2014-19-03

Marion L. Williams Interview (MORS)

Williams, Marion L.

Military Operations Research Society (MORS) Oral History Project Interview of Dr. Marion L. Williams, FS

Mr. Greg Keethler, FS

gkeethler@gmail.com

Dr. Bob Sheldon, FS

Group W, Inc.
bs@group-w-inc.com

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. Marion L. Williams was MORS President from 1982 to 1983 and was elected a MORS Fellow and selected as the Wanner Laureate in 1991. Dr. Williams was the Chief Scientist and Technical Director, Headquarters Air Force Operational Test and Evaluation Center (AFOTEC), Kirtland Air Force Base (AFB), New Mexico, from 1974 until 2005. This interview was conducted on May 1, 2002 in Dr. Williams's office at Headquarters, AFOTEC, and June 13, 2012 at the US Air Force Academy.

MORS ORAL HISTORY

Interview with Dr. Marion Williams, FS
Mr. Greg Keethler, FS, and Dr. Bob Sheldon, FS, Interviewers

Greg Keethler: Tell us where you were born, raised, and educated?

Marion Williams: I was born and attended public schools in Abilene, Texas.

Greg Keethler: What are your parents' names and how might they have influenced your academic or professional directions?

Marion Williams: My parents were Lester and Faye Williams. Although both were strong influences in that they encouraged me to ''do my best,'' they left the definition of what was best strictly up to me. The advice from my father I remember the most was on helping others. He repeatedly told me that if someone around me was working, always offer to help—at least don't stand around with your hands in your pockets. Probably more influence on the academic side came from my paternal grandfather. He was superintendent of schools for the county in Texas where we lived, and the most educated man in our family. Like my father, he wasn't particular about the type of education, just that we got it.

Greg Keethler: Did you take an early interest in math and science in high school?

Marion Williams: Both math and science were of interest. Engineering was always my intended career, although I don't remember when or why that decision was made, and I don't recall anyone even suggesting it. Nothing else was seriously considered, except for a short time in high school when I wanted to be an FBI agent. I went to the local FBI office, introduced myself to the only agent there, and asked if he could help me learn what an agent did. He explained the job, and even took me with him on minor investigations. However, when he explained that I had to get a degree in accounting or law, I quickly lost interest. I just wanted to carry a gun and chase bad guys.

Greg Keethler: Where did you complete your undergraduate degree?

Marion Williams: I graduated from Texas A&M University in aeronautical engineering. My specialty was aerodynamics.

Greg Keethler: Were you in Air Force Reserve Officer Training Corps (ROTC) in college or did you go through Officer Training School (OTS) for commissioning?

Marion Williams: Like engineering, ROTC and Texas A&M were other automatic decisions, since I had hopes of becoming a test pilot. But if not, then I could design airplanes for other test pilots to fly. My hero was Chuck Yeager, the first person to break the sound barrier. During my senior year in college, I had an opportunity to meet him at Edwards AFB, California, during ROTC summer camp.

Greg Keethler: How did you choose your undergraduate major in aero?

Marion Williams: Before high school, my primary goal was to be a test pilot. That mapped my planned career of engineering, military service and then test pilot in the military or industry. Aeronautical engineering was the natural field for that job. In my sophomore year in high school, I received one of the major disappointments of my young life—I had to start wearing glasses. My career as a test pilot was ruined before it even started. I was heavily into the Civil Air Patrol (CAP) at that time and was chosen for a trip to Denmark as an exchange cadet. The CAP leader and my high school teachers encouraged me to stay in aeronautical engineering, with a hope that the Air Force would waive the 20/20 eyesight requirement by the time I was out of college.

Because of my interest in aero engineering, after graduation from A&M in 1956, I accepted a job at Sandia Laboratory in Albuquerque, New Mexico, as an aerodynamicist

at their supersonic wind tunnel facility. Primarily, I was testing nuclear weapon shapes and doing some wind tunnel and instrumentation design. My main reason for choosing Sandia was that one of the nation's experts in aerodynamics, Alan Pope, was to be my boss. He wrote the book on wind tunnel testing (*Wind-Tunnel Testing*, 1954). After a few months at Sandia, I was called to active duty as an Air Force second lieutenant at the Air Force Special Weapons Center (AFSWC), now the Air Force Weapons Laboratory.

Greg Keethler: What was your Air Force officer career field?

Marion Williams: My Air Force career was as a weapon systems engineer. It was more of an engineering management job than anything else.

Greg Keethler: What weapons programs did you work on at AFSWC and what impact did you have? Where was AFSWC located?

Marion Williams: The Special Weapons Center was at Kirtland AFB, New Mexico. My job as a weapon systems engineer was to oversee integration of a nuclear warhead into the TM-76 Matador missile. We had electrical and mechanical engineers in the Air Force and at the Glen L. Martin Company doing the technical work; I was more of an engineering project manager. We did the initial fit check of the warhead, which was developed by Sandia Laboratory and Los Alamos Laboratory, into the Matador at White Sands Missile Range, New Mexico. It wasn't a very good experience. We found that the warhead simply didn't fit; we didn't have computer-aided design (CAD) at the time, and somewhere along the line, the dimensions didn't match. I had borrowed an inert warhead from Sandia for the fit check, and accompanied it to Holloman AFB, New Mexico, for transport to White Sands Missile Range. We couldn't get the warhead to fit in the warhead compartment. The return trip to Kirtland was as much fun as the fit check. I had to wait on the runway at Holloman with the warhead for several hours until the airplane showed up; and I couldn't leave for coffee or any other essential functions. Then we had problems tying the warhead into the cargo airplane on the return trip. It broke loose and caught my hand between the warhead and airplane bulkhead. When we got back to Kirtland I was ready to get rid of the thing and go home. I turned it over to the base, got my hand receipt and left. The next week I received a call from Sandia wanting their warhead back; they claimed it was never received and I couldn't find my receipt. I could see my Air Force career ending with a court martial for losing a top secret nuclear warhead. Fortunately, the warhead turned up, so a court martial wasn't necessary.

Greg Keethler: Did you learn anything valuable from your active duty time that was useful later on in your career?

Marion Williams: There was nothing in particular; but a tour in the military is always useful in shoving a young lieutenant into the thick of engineering, which helps build confidence. My plan was to remain in the Air Force as a career, but it didn't seem right to be in the Air Force and not be a pilot. In one of our career encouragement opportunities, General Bernard Schriever, the Commander of the Air Force Systems Command, asked whether I was going to stay in the Air Force. I explained that pilots seemed to get the best jobs, and if I couldn't get one of the best jobs, I would probably get out.

After three years in the Air Force, I returned to Sandia National Laboratory in 1959. Then in 1961, to broaden my experience outside of aerodynamics, I went to work for the Naval Weapons Evaluation Facility (NWEF), also in Albuquerque, as a weapons system engineer. Then my goal changed from aeronautical engineering to operations research (OR), so I left NWEF in 1966 and took a job with the Joint Chiefs of Staff's (JCS's) Joint Task Force II (JTF II), my first real OR job.

Greg Keethler: What led you into OR? How did that happen?

Marion Williams: It was sort of evolutionary. When I was at Sandia I was working on aerodynamics, looking at a small piece of the problem. But I didn't know how a weapon coefficient of drag contributed to the overall problem—how it affected accomplishing the mission. I kept thinking that there has got to be something bigger than this, and during that time OR was just coming back into vogue. During World War II it was a big area, but then it seemed like it went away; at least I wasn't aware of it as a career field.

Greg Keethler: That was in the late 1950s?

Marion Williams: That was in the early 1960s when I started doing some reading on OR and thinking that it would be great to get into that field. When I was still at Sandia, I read an article about Chance Vought Aircraft putting together an OR organization to help design aircraft and weapons. The article discussed OR techniques and some of the operations analysis conducted during WWII. I went to Chance Vought to talk to their OR group, and started reading OR textbooks. It was then I found out that Sandia had a very small OR group, so I talked to the director there to see if I could switch from aerodynamics into OR. He said I could do that, but I needed to understand that everyone in the organization would have a PhD but me; at that time I only had a bachelor's degree. He said there would be some challenges there. I thought the challenges were too great, so I didn't move, but did keep thinking about OR. However, OR was more of a hobby for me until I went to JTF II.

JTF II was a new technically based organization headed by then-Maj Gen George Brown, who later became the Air Force Chief of Staff, with a Navy Rear Admiral deputy and a number of high-level military officers and civilians. JTF II was established because we didn't understand the ability of aircraft to survive when flying at low altitudes: How low can airplanes fly? What's the effect of multipath and clutter on radar tracking? Can airplanes effectively and safely penetrate at low altitude? It was a whole area—offensive and defensive—that we didn't understand and couldn't predict.

Greg Keethler: What was the focus of the JTF II's work?

Marion Williams: It was focused on fighters, bombers, missiles—anything at low altitude. It even extended into medium altitude as well. But it was primarily an engineering/scientific/testing organization investigating effects such as multipath, clutter, radar scintillation, and human factors that affected low-altitude penetration.

JTF II was based at Kirtland AFB, and we did really large-scale field testing. We used Oklahoma, Arkansas, and Texas for our test range. We did penetration, low-altitude radar tracking, all kinds of different types of tests of weapon effectiveness and survivability. We had a series of tests planned over a 10-year period, with a bunch of top-notch statisticians from Sandia Laboratory to help do the test design and statistical analyses. We were really one of the first simulation-based organizations. We did a lot of hardware and computer simulation at that time, even in the late 1960s. The technically based part ran the organization—what we did and how we did things. It was a lot of fun and an ideal job. You walked in the office every morning saying. ''It just can't get better than this.'' We worked with the top statisticians in the country, and had the freedom to talk with experts, do research, take courses, and just figure out how you do a better job of testing, simulation, and statistics. It was great.

JTF II was one of the first organizations to effectively combine test and evaluation (T&E) with modeling and simulation (M&S).

Greg Keethler: You had been dissuaded from pursuing an OR position at Sandia because of the high level of education of the people there. Did you have a hard time getting into this OR analyst position?

Marion Williams: Not really. They were looking for people in OR, but also wanted people with an engineering background. Because of the shortage of trained OR analysts they were taking on engineers, even guys like me, to be trained as OR analysts.

Greg Keethler: Even Aggies.

Marion Williams: Even Aggies. At that time it was fairly easy to get into OR, and then get trained in OR techniques. There was a Navy Commander named Charlie Luff, who came from the Naval Postgraduate School (NPS) where he studied OR. My agreement with him was that if I went to work at JTF II, he would mentor me in OR.

Greg Keethler: So he was your mentor?

Marion Williams: He was my mentor, and he is still in Albuquerque, retired from the Navy. He is probably still working. He was the first person who helped train me in OR. I didn't understand all of it. I had done some reading but didn't have any formal education.

The main customers of JTF II products were in industry, where people used our models and data for weapon systems engineering and evaluation, and in weapons system employment planning. However, they weren't paying customers, and it's a bad situation when the customer

doesn't put the money into the product. All the funding came from the Department of Defense (DoD), and it was an expensive operation. Our budget at that time was on the order of $50–$60 million a year. In those days, that was a lot of money. But then there was a change in emphasis—either intentional or unintentional—from evaluation of "low-altitude penetration" to evaluation of Service weapon systems. We didn't last very long after that due to pressure from the Services; they didn't like the idea of a JCS organization becoming involved in testing of their new systems. JTF II was disestablished in 1968.

Greg Keethler: What were the greatest difficulties you faced back in your early days at JTF II? What was the equipment like?

Marion Williams: It was different. I recall I had this huge piece of equipment on my desk that was maybe 18 inches wide and two feet deep, and the most complex thing it could do was calculate square roots. It cost about $4,000. Most calculations were done by hand. It was computationally intensive. When I worked at Sandia in aerodynamics, calculations were done with paper spreadsheets, hand calculations, and a lot of double-checking. More than once, I had to go back and do an experiment again because I made a mistake in a calculation. But that was the worst part of it. The most fun was trying to apply theories to real data, to see where things really worked like the theories said, or things didn't work like the theories said. There were some pretty good working simulations at the time, even with hand computations.

Greg Keethler: Were there any problems in acceptance of the results by the decision makers, who didn't necessarily understand all these things?

Marion Williams: To be honest, that hasn't changed a lot over the years. The presentation is probably still as important as the basis for your analysis. If you can't get the point you are trying to make across, the analysis doesn't matter. But I think that we had more data at that time to make a specific point. At JTF II, if you wanted to know how low you could fly, we had real test data, with different airplanes, different pilots, experimental designs, with learning factors included. We could understand low-altitude flying and understand which factors had an effect. We started off with a statistical test design. We had to define our regression equation, define the factors, and define factor levels. We had to randomize the tests so they weren't biased. We would even randomly pick the next pilot to fly. Mathematical purity was a goal.

