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Seth Bonder Interview (MORS)

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Military Operations Research Society (MORS) Oral History Project Interview of Dr. Seth Bonder, FS

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MILITARY OPS RESEARCH HERITAGE ARTICLE

INTRODUCTION

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

Dr. Seth Bonder, FS, was MORS President from 1969 to 1970. He received the Vance R. Wanner Memorial Award in 1986 and was inducted as a MORS Fellow in 1994. He was a full-time faculty member in the Department of Industrial and Operations Engineering, the University of Michigan, from 1965 to 1972, and was an adjunct professor in that department from 1972 to 2011. He was the founder, former chair and CEO of Vector Research, Incorporated (VRI) during the period 1972–2001. Dr. Bonder was president of the Operations Research Society of America from 1978 to 1979 and inducted as an Institute for Operations Research and the Management Sciences (INFORMS) Fellow in 2001 (Charter Member). He was vice president of the International Federation of Operational Research Societies (IFORS) from 1985 to 1988. He received the Jacinto Steinhardt Memorial Award from the INFORMS Military Applications Society (MAS) in 1999. He was a member of the National Academy of Engineering. This interview was conducted on two separate occasions in the MORS office in Alexandria, Virginia: June 23, 2004 and November 28, 2005. Dr. Bonder died in Ann Arbor, Michigan on October 29, 2011.

MORS ORAL HISTORY

Interview with Dr. Seth Bonder, FS
Mr. Mike Garrambone and Dr. Bob Sheldon, FS, Interviewers

Bob Sheldon: First of all, tell us where you were born.

Seth Bonder: I was born on 14 July 1932 in South Bronx of New York City. I grew up there and lived in the same apartment through the depression years and until I left in 1951.

Bob Sheldon: Give us your parents' names and tell us how they might have influenced your decisions academically or professionally.

Seth Bonder: My parents were Al and Minnie Bonder. No real influence. They were Russian immigrants. They came over separately and met here in the United States. They never had a day of schooling so it hardly had any influence or effect on my education. In fact, my education didn't start until much later in life, as you will see.

Mike Garrambone: Did you speak Russian at home? What were your parents' professions or businesses?

Seth Bonder: Although some of my relatives spoke Russian in their household, we spoke English. I knew a few Russian phrases from spending time with my relatives.

My parents weren't professionals—remember I said they did not have any schooling. Both worked in the garment district of Manhattan. My mother was an ''operator''–she sewed dresses together. My father was a dress presser who worked with 25-30 other pressers in a large room. He would use a heavy cast-iron steam iron to steam dresses before they were delivered to department stores in Manhattan. It was a real sweatshop and a tough job! He earned every dime he made.

Returning to Bob's question, I had little interest in education as I grew up in the Bronx. I spent very little time with books. At 15, I was a pretty good three-cushion billiards and pool hustler, played a lot of stickball, and also played a little basketball in high school. I was a poor student and not interested in academic activities.

Mike Garrambone: How did you get good at three-cushion billiards?

Seth Bonder: When I was a youngster, I spent most of my time on the street like many of my friends, playing stickball, basketball, and getting into trouble. We lived in a one bedroom apartment which was where I went to eat and sleep. When I was about 12, I started hanging around the neighborhood pool hall (which was illegal in those days). This was not some upper society pool hall or even what you think of as a pool hall today. It was a rough crowd, many interested in gambling, corruption, and even some drug peddling. Some of the older poolroom crowd took me under their wing and taught me how to play pocket pool, but I quickly gravitated to three-cushion billiards. I thought it was a fascinating game. Much more skill was involved. After a couple of years with a lot of time on the table and watching good players, I became quite good at understanding the geometries, controlling ball position, and the strategy of the game. At 15 I started playing in some three-cushion and pocket pool big money games, money provided by big betters and I would get a small piece of the winnings. When I was 16 I had a part-time job running and closing down the pool hall a few nights of the week.

Bob Sheldon: You said you did poorly in high school. Even in math?

Seth Bonder: Everything. I was not interested in school. When I was 15, I became a member of one of the local gangs, which served as a mutual protection society. Gang members didn't think much about schooling; we just survived in very strange ways. So there was really nothing driving me to do anything academic.

I actually started at City College of New York (CCNY) because you didn't need grades to get in, it was free, I could avoid the draft, and I thought I might get to play some basketball there. But then they had a big scandal when the varsity team threw basketball games. In those days, teams could play in both the National Collegiate Athletic Association (NCAA) and the National Invitation Tournament (NIT) basketball tournaments, and CCNY won both of them. The next year they started throwing basketball games. They got caught by the District Attorney, and the whole program fell apart. Not long after that, I broke a leg, drove a truck for a while, and then the Army came after me.

Bob Sheldon: You took one year of college?

Seth Bonder: Well, it wasn't even that. I attended for a little more than a semester and hardly ever went to class. I was in the pool room or on the basketball court. That's all I did. I was there to play, but then I had to leave because I never attended class. I went to the Air Force before the Army got me. Not unlike you did, Bob. I went in as an enlisted man but got commissioned later.

Mike Garrambone: When did you start basic training and where did you go for it?

Seth Bonder: It was late 1951 at Sampson Air Force Base (AFB) in upstate New York. It was quite cold. I don't believe it exists anymore. The Korean War was on. During that time period they allowed enlisted men who did not have a college degree to go to flight school through the Aviation Cadet program.

Bob Sheldon: So you didn't have a degree, but you were commissioned?

Seth Bonder: Right. I was commissioned through the Aviation Cadet program.

Bob Sheldon: How long were you in the Air Force?

Seth Bonder: About five years. It was during this period that the seeds of my future life were planted. Everyone I worked with and socialized with had college educations—many with advanced degrees. It became clear to me that a college education was necessary to have a meaningful career in the Air Force. A related anecdote sent a message loud and clear. One of my assignments was at Edwards AFB, California, the Flight Test Center. I was at the bar one night with the personnel officer. He informed me that a quarter of one percent of the officers at Edwards AFB did not have a college degree. And that was me!

He said, "If you stay in the Air Force you have to get a degree." I said, "How do I do that?" He said, "You need to get two years of college completed through extension courses before the Air Force will send you full time to get a degree." I thought this would take forever and wasn't worthwhile. I decided I'd get out and try college on my own.

Well, I got out of the Air Force at the end of 1956 and couldn't get into college. Nobody would take me. I called Senator Dollinger from New York. I said, "Look, I'm a vet. I don't have very good grades but I'm pretty sure I could do well if they'd let me into college." He convinced the University of Maryland to let me in. I picked Maryland because I did not want to return to New York.

That's how my education started. My parents had little influence on my decision to get a college education. Rather it was my Air Force experience and friends, most of whom had one or more college degrees. I went into engineering because of my Air Force experience and because I thought I could probably dig a better ditch if we had a depression again. I started in February 1957 and finished in June 1960 with a degree—major in mechanical engineering and a minor in physics.

Actually my first semester in Maryland was a disaster. I don't think I got higher than a 40 in my first round of midterms. I wanted to be back in the Air Force. But I went to counseling sessions and learned how to study. That year the academic light was turned on. It was an amazing transition. I realized I actually had a brain and could use it. I finished in about three and a half years even while driving a taxicab 4-5 nights a week. (After one of my MORS keynote addresses in the 1970s, an attendee came over and introduced himself as my Maryland study counselor. He said he was pleased that his guidance had helped. So was I!)

After graduation I went to work at Westinghouse in Baltimore for a couple of months. I hated it. Sitting at a drafting board was not stimulating, nor what I wanted to do.

Bob Sheldon: Did you work as a mechanical engineer?

Seth Bonder: Yes. And fortunately a friend of mine who was stationed with me at Edwards AFB was getting a PhD at Johns Hopkins in a new field called operations research (OR). He was doing this in 1958–1960. He actually applied to graduate school for me at two places: Johns Hopkins, where he was going, and Ohio State University (OSU). When I was getting ready to quit Westinghouse, he informed me he did this and he felt it would be an interesting field for me because I liked people-oriented operational problems.

Bob Sheldon: Did you test for scholarships or were you just offered them?

Seth Bonder: No, they were offered because I did very well at Maryland. I was in all the honor and leadership societies. I was older than everybody so that was to my advantage. I did some interesting things at Maryland. A Navy pilot and I helped rejuvenate a flying club in College Park, and because of my disastrous first semester, I started a freshman tutoring service for the University.

Anyway, I went over to Johns Hopkins and talked to Professor Charlie Flagle. I don't know if that name's familiar to you. In later years he and I did a lot of work together in the Operations Research Society of America (ORSA). As an aside, very few analysts in MORS ever get involved in ORSA. It's like two different worlds. It's a big mistake for the military OR community—I believe it would be useful to the profession if more military OR analysts were involved with ORSA (now INFORMS) through MAS (the Military Applications Society).

Charlie Flagle was one of the pioneers of academic OR. He authored the Flagle, Huggins and Roy book *Operations Research and Systems Engineering*. That was one of the primary OR books that came out in the late 1950s. Charlie Flagle was a big name in the field. I went to see him and he told me the scholarship was for $1200 a year. And I said, ''That's pretty tight to live on.''

When I went to school at Maryland, I had the GI bill and I drove a cab at night in Prince George's County and the Washington, D.C. area to make a living. I'd get the cab from a private owner. He'd switch cars with me about 7 pm, I'd drive until about one in the morning, and then he would switch cars back again about 6 am. I would drive about three nights during the week and 1–2 days during the weekend. I actually did my first simple operations analysis as a cab driver: I collected and analyzed the call demand data in Prince George's County to learn how to be ''nearby'' when calls were broadcast for drivers to bid on.

Bob Sheldon: Do you know the roads around here pretty well?

Seth Bonder: I do. Well, not here in Virginia, but I did know Prince George's County and D.C. pretty well. Anyway, Charlie said, ''You have to pay tuition out of that too.'' And I said, ''How much is tuition?'' He said, ''$1200.'' I said, ''I must tell you, Professor Flagle, I can't do that and will have to go to another school.''

I called Dan Howland, the professor at OSU who extended the fellowship, and said, ''You offered me a fellowship. What is it?'' He said, ''You get $300 a month. You don't have to work for the first year. Tuition is covered. We guarantee you a job the second year as a research assistant.'' And I said, ''I'm coming.'' So it turned out I was the first Systems Fellow at OSU and that's how I got into the OR field.

When I arrived at OSU, I learned that Dan Howland was also the head of the Systems Research Group (SRG). Dan is now long gone. He had a bunch of research projects in OR, in the military area and in the health area. He said, ''You can pick an area to work in after the first year. Since you have a military background, why don't you work on a military project?'' That's how I got into the military OR business.

At the end of my first year, which was in 1960, I worked on a little project for the military at Fort Knox, Kentucky. Dan had a number of contracts with Fort Knox. Nobody in the research group knew much about the Army, but we had contracts. This one was to determine the optimal path for a tank to drive through a contaminated nuclear battlefield. It was being done for the group at Fort Knox that was called the Combat Developments Group. I spent time at Knox learning about the Army. I got intrigued by what one of the groups was doing. They were responsible for writing requirements for new armored systems.

Bob Sheldon: Did you drive to Fort Knox from OSU?

Seth Bonder: We'd fly down or drive down to observe their work activities. That was in 1961. I observed that it didn't make any sense the way they were drafting requirements for new systems. They would sit on large butcher paper with their slide rules drafting requirements for the next generation tank. They would calculate values for many performance characteristics such as how fast it should go, how far it should be able to go, how reliable it should be, how well it should detect targets, the accuracy of firing the tank's main gun, and the vulnerability of the tank.

I was amazed how primitive the process was to develop requirements for the next generation armored systems. It dawned on me that there were two major problems that would be interesting to work on. One was the *feasibility* of the requirements. If you wrote numbers down for those variables or parameters, could you, in fact, build a system with those capabilities? I doubted it because the process they were using didn't take into account the physical interactions among the various parameters.

For example, if they wanted to increase how fast and how far they wanted the tank to go, they would specify the speed of the tank and its cruising range. And if they wanted to, for example, increase the survivability of the tank they'd add more armor to it. They neglected to recognize that if you added more armor and, if you wanted to go as fast as you said you wanted to go, you needed a bigger engine. If you added a bigger engine, you needed more fuel to maintain the tank's cruising range. This would decrease the survivability because the additional fuel made the tank more vulnerable to enemy fire. They had no idea how to handle those engineering-level interactions. Based on some recent experience, I believe this problem may still exist in a number of military domain areas today.

The second problem was the issue of what would be useful to write as performance specs? Hopefully the specs they wrote would produce a tank that was operationally *effective*. That is, the next generation system would prove to have a high operational utility on the battlefield. So you had the feasibility issue and the effectiveness issue. I discussed this with Dan Howland who became my PhD advisor. He said, ''Why don't you write a proposal to address these?'' I'd never written one but thought it would be a learning experience.

I wrote it for Fort Knox which was then part of the Continental Army Command (CONARC). I'm not sure they understood it. In fact, the first time I presented it to the Colonel in charge of the Combat Developments Group at Fort Knox, he essentially threw me out of his office. But his staff convinced him it was worthwhile doing and they moved it forward to General Daly who was then the Commanding General of CONARC. He was a four-star who looked like General Curtis LeMay, Commander of the Air Force's Strategic Air Command, smoked cigars like him, and acted like him.

I was asked to give a briefing at Fort Monroe, Virginia, the home of CONARC Headquarters. Fortunately we had a number of Army officers who were in the master's program with me at OSU. One of them was Rick Anson, a captain who went on to become a two-star general. Others were Major Bill Bercaw, LTC Larry Caid, and Captain Dan Schroeder.

Bob Sheldon: These were active duty officers?

Seth Bonder: Yes. They were active duty officers who went to OSU because there weren't many OR educational programs in the country. You had Case Western, OSU and Johns Hopkins. These were the three main OR programs that existed in the early 1960s.

They helped me write the proposal which enhanced it by including appropriate Army jargon. Rick Ansen, who used to be a trainer of Army briefers said, "Let me show you how to do a briefing." This was my first exposure on how to market in the Department of Defense (DoD). He said we needed to have slides. In those days you didn't have computers to create graphics. It was very labor intensive at the Systems Group. They'd make big cardboard pictures, photograph them, and then put the negatives in 3½ x 4 inch glass slides (called lantern slides).

We went down to Fort Monroe. I gave the briefing and the argument was over which organization would house it, not whether I'd get it. General Davis said, "Look, let's not air our dirty laundry in front of these guys from the University." To make a long story short, I received a $2 million contract as a graduate student to run the proposed four-year research program.

I was the project director and a full-time graduate student. We started building models to address the *feasibility* issue. That is, predict how fast the vehicle could go, how well the crew could detect, how accurately it could fire the main gun, how vulnerable the tank was, etc. My research staff consisted mainly of graduate students, supplemented by a few faculty members from various disciplines and departments. That research slowly brought me into the mainstream of what was happening at the tactical level of Army OR at that time.

Although OR was conducted for the Army in WWII, I believe it first got codified in the late 1940s with the establishment of the Operations Research Office (ORO). ORO was formed and run by Johns Hopkins University as the Army's first FFRDC (Federally Funded Research and Development Center) to do strategic-level and policy-level analysis. This later became Research Analysis Corporation (RAC) in the early 1960s. RAC was sold to General Research Corporation (GRC) in Sep 1972 after the Army informed RAC it would no longer be supported as an FFRDC. The Army Strategy and Tactics Analysis Group (STAG) was established as a field activity under the staff supervision of the DCSOPS in August 1960 and was subsumed by the Army Concepts Analysis Agency (CAA) when it stood up in 1973.

The tactical level of Army OR and experimentation in the 1950s and 1960s was conducted and supported by the Ballistics Research Laboratory (BRL), run by guys like Joe Sperrazza, Keith Myers, and Dave Hardison, and the Combat Operations Research Group (CORG) which was a field office of ORO. Some of my future professional friends such as E.B. Vandiver, Hunter Woodall and John Riente worked there.

I got into the business at the tactical level of Army OR at OSU under the contract that I had with Fort Knox. CORG was doing a bunch of experiments. That was a period in Army OR that was a scientific era: the 1950s, 1960s, and 1970s when the Army did a lot of experimentation. They experimented with tank accuracy, firing times, vulnerability, pinpoint detection and other performance characteristics.

I did some limited experimentation at OSU to assist in developing engineering-level models of performance capabilities for armored weapon systems. We worked on vehicle speed and the impact of vibrations on cross-country speed. We did theoretical work in the engineering mechanics lab, and ran mobility experiments with a local reserve unit. We did experiments on visual detection to see how well you could detect stationary and moving tanks. I almost lost a couple of my researchers (who were students) because when a tank was coming at you, you didn't see any contrast changes. Some of them almost got run over. It was an exciting time. We were supplementing some of the Army's experimentation activities and building models to estimate performance capabilities of future armored systems.

Mike Garrambone: Were these mathematical models?

Seth Bonder: Mostly. They were engineering models that described the physics of various physical and operational processes.

To assist with our performance-level modeling, Bill Bercaw, an Army major who was a student at OSU, arranged for us to go to Fort Knox and shoot on the firing range. Most of my researchers were students, and some were women. We'd go out to the range and fire the tank main gun and the 7.62 mm machine gun. I had one researcher who was an unbelievable gunner. They would pull pallets as targets at different ranges and the idea was to fire a round through the first pallet and then, after the gun was reloaded, fire a round through the second pallet if you could. My researcher did that when the pallets were being pulled at 1800 meters! Our project officer was quite proud of the research team.

As I said, Army OR was heavily into experimentation which was necessary and useful. We learned about the feasibility of achieving performance levels, and to model performance capabilities such as hit probabilities, firing rates, firing times, and loading times. At Fort Knox, I used to have someone clocking the loading times and the firing times and used this data to develop models as a function of the characteristics of the system. BRL was heavily involved in estimating the lethality and vulnerability performance characteristics for armored systems.

The Army Materiel Systems Analysis Activity (AMSAA) was a spin-off of BRL. BRL was much more a hard engineering group and AMSAA looked at issues of system performance and effectiveness, just like we were doing in a smaller way at OSU and CORG was doing. They were also heavily involved in Army experimentation.

