairly bare and exposed to the forces that could fire at us from higher altitudes.

He was trying to make sense of this and getting nowhere, and he got called away on some more urgent project. Then I was thrown this data, literally into my lap. "Here Joe, see if you can make some sense out of this." I was delighted, because almost immediately, the thought came to me that we might be able to look at it with Lanchester Equations to see whether they applied. I didn't have quite enough information. I lacked the information about Japanese casualties, except that they went from roughly 22,000 to almost zero over a 45-day period, but I didn't know how it got there. I just knew that they hadn't been reinforced during this period, and they all died off at the end of the battle. They did not surrender. On the other hand, we had all of this data about the actual land mass that they were occupying, and that we were occupying. I decided that I was going to completely ignore the area considerations and just think of it as two forces facing one another, each of which could fire at everybody in the other force. In a certain sense, that was true. We could send our artillery or mortars or long-range weapons to any point on the island, if we chose to. They, with more or less comparable weapons, could certainly fire everyplace on the beach.

That's the kind of situation that Lanchester's modern warfare equations were supposed to be relevant to. I took that data, and I started trying to analyze it. To come up with a specific set of equations, I had to use the data that I did have and try to get a casualty-producing rate that the average weapons on the Japanese side could multiply by the number of troops that they had at a given time, to determine the rate at which they would produce casualties on the American side. That depended on the rate at which they produced casualties, and how many troops we had. Similarly, the other way. So there were these two coefficients that were needed. I figured out a way of estimating the casualty-producing rate of the Japanese from the data that I had, but I didn't have the rate at which we could produce casualties on the Japanese, because I didn't know the rate at which we could produce casualties. However, if you set up the right kind of differential equation, and you figure that the rates that you've got were assumed to result from constant casualty rates, you could fit the data on the one side into an equation, and then choose two casualty rates, which gave you as good as possible a fit to the actual data that you had on the American side. That meant you had essentially 45 points to play with, namely the casualties produced on the first, the second and so on, through the 45th day when the battle ended. That was pretty good under the circumstances, even though we were dealing with large numbers of forces; around 22,000 on the Japanese side and the 50,000 that we finally put ashore altogether on the US side.

With that large a sample, we could assume that somehow we were working out pretty close to the average, by the law of large numbers. With that approach, I was able to derive these two casualty-producing rates that we needed to bring these equations to life. The nature of the process would produce a very high coefficient of correlation, when you compared what the equations gave you to the actual data. You could also then look at the results that you got from the equations that would fit the Japanese forces. You got results which would be consistent with both sides of the equation, if the results were correct, and would give you a very high coefficient of correlation with each side, if you examined complete data on both sides. When I made this

calculation, I didn't do it with differential equations. I did it with difference equations, which in the limit become the same as differential equations. So it didn't really matter, because one-forty-fifth of an interval is a pretty small piece of it, so it's going to be a good approximation. The coefficient of correlation of the theoretical results with the actual results on the US side was around 0.99. That, to me, sounded like a very good fit.

When I made a graph that showed the number of survivors at the end of each day of combat, it had a fairly steep rise on the first day to something like 5,000 and then a drop on the second day to about 3,000. Casualties were very heavy, and the surf was roiled up and we weren't able to put any replacement troops ashore. This had to be cranked into the equations, but that was fairly routine if you knew what the replacement rates were. Then it went up to a second peak on the third day, dropped a little bit on the fourth day, then went up to its final peak, very near two or three thousand less than the total number of troops that we had actually put ashore, on the fifth day. Thereafter, the number of troops that the Americans had declined gradually, slowing down in its decline, in what amounted to a drawdown curve, until at the end of the 45th day, we had somewhere between 20 and 30 thousand survivors still effective on the field, whereas the Japanese had been totally wiped out. They did not surrender when their forces got below what they considered to be a viable level for them. The viable level for continuing to fight was zero, and that was the end; and only 200 of the 22,000 survived and were captured.

I was very pleased with our results. I showed them to my immediate supervisor, Joe Neuendorfer, who was active in the Operations Research Society of America (ORSA) in those early days. He was a WW II veteran of the naval predecessor of OEG, so he had seen OR in the days when it was first being born. When he got this memo of mine, he saw its significance immediately, and he insisted that we rewrite it to make it into a study. We arranged to make a presentation to a large audience of naval officers that were involved in the various kinds of analyses that we were supposed to be helping the Navy with.

There was an audience of between two and four hundred people, and it was well received. Shortly after that, I was walking through the corridor in the Pentagon where we had our offices, and I passed a newsstand, and stopped to look at what they had on the newsstand. Lo and behold there was a paperback that said, *The Battle of Iwo Jima*. I took a look, flipped back to the index, and found my casualty data. It had been completely declassified, and my numbers matched exactly what I had for the US troops. I bought a copy, showed it to my supervisor and the Director, and we all agreed that I should make an unclassified publication out of this. I wrote it up as a manuscript to be submitted to *Operations Research*. We submitted it to the Navy for clearance, because it had originally been based on classified data, and they gave it their OK. I submitted it for publication and it was accepted, and it appeared in 1954. That was my first published article on any phase of OR, outside of what I had done professionally within the confines of classified areas of OEG and the Department of the Navy. I was very happy about that. In the first year, it received about 400 citations. It was quoted in that many other documents.

Another thing of interest about the year 1954 is that was the first year the Lanchester prize was awarded by ORSA, and Leslie Edie won the first Lanchester prize. His article was on the effects of platooning in highway traffic. (*Editor's note: Platooning is linking vehicles in a high-efficiency group like a train without the train tracks.*) If you platooned the cars by traffic lights, as they go through the Holland Tunnel, the cars all travel at close to the maximum rate of speed. No traffic jam, so they get through faster, and they minimize disturbances on the highway. I was thinking to myself, wouldn't it have been poetic, if the first Lanchester prize had been awarded for validating the Lanchester equations.

Bob Sheldon: Did your Lanchester analysis impact any operational analyses for current day in the 1950s Navy operations?

Joe Engel: I would say yes, both directly and indirectly. The direct way was in a planning situation. If you had a good estimate of what kind of troops you were going against in some future battle, and you knew how many troops there were on the enemy side, and you knew what

their capabilities were both of a defensive and offensive nature, you could then take the various weapons systems that you had on your side and figure out how many troops you needed to win the battle. You would have to state what your criteria were for winning and losing, and similarly for the other side, but when you make all of these assumptions you can make the calculations and you can figure out what size force you need. Obviously, the bigger force advantage you have, the more your chances of winning are. That's the thing with the Lanchester square law, you strike first with the most, and that's a very good tactic in general. If you have enough, you'll win. This was a direct usage that was used in planning many attacks when things were really under control, and you could make good estimates.

However, I think even more important than that was this particular paper, which validated a theoretical approach to the calculation of casualties on both sides of the battle. It was very encouraging to analysts who were struggling with the same kind of problem. It encouraged them not only to use the Lanchester equations if they wanted to, but to invent their own models, or to set up simulations in which they could make believe they're fighting a war, and collect the casualty data and see what happens. As a result, the entire field of calculating what happens in a war was greatly encouraged to go ahead and think, and get out of the box. Validate it, make sure that it seems to work, and go ahead and use it.

I was glad that I came up with that result, because I had no idea what was going to come when that data was thrown into my lap.

Bob Sheldon: What was your next project after the VX-1?

Joe Engel: I came back to the Pentagon and managed to buy my first house. We'd been living in rental housing in the Washington area.

When I returned to the Pentagon, I got a promotion. I was a team leader. I had been shifted from undersea warfare, or air defense warfare, this time to mining and mine countermeasures. At first I functioned as what was called a scientific analyst, which meant that it was my responsibility to deal with the people in the Pentagon who were on the staff, and concerned about the conduct of mine warfare and mine countermeasures. There were two staff placements that did this. One was a staff organization that dealt with mine warfare, and the other was devoted to mine countermeasures. For a while, I functioned as a scientific analyst to those organizations, and then I became the team leader, within OEG, to actually supervise the work that we did in the home office on mine warfare. One of the people on my team, who later replaced me, was Al Bottoms. I think he has retained an interest in mining, and mine warfare, since those early days. This was around 1954, shortly before I went off on my next full-fledged field assignment to the 7th Fleet in the Pacific.

I did a lot of fascinating work while I was in mine warfare, and some of it got written up in OEG publications. Some of it was presented in big seminars that naval personnel would be invited to. On one occasion, I did something that was fairly new from a theoretical point of view. There is the concept between the performance of a system when it is in equilibrium, and the performance of a system when it's in a transient state. That means it's in a situation where things haven't steadied down, e.g., when you're just starting to lay a minefield, and you can only lay so many mines a day.

If you started with an empty harbor and you only sent one aircraft and it only dropped one mine, that one mine is not going to close that harbor. Most of the ships wouldn't be sailing near enough to it to trigger the mine. Sometimes a ship bumps against one of the spikes that stick out of an anchored mine floating up from the bottom. Sometimes a ship creates a pressure wave or a magnetic wave in the water. Sometimes there may be a change in some other sensing mechanism that the mine can take advantage of. You have to lay lots of mines, and it may take you a long time to get enough mines laid that you get the level of effectiveness that you're looking for.

To really be able to talk well about what you're doing with your mines, you should understand the mathematics of minefields in transition, and how long it takes them to get up to equilibrium.