Greg Keethler: Some sort of factorial design?

Marion Williams: Yes, and fractional factorial designs. All those things were there, and you had some good theoretical guys to help out. We had some good tests where we could control the factors. We also had the money for instrumentation. With control and instrumentation, you can understand what happens and why it happens. An advantage then that we may not have now is that we don't have as much control because we don't have as much funding today.

Greg Keethler: Do you consider your work at JTF II your most important work in the early days?

Marion Williams: It was probably the most important from the standpoint of doing experimentation with the ability to really control test variables—the scientific approach to testing. I don't want to say it was the most important work; operational test and evaluation (OT&E) has the most impact. JTF II was a better training ground.

JTF II was a stimulating organization, and was beginning to evolve into a joint operational test organization when it was disestablished and I transferred to the Defense Nuclear Agency (DNA), also at Kirtland AFB, doing nuclear weapon reliability assessment.

Greg Keethler: For the nuclear weapon reliability assessments you did for DNA, were those largely theoretical or based on empirical studies?

Marion Williams: It was both. There are few opportunities to fully test a nuclear warhead—usually one or none. Reliability was based on combining lots of subcomponent and component tests with a very few system tests; to include detonation of the nonnuclear components, and maybe one underground test of the nuclear device itself. Combining component and system data provided an approximation of whether it would work as intended, but there was no theoretical way to give more credence, or weight, to system data over component data. Bayesian statistics provided a way of combining

data at different levels to get an assessment of reliability—the probability that the system will detonate at its full nuclear yield when triggered by a fuze. There were two schools of thought in reliability assessment: the classical approach and the Bayesian approach. Unfortunately, even the basic concept of confidence levels has a different meaning in each camp, and technical discussions become emotional very quickly.

At DNA I was applying Bayesian statistics to nuclear weapon reliability evaluation, which was fun. With JTF II, I had worked with a number of classical statisticians; but the emphasis in nuclear weapon reliability at that time was on Bayesian statistics. There are fundamental differences between classical statisticians and Bayesian statisticians.

Greg Keethler: There are always arguments with the Bayesian guys.

Marion Williams: Oh yes. Engineers make good Bayesians; classical statisticians don't make good Bayesians.

Greg Keethler: Philosophically speaking and practically speaking, how do you view the applicability of Bayesian vs. classical statistics in the T&E community?

Marion Williams: With my limited experience in both, I viewed Bayesian as an engineering approach to statistics and classical statistics as a mathematical approach. Bayes' theory gave a way to combine data at different levels, weighting system data more than component data, which seemed to make sense, although the weighting scheme was somewhat built-in rather than controlled. The Bayesian approach requires a prior distribution—a representation of a belief about reliability before getting any data. The initial prior is usually a uniform distribution, representing equally likely values. One of my classical statistician friends, Dr. Richard Prairie of Sandia Laboratory, demonstrated mathematically that a uniform prior introduces a bias in the answer. That fueled the debate, but didn't help convince either side.

Greg Keethler: How do you argue for Bayesian approaches when talking to a ''nonbeliever''?

Marion Williams: I never found a good way to do that. We had several meetings between Bayesians and Classicals trying to find a common ground. We just argued and never resolved our differences. Those same arguments probably still go on.

I enjoyed statistics, but kept thinking about OR. Statistics is a basic skill in OR, and I considered pursuing advanced degrees in math and statistics, since many of the people I had worked with at Sandia were world-class statisticians—-George Steck, Dick Prairie, and a number of others. Still, OR was my career of choice. I had worked part time to get a master's degree in engineering, but the local university didn't offer a degree in OR. So I went back to school to get a doctorate in OR at Oklahoma State University (OSU).

Greg Keethler: Sponsored by the government?

Marion Williams: Yes. It was one of those really good deals. I was in the DNA where there was a lot of work in the mathematics of reliability, and they saw a need for more education. So I talked them into letting me go back to school for one year.

Greg Keethler: One year? That's all?

Marion Williams: One year—that's all the time they could afford; anything beyond that was on my own. I did all the course work on campus in a year, and then did the dissertation in Albuquerque applied to the work I was doing at DNA. So it worked out pretty well.

Greg Keethler: That was at Oklahoma State University?

Marion Williams: OSU in Stillwater, Oklahoma. The reason I went to OSU was that several statisticians I worked with at Sandia Laboratory studied there. OSU was an applied engineering school. I visited OSU, talked to the professors and explained my interests. I was impressed, and they accepted me as a doctoral candidate. My only question was how long it would take. They would never give me a direct answer. ''Just enroll and we will see how well you do.''

Greg Keethler: You mentioned a master's degree. Did you complete that before you started on the PhD program?

Marion Williams: Yes. I went to night school part-time for six years to get a master's degree at the University of New Mexico (UNM) in mechanical engineering. OR wasn't offered at UNM, and neither were aerodynamics or aeronautical engineering advanced degrees, so the nearest thing was mechanical engineering.

Greg Keethler: What was your dissertation topic at OSU? How was it related to your work at the time?

Marion Williams: My primary interest at OSU was mathematical programming, linear programming, dynamic programming, etc., as an approach to optimization. My dissertation was on a technique called ''recursive search dynamic programming,'' a method for mathematical optimization. My only application of that technique was in a trade-off analysis between nuclear hardening and bomber dispersal, completed when I was on Air Force reserve duty, which probably never saw the light of day.

I went back to work at DNA in 1970. However, I still had a desire to get more into OR. After a few years doing nuclear weapon reliability assessment, Charlie Jacobs, a friend from JTF II who had worked for the Air Force in OR, called me and suggested that I consider an OR job as principal scientist at the Supreme Headquarters Allied Powers Europe (SHAPE) Technical Center (STC) in The Hague, Netherlands. It was particularly attractive since it was an operations research position in an international organization.

It was an Air Force position assigned to SHAPE. At that time, OR was strong in the Air Force. There was a civilian organization in the Pentagon—before Air Force Studies and Analysis—heavy in OR studies named the Air Force Operations Analysis (their office symbol was AFGOA). It had guys like Clay Thomas, who was an icon for Air Force OR. At that time, OR was almost a clique within the Air Force and really hard to get into. There was an Air Force Colonel in the Pentagon, Colonel Calafato, who was the key. If he OK'd you, you could get into OR anywhere in the Air Force; if he didn't, you were dead in the water.

Greg Keethler: Much more centralized than it is now?

Marion Williams: Oh, much more so—or at least that was my perception. The group in the Pentagon seemed to be the center of Air Force OR. There were also OR organizations at Tactical Air Command (TAC) Headquarters (HQ), Strategic Air Command (SAC) HQ, Nellis AFB, Eglin AFB, United States Air Forces in Europe (USAFE), Pacific Air Forces (PACAF), and many other key commands. There was an OR network within the Air Force. Cooperation among those OR organizations was outstanding; at least that's the way it looked to those of us on the outside.

Greg Keethler: And this guy controlled all of that?

Marion Williams: Colonel Calafato seemed to be the ''keeper of the keys'' for civilians to get in those organizations. I went to Washington, DC, and talked to him. We got along pretty well. I applied for the job at STC and was accepted. My boss at DNA agreed that it was a job I couldn't afford to turn down, so my wife, two daughters, and I moved to The Hague, Netherlands, in 1971.

STC was an international organization with about 100 scientists from the North Atlantic Treaty Organization (NATO) countries. The Air Force provided about 15 US civilian slots. You worked for the Air Force on loan to SHAPE for a three-year period.

Greg Keethler: They still do that?

Marion Williams: Yes, except now they don't work for the Air Force. They work directly for SHAPE. You are paid a NATO salary, and you get benefits such as paying no income tax. When I was there you paid all the US income taxes.

Greg Keethler: So you were not part of the civil service when you worked for them?

Marion Williams: We were still US government employees. NATO paid the Air Force, the Air Force paid us. We were assigned to the American Embassy in The Hague, so we had Post Exchange (PX) privileges in Europe, and our kids went to the American schools there. There were a lot of benefits.

Greg Keethler: What was one of your most memorable projects or experiences while you were at STC?

Marion Williams: Most of the work involved studies using computer models. I did one study for the British and Dutch on attack of airfields; looking at the tactics that would be effective and also ensure survivability of the aircraft—factors like speed, altitude, electronic countermeasures (ECM), and so forth. That became very interesting because the US Air Force believed in ECM, but the British pilots weren't so sure. We were briefing a group of British and Dutch pilots on the study results, showing predictions of survivability. I explained that the analysis showed that

they should fly at medium altitude and trust their ECM. One British pilot commented that he would prefer to fly at low altitude and trust his luck.

Greg Keethler: How long were you at STC, and what did you do following that?

Marion Williams: I went over in 1971, came back in 1974—a three-year tour. STC wasn't someplace where you stayed for a career. They wanted people to come in, do three years, and then go back to their own organizations.

The memorable experiences were just living in Holland. We lived on the economy, had Dutch, German, English, and American friends, enjoyed the food and travel, as well as the normal Dutch living with weekly visits from the flower man, bread man, chicken man, etc.

When it came time to return to the US, my old job was gone. The DNA reliability office had been abolished while I was in Europe, so I had to find another job. I had an offer to return to Sandia Laboratory, so that was initially my first choice.

Greg Keethler: Now you could ''fit in'' at the Sandia OR Group, because you had completed your PhD?

Marion Williams: Yes, I thought I could compete with the other PhDs. However, as I started moving back to Albuquerque to work for Sandia, I got a letter from the Air Force saying, ''By the way, the Air Force sent you to Europe and brought you back, so you owe the Air Force one more year.'' There was no way to get out of that, so I went to the newly established Air Force Test and Evaluation Center (AFTEC, which later became the Air Force Operational Test and Evaluation Center [AFOTEC] in 1983) at Kirtland AFB. AFTEC and the Office of Aerospace Studies at Kirtland had openings. I decided to go to work for AFTEC for a year, then I figured I could still go back to Sandia.

Greg Keethler: Who was the Commander of AFTEC then?

Marion Williams: Maj Gen Richard Cross. Lt Gen John Burns, the first AFTEC Commander, had just left.

I planned to stay for a year but found that it was a lot like JTF II … a fairly small organization with a new mission. The plan was to grow to 208 people, with six or seven OR analysts. It grew way beyond those expectations. It was a lot like JTF II in that it was new, exciting, innovative, and a lot of fun. So I never went back to Sandia.

Greg Keethler: Were you brought in as a line analyst, or were you in charge of other analysts?

Marion Williams: I was a line analyst, a GS-14.

Greg Keethler: Who was in charge?

Marion Williams: Colonel Bobby Dunagan was the head of the operations analysis (OA) directorate at AFTEC. One of his rules was he didn't see any need for a computer at AFTEC because we could do what we had to do with slide rules and hand calculators. He changed his mind later on and we used computers, of course. But he was the first colonel in charge of analysis.

Greg Keethler: Was there anything particularly relevant from your education, either bachelor's or master's, and ultimately your PhD, that served you well when you first started doing OR?

Marion Williams: I think that statistics really helped. Mathematical programming and optimization theory also helped. Probably as much as anything was the way you thought about a problem—decomposing it and providing a structure. In testing we do that through definition of issues, objectives, and measures of effectiveness (MOEs). The structured thinking helped a lot.

Greg Keethler: Give us your perspective on statistical design of experiments. In your experience, which kinds of designs work best for which applications?

Marion Williams: My dyed-in-the-wool classical friend at Sandia, Dick Prairie, was an expert in experimental design, and had forgiven me for my Bayesian transgressions, so I asked him to give a course in design of experiments (DOE) for AFOTEC analysts. It was a practical approach to experimental design, with hands-on problem solving. Operational testing is much more complex than the controlled conditions of most analysis problems, although the principles directly apply. Our vision was to turn operational field testing into a controlled experiment; much like the testing we accomplished during my time at JTF II, where we had 50 or so Sandia engineers and statisticians supporting test design and analysis of large-scale tests to better understand low-altitude penetration tactics from both an offensive and defensive standpoint.

Although we made good strides in applying the theory to field testing, as the design moved from the test planners to the test executors, the influence of test design seemed to get lost in the practical constraints and limitations of field tests. It worked well in controlled tests using hybrid simulation; it worked less well in tests where operators and tactical decision makers made on-the-spot decisions based on somewhat random events. I have since learned that I needed to know more about complexity theory.