Experiments were done on many of the armored system performance capabilities. Some of the early ones were the STALK experiments (Project STALK tank-versus-tank experiments in 1953) which Dave Hardison analyzed. It looked at the impact of different threats, different fire control systems, different size guns and a few other dimensions on hit probabilities, kill probabilities, detection probabilities, firing rates, and others.

During the 1950s, 1960s, and 1970s the Army OR community was involved in extensive experimentation to gather data about these performance parameters and to understand fundamental land combat processes. We learned what was important. We learned how to start building models to predict the system's performance parameters (capabilities) as a function of its engineering characteristics. (For example, the model for hit probability was a bivariate normal distribution with a number of fixed and variable biases that you had to estimate to predict the probability of hitting a specific target with a particular type tank gun. Models of this performance capability, as well as estimates of the various biases, were being developed using field and laboratory experimental data.) We learned how to improve these performance capabilities, how to represent them in combat models, and how to model land combat processes.

Another experiment was Project Pinpoint, which John Young ran, to determine how well you could detect an enemy when they fired a round and the associated location errors. Our research at OSU involved theoretical and experimental activities to develop models of detection capability based on differences in contrast stimuli.

It was a wonderful community of academics like me, industry researchers, and the Army that participated in these experiments. That's when I first met Walt Hollis. In my early days at OSU we were trying to build models of systems accuracy and we needed estimates of the biases and how to predict the biases. Walt Hollis worked at Frankford Arsenal where he was a recognized expert in the area. I met Walt there and learned all about firing main tank guns.

As I said, it was an exciting period of time. I was not only developing models to estimate some of the performance capabilities but developing software to integrate my models and the Army's performance-level models so we could estimate the *feasibility* of building a tank weapon system with these capabilities, considering all the interactions among them. While this feasibility-related research was going on, I also started thinking about the potential *effectiveness* of the systems in an operational setting.

Bob Sheldon: When you were building the models, did you use your academic knowledge or were you using knowledge based on experience?

Seth Bonder: I think both—academic capability and experience from some of the modeling and experimentation work being conducted by the Army. Obviously, the mathematics was important. My initial foray into building effectiveness models back in 1962 or 1963 (which led to the Bonder Individual Unit Action (IUA) and many subsequent developments downstream) was stimulated by a two-star general from the Combat Developments Command. It was Major

General Pickett who was responsible for building the Sheridan system.

He was actually a descendant of George Pickett from Pickett's Charge in the Civil War. He knew I had the contract with Fort Knox. He came to OSU and said, "I need to know about the effectiveness of alternative potential Sheridan light tanks described by alternative sets of performance characteristics. Can you help me?"

There weren't any combat models around that I could responsively use to address his problem. This was a quick reaction six-week study – and I was surprised the University let me do it. We kept it unclassified.

In 1962 there were a small number of combat models being used in Army studies but which I deemed inappropriate for the Sheridan study. There were large gaming models which were inappropriate both because they were more operational-level models, not tactical-level ones, and due to the requirement for many players to run them. Then there was ATLAS (A Tactical, Logistical and Air Simulation), an aggregated "firepower score (FPS)" based model of theater-level combat which was inappropriate for many reasons. It was a "holistic" model that did not represent details of military systems or processes that I needed to consider. It rolled all of these up in a many-to-one transformation to produce the FPS for each side. In turn the downstream loss of each side's FPS and each side's movement was determined via some very questionable look-up tables. I thought the FPS construct was somewhat "garbage"—it had no scientific basis and in later years I referred to it as the "Phlogiston" theory of combat. (The phlogiston theory is an obsolete scientific theory of combustion from the 1700s—phlogiston theory claimed that combustion gave off oxygen, rather than using it.)

Then we had the Monte Carlo simulation: Carmonette. I believe it was developed by Dick Zimmerman at CORG back in the 1950s. This was an Army company-level model for blue defense against a red attack or the other way around. Over the years it was expanded to battalion-level. It was a "synthetic" model as opposed to holistic. It considered many of the relevant tactical-level military processes and details of the system performance capabilities like the vehicle speed, hit probabilities, kill probabilities, detection probabilities, etc.

Bob Sheldon: Were you building digital simulation models?

Seth Bonder: I wasn't, but Carmonette was a digital simulation model. Carmonette was the only tactical-level model around; but it took, even at a company level, 30–60 minutes of computer time to replicate a single company-level combat engagement and 15–30 replications to achieve some statistical stability. Thus we were looking at 10–15 hours to simulate a single engagement because computers were slow in those days. I needed a model that would be able to run parametric analysis on the Sheridan's performance capabilities.

A class of analytic models which were more theoretical in nature, and primarily of interest to the academic community, was the Lanchester differential equations models with coefficients ("attrition rates") no one knew how to estimate except after an engagement. There were a lot of theoretical papers written about various forms of the equations. A square law, a linear law, a logarithmic law—but they weren't useful in real studies. Although the differential equations proposed by Lanchester had many problems, their simplicity was appealing.

To do the Sheridan study, I decided I would try to compute attrition in a small unit combat engagement using the differential equations by developing ways to estimate the attrition rate, *a priori*. I built simplified models that considered hit probabilities, firing times, and kill probabilities to estimate attrition rates. So for the Sheridan study, I started using what we think of as Lanchester models. However, based on doing this simple study it was clear that a fundamental assumption in the classic Lanchester models that the attrition rates are constant is unrealistic if combatants move during an engagement. For example, consider some of the parameters I used in estimating the rates for the study.

The hit probabilities increased the closer you got to a target. You could detect better as you got closer to potential targets. Clearly those parameters and others were changing over time which suggested the attrition rate had to change over time during an engagement. Recognizing this, I performed the study by building a simplified scenario involving opposing forces, and solved the variable coefficient differential equations numerically for the alternative Sheridan candidates.

After the study was completed, I compared the results with those achieved by using the classical Lanchester constant-coefficient formulation, using various "average" coefficients calculated with the parameter data. The differences were quite significant. I had stumbled on an interesting research area for my dissertation research at OSU (from 1963–1965), and for later research areas at the University of Michigan (1965–1972) and VRI beyond 1972.

As I delved into my dissertation, it was clear that, not only was the attrition rate a variable as forces moved about the battlefield, but because many of the processes I considered in computing the attrition rates in the Sheridan study were stochastic in nature (firing accuracy, firing times, target detection, etc.), then the attrition rate was also stochastic. If you think about the concept of a stochastic attrition rate that varied over time, the differential equations became variable coefficient (sometime nonlinear) stochastic differential equations. Recognizing the extreme difficulty in trying to solve these formulations in finite time, I made some tactical decisions to proceed with the dissertation: I would develop models to estimate the probability distribution of attrition rates as a function of the fundamental performance characteristics of a weapon system. I would then suppress the stochastic aspects by using some statistic from the distribution as the variable attrition rates in the variable coefficient differential equation formulations.

In the 1964 time period, I assumed that the expected value of the attrition rate random variable was an appropriate statistic to use in the differential equations. In 1967, using some arguments from renewal theory and Blackwell's theorem, my colleagues at Michigan demonstrated that the time-to-kill a target, not the rate, was the relevant random variable, and that a theoretically sound definition of the attrition rate is the reciprocal of the expected time to kill a target. Bernie Barfoot (Charles Bernard Barfoot) from the Center for Naval Analyses (CNA) made a similar observation in 1969 using some harmonic mean and Markov chain arguments.

During the 1963–1965 period, I did some interesting theoretical work on the variable coefficient differential equations, particularly looking at the impact of movement of forces during an engagement. The military has known for years that mass is a nonlinear force multiplier in combat. Some of my research suggested that attacking with sufficient speed also is a nonlinear force multiplier and, along with mass, can be used to rapidly saturate a defender's retaliatory capability.

As an interesting but scary aside, sometime in 1964 I read an abstract in the Defense Technical Information Center (DTIC) by George Gamow, the famous physicist, summarizing work he was doing on variable coefficient differential equations for the DoD. I saw my dissertation fly out the window! Fortunately, when I received and read the report, it described some military activities that could be modeled by the equations but ended up concluding that they were very difficult to solve.

As part of the dissertation, I developed models of the time-to-kill probability distributions for armored weapon systems and thus their associated attrition rates. This involved some fundamental process modeling of the system's doctrine for attacking targets and included its detection, firing, readjustment of fires, lethality and other performance capabilities against various type targets.

During the same period (1963–1965) I continued to run the Fort Knox project, referred to as The Tank Weapon System project in which we were developing and integrating models to predict the performance characteristics of proposed armored systems, including the interactions among them. These were intended to assess the *feasibility* of achieving the capabilities and to feed these parameters into the differential models of combat to assess the *effectiveness* of proposed alternative armored systems.

Bob Sheldon: Who helped you come up with these variables?

Seth Bonder: A combination of sources. By reviewing available experimental data; by working with armored officers at Fort Knox who were writing requirements for future armored systems; by talking to other Army analysts like Hunter Woodall and Dave Hardison; and by reading historical accounts of military engagements. Many of the parameters highlighted earlier were included in the attrition rates, including the lethality of munitions which we had to estimate through some modeling.

Obviously, the lethality of fire was an important dimension in a tank's capability to defeat a target. This was one of BRL's areas of expertise. We needed to be able to predict the probability of a kill given a hit, and other related lethality parameters. BRL was working on models of these parameters. They had developed sophisticated computer programs that fired rounds into a tank at four inch squares, assessed damage to components behind it, and computed the relevant kill probabilities.

I asked them if we could transfer the program to OSU. They said to me, ''You couldn't program this in OSU's computers in less than a year.'' I said, ''Why don't you give it to me anyway. We'll try it.'' Remember, some of my researchers were students and some were faculty. I went back home and said to one of my student researchers named Dick Freedman, who was an absolutely fantastic computer programmer, ''Dick, I just came from BRL. They gave me their vulnerability program and said it would take at least four months for us to convert it.'' He took this as a challenge and had it running in eight weeks. And he enhanced it by improving the computational algorithms for the normal distribution. The students I hired were very smart.

I completed my dissertation and graduated from OSU in 1965. The five years I spent at OSU were the beginning of my professional life in military OR. I had become acquainted with many Army OR analysts and familiar with many of the Army's tactical-level planning problems. It was the start of the research that eventually led to the Bonder-Farrell differential equations of combat and eventually the VECTOR series of campaign models.

When I left in 1965, the Fort Knox project wasn't completed. I turned that over to Gordon Clark who had just returned from the Marine Corps to get his PhD. He took a totally different approach for more analytic effectiveness models. He built the Dynamic Tactics Simulation (DYNTACS), a Monte Carlo simulation whose outputs fed the Combat Analysis model (COMAN) which was one of the first fitted-parameter analytic combat models. It was a very interesting concept. It was a competitive approach to mine. Where mine was purely analytic, with independently calculated input parameters, he fit parameters from his Monte Carlo simulation of combat for use in the COMAN model. Two different approaches to analytical combat modeling.

When I graduated, I was going to go to industry to make some money. I was so broke as a PhD student making $3,000 a year that in my last year as a graduate student I decided to ask for a substantial raise. My advisor said, ''What do you want?'' I said, ''I want $10,000 a year.'' I was married in 1962 and I had my first child in 1964. I wanted a raise. The Dean said, ''That's more than some faculty make. I can't give you that much.'' I said, ''Then I'm going to quit. I'll just write my dissertation. I'm not going to run the project anymore.'' He said, ''You're holding a gun to my head.'' They gave me the $10,000.

In late 1964, Jack Borsting approached me about joining the faculty at the Naval Postgraduate School (NPS) in Monterey. Jack was then growing its OR department and was hiring a number of new faculty, including Steve Pollock who later joined me at Michigan. I told Jack that I was looking for a job in industry.

Bob Sheldon: How did you decide to go to the University of Michigan?

Seth Bonder: In early 1965 I went to Chrysler in Detroit to interview for a job. They were in the defense business as builders of tanks. They knew what I was doing at OSU, and wanted me to come up there and start a military OR group. Prior to going for the interview, one of the members of my PhD committee, a mathematician named Henry Colson, had a good friend at the University of Michigan who headed the Industrial Engineering Department (since 1971 called the Department of Industrial and Operations Engineering). Henry said, ''You've got to hire this guy. They throw money at him.'' Universities love professors who bring in money. He called me and he said, ''Would you interview for a faculty position?'' I explained I was not looking for a university job. He volunteered to pick me up at Chrysler so I could give a seminar in the afternoon.

So I went to Michigan in the afternoon after my Chrysler interview. It was intellectually stimulating. I presented my dissertation at the seminar and got arguments from some very strong mathematicians. It was just a fabulous activity and I thought, ''I can get smarter if I come here for a few years.'' On the way back to the airport, the Department Chair offered me a job as an assistant professor and I would become an associate in a year or two. That was a pretty good job offer and it was for all of $16,000 for a nine-month year. I accepted the job offer on the way back to the airport. When I got home, I told my wife and she said, ''I thought we were going to industry.'' I said, ''I thought we were too, but now we're going to the University.'' My wife had been a cheerleader at OSU so going to Michigan was like going into enemy territory.

I joined the faculty at the University of Michigan in 1965. I'd never taught before. The first year all I did was write course materials. The first semester I was asked to start a new course to teach linear programming for juniors and to teach a senior level inventory control course. Through the period 1965–1972, I taught a spectrum of OR courses, developed a three-course sequence in decision theory, and started a course to teach students how to model operational phenomena—later referred to as the ''Modeling Studio.''

Mike Garrambone: Did you use Saul Gass' book for the linear programming course?

Seth Bonder: No. With few exceptions I usually taught courses using my own notes and, if they existed, made textbooks available as references for the students. In those days there clearly weren't any texts to teach modeling. I can't be sure, but I believe my course in modeling may have been the first of its kind since I was attempting to teach students the art of modeling operational processes (leading to the name ''modeling studio''). There were courses with the word 'modeling' in the title, but they were teaching linear programming models, inventory models, etc., not the process of modeling operational phenomena. Doing so is not easy. In fact, my first couple of attempts were failures, but I think I got it right the third time. The students learned how to start with very simplifying assumptions for their initial version of a model and how to remove them through a series of enhancements until they had a version that could be used to address the decision issue I posed to start the process. They worked on a number of these in a semester, with individual critiques from me.

It was a demanding, time-consuming activity for one course. It also was a little frustrating in that I couldn't describe what I ''covered'' in the course. In a linear programming course you cover the simplex algorithm, duality, etc; in a queueing course you cover M/M/1 queues, etc; in a decision theory course you cover decisions under risk, utility theory, Bayesian updating, strategy development, etc. I finally figured out that I wasn't covering material but rather was providing the students mentored experience in the art of modeling operational phenomena. I was pleased to learn a few years ago that the number of such courses has grown in engineering and business OR programs around the country.

At the end of the first year, the Department Chairman said, ''I've got money. If you want to do some research over the summer, I'll pay for it.'' I formed a little research group which I eventually turned into the Systems Research Laboratory (SRL) at Michigan. I hired some grad students, one of whom happened to be Bob Farrell. Bob was then a 21-year-old typist in the Department Chair's office. One of my colleagues said to me, ''You ought to hire this guy. He's really bright.'' I said, ''So why's he typing?'' Anyway, he joined me. Over the summer I and 10 graduate students developed a long-term research strategy for Michigan's new Highway Safety Research Institute.

Just a little aside on Bob. This was 1966. He was getting his PhD in theoretical math. In the previous year, he claimed to have solved the well-known Four Color Problem analytically. And as best I understood, they accepted it. Then three weeks later, he proved he was wrong. If I were in that department, I would have given him the degree for that proof. Well, they didn't give him a degree. Then when he joined me as part of the research faculty in 1966, he started taking the comprehensive exams in the Department of Statistics without ever taking any of the coursework. On his own he had learned graduate-level probability and statistics. The next year he wrote a dissertation.

His two advisors were Bill Ericson and, I think, Floyd Hill. They both had been students of Savage, a pioneer in Bayesian statistics research. They said, ''It's a great dissertation except it's not for Bob Farrell. He's too smart. He's got to do more.'' So he quit and never received a PhD. But he was, without a doubt, the brightest man I've ever known—immeasurable IQ, unbelievable individual and wonderful to work with. He was a close collaborator, partner, antagonist, and friend for over 30 years.

Anyway, Bob joined my SRL Lab in 1966. The first project was a contract out of Redstone to do some missile air defense simulation modeling and another out of Rock Island Arsenal to do some air defense gun modeling. Then I obtained a contract with Office of Naval Research (ONR) to continue the research I started at OSU on analytic models of combat.

I also received a contract from the Army Assistant Vice Chief of Staff's (AVCS) office in the Pentagon. My contacts in that office were Dick Trainor and John Honig. John was then and has been very active in MORS for many years. The AVCS was Lieutenant General Bill DePuy who, as I will comment on later, played an important role in my professional life. Many officers in the AVCS office became generals later in their careers, including Max Thurman, Dave Maddox, and others.

The ONR and AVCS contracts with the SRL were to continue my theoretical work on the analytic models of combat. It involved a broad spectrum of research on tactical-level combat operations. It also included building predictive models of attrition rates for different weapon systems. Not just tanks but infantry systems, for helicopters, for artillery, for various other weapon systems. It included expanding the differential models of combat to represent multiple types of systems in combat operations—multiple systems on one side and multiple systems on the other side. We expanded the concept of the attrition rate to the attrition coefficient to include the allocation of fires among different targets. The latter included one of my student's theoretical research with differential game theory for his dissertation to determine the optimal way to allocate fires across enemy forces. We expanded the target acquisition process to include line of sight (LOS) effects, visual pinpoint detection and false targets. There were models of other combat processes, all of which fed into the attrition coefficient which, in turn, fed into expanded differential models of combat. That work took place from 1966 through 1972 in the SRL. Much of this research was documented in an extensive report for ONR and the AVCS office—referred to as the ''Bible.''

Bob Sheldon: This laboratory was at the University?

Seth Bonder: Yes. It was part of the Industrial and Operations Engineering Department of the University of Michigan. The SRL included a number of bright PhD students (including Peter Cherry, George Miller, and some military officers), a cadre of masters students (including David Thompson), and some faculty (including Ralph Disney, a superb stochastic process researcher). Bob Farrell was a full-time research scientist in the lab.