You cannot think of the minefield as being immediately in equilibrium. That means that you have to set up what amounts to a set of partial differential equations which pertain to the whole system in which time is the primary

variable. The other variables are the shipping rates, the mine emplacement rates and the rate of occurrence of false targets. Also, there is the likelihood that when a mine blows up, it will actually destroy the target that caused it to blow up. That would be what you are hoping for. If the mine blows up and fails to destroy the target, that's an error of the first kind. If, on the other hand, the mine blows up because it received some kind of signal that didn't come from a ship, this is an error of the second kind. You have to take these into account, so you work with partial derivatives that involve all these kinds of things.

That's where you get the idea of the minefield that is increasing in strength as you lay mines, decreasing in strength as the mines are blowing up under the shipping that they're out to get, so that eventually a balance is struck. If you've laid enough mines, then the chances are that you will be blowing up a large number of ships, and you will do the job at the level that you wanted to do.

This was important enough that when it was done, we wrote it up. I also was ready to submit an article for publication in *Operations Research*, but I was beaten to the punch by another analyst who worked for the Naval Ordnance Laboratory. We knew and dealt with them, but they were not part of our organization. Unbeknownst to us, we were both working on the same problem at the same time. He came up with virtually identical equations and models. I got to read his article and it was practically the same as mine. His article was submitted for publication about a month before mine. I had already published my article within classified naval channels; I then submitted it for declassification. I had received an okay for declassification when I learned that this other guy had beaten me to the punch.

On one occasion, I was asked to take this work out to one of my colleagues in Hawaii attached to the Commander-in-Chief US Pacific Fleet (CINCPACFLT). He was there for a year or two. Some of his work involved mines, and clearly this was a subject that CINCPACFLT might well be interested in. After some routine calculations, people had a straightforward idea of how long you'd have to work to expect that your minefield was where you want it to be. You'd learn the rate at which you could replenish the field, and this would determine how often you would revisit certain targets.

Bob Sheldon: The other gentleman who beat you to publication, were his results the same as yours?

Joe Engel: Pretty much the same. The mathematics, when you are dealing with systems in transition, is much more complicated than the mathematics of equilibrium. Time is involved, and when you work with time, forward distance, lateral distance, mass of the targets, and length, it's a multivariate problem. With time involved, you have to calculate partial derivatives, the rate of change of combinations of circumstances in which perhaps only one variable is being examined. You hold all other things fixed, and you get the amount of variation in your measure of effectiveness in terms of that particular variable.

In equilibrium, nothing is changing. This is much easier to solve because you set the derivatives equal to zero. When you solve that equation, the minefield is in equilibrium; but in transition it's much harder. That's one of the things I was able to do, and I got to visit Hawaii, and I fell in love with it. I'd been there once briefly during WW II.

Bob Sheldon: How long was your tour of duty with 7th Fleet?

Joe Engel: By this time the Korean War was on, and I was originally sent for a six-month tour. I was not allowed to bring my family because it was not suitable to have them exposed to danger. I went for six months, and I relieved Ralph Beatty. He was there with his wife Doris and his children, all of whom I knew very well and whom I visited when I was ashore in their vicinity. They lived in a suburb along the coast, south of Tokyo.

I had a wonderful experience in the 7th Fleet. All my experiences with the Navy when I was in OEG were absolutely fabulous. It was truly a challenge, but it was not insurmountable. We were able to do useful and important work that hadn't been done before. Frequently, the foreign places we went to were sufficiently different to be interesting—the people we dealt with, the climate, the culture of the local region.

Bob Sheldon: What kind of projects did you tackle for 7th Fleet?

Joe Engel: One of the big projects in our overseas fleet activities was defending the fleet from air attack. The fleet always maintained operational readiness when it was out, so if something happened, they were trained up to it. They did their best to remain ready-to-go.

They set up staged exercises in which a carrier task force would be placed somewhere at sea, under some simulated combat situation, in which they were going to be attacked by hostile aircraft. The hostile aircraft would attack, and we were in the combat information center (CIC) watching the naval personnel do their jobs. This consisted of placing all the various ships in the right places and placing all the aircraft that would go out to intercept an enemy target before it got to the point where your combat air patrol (CAP) might be working. CAP was analogous to the destroyer escorts in the convoy.

You had to know about all these things, and how they worked together. You might be worried about friendly and enemy submarines, and transient merchant shipping going through the waters that might be mistaken by the enemy or by the task force for an enemy ship. We had devised a standard exercise and had a code name for it. We ran these things regularly.

We would go down to the CIC on the ship that was being manned by the commander of the task force. He and his staff had access to the CIC where all the data pertaining to this operation was gathered. It was collected by naval personnel, probably clerks with a rating of yeoman. They knew how to listen to what was coming in on the intercom, see what was to be seen on the screens (e.g., targets coming in trying to hit the carrier or other ships), keep track, and record the results. At certain stages of the game, there were procedures for declaring a particular target destroyed out of action, or a combat air patrol aircraft out of action, or a destroyer out of action. We set up procedures for recording this data so that we could analyze it afterwards and figure out what had happened and make our analysis sufficiently cogent and relevant. We then could make recommendations as to where certain changes would improve their performance.

Bob Sheldon: Do you remember any significant findings?

Joe Engel: Usually we came up with results that reported that the task force survived without the carrier being hit. Sometimes there might be a hit that resulted only in peripheral damage. Various other aircraft or ships might have been destroyed or disabled, and that's usually what happened.

I can only recall one occasion in which a carrier was destroyed in an exercise. This was a fascinating incident, because it did not take place when I was in the 6th Fleet or 7th Fleet. This happened in about 1953 when I had been sent to Europe just after getting back from Key West, before I settled down and became a mine warfare team leader. We were in Wiesbaden, Germany for three months conducting a NATO (North Atlantic Treaty Organization) exercise known as Project Whiskey. Whiskey was the code word for the letter W, which stood for warfare.

We were not doing it with actual ships; we were doing it by gaming. We had people playing the role of the attacker and the role of the defender. The exercise was being conducted by these officers, all sitting in an office big enough to hold something like 40 people. We had to be in separate little units where no one could eavesdrop on what the other one was doing. It was a spacious place with good working conditions. We kept all of the appropriate charts, very similar to the ones that existed in the CIC of a carrier. To do this, we had probabilities associated with the various events that might take place. We turned to the operations analysts who had been instrumental in helping to develop these in the first place.

There were two of us from OEG there to participate in the simulation of defense of a NATO task force operating in the Mediterranean. I loved the work. This was the first time I'd ever been to Europe, and Wiesbaden was a lovely place to spend some off-duty time.

While we were there, the other guy with me was assigned to the actual naval unit, which represented either the NATO force, or the US naval air task force; I was attached to the umpires. When the activities impinged on the naval air task force, I was the one who had to assess the outcome. We did this by checking the tables of probabilities, and then conducting a random draw. If it came up right, from the point of view of the task force, nothing happened to the task force. But if it came up wrong, then they

suffered whatever damage was being tested by this device.

What it amounted to was craps shooting, and I threw the dice. So whatever number comes up determines what gets reported. If the number is bad enough, you declare a casualty.

The first time this happened, I threw the dice, and we blew the carrier out of the water. *(Laughter.)* My colleague from OEG, who was working with the task force itself, appealed the decision of the umpire, because he wanted the Navy to show up well. You have to consider these things very seriously. You certainly don't want to destroy something on paper if the chances that it would be destroyed were very low. So he appealed and the umpires accepted the appeal. As a result, I was asked to throw the dice a second time. This time, the dice came up with a low number and the task force survived. Everybody, but me, was greatly relieved. I think they might have learned something else. If the carrier had been destroyed, they would have had to do something then to prevent the enemy strike force that was trying to come in to hit Europe from the Mediterranean. It might have been a very valuable exercise.

I'm not saying it was wrong to do this; I'm saying it would have been different if we had not run it a second time. But this is the kind of thing that has to happen whenever you do training. If you really want to be realistic, you have to allow for the possibility of the thing that you don't want to happen.

Wiesbaden was wonderful. As we strolled down this boulevard, there was a café every 50 feet on both sides of the street for several blocks. Every place had its own brand of beer, and they had wonderful sandwiches at these outdoor cafes. We settled on one particular café called Blum's which had a delicious beer and wonderful sandwiches.

I also had the opportunity to take time off in the middle of the trip. I went to Cologne, and I took a trip on a Rhine riverboat. I saw the wartime devastation of Cologne, which still had by no means been repaired in 1953, only eight years after the war had ended. It was a fascinating trip, and I loved every minute of it.

Bob Sheldon: Getting back to 7th Fleet, since you were there during the Korean War, were any of your exercises affected by that?

Joe Engel: Definitely. While the war was going on, the mainland Chinese, the Communists, were busy harassing Taiwan in hopes that they'd scare the Taiwanese into capitulating. Chiang Kai-shek was tough and hadn't, as yet, thrown in the towel. The mainland Chinese were threatening to lob surface-to-surface missiles against Taiwan. There were some Taiwanese who were making their livelihood fishing from the Tachen Islands, which were closer to the mainland than Taiwan. These were Taiwanese civilians, and their job was to take big fish vessels out, catch a lot of fish, and bring them home to be used by the people in Taiwan. The mainland Chinese said they were going to occupy the Tachen Islands. When they made that announcement, the United States said, "Before you occupy those islands, you better let those people get off, or you'll hear from us." The mainland Chinese said, "We won't do anything to jeopardize their lives. We will allow them a peaceful evacuation. Nevertheless, we're going to do it."

So the 7th Fleet was ordered to steam to positions where we could observe what was going on. We were in a high state of readiness, and if fighting had broken out, we would have been right in the middle of it. Fortunately, the Communists allowed the evacuation to take place, and that was the end of that particular close call.