Greg Keethler: When have you successfully used full factorial, fractional factorial, central composite designs, Latin squares, etc.? And conversely, which ones have you seen fail miserably in which applications?

Marion Williams: Full factorial was seldom a choice except in simple tests, so fractional factorial was the design of choice. There were few catastrophic failures or blazing successes in either case, although fractional factorial design had the problem of confounding, so some interactions were not accounted for; and we never knew for sure when it made a difference.

Greg Keethler: It seems like most of the people you worked with at JTF II and at AFOTEC were very quantitatively oriented. What were their backgrounds?

Marion Williams: Primarily engineers and mathematicians. There were very few OR analysts. I think when AFTEC first started, there were six or seven analysts and a director. It didn't take long to grow to 30 or so analysts; still mostly engineers with OR titles. There were four analysis divisions. Each division had five or six people, so we had 20 to 30 people in the directorate, with Bobby Dunagan as the director. One of the reasons for getting into OR was a need for people in those fields—and it continued to grow. Later AFOTEC had about 100 analysts in the directorate.

Greg Keethler: There were some significant organizational evolutions in AFOTEC and I'm sure you had a big hand in those because of your position. Could you talk about how you had to alter the organization as the demands of the Air Force grew?

Marion Williams: When AFOTEC was created, the head of the Operations Analysis Directorate thought that six analysts would be plenty. Maybe a dozen at the most, because all they had to do was tell the test team how many times they needed to run a test. And there was probably no need for a computer. As the number of test programs grew, we found that testing is a complicated process. With time, the number of analysts grew and we even bought a computer.

Greg Keethler: That was SA—systems analysis.

Marion Williams: Yes, with about 100 people. Later we established another analysis organization doing the logistics evaluation. Initially they were a part of the analysis organization, but then we split that off into the LG (logistics) organization; also about 100 people addressing suitability. They did a lot of modeling using component test information and the series/parallel relationships. Component data and a reliability model allowed them to use reliability data and maintenance data to estimate metrics such as mean-time-between-failure, mean-time-to-repair, and sortie rate.

Greg Keethler: Using discrete event modeling?

Marion Williams: Yes, that's right.

Greg Keethler: What is that model that the loggies had?

Marion Williams: Dick Gunkel was the person who brought LCOM—the Logistics Composite Model—to AFOTEC. It was an existing model in use at several organizations. LCOM was used to estimate not only sortie rate, but number of maintenance personnel required, number of spares required, and other metrics to help determine the manpower and funding needed to support the system when it became operational.

That was a huge effort and that came about because Lt Gen Howard Leaf, AFOTEC Commander at the time, was very concerned about logistics. Although we were doing a decent job of estimating the operational effectiveness of a system when deployed, we weren't doing well in estimating suitability and what it would take to keep a system operating.

Bob Sheldon: Did the logistics analysts at AFOTEC address the issue of reliability growth, where the reliability improves over time?

Marion Williams: We tried to do assessments of reliability growth. One of the big problems was with the LANTIRN (Low Altitude Navigation and Targeting Infrared for Night) program with Colonel (later Lt Gen) Jim Fain as the program manager. He was a very aggressive

program manager. He initially had a very low reliability but projected a large growth that would meet production requirements by the time it was deployed. We said, ''You can assume you can run a one-minute mile, but you know you're not going to get there.'' [*Laughter*]

We really didn't know how to project reliability growth; all we had were contractor plans for improvement with no hard data.

Greg Keethler: We had a team of software analysts in AFOTEC for a while. They did some analysis that related delays in development to number of lines of code in software. With Maj Gen Cliver and Dr. Williams' endorsement, we took that up to the Air Staff. Darleen Druyun was the person in charge at that time. They did not want to hear that story.

Marion Williams: Correct. We looked at the lines of code and analyzed the structure of the code. How easy is it to maintain? How easy to change the code? Is the code well documented? The software evaluation group that worked for Greg did a great job.

AFOTEC increased the number of analysts in the Operations Analysis Directorate and in the Logistics Evaluation Directorate to about 200 analysts—100 in each. Unfortunately, due to manning and funding cuts, the number of analysts in the Headquarters had been drastically reduced; the Logistics Directorate and the Software Evaluation Group were eliminated.

Greg Keethler: Haven't they pushed a lot of that out to the detachments (dets), now?

Marion Williams: Most of the analysts are now in the dets; many are contractors.

Greg Keethler: All the dets wanted analysts out of the headquarters and at the dets, but then when I went out to the dets and talked to the analysts, they weren't doing analyst's work.

Marion Williams: They were doing project management work.

Greg Keethler: And administrative work and other things. So really it was a net reduction in analytical capability to push the analysts to the dets.

Marion Williams: Yes. Maj Gen Eichhorn tried to reverse that and bring analysts back into Headquarters—building it up as much as he could.

Greg Keethler: What drove having to set up the dets rather than do things from the Headquarters?

Marion Williams: Lt Gen Leaf was the one who started moving test teams to the detachments because we had testing being conducted in several locations. Creating dets in different operating locations increased the efficiency of those test programs by being located at the test site. Eventually we had dets in several locations in the US and one in Germany. The Detachment Commanders handled care and feeding of the test teams in their deployed locations.

At one time, the Headquarters wrote the test plan, then gave it to the dets and they would execute the test. Then it would come back to Headquarters for analysis and report writing. That has changed over the years until at one point most of the planning, execution, and analysis was being done at the det. The analysis process has evolved with the evolution of the analysis organization—from very small to rather large back to small again.

Greg Keethler: When you were at AFOTEC, who were the influential actors in military OR in terms of people or organizations?

Marion Williams: Before Air Force Studies and Analysis there were two analysis groups in the Air Force, a civilian and a military group. As I understand it, those groups were merged and became the Air Force Studies and Analyses Agency (AFSAA). I started in OR after Studies and Analysis was formed. Clay Thomas and Sylvia Waller were the two I had the most dealings with. Sylvia was the chief scientist at AFSAA until she retired and Clay took that job. We worked on a number of programs where AFSAA supported AFOTEC using their computer models. The A-10 was an example. We did the test; they did the survivability analysis using our test data. The Advanced Medium-Range Air-to-Air Missile (AMRAAM) Operational Utility Evaluation was another. Captain Greg Keethler was the analyst on that program.

Joint Test and Evaluation had a lot of influence, and was a lot larger then. A joint test had about $l00 million in funding over a three-year period, along with a fairly large number of Air Force, Army, and Navy personnel. Walt Hollis was at the Army Operational Test and Evaluation Agency (OTEA) at the time—now called the Army Test and Evaluation Command (ATEC). He was the chief scientist/tech director at OTEA. Lt Gen Glenn Kent had just retired

from the Weapons Systems Evaluation Group (WSEG) and was working for RAND Corporation. He was still a major influence in MORS and in OR. Maj Gen Jasper Welch was the director of AFSAA. Wilbur Payne was head of the Army analysis group at Training and Doctrine Command (TRADOC) Analysis Center (TRAC) at White Sands. We had a lot of cooperation between AFOTEC, Jasper Welch at AFSAA and Wilbur Payne at TRAC. There were some major joint tests on electronic warfare (EW) where the Army and Air Force analysis organizations supported the test with a lot of simulation. We had good cooperation. I think we were less stovepiped then than we are right now.

Greg Keethler: How about interaction with the Office of the Secretary of Defense (OSD) Program Analysis and Evaluation (PA&E)?

Marion Williams: We worked a lot with Tom Christie who was in PA&E at the time. There was a time when Christie's group at PA&E had oversight of OT&E. We briefed Tom a number of times on some of our OT&E programs, and he often expressed his concern about our tests and evaluations. The IIR (imaging infrared) Maverick missile was one. We had conducted operational testing to demonstrate system performance, but Tom was concerned about survivability of the airplane during the time the pilot was trying to locate and engage a target; especially in a cluttered battlefield. Tom insisted that we conduct some additional tests at Fort Polk, Louisiana, with burning tanks and IR countermeasures to evaluate whether the enemy guns and missiles could engage the aircraft while it was trying to attack moving tanks. PA&E had a strong hand in how we designed and analyzed tests.

Greg Keethler: What was the most essential part of your OR background?

Marion Williams: Just learning that the hardest part of any problem is the up-front thinking to find out the most important elements of any problem—how to figure out what the driving factors are in a problem, and how you analyze them.

Greg Keethler: How you learn what to ignore.

Marion Williams: That's right—defining what's important. It's the same now. The 80/20 rule (Pareto principle) applies: 80 percent of the problem can be solved with 20 percent of the effort—if you find the right 20 percent. If you can find 20 percent of the most important factors, you can solve 80 percent of the problem.

Greg Keethler: When did you go to your first MORS Symposium (MORSS)?

Marion Williams: That was in 1974. I was just back from the SHAPE Technical Centre and working for AFTEC. Colonel Bobby Dunagan encouraged me to attend a MORSS in Fort Eustis, Virginia.

Greg Keethler: How did you like that first MORSS?

Marion Williams: I really didn't understand the organization or the wide range of topics available in the different groups, but I was impressed with the variety of working groups. The technical discussions in the working groups really helped me better understand OR applications. The thing that impressed me most was having an opportunity to meet famous people such as Glenn Kent, Jasper Welch, Wilbur Payne, and the like. Later I also had the chance to meet others like Walt Hollis and Clay Thomas. Those were the visionaries; people who were too busy to talk to me during normal office hours but would spend time explaining policy and politics during MORSS coffee breaks and dinners. They had the long-term vision on how the DoD ought to be organized, what senior leadership wanted to do, how OR should be applied, and how T&E and OR could work together.

Greg Keethler: Did you meet any other notable MORS people at that first MORSS?

Marion Williams: I happened to be standing at the registration desk and saw the name tag, Marion Bryson. Having the same first name, we started talking and I learned that he was on the Board of Directors for MORS. I asked him how one could get involved with MORS. My question really was, ''How do I get a job like yours?'' My goal was to be a director; becoming MORS President was way beyond my dreams at the time. Marion explained that you start with a working group, then become the chair of a working group, and work up from there, helping to organize a committee and finally run a symposium. So I just started following that guidance. I started helping with the organization, and found it was more fun than just attending. The workers in the MORS office, the Board

of Directors, and MORSS attendees, are all outstanding.

Greg Keethler: What was MORSS like back then?

Marion Williams: A lot more informal, and a lot smaller. The attendance at the first MORSS I attended in 1971 was about 600. Later, when the number of symposia was decreased from two to one per year (with workshops), the attendance averaged around 1,000.

It was probably not a lot different from today, except for size. There are still the same common interests and the same enthusiasm for OR. The unique thing about MORSS is the ability to conduct classified discussions. The working groups were essentially peer reviews—analysts came to discuss their work, discuss the analysis, and debate their assumptions. It's the interaction between the audience and the speaker that makes the working groups really interesting.

Greg Keethler: Has any of that changed over the years?

Marion Williams: Somewhat. It varies with time. Quite often you get into a lecture where there is a presentation without much interaction with the audience. As MORS President, I tried to have rules to encourage interaction, such as no more than 20 slides in a briefing with more time allocated to discussion. The discussion is the most important part of a working group. We also encouraged staying in one working group throughout the whole MORSS; you didn't go from group to group. It wasn't required, but if individuals stayed with one group they got to know the people in that group. The first day you met the people, and by the third day you were arguing and debating with them about technical points. The learning of new techniques and the interaction among the working group made it interesting.

Greg Keethler: This phenomenon of everybody madly flipping through the MORSS program, picking out the different working groups to go to, that was different back then?

Marion Williams: You didn't move from one working group to another. You stayed with one group in your specialty. Of course, there were not that many working groups; there were probably half the number of working groups as now, so there were fewer selections at that time.

Greg Keethler: But it seemed well organized?

Marion Williams: I thought so. But one of the reasons we could always justify going to MORSS was not so much the organization as the opportunity to talk to Jasper Welch or Maj Gen George Harrison or Walt Hollis and other key decision makers. Those were real learning opportunities with leaders in the business.

Greg Keethler: In addition to the technical discussions, it was a good networking opportunity.

Marion Williams: Outstanding networking opportunities. And that is still the case. MORSS still is the place to go to meet people in your field. It was a place to get free advice, talk to people, get ideas, or solve problems. It was very beneficial and still is. All of this is in addition to exposure to new methods and problem solutions.

Greg Keethler: How did your involvement in MORS progress?

Marion Williams: It was really standard; I started off with a working group, first just listening, then making presentations.

Greg Keethler: Which working group?