During my time at Michigan, I also did some consulting for industry. One of my clients was the Chrysler Defense Group in Detroit. They asked me to work with them four days a month for a couple of years as they prepared to bid on the next generation tank system. Although I had not consulted before, one of my senior colleagues said my rate should be $300 per day. Senior management at Chrysler said that was way too high. So I offered them an alternative: I would work four days a month until the program was awarded (which could have been 3–4 years) for free, but if they won the contract (worth billions) I would get a million dollar bonus. Needless to say, they paid the rate I originally requested. With the Chrysler work, I was consulting about 5–6 days per month, while teaching 1–2 courses, doing research, managing the research and marketing for the SRL, guiding about 3–4 PhD dissertations and some masters students, serving on departmental committees, teaching summer short courses, and trying to participate in professional societies. It was a very busy time in my professional life.

I would be remiss if I didn't mention some of the wonderful faculty colleagues I had at the University of Michigan. This included Ralph

Disney, Bob Thrall, Herb Galliher, Dick Wilson, Walt Hancock, and others who taught me much about teaching and academic politics. In 1969 I recruited Steve Pollock from NPS who picked up my modeling and decision theory courses when I left the University in 1972. Of local relevance here in Virginia, in the late 1960s I advised a bright assistant professor to move to the business school since our department was not going to grow an IT program—his name is Alan Merten who is now the President of George Mason University. A wonderful guy who I understand has done a great job at Mason.

In addition to the extensive research on land combat, I did some research at Michigan in the area of search theory with some of my military students. Bernie Koopman in World War II developed search theories for the Navy which I'm sure you're well aware of. That was powerful work for sea warfare, but it wasn't appropriate for land combat because of LOS issues.

In the sea, except in rare cases, you always have LOS to a target although you may not detect it. In land warfare, which is much more complicated, you may lose your LOS intermittently so you have to start the detection process over. I had an Air Force major work on this for his dissertation—the impact of LOS on search theory and ability to acquire targets. It turns out the optimal search concepts are quite different than the original Koopman research.

Following that we added another dimension to the search process: What are the consequences if you find a target? There are times when finding it may be detrimental to your health. How does that affect your search process? That led to another dissertation. Within a span of four years, I probably had six or seven dissertations and many master's theses written based on the research we did on military operations in the lab.

Bob Sheldon: Were most of these active duty officers doing research?

Seth Bonder: No. There were about 25 students and faculty doing research in the lab. This included 5–6 Army, Air Force, and Navy officers. When I was at Michigan, the Army offered to send me 30–40 Army students a year. I said, ''There is no way I would take on that many Army students a year.'' This was true for a lot of reasons. One, it was too big an overload for our department. But more importantly, I thought since OR was such a burgeoning field, it would be appropriate for Army soldiers who get educated in this field to have different perspectives on it. They should have faculty members other than me because I had one view on how to do research and analysis that likely would be different from other faculty members.

They eventually gave contracts to, I think it was Tulane, and then to NPS in Monterey where they sent most of the officers. I got two or three a year from the services. One Navy lieutenant commander was writing a classified dissertation on fleet air defense. I asked Ervin Kapos who was then at CNA to serve on the committee. (I'll tell you shortly how I met Ervin in 1966.)

This Navy lieutenant commander was almost done with his dissertation, but ran out of time and had to go back to the Navy. Since I had invested so much time in this research, I arranged with Jack Borsting for him to go to NPS to finish his dissertation. But then he got an offer from Navy Admiral Zumwalt to come to a new Navy analysis office called OP-96. He said to me, ''What should I do?'' I said, ''Well, we spent a lot of time on this dissertation but, if I were you, I'd go to Zumwalt. You'll become an admiral.'' Well, he eventually became a two-star admiral, but never finished the dissertation.

I should backtrack to Ervin Kapos because I'm bouncing around a bit.

Bob Sheldon: That's fine. I interviewed Ervin this summer.

Seth Bonder: My first interaction with MORS was through Ervin Kapos. I arrived at the University of Michigan in 1965. In 1966, I received a call from Ervin who said, ''My name is Ervin Kapos. I'm running a symposium for an organization called the Military Operation Research Symposium (later changed to Society) and would like you to referee the submitted papers.'' Stroking my ego, he said he heard I had high standards and he wanted a quality symposium. I believe he heard this from a couple of Army officers who worked with me at OSU where I did develop some high standards for analysis and research. (Looking back, I believe I embedded these standards into the VRI culture and to other activities in my professional life.) Being a good academic and not understanding the magnitude of the task, I agreed to review some papers. Big mistake on both our parts! He sent *all* the submitted papers for me to review (I expected 10–20), and I rejected all but a handful—clearly not enough for a MORS Symposium. (Knowing Ervin, if he were the reviewer I'm sure he also would have rejected most of the papers.) But he had a symposium to run. At his request, I gave him the top 20 which he included in the symposium.

That's how I first met Ervin and learned of MORS. I attended the 1966 symposium in Monterey and began a long association with the Society. I joined the Board of Directors in 1967 and became President in 1969. I continued to work at many of the subsequent meetings and presented some plenary and keynote speeches over the years. In one of my early keynotes, I tried to highlight the ''tyranny of numbers'' and ''rubber threat'' which the Office of Secretary of Defense (OSD) Program Analysis and Evaluation (PA&E) used to control the Services' budgets and expenditures. At a later one I introduced the methodology of ''versatility planning'' as a rational alternative to the ''cost-effectiveness'' paradigm for defense planning. (I believe many of the versatility concepts are now embedded in some of the services and Joint Chiefs of Staff [JCS] planning approaches.) In a MORS plenary talk a few years ago I presented some ''lessons learned'' for military model building and analysis based on my 40+ years of experience in the field. I've always cherished my time with MORS and the MORS Board. Early on I met some very bright dedicated analysts (such as John Honig, Murray Greyson, Art Stein, Gene Visco, Joann Langston, John Kettelle, Clayton Thomas, Jack Borsting, Dave Schrady and many others) and established some lifetime friendships through MORS.

Bob Sheldon: Are we still in the 1965–1972 period?

Seth Bonder: Yes. A number of key events occurred in the 1965–1972 period which had significant impact on my future professional life. I've already mentioned the phone call from Ervin Kapos which led to a lifelong association with MORS.

During the 1960s, many individuals in industry, the Army, and the DoD who were practicing or wanted to do military operations analysis were engineers and physicists (even a few economists!). They had good technical backgrounds but little formal training or exposure to the concepts, mathematics, and phenomenology of military operations analysis (e.g., model building, decision analysis, probability theory and stochastic processes, simulation experiments, combat processes, effectiveness measures, life cycle cost analyses, etc.). Believing there was a demand for this type of course, in the summer of 1967 I started a one-week, 8 hours per day, short course titled ''Topics in Military Operations Research'' at the University of Michigan's continuing education program. Given the large enrollment the first year, I extended the course to two weeks in 1968. Along with some of the other lecturers, I developed course notes that were 1,000 pages of lectures on background material and the work I had been doing in the 1960s on predicting system performance capabilities, on estimating force effectiveness, and on how to perform logically sound analyses. I had approximately 100 students attend each year. Dave Maddox attended in the late 1960s when he was a captain or major. In addition to offering the course at Michigan, I taught it at a couple of DoD agencies. If I remember correctly, one of them was the Army's Virginia-based Systems Analysis Group in 1969–1970 run by Marion Bryson, and another was BRL in 1971 or 1972. Although I stopped offering the course after 1972 when I left the University, I continue to run into former students who tell me how useful the course was and how valuable the course notes have been. I met many interesting folks who were involved in DoD-related activities through the course.

Bob Sheldon: Did you ever publish a book out of it?

Seth Bonder: No, I never made a book out of it. But a lot of people still have the notes and use them as reference materials. I noted earlier that much of the theoretical underpinnings for the ''Bonder-IUA'' and subsequent models (discussed later) were codified in a large SRL report referred to as the ''Bible.'' The course notes contain some of this material.

In 1965 I met Wilbur Payne for the first time. He was the Deputy Under Secretary of the Army for Operations Research [DUSA (OR)] and was running the Hawthorne Committee.

He had obtained my name from an Army officer who worked for me at OSU. He called me at Michigan and asked if I would join the committee which had many senior Army and OSD members on it. It was a large committee. Probably because of my lack of knowledge about the subject, I asked some embarrassing questions at the first meeting. Wilbur loved it and we became fast friends and continued for the rest of our professional lives. For many years after this initial meeting, we spent a lot of time together working on committees, discussing Army issues, brainstorming new approaches and methods for analysis. I tutored him on some new stochastic process book, and drinking. Over the years we did a lot of the latter at his houses in Washington, D.C. and El Paso, Texas, in Juarez, Mexico, and on temporary duty (TDY) together. Wilbur worked for me for 2–4 days per month as a VRI Associate after he retired and until he died in 1990. Wilbur was bright and dedicated to making the Army better, and had a unique talent for getting change to happen in the DoD bureaucracy. I learned much from him, and he surfaced at many places in my career.

Bob Sheldon: In what way?

Seth Bonder: He was instrumental in my getting involved with the Army Science Board, was a strong advocate for implementing the new modeling approaches I developed in my research, and, as I said, was a VRI associate for a number of years after he retired from government service.

While still in the 1965–1972 period, in addition to my teaching, I directed the SRL at the University of Michigan which conducted extensive research on military operations, some of which I discussed earlier. Most of that work was theoretical in nature with lots of mathematical formulations. Although our research was on a spectrum of combat processes, we did not have a model, per se, to use for analysis of combat issues. In August of 1969, I received a call from Dick Trainor, who was my sponsor from the Army's AVCS office, asking me to meet with the AVCS, General Bill DePuy (then a three-star general). I had not met him before, but apparently he had an innate feeling for the power of analysis.

At the meeting, General DePuy said, ''I'm having some problems with Mr. Packard (who was the Deputy Secretary of Defense) on the new Main Battle Tank (MBT-70). I need some analysis of the MBT-70 alternatives, but the Lockheed Corporation wants to charge me an outrageous amount of money to do the study using their new IUA simulation model. I understand we've been giving you money to build models of warfare. I want you to use them to do an analysis to determine the pros and cons of the MBT-70 for my meeting with Mr. Packard in mid-November. I want the results by 1 November.'' I explained I had two problems with his request: (1) my theoretical research was to develop mathematics of combat processes—I did not have a credible model of small unit land combat to do the study; and (2) the study is obviously going to be classified and I can't do classified research at the University—it had a moratorium on classified research. DePuy told me he needed my help so I should use whatever mathematics I had to do the study, and that I should start a company to handle the classified material. I said, somewhat facetiously, ''Right, I'll go start a company.''

At the end of that week, I had security folks from the Pentagon come to Ann Arbor to clear an office in a company that I didn't have. I didn't have an office, and I didn't have a company. But that's how VRI got started. Bill DePuy was the stimulus behind the founding of VRI, with me, Bob Farrell, and an economist friend, Dave Brophy, as the founders. The security people said, ''We'll be back next week. Get an office.'' I went to a friend of mine who had a little company and he gave me a desk, and I bought a safe. They came back and said, ''This isn't good enough. You have to have an office.'' I rented a room in a motel-like building and moved the safe there. (The safe moving process became a classic traditional story in Vector's history.) They cleared the office. Bob and I moonlighted from our university responsibilities and did the study from September to mid-November. In addition, since I was President of MORS at the time, I had to attend the MORS Symposium in early November.

Bob Sheldon: Interesting way to start a company. Did you complete the study?

Seth Bonder: Yes, and it became the seed of my career after teaching and research at the University. It is interesting to remember this stressful three-month study because it gave rise to the first hybrid analytic-simulation model of land combat engagements, later referred to as the Bonder-IUA model. Soon after the office was cleared, we received descriptions of the different MBT candidates; descriptions of scenarios for six different land combat engagements involving battalion size forces; 100,000 IBM cards containing performance data for the different MBT candidates, terrain data for the scenarios, etc. We began a hectic process of building a new model of land combat from our theoretical formulations. Bob wrote code to implement the formulations and the scenarios (because I can't type!), and I modified the mathematics to represent the different types of weapon systems in the scenarios and their different firing doctrines.

Then we had to drag boxes of IBM cards over to the computer center to make some trial runs with the candidate MBT systems. John Honig was the government project officer for the study. He wanted us to verify that our analytic model would produce results similar to the IUA simulation before making runs for analysis of the candidate systems. (I was not sure why that was a good credibility criterion for our model.) Our first set of test runs didn't match at all—they were way off. I reviewed our results and believed them to be consistent with the mathematical structure of our model—that is, the code that Bob had written was correctly transforming the model inputs and our mathematical formulations to output combat results for the test cases. We reviewed the IUA simulation code and found numerous programming errors as well as some different decision logic representations between our model and the Lockheed simulation. Once these were corrected, we and Lockheed made some new test runs. John made this a blind comparison—we provided him with our results which he compared to Lockheed's. John thought the comparison was good enough to go forward with the study. That is, the deterministic results of our hybrid analytic-simulation compared quite well with the mean of the results from the IUA Monte Carlo simulation over the six different engagement scenarios.

Bob Sheldon: Was that like a verification, validation, and accreditation (VV&A) process?

Seth Bonder: Not quite. We did not compare our results to real combat operations (which is today's validation process), but did more than check to see that the code implemented our mathematical formulations (which is today's verification process). We compared our hybrid analytic–simulation model's results to results from the IUA simulation which led to the Army approving (this is today's accreditation process) our model for use in the study.

In the remaining time we made hundreds of runs over the six different engagement scenarios and analyzed them in our make-shift motel office. Amidst boxes of pizza, we called the results into John on a Sunday evening who then prepared a briefing for General DePuy to present to Mr. Packard that week. General DePuy was very pleased with the results and our responsiveness. John referred to our battalion-level combat model as the "Bonder-IUA." Later he and I wrote a paper describing the study, presented it at a MORS Symposium, and won the Rist prize.

The first VRI study in November 1969 was a success and had many positive downstream impacts on VRI's future reputation and growth. The structures and equations developed in the SRL and used in the Bonder-IUA (and subsequent) models appropriately became known as the Bonder-Farrell methodology. Because the methodology and models involve differential equations, and because Lanchester pioneered differential equation models of attrition, some people call these Lanchester models. To the extent that this is intended as a well-deserved tribute to a pioneering analyst, this is fine terminology. However the methods of Bonder-Farrell are synthetic in nature and are quite different from Lanchester's, which are holistic models.

So VRI was formed in 1969 at the impetus of General DePuy, with me and Bob Farrell as principals. I moonlighted with VRI for a couple of years during which we hired some early employees (most notably David Thompson, George Miller, Peter Cherry, Dick Freedman and Alan Weintraub, all of whom worked for the company for 30+ years), and moved into a sizable office in 1971. Although I was due for a sabbatical, I took a six month leave of absence from the University the same year. In 1972 I left my full-time faculty position at the University of Michigan, became an Adjunct Professor, and joined VRI as its full-time Chairman, CEO, and President. Bob joined as the first Vice President and Chief Scientist.

Bob Sheldon: After starting Vector, did you continue activities with the University?

Seth Bonder: I did. I periodically taught courses in decision theory and the modeling course after 1972. The period 1960–1972 was focused in academia and was a busy and productive period of my professional OR life. The period 1972–1989 started the OR business part of my professional history which, like the previous period, was affected by many related events and activities of others.

In 1972, Bill DePuy formed and became the Commanding General of the Army's new Training and Doctrine Command (TRADOC). In this role he became the architect of the post-Vietnam Army, responsible for the planning of all the Army's new tactical and operational-level systems, new force designs (organization and composition of fighting units), new operational concepts and doctrine (how units should fight and be supported), and for schoolhouse training. And he became a champion of operations analysis to help him with these responsibilities. He formed a number of OR analysis groups in the command: sizable ones at Fort Leavenworth, White Sands, and Fort Lee; and smaller ones at many of the branch (e.g., armor, infantry, artillery, etc.) schools.

Bob Sheldon: Did you provide support to him?

Seth Bonder: Yes, to him and the command for many years. At the tactical level of operations, we (that is, me and my colleagues at VRI) and the Army analysis community had extensive experimental, research, and model building activities to use as a solid base for conducting analyses at that level. In the early 1970s, VRI began providing analysis support to TRADOC for the development of new tactical-level systems such as the M1 tank, the Bradley fighting vehicle, the Apache helicopter, and others. We provided analytical support for new tactics, and the incorporation of new technologies such as fiber optic, laser-designated, self-designated precision munitions into new systems.

Our initial studies for the Army in the early 1970s used the Bonder-IUA model to examine the impacts of different armored systems on the outcomes of battalion-level engagements. Even though we had used it to support General DePuy in his successful negotiations with Mr. Packard, it was not an easy sell to convince other senior military and the Army's analytic community that it was an appropriate vehicle for analysis of small unit combat engagements. The model, which I referred to as a "hybrid analytic-simulation model," considered groups of homogeneous systems (i.e., multiple systems of a single type all at one location) on the battlefield and *analytically* integrated their detection, firing, accuracy, lethality, and vulnerability stochastic processes to compute attrition deterministically during the course of a battle. It *simulated* movement of the groups over the terrain, command and control of the groups through decision rules, and LOS computations between combatants by embedding a digitized map into the model. This was in contrast to the Army's primary model of small unit engagements, Carmonette, which kept track of individual systems and explicitly sampled each of the stochastic processes using Monte Carlo techniques to develop a distribution of attrition results during the course of the battle. Although both were useful tools, I personally thought the hybrid model was easier to use in a study. It ran faster to simulate a battle, did not require replications, and most importantly, I thought it was easier to interpret the extensive outputs by referring to the underlying mathematical formulations. It allowed us to perform the necessary extensive parametric and sensitivity analyses during a study.

Bob Sheldon: Were you successful in getting the Army to use the model?