We did lose one OEG man during the Korean War, back in the 7th Fleet. His name was Irving "Spike" Shaknov. He was attached to one of the air units during the Korean War. He had become very close with some of the personnel that he was attached to, because he was supposed to be observing what was going on in that squadron. Somehow he was allowed to go on a mission with a friend of his. They went up on this mission, and neither the pilot nor Spike ever came back.

Bob Sheldon: Spike was an analyst at OEG?

Joe Engel: Yes. There was a room in the headquarters of OEG (now the Center for Naval Analyses, or CNA) named the Spike Shaknov auditorium, in honor of Spike.

I went from the 7th Fleet to the 6th Fleet almost immediately afterward. While I was there, the Suez Canal Crisis took place. The British and the French together captured the Suez Canal. It had been a secret operation that the US had not been informed about in advance, and we made

a lot of noise to our allies because we didn't want to see anything widespread coming about. We urged the British and the French to give the Suez Canal back to Egypt. The 6th Fleet was in a position where we knew what was going on in real time. We could observe what we felt we had to observe, and if necessary, we would have been in action. There I was, participating along with all of the other folks, in the potential danger that a war might break out.

Bob Sheldon: After 6th Fleet, where did you go?

Joe Engel: I returned to the Washington area. By this time, OEG was about to leave the Pentagon. We left about a year later, to our first offices out of the Pentagon, which were in Rosslyn, Virginia. Soon after this period, I became a Deputy Director in OEG, which meant that I was responsible for one whole side of the office. We had the air side, the surface side, and the subsurface side. The fact is, this was a position that got passed around. Some of the old timers who had served during WW II were called on to function as deputy directors. They didn't really care for it, because like all good old warhorses, they want to do what they know how to do; not tell somebody else how to do it. Jay Steinhardt had the philosophy that he didn't want anybody telling the people in our group what to do, unless they knew how to do it themselves, and unless they had real, meaningful field experience.

These guys had to do some administrative work in addition to straight analysis and going to the field. I felt pretty much the same way, but I recognized that this was perfectly legitimate and it meant that you were doing well in the organization. If I had to take a turn, I was willing to do it. I became a Deputy Director when we moved to Reston, and that changed the nature of my responsibilities.

Bob Sheldon: How many people worked for you?

Joe Engel: I had about three or four teams working for me, and each team had about five, so somewhere around 20 people. There were a few secretaries who reported to us, including the one that I had. We had access to the use of our computer section; those were the days of mainframes. You had to have professionals who knew what they were doing to run it for you.

At one time, we had to study computer languages. I remember taking a six-week course at OEG in computer programming using FORTRAN. I never put it to use, but at least I understood it well enough to discuss it with the computer section. I recall having an opportunity where I was required to make some computations, before I was a Deputy Director.

I had to calculate something to do with air warfare, primarily air-to-air combat. When each aircraft is shooting at the other one, you want to get the element of surprise, and you want to be able to fire as soon as you can, hopefully before the other guy fires at you. You need to know the physics of approach, of one moving target to another, whether the moving target is evading or not. The large part of OEG's work was figuring out detailed tactics, not strategy. Where do you dunk the sonar? What angle do you come in on when you see a target over there, so you can make a turn which will put you in an advantageous position? What if the wind is wrong and you're not exactly in the right spot?

During this period, I gave the computer section this problem, and they computed isohodes. An isohode is a line of equal penalty. The ''iso'' means equal, and the ''hode'' is the penalty. Isohodes come into play when one aircraft is trying to attack and destroy another one. The idea is to find out the ideal maneuver, which when you're traveling in a certain definite direction, and you detect the enemy at a certain distance and bearing off of that track, you know where the enemy aircraft is relative to you. You know what speed he's going and what track he's on. You're both looking for each other. You have to decide on a path that will get you to that spot, with just one turn, and get in. That's the ideal intercept.

Bob Sheldon: Transitioning to MORS. In 1967 you were elected to the MORS Board of Directors. What was your involvement with MORS before 1967?

Joe Engel: I had been active in various sections of ORSA. One of these was the Military Applications Section (MAS, later called the Military Applications Society) of ORSA. As a member of MAS, I was keenly aware that we could only discuss unclassified material. We could not get the benefit of real cross-talk in MAS or in the activities of ORSA as a whole. I was one

of the people, who before MORS came into existence, lobbied for the establishment of such a society, which would have access to classified data and would allow colleagues who had appropriate clearance, to discuss these things with one another in a fairly open intellectual forum. My early activities with MORS consisted of trying to agitate and get people to form such a society. It took about 10 years from its first formal activity to the establishment of the Society. That being the case, it made sense that I was invited to serve on the Board in 1967. I cannot recall that I did anything notable while I was on the Board, but I was very happy to be there. I helped to find speakers and to facilitate the organization of a symposium, although I never directed a symposium. I think my best work on behalf of MORS was done before it got started.

I became an active participant in MORS some years later, at which time I was further removed from classified work, because I was no longer working for OEG or CNA. For some years after I had left those organizations, I continued to work with NATO strictly as a volunteer. I was a member of APOR, Advisory Panel on Operations Research, which was part of the Science Committee of NATO. We ran some good international meetings for all the NATO countries, and we encouraged people to contribute papers of suitable classification, and we frequently published those results. That was doing, on an international level, a lot of the same kind of work that MORS had been doing strictly within the United States.

One of the things that I liked best about that activity was an apprenticeship program whereby young analysts from the less wealthy and established OR countries in NATO could come to some of the older, more well-established OR countries. They could take internship and apprenticeship and they could be hired by the organization for which they had interned, or they could improve their careers professionally and work elsewhere.

I inherited the post of chairman of APOR two years after I had joined. I inherited this position from Bernard Koopman, who was the 6th President of ORSA and had been one of my mentors in OEG. He used to come in regularly as an advisor and consultant to the group. He recommended me to the panel as his successor. He was my mentor, and I was delighted to have the benefits of his advice.

Bob Sheldon: What about your involvement in MAS in the early days?

Joe Engel: I was a bit more active in the MAS than in MORS, because I didn't want to stretch myself too thin. I was heavily involved, for quite a long period, in ORSA as well as doing my full-time work, taking care of my family, traveling around the world, and having a lot of fun in other fields as well. Most of my professional activities were within ORSA. I was always happy to learn about what was going on in MORS, and if there was something that I was able to do for a short period, I would do so.

Bob Sheldon: When you went to the INFORMS/ORSA meetings, did you sit on any nonmilitary sessions?

Joe Engel: Yes, I did. My interests were rather broad, both in subjects to which the methodology of OR could be applied, as well as to the details of the actual methodology—i.e., the applied and the pure forms of OR. Consequently, I was interested in transportation. At a distance, I learned something about the application of OR to medicine, and I remember one early OR practitioner named Ron Gue. Ron developed one of the early prototypes for keeping efficient track of what went on in a medical organization, like a hospital or a clinic.

I was also very interested in standards technology. I worked for a while as a consultant to the Underwriters Laboratory based in the Chicago area, which puts its stamp of approval on many industrial projects. One of these was the Chicago Area Transit Study, or CATS. There was one method called a gravity model, which CATS had become very fond of, and there was a big report that I had been given to analyze. Based on that report, I concluded that the methodology in the report wasn't very good, because you could get almost any results you wanted out of that particular model. It was a gravity model in the sense that the amount of traffic between two points on the network was proportional to the number of users in the two areas—shades of Lanchester Equations.

Bob Sheldon: Let's get back to your experience in managing.

Joe Engel: I started becoming a manager occasionally, even before entering OR. My responsibilities increased over time.

More specific to OR, I proceeded through the ranks in my professional society by serving on committees, and then becoming an officer of ORSA, and moving up in the ranks. I served on one or two minor committees, and then the nominating committee. The following year I became Secretary. I served three terms as Secretary, was nominated for Vice President, and then President.

In my job, I moved from being a very junior analyst where I was sent on relatively unimportant short-term field assignments, to the responsibility of observing what the Navy was doing under operational or training conditions. I had to observe the way they collected their data, and made suggestions on how they should collect their data, and what data they should collect. OEG made great contributions not only to the Navy, but to all of the Services.

OEG, starting during WW II and in the years thereafter, set up standardized ways of collecting data on exercises. One of the things that an OEG representative did when he went to the field and was assigned to one of the fleets was to analyze these data. The OEG representative to a Fleet had to be there when these exercises were conducted. He had to be down in the CIC, the nerve cell usually on board the carrier. He would observe everything that went on, and listen to the radio circuits. Sometimes, something would be played on a loud speaker so that everybody could hear it. You would look at all the vertical plots around the room and the radar screens and the sonar screens. After this long (hours or days) simulated battle in which data had been carefully or carelessly collected, it was up to the OEG rep to look over all this data, analyze it, and make a report. He would give the report to the 300 commanding officers, operations officers, and logistics supply officers who had participated in the various subordinate units. He would report on what he saw and what had happened in this exercise. What went wrong and what went right. That was quite a responsibility.

Bob Sheldon: Can you give an example of what you reported?

Joe Engel: Generally speaking, what we reported were the results of the battle. What percentage of aircraft that the carrier was using for its own defense were casualties? What was the rate at which you destroyed enemy aircraft that were trying to attack the carrier? What happened to other ships in the force? This was the kind of data that we reported.

We also reported details that were perplexing. I recall one incident during an air defense exercise, when a bogey had been reported. Bogey is slang for an enemy aircraft coming in. It was being tracked by radars and was getting about to where our CAPs were located.