Marion Williams: T&E. I had done studies, M&S, and the like, but OT&E was somewhat new. Later I became a chair of the working group, then chair of a composite group, then chair of one of the organizing committees. My big opportunity came when Dave Spencer, MORS President at the time, called to see if I would be interested in being the program chair for the 41st MORSS at Fort McNair, Washington, DC, in June 1978. That was just a few months before the symposium. The person who was to be the program chair had resigned. There was a lot to be done and a short time to get everything organized, but I didn't want to pass up the opportunity. It was hectic, but the MORS office and everyone on the other committees did most of the work. It was a good experience. I continued to work on different committees, then was elected to the Board of Directors. Later I was elected Vice President, then President—that occurred at the symposium in Albuquerque in 1982. According to Jim Bexfield, I was the second longest-serving MORS President at 15 months (Bex having been the longest-serving MORS President at 21 months).

One big problem I had during my presidency occurred when the Navy decided to withdraw their MORS sponsorship, and not send any Navy participants to the symposia. The Vice

Chief of Naval Operations (VCNO), ADM Hays, decided MORS was not all that beneficial, so they were not going to participate. I spent a lot of time talking to him.

Greg Keethler: What did you tell him?

Marion Williams: I tried to explain all the benefits for Navy participation in the symposia, and in having the Navy as a sponsor. Several of us tried to quantify the benefits of MORS, but it is hard to show a cost savings due to someone learning about a new tool or technique at a symposium. However, after a number of conversations, the Navy VCNO decided to continue Navy support to MORS.

During the same timeframe, the Army decided that two symposia each year was too many, while the Air Force still wanted two symposia each year. For a period of about two years there were continuous changes in the sponsorship and the number of meetings. It was interesting. We also started the mini-symposia (special meetings) and the workshops to address specific issues. MORS changed during that time—I think for the better.

Greg Keethler: What were your MORS positions before you became President?

Marion Williams: The officer positions were different from what they are now. I was on the Board of Directors, then Vice President for Symposium Operations, then President. Being a member of the Board of Directors and an officer involves a lot of work, but rewarding work.

Greg Keethler: Once you got involved in MORS, was there someone mentoring you, or was it your own initiative as you went along?

Marion Williams: Some of each. It provided an efficient way to talk to a lot of people about T&E and M&S at one time, and also do a lot of coordination on normal business topics. It was just a great opportunity to learn and get things done. Plus, the people who ran MORS—Ed Napier, Dick Wiles, Brian Engler, and of course Natalie Kelly and the other people in the office, were always a pleasure to work with.

Greg Keethler: What did MORS do for its members back then, and how does that compare with today?

Marion Williams: It is at least as good; maybe better now because the community is bigger. In some ways the community is more stove-piped now, even though we try to tear down the stovepipes and become ''joint.'' MORS provides a way to talk to people from different organizations, understand what they are doing, and take advantage of new ideas. There are a lot of good things in the OR community if you can just leverage, maybe even plagiarize, those ideas. One of the major benefits is the opportunity to understand what other people are doing, and take advantage of it.

Greg Keethler: When did MORS start doing workshops?

Marion Williams: It was during the period I was President (1982), when we went from two symposia a year to one symposium a year. I can remember asking Walt Hollis whether we could do only one symposium a year, but also have workshops. Walt said that if we tried to have a symposium and also a 500-person workshop, we would be cheating. However, one symposium and a small workshop would be okay. So we started having one MORSS and one workshop each year, and later went to multiple workshops.

I think MORS has had some very good workshops, but we don't always take advantage of them by fully using the products, such as using workshop recommendations to improve DoD analysis. I think simulation validation (SIMVAL) was one that made a difference. The attendees at SIMVAL defined the verification, validation, and accreditation (VV&A) terms that were later adopted by DoD. Later SIMVAL workshops defined the elements of VV&A. Although we still haven't solved that issue, the process is better understood. MORS had a workshop several years ago called ''How Much Testing Is Enough?'' It was sponsored by both MORS and the International Test and Evaluation Association (ITEA). There were some really good findings from that workshop. There were some penetrating discussions and good insights. The goal was to try to implement some of the workshop conclusions, but like many similar efforts no one has time to follow up.

Greg Keethler: Who should do that? Sponsors?

Marion Williams: It really should be the organizing committee. But it takes work and it takes time. Not many people have the time. Although carrying through with workshop recommendations is not easy, a lot of good things come out of them, so it ought to make a difference.

Part of the benefits of symposia and workshops comes from learning from others. But workshops designed to address a specific issue should result in something getting better.

Greg Keethler: Can you think of any things that MORS did back then that are different from what we do today?

Marion Williams: We had more intense discussions then, because of the smaller size of the organization. As an organization gets bigger, the time for discussions and interactions is reduced. There was also more time at the end of the day for talking about what we learned, and for asking penetrating questions. We probably argued a lot more then because it was seen as a peer review and a time to question methods and assumptions. When Maj Gen George Harrison was head of AFSAA, he insisted that there be no AFSAA preview of presentations for MORS since it was a time for discussing work in progress—not public relations (PR) briefings on finished products.

Greg Keethler: Do you have any recollections of things that you look back and laugh about from your MORS experiences?

Marion Williams: There were some things I didn't admit for a long time. One was the origin of SIMVAL—the MORS workshop on VV&A. I thought it was a successful workshop. M&S was growing in popularity, but a ''process'' for making sure a model is good enough for a specific application wasn't that well defined. However, SIMVAL didn't come about because of a technical need; it came about because of a desire to hold a meeting in Albuquerque during the Balloon Fiesta—a major event in Albuquerque. An unnamed person in the MORS office had said, ''Marion, we need to come to Albuquerque during the Balloon Fiesta. Can you think of some workshop we can hold out there?'' I talked to Jim Sikora, and we agreed that M&S was becoming more important, so a workshop on VV&A should be able to produce something worthwhile. And it did. We had attendees from all of the major study organizations, as well as Maj Gen George Harrison, the head of AFSAA. That workshop developed definitions and elements of VV&A that are still in use today. However, the workshop came about because of the Balloon Fiesta. As we were setting the agenda, I talked to Clay Thomas to get his advice. He said, ''Marion, keep your goal high but your expectations low. If you try to solve all the world's VV&A issues, you will be disappointed.'' That was good advice. We developed definitions and elements, and we had general agreement on the product of the workshop.

Greg Keethler: And they've stuck.

Marion Williams: And they've stuck. We had M&S experts at the workshop from the Services, RAND, and industry. The Army had a good start; we just expanded their definitions and developed the essential elements of VV&A. We then briefed the Army, Navy, Air Force, and OSD PA&E, and they adopted those terms. It is really important to have a common way of thinking about something like VV&A. MORS provides a forum for doing that.

Greg Keethler: So the Balloon Fiesta was the genesis of these breakthroughs.

Marion Williams: Yes, and we did it again at a Balloon Fiesta a number of years later. We have had several SIMVAL workshops. We haven't been as successful in getting good examples of VV&A. We have a good theory; successful applications of that theory are harder to come by.

Greg Keethler: Has MORS been good for the military OR profession?

Marion Williams: Oh, very much so. I don't know what we would have done without MORS. I may have a biased view because of a long association with MORS, but there is no way that you can read everything that is being written about OR. A better way is to attend MORSS and ask questions, or listen to presentations from DoD leadership and from peer groups. If I have any criticism, it would be that we tend to have more lectures than discussions. MORSS working groups should be for peer review of work in progress, with the briefer wanting to improve rather than sell.

Greg Keethler: Looking back on your involvement with MORS over the years, what stands out? Is it being President, is it SIMVAL, another workshop?

Marion Williams: I think SIMVAL was a big part of it, but it was also just meeting the people who attended, and the DoD leaders who came to speak. Where else would you get the opportunity to talk to the head of the Central

Intelligence Agency (CIA), Bobby Inman? Or Gene Woolsey from the Colorado School of Mines. Gene is a true OR analyst and teacher. At the time he had small classes devoted to innovative OR studies to improve industry and government applications. His students had to have passports to go anywhere, anytime. If they took on a project and saved the client at least a million dollars, they received a diamond pin. I think he had lots of students with diamond pins based on OR applications.

It was also possible to spend hours with one of my heroes—Lt Gen Glenn Kent—discussing his experiences at AFSAA and the Weapons Systems Evaluation Group. And meeting these people at MORS made it much easier to call on them for assistance or advice on AFOTEC business matters.

Greg Keethler: What concerns do you have about the future of MORS?

Marion Williams: Spending too much time on OR tools rather than the application of those tools in the context of an analysis. M&S isn't a solution; it is tool to help arrive at a solution. MORS needs to stress the importance of analysis, and the need to understand that importance. They need to put more emphasis on thinking about a problem and deciding what is important before trying to solve it. We are getting into a complex age, where systems have to interact with other systems. Nothing works by itself anymore; each system works with other systems, including off-board sensors and fused data. Interoperability is critical, yet hard to evaluate. The emphasis has to be on how well a system can accomplish its mission rather than how well the equipment performs. Mission-level evaluation is becoming more important, but the tools to understand and put threshold values on mission parameters are never easy – sometimes not even possible.

Developing analytical tools was an important topic in MORS. Several years ago, Walt Hollis asked the question, ''Why is it that the Services use different engagement models to address the same problem? The Air Force uses SUPPRESSOR, the Army uses COMO, and Navy uses WEBTAC. Each does about the same thing, but when you use an Army model, the airplanes all get killed. When you use the Air Force model, no airplanes get killed. Why can't we agree on one air defense model?'' Walt recommended that MORS help address that issue, and MORS sponsored the Air Defense Modeling and Simulation (ADMAS) workshop. Jim Bexfield and I cochaired the workshop to address that question. The idea was to take different applications, use the three Service models, and figure out which model did the best job. We had six different study applications: bomber penetration, air defense, self-protection, and some others. We tried to trace the MOEs back to the important factors in the model, and figure out how the model treats that phenomenon, and then what's right and wrong with the model. The idea was to pick the best model for those applications—hopefully the same model. However, before the workshop we were told that we could not recommend a common model for the Services. That was probably a good recommendation, but didn't answer Walt's question. We assessed all three models, showing the good and bad parts of each one, and wrote a report—and nothing changed. The Army still used COMO (at that time), the Air Force still uses SUPPRESSOR, the Navy may still use WEBTAC. The analysis got confounded with politics; still, it was a good idea.

Many of the MORS workshops addressed modeling and simulation, which generated considerable interest in the use of M&S to address testing issues; especially in OT&E. At the time, the primary guidance in OT&E prohibited the use of M&S ''as a substitute for testing.'' At the same time, there were many issues that couldn't be addressed with field testing, and there was also pressure to reduce the cost of T&E through the use of simulation.

There were numerous sessions between the Services and OSD on where and how M&S could and could not be used. One of the key issues in those sessions was model validation – comparison of model results with real-world results to make sure the model was accurate. Validation is a complicated problem that is still debated within the DoD. The MORS workshops that addressed the VV&A issue were a big step forward because they developed the definitions and principles. The key concept in model validation is the comparison of model results with data. However, getting real data for comparison isn't easy or cheap; so many organizations

adopted the use of subject matter experts' opinions instead of comparing to physics or experimental data. That is a risky approach, and we have a long way to go to get it right.

Greg Keethler: Before we go on to the next section, any other comments you might have on MORS itself?

Marion Williams: Just the fact that it was a lot of fun. MORS is a technical and social organization. You go there to meet the people, and to learn. Part of the entertainment at MORSS was—at one time—formal dinners with speakers.

Greg Keethler: How long ago was that?

Marion Williams: Not too many years ago—the 1980s or early 1990s. I can recall a dinner with a speaker on maritime law. It lasted about two hours. I'm not sure why we picked that topic, but it must have been of interest to someone. We also had Admiral Inman as a speaker, which was fascinating. In a chance meeting with Admiral Inman sometime later, he mentioned that during his talk at MORS, Ross Perot was waiting for him in his hotel to discuss some very current events regarding rescuing some personnel. Dinners were very formal and structured. After some years of that, MORSS dinners became very informal with seafood, barbeque, crab, and the like—and no speaker. That is much better because you can more easily move around and talk to more people. I thought it was Natalie's idea, but according to Natalie's oral history, it was originally Socky Solomon who started it with an Alabama pig pickin' in 1987.

Greg Keethler: I'd like to transition now to the subject of military OR as a profession. How does it compare now to what it was like when you first got into it?