Seth Bonder: Only with extensive influence and help from Wilbur Payne, initially in his position as the DUSA(OR), (the highest level OR position in the Army), and later when Bill DePuy made him the Director of the TRADOC Systems Analysis Agency (TRASANA) at White Sands. We started doing studies for DePuy and Wilbur and after a while various Army agencies began using the Bonder-IUA along with their other models. Using a "prototyping process" of model enhancements to conduct studies of new systems, technologies, and tactics, VRI and the government developed a large family of hybrid analytic-simulation models of battalion-level engagements during the period 1972–1989. A VRI branch of the developments led to the Battalion Level Differential Model (BLDM) which was used at least through 1990 and the Bonder-Farrell methodology was adapted for use in the JANUS gaming version which I believe is still being used today.

I think it is important to remember that this family of battalion-level models was built on a strong base of field experimentation (directed or performed by Army analysts such as Wilbur Payne, Dave Hardison, E.B. Vandiver, Walt Hollis, Marion Bryson, Hunter Woodall, and others), and theoretical research using this experimental data. They were not built on a ''base of sand'' as some military analysts who were not involved with or aware of the experimentation and research like to publicly broadcast. As we will discuss shortly, these models later became the basis for building the VECTOR series and other campaign models used in the DoD.

While he was still the AVCS, Bill DePuy and senior leaders in the Air Force, JCS, and OSD were expressing new interest in operational-level issues (corps and echelons above corps)—new operational concepts and doctrine, new force designs, and new operational-level systems. This was perhaps driven by the Soviets focus on the operational level of war as described in a number of classified documents. Efforts were underway to develop or enhance existing operational-level models that could consider the effects of combined arms (multiple army branches) and joint (multiservice) forces in a large-scale campaign.

Bob Sheldon: What were some of the campaign models?

Seth Bonder: If I remember correctly, there was IDAGAM (Institute for Defense Analyses Gaming Model) developed by the Institute for Defense Analyses (IDA). This later became TACWAR (Tactical Warfare simulation) that used an eigenvalue methodology to assess attrition in large unit battles. And there was ATLAS run by RAC which in the early 1970s used a FPS approach to assess attrition and movement of major units. Because of DePuy's interest in operational-level issues in 1970, we did some research in the SRL at Michigan on the possibility of cascading our battalion-level combat methodology into a campaign model.

In early 1971, I received a telephone call from Air Force Lt Gen Glenn Kent, who was then the Director of the Weapons Systems Evaluation Group (WSEG), an OSD agency that I believe served as the military side of IDA. He said he had heard of my modeling research efforts and wanted my opinion on some of the ongoing campaign model development efforts. At a subsequent meeting I told him I thought the IDAGAM eigenvalue approach led to some very unstable attrition predictions and that the FPS approach used in ATLAS was an unscientific hypothetical construct that was not credible to assess the utility of future systems or forces. I think he agreed with me, but then out of the blue said he wanted me to go to Europe for about six weeks on an intelligence related task for the Secretary of Defense. I told him I thought it would be interesting, but couldn't do it since I had just taken a leave from the University to get a new company off the ground. He said he wanted me to go and would have some campaign modeling work for me to do when I returned.

Bob Sheldon: What did he think your qualifications were for the European task?

Seth Bonder: I had no idea then but after the fact believe he thought I had sufficient technical training in OR and reasonable knowledge in the state of the art in military OR modeling and analysis. (Glenn has confirmed that the project and associated reports discussed below were declassified many years ago.)

The project involved interrogation of a defector from one of the Warsaw Pact nations who claimed to be a military OR analyst knowledgeable in Pact and Soviet modeling and analysis activities. I went to Europe with two other individuals—one from the Central Intelligence Agency (CIA) and one from the Defense Intelligence Agency (DIA)—to assess his credibility and learn what we could about the Pact's analysis capabilities and analyses. We operated out of the Fifth Corps HQ (the I. G. Farben building) in Frankfurt and met with the defector at some safe house daily. The other two analysts (clearly intelligence; not OR) worked with me only part of the time I was there. During our discussions, I tried to evaluate his technical OR capabilities, his knowledge of Pact and Soviet OR analysis methodologies, his knowledge of ongoing studies, and other topics I thought would be of interest to the DoD. Although he had some superficial knowledge about US tools, (e.g., Carmonette and my attrition rate work), his technical capabilities were light, and his knowledge about Soviet studies was limited to artillery firing table developments. I believed he had very limited access to what the Soviets were actually doing. I drafted my report to Glenn on the weekends at an office in the Farben building.

Although it sounds like a benign assignment, I should have asked Glenn for hazardous duty pay due to the Baader-Meinhof gang. One night I was evacuated from the VIP quarters due to a bomb threat. A bomb exploded at the front of the Farben building hours before my morning jog past the building, and a bomb exploded near the Officers' Club one day which blew out all the windows on the backside of the Farben building.

Bob Sheldon: How long were you in Europe on this task?

Seth Bonder: It was a little less than the six weeks Glenn thought it would. When I returned and debriefed him, General Kent later asked me to start a program to build a campaign model that didn't rely on the techniques used in IDAGAM or ATLAS, but was more related to the work I had done in developing the battalion-level models. I thought this was appropriate since, as I noted earlier, our models had stronger linkages to extensive field experimentation and related research.

While still in the SRL, I, Bob Farrell, and a couple of others had given some thought to *nesting* or embedding the hybrid analytic-simulation battalion-level models inside a campaign model to compute the land warfare results during a campaign. (This is in contrast to the *hierarchy* approach used later in CEM [Concepts Evaluation Model] whereby land combat attrition data are input to the campaign model for many different situations based on runs of a separate ground combat model). Bob thought we would have some major computational problems in doing so. A campaign could involve a number of corps-size units on the friendly side, with each corps containing about 25 battalions. Recall the Bonder-IUA (and all succeeding ones in the differential family) had an embedded digitized map of the terrain which was used to locate and move system groups and to compute the duration of LOS dynamically between all potential shooters and targets. The latter was a major share of the run time for a single battalion engagement. Bob estimated that it would require eons to compute LOS in this fashion for a large scale campaign. We needed a way to analytically represent LOS in a campaign model rather than try to simulate it as we had in the battalion models. We considered the following logic:

As a shooter moves around a battlefield, he will have a specific *realization* of LOS windows as he approaches a target. If he takes a slightly different path, he will have a different realization of LOS windows. Slight variations in attack paths produce different realizations of LOS windows. Although in planning studies we usually script specific attack paths, we don't know what attack routes will actually be used in a future combat operation, even if it is on the exact piece of terrain used in a planning study. Accordingly, we can think of these as realizations from some underlying LOS stochastic process. Conceptually, as any shooter approaches a target there is some *probability* that he will obtain LOS with a potential target and *the duration of that LOS is a random variable* described by some probability distribution. One might expect that the distribution would have different statistics as the range between shooter and target change. Given this simplified logic and a couple of assumptions, one might mathematically characterize the LOS process as a non-stationary Renewal Process.

About the same time, Bob (and I believe Wilbur Payne) conducted an extensive analysis of the LOS data generated in the TETAM (Tactical Effectiveness Testing of Antitank Missiles) experiments. (The TETAM experiments were conducted by Marion Bryson at the Combat Developments Experimentation Command [CDEC] and I believe Dave Hardison, based on observing in an earlier experiment that there were a reasonable number of 3000+ meter LOS in Europe, developed the tube-launched, optically tracked, wire-guided [TOW] long-range missile.) Bob's analysis suggested that the LOS process could reasonably be approximated by a Markov Renewal Process with parameters that varied with range to the target. We developed the mathematics to integrate LOS into an extended version of the Bonder-Farrell methodology and procedures for estimating the LOS parameters (mean LOS, mean non-LOS windows) from digitized maps of a battlefield area. Subsequent tests using digitized LOS in the Bonder-IUA model with a version that used the analytic LOS formulation produced comparable combat results.

Bob Sheldon: When did you develop the first campaign model under the program with General Kent?

Seth Bonder: Based on my experience with the battalion-level models, my approach to building models was (and is) quite different from what I see in the community today. Using many simplifying assumptions, I believe an initial prototype should be built as quickly as possible, used in studies, and enhanced (continual removal of simplifying assumptions) based on those studies and the needs for future studies. I guess in today's vernacular it would be called a ''prototyping'' development process. I can't prove it, but I believe the recent JSIMS (Joint Simulation System) model development program failed (at a cost of 1–2 *billion* dollars) because it didn't follow this type of approach.

Starting in 1972, the first prototype version, originally called the BATTLE (Battalion Through Theater Level Engagement) model but soon changed to VECTOR-0, was developed in 10 months. It included the effects of some air operations but was primarily a land warfare model. It was used to address some issues for General Kent. I like to tell a little story about one of my interactions with Glenn in this time period.

Bob Farrell and I met with him to discuss results of one of the issues we were looking at with VECTOR-0. Glenn has a wonderful semantic technique for boring in on study results (and the briefer!). We were arguing back and forth about a particular result which was not intuitive to him when he said something like ''no, no, no, you don't understand.'' Sitting on the side, Bob shrugged his shoulders. Glenn whirled on him, and said ''Don't look at me like I'm a stupid general,'' and went to the white board to make his point. At the board he stopped, thought for a moment, understood the point I was trying to make and said ''I guess I am a stupid general.'' Obviously nothing could be further from the truth—he's one of the smartest military analysts I've known, but confident enough as a senior military leader and analyst to admit to missing a subtle result. In our subsequent discussion we agreed that the validity of a planning study's results should not be based solely on agreement with one's intuition because an individual's intuition is based on his or her past experience. Valid model-based analysis about future operations may appear counterintuitive initially, but eventually can be used to enhance one's intuition about complex operations.

The issues we addressed for General Kent served as a basis for developing VECTOR-1 in 1974 which added some rear area operations. Using this prototyping process, a lineage of VECTOR campaign models was developed from 1974 to 1990, including VECTOR-1 Nuclear, VECTOR-2 (which enhanced the rear area representations, including complete joint air operations), VECTOR-2 SWASIA (Southwest Asia), VECTOR-3 (which added complete joint logistical operations, including intratheater airlift), VECTOR-3 intelligence and electronic warfare (IEW), and other versions as needed to address relevant military decisions. New code was written and new documentation prepared with each new major version. This codified all the changes made to the earlier version as it was used in studies and all the new content (entities, processes, etc.) added to create the new version.

Bob Sheldon: When you added logistics, did it include munitions?

Seth Bonder: Yes, for all service systems. It included all classes of supplies, transshipment of supplies, maintenance activities, and all other activities of a logistical system integrated with combat operations.

Bob Sheldon: Did the VECTOR models go through the VV&A process?

Seth Bonder: I don't think so, but let me tell you what I do know regarding the VECTOR models and that process. In the early 1990s, VECTOR-3 was accredited by TRADOC Analysis Center (TRAC) or Walt Hollis for VRI to do some deep strike studies that were mandated by Congress. Perhaps more significantly, in 1982 I directed a study for the CIA in which VECTOR-2 was successfully blind tested against the 1973 Golan Heights campaign (before performing a Mideast balance analysis for the same sponsor). This was what I believe would be called a validation study in that we compared the results of VECTOR-2 when it simulated the Golan Heights campaign to historical results of that campaign. I believe that study produced two useful results for the military OR community and profession.

I believe it is the first campaign model that has been ''validated'' against real-world military operations and the study provided a logical methodology for doing so. VECTOR-2 is a deterministic model which produces a single, central-value realization of a *simulated war* drawn from some underlying *simulation* stochastic process. I expected that we would be comparing this to a single realization of the *actual war* drawn from some underlying *real-war* stochastic process. It turned out that there were eight different reports of how the war unfolded (i.e., unit locations, attrition, etc. over the course of the war). There were two classified reports and six unclassified ones. (I'm not sure if anyone can ever really determine how a war unfolded.) From a methodological perspective we treated the eight reports as eight realizations of the underlying stochastic process of the real war and tested the hypothesis that the VECTOR-2 realization came from the same underlying stochastic process. It was a blind test in that we did not see any of the eight realizations until the comparisons were made. As input, the CIA provided us with system capabilities, force lists and how each side (Israelis and Syrians) would employ them.

The comparisons suggested that there was no reason to reject the hypothesis that the dynamics of the VECTOR-2 simulated war was similar to the dynamics of the actual Golan Heights campaign. From a broader perspective for the military OR profession (not just the VECTOR models), the results verify (or do not reject) our implicit hypothesis that we can model the operations of large-scale military conflict. About the same time AMSAA was testing one of their division-level models (which I believe used the Bonder-Farrell methodology to assess ground combat operations) against similar data.

Bob Sheldon: You modeled the weapon system capability. Did you model the difference in the capabilities of the various countries' ground fighters like Trevor Dupuy did in his Historical Evaluation and Research Organization (HERO) model?

Seth Bonder: VRI and Mathematica had a contract to review the sources of the HERO data and its statistical methods years ago. At that time I thought the HERO model content was spurious so don't want to discuss how he represented different countries. In this validation study, the CIA provided us with the systems' capabilities *as used by the different combatants*. In the first iteration of comparisons, they neglected to tell us that a Syrian tank crew would abandon their tank if a shot fired at them was a near miss or hit their tank but didn't cause any damage. The CIA counted that as a killed tank and thus estimated significantly more Syrian losses that we did. The comparisons were much better when we simulated the same Syrian behavior.

Although the VECTOR models have not gone through the formal VV&A process, the stochastic LOS version of the Bonder-Farrell attrition methodology used in the VECTOR models has been used by other modelers in campaign models such as Vector-in-Commander (VIC), STAR, Eagle, and most recently the Joint Warfare Simulation (JWARS) model. I believe some of these have been through the VV&A process. I was surprised to learn recently that some models are still using a form of FPS methods to assess land combat results.

Returning to my association with General Kent, he was a wonderful sponsor for the development of the VECTOR campaign models. He had great insight and vision as to the kinds of models needed to support the DoD. We maintained our professional association after he left WSEG. It was always a pleasure to occasionally see him at various meetings. The next director of WSEG was Admiral Ed Waller who was charged with closing the WSEG. A smart military man but not analytically oriented like Glenn. During Ed's tenure, I used to meet with him for breakfast once a month and spend an hour teaching him the basics of decision theory.

They were enjoyable sessions since I really do like teaching.

Bob Sheldon: Let me backtrack a bit. Where did the name Vector come from?

Seth Bonder: I'm not sure whether you mean the company name or the name of the campaign models, but let me comment on both. In selecting a name for the company, we wanted something with a mathematical connotation. After exploring a number of the obvious Greek letters (the Alpha Corp, etc.), Vector was suggested. We all liked it since it not only had the magnitude and direction associated with a mathematical vector, but also the *n*-dimensionality of a vector. Our VRI logo (created by one of my grad students, my secretary, and me) contains associated vector line segments (the arrows) and a mathematical vector dot product. My friendly competitor Dan McDonald from BDM used to say, ''They don't know which way they are going—up, down, or sideways.''

As noted earlier, the original name created by Bob Farrell was the BATTLE (Battalion Through Theater Level Engagement) model which was a perfect acronym. But I changed it to VECTOR to get visibility for our new and very small company. In 1972 I think we had eight employees. At that time I did not have any growth plans or desires, but wanted to build our reputation as a high quality, innovative research and analysis company that could help address potential clients' most difficult decision problems. That philosophy continued until 1988–1989 when I had to establish specific growth plans.

Before continuing with my professional history in the 1972–1989 time period, I think it is important to note a somewhat random event in 1972 that led to some of VRI's downstream growth. That year, strictly on a whim, I responded to a Commerce Business Daily Request for Proposal (RFP) for an analysis of national nurse supply and demand issues. I knew very little about the area. Probably the worst proposal I have ever written, but (of course) we were awarded the contract. That got VRI into the health-related business. I hired an Army Medical Corps captain, Tim Doyle, to run the contract and build that side of the business. Over time he developed significant OR, econometric, and information technology (IT) and database work with the Military Health System. I did not get deeply involved with VRI's health business until 1994–1995, surprisingly enough at the request of General Max Thurman. More on that later.

Although we were getting some support from Bill DePuy to do small tactical-level studies, and from Glenn Kent to work on the development of campaign models, I did not know, and was not known by, the senior-level Army planning and operations community. In 1975, General DePuy set in motion activities for an Armor/Anti-Armor Systems Program Review (AASPR) with all the Army's senior leadership to review plans for new major armor programs. All the analyses for that program review were being conducted at the Combined Arms Center in Fort Leavenworth, Kansas. About three months before the review, General DePuy asked me to meet him at Leavenworth to see what MG Cushman (the Commanding General [CG] of Fort Leavenworth) was doing in preparation for the AASPR. He and a couple of majors presented some slides with the worst analytic gibberish I had ever seen. General DePuy asked me ''What do you think?'' I said, ''I think he has no idea what he is doing and may be having a strong negative effect on preparation for the AASPR.'' Bill asked me to spend a little time with Colonel Reed Davis who was directing analyses in preparation for the AASPR with another analysis group at Leavenworth, and to attend the major review.

The AASPR was attended by a cast of thousands—a few Army four-stars, some three-stars, a host of two- and one-stars, and a large number of other Army military officers and civilians. It was so large that only the senior leadership were seated in the main presentation room, while the others watched on TVs in adjoining rooms where I had planned to be. When General DePuy arrived with his entourage, he beckoned me to join him saying, ''I have a seat for you.'' I thought that was nice of him. Little did I realize how nice—I sat with him and General Kerwin, then the Army's Vice Chief of Staff. Bill introduced me to a large number of the current and future Army senior leadership and set the image that I was someone who, through quality analyses and consulting, could be helpful to that leadership. I believe Bill purposely did this to help me and VRI.

At that meeting and subsequent ones, I met Army officers who became clients and eventually lifetime friends such as Generals Max and Roy Thurman, Glenn Otis, Dave Maddox, John Foss, Jack Merritt, Ted Stroup, Paul Gorman, Gordon Sullivan, and others. He provided the entrée for VRI to perform work for many TRADOC agencies, other organizations in the Army, and the leadership at Department of Army (DA) Headquarters in the 1972–1989 time period. Using the VECTOR series models (including one that had player interaction capabilities), I studied a broad range of operational-level issues in the 1972–1989 period, including operational concepts and doctrine (e.g., AirLand Battle doctrine), new operational organizations (e.g., Division-86, the Light Division, Army-86, the Army of Excellence), and operational-level systems (e.g., the Army Tactical Missile System [ATACMS], the Joint Surveillance Target Attack Radar System [JSTARS]).