We knew where these CAP were operating and were tracking them. A strange anomaly developed in which it appeared that the aircraft that was trying to attack the force started flying in circles. We lost sight completely of where the CAP intercept aircraft that had been vectored towards the enemy aircraft was. Nobody knew what had happened, because there were no reports of damage by this CAP. When he had come back, he had survived, and he had never seen the enemy, and we had no reports on whether the enemy had ever seen this interceptor either. So we couldn't make heads or tails out of it.

I studied this and finally figured out what happened. The CAP interceptor had strayed off course by a few miles, perhaps because of a stronger wind or wind from another direction. Under those circumstances the CAP works with what they've been briefed, unless there's a terrific weather change, because they don't have the ability to really do much precision navigation. Generally speaking, when you're on a mission like that there are people watching you. When it's time for you to come home you call up and say I'm coming home. They have means by which they can give you a steer to get back to where you want to land. That must be how he got home at the end of the mission, but in the meantime he was lost and this poor bogey was supposedly going around in circles.

So this CAP aircraft had drifted off station, and when the interceptor was closing the two blips began to merge. Then when they separated, perhaps one of them was lost, and the people in the task force who were monitoring this scene started to watch the blip that referred to the interceptor rather than the bogey. They were giving the interceptor vectors on how to turn to catch the bogey. When they gave him

those vectors he would turn and the people back in the task force noticed the turn and they thought it was the interceptor turning. *(Laughter.)* So they gave the CAP another turn in the same direction, so that they accelerated the whole spiral operation that was taking place. That was something that came out of very careful painstaking analysis of the data we had. People accepted this as being a legitimate explanation, and it gave them something to watch out for in the future.

The same kind of things that happen in air defense exercises also happen in surface warfare, and in submarine warfare.

Bob Sheldon: During your tenure as a manager at OEG, who were your shining stars that you mentored?

Joe Engel: There were lots of people and a few of them rose to great heights within OEG, CNA, and ORSA. I'll mention some of the people whom I thought were outstanding. Jay Steinhardt was the first peacetime Director of what became OEG; he started in 1945 and went to 1962. I was Director of OEG after Jay and served from 1962 to 1967. I can't really say that I mentored the mathematicians because they had comparable training to mine in mathematics. They weren't out in the field as much as I was, but they could bring beautiful insights to bear on problems that were presented to them. The outstanding one in this capacity was Rufus Isaacs. He did very fine work on dynamic programming. Some of Rufus Isaacs's insights were profound. I was followed by Erwin Baumgarten. Erwin was Director for two years and was followed by Erv Kapos in 1969. Phil Depoy became the Director some years after me, in 1974. He stayed on as Director for 17 years.

Bob Sheldon: Any interesting events while you were Director?

Joe Engel: I was visiting the 7th Fleet during the Vietnam War. I was on a routine, once-a-year swing around all our field assignments in the operating areas, as well as the nonoperating areas. We had people all over the world. I was visiting the commander of Task Force 77, Admiral John McCain, the father of Senator McCain. When I came aboard, I was escorted down to the CIC, where the Admiral was, because there was an operation going on at the time. Since it was not actually in a combat area, I was not violating any regulations by being there.

I introduced myself to the Admiral, and he was standing at one of these admiral's desks. He had his headset on, covering one ear, so he could talk to me and listen through the open ear, while he was also listening on the radio circuit. This mission was going on, and he was getting flight reports. Meanwhile, he introduced himself, I introduced myself. I told him my main job was to see if he was satisfied with the way his OEG representative was working. I also asked if there were other things beyond the scope of our representative and whether he wanted us to do some work back in Washington. While we're having this conversation, he suddenly jumped to his feet like a small boy and yelled, ''Yippee!'' I looked at him with surprise, and he said, ''I just got news that our boys got back without any casualties from our first strikes against Mu Gia Pass.'' Mu Gia Pass was a slope of a long, extended valley between a north-south running range of mountains in the area which runs down from North Vietnam to South Vietnam. The North Vietnamese were using that as a supply route to bring their logistics support and troops from North Vietnam to where the front lines were in South Vietnam. It was a very important target, and he was delighted that we got this strike off, and that none of our boys were hurt.

As a conscientious naval officer, he had every right to be excited. I was pleased to be there to observe how seriously the Admiral took his job, and I was delighted we had men of his caliber there.

Bob Sheldon: During your five years as head of OEG, did you see any major trends in OEG?

Joe Engel: Yes. The first big trend was that OEG was getting more heavily into logistics. All of the work that they did was influenced by logistics and influenced logistics. I recall collaborating with my boss, the Director, on some fascinating work. Jay Steinhardt and I coauthored a paper on how you provide for resupply, and I delivered that paper at a meeting in Japan. On the one hand, you want to have the supplies on the line on time, otherwise your combat effectiveness suffers; on the other hand, you don't want the stuff to be lying around in warehouses, where you're spending money to get them to the warehouse fast and to keep them there under controlled conditions. You always

have to strike a balance, and this is what logistics management is all about. Our paper had to do with ensuring the probability of supplies being delivered when they were needed was high enough to satisfy the user. We came up with policies which took this probability of timeliness as a parameter, and we developed techniques to make certain you would meet that.

Around this time, I was asked by Jay Steinhardt to head up a study group, and we were convened to look into ways of keeping better track of possibly hostile submarines or hostile naval vessels. This was conducted at the Endicott House in Dedham, Massachusetts, in the outskirts of Boston. It was a beautiful house and had a fence around it, so it could be secured for classified work. We came up with this Endicott Report, looking at long wave sonar on the ocean bottom.

There were other people who devoted themselves full time to this. Another trend was we were being urgently requested by the Navy to do much more on the financial side. We had been concentrating on hardware and tactics for using the hardware, and on the strategy for a large campaign. We had the Naval Warfare Analysis Group (NAVWAG), which did analysis of the effective cost of what was going on. The Navy wanted more of that. The first head of NAVWAG happened to be my first immediate supervisor in OEG, Doug Brooks.

Bob Sheldon: Can you talk about OEG's history and some of its difficulties in the transition to CNA?

Joe Engel: The problems arose in 1960 and 1961. OEG was founded in 1945, so it had been in existence about 16 years. During that entire period, one contractor had the contract with ONR to provide OR services to the office of the CNO. That contract was with MIT, the Massachusetts Institute of Technology. This made good sense because Phil Morse was there. The Office of Scientific Research and Development, early in WW II, sent Phil Morse to England to talk to the British about ''operational research.'' Then he came back and helped the Department of the Navy and the Office of Scientific Research and Development figure out what they should do. Morse brought the philosophy of British operational research back and the United States decided to emulate it.

Morse was a prominent physicist. As a result of his leadership as the Director, OEG, the Navy work that was done by the people he hired worked very effectively for the Navy during WW II.

It was logical after WW II to set up peacetime OR groups for the Services, so we had the OEG, the ORO, and OA for the Navy, Army, and Air Force respectively. When these new groups were set up, Phil Morse no longer remained on board as Director, although he continued to have an important influence in OR. He returned to MIT where he served in faculty positions related to his first love, which was physics.

He continued to be an influence in establishing ORSA, from which everything else we're talking about stems. He also met with people from other countries, such as Sir Charles Goodeve in England who had a position comparable to his, and P.M.S. Blackett, also in England. They held the first international meeting of Operational Research Societies and from that came the International Federation of Operational Research Societies (IFORS).

Morse did important work pertaining to the Battle of the Atlantic, on the development of tactics and strategy for the defense of convoys sailing from South America and the Caribbean to Europe. On the basis of that work and his proficiency, he got medals; a few medals were given out in all the Services to some of the civilians.

Morse was a good choice to serve as Director, with excellent academic credentials and relevant experience. He was admired, respected, loved, hated, and feared *(laughter)* by various people in his groups. I think that the ones who both admired him most and recognized his shortcomings most, were the more senior people who had served with him during WW II, as well as coming along with him into the civilian peacetime operation.

When Phil Morse left OEG and went back to MIT, his job went to Jay Steinhardt, who had been in the wartime OR group.

OEG grew and prospered for many years and was accepted very well by the Navy and the other Services and by the Secretary of Defense as well, because we did good work. OEG had been battle tested to begin with, and it grew in a very logical way.

Later, it turned out that the Navy wasn't quite satisfied with the way things were going at OEG. An impasse was reached between the trustees of MIT, who held the contract, and their opposite numbers in the Office of Naval Research (ONR), who were essentially the spokesmen for the Chief of Naval Operations (CNO) and the Secretary of the Navy (SECNAV).

They couldn't come to a meeting of the minds, so they decided to put out bids for a new contract. Of course, MIT had the right to bid on that new contract, but MIT did not win. They decided to give it to another contractor, the Franklin Institute of Philadelphia. This meant bringing in a new Board of Trustees and bringing in a new Chief Scientist, so that the Board of Trustees could essentially run the superstructure and establish the administrative, to spend our money wisely. This was around 1960 and it took about two years to actually take place.

As a result Jay Steinhardt resigned; virtually all of us in OEG thought that Jay Steinhardt had been an excellent director. Before the new contract was signed, Jay had recommended to ONR and the new organization that I be named the new Director of OEG and the new, larger CNA. I moved from being the right-hand man to Jay Steinhardt, and instead became his replacement as Director of the new OEG. I would function during a transition period and hold the troops together. I would see to it that we were able to continue working well for the new management. The transition involved a mixture of two emotions; one being pride and the other being fear. You're proud if you do a good job and can continue to do a good job; and you're afraid you're going to get fired if you don't do a good job. This is the conflict that was taking place in OEG.