Marion Williams: Tough question. I think at that time it was more respected as a profession. That probably isn't the right term, but it was viewed as a high-level problem-solving activity. There were formal OR groups in most organizations, a lot more than exist today. In the Air Force, there were ''chief scientists'' at operations analysis organizations at SAC, TAC, AFOTEC, and most of the other major organizations. Most of the Air Force OA offices have been abolished. We use analysis tools to gain insight and help draw conclusions, but the tools seem to be the focus rather than the analysis—and the analysts. Maybe we focus more on the process than on the product. Assumptions, approach, and conclusions don't seem to be as glaringly obvious in some studies as they were in the past, or at least as I remember them. Then again, it may be the same and my memory is faulty. Still, it isn't as evident that studies provide the insight that is needed to make decisions.

Greg Keethler: How does the typical military OR practitioner today compare to his or her counterpart back in the early days of the profession?

Marion Williams: Better educated now. I think that in earlier days we relied as much on mentors as on education. Current OR education seems to focus more on the tools than on the art of problem solving. Much of that has to come from selected mentors—those with vision and a sense of the Air Force, or Army, or Navy. The key was working for or knowing people with those skills, and the desire to talk about problems and potential solutions. People like Jasper Welch, Wilbur Payne, Glenn Kent, George Harrison, and others too numerous to name. And people who weren't analysts, but had vision and a sense of the military like Lt Gen Howard Leaf—one of my favorite people in the world, and the one who helped me most in my career.

Greg Keethler: Talk about Lt Gen Leaf. How did he influence you? How did he influence the overall T&E community?

Marion Williams: Lt Gen Howard Leaf was a rare leader. Working for him when he was AFOTEC Commander and then when he was Director of Air Force Test and Evaluation was pure pleasure. He had all the good leadership qualities of listening to his people (military or civilian), insight into tough problems, and a reputation that allowed him access to anyone at any level. He worked tirelessly to understand an issue, pick the right people to solve problems, and open doors to get things done. Any door could be opened by saying ''General Leaf asked that I talk to you.'' He also sought the opinion of others on important issues: the Air Force Scientific Advisory Board (SAB), the Director of Operational Test and Evaluation (Jack Krings, Phil Coyle, Tom Christie), individuals such as Lt Gen (retired) Glenn Kent, Dr. Jacques Gansler, or any one of several dozen other experts in their field.

His insight was evident when we were testing the E-4B. At the time, nuclear effects such as electromagnetic pulse (EMP) were not considered part of OT&E. Those issues were left to the laboratories. However, Lt Gen Leaf was concerned that the E-4B, with its long trailing wire antenna, could suffer damage in the event of a high-level EMP, so he asked what we planned to do as a part of OT&E. I explained that we simply didn't do nuclear effects analysis; we had no nuclear effects experts, and it wasn't part of our mission at AFOTEC. Lt Gen Leaf then carefully explained to me that it was an operational issue, and we had to do something about it. I think his words were something like ''Dammit Marion, do something!'' We found some nuclear experts, conducted a short study, and drew conclusions about survivability of the E-4B to EMP, with recommendations for additional tests to examine upset effects — transient effects that cause the computers to freeze and have to be rebooted. Lt Gen Leaf's insight was correct in that EMP effects were raised as a concern at the Defense Advisory Board where production decisions were made. Lt Gen Leaf took along a three-slide briefing we had prepared for him ''just in case.'' That short briefing covered the points of concern to the decision makers.

Later Lt Gen Leaf asked that I work on an expansion to the AFOTEC mission to include nuclear effects on new weapon systems. I again argued that it wasn't in our mission, we didn't have the right expertise, and the Air Force probably wouldn't even let us take on that mission. After another ''Dammit, Marion, do something!'' we did a study on nuclear effects evaluation in OT&E. Much to our surprise, it was enthusiastically approved by the Air Force, with more people and more funding added to AFOTEC for the additional tasks. I never again questioned Lt Gen Leaf's insight and judgment.

One of the highlights of my career was when Maj Gen George Harrison became the AFOTEC Commander in 1993. With Lt Gen Leaf at AF/TE and Maj Gen Harrison at AFOTEC, the test and analytical worlds were in harmony. Maj Gen Harrison would come in on Saturday mornings and we would draw on the whiteboard and discuss technical issues; later briefing Lt Gen Leaf to get his insight and support. Lt Gen Leaf would sometimes send us to brief members of the AF Scientific Advisory Board, and as many other general officers and senior leaders as necessary to ensure that we had the problem correctly defined and a solution that made sense. When there were still doubts, Lt Gen Leaf brought in Lt Gen Kent, or Harold Smith or Jacques Gansler or Norm Augustine or other well-known technical personalities to make sure we were on the right track.

Maj Gen Harrison and I had one specific problem in an area of special interest to Lt Gen Leaf—electronic warfare. Lt Gen Leaf worried because we didn't have a way to measure ECM effectiveness. There were no ''holes in the ground''; measurement of ECM effectiveness was normally ''reduction in lethality'' or RIL, which was in reality the increase in number of misses caused by ECM—related to survivability but not a direct measure of survivability. If an aircraft was particularity vulnerable to a threat system, an increase in the number of misses often still meant an unacceptable probability of survival. Lt Gen Leaf wanted a more meaningful measure. Captain Suzanne Beers (later Col/Dr. Beers) was assigned the job of building an ''elephant brief'' to explain the complicated problem of ECM OT&E—from the tale of the three blind men feeling an elephant, then each describing it differently. There was a lack of common understanding of the complexity of the problem. Suzanne briefed the results of that effort and we developed an approach for ECM OT&E using field testing, hybrid simulation, and computer simulation, with a lot of experimental results to provide data inputs.

However, the M&S required, in addition to operational testing, to evaluate the survivability of an aircraft being engaged by an advanced threat requires not only complicated models, but also extensive input data, including bistatic radar signatures. Existing models required major upgrades and bistatic radar signature data that weren't available. Years later, Dr. Frank Gray conducted tests in a hybrid simulation facility using radar data provided by one of the Air Force radar laboratories. Unfortunately Lt Gen Leaf had retired as Director of Test and Evaluation so we didn't complete the effort in time to answer his questions.

Another problem was ECM technique robustness (the impact of changes in individual

threat systems caused by calibration, alignment, and manufacturing tolerances) as well as counter-countermeasures. This ''round-to-round'' variation sometimes meant that an ECM technique tested against a threat system on a test range might or might not work against other threat systems of the same type on another test range; the implication being that it might not work in combat against that type of threat system!

Maj Gen Harrison funded some experiments where we flew an aircraft with an ECM pod against two threat systems of the same type at different test ranges—and got different test results! Although the ECM technique was effective against the threat system at one range, it was not effective against the same type of threat at another range. It was good news in that we confirmed our theory, but bad news in that we found an operational problem that had to be fixed.

The technical approach we thought would be required was to use computer models of threat systems to understand the impact of the variations that caused the robustness problem. We figured that the models could tell us which techniques were more robust (not affected by ''round-to-round'' variations).

Because of the implications of our findings, Lt Gen Leaf asked us to brief dozens of general officers, admirals, and all kinds of Senior Executive Service (SES) individuals and industry personnel, including members of the Air Force SAB. We briefed Corona (an Air Force general officers forum), the JCS Joint Requirements Oversight Council (JROC), the Director of Operational Test and Evaluation, the OSD Acquisition Executive, the Commander of TAC (now ACC, Air Combat Command), the United States Air Forces in Europe (USAFE) Vice Commander, and the Secretary and Chief of Staff of the Air Force as well as senior military and civilian leaders in the Army and Navy.

There were no disagreements on our findings from the DoD leadership nor from the technical community. However, it involved considerable M&S, beyond that normally used by an operational test agency. It required the cooperation of many organizations across the DoD and in industry; especially the Defense Intelligence Agency (DIA). Fortunately, there were many people in leadership and technical management positions who quickly understood the problem and wanted a rapid solution. Key individuals included Dr. Bill Lummus at the Missile and Space Intelligence Agency, Stassi Cramm and Rob Dunn at Edwards AFB, Jim Oliver at the Air Force Electronic Warfare Center, and Jerry Palon at Lockheed Fort Worth. There were dozens of others as well.

The overall strategy, as supported by Air Force leadership and approved by the SAB, was to use computer models of threat systems for the evaluation of robustness and improvement of ECM techniques. Those models would be developed by the Service intelligence agencies, consistent with their responsibilities of providing intelligence products. Instead of a pile of intelligence documents, their primary product would be models of threat systems that could be used by all test ranges and hybrid simulation facilities, as well as other applications.

The implementation of that concept is a story for another time. The point is that the influence Lt Gen Leaf had was evident in addressing this T&E issue. Lt Gen Howard Leaf was truly a legend. Everyone who worked with, or for, Lt Gen Leaf has similar stories of his insight and influence. There is not a single one of us who doesn't refer to that era as ''the good old days.''

Greg Keethler: The ''story for another time''—does that have to do with the Joint Modeling and Simulation System (JMASS) and the Joint Warfare System (JWARS)? Could you go ahead and tell us the story?

Marion Williams: Yes, it involved JMASS. As I said, we had problems with EW and concerns about the fact that some ECM techniques worked and some didn't, depending on factors such as manufacturing tolerances in threat components or time from last calibration. There was no way we could test against several threat systems of the same type to evaluate ECM technique ''robustness.'' The only reasonable way to attack the problem was to develop models of the threat systems, simulate system variations, and use the model to assess whether the ECM techniques would work against all potential variations.

JMASS was a modeling technique and an architecture that could be used to mathematically represent a threat system, as well as blue

aircraft and ECM techniques. We thought it was ideally suited to address the ECM technique robustness issue. To prove the point, we asked for and received several million dollars from OSD to work with the intelligence agencies to develop an engineering model of a specific threat system and use the model in our ECM testing. Lt Gen Leaf asked the SAB to review the process we used as well as example problems where we demonstrated JMASS in tactical engagements. If we could prove that JMASS was valid in demonstrating ECM effectiveness, then we would recommend developing JMASS models for a range of threat systems and use those models to supplement open air testing.

The SAB concurred in the use of JMASS, but in Lt Gen Leaf's usual mode, he asked us to brief a long list of general and flag officers in the three Services, as well as OSD leaders, to make sure we hadn't missed something. We didn't encounter any technical issues, but there were some political issues since we were recommending processes and models that were different from those being used by the Services at the time.

Greg Keethler: Was that where the threat models would be standardized and there would be a single threat model?

Marion Williams: Yes, it was assumed that we could develop a model of each threat system (such as the SA-8) that could be used for a range of applications across the Services. We met with the Army and Navy, primarily the Naval Research Lab (NRL), to get them to adopt that philosophy so they would use those models.

Then, at Lt Gen Leaf's direction, we talked to the DIA about getting funding for the threat models. The DIA director said that they could not fund model development, but would fund the maintenance of the models, including model updates. The Air Force agreed to fund model development, and established a program element for that purpose.

Greg Keethler: You used JMASS for that?

Marion Williams: Yes, we used JMASS as the architecture. The concept was to get the appropriate scientific and technical intelligence center to develop threat models, the program offices to develop blue models, and develop common models for atmospheric and terrain effects—all in JMASS, which used common methodology, primarily MATLAB and Simulink. Lt Gen Leaf owned the program element for JMASS. He wanted to use that architecture since it was already being developed. We met with Air Force, Army, and Navy test ranges and hardware-in-the-loop (HWIL) facilities to get them to use the JMASS threat models. For example, the fly-out model for the SA-8 developed to evaluate ECM technique robustness could be used on the Nellis and China Lake test ranges as well as the Air Force Electronic Warfare Environmental Simulator (AFEWES) and other HWIL facilities. When a threat system changed, the fly-out model would be modified by the cognizant intelligence agency and ''plugged-in'' at the different test ranges and HWIL facilities. Those facilities developed ''cradles'' to accept the common JMASS fly-out models.

It was a great concept. Otherwise, each test range and HWIL facility would have different configurations of the same threat system, requiring the intel agencies to provide updates to each one individually. With the common model concept, the intel agency could make one modification and plug it into the different test or HWIL applications.

Bob Sheldon: What was the timeframe for that JMASS work?

Marion Williams: It was in the 1990s, when Lt Gen Leaf was in the Pentagon as AF/TE. JMASS was initiated as an OT&E tool, but had a broad range of applications as long as the users adopted the JMASS architecture. The basic idea partially came about because Lt Gen Leaf found that, for any specific threat system, hundreds of threat models were being developed by different Service test agencies and hardware contractors. Lt Gen Leaf would often ask why each organization needed its own model of the same threat since the threat didn't care if it was engaging an Air Force aircraft or a Navy aircraft or an Army helicopter.