Bob Sheldon: How many studies were you involved with during this time period? Do you have any favorite ones?

Seth Bonder: Not counting my Army Science Board work, prior to 1989 (when we switched to quick response analyses) I would estimate that I technically directed or closely oversaw five to six projects per year at VRI for the Army, other DoD agencies, and industry, so probably 75–80 from 1975 to 1988. Many were studies and some model development projects. VRI had great technical professionals to work with which made this workload feasible and stimulating. Even with my business development and managerial responsibilities, I tried to spend at least 30 % of my time working on technical projects (based on my usual 70–75 hour weeks). I was never a believer of having separate marketing and production staffs. In an analysis firm, I always thought that technical folks should be interfacing with potential clients to determine what was needed to help with their decision issues *and* to assess what was feasible to achieve in the allotted time. I believe the pure business developer did not have these capabilities.

I really liked doing analyses for clients, so many were favorites. I tried to make sure that the VRI studies and development projects were *useful, useable, and used* by the clients. I particularly liked those in which I not only helped the client with his decision issues, but learned something about operational phenomena. Some examples during this period might illustrate the point. Through Glenn Kent I met Jasper Welch at the Air Force Studies and Analysis Agency (AFSAA) when he was a Colonel. Later when he was a one-star and I believe the Assistant Secretary of Defense (ASD)-Atomic Energy, he asked me to do ‘‘something bright’’ for the Defense Nuclear Agency, an agency which had a small set of favored contractors that did not include me. At that time the North Atlantic Treaty Organization (NATO) strategy against a Soviet offensive in Europe was to try to stop penetration of the Inter-German Border (IGB) conventionally and if that failed, use tactical nuclear weapons to stop the penetration. We thought it would be useful to understand and perhaps develop some good nuclear targeting priorities. In doing the study we developed the targeting priorities for our nuclear systems. In addition, this study in conjunction with a couple of others, gave rise to a powerful insight about tactical nuclear operations which I thought brought the whole NATO strategy for European defense into question. So did Jasper. (I can't describe the specific insight because I don't know if the study has been declassified.) Jasper asked me to brief the SACEUR (Supreme Allied Commander, Europe) within a couple of weeks. The study was useful to the DoD and I developed some insights regarding the interactions between tactical nuclear and conventional warfare operations.

As another example, in doing a number of studies for General DePuy during the early 1970s, we analyzed a large number of simulated US battalion-level defensive engagements against an attacking Soviet force. We observed that the instantaneous *loss exchange ratio (LER)* – the ratio of the rates of attacker and defender losses – was very high and relatively independent of the threat size early in the battle because of concealment and first shot advantages accrued to the defender. The LER advantage moves to the attacker as the forces become more decisively engaged and the concentration and saturation phenomena come into play for the attacker. This suggested that an indepth use of a large number of small-unit engagements in which defenders get off a small number of shots (operate at the high end of the LER) and then fall back to prepared positions to repeat the process would be an effective tactic for Europe. In essence, to prevent a Soviet penetration, the concept was to trade ground for high attrition of the attacker while continually increasing the density of the defensive positions. I subsequently wrote a paper describing this as a "Variable-Density" defense. One of my colleagues convinced me this would not be a good acronym, so I changed it to the "Dynamic Density" defense. General Shy Meyer who was the CG of the 3rd Infantry Division (ID) in Europe at the time (later became the Army's Chief of Staff) thought that he could find a large number of such fall-back positions in his sector to make the concept work. I believe Bill DePuy used these ideas as the basis for his Active Defense concept. The study helped the client, and I learned something about combat operations.

Bob Sheldon: Sounds like you and General DePuy had a good working relationship.

Seth Bonder: We did and it blossomed into a lifetime friendship after he retired in 1977. When he retired he wanted to do some consulting for VRI. I told him we needed to wait a year to avoid any misperceptions related to our contractual work with TRADOC. After 9 months of consulting with some of my competitors, he complained that they never gave him substantive work to do—he would spend minimal time on a large number of proposals and projects each consulting day so they could use his name for marketing purposes. He became a VRI Associate shortly thereafter, working two to four days per month until a year or two before he died in 1992. Most often he would come to our Ann Arbor office (he loved walking around the University's campus). He'd walk in, salute, and say: "Reporting for duty." He would help fix a project that was having some difficulties, help structure a new project, or the two of us would work on an important issue for the Army. He often prepared papers describing the work he did on fixing and structuring projects or think pieces on Army and Joint service issues – products not often provided by senior military consultants. He was the first member and Chairman of my small Advisory Board (AB) that I formed, not only to review ongoing programs, but to serve as a sounding (more often pinging) board on how I was managing and leading the company.

Bob Sheldon: Was all of your work at VRI for the federal government?

Seth Bonder: Initially yes, but through contacts I made at the ASB, MORS, and other meeting venues, we started doing a small amount of work with defense industry. I believe our first industrial client was Raytheon in the early to mid-1970s. Initially it was analysis in support of their air defense programs and then grew to support their land warfare programs. I believe we had more than 25 quick response projects with them over the years. Generally, our analyses examined the operational utility of the systems that industry was developing and assessed design capability tradeoffs to make the systems more cost-effective. Through the 1970s and 1980s I expanded our industrial client base to include Lockheed, IBM, Boeing, United Defense, LTV, and a number of others who were developing systems for the military services.

One of our largest studies for an industrial client, and perhaps the most difficult one for me, was done for the Georgia Lockheed Aircraft Corporation (GELAC) in the 1984–1985 time period. The purpose was to assist them in the development of a new intratheater airlifter that could be operationally effective in the Northern Army Group region of NATO and in Southwest Asia. It involved the addition of complete joint service logistical operations to the VECTOR series models to create VECTOR-3, the development of scenarios and databases for both regions, and extensive parametric analyses to examine the operational effectiveness of alternative airlifters. We had the continuous participation and review by a board of senior Army and Air Force generals chaired by Bill DePuy. This is where I first met Bill Moore, former Commander in Chief (CINC) of the Air Force Air Mobility Command and Fritz Kroesen, the former CINC, US Army Europe (CINCUSAREUR) whose sedan was fired on by the Baader-Meinhof group in Germany. He looked like a Hollywood type-cast Army general and was deceptively very, very bright. The program was substantially underbid and grossly mismanaged by one of my VPs. The project was headed for a gigantic loss (about a year's earnings for VRI), and possibly default. At Bill's urgings, with six months remaining I relieved the VP, and spent five to six months of 15–17 hour days, six days a week managing and working on the project team to complete it with about 50% of the anticipated loss. I was so fatigued that a cardiologist friend made me take a stress test and go through a catheterization procedure. He assured me nothing was wrong that a long beach vacation wouldn't cure. He was right.

Not long after that I directed what was probably the first operational effectiveness analysis of a potential stealth (low-observable) fighter for the Lockheed's Skunk Works. It was interesting to learn that it could have a dramatic impact on an air campaign if employed appropriately. It not only was itself more survivable, but its low-observability made it easier for it to take out the threat's air command and control system, thus making all the nonstealthy fighters in the fleet more survivable.

Bob Sheldon: Did you do any work with the European OR community?

Seth Bonder: Yes. That's a dimension of my career that started with a 1974 NATO conference in Munich organized by Reiner (Sam) Huber, who then was the director of military analysis at the IABG (Industrieanlagen-Betriebsgesellschaft) of Messerschmitt Aircraft. Although I had never met him, he invited me to present a paper and chair a session on new combat models. (That was the beginning of a lifelong friendship with Sam and his family, with visits in Germany and the US every couple of years.) I was also asked by Ted Roderberg, the Director of OR at the Supreme Headquarters Allied Powers Europe (SHAPE) Technical Centre (STC), to present some talks to his staff before attending the Munich meeting. At the Munich conference I met a number of other European analysts including David Dare, Jeff Hawkins, Hans Hoffman, John Gibson and others from many different countries whose names I have unfortunately forgotten. In subsequent years I and my VRI colleagues attended a number of other conferences with this community, which led to studies for some of them in the 1970s and 1980s. A number of the analytic groups throughout Europe implemented various versions of the Bonder-Farrell hybrid analytic-simulation differential models of combat for analyses of battalion-sized engagements.

The most significant project was performed for STC during the period 1979–1982. The purpose was to perform an Armor/Anti-armor capabilities analysis for each of the eight Corps sectors in NATO's Central Region using the VECTOR-2 model, and in the process, transfer the model to STC and train their analysts in its use. The study required an enormous data collection effort to obtain data for each of the Corps and their opposing Warsaw Pact threats. The training was accomplished by having the STC analysts work hand-in-hand with the VRI analysts, some of whom were stationed at STC for extended periods. The study was completed and Corps-by-Corps analysis results provided to NATO's senior leadership in early 1983. Based on some of the initial work by Paul Haraschu at STC, Bob Farrell built a methodology for constructing an aggregate "meta-model" to simulate VECTOR model campaigns based on previous campaign results. He applied and tested the methodology using all the STC results to develop the first version of the "MACRO" model which could simulate central region campaigns orders of magnitude faster than VECTOR-2. The methodology and subsequent versions of MACRO were extremely valuable in conducting analyses after the Cold War ended.

Mike Garrambone: You've talked about your work in the 1972–1989 time period, and I know you were involved with other professional activities besides MORS. Could you comment on these?

Seth Bonder: Sure. In addition to periodically giving some talks at MORS meetings, I was actively involved with the ASB, the Operations Research Society of America (ORSA), the International Federation of Operational Research Societies (IFORS), and with advisory activities to a number of universities. Let me comment on each of these.

My association with the ASB started in the early 1970s and was instigated by Wilbur Payne. Wilbur and Dave Hardison wanted to put an operations researcher on the Army Science Board. (In those days it was called the Army Science Advisory Panel – the ASAP). The Army's Chief Scientist, Marvin Lasser, refused stating they just wanted technologists on the Board. Wilbur and Dave threatened to form an ORSAP, an OR Science Advisory Panel, with a member of one on it, me. Marv was worried about the competition so he agreed to have an OR type on the Board. I believe I was appointed in 1972–1973 and have been on it for many years.

In the early 1970s there were only 25 full members on the Board, mostly from hardware industry firms. Given their particular corporation's business, it was easy to identify potential conflict of interest issues on ASB studies so they could recuse themselves when appropriate. Not long after I joined, Norm Augustine (then the Army's Assistant Secretary for Research and Development [R&D]) told me he had received a letter objecting to me (an OR type) being on the Board because (as part of my VRI business) I did studies across a spectrum of Army problem areas and couldn't be objective in performing a study for the ASB. Norm's response to the letter was typical Augustine: ''If he were objective he wouldn't know anything.'' I stayed on the Board.

Bob Sheldon: How long were you on the ASB?

Seth Bonder: I was on the ASB until 1978 and left after an argument over the Copperhead system with Percy Pierre who was the new Assistant Secretary of the Army for Research, Development, and Acquisition (RDA). Percy was a big advocate for Copperhead (which was a laser-guided artillery round), but apparently was having some problems selling it to Congress. At one of the plenary meetings, he and the four-star CG of the Army Materiel Command (AMC) touted Copperhead's virtues. Having recently completed some studies on the system, I pointed out that it would be effective if and only if the Army made some significant doctrinal changes for its employment. Percy was furious. At break time he cornered me and said, ''I'm reconstituting the ASB and if you want to be on board the ship when it leaves the dock I want your support for Copperhead.'' I realized he was trying to buy my advocacy so I got upset and said something like, ''What makes you think I would want to be on your ASB?'' Needless to say I wasn't.

The story raises an important principle, at least for me. I believe analysts should be objective in developing and providing information to assist clients in making rational decisions. The client may use that information to advocate their position on an issue. I don't believe the analyst should become an advocate – you need to maintain your reputation as an objective developer of the information. I have no problem briefing study results if they support the client's position. And I have no problem with clients not using my study results if they don't support their position. I do have problems when clients want to ''modify'' some of the results as a basis for advocacy–then my name comes off the study. Ethics and principles are critical in maintaining one's professional reputation.

Bob Sheldon: That's what General Kent says too.

Seth Bonder: That doesn't surprise me. He understands the importance of honest and objective analysis.

I was off the ASB for four years, and in 1982 the new Assistant Secretary Jay Scully put me back on the Board. I was on through 1992 when I quit due to business commitments and suggested that Peter Cherry become a member. I was reappointed in 1998 when Peter's tour was up. My latest tour on the ASB expired in 2005.

As with MORS, I met some wonderful bright individuals on the ASB (such as Russ O'Neill, Norm Augustine, Larry Delaney, Jack Vessey, Larry O'Neill, Gil Decker, Walt Laberge, Dick Montgomery, Joe Braddock, George Singley, and many others) who were dedicated to helping the Army. Many of these individuals have become lifelong friends. Although I worked on a number of studies for the National Academy of Sciences and the National Research Council, the ASB was the primary vehicle for performing many of my *pro bono* study activities during the 1972–1989 period. One of the early studies was analysis of the Dragon antitank system operational test data to help General DePuy (then CG TRADOC) make a procurement buy decision. Another helped General Miley (then AMC Commander) determine how to fix the SAM-D (now Patriot) air defense system and its testing process when one of the first ones failed and essentially fell off the test pad. In 1983, I chaired a panel for Secretary Scully to review the development of the Army's All Source Analysis System which was experiencing significant delays. With the help of some bright ASB intelligence and IT members, we determined that applications software development was the critical problem causing delays. I recommended that an initial system with lots of memory be procured from the black community, fielded to an operating division with a cadre of programmers, and that the software be developed jointly with the operating personnel. We estimated a full Division set could be operational by 1987. The Under Secretary complained that I was going around the established acquisition process (which was correct), and gave the program to one of the national laboratories to complete. I don't believe anything was fielded until the late 1990s.

One of the nice benefits of being on the ASB, at least years ago, was the occasional opportunities to be with the operational troops in the field. As an example, in the mid-1980s five of us on an Intelligence and Electronic Warfare (IEW) panel were invited by General Pat Criser, CG of the 3rd ID, to observe EW operations as part of the REFORGER (Return of Forces to Germany) exercises in Germany. During the exercise, we were helpful in getting EW activities more integrated with the operations side of the division and I was able to observe the ''fog of war'' first hand. For a couple of days Pat put us in a helicopter to observe operations of various units during the exercise. At the command briefing one evening, one of the units reported that it had successfully fought its way across the Donau River, which I knew was not true since I had just returned from that unit a few minutes before. I expect today's IT capabilities might prevent errors of this type.

Perhaps the most memorable enjoyable ASB study, and possibly the most useful one, was the ad hoc ASB study I chaired in 1986 to develop competitive strategies for defeating the Union of Soviet Socialist Republics (USSR) and the Warsaw Pact. The ASB Chairperson wanted me to have 10–15 ASB members on the study. Fortunately, I was able to argue for a much smaller team which included Larry Delaney and Larry O'Neill –both broad thinkers with strong technical capabilities. This study gave rise to some new doctrinal thinking and started a large number of black programs, many of which have resulted in technologies used by our operational forces in the field today. I think this study is an example of the wise axiom that, given a critical mass, productivity of a study team is inversely proportional to the number of team members.

Mike Garrambone: Any relation between your ASB and ORSA activities?

Seth Bonder: No, not really. The ASB is a scientific advisory body while ORSA is an OR professional society, although it has been involved with a few national issues such as scientific assessment of the START-II Treaty. I was ORSA's representative and Norm Augustine was the Aeronautical Society's representative to discussions with the State Department regarding the impacts of START-II. Since ORSA was heavily governed by academics, there was a little MORS overlap with folks like Jack Borsting and Dave Schrady. ORSA is the venue where Saul Gass and I became professional colleagues and good friends.

I joined ORSA as a student member in 1962. I started attending the national meetings after graduating from OSU in 1965 and became active in the Military Applications Section (MAS) for many years. I chaired the MAS in the late 1960s or early 1970s, but don't remember the exact year. I ran numerous technical sessions, organized and chaired a national meeting, became Secretary in 1974 and President in 1979. That was a difficult time financially for the Society. We only had about 20% of our annual expenses (for the meetings, journals and the administrative office) as a cash reserve. Any slip in revenues from dues, journal subscriptions, or meeting attendance would have been a disaster. Dave Schrady was Treasurer at the time. With his help and Council approval, I drastically cut expenses, raised dues by almost 300 percent (that made Council members very nervous), and set a policy that no new initiatives could be started until the cash reserve equaled the annual expenses. I cut all journal pages in half, which had all the editors screaming at me and David. But the Society survived and grew over the years with a sound financial basis.

For many years, The Institute of Management Sciences (TIMS) and ORSA ran competitive meetings and a lot of redundant activities for essentially the same membership. During my active time on the Council, we combined the meetings and (with the help of Sid Hess) I created the concept of a Joint Council of the two organizations to minimize the redundant activities between them. This trend culminated with the merger of ORSA and TIMS in the mid-1990s into the Institute for Operations Research and Management Sciences (INFORMS).

As I did through my activities with the University of Michigan, VRI, the ASB, and MORS, I developed professional associations, and in some cases lifetime friendships, with yet another group of professionals. Some of these include Al Blumstein (Carnegie Mellon), Saul Gass (University of Maryland), Jack Borsting, Dave Schrady, Bill Pierskalla (UCLA), Mike Thomas (Georgia Tech), Tom Magnanti (MIT), George Nemhauser (Georgia Tech), Art Geoffrion (UCLA), Dick Larson (MIT) and many other bright academics and researchers. (Some even knew how to have fun—you should ask Saul Gass about the late evening parties after long Council meetings and dinner.) I have very fond memories of actively working with all of them for many years to establish and grow the OR profession. I was able to expand these professional activities on an international scale by presenting plenary papers at the tri-annual meetings of IFORS – an organization with over 130 nation members –and even more so when I was elected as a Vice President of the Federation in 1985.