During the first two years of my tenure as Director, I had to supervise what was done when ONR made its annual visit to OEG. Prior to the change in management, it had been a time-honored custom for approximately 19 years that the representatives from ONR and from MIT's Board of Trustees would come to OEG about once a year for an inspection tour of OEG. Jay would line up a table, and he would put around the edges of the table a copy of every OEG study and every OEG report and every CNO presentation that we made plus whatever else he felt like laying around the table. Reports were generally a compendium of studies dealing with a related topic in various forms of progression, and the report would bring an entire field and several years of work together and make something very substantial out of it. Reports also included the proceedings which we gave in person to the Navy at these large presentations, for which the transcripts had been published.

The representatives of ONR and the Board of Trustees would walk around the table and glance at these documents, and we would hold a meeting that lasted one or two days. Various people from OEG spoke on contemporary developments, and people from the Board of Trustees or ONR spoke about their goals, and then the members of the Board of Trustees met and they decided whether they liked what they had seen and heard. This had a lot to do with whether or not the contract would be renewed. For 19 years this had worked, and they had always renewed the contract, until 1962.

When I gave them this big, full table of high-quality work, I was helping to maintain the standards. We did this two years running.

I was only the Director of OEG for three years, and then I got kicked upstairs; I became the Assistant Chief Scientist of CNA.

Bob Sheldon: What was your role there?

Joe Engel: For a while I had a subtitle, I was the Assistant Chief Scientist for Program Review. That meant I made the final determination on the validity of the works that we were preparing to publish and submit to the Navy on behalf of CNA. It was quite a responsible position, and I had good experience by then. I had seen just about all of the kinds of operations the Navy performed, sometimes with hands-on familiarity, of what was going on in the whole organization. I liked writing professional material. I had the theoretical background as well, so I was a very good choice for that. But I didn't have any troops under me. When I was the Director of OEG, I had something like 90 percent of the personnel at CNA reporting to me.

Bob Sheldon: About how many people?

Joe Engel: I had somewhere around 250 full-time analysts working for me; also others who were support personnel, both secretarial and technical. There were also our security personnel. We had a Director of Security responsible

for maintenance of the security of all the classified material that we had in OEG. That meant our secretaries would come in at the end of the day, take every classified document off our desks, and either put it away in files in locked safes, or take it back to the main classified stacks and see to it that security was maintained.

We had a computer section which consisted of the early variety of nerds or geeks who specialize in working with computers. They were the interface between the analysts and the computer because they knew how to speak the computer languages, and they could take our mathematics or physics, and change it into a program and put it into the computer and see that it ran. In those days we were working strictly on IBM main frames, a 360 and then a 365. They were the equivalent of a palm pilot that we play with these days.

Bob Sheldon: What were the projects you worked on as Assistant Chief Scientist?

Joe Engel: I served for about a year as the Assistant Chief Scientist for Program Review, and then became the Assistant Chief Scientist. I was directly under the Chief Scientist. That meant I had whatever assistant capabilities I could provide in the overall operations of the group. By then a lot of people had left OEG, stripping it back to primarily its field responsibilities and only secondarily to analytic responsibilities. That was a very important and demanding responsibility.

Frank Bothwell was brought into CNA by the new management as the new Chief Scientist of our new organization. He came from a position that he had in weapons development working for the Navy in California. A lot of our most senior people knew of him by reputation and respected him for his previous work. There were either enthusiastic or terrible responses to this man, but I didn't have an opinion.

The directors of the other groups, which were going to be part of CNA, were taken out of OEG. We had a Marine Corps Operations Analysis Group (MCOAG) formed out of our OEG analysts who went to work with the Marines. We had a Technical Analysis Division which became the Technical Analysis Group, and was more concerned with questions of hardware than specific warfare kinds of systems. We had another one, which was stationed up at Cambridge, where they had been set up because of their proximity to MIT.

Although CNA had been in existence a year or two, we were operating pretty much the way we had been, except under new management, including a new Chief Scientist and the people from the Franklin Institute who gave us our basic direction.

Bob Sheldon: How many people changed jobs during the reorganization?

Joe Engel: The number of people who were taken out of OEG or appointed from outside to new managerial positions within the CNA was quite small. I was working with the other groups that had been set up, which didn't have sufficient personnel to do any real work. They could write a few policy papers from time to time. They could pass them to our upper management and through Bothwell to the Board of Trustees. Perhaps they would be given directly to people with whom they had liaison in various parts of the naval establishment. This was as diverse as including MCOAG, a former division of OEG, that dealt with Marine Corps problems. Russ Coile was the first head of that group in the new CNA and he was one of the wartime people from OEG.

Another was NAVWAG, which Doug Brooks now was the head of. We were both very concerned that his group did not have enough people to do any work, because he had his secretary and his assistant and maybe one other guy. When they had worked in OEG, he had at least a dozen, maybe 20 people who were on the scientific staff who were working for him, plus access to the total support staff that the whole group had. After the shuffle, he still had that access, but he didn't have anybody to use it. This was also true of MCOAG and the Systems Evaluation Group up at Cambridge, which had a new man who had recently joined OEG, just a year or two before.

I think we had four divisions in all: the Operations Evaluation Division (OED), MCOAG, NAVWAG, and the Economics Division. One of the things I did was to separate OED into two sections: the field section and the home office. There had not been, on paper at any rate, a separation of these into two separate entities. It struck me, at the time, to make good sense to have two separate entities that could talk to

one another properly. That helped the Director of OEG decide who was going on the next field assignment and what would happen to that guy when he finished the field assignment and came back to the home group.

The CNA reorganization wasn't as effective as the Navy had hoped. After five years, in about 1967, the Navy took the contract away from the Franklin Institute and gave it to the University of Rochester, and then later they moved it to the Hudson Institute. I left CNA in 1967.

Bob Sheldon: When you left CNA, where did you go?

Joe Engel: I went to COMSAT (Communications Satellite Corporation) after being with CNA. I moved to COMSAT with a little help from my friends *(laughing)* to a position where I could work on something positive, like helping the far-flung corners of the earth to communicate with one another. What could be better than that?

Bob Sheldon: Did any of your friends leave OEG about the same time you left?

Joe Engel: Yes. A good friend of mine, Howard Kreiner, who I relieved in Key West, later followed me to COMSAT.

Bob Sheldon: What kind of job did you go to at COMSAT?

Joe Engel: I got a position as a special adviser to Lucius Battle. Battle had served in the State Department and was Vice President of COMSAT. I had met him once, quite by chance, long before this COMSAT business, in the Mediterranean when I was serving with the 6th Fleet and he was there as a governmental envoy. He had been the Ambassador for the United States to the United Arab Republic (Egypt). We had some pleasant conversations and he may have remembered those. They had an active COMSAT laboratory, but my efforts were to work for the upper staff management. Lucius Battle was my immediate boss, and I was supposed to do OR studies pertaining to the overall operations of COMSAT, not specifically to the scientific endeavors that were the province of the laboratory.

COMSAT was an organization that was developing equipment, hardware and software. It would then persuade members of the international community to acquire and use this equipment to set up a network of communication satellites that would enable communications to be sent all around the world.

Bob Sheldon: Were you involved in ORSA while you were at COMSAT?

Joe Engel: Yes. I was still a member of the ORSA community. I became the 17th President of ORSA in 1968.

Bob Sheldon: You mentioned offline that you testified before the Senate Armed Services Committee. Can you give us the story behind that?

Joe Engel: In the late 1960s, various weapon systems for use by the military were under consideration, for budgetary purposes, to determine whether they should be procured and whether research should be done on them. The federal government has to approve the budgets; the budgets are created by Congress and signed by the President. It's necessary for all these people to be well informed on the matters under discussion.

They were looking into the potential of a weapon system called Safeguard. I was called to testify because I had been, along with other well-known and experienced people in the OR community, asked to prepare a report on the manner in which two other people had testified on this same weapon system.

One of the people was Albert Wohlstetter, who was based in Chicago. The other was George Rathjens. He was the Chief Scientist of the Weapons Systems Evaluation Group (WSEG) in the Institute for Defense Analyses (IDA), which supported the Joint Chiefs of Staff. It was certainly appropriate that IDA be considered as a source of information.

It was logical for Rathjens to testify on this subject; and it was logical to have Wohlstetter, because he was definitely involved professionally working on the development of the Safeguard system, and he was a proponent of this system. They both testified.

Bob Sheldon: What was Safeguard?

Joe Engel: Safeguard was an antiballistic missile system to protect US ICBM sites. It also had a guidance system and it had a certain range and this combination of characteristics warranted consideration by the military, because it could do something that they had not been able to do before. It represented an improvement and something that they ought to have if they could get it at an economical price.

When these two guys testified, they differed in their statements about the potential of this equipment and its costs. There were very strong differences between them. This had the Senate confused, and they needed to get some objective means of evaluating these two conflicting statements so that they could reach their own conclusions.

What had preceded my testimony was the fact that Wohlstetter wrote a letter to ORSA complaining that he felt that the testimony that Rathjens offered to the Senate was biased and wrong, and that he was guilty of distorting the facts. Rathjens rebutted by saying that, on the contrary, he felt that because Wohlstetter was personally involved in the development of the Safeguard, that he had left the realm of ''proper OR,'' which is to make recommendations to a client. He had instead become an advocate, which you're really not supposed to do when you are providing objective analysis.