The JMASS concept was valid; the primary issue was that it was designed to solve the narrow problem of T&E of EW: building threat models, correlating the models with threat hardware, then evaluating whether ECM techniques used against those threats would be effective. However, some people in DoD assumed JMASS could also serve as the engagement-level model for all applications, supplementing the Joint Simulation System (JSIMS) as the training model

and JWARS as the campaign model. The management of JMASS also became an issue. The intel customers, those building threat models in the JMASS architecture, faced continuing changes in the JMASS architecture as they tried to develop compatible models.

Even without reaching a steady state in JMASS architecture improvement and model development, the perception that JMASS was to be a model for all engagement-level analysis problems grew.

Greg Keethler: They started mandating it.

Marion Williams: OSD mandated it. You started seeing pyramid slides in briefings depicting JWARS, JMASS, and JSIMS as the integrated M&S solution to any analysis problem from engineering-level one-on-one models to campaign level. The implication was that JMASS engagement-level models would provide input to JWARS campaign-level models, with physics-based models providing inputs to JMASS where required. In some cases, that was true; for example, terrain models were used to develop inputs for terrain effects. However, it was not a universal process.

Greg Keethler: It was a PowerPoint process.

Marion Williams: For the most part it was a PowerPoint process. But there are still good examples of developing and using common models, as long as analysts recognize that they have to ensure that the model is appropriate for their specific application: the accreditation part of VV&A. Remember ESAMS (Enhanced Surface-to-Air Missile Simulation)?

Greg Keethler: Yes.

Marion Williams: The DoD and contractors still use ESAMS models, developed by Sam Baty in the early 1980s. They were good functional threat models used by several test range and HWIL facilities. We used ESAMS models for the B-2 when AFOTEC first got involved in classified programs. One of the big B-2 issues was survivability; something you can rarely address in operational field testing. We knew M&S would have to be a big player.

I went to Northrop to see how survivability requirements were developed, and how they used M&S in their design studies. If we knew what factors made the B-2 ''highly survivable,'' we would have a better idea about how to test those factors in OT&E. Maybe we could even use some of their models in test planning. I asked if they used models, and if we could use them. They said, ''Yes, we use models and no, you can't have them because you didn't pay for them in the first place. And, even if we could give them to you, the people who used the models have moved on so we don't have anyone with the expertise to explain the models and to train you in their use.''

We decided that we would build our own M&S capability using ESAMS as threat models in an integrated architecture to evaluate B-2 survivability. We talked to the operational user, the Strategic Air Command (SAC, which later became STRATCOM [Strategic Command]), to get their okay. They agreed with the proposal, and also wanted AFOTEC to help them in building M&S tools for evaluating employment options, such as penetration route planning. They gave AFOTEC about $10 million to build a computer simulation of the B-2 engagements to support OT&E and also support their operational employment assessments.

Bob Sheldon: When was that?

Marion Williams: I think it was in the late 1980s. It was before the B-2 was even acknowledged as a program. At that time, we weren't even allowed to talk about the B-2. Our testers would have to go to locations under a guise of some other program because we couldn't talk about what we were really doing.

To evaluate B-2 survivability, the Special Test Directorate at AFOTEC developed a ''cradle'' that would take different threat models, such as ESAMS, to examine B-2 penetration of an integrated air defense system (IADS), where the B-2 was represented by those characteristics that would affect survivability, such as radar cross section (RCS). The cradle would essentially coordinate all the simulated actions of the B-2 and the threat systems as the aircraft penetrated the IADS.

Bob Sheldon: You had a B-2 RCS model?

Marion Williams: Yes, along with flight profiles and environmental factors such as terrain. ESAMS provided the surface-to-air threat models.

Greg Keethler: Who wrote the programs? The models?

Marion Williams: The AFOTEC contractor developed the cradle that accommodated existing

ESAMS models as well as the B-2 flight profiles, RCS, and other factors.

Greg Keethler: Was it a det or was it the operations analysis organization?

Marion Williams: It was the Special Test Directorate.

After completing the simulation and conducting field tests with the B-2, we had a two-part briefing on the results for participants and other interested agencies. The morning session was devoted to the field test results—one B-2 vs. threat simulators on the test range, with the afternoon session to discuss the model results—penetration of an IADS.

We had a huge crowd for the test results, but when we talked about the model results, most of them left. They wanted to know how well the B-2 performed; there was less interest in how the model said the B-2 performed.

However, it got AFOTEC involved in classified programs, many of them involving low observable (LO) programs. LO was a new technology to the testers, so we went to MIT Lincoln Laboratory and asked them to provide some training in LO technology. They were the experts in the field; we figured that we needed some basic training on the principles in order to conduct efficient OT&E programs. Lincoln Laboratory set up a course at no cost to us. We sent a test team of about 20 or 30 people to Boston for special training.

Greg Keethler: I know there were some unique tests, nontraditional things like the AMRAAM operational utility evaluation (OUE) that AFOTEC got into. What were your feelings on those kinds of things?

Marion Williams: OUEs were nontraditional but useful efforts to assess the potential utility of new systems such as the AMRAAM. Initially there was a concern about AFOTEC conducting OUEs because it didn't involve real testing; it was a simulation to demonstrate the utility of AMRAAM characteristics: Can we use a launch-and-leave weapon; can the pilot handle the workload; and would the AMRAAM be more effective than current air-to-air weapons? It wasn't a test of the AMRAAM, but an assessment of the concept to assist in the decision on whether to build the AMRAAM. It was something unique to AFOTEC, and not without controversy since it really wasn't a test.

Greg Keethler: And Congress put a condition that we answer those questions before they would fund it.

Marion Williams: That's right. We used the contractor's St. Louis dome facility to do the first AFOTEC OUE: evaluating what the AMRAAM could do by putting the design characteristics of the AMRAAM in a simulation to see if the pilot could take advantage of its new capabilities and improve air-to-air intercepts. That effort, with Capt Greg Keethler as the analyst, proved the idea of an OUE to understand something about a new system before it was available for OT&E. There were several OUEs conducted on complex systems after that. It initiated the idea of doing more analysis before having hardware for OT&E.

The Air Force approach to OT&E evolved over the history of AFOTEC. When AFOTEC (previously AFTEC) first started, we would be told of a program requiring OT&E about six months before the report was due. We didn't do a lot of long-term planning. As programs got more complex, we knew that we needed to start doing earlier planning, which meant getting involved in a program much earlier. We started doing early operational assessments (EOAs), operational assessments (OAs), and OUEs.

We called it ''evaluation using other people's data.'' You look at the concept of the system and what happened in contractor testing and Air Force developmental testing to identify, from an operational standpoint, things that could cause problems in operational employment. One of the classic examples was a battery to be installed under the ejection seat in one of the airplanes scheduled for OT&E, which required that the ejection seat be removed in order to change the battery. The AFOTEC maintenance group looked at the design and pointed out the need to relocate the battery to speed up the maintenance process. This was done before design was finalized.

It was the maintenance people, the chiefs and airmen, who would point out problems so the design could be changed before the system was put into production. A panel with screw fasteners was in the design for one aircraft, where the panel had to be removed and replaced for quick turnaround. The maintainability operators

recommended using quick release fasteners instead of screw fasteners ''to get the panel off and on quicker.'' It was a simple fix to increase sortie rate that would require major changes if discovered during production. It was a different way to look at the responsibilities of an OT&E organization.

Greg Keethler: Any other unique test challenges that come to mind?

Marion Williams: Oh yes. One of the big issues on the F-22 was the ''twice as effective as the F-15'' criteria established by OSD. The F-22 COEA (cost and operational effectiveness analysis) used to help justify the F-22, indicated that it would be ''X times'' more effective than the F-15. I can't recall what the number was, but it was used as a rationale for building the F-22.

When it came to developing OT&E criteria for the F-22, OSD quoted the COEA regarding being more effective than the F-15 and decided that the F-22 ought to be at least twice as effective as the F-15; and demonstrated in the OT&E.

Greg Keethler: What does that mean?

Marion Williams: They didn't define what was meant by ''twice as effective'' but we assumed it must mean kill ratio (also referred to as loss exchange ratio—red losses divided by blue losses) because that's what the COEA used.

Greg Keethler: And the F-15 has a zero denominator for that, right?

Marion Williams: Or at least close to it. Until that time, the purpose of OT&E was primarily to determine whether the system was ''effective and suitable'' in a realistic test environment. Using kill ratio as an MOE was something new. We went to RAND, the agency responsible for the F-22 COEA, to see if there were particular factors (such as detection range) that drove the difference in effectiveness between the F-22 and the F-15. We never found the magic bullet, and OSD required that the twice as effective criteria be demonstrated in the OT&E. That required a new level of analysis for a field test, including things like real-time kill removal.

AFOTEC spent about $5 million for simulations of the F-15 and F-22 against common threats at Northrup's Phantom Works to better understand how to evaluate kill ratio in a field test.

Greg Keethler: You used real airplanes?

Marion Williams: Yes, in the OT&E. We had to keep the then-current F-15 available to test against the F-22. In addition, the acquisition language required that the test be conducted ''on a level playing field''—we couldn't give the F-22 credit for something the F-15 couldn't do.

It was a tough test, but comparing a new system's capability to an existing capability is a good way to measure increase in capability. Dr. Seglie, the Director, Operational Test and Evaluation (DOT&E) Science Advisor, pushed for comparison testing for all OT&E. However, the Air Force decided that the cost of baseline testing was more than they could afford; essentially doubling the cost of OT&E.

Bob Sheldon: What are your thoughts on small sample size tests, especially with respect to live fire testing? How did you address that?

Marion Williams: Small sample size is an inherent problem in OT&E. We tried different approaches to mitigate the problem of small sample size, including using Bayesian statistics to help analyze reliability. With Bayes theory we could combine component test results (from contractor or development testing) with system level data from OT&E to predict weapon system operational reliability.

The problem is the same as we found at JTF II; classical statisticians and Bayesians don't agree. Even the definition of confidence limits is different in the classical statistics community and the Bayesian community. We went to the OSR (Air Force Office of Scientific Research) to get their help in applying Bayesian statistics. However, we could never get agreement on the use of Bayesian statistics, so we didn't use confidence bounds in our briefings very often because classical confidence bounds get very wide with small sample sizes. Plus, some general officers don't resonate with confidence bounds.

Greg Keethler: Their eyes glaze over.

Marion Williams: That's right. That's why we used colors to portray ratings: Blue for great; Green for OK; and Red for not satisfactory. However, that often goes too far, and sometimes the audience doesn't really grasp the uncertainties in the conclusions.

There was a study by the National Research Council on OT&E and statistics sponsored by DOT&E when Tom Christie was the Director with a bunch of us from the operational test agencies working with them for about six months.

They insisted that testers explain test results in detail to the decision makers, including test uncertainties. We tried to explain that a briefer has 30 minutes to explain the results of testing a billion dollar system; there just isn't enough time to go into that level of detail.

Greg Keethler: What major changes have you seen over the years in methodologies, and how they are applied?

Marion Williams: There have been major advances, especially in computational capability. However, I don't think we have improved a lot in trying to tie actual data back into the models. We need to do better in pretest prediction and posttest correlation using models. We have tons of opportunity to do that, but it was always a secondary objective. It falls through the crack because it doesn't have a short-term payoff.

The use of M&S is different for different applications. In analysis, models provide insight into whether option A is better than option B. In T&E—especially OT&E—the question is more absolute: how well will system Z be able to accomplish mission X? We are interested in how well that system can accomplish its mission; not how well the model of the system can accomplish a mission. Models can, however, help plan more efficient tests and provide more complete analysis by investigating areas not possible in tests. Models help with the insight to allow more efficient testing and more complete analysis. However, the use of M&S in T&E—especially constructive models—is limited because most models aren't designed with a test application in mind. And most of the time, the testers don't have the time or the funding to make models more applicable to their test applications. Until system models are developed with the total acquisition process in mind—including the T&E part—T&E applications of models will continue to be fragmented and not very efficient. At some point in time, we ought to be able to use models for evaluation in areas where there is low risk, and then focus testing in areas where there is high risk.

Greg Keethler: Are you concerned about the tendency to focus on M&S?

Marion Williams: Yes. M&S is a tool and you can sometimes pick the wrong tool. Stassi Cramm, when she worked at the 412th Test Wing/EW, had a favorite saying: ''A fool with a tool is still a fool.''

Models have to be tailored for the application since there are no universally valid models. Analysts have to decide what is important, then select a model—or models—that correctly represents those important parts of the real world. In the Air Defense Modeling and Simulation (ADMAS) workshop, we had to understand what aspects of the real world were important for each application. For radio frequency (RF) detection, the RCS of the target must be adequately represented; and the requirement for modeling the RCS is different for semi-active missiles than for command-guided missiles. Each important factor had to be identified, then the representation of that feature in the model assessed. Analysis is the key.