During this period I was also involved with ORSA's founding of the ''Visiting Lectureship Program'' and was one of its early lecturers. The objective of the program was to expand the academic side of OR by having senior professionals visit with departments who had expressed an interest in starting an academic OR program in their university. The lecturers would present applied or theoretical research seminars to interested faculty and students, participate in a question and answer (Q&A) session about the OR profession, meet with potential faculty regarding possible curriculum designs, and other activities to promote the development of OR programs and attract students to the OR profession. Because of my academic activities via ORSA and the University of Michigan, and my applied OR, engineering and business experience through VRI, I have served on a number of advisory committees for engineering and business schools throughout the country, including the University of Southern California (USC), Virginia Tech, OSU, University of Miami, and others.

A couple of points before turning to the 1989–2000 period of this history which was exciting for me professionally and challenging for me personally. I noted earlier that the Army had performed extensive experimentation in the 1950s, 1960s, and 1970s to develop fundamental knowledge about combat processes at the tactical level which we used as a basis for developing many of the combat models. Much of this experimentation was conducted at the Army's Combat Developments Experimentation Command (CDEC). In 1981 there appeared to be a shift to more operational testing of systems at CDEC and less real experimentation. This trend continued until the early 1990s when I believe CDEC was closed.

Bob Sheldon: What was the rationale for that decision?

Seth Bonder: I really don't know. TRADOC had some good senior leadership at that time who understood the value of analysis and experimentation, including Generals DePuy, Starry, Otis, Thurman, and Foss. The best person to ask would be Marion Bryson who ran CDEC for many years. Perhaps there was no longer a need for it then. I believe there clearly is a need today given the major changes in operational processes and technologies associated with net-centric warfare, systems-of-systems, counterinsurgency operations, stability operations and other new activities for our operating forces. I don't think we have an empirically based understanding of these processes for their effective implementation or experimental data as a basis for building models to help us develop this understanding.

As noted earlier, I did not have any growth plans when I started VRI, effectively in 1971. I was primarily interested in establishing a quality reputation for the company. I enjoyed doing technical work and didn't want to be just a corporate manager. Although we had grown every year until the mid-1980s, after a couple of minimal growth years, Bob convinced me we needed to develop plans for more aggressive growth. This would provide needed career-growth paths for long-term employees and allow us to bid on larger programs in the DoD. I decided we would try to achieve this growth internally (i.e., not by acquisition) by expanding our military and health analysis lines of business and by leveraging these to make an entrée into the IT business. Much of this happened in the 1989–2000 period of my career. This is probably a good time to segue into this period of dramatic changes in my professional and personal life.

Bob Sheldon: What made it so different from the 1972–1989 period?

Seth Bonder: So many things changed. There was a major change in the global security environment which led to changes in how I did studies, the types of decisions addressed, and the decision makers I worked for. In addition to the tactical and operational-level issues addressed in the previous two decades, in the 1990s my analysis activities expanded to include strategic and policy level issues for senior leadership of the Army, JCS, and CINCs of Unified Commands (now referred to as Combatant Commands). It also was the first time I became deeply involved in VRI's health analysis business. I had a major life style change toward the end of this period, but let me take a moment to remind you what the security environment looked like before this period of time as background for these changes.

Bob Sheldon: Are you referring to the Cold War?

Seth Bonder: Exactly. Throughout most of the Cold War, the Soviet-led Warsaw Pact had a significant conventional force advantage over the NATO alliance—approximately two-to-one strategic advantage in armored systems and a much greater operational and tactical advantage if they chose to mass in a specific area for an attack against NATO. NATO relied on nuclear means to deter a massive Soviet offensive in Europe. Strange as it may sound, this was a relatively stable global security environment. The US and the Soviet Union were military superpowers who maintained strong influence over policies and activities within their alliances. The Soviets simultaneously maintained significant control over many Third World country military activities through economic means, technology controls, and military pressure. Although regional conflicts occurred during this period, with hindsight from 2006, it really was a relatively stable security environment.

Although many analysts may not remember this, there was also stability for defense planning and associated OR analyses during this time frame. And there was very little uncertainty in our defense planning activities. The focus was Europe—the US and NATO were committed to stop a Soviet-led Warsaw Pact attack. The threat was clear. We knew the size and location of Soviet and Pact forces. We knew their attack options, how they would fight, their equipment, their command and control processes, and many other characteristics. Based on years of analyses, we understood the warfighting dynamics of a potential conflict, and we recognized that *feasible* conventional changes in various components of warfighting capability (e.g., new systems, force designs, and force size) could not alter the conventional force imbalance. Changes that were made I believe were intended to raise the nuclear threshold. I apologize for the political-military history lesson but it provides the backdrop for why I made so many changes in VRI's analysis business starting in 1989–1990.

Bob Sheldon: What motivated the changes at that time?

Seth Bonder: Eventually it was the end of the Cold War, but I started making changes before then due to a couple of conversations I had with General Max Thurman in 1987. I met Max many years before when he worked for General DePuy, and did some personnel-related studies for him in the early 1980s when he was the Army's Deputy Chief of Staff for Personnel (DCSPER). He was still the Vice Chief of Staff in 1987. We would occasionally get together to discuss some of the studies I was doing for TRADOC and other Army agencies.

During one of these meetings he casually said, "Soviet communism will collapse soon and the Soviet Union will be dissolved," which also implied that the Warsaw Pact would break up. I thought he was smoking something or just losing it. He said, "The Wall will come down in a couple of years." Max obviously had knowledge that I didn't have. My guess is that he had been privy to conversations taking place between the Reagan administration and the Soviet Union led by Mr. Gorbachev. Not long after that, Max became the TRADOC Commander. At a subsequent meeting in his office at TRADOC

Headquarters, we discussed the ramifications of his conjecture. It seemed clear that without the influence of both superpowers, the world would become much more unstable. He suggested that there would be major changes in the military forces of the US and NATO nations, and that I should ''set up a hot plant to conduct extensive analyses over the next five to ten years.'' Sure enough, the Conventional Forces in Europe (CFE) talks and negotiations started in 1989, which eventually led to the treaty. The events in 1989–1995 proved how omniscient Max was.

Bob Sheldon: Were you involved in the CFE activities?

Seth Bonder: I was not in the line activity of structuring the negotiation packages, but I started doing CFE related studies in late 1989 in support of it. In 1989, General Thurman left TRADOC to become Commander of US Southern Command (SOUTHCOM) where he led the Panama invasion (''Operation Just Cause'') and captured Mr. Noriega in 1990. General John Foss replaced Max as the TRADOC Commander and asked me to do a risk analysis of the potential CFE treaty. I was pleased that General Glenn Otis, who had just retired as the CINCUSAREUR, joined VRI as an Associate and agreed to work on the CFE study with me.

The intent of the CFE talks was to asymmetrically reduce forces in both the Warsaw Pact and NATO (accordingly, oftentimes referred to as the Asymmetric Force Reductions talks) to a position of parity of both forces. This was a gigantic step for the Soviet Union. I may be slightly off from the exact numbers, but the Warsaw Pact had something like 56,000 tanks and NATO had 20,000 tanks –reductions would bring both alliances to the same level somewhere below 20,000! There were to be similar reductions in artillery, aircraft, and other weapon systems. The eventual treaty involved a large number of details regarding which nations would take which reductions, verification of reductions, etc.

Bob Sheldon: Why would the Soviets agree to such a large disparity in reductions?

Seth Bonder: I don't know, and I would be surprised if many people knew at the time. The logic provided to the public was that the Soviets could not afford to keep up with President Reagan's large expenditures to modernize the US military forces (his ''offset strategy''). I'm sure there must be a number of books written on that topic since then. Anyway, this quick-response study was completed in late 1989 or early 1990. It concluded that there was no ''operational minimum,'' and that there could be significant risks with the treaty. Let me briefly comment on each of these.

Although there was agreement that there should be parity of forces, it wasn't clear what that level should be. Studies by RAND suggested that there was a floor that the reductions should not go below. The VRI study indicated that there was no such floor—it would not be detrimental, from a warfighting perspective, to go further down the parity line than the level being discussed by the negotiators. After lots of discussions and comparing analyses, the RAND analysts sort of agreed that there might not be an operational minimum.

Based on the VRI analysis, I concluded that there could be significant warfighting risks when the forces reduced to the agreed-upon parity levels because the basic warfighting physics appeared to change when the major force imbalance was removed. In contrast to the Cold War period when feasible changes in warfighting components (such as new systems, additional forces, and reinforcement capability) could not alter the warfighting imbalance, this study suggested that relatively minor advantages or small changes in many of these dimensions by either side could significantly affect warfighting capability when the alliances were at those suggested parity force levels. Modernization had an especially large impact on warfighting capability. Accordingly, I thought that slight modernization improvements by the Soviets, additional unilateral reductions by some NATO nations, slight delays in expected warning times, and other small changes in any of these dimensions *after the treaty was implemented* posed significant warfighting risks for the NATO alliance.

When the study was completed, John Foss asked me to brief the results to General Jack Galvin who was the SACEUR (the military commander for all of NATO, and also the Commander of the US European Command –EUCOM). Although I briefed him at SHAPE (Supreme Headquarters Allied Powers Europe) Headquarters in Mons, Belgium, I only briefed his US commanders because the study was classified Secret, No Foreign.

General Galvin's SHAPE HQ was in Mons and his EUCOM HQ was in Stuttgart, Germany. NATO HQ is in Brussels. At the end of my briefing in the morning, Jack thought the results were so important that I should brief it to all the NATO commanders in the afternoon. (I may have this number wrong, but believe he had 60 four-star generals reporting to him.) I reminded him that the study was classified No Foreign. He got on the phone, quickly removed that impediment, and I briefed the NATO commanders that afternoon. Although he liked the study, General Galvin was upset and wanted to know why a study about his theater of operations was being done by someone else. Before I could attempt an answer, he said: ''If you do studies about my theater, I want you to do them for me.'' And we did a few studies for him over the next three years. One of them examined the impacts of nonlinear operational concepts and deep-strike precision munitions on warfighting capability after CFE. Another was to help him design NATO's Rapid Reaction Force in 1991 which I will comment on a little later in this interview.

As you know, the CFE treaty led to the breakup of the Warsaw Pact alliance, reunification of Germany, collapse of Soviet communism, and dissolution of the Soviet Union in the early 1990s. This further reduced the massive threat to NATO and led to unilateral reductions by our NATO allies and major cuts in US defense budgets to achieve the ''peace dividend,'' which of course led to major reductions in our military forces.

Before coming to TRADOC, General Foss was the CG of the 18th Airborne Corps whose forces would fight in Southwest Asia (SWA—Iraq, Iran, etc.) if a conflict required US forces in that region. Apparently, John was already concerned about the region's instability because in late July 1990 he asked me to assess US war fighting capability in the region using one of TRADOC's Southwest Asia scenarios (called SWASIA). In early August, Saddam Hussein invaded Kuwait—John must have had a crystal ball! Operation Desert Shield started thereafter and the buildup to Operation Desert Storm began. The air war started in January 1991, and the ground war about a month later. We completed our study by February 1991, including assessment of the scenario's version of the left-hook strategy.

Bob Sheldon: Sounds like General Thurman had it right.

Seth Bonder: Absolutely. He was right about the Soviet threat to NATO going away; about the world becoming more unstable with Desert Storm, Bosnia, Kosovo, some others, and the current Iraq conflict as evidence; and the need for major changes in the military forces of the US and NATO. He was also right about VRI performing extensive analyses to assist military leadership with some of these changes. I've already mentioned the CFE and Desert Storm ones and will get to others shortly after I comment on what the global security environment now looked like after collapse of the Soviet Union and Desert Storm.

Desert Storm gave us a view into future potential conflicts: They would be ad hoc coalition conflicts, contingency operations requiring power projection, and opponents could have modern technologies for use on the battlefield because they were becoming available to all nations. In a few short years, the Cold War's two-superpower era transformed into a new multipolar world that was more disorderly, more unstable, and more uncertain, with increased likelihood of multiple Third World conflicts. And the uncertainty was large. We no longer were going to Europe to fight the Warsaw Pact. We didn't know what type of contingency operations we were going to be committed to (later referred to as major theater of war [MTW], small scale contingency [SSC], stability and support operations [SASO]); we didn't know where geographically we might have to operate; didn't know who our coalition partners might be; and we didn't know who the threat might be, how he would be equipped, and how he would fight.

The US was the sole remaining superpower in this environment. We created a new National Military Strategy which emphasized regional conflicts and crisis response. The National Military Strategy success criterion was to apply ''decisive'' force to win swiftly and minimize casualties. The strategy was to draw down overseas forces and rely on power projection for contingency operations. As Max had forecast, US military capability had to be restructured to achieve the peace dividend by considering changes in force structure and design, modernization, forward stationing, strategic lift, mobilization, active-reserve force mix, and other dimensions of our military capability.

I believe VRI and other OR analysis organizations such as Center for Army Analysis (CAA) formerly Concepts Analysis Agency, and TRAC, played a significant role in this restructuring of the Army and the other military services.

I think it would be appropriate at this point to discuss the 1989–2000 period of my career. But first, I have to go back a bit to 1987 and set the stage by explaining my role with Vector Analysis.

I ran a lot of the projects. I would direct three or four at a time, and people would be implementing and creating results, but I always wanted to be involved no matter how big the company got because I liked doing analysis. So I would direct two or three, and I may even have done one with two or three people; I had some superb people like Mike Farrell (as distinguished from Bob Farrell, whom I mentioned earlier) and George Miller who could implement. We would brainstorm together; then they would go run some models and prepare some results. When I say ''I,'' I really mean I and my teams at Vector. But, for lots of different reasons, during the period of 1989–2000, we grew beyond the operational level of analysis to what I think is strategic policy-level analysis. We created a new OR analysis paradigm that had not existed prior to 1989.

VRI expanded significantly in size. And I had a lifestyle change at the same time. There was a new military strategy that came out at that time. I think the buzzwords were 'Win Decisively'; that is, you have a decisive force to win swiftly and minimize casualties. That was the new criterion. During the Cold War, we did studies and, if we lost 100,000 people, this was expected. Then, in Desert Storm, we lost around 100. This led to the opinion that from now on or in any war, we cannot lose more than 100. But now and at the end of the Cold War, the entire US military force had to be restructured because we needed a 30–35% cut in forces in order to achieve that peace dividend. We knew there were going to be changes in force structure and force design, there were going to be big changes in modernization, there were going to be big changes in strategic lift and strategic mobility because we went from prepositioned forces in Europe to a force in CONUS (Continental United States) that we had to take wherever we wished to go fight. The whole concept of how we were going to fight wars changed in a period of three or four years. That led to lots of changes in how we did analysis. VRI and many other organizations played a big role in this restructuring of the Army, and I assume the Air Force and the Navy, although most of my work was for the Army. It was done in joint analysis, but I was not restructuring the Air Force. I was restructuring the Army.

Analysis became very difficult to perform for a number of reasons. First of all, budgets got tight. Not only the budget for analysis, but the budget for the things you wished to buy. We had to think about power projection. We never thought about that before; the analysis was all done, we're in Europe, and we're starting a war. Now the issue was that we had to mobilize back in the US, then we had to embark to go some place, then we had to debark, and then go fight a war. We were then faced with the mobilization problem, the deployment problem, and then the theater employment problem. We had to worry about them simultaneously, because they traded off between each other. Let me give you an example. You can have a force you deploy very fast because it is light, but when it gets there, it cannot do anything. On the other hand, you can take a long time getting a force there, and when it gets there, it will be useful. How to make those trades and also trades back into the mobilization process of railroads and materiel getting to our ports, what ports do they go to, what airfields do they go to, what do you fly over first, what do you send by ship—all of those topics became analysis issues from 1989 to 1996, and I think remain even now.

So, we had the power projection problem, and we had a new National Military Strategy. You had to have decisive force, so you could win swiftly and lose almost nobody. The decision making process in the Pentagon became very volatile. I do not mean they were screaming at each other, but things changed rapidly. The Services filling the various roles were changing. Who was doing deep strike? Was it Army or the Air Force? We weren't quite sure. Funding changed. You may have had funding for a study, which had funding for the systems you're looking at, and the next morning that would change. The assumptions on funding would change; the administrative policies of how you did studies would change.

The things I refer to as uncontrollable, I sum all up in what I call operations situations, including who the threat is, where the threat is, what does the environment look like, what does the threat have, how are they going to fight, all the things you cannot control in an analysis. During the Cold War, we knew all of those. Post-Cold War, you knew none of it. It came down to: you can't just pick a spot to fight and therefore, have these systems. I had to worry about where in the world I was going, who I was going to fight, and who my coalition partners were in order to arrive at some reasonable alternative systems or force structures or force designs or strategic lift or all the other issues. Analysis thus became very different. I know a number of studies during the Cold War that took two years to do, would involve 20 to 30 analysts, and, after two years, nothing much came of it because you just kept doing the same studies over and over because you knew what was in Europe. Now the paradigm for analysis changed, and I made a big change at Vector. I presented some papers on it, and I think other people did similarly. We moved to what was then called "quick reaction analysis." I think the longest one we ever did was five months. On average, they were two or three months. Some of them were two weeks. Some of them were days.

Mike Garrambone: What was the pressure for those older analysis types? They used to take much longer.

Seth Bonder: There was no pressure for those.

Mike Garrambone: Now all of a sudden you have one-third the time or less for the "quick reaction" types?

Seth Bonder: Much less, because they needed answers. They were making decisions on restructuring on all of the types of problems I mentioned earlier: the force structure, force design, new modernization, and strategic lift. They wanted answers for all of these strategic policy-level issues tomorrow. Let me give you an example. In fact, it led to the concept that I call continuous analysis.

Analysis used to be an event. Somebody had a problem, they came to you, you did your analysis and you gave them the answer and you went away. I convinced General Carl Vuono when he was Chief of Staff of the Army, by saying, "Look, we have a million issues to look at. Why don't we just start doing analysis continuously, and when you have questions, we'll try to address them." It had a high value because if you did continuous analysis, when our intuition got better, we would work on problems we thought were important to them. We discussed with them which were important, and we could respond quickly with analysis we had already done to issues that got raised later. Let me give you the prime example. We were doing studies on roles and missions for the Army.