Once you become an advocate, you become someone who supports the position that you have put forward for the client. And you can support a position without lying by not mentioning certain things. This is why when you're in court and they ask a witness to tell the truth, the *whole* truth and nothing but the truth; they are seeking truly objective information. Whereas, if you don't necessarily tell the whole truth you may be omitting certain facts about the subject, that do not support the position you are advocating. This is why saying ''the whole truth'' is so important and this is why it is conceivable that Wohlstetter's statements could have been biased in this manner even though he may not actually have been lying, but he was shading the results by omission.

I'm not accusing him of this; I'm saying it is a possibility that could apply to any individual. The same thing might be said of Rathjens, because he was advocating a position strongly in opposition to what Wohlstetter was saying. He might not have been lying; he might have been merely omitting certain things or emphasizing certain things that Wohlstetter did not choose to emphasize. It was a question of the conflict between advocacy and objectivity which was brought into focus as something that ORSA should look at, and we did.

I was a Past President of ORSA at the time, and I was one of about five Past Presidents and a couple of other guys who were invited to look into this matter and report to the society.

Bob Sheldon: Who were the other guys?

Joe Engel: Hugh Miser was one of them and there were two or three other Past Presidents of ORSA and a couple of other people.

We completed our report and called it ''Guidelines for the Practice of Operations Research.'' It was published as a special edition of *Operations Research*, the journal of the Operations Research Society of America (*Operations Research*, Vol. 19, No. 5, Sep, 1971, pp. 1246–1258). In addition to analyzing the testimony of both the people involved and offering our opinion on whether it was good testimony or not, we also wrote what amounted to a moral code for how to perform honest consultation for your client. This was a potentially valuable contribution. At the time, the President of ORSA was Bob Machol, and he wrote a prologue to the report saying that it should be regarded very seriously by all members of the OR community as a guideline to how OR should be done. This made it a very emotional issue for a lot of people, scientists in or out of the OR community, because essentially it was suggesting there's a certain way a scientist should behave.

Bob Sheldon: Were your opinions unanimous, you and the other guys?

Joe Engel: Yes, they were. We had considerable discussion, but we reached unanimity and a report was written which we all voted to accept. While we were taking testimony and evaluating, we spoke to both Rathjens and Wohlstetter and we read their writings. We formed our conclusions about what they had written and said, as well as our conclusions as to the validity of this work.

We were making an independent examination and we tried to be objective. We reported flaws on both sides to make it quite clear that we were not trying to choose sides, but we were trying to point to errors in process.

We produced this document as an official publication of ORSA, which bore the endorsement of the society, and this was obvious because the prologue agreed with the positions that we had taken. It was interesting to see who lined

up on the sides of this controversy that was stirred up by this report.

Bob Sheldon: Do you mean in the Senate or in the OR community?

Joe Engel: In the OR community. The people who were high up in the faculty or former associates of MIT were strongly opposed to the report. Aside from the philosophical reasons, scientists should not be hampered in the accomplishment of their research by any considerations outside the research itself. The reason that people at MIT may have been influenced by that is George Rathjens had been affiliated with MIT and he was one of their favored sons. Other people at MIT were involved in the development of OR, including Phil Morse, who was the founder of ORSA. My ex-boss, Jay Steinhardt, was also an MIT affiliate and had been an employee of MIT and was heavily involved. There was a commitment of these affiliates of MIT who opposed this report because it was, by implication, critical of things that MIT had supported.

I was personally attacked, as were other members who participated in writing this report, by people who opposed the position that ORSA had taken as voiced by the authors of this report.

Bob Sheldon: How was it received by the Senate when you testified?

Joe Engel: They had access to the report and could have read it and maybe they did. Then I testified. What I gave was an encapsulated summary of the report in which we questioned the objectivity of both Rathjens and Wohlstetter. We pointed to errors that we found in the recorded testimony and publications of these two gentlemen on issues pertaining to this particular weapon system.

What this did to the Senate was show them that they were looking at a bucket of worms, and it was up to them to decide how to pick the spaghetti out from the worms. We helped them by clarifying the distinctions between the worms and the spaghetti. It was up to them to do that because they were the ones who had to make the decision.

We tried to be objective. One of the points of our entire report was when you make a recommendation to a client, you should give him your conclusions about the good and bad points, so that he can then draw conclusions. He will then have all the information that you can put at his command, including information that may not necessarily support the recommendation that you're making.

Not only do you put in your conclusions and recommendations, but you give a complete summary of all of the things that you looked at and learned and what the limitations were. And then he can accept or reject your conclusions, in whole or in part. That was the gist of our report. And frankly I don't think that is offensive, except perhaps to some individuals who feel that questioning their results is a slur.

Bob Sheldon: This list of ethical guidelines, has that stayed current in the INFORMS community? Do people still use such guidelines?

Joe Engel: I don't know that it was ever reissued by ORSA. It may be that it's a dead issue in the sense that people say, ''That's right, but we know how to do that.''

Bob Sheldon: Let's transition now to academia. What motivated you to go to academia?

Joe Engel: I was recruited.

Bob Sheldon: Did one of your ORSA friends recruit you?

Joe Engel: Yes. Bob Machol had been working on me since before I went to COMSAT. Bob followed me as President of ORSA in 1971, and he had been at OEG. Bob was the editor of all the publications that ORSA produced. That was a very important and responsible position, because you had to know how to be an editor, as well as to understand the subject matter.

Bob and I got to be good friends during that early period at OEG, both personal and professional. He left a couple of years after I got to OEG and went to the University of Michigan to do graduate work. He finished his dissertation and wrote a book, the first textbook on systems engineering.

Bob Sheldon: He was in Chicago when he recruited you?

Joe Engel: He had gone to the University of Illinois at Chicago Circle, which was in Chicago. When they set up the University of Illinois at Chicago Circle, they bought the land and put up new buildings.

Bob Sheldon: The University of Chicago had a good reputation for math.

Joe Engel: Yes, they had a very fine math department. Bob Machol was one of the people they recruited as the Chairman of the Systems Engineering Department in the College of Engineering. They also set up a Department of Management Science in the economic side of the college.

I went there as the chairman of the department, at Bob's urging. Later, he went to Northwestern University, where he served on their OR faculty. But at the time that he approached me after I had had this experience at COMSAT, I had already decided that I was going to leave.

Bob Sheldon: Did you have administrative duties and teach classes?

Joe Engel: Yes, I did. I had the full panoply of responsibilities. When I accepted the position, I was retained as a full professor with tenure and as a department head. I was expected to teach and conduct research and go out and get grants and administer my department and participate in committees to facilitate what was going on in the university as a whole.

Bob Sheldon: How many professors were in your department?

Joe Engel: I had three instructors, three assistant professors, and two associate professors. It was a small department, but it ran an adequate program of undergraduate studies at the time that I arrived.

Within two years, I proposed and developed a program of graduate studies, which was accepted by the college and enabled us to institute graduate education and authorized the granting of the master's degree. Later, this was extended to the doctorate, although not in my time. I had the privilege of being the first dissertation advisor of someone taking the master's degree. I also conducted graduate courses, as well as previously having conducted undergraduate courses.

Bob Sheldon: What kind of courses did you teach?

Joe Engel: One was a seminar that was devised as a laboratory course for the graduate students. In that seminar, the graduate students had to conduct experiments and collect data and analyze the data that pertained to systems engineering. It was a popular course and lots of good students took it.

I told one of the university administrators about some of the results that the students had. One of the results was that he had understated the cost of excavation of the foundations for the university's buildings and these costs were substantial. That was fun, and I enjoyed it very much.

Bob Sheldon: Was it while you were at Chicago that you co-founded Omega Rho?

Joe Engel: We started before that. For some time, there was discussion in the OR community that the more established sciences had honor societies. This was a means of recognizing the quality of work that students were performing in these fields and giving them recognition, and promoting their careers and thereby doing better work for the community at large.

One of the proponents of this was Clinton Ancker, well known in the OR community. He asked if I would like to work with him to establish an honor society. I liked the idea and accepted. I then became involved in all the minutiae that you have to go through when you set up a new organization.

We wanted to follow the guidelines of the Association of College Honor Societies, and Clint knew about this because he had more experience in academia. Clint did most of that work. I got the tasks of setting up this organization. That involved the details of how this organization should be accepted. We would have to make our wishes known to the various universities that would wish to have such an honor society on their campus. Wouldn't you like to be one of the universities that has a chapter like this on your campus, and if you hurry up right now, you can be a founding chapter. We had to know how to set up chapters. We had to write a constitution for the overall organization and the methods of control of it and whether it would have a board.

A lot of the details were hammered out by the two of us. I suspect Clint had the major brunt, while I concentrated on the details.

It was important to develop a name, the symbols that would epitomize the society. Like Pi Tau Sigma and Tau Beta Pi and Phi Kappa Phi and other honor societies that had Greek letters. I had to choose Greek letters that would be appropriate, and I did the logical thing. I approached the Greeks who ran the Greek OR society in the international community to get their advice on how we might name this new honor society.

They told me that the Greek words for OR were very long and complicated compared with English. Operations is three syllables. Research is two syllables. The Greek words had more syllables and they might have been harder to pronounce for people who didn't already speak Greek. And they had different initials than the English words for OR. They recommended that what we ought to do, because it would be easily recognized by the international community, was to use the letters Omega and Rho, which are the Greek letters for O and R. Omega and Rho became the initials of our society and the name of it.

Bob Sheldon: Did you get help from the ORSA community?