Greg Keethler: As Clay Thomas used to say, ''Big A, and little M&S.'' I remember you spent a lot of time on M&S issues associated with the B-1 avionics system.

Marion Williams: That was because when Lt Gen Leaf was AFOTEC Commander he believed that M&S should be a tool to help address complex testing issues. As T&E became more complicated, issues came up that couldn't be addressed solely by field testing and he saw M&S as a solution to addressing some of those issues. One of the major test programs at the time was the B-1B. The Air Force had developed the ALQ-161 Defensive Avionics System for the B-1, but it wasn't very effective. Lt Gen Leaf said that each Service had systems that turned out to be less effective or less suitable than required. The B-1B ALQ-161 was the Air Force's program, the Navy had the A-12, and the Army had Sergeant York. Those programs resulted in more attention to OT&E and more pressure to make sure weapons worked properly when deployed.

AFOTEC was to test the B-1B Defensive System Upgrade to replace the original ALQ-161, and Lt Gen Leaf firmly told us that we were going to make sure it was tested completely, and ensure that it was an effective system. He saw M&S as a tool for that OT&E.

Greg Keethler: We were talking offline yesterday about how that was touted as the way to save time on the F-35 development. All this simulation-based development would save time

on the testing and it hasn't really worked out that way.

Marion Williams: In my opinion it really hasn't, because assumptions that were made in the model often aren't correct. We use models to make acquisition more efficient, but sometimes the models are wrong, and changes have to be made to systems already through the production line at a much greater cost than if those problems had been discovered through testing and fixed before production.

Greg Keethler: You have to slow down the production line.

Marion Williams: We have to slow down production, make fixes, and maybe delay deployment. We are finding that simulation is a really good tool for better understanding a problem. However, to make acquisition more effective, we want to use models for answers that will speed up acquisition. We have moved from models to gain insight to models that give answers on how well a system will work in combat. We need to be careful; models are helpful in understanding the bounds of our conclusions—that is, if we test at a certain altitude, models can help understand whether those conclusions would be valid at different altitudes, and can point out where additional testing is necessary.

However, in some cases field testing isn't feasible, such as firing real weapons at real airplanes. Simulation can fill the gap of knowledge in areas such as probability of kill—using a model to simulate the trajectory of a missile to its target and the damage that would occur if it hit the target.

But relying on models for test conclusions has risk. Credible VV&A can greatly mitigate the risk, but we aren't very good at doing VV&A in OT&E because few testers have the expertise, time, or funding to do it properly.

Greg Keethler: Do you see the pendulum coming back the other way, given what's happened with the F-35?

Marion Williams: I don't think everyone recognizes the problem yet.

Greg Keethler: So they haven't connected the dots. The fact that we have to slow down production to do more testing, they haven't connected that?

Marion Williams: Not yet.

Greg Keethler: We worked together in the late 1990s when the pressure was huge on simulation-based acquisition. I remember you as one of the very few, if not the only person at a senior level that said, ''I'm not so sure this is a great idea for OT&E.'' Did you find it difficult to take that position and did you get a lot of pressure to back off of that?

Marion Williams: Not really. Most people understood the risk. When Dr. Ernest Seglie was science advisor at DOT&E, we had a lot of conversations on how to use models. Ernie was not a proponent of models; he liked the quote that ''all models are wrong; some are useful.'' He was also a proponent of using hard data to decide if a new system was better than the system it replaced by testing the new system ''side by side'' with the old system. Models are less useful in those situations.

Greg Keethler: You've got to test the baseline.

Marion Williams: That's right. However, you would need side-by-side testing, which practically doubles the cost of a test. Or you have to use existing data on the old system to compare to test data on the new system. Using old test data seldom works because the conditions and assumptions in the original test are seldom captured and archived. It isn't that it can't be done; it is just that few organizations do it well.

On the other hand, if we could build models that were validated with test data, and maintain those models, they could be used to compare the new system with the old system. That was often a topic of conversation related to baseline testing, but it was never realistically examined. However, models need to be validated with test data, which is seldom accomplished.

AFOTEC often volunteered to gather test data for model validation if we had a customer. We fired a number of missiles against a drone and gathered data on missile trajectory, as well as other data. We sent messages out looking for anyone interested in the data for validation of models of the systems being tested. We even sent the missiles to MSIC (Missile and Space Intelligence Center) to do a characterization, along with weight and balance measurements to understand the variations. We had some good case studies to not only evaluate missile performance, but also compare flight test results with

model results. However, we didn't get any takers for the data outside of AFOTEC.

There were a number of areas where we had perplexing T&E problems that went beyond normal field testing. Examples included the large target problem where we could only test our missiles against small drone targets with no way to extrapolate to a bomber size target where glint and scintillation are more significant. We had an airplane with a mile-long trailing wire antenna that could affect aircraft survivability in case of an EMP with no way to do a realistic test. We needed to estimate the effect of sunspots on an over-the-horizon radar when we could only test during periods of low sunspot activity. We needed a way to evaluate the impact of manufacturing tolerances on ECM techniques against enemy missiles; and there were dozens of similar problems. Each of those problems could only be addressed through the use of M&S to supplement field testing.

Lt Gen Leaf, when he was AFOTEC Commander and later Director of the Air Force Test and Evaluation office, always tried to get cooperation with the other Services and industry in figuring out how to address these tough problems. He would call some of us into his office and say ''we have a problem; figure out how to solve it.'' Those were exciting times. We would try to find the experts, get together to discuss the problem, brainstorm solutions, and take them back to Lt Gen Leaf. He would normally send us to the experts, like Lt Gen Glenn Kent, Norm Augustine, Harold Smith, the Air Force Chief Scientist, and as many others as he could think of, so they could decide if we had a good solution.

Bob Sheldon: You referred to Lt Gen Leaf and Lt Gen Kent. You knew both of them well—any further thoughts about their impact?

Marion Williams: I could go on for hours talking about ''the good old days'' with Lt Gen Leaf and Lt Gen Kent. They were great leaders and I considered both my mentors and critics.

It's hard to think of all the things that they taught me. Lt Gen Leaf had a habit of asking some really tough questions, such as, ''How do we evaluate ECM?'' or ''How do we evaluate nuclear weapon effects in operational testing?'' He didn't mind if we didn't have an immediate answer, but he expected an answer in a very short period of time. When he did get an answer, he usually insisted that we brief the experts in the field (and he knew most of them) to make sure we didn't get something wrong.

With contacts through MORS, it was always possible to take advantage of the expertise of a lot of individuals. Over the years I called Tom Allen or Natalie Crawford with, ''I've got a problem that Lt Gen Leaf wants solved. Can I come and talk to you?'' With Lt Gen Leaf, I had the ability to go where I wanted to and talk to anyone and try to solve the problem—as long as I could come back with some kind collective agreement on an answer.

Lt Gen Kent was a person that you did not want to formally brief unless absolutely necessary. To Lt Gen Kent, imprecise language was as bad as sloppy mathematics. We would spend hours trying to explain to him what the words on our slides really meant and what our objective really was. I know that a 30-minute briefing one time lasted five hours with Lt Gen Kent because he kept asking questions. The benefit was that by the time that you finished the discussion, you understood what you were saying better than you did when you started out.

The story goes that at one time Lt Gen Kent was being briefed and one particular slide gave him problems; he kept asking the briefer more and more questions. And the briefer got in more and more trouble. Finally, Lt Gen Kent went to the projector and picked up the viewgraph, stuck a pencil through it, and said, ''You will never use that slide again.'' [*Laughter*]

He was a tiger in receiving briefings. In a briefing on the Wild Weasel program where Lt Gen Kent was a reviewer, Lt Gen Leaf asked how we were going to do operational testing.

One of the questions we were trying to address was, ''How effective is the Wild Weasel against enemy radars?'' Lt Gen Kent said, ''The broader question is 'Can you suppress enemy radars just due to the threat of the Wild Weasel?' I want to quantify that as well.''

So we started looking into some computer models like TACOS (Tactical Air Defense Computer Operational Simulation). Lt Gen Kent hated computer models; he preferred to derive simple equations for complex problems. He came out to Albuquerque one time after asking

the question on Wild Weasel effectiveness and gave me a single sheet of paper where he had listed the factors that would go into suppression of enemy air defenses in an ''e to the minus x'' equation. He said ''you can solve it with this; but throw away your computer models and think about the problem instead of using brute force.''

Bob Sheldon: Your comment about Norm Augustine was the first time I've heard that he talked to analysts or testers. What was his relationship with the test community?

Marion Williams: He was on the OT&E Advisory Group that Lt Gen Leaf put together.

Greg Keethler: How did he get put on there?

Marion Williams: Lt Gen Leaf asked Augustine to be on his advisory group because he had a lot of respect for him. Norm came to every meeting and we enjoyed the interchanges.

Because of Lt Gen Leaf's influence we had what I considered a collection of great minds—Leaf, Kent, Augustine, Hal Smith, the current Air Force Chief Scientist, and others.

Lt Gen Leaf used to say there were three reasons he brought these people in. First, it would make us think through our answers in order to explain our plan to the group. That would help us identify weak areas. Second, they were smart enough to identify weak points or better solutions and help us improve our plan. Third, we had a group of respected, high-level personnel that would give credibility when we briefed the Pentagon.

Bob Sheldon: When did he put that panel together?

Marion Williams: About 1977 when Lt Gen Leaf was AFOTEC Commander. He was the longest serving Commander at AFOTEC.

Bob Sheldon: So Augustine had a pretty big reputation at that point.

Marion Williams: Yes, he did. Augustine was on the SAB at the time Lt Gen Leaf formed his OTEAG (Operational Test and Evaluation Advisory Group). He also picked others from the SAB such as Jasper Welch.

Bob Sheldon: Did you ever interact with Dr. Bob Ball from NPS (Naval Postgraduate School) regarding aircraft combat survivability?

Marion Williams: Yes. We had a lot of interface with Dr. Ball when the JTCG (Joint Technical Coordinating Group) was formed within OSD. We were looking at aircraft survivability at the time because that was one of the areas where we really had technical problems. In OT&E of systems employing ECM, the MOE in the requirement documents was usually reduction in lethality (RIL). Essentially, if an enemy missile came within a lethal radius of the airplane, it assumed that it could do major damage. The requirement was that the ECM being used to defeat that missile ought to reduce the number of missiles that came within that lethal radius by 20 percent.

If you look at it from the standpoint of mathematics, it essentially says that if I fly in an engagement zone and get hit 10 times without ECM and only eight hits when using ECM, I pass the requirement. However, it really doesn't prove that the aircraft would survive. We tried to show that it wasn't a very good measure for the B-1 and really needed to address aircraft survivability. Bob was—and probably still is—the expert in survivability, so he was a big help.

Aircraft survivability is a complex issue; especially in operational testing. There are many test limitations that leave voids that have to be filled with hybrid and digital simulation. When Lt Gen Leaf started asking about B-1B survivability, especially improvement in survivability using ECM, we started looking into what test assets and aircraft data would be needed. We knew that testing a threat missile engagement with the B-1B would have to be accomplished with HWIL simulation; we had done similar testing for the E-2B at the Air Force Electronic Warfare Environmental Simulator (AFEWES) in Fort Worth. It isn't feasible to fire a real missile against a real airplane in flight, so AFEWES testing was necessary. One area where we needed data that seems simple but is in reality very complicated is RCS. Since the threat of interest was a semi-active missile, we needed bistatic RCS data since the threat transmitting radar is at a different location than the receiving radar. The angle between the two is the bistatic angle. Getting bistatic radar data is expensive since we needed radar returns for multiple geometries at multiple bistatic angles.

There are different ways that you can do that. There is a bistatic measurement facility at Holloman but the cost and complexity of getting

data on a B-1 at Holloman made it not a feasible solution. There are also approximations to bistatic returns.

AFOTEC, led by Dr. Frank Gray, used the AFEWES HWIL simulation where some functions are simulated with software, such as aerodynamics and flight control, coupled with real missile seekers. B-1B radar return was simulated using RCS data along with vulnerability data to allow Pk (probability of kill) calculations. Studies like that are constantly needed to better understand the best way to address M&S and T&E issues.

Greg Keethler: Can you give us any other interesting stories about M&S issues?

Marion Williams: JTF II adopted a nontraditional approach to T&E—primarily based on a philosophy developed by Charlie Jacobs, the chief civilian scientist. That philosophy was that the purpose of testing should be to validate models, with the models being used for evaluation. He believed that test results were too limited to be used as the basis for evaluation. Models were used to synthesize the test results to allow analysis beyond the bounds of the test. That philosophy was revolutionary at the time (1966) although some ''blue ribbon'' committees recommended this approach even before ''simulation-based acquisition.'' Later, at AFOTEC, we tried to integrate M&S into operational testing, but not in the same way as JTF II. In JTF II, the model became the focus of the evaluation. The purpose of OT&E, on the other hand, is to ensure that systems work as intended—a different perspective. Models are used to gain insight, not to evaluate how well a system will work in combat. As models get better, they can be used more for evaluation instead of insight, but we don't have a consistent process to ''validate'' models with test data now, nor do we have the first-principle or physics-based models that we need. However, models are much better now than those we used at STC. The primary model there was COMO, which stands for Computer Model.

COMO was developed by Dutch and British engineers and scientists at STC. It was a flexible model that could be made more detailed as required for the application—simple look-up tables down to complex algorithms or even physics-based. We used it on the airfield attack study for the British and Dutch Air Forces. The Army got interested in COMO and Dan Willard from the office of the Deputy Under Secretary of the Army (OR) came to STC to see if it could be applied to some Army programs. We sent a couple of scientists from the Hague to Huntsville to help the Army install and use the model for studies on the Patriot system. When I came back to the US to work for AFOTEC, COMO was still being used. Jasper Welch, head of AFSAA and Wilbur Payne, Director at TRAC White Sands and I talked about a combined effort between Air Force and Army studies using COMO. Both believed that we needed cooperative M&S efforts, and we discussed a combined effort to address common Air Force and Army issues. Cooperation would reduce cost and duplication. Sound familiar?

Greg Keethler: I've heard that before.

Marion Williams: It never worked. But we keep trying. Maj Gen Welch and Wilbur Payne wanted to use COMO to support a Joint Test and Evaluation program—Electronic Warfare in Close Air Support (EW/CAS), a four-year, $100M+ program. Although EW/CAS was a test program, they recognized that a common Air Force/Army model could be used in the evaluation of tactics against surface-to-air missile system simulators developed to support EW/CAS. It sounded like an ideal way to model EW engagements: validate the model with test data, and then use the model in other applications. There were other joint tests planned that could also use those models; the model could become the synthesis of the test results. It started off really well. However, the models were more complex than we thought, and model development slipped beyond the test, and when we finished the test, the models weren't ready. It didn't work out as well as we hoped. Even though Wilbur Payne and Jasper Welch agreed in principle, their own organizations had their favorite models. Overall the theory was good; the execution was less than desired. It is a good concept; just hard to do. Still, it was challenging—getting the right balance of testing and simulation.

Bob Sheldon: Let me ask a question about your ''twin'' in the Army, Marion Bryson. During his oral history interview, he said he was proud of the fact that his integrity was

challenged but maintained when he killed a program. One general officer called him on the carpet. ''You've got to do these tests to pass this.'' But he stood his ground. He's also proud of the fact that he calls himself the father of Apache because he brought that one through the requirements process and through testing. Are there any particular systems that you feel proud of where you either killed the program or helped do a really good test to validate it?

Marion Williams: To the best of my memory AFOTEC only ''killed'' one program—the Strategic Air Command Digital Information Network (SACDIN), which just didn't work in OT&E. I recall the number of times we had to draft and redraft the message to SAC with the conclusion that SACDIN wasn't effective. Lt Gen Leaf had a rule to send bad news early and we didn't know how to say it gently.

The reason there were so few is that OT&E shouldn't kill programs. By the time you go to OT&E, you have committed 80 percent of the funds on the program. If you kill it then, you've got to start all over again. So you don't try to kill a program in OT&E; you try to identify problems and make it better. I think that AFOTEC made a lot of programs better by identifying things that had to be fixed before they went into combat.

I worry because I think that the philosophy of OT&E is changing. The philosophy initially was, ''Does the system work?'' We found in Vietnam and other conflicts that things that went into the field didn't work, like the old M-16 rifle, which jammed when it got dirt in the mechanism. By putting a real system in an operationally realistic environment with real operators, many problems can be found and fixed before the system goes into production and into combat.

However, we are losing the ''does it work'' question in an attempt to get to the broader issues that can't be addressed with field testing—issues that require simulation and are often at the operational level. It is essential because we are in a system-of-systems operational environment, but it requires more tools and different expertise. OSD has approved a concept for distributed testing using live, virtual, and constructive systems. However, we still haven't figured out how to make that type of testing affordable. We just know that it is cheaper than field testing as we know it.

However, when it comes to testing of cybersecurity (offensive and defensive cyberwarfare), distributed LVC (live, virtual, and constructive) testing is essential, but still difficult.

Greg Keethler: How does OR as a discipline compare to engineering?

Marion Williams: OR is not a tool like aerodynamics; it's a bunch of tools you use to try to solve a problem. However, there seems to be more focus on the tools than on problem solving. It should help structure thinking—thinking about solving an analysis problem or structuring an OT&E. OR is as much art as it is a science. You learn the art by experience, by learning from someone who has done it before, and from mentors, not just formal education. I still think that the mentoring isn't considered as important, and case studies don't have enough emphasis.

Greg Keethler: What do you see as the state of methods and acceptance of military OR today as compared to times past, and how do you think we need to improve it?

Marion Williams: It would take some time to think about that. I'm not too sure I even know the answer. As a profession, it's gotten a lot tougher over the years, because the acquisition systems are a lot more complex. We are moving from system platform battles to network-centric warfare. The analysis tools are getting better because of computer technology, but we still don't have tools that help assess how well systems work together in the network-centric environment—the system-of-systems environment. We can't always test in that environment because it is too costly, and there probably isn't enough control to really understand cause and effect. That means we have to rely a lot more on M&S (including LVC testing) to provide a basis for our analyses, and, in my opinion, M&S in the DoD just isn't ready to handle that just yet because we haven't thought through all the problems. We are still too fragmented. There ought to be first-principle or physics-based models to support the functional models we use for engagement analysis, but they rarely exist. We need a consistent way to correlate model and test results, but this is seldom a goal in T&E or in M&S. This isn't to say that M&S isn't being

effectively used; it is just that successful M&S is program or test specific, not uniform across programs and processes. And making sure the models are ''good enough'' is a tough task that we aren't equipped to handle just yet.

Greg Keethler: How has the acceptance of OR changed over the years?

Marion Williams: I think it's harder now because the systems are more complex and the answers aren't as clear-cut. It seems that studies in the past had well-defined assumptions, evaluations, and conclusions. When you listened to a briefing about a study result, the question that was being asked was clear and there were obvious insights from the study that helped decision-makers make decisions. Maybe that was because we had to do our own briefings so we tried to minimize the number of slides, which meant we had to get to the point fast. At least it was obvious when a study helped with a decision. Modern studies seem to provide a lot of information, but it isn't as clear that the studies lead to a conclusion, or even that the study helped with the insight that led to a decision. Many modern briefings have lots of data (and tons of slides) but it seems harder to keep track of the questions and the answers. There is no ''thread,'' as Lt Gen Leaf often commented about briefings. Again, it may be that studies have gotten more complex because the nature of warfare has gotten more complex. If you look at the number of OR organizations 20 years ago as compared to now, OR acceptance must have decreased because the number of OR organizations has decreased.

Greg Keethler: Are there some OR groups that do that better than others?

Marion Williams: I don't know that there are better groups, but any group that makes sure the question to be addressed is well defined always has a much better rate of success. I received several briefings on studies conducted for AFOTEC where my reaction was, ''Good briefing, but you didn't answer the question that was asked.'' Sometimes it was, ''I don't have any idea what you are trying to tell me.''

Greg Keethler: What advice would you give to someone who is starting out today in the military OR profession?

Marion Williams: Start out as an engineer, then add OR tools like statistics, simulation, optimization, and so forth. Engineering helps with a structured thought process, which is basic to problem solving. It helps to have military experience as well.

Greg Keethler: How could the training and education of OR analysts be improved?

Marion Williams: We teach the tools; we are lacking in the applications in problem solving. In T&E, defining a ''test concept''—the basic structure of a test and the evaluation of the test data—requires as much art as science. Test design, statistical analysis, simulation, and other tools are the elements, but putting them together to define the overall test concept defines a good test program.

Greg Keethler: From your perspective, what are the most important issues facing the military OR profession today, and what needs to be done about them, and in particular, what can MORS do about them?

Marion Williams: The main issue is making OR relevant to the DoD, which means providing analyses that help decision makers make decisions. The analysts need to be able to (1) reduce lots of work into short, meaningful results that clearly provide the information for decisions; (2) ensure that questions are well defined before trying to answer them; and (3) provide objective results.

MORS can help, as it always has, in bringing people together for education and training, and for communication, and for peer review. Working groups need to be discussions, not lectures—a place to expose your assumptions and analysis methodology so your study or test or analysis can be improved. Like OR, the secret is getting people with different backgrounds and different expertise together to attack a problem. General sessions are where the analysts learn about DoD expectations from key leaders; working groups are where they argue about techniques and results.

MORS can help train analysts to first define the problem before starting to solve it. I talked to a man who was one of the designers of the B-2 bomber; he was working on reducing the IR signature. He said he sat down and thought about the problem for eight days before he put pen to paper. We sometimes just dive into a problem without thinking it through ahead of time. That can lead to selecting the wrong OR tool. Dr. Sam

Stearns, one of my early mentors, would help me with my homework when I was in graduate school at UNM. I would try to find an equation that matched the problem, but Sam always wanted me to start with $F = ma$ and derive the equation I needed. He always started with the basics. In OR, we need to learn to start with the basics and think the problem through before starting to solve it.

MORS has access to a wealth of experienced OR analysts—people who have been there and done that. The junior/senior analysts forum at MORSS is a great idea. We need to learn from people like Glenn Kent, Walt Hollis, E.B. Vandiver, and others that I need to include but am afraid I would miss someone. If I were a young analyst, I would offer to go to work for someone like Glenn Kent for minimum wage just for the opportunity to learn from him and to listen to his stories about his early days; even about his experience as a basketball coach. Lt Gen Kent and his peers have, or did have, the expertise and experience we need to teach us how to be good OR analysts.

I can't think of anything that we haven't covered, except to re-emphasize the need to mentor and be mentored. Of all my memories in MORS, among the best were dinners with the great thinkers in OR. We have already lost some of the great ones like Glenn Kent, Wilbur Payne, and Clay Thomas. We need to take advantage of those still with us.

Greg Keethler: We haven't touched on mentorship. I consider you one of my primary mentors as I was coming up as a young analyst. I think a lot of people do—for example, John Andrew and Suzanne Beers. Any observations you want to make about that?

Marion Williams: I think there were a lot of people—Suzanne, John, you, and others who had a very innovative approach to analysis. Suzanne Beers was a very sharp individual that George Harrison and I sent back to school to get a PhD. Suzanne worked for me as a Captain, but wanted to go to MIT for a PhD degree. However, Maj Gen Harrison and I had helped establish a program in test and evaluation at Georgia Tech, and thought Suzanne would be an ideal candidate for that program. After a lot of persuading, Suzanne agreed to go to Georgia Tech rather than MIT. Maj Gen Harrison also arranged to have her assigned back to AFOTEC after graduation. After returning to AFOTEC, followed by other assignments, she was promoted to Colonel and assigned as the Commander of the AFOTEC Detachment in Colorado Springs.

There were others like Stassi Cramm. Stassi worked at Edwards AFB but was a major asset in helping plan and execute OT&E at the Western Test Range. She worked with AFOTEC in developing and implementing new test concepts, as well as major simulation efforts such as JMASS. Rob Dunn also worked with Stassi on M&S and T&E.

Pat Sanders, like Suzanne and Stassi, was a major player in instituting M&S in OT&E at AFOTEC. Pat later became an SES in OSD and then Missile Defense Agency Executive Director.

I wouldn't claim to be a mentor of any of them; I probably learned more from them than they did from me.

There were also others who were instrumental in helping me at AFOTEC, including captains and majors too numerous to name. I was especially grateful to Dr. Frank Gray and Mike Dieckhoff; my deputies during the later stages of my career at AFOTEC, as well as the 13 general officers I worked for.

I guess if I had to pick an ending to my oral history, it would be something to do with the people and organizations I was privileged to work with, and the extraordinary cooperation among different organizations in the Services, in OSD and in industry. It would also have something to do with lessons learned, or maybe lessons not learned. There were great opportunities to learn from great individuals and great minds; I only wish I had been smart enough to listen harder when I had the chance.