Mike Garrambone: That's a volatile issue.

Seth Bonder: Oh, it was very volatile.

Mike Garrambone: I think that has been kicking around since 1947.

Seth Bonder: Yes, but after the demise of the Soviet Union and Warsaw Pact it raised its head again because we started to get into deep strike systems, who's really going to do deep strike, and who's in charge of what.

The point I'm making is the concept of continuous analysis gets you smarter and makes you more responsive to the decision makers, and I thought that was very useful. We did a lot of that.

Quick response analysis was done by four or five analysts—not 20, and may have included the computer programmer who made changes for us in the models. We created some new metrics and used some old ones. We used to call it the warfighting-casualty curve. On the x-axis was the FER, the Force Exchange Ratio, and on the y-axis were casualties. It was by theater. We would run hundreds and hundreds of simulation runs and plot these curves to try to get a handle on what the word decisive meant. We used the FER against casualties.

Dr. Bob Helmbold at CAA used another variable, and he said ours was wrong. Our variables were inverted, so his curves looked different from our curves. But once we got on the same wavelength, the curves looked very similar.

Winning was defined in a certain way in the historical data. We used what Bob did, and did our own analysis with it, and created what people used to call the horseshoe curve or the heel. It allowed us to take a look at how to define ''decisive'' to meet the National Military Strategy. I'll get to that more in a little bit. We created something called the window of risk. Because we were a deployment force now, we were interested in how fast you had to get there, and the window of risk. It was defined as the period of time from C-Day (the day a deployment operation begins) until you arrive. You had to get there within that window or you could not win; the threat would win. Take over the ports or whatever you needed. The US couldn't get there fast enough to do the job.

We also changed the concept of analysis. We used to make maybe 15 simulation runs during the Cold War, that is, 15 runs for every case; so we'd make thousands of runs because we had extensive sensitivity analysis. Well, suppose you didn't have this system but had a different system. Suppose this force was a little bigger than we assumed it was, there were so many uncertainties, we had to do sensitivity analysis around all the uncertainties.

Bob Sheldon: How did you account for false alarm rate, i.e., mobilize when you really shouldn't have?

Seth Bonder: We couldn't care less *(Laughs)*. That is, if we mobilize and are ready to go somewhere – to start early. We did some studies for General Galvin on a Rapid Reaction Force, where in fact, we said, you have to start before C-Day *(Laughs)*. You have to have started back then if you're going to be able to do anything useful tomorrow.

Mike Garrambone: Who made that call?

Seth Bonder: The military did. A lot of our briefings said the following: ''If you believe, A, B, C and D, then, do this.'' If a US decision maker doesn't think that's a good assumption, we have to go back to the drawing board. Because this depended on so many underlying uncertainties, we tried to look at and see the impact of assumptions and point out the big ones. In our Rapid Reaction Force Study in 1992, we recommended that for him to go to a certain place, he had to start way earlier than he thought and get his boats moving *(Laughs)* to where they needed to be. If they got there and nothing happened, then fine, turn around and come home. That's okay. Because the downside risk of delaying was the real disaster.

In Operation Desert Storm, if Saddam had had half-a-brain *(Laughs)* he'd have kept right on coming down over the ports. We couldn't stop him; we couldn't get there soon enough. He waited until we built up this gigantic force and then decided to come. I have no idea why, but our analysis suggested he could have done better; too bad for him he didn't have a good analysis team.

Let's continue with this new paradigm. We had all these extensive sensitivity analyses to do, and in a previous phase of this discussion we're having, I talked about all the VECTOR Series, campaign models that we used. But you couldn't make 3,000 sensitivity runs of VECTOR 3 or 2, so we learned to build analytical models that simulated the VECTOR models. We called these macro models. We'd run the VECTOR Series, do some fitting, run the macro some more, come back and test it out in VECTOR. So, as part of the new paradigm, we had models that simulated the models.

Bob Sheldon: Were those macros statistical regressions?

Seth Bonder: Yes, they were regressions. We got a lot of data, not only out of the VECTOR Series here, but out of SHAPE Technical Centre; we did studies for them. They did a bunch of Armor/Anti-armor studies when they brought our VECTOR 2 model on board in Europe. We used a lot of that data to create a macro, to do a lot of their studies, especially the sensitivity analyses. We also changed some of the administrative aspects. We had all of these results, and the question was, ''How do you present them?'' During the Cold War, briefings went as follows: ''Good afternoon. Here's the question we addressed. Let me tell you how we addressed it.'' We spent 45 minutes describing all of the models and all of the assumptions, everything and then, in the last 15 minutes, if there was any time left, we'd explain the results.

Now, we turned that whole paradigm backwards. When we talked to the senior guys, the decision makers, which were the four-stars or the Secretaries, we would go in and say, "Here's the issue, here's what we did—not how, and here's the results."

Now the question is, "How do you present those results when you have thousands of data points?" We figured out how to use color to represent content, not aesthetics, not to look pretty, but the color represented content. Originally, we used stop light charts, which related to some criteria, but we got even better than that. Learning how to use color, so when the decision maker looked at it, he could get a good feel for the results, even though he was looking at maybe a thousand campaign results. In a color graph—one graph, two graphs, and he could get a feel for where the power was and where his weaknesses were, for the decision he was to make. Now, how we did this. We would normally meet with his staff earlier, discuss the how, and go through that wicket with them before we got to the boss. Although for many of our senior decision maker studies, and I'm talking three-star, four-star, Secretariat, it was clear to me that we could not do a study for them unless they were involved; I mean really involved.

When, for example, General Galvin said, "I want you to do a study on the Rapid Reaction Force," I said, "You have to be part of it." He said, "What do I have to do?" I said, "Just meet at the beginning with me, so we can make sure we have the right question. Meet about halfway through, for an hour, and I'll show you what we're doing and if it makes sense to you; and then I'll give you a sense of what kind of results you might expect to see. If you think it's nonsense, we learn right there, and then on the back end, I want to spend time really debriefing you on what we did." Some of the clients actually got involved in the studies. It was so important that you didn't address the wrong question. We always made sure we had the right question because at the very first meeting, I'd say, "Let me pose to you the type of output results we may get for you. Will that satisfy you?" And he'd say, "I don't want that." I said, "Well, that's the question we thought we heard you ask" and he'd say, "Well, let me rephrase the question."

That was very useful and to the extent you can do that on studies – even if you're doing it for lower level staff, you then have an advocate for the study. Of course, they're part of the study. We, Vector, got involved and I ran quite a few of the studies, not all of them, but quite a few of them. If I didn't direct them, I knew what was going on in them because nothing left Vector for high level review that I was not involved in.

For force levels to meet the National Military Strategy, we started to really look at how much is enough. That was never touched years ago, because we never could get enough information *(Laughs)*. When we did the Army Roles and Missions studies, we did stationing strategies—where should we place forces, if we placed any; we did the tradeoff study between mobilization, deployment and employment. In those days, we worried about Southwest Asia and Korea. We created the concept for zero-based force design. Everybody kept saying, you can't have the same forces you had during the Cold War. So we said, "Okay, we'll design a force from the bottom up." We created a methodology for doing that, starting with a totally clean slate. We knew we would have to have a command and control function. We knew we would have to have an intelligence function, and some sort of fundamentals. After that, we built it all up; all the warfighting parts, the logistic parts, intelligence parts – built up, from the zero-base. I think that study may have been done for Mike Stone, the Secretary of the Army.

We did a really interesting study on force structure versus modernization for Carl Vuono. The issue he had for the overall Army was to come down from 680,000; the question was what level to come down to? And how much money do I put into modernization? On a previous study, I'd convinced him that modernization is an important player. The catchphrase I created was, "The smaller the force, the more modern it has to be." And that's a true statement. It has more of an impact. So we did the study in which we started at 680,000 and reduced the end strength at each introduction changing the whole force design, reducing the end strength. As we did that, we saved money and put more money into modernization, and we created one curve that had for the x-axis end strength versus the modernization, how much modernization and how much end strength. We had different budget levels. We showed effectiveness-investment curves, as a function of all those data points. Each data point created a force, which we then fought a war with, and did this again and again for all different combinations and created effectiveness-investment curves. But, we overlaid budgets on top of that. If you look at the curves, you'd see what's called an efficient point. It's where the highest effectiveness just touches the budget level that you can get. Clearly, the more budget, the better you can do; you can get more out there. We learned a lot of things such as if you move off that efficient point, you need a lot of modernization to account for the small falloff in end strength. And, we learned a lot about the dynamics of trading modernization for end strength.

That one curve was the big study result; it took us four months to get it. I called General Vuono and I said, "I'll be coming into town. Can you spare time to look at this?" He said, "How much do you want?" I said, "At least an hour." He said, "Come on into town and then come fly with me." So I flew with him, out to the west coast. We had lots of time, and we sat down and went through the curves. It was very complicated to look at, until you got an explanation. Anyway, we recommended 508,000 at the time and that's what they eventually decided, but then they dropped to 480,000. Wonderful studies are the things that I love to do because we were really helping people, we were applied, and learning a lot about tradeoffs in planning for forces, and then tradeoffs in operations. We did our analysis in both Southwest Asia and Korea; we didn't do much in Europe anymore, and all of our analysis tended to be joint and coalition campaigns. So we had the Air Force, the Navy offshore with deep strike, and coalition partners.

We did studies for a lot of senior leadership. I already mentioned some: the study of conventional forces in Europe was for General John Foss, who was head of TRADOC; we did studies for JCS, the CINCs and the Chiefs of Staff and the Secretaries. We did force structure versus modernization for General Vuono. We did the Air Force CAS (Close Air Support) to the Army for General Sullivan; that was a four-day study—it was short and easy. We did a study for General RisCassi, who was in charge in Korea; I'm guessing the time period was September 1992 or 1993. General RisCassi told me, "I want you to do a study so that I can present a paper, along with General Colin Powell, Chairman, JCS, telling the ROK (Republic of Korea) Army how they should modernize for the next ten years."

I said, "Okay, this is September, when do you want it?" He said, "Well, the ministerials are in November." *(Laughs)* I said, "It's September." He said, "I know; we need a six-week study." I said, "You know, you're over there and we're over here." Well we had all these classified lines for phone talk; I made two trips over there in between, meeting with him. He came back on one trip and we went over and presented the study to him and gave him all the charts he then took to the ministerials.

Bob Sheldon: What assumptions did you make on US force presence?

Seth Bonder: We then had 37,000 – whatever forces we had at the time, but now we brought more in. Not on D-Day, not in the beginning. We showed how ROK modernization could reduce their dependence on the US; that was the intent. The US would basically pay for a lot of the modernization and give them our systems. It was interesting because there is also an analysis shop in Korea. They redid the study and came up with the same answers.

Bob Sheldon: Korean Institute for Defense Analyses (KIDA)?

Seth Bonder: Yes, something like that. I guess RisCassi said they should do a parallel study, and they came up with very similar results – not exactly the same, obviously. We did a study for General Galvin, which I will expound on a little bit because it was an important study; it led to another sort of criterion. I think it was 1992, maybe 1993, when he said, "Look, the Soviet Union's gone; we have a new NATO now. I want to have a force that I can use for crises response." He called it the Rapid Reaction Force; it eventually became the Rapid Reaction Corps. He said, "I want a force that is politically feasible."

Each of the nations had to play. The US had to commit some combat forces, because if we didn't have combat forces, we wouldn't suffer casualties, and the other nations wanted us to suffer with them. There were a lot of political constraints like this on this force. So General Galvin said, ''Politically feasible and capable of crisis response and capable of holding in some areas until the bigger force can get there.'' The first part was a political analysis, in which we were trying to find out what the constraints were that would limit what the force could look like. He put one other constraint on us, which was critical. He said (I may be exaggerating) ''I don't want this 'fully adapt' stuff anymore; we're done with that.'' He said, ''I want you to think about wars anywhere that NATO will have to go.'' I said, ''It's a lot of places, right?'' We created the concept of a parametric threat, parametric scenarios. In fact, what we created was 27 different scenarios that were used. We did the analysis and built the force from the ground up; we did a zero-base build. We started with zero and built up the force, to get a Rapid Reaction Force that was limited by the constraints. Eventually we would add a package, run it through the political constraints—and it was okayed. We would run it through a warfight to see if it enhanced warfighting. There were lots of alternative packages, so we ran different kinds of alternative packages through and added the one that was a heuristic that gave the highest return on the margins. This was not conventional; there are better algorithms, but we were using a heuristic.

Once we got a force that was good for this scenario, we switched in a new scenario, and we'd build again. We built 27 different forces, and then looked for the commonality across them, to see if we could arrive at a force that was politically feasible, and did okay in the 27 scenarios. The force we eventually arrived at was green (OK) in about 19 of them, yellow (iffy) in about 2, and it was red (meaning it was bad) in a few others. We figured out ways to supplement that quickly with tactical air wings. We had some air force assets standing by for that part of the force, but we added separate tactical air wings that could come from Italy or Germany or elsewhere.

Jim McCarthy, an Air Force four-star, was Galvin's deputy at the time. Every time I'd do a study for General Galvin, McCarthy would say to me, ''You don't have it right. You don't have enough air power.'' And I'd say, ''Well, that's the air power we have; that's the data,'' and he said, ''Well, go to Air Force Studies and Analysis.'' I'd go to Studies and Analysis and they said, ''We don't have data on that.'' *(Laughs)*. ''Where does McCarthy think we have that data?'' Not only sorties, but attrition on each sortie, because we were representing flights of aircraft in the models.

By just trying to come up with a way to make the Rapid Reaction Force cover all of the 27 scenarios, this highlighted the whole concept for a need of battlefield versatility. I suggested adding two or maybe four tactical air wings when I briefed General Galvin. I had Mike Farrell with me (Mike, mentioned earlier, was a VRI employee and a retired Army lieutenant colonel). He gave me two hours when we were at Ramstein Air Base in Germany, and I hadn't finished. General Galvin said, ''I've got an activity. Let me make a call.'' He called his aide and said, ''Bring my Class A uniform over and I'll change here.'' And we stayed for three hours and that's when McCarthy said, ''You got it right now.'' Meaning, I had enough air power in it.

That was the first real application of the versatility planning concept that I actually created in 1975. Nobody paid any attention to me then in 1975. But I brought it up again in John Galvin's study, where it worked, and we were able to ''not try to get'' the most cost effective alternative, but one that was the most versatile. We wanted one that could cover all of these potential situations, and, as expected, you don't have any probability distribution on that. I developed some theories back in 1975 about the whole notion of a state space without the probability distribution; and how do you plan, given that circumstance. Here you can use various criteria like min-max and stuff of that kind. Anyway, that was the first application for General Galvin in 1992.

The next really big study we did was for General Maddox, who was then CINCUSAREUR. His job was to reduce the total Army personnel in Europe, including civilians, from 300,000 down to 65,000. I was going to a conference in Portugal with my wife. General Maddox called me and asked, ''Can you stop on the way over?''

We stopped and had dinner, and we did some business and chatted about this study we decided to do for him. The study was to help him determine how to draw the force down, so he could perform the missions that he was sure he would have in this new environment we lived in. He had all the special missions and he had warfighting missions. He had noncombatant evacuation missions; he had security missions; he had all sorts of different missions to perform. He said, ''I want you to bring the force down to 65,000 and tell me how many of these missions that I can perform.'' I said, ''Okay. We'll go home.''

I went home and what we built—myself and two other analysts—was eventually called the bean counter. We looked at all those different missions and built a system that we could use interactively. That is, we could say to the system, I want to do one of these types of missions out of the potential four, or I want to do two of these.

Bob Sheldon: Simultaneously?

Seth Bonder: Simultaneously. And the system would print graphics that showed the effectiveness and the risk for each of the missions. We completed the study in Orlando, Florida. We went down for an AUSA (Association of the United States Army) meeting and I asked, ''David, are you coming over?'' He said, ''Yes'' because I was going to go there; he would come, too. I said, ''Let's put aside, at least the morning, probably most of the day. We're going to build you a force interactively.'' It covered all of NATO where he had to play.

I had a guy on the computer, myself and Maddox, and I said, ''Okay, start off with what you think you'd like to do,'' and he said, ''Well, I want to do two of these. I want to be able to do one of these,'' and we had a list. I said, ''That's about 182,000 troops you need, and here are the curves.'' He said, ''Well, I can give up risk over here; I could accept more risk over here.'' We ran it, accepting more risk, and you get the troops down lower. We interactively did what I call a multi-objective decision problem, interactively with the decision maker. We looked at all these different objectives: the cost, risk and effectiveness, and troop levels, which were the cost function. We got it down to 67,000 troops. We not only gave him that answer, but we delivered the bean counter system to them, over there. So they used it, and continued to use it, to learn more about it. It was a wonderful study and one I enjoyed doing. Working with clients who have analytic skills is wonderful. They really become part of the team.

In my theoretical research, I learned about the importance of attack speed and how speed interacts with mass; that was all theory. But, in doing studies, I have over the years learned about the dynamics of warfare, the dynamics of forces, how they interact with each other. Let me give you a couple of examples.

When General DePuy was looking for new operational concepts to take on tactical fights in Europe, we looked at battalion firefights and learned something about their dynamics. I noted that when an attacking brigade of several battalions takes on a single battalion, we have up to a six-to-one force ratio. If we look at the ratio of attacker losses to defender losses, the LER, we could tell if it was going to be a good campaign or a not-so-good campaign. By plotting the instantaneous LER over the course of the battle, the change in the LER shows that at the very beginning of a battle, the defender has a big advantage. That is because the defender sees the attacker, because the attacker is moving and the defender is hiding in defilade—you can hardly find him. The defender is getting a shot or two or three shots off before the attacker gets a shot off. The attacker is looking to find the defender but it's harder for the attacker to bring to bear his mass of fires early on. The longer you stay engaged, you start to come down that curve. The longer the defender stays in defilade the more rapidly the curve falls off, so the advantage shifts to the attacker. Because the attacker is firing much more at you, he's getting the effect of mass and speed. The attacker is starting to saturate the defender's retaliatory capability. If you look at that curve, you say to yourself, if I were a smart general, I'd want to fight with the ratio high; that is, I'd want to initially fight very short little battles.

In our discussions with Bill DePuy, he called this the active defense. General Meyer, the 3rd ID commander, then looked at places to conduct these early defenses by building a set of company-size positions, in depth. When somebody attacks you, just take two or three shots, then get out of there by falling back. Now you cannot do that ad infinitum but you are really trading ground for attrition. After a while you have to stiffen the fighting up a bit. General Meyer and I walked the terrain and he showed me where we could build those positions. So the active defense became a concept that TRADOC would embrace, that later on went to other approaches like AirLand Battle. The point I'm making is analysts can do more than help to solve the decision problem. You can actually help with learning battlefield dynamics. If you ask the right questions, you will understand why things are occurring, rather than just numbers. That was at the tactical level. When I got to the operational level and the strategic level, we learned more things that were of interest.

There are things I learned at the operational and strategic level which I published in a long article in the *AUSA Journal.* I think it was Gordon Sullivan or Ted Stroup who asked me to write the article. What I'd been learning from 1989 to 1993 were important things about the new security environment. I had mentioned earlier that if we were still going to be in Europe, with the big reduction in forces, at parity, everything became important. What I found was the conventional warfighting would become very sensitive to many different things: modernization, tactics, and lots of different factors that could affect warfighting. Modernization provides major leverage; the smaller you are, the more modern you should be. The example I always use is one tank on one tank. The guy who has the better tank is going to win, particularly if he gets the first round off. We learned that in order to minimize casualties, you really needed to have a decisive overmatch. Now the question was, ''What does decisive mean?'' When the National Military Strategy came out at the same time we were doing these studies, everybody was saying, ''It's decisive because it's in the strategy.'' By doing a bunch of analyses, we created what we called rainbow charts for particular regions of the world.

Bob Sheldon: Describe the rainbow chart.

Seth Bonder: Part of the dynamics we learned, when the National Military Strategy described decisive overmatch was to win swiftly and minimize casualties. We had already been working on how to quantify that notion with Bob Helmbold, and were able to create curves, that we called the casualty warfighting relationships. The force exchange ratio (FER) was on the x-axis and the casualties for particular regions of the world were on the y-axis. The curve shows how many casualties you get, as a function of the FER.

What we did in doing a lot of studies in Southwest Asia, was to define ''decisive overmatch'' as having an FER of at least five, or greater. That not only provided good warfighting capability, it minimized casualties, it increased significantly the win probability, and it provided you with a degree of robustness.

Given the shape of the curves, if you decided to mount a force that could only register an FER 3, even though the casualties were low, you were a lot less robust. Because if you made a mistake on what the threat was, say by 20 percent, it would jump you way up that curve, which is exponential. We like the definition, FER 5 or greater; it minimizes our casualties, it maximizes the probability of a win—within some constraints—and gives you robustness, in case you make a mistake.

The same curves can be used to describe the probability of a win along the FER axis. To look at alternative solutions to problems we would display other information such as Research and Development dollars on the curve to show what you get if you spend 12 billion dollars versus 6 billion dollars. We did a study for Jay Garner, when he was the Director, Force Development, to show the impact of different amounts of money devoted to Research, Development and Acquisition. We were able to plot the differences on the curves and put the alternatives right on the curves and show him how robust it was. What you bought for more money was not only better capability, but robustness of the force.

Another piece of dynamics we learned was in looking at force projection—some of this work was done with General Max Thurman. We found that early arriving forces in theater, nonlinearly reduces the risk in warfighting, and gives you a better warfighting capability. It reduces overall casualties, increases the probability you will win and gives you some robustness. You need to get there with the right things earlier—including your intelligence and air defense.

As General Thurman said, ''You have to be able to see, you have to be able to protect yourself, provide air defense, and communicate with each other.'' You must get these initial units in, and come in right behind them with the maneuver units – if you didn't get there fast enough, then you suffer higher casualties. That's what led us to create the window of risk we spoke about earlier. The window was the limited period of time that you were going to lose, with the force that you were going to bring over, given the mobilization, the deployment capability, and the warfighting capability. If you could increase the deployment capability and make it better, so you got there faster or you got there with better stuff, then you could reduce the window of risk.

Bob Sheldon: Is this assuming a major regional conflict (MRC), where you're in the halt phase?

Seth Bonder: Yes, it was actually on the defensive part, but we would fight the defense and then fight the counterattack. We'd fight both of them. That was the window, and again, we learned something about dynamics.

Bob Sheldon: Somewhat in the operational art?

Seth Bonder: Yes, the operational physics is what you're learning about war. It was nice because I could talk to the guys who really understood it, it was really the military. I'd say something and they'd know it because they're military people, and very smart. During that same period of time, Mike Garrambone suggested I give a paper at the MORS Symposium in Colorado Springs in 2000, and I presented a paper on Lessons Learned from Modeling and Analysis.

I included in those lessons at the Symposium several I had written in an article for the *Operations Research* journal, the 50th Anniversary Issue where I wrote about the history of Army OR. Later, I included some of those lessons learned in a book on modeling and analyses, which I think were very useful to a lot of people.

In 2000, I also got elected to the National Academy of Engineering. A lot of my friends who were in the Academy said I should have been there 15 years earlier, but they're slow in what they do. It was quite an honor. I was very pleased to be a member of the Academy and they make use of you.

Bob Sheldon: The MORS Symposium you're referring to is when you presented in the same session with Wayne Hughes?

Seth Bonder: Yes. ''Army and OR analysis—Is the marriage over?'' Our review showed the history of OR and I raised questions about where it was going from 1995 out.

Let me go back to the 1987–1988 period and discuss the VRI component. I've talked about some studies, but VRI changed a lot in that time period too. Prior to 1988, company growth was not a VRI objective for a lot of reasons. The main one is I wanted to continue doing my own hands-on analysis. If we were going to grow, the way you really grow—big time—is by acquisitions. That meant I'd be out searching for companies and working to acquire companies, which I didn't want to do.

In the early 1980s, Bob Farrell – Bob was my Treasurer and Executive VP and everything else—asked me if I wanted to grow by acquisition, and I said, ''No.'' So we started giving money out to people around me and also saving money to buy companies in the future. That was prior to 1987. In 1988–1989, Vector was about 80 people. I recognized then that we had to grow, because I was losing people who wanted career paths. If you're not growing very much, you don't have career paths. So we made a decision to grow, but to grow internally. Military analysis grew from 1989 to 1995. We also saw significant growth in the Health Analysis Division, and we added an IT Division eventually.

Our clients wanted to use the study results we had, so we created decision support systems for them. Then they needed databases to use the data, the decisions part. So we learned how to create databases—we hired IT guys to do that. Finally they said, ''Why don't you just build systems for it?'' So we got into the systems building business, which we did for DoD, the Military Health System. We got some new clients. We started working for various Headquarters, Department of Treasury, the IRS, and for Commerce, building systems.

One of the big studies we did for Treasury was the Y2K (the Year 2000) Case Study. We built them a Command Center so they could observe the progress of the 600 computer systems, getting ready for Y2K. We had this big Command Center, with graphics, so we could keep track of the developments.

The Secretary of the Treasury, Larry Summers, would occasionally come in and look at the Center. We would show them which systems were behind, and needed more money. It turns out, on New Years Eve of 2000, he came there. I wasn't there but the guys at the Command Center said he brought them cookies. *(Laughter)*. It was very nice. We did some other studies with Treasury, which were IT-related for the IRS and other people. So that helped us grow some. By roughly 1995, we numbered 350. By 1997 or 1998, we were over 400. I think our peak was about in 1999, when we numbered about 430 people. We were grossing roughly $55 million a year. It was a nice size company.

A lot of generals retired and came to work for us. When General Thurman got out, he got leukemia, and was very sick. He was at Johns Hopkins Hospital, survived that, and came here after they let him out of Walter Reed Army Medical Center. He came to work for me three or four days a month. He was a wonderful guy, with great ideas, but would never put anything on paper, *(Laughs)* never. He said, ''You gotta get in the health business.'' I said, ''Max, Vector is in the health business. We've been in the health business since 1972, when I wrote that first proposal, when we worked for National Institute of Health (NIH).'' He said, ''No, no, no – *you* have to be in the health business,'' meaning me, personally. I said, ''I have enough to do with running the company and trying to get 30 percent direct charge.'' That was because I always wanted to work on projects. Even when we were that big, I was still doing 30 percent direct charge out—if I could—on contracts. It's additional to the marketing I was doing. I said, "Okay." So he introduced me to a gentleman named Russ Zajtchuk, who was a Major General and head of the Army's Medical Research and Development Command at Fort Detrick, Maryland. He had worked for Max and in fact, may have extended Max's life; they were very good friends.

So, as my first foray into the health business, General Zajtchuk invited me to a Military Health System Telemedicine Conference, a working session, a small group of maybe 50 people worked with us. I went there and I asked Russ if I could take Peter Cherry and George Miller with me. We observed what they were doing in the health business. The guys I brought with me were military analysts, not health analysts. The health people tended to be much more economics-oriented, running large information systems for the military health organizations; something called DMIS, Defense Medical Information System.

They were talking about what services you were getting. So I took my two military guys and the moderator was Charlie Flagle, who had offered me the scholarship at Johns Hopkins, way back in 1960. *(Laughs)*. I'd seen him occasionally through my ORSA associations, but hadn't seen him in quite a few years, and he was moderating this. It ran for two days, and I just went around and listened to what people were talking about. At the end they had a debrief, and some of the key panel heads got up and talked about what they were doing or what they learned.

It was then that Charlie made a mistake. He asked me what I thought about the conference. And I said, ''Well I think that the conference was good." I said, "I'm just shocked at the state of the art of analysis, if you call it that, in the health area. It's archaic. It's in the Dark Ages." I went on to really tear into the Telemedicine part of the Military Health System, and Charlie said, ''You know, he's right, we really are. We have not pushed and moved to the state of the art of what could be done.'' Peter Cherry and George Miller both agreed with me. So we did the Telemedicine thing and then moved over, more into the broader aspect of the healthcare delivery system within the Military Health System.

As an example of telemedicine, you are a patient and you're out in Seattle, but you're not near where your dermatologist is. You get on a TV screen and the dermatologist looks at the thing on your hand and says, ''Oh, I know what that is'' and he prescribes something for you, and he never sees you. It's medicine via television. It's more sophisticated than that now. A lot of ophthalmology can be done this way now. You don't have to be anywhere near the ophthalmologist. But he can look right into your eye, from afar. My early work was in the telemedicine arena in trying to understand how it could have some value. That got me personally into the whole issue of the healthcare delivery system, with two aspects. One is the treatment of acute disease, and how we treat people when you find something wrong with them. And, second, the prevention and management of disease. It became clear to me that everything they did to analyze what to do for the future was retrospective.

They looked at what they had been doing and to make a change, it was trial and error. They'd say, ''We're going to try this new piece of equipment.'' They'd put it in, they'd watch it for three years and then throw it away. The whole business was a trial and error. Costs were escalating, it was inefficient. I said, ''There has to be a better way to do this.'' I said, ''We need to bring analysis into this process—an analysis process—not retrospective statistical analysis, because that's nice for evaluating how well you're doing. And maybe we need to introduce inference a little beyond what you're doing.'' So we use these models much more for prediction and planning, not evaluation. Instead of evaluation and inference, we're talking about planning and prediction.

All of the models that they used were statistical models. All the economists regress on some parameters, and they have Cobb-Douglas functions and various things, but it's all statistical. It dawned on me, and I think others will agree, that's nice for evaluation and inference; but the model depends on the data that you feed the model. That data describes a current system. It has nothing to do with the future, and in fact, it's irrelevant when you talk about the future. That is, your model has intrinsic value, the current system. You can't get to the future system.

And I said, ''But you're using all the data from the current system. You can't do 10-year forecasts. We're going to build the analog of the VECTOR Series models and help.'' My guys thought I was nuts, because I didn't know anything about health. But, I had read and I had gone to conferences. Some of the words would run way over my head but I said, ''We're going to do that.''

I got Russ Zajtchuk to fund us and we built what was called the Healthcare Complex Model, because it models a large complex that delivers healthcare services. It is a model that simulates a large enterprise. Let me give you an example of size. The model can be used to analyze the delivery of healthcare in an enterprise that has one or two major medical centers, like Bethesda and Walter Reed, operating, with patients coming in and out. Five or 10 hospitals and maybe 10 or 15 clinics, that's an enterprise, that's complex. The model simulates, just like a simulation, but has a lot of analytic stuff in it. The model simulates individual patients coming in and following them through a complete episode of care.

Now an episode of care means, you walk in, then you get diagnosed and you have a chest pain and you need to have tests; you get an EKG, you get a thallium stress test, take blood; you assess that, you come up with a diagnosis, you get treated. And when you come out the other end—you're not just there to visit—you come out the other end and you are a recovering patient. Or, maybe, you go to surgery. Then you are a recovering surgical patient, and the simulation keeps track of you. The model simulates all of that. I'll give you an example of this. We tested it at Madigan, which is an Army Medical Center in Seattle, with some small number of hospitals and clinics. We ran that model for a year and then compared it to the results that they had the year that we built the model for.

The model simulates more than a million outpatient services. Inside it are the brains of the models called protocols. If you say somebody comes in with this pain and here's what you do with that person: get these tests, do this, check his ear, have him see an ophthalmologist; they're called protocols, they're called pathways, sometimes guidelines, that's what physicians do. It has something like 1,500 guidelines for different diseases and actions you could take. It keeps track of 60 different physician types. It's fairly sophisticated—it's like fighting a war in Europe, in the VECTOR Series. We built that and we started using it in the military health system.

I believe they've been using it for some counter-terrorism stuff in looking at the impact of a terrorist attack that would result in mass casualties and how the health system would respond. They need to see what kind of capability the hospital system has to take care of patients. They keep adding to the model. So that was one of the models we built, which was really directed by me. Not that I know a lot about the health care industry personally, but we got all the health people to build it, and I was there, making sure we were physics-based, to the extent we could be.

The other piece we did, that I got involved with, was diabetes. One of my guys built a model of the aging of diabetics. You're a diabetic and we're going to age you 20 years—we'll see what happens to your disease, given different treatment protocols. So we simulated out 20 years. What was interesting is that after five years of the simulation, we could see that the number of good protocols, as opposed to the bad ones, reduced the number of emergency visits by almost 30 percent. The number of really bad physical problems—like going blind, and having to cut limbs off—dropped by about 25 percent. The Army liked this study so much, they then funded clinical trials for five years at Brooks Air Force Base in San Antonio.

I haven't tracked it since then, but after three years of the trials, they looked pretty much like the simulation. This doesn't say that we showed how to fix diabetes, because the real issue is to get people to follow the protocols. But we can do analysis on the protocols.

I think I stimulated getting more OR into our Health Division, and they still have lots of work to do. A lot of the military guys have moved over to the health side. George Miller is in the health business; Mike Potter, and a whole bunch of others, have moved over there and they are very powerful people. That was the health stuff in Vector as the company grew. I did get Vector into the Veterans Administration (VA) for studies and we started to expand out in the military health system but most importantly, I think we got the military health system to start thinking about their system and that I think was very useful. Now that ends the period up to 2000, so let's talk about 2000 to the present. Which is fortunate—my career is still going *(Laughs)*. As I said earlier, I call this my age of freedom. It's freedom because I don't have to manage anybody anymore.

Bob Sheldon: Before, you said you had a major lifestyle change?

Seth Bonder: I didn't quite finish there. A couple of things happened. In 1999, I did decide that we would change how we would grow because we started to level off again after 10 years. I now decided we're just going to have to do acquisitions and we were a $50 million dollar company and I thought that the only way to take it to a $500 million dollar company was to start acquiring companies. We had some money saved—we had not issued many dividends. So that's what I decided in 1999; and wouldn't you know it, in October of 1999, I came down with kidney cancer.

A friend of mine is an oncologist and one day he said, ''You know, you never had Computerized Axial Tomography (CAT) scans, and you haven't had a colonoscopy.'' I was then 68 years old and didn't feel bad. I said, ''Okay.'' So I submitted to all those.

He had my secretary, Lisa Gaines, get hold of me and asked me to come in to see him. I did and he said, ''You have a lesion in your kidney.'' For some reason lesion and tumor didn't connect. I said, ''A lesion?'' He said, ''A tumor.'' I said, ''Is it cancer?'' He said, ''Yes.'' I said, ''You sure?'' He said, ''Absolutely.'' And I said, "What are we going to do?" *(Laughs)* ''We're going to take it out.'' I said, ''The cancer?'' He said, "No, the kidney.'' ''When are we going to do that?'' He said, ''Next Wednesday. The surgeon's coming over now, the urologist, a guy named Hugh Sullivan.'' He came in to see me and he said, ''We're going to take it out next Wednesday because it's encapsulated, meaning it's inside the kidney; that's good.'' I said, ''Is it possible to postpone it for a week?'' He said, ''Why?'' I said, ''Well, I'm lecturing at NPS next Monday and Tuesday and then I want to go to the wine country and party for a little bit.'' *(Laughs)* He said, ''Of course, you're joking?'' And I said, ''No, I'm serious.'' The surgeon looked at the x-ray and he said, ''We can see how fast it's been growing, you have a week, but you have to be back the following Monday.'' I said, ''Okay.''

So I went to Monterey and I came back, and they took the kidney out. I was out of work for about four weeks, recuperating. It was a significant surgery. Today it'd be a lot better; they were doing laparoscopically. Vector used to have three or four physicians in the company besides nurses, and Jack Taylor said, ''You get the doughboy operation. They cut you and then they twist you open and they stick *(Laughter)* your head in there.'' ''That's great Jack. Thank you.'' Anyway, they did that and I went back to work. A little hard walking, I got on the treadmill early and started doing stuff.

It became clear to me that there was no way I would have the energy to take the company to the $500 million level. So I told folks in the company, "I think it's time that I have to sell it." I've got 31 years into it and I offered management first option. I said, "If you can raise enough money to buy it at what I think is a reasonable price, you guys can buy it." A lot of them already had stock. Over the course of the years, I ended up with 51 percent of this company. I wouldn't go lower than that. My good friend John Kettelle had taught me a lesson years