Joe Engel: Yes. We were encouraged by the ORSA community to approach various organizations, and when we needed information they gave it to us. Not only did I devise the initials, and consequently the name of the organization, I helped to set up the idea that we needed someone to act as a director of operations who would run the thing on a daily basis.

We wrote a constitution, which called for election of the officers of the society. When Omega Rho was incorporated and had a few charter chapters, the first election was held, and Clinton Ancker was the first President. He was the principal founder and he deserved it. He ran unopposed. I was nominated and elected as the first Vice President. Then I got elected as the next President. More universities asked for chapters on their campuses and we were happy to see to it that they were accepted. There were procedures for accepting or rejecting an application.

Bob Sheldon: You left academia and chose to go to the National Highway Traffic Safety Administration (NHTSA). Why there?

Joe Engel: A friend of mine, Don Maylo, was director of the division in NHTSA that was responsible for statistical analysis concerning safety. Don was an ex-OEG member. I had been his senior because I arrived earlier and was his first project team leader. We had many contacts over the years, and he decided to leave NHSTA about the time that I decided to leave academia. He was looking around for a successor, because he was planning to leave the OR field and become a lawyer. Eventually he became a good lawyer. He told me what the job was and urged me to apply for it.

I applied for that position and received an offer from NHTSA, so I took the job. My supervisor was Bill Scott. They had three divisions in this part of the NHSTA. My division analyzed statistics concerning accidents, fatalities, casualties in various kinds of traffic situations, and whether they were head on, a side swipe, around a curve, crossing a railroad crossing or at an intersection, or speeding. These were the various kinds of parameters on the road that could influence how safe you were at the time. Safety had to be measured in terms of the number of accidents per something which is a measure of exposure to that condition. The usual measure of exposure was passenger miles. You would learn how many accidents or fatalities or injuries or just plain accidents occurred per thousands of passenger miles.

The numerators were a count of how many incidents occurred during a standard number of passenger miles. Another way was, instead of using passenger miles, use just plain miles, which would be the case for safety or vulnerability of the vehicle to accidents. Vulnerability might be affected by the number and age of the passengers. I found the work interesting and traveled around the country to talk to people involved in safety, and then I returned to the office and made these calculations and gave reports. I was briefed on assignments from Bill Scott on what type of safety they were concerned with, and we would go do it.

Bob Sheldon: How many people worked for you?

Joe Engel: About a half dozen. Some of the people were quite competent, because they had been there about five or 10 years. They were about my age and I enjoyed working with them. It was a good intellectual climate and the work was very important.

Bob Sheldon: How did you deal with the fuzzy data, for instance the miles?

Joe Engel: This is precisely the problem that troubled me. It turned out that the best way to estimate exposure or miles or the number of cars that crossed a given point per day, was to go out into the field and count what you were interested in. One of the automatic devices didn't require manpower on the site for days at a time

was a little rubber cable that stretched across the road, and every time a set of wheels went over it, it would make a jagged interruption in a smooth line that occurred when it was empty. You could tell from the recording whether the vehicle was a motorcycle or a four-wheel passenger car or a small truck or an 18 wheeler, because you could count the number of peaks on the right and the left side of the wheels. You would know what time it was, so you could get the influence of night and you could discover what was happening in rainy weather, sunny weather, or cloudy weather by relaying the weather conditions.

You would learn also when the rush hours were and the rate in equilibrium, or at night when there was practically nobody on the road. Incidents were simpler to count, because you would look at police or insurance records and get hard information.

In spite of the fact that the pressure cable was less expensive than manpower, it was expensive because you had to have the equipment, you had to maintain it, you had to collect the tapes that were produced and then analyze it. But in the very early 1980s, federal money was tight and when there was a budget cut, part of that cut went to my division and I didn't have enough money to make the measurements that I needed. That was the main reason I decided I didn't want to stay there, because I was hit too hard by the budget. Sooner or later the quality of the work we put out would suffer.

Bob Sheldon: Do you recall any dramatic issues during your time there?

Joe Engel: We were interested in the impact of improved safety features on the vehicles. There were lots of things done over the years, and the controversies at that time related primarily to the introduction of airbags. Safety belts were already in use at the time. There were different varieties, and one could quibble about which kind of seatbelt was better—whether you anchor them to the floor or to the side frame or the ceiling. There were similar controversies about the airbag. The technology for producing both seatbelts and airbags had been around for quite some time by the 1980s and the system had matured to the point where the car makers were putting seatbelts in all of the cars.

We were now going through this kind of debate with car makers concerning airbags. They didn't want to install airbags on all of their vehicles initially, and we preferred not to pass a federal law requiring them to install airbags. There was a carrot-and-stick game going on, where you would offer carrots to them if they did these things voluntarily, and you would threaten them with the stick of making it compulsorily. That was a history of the way new safety features developed as technology improved. After some resistance, the car makers would install those things, and then sometime later it would be compulsory at the federal level.

The administrator of NHTSA at that time was Joan Claybrook. She had been a long-time protégé of Ralph Nader. They both were interested in the consumer, quality of life, and the environment. She was sincerely interested in the consumer welfare, which is what you really need in an agency concerned with traffic safety.

After a couple of years at this, I found that because I could not measure the data I needed for calculating safety, it was meaningless. I called the Bureau of Standards—they had also wanted to hire me when I left Chicago—and I was hired.

Bob Sheldon: What year did you go to work for the Bureau of Standards?

Joe Engel: About 1982. I stayed with them and retired from the civil service in 1986, when I was 68 years old.

My contact at the Bureau of Standards was Burton Colvin. He had been a supervisor of teaching assistants when I did my graduate work at the University of Wisconsin. I taught calculus, and Colvin was my supervisor. Colvin knew that I had been a good teaching assistant and he knew from my record that I was well qualified. I applied to him and got the position. I started out with a very positive attitude and had a very competent staff.

Alan Goldman, whom I had relieved at the Bureau of Standards, had this huge stack of documents roughly three feet high on his table; they were unclassified so he wasn't breaking any rules, but sloppy as all get out. Someone would come in and ask for some document and Alan would just reach into the pile, pull out the document, and hand it to him; it was amazing.

Bob Sheldon: What kind of projects did you work on at the Bureau of Standards?

Joe Engel: Some of them were highly theoretical. They were continuations of research by people that were still there. There was research on fire safety and building construction. There was some work on crystals recently discovered that were not symmetric. Someone was working on tiling. Tiling is a method of filling a plane with a simple polyhedral. At that time, most of the emphasis was on crystal shapes that were symmetric, such as a diamond, equilateral triangle, rectangle, or square.

You can get checkerboards with staggered rectangles of various ratios of length to width and that's fairly straightforward, but when unsymmetrical crystals were discovered, the question of tiling those things came up.

This one man made fantastic strides and discovered how to do unsymmetrical tiles. He worked primarily with triangles that were not isosceles or equilateral. The isosceles and equilateral, being symmetric, were easy. You can construct diamonds by putting together two isosceles triangles, and the word for a diamond is a rhombus; it's an equilateral parallelogram. With other triangles that were not symmetric, you could drop an altitude from the vertex opposite the longest side of the triangle and then you could make ratios of the altitude and the long segment of the hypotenuse.

Certain of those ratios would allow you to make a fit that would relate those triangles to something that you could replicate in a space-filling manner. Once you have a rectangle, you can fill space in all kinds of ways and get interesting patterns on a countertop. This has been done, and you can eliminate the rectangles if you wanted to do it solely with triangles.

As a division chief, when I walked in, they gave me an IBM desktop to keep track of my budget, performance ratings, and other administrative responsibilities, including report writing. I started out knowing absolutely nothing, except a theory of how to use a desktop computer and it proved to be just about as easy as that. Prior to that time, I didn't work much with computers. I prepared material for a specialist who worked with computers. Desktop computers came out in about 1978, and they had become prevalent by 1980 when I first started working for the civil service.

I had a word processor and spreadsheets. I learned how to calculate various mathematical functions of data with spreadsheets. I could write my own reports and do my own research. A lot of what I had to do on the computer was material that in older times would have been turned over to a secretary or a computer section.

At the end of the second year and near the end of the fourth year, we had to undergo RIFs (reductions in force) because federal funds for the Bureau of Standards were cut. The RIFs were very disruptive. You were forced to RIF enough people to recover the salaries that amounted to the money being cut for personnel from your unit. That meant essentially that I was firing people. The first RIF was comparatively minor. I had to RIF two or three people from my division.

The second RIF was much more serious. Big cuts were made all over the place. The OR division in its entirety was RIF'd. That meant that I had to write the papers and inform the people that every one of them was RIF'd, and I was informed that I was among the people being RIF'd. I could remain at the Bureau, but not as a division chief. I would have to take a cut in grade. I was at that time in the Senior Executive Service. I was told that I could take on a GS-15 rating, and I would retain my salary for the next two years, but after that I would revert to the normal salary for a GS-15. I decided under those circumstances that I would retire. I announced that decision after I found someplace else to go.

I went to Arthur D. Little, a global management consulting company. My friend there was Dave Boodman. He had arrived at OEG a year before I did, but he was about my age and had very similar experiences to mine. He was active in ORSA, and we were good friends. I called up Dave and filled out the application. About two weeks later, I received a phone call from the Washington office of Arthur D. Little and was interviewed and was made an offer and accepted.

I worked for Arthur D. Little for four years. I worked as a consultant rather than an employee, but I got a good salary, a little higher than what I had been making as a civil servant. Arthur D. Little had several contracts dealing with the Postal Service, but the project on which I was consulting was the cream of the projects.

It involved developing a real-time computer system for the Postal Service that would enter all of their operations and enlist all of their facilities, their trucks, airplanes, and trains, and the schedules when they sent things. The system included the entire operational status of the things they did, and the relationship of the equipment and the customer demand to the efficiency with which they accomplished their operations. You could talk about the percentage of mail that didn't get delivered in accordance with the performance standards for that class of mail. We also had to compute the procurement costs of the facilities and equipment, as well as the operating costs and scheduling information.

I said you have to include maintenance. I was given the responsibility for what I felt was part of the project on which, up until then, nothing had been done.

Bob Sheldon: Did you get it done in four years?

Joe Engel: Yes, I did. The group, before I got there, did essentially the dog work. They developed the work required to record, store, and display the actual geography of the mailing system: big maps with the time zones shown on them; locations of facilities and airports; and road maps. They were able to take into account schedules of the airlines and trains, and the speeds of the trucks on the road.

They were also able to keep track of these things going from the first unit to the zip code collection facility, then from the zip code to the region, from that region to the next region, back down to the zip code, and finally to the individual unit. But they didn't have any of the maintenance effects in it.

With them providing the foundation, I figured out what would happen if maintenance were perfect. I then added what would happen if maintenance was not perfect—if you had breakdowns, or flat tires, or a train derailed. Comparing to perfect maintenance, you could get results of nonperformance if schedules were not met. When they were ready to install that equipment shortly thereafter, I looked over my data from test runs, and I compiled a report.

My report stated that there was one particular class of mail that would not meet performance standards, and that was air mail. I said half of the air mail won't get there on time because of the airline scheduling and the facility scheduling that had to blend with it. I made that prediction on the basis of the model that we had derived, and the next month my prediction was borne out by operational data.

We had known that we were having trouble with air mail, but here was a theoretical derivation from analysis of what they had. It verified what had been essentially old wives' tales—there had not really been a truly systematic collection of this data. I was able to tell them that this problem was an inevitable result of the combined conflicts of schedule, and that meant that something had to be changed if you wanted to improve performance. Following that, the Postal Service made attempts to modify scheduling so they could improve their air mail performance.

I then went to work on the maintenance model. We had a very simple maintenance model. Under a periodic maintenance regime, there are standard equations that tell you the frequency of occurrence. It enables you to figure out what fraction will be delayed during a period of downtime. I had this fully developed and ready to go after I had been there four years. Then we suddenly received notice at Arthur D. Little that the liaison officer we dealt with, who was responsible for the OR efforts of the entire Postal Service, was out of funds.

He discontinued the contract on a very short notice, so at the end of the period at which this was done, Arthur D. Little would no longer be working for the Postal Service. That meant that the entire Washington office had to be closed. As a consultant, I had no seniority rights and they released all of the consultants.

I had retired from the civil service in 1986, but I effectively retired in 1990. I had started working when I was 21, and now I was in my early 70s. I had a long career by that time, so I began collecting my retirement pensions, which were good enough for me to live comfortably on at the time.

Bob Sheldon: I assume you continued your involvement in the OR community?

Joe Engel: I did a little more paid consulting in the next couple of years. Somebody who was writing an encyclopedia of OR asked me to work on specific topics that eventually appeared in that encyclopedia. I continued for some time to maintain an active interest in OR, and I continued to

attend INFORMS meetings and occasionally do something on behalf of the society on request, as well as some committee work.

I participated in meetings that suggested a merger of TIMS (The Institute of Management Sciences) with ORSA, from which INFORMS was started. TIMS had been an offshoot of ORSA. I was always against the split and I was one of the strong advocates for its reunification. I served on a committee and we came up with that recommendation, and that recommendation was implemented almost immediately. By the time INFORMS was started, there was recognition in both societies that they had approximately a two-thirds overlap of membership; it was inefficient, and the societies were not getting enough money.

The early ORSA was dominated by the military side of things, and the commercial aspects of OR were beginning to grow. This is OR, only they call it management science, just to show that they were different, and it didn't have military overtones. That's why TIMS was formed in the first place, because the nonmilitary people who had become involved didn't have enough of a voice in ORSA. This is attested to by the fact that Phil Morse was the first President of ORSA and Jay Steinhardt was the third President of ORSA.

Bob Sheldon: MAS created an award in his name, the Jay Steinhardt Prize.

Joe Engel: Yes. Jay died a few years after he left OEG. He had taken a position as Scientific Advisor to the President of Georgetown University. It was a respected position, and he was well thought of. He was also a scientist in his own right, one of the early biomedical engineers. He used to go to Woods Hole, Massachusetts, every summer and work on his biomedical research. He was trying to deconstruct and reconstruct the human hemoglobin molecule. This was long before the time when our knowledge of DNA was what it later became. I was well known to the family because I had been Jay's right-hand man, and I was his good friend after he left OEG.

They asked me to conduct a memorial service for him in the chapel at Georgetown University. After that service was over, I met privately with the family at their request and they asked me what could be done to memorialize Jay's name. I suggested that they set up a medal to be awarded to the person who best embodied the concept of a fruitful career in military OR.

ORSA acted on that suggestion and went to MAS. I wasn't active in MAS at the time, although I had been in the past. I was surprised and embarrassed when it turned out that the first recipient of this particular award was me. *(Laughter.)* I had no idea that this was coming, and to an outsider it might have looked as if I was feathering my own nest in the sense that I recommended an award that would be given to me, which was not what I had in mind. I had in mind some of the more senior people, for example, Bernard Koopman. I wasn't restricting it to just OEG; I meant it for all the Services. There were plenty of people who could have been given this medal. Nevertheless, I think it was a good award in principal and I'm pleased that I did get the award.

Getting back to the leadership of ORSA, the first 16 ORSA presidents were all from the military. I was the 17th President of ORSA, and by the time we celebrated our 20th anniversary of ORSA in New Orleans, I was no longer even the junior President. We had a wonderful time at that 20th anniversary of ORSA. We were told in advance that we were going to be receiving gifts from the society in recognition as past presidents of the contributions we had made to the society. We received gavels with the names of all of the first 20 presidents. I prepared a gift that I gave to the society in return when I was given my gavel. I gave them a banner which had the ORSA logo on it and dates from 1952, when the society was formed, to infinity on the other side.

I did something like that later on for Omega Rho when that society was established. I gave a motto to Omega Rho. The motto was *Ad Astra*, which is Latin for ''to the stars.'' I gave them this flag I had drawn by hand on a bed sheet. Later, they made it up in felt and they hung it for a few years at the annual meetings. It was the official flag of the society.

I mentioned to you earlier in this interview that I had done some unpaid work for NATO, as a volunteer representative from the US ORSA activity to NATO. I viewed all of the activities that we performed as constructive. We were

there to help NATO do a good overall job, and that included more than fighting wars. We helped to set up and support the IFORS meetings. There were other people outside NATO who supported IFORS, and that included potential enemies as well as friends. But in IFORS we were essentially all working for the general good of mankind, which made IFORS a wonderful organization.

I continued for quite some time. I was one of four people who had attended all the IFORS meetings up through the ninth IFORS meeting. IFORS were held every three years, which meant I spent 27 years as an attendee and representative of the United States at these meetings. For a couple of years running, I was a Vice President of IFORS. I was the chairman of one of the meetings and was the after-dinner speaker. I decided that I would have some fun at this meeting and give the people a history of IFORS in terms of the kind of contributions that IFORS had made, and with very specific reference to what transpired at each different IFORS meeting. These IFORS meetings occurred all over the world: Japan, the United States, Mexico, Canada, England, France, Spain, Greece, Italy, Israel, and so on.

I gave a talk full of slides taken at various locations and the tourist things one could view, and made comments ostensibly related to the operations research aspects of the site. When I showed slides of Oslo, where the third IFORS meeting took place, you could observe the sod houses there that used to be the farm houses for people living in Norway. You could see the grass growing on the roofs and I said, ''Here you see an early OR solution to optimal housing for the climate and activities taking place in Norway at the time these were developed,'' and everybody laughed.

I covered each of the nine meetings that I had attended. There were some pictures that were taken up in New England, where we went swimming on one of our off days, and people like George Kimball were there. I took pictures of them in their bathing suits. I later made sketches of them in pen and ink at home, which I passed around. I made comments referring, without mentioning names, to the physical shape of these individuals, so there were quite a few people with pot bellies and a little bit knock-kneed or bow-legged.

I have gone to a few other OR meetings. I was at the big anniversary of INFORMS that was held in California in 2002. I gave a talk and I received an award as an INFORMS Fellow when the award was first instituted. I delivered a paper, the Lanchester paper you were talking about, at that meeting, and that was my swan song in the OR community. I'm still alive, but I've withdrawn from politics on the one hand and OR on the other.

Bob Sheldon: What advice would you give to someone interested in OR?

Joe Engel: My advice would be the same as it would be for specialized careers of any sort. Find out what kind of knowledge and experience you ought to have before embarking upon such a career. Get formal academic training to prepare yourself and decide whether you are prepared to undergo that kind of training.

The main thing is whether or not you think you are interested in the kind of activity that you are contemplating. There are all sorts of facets to OR. When I entered OR, I was unaware of most of these considerations. The subject matter of OEG was fascinating, mainly how should the Navy do its work better and how do you find out? You go and visit them and see how they work and you travel all over the world in all kinds of ships and aircraft.

I happened to pick a job that I was temperamentally well suited for, and it was a place that had a high quality of performance associated with it. I met top people from the very beginning. I was given fascinating assignments and had a chance to travel and to support my family well, so it was an ideal job for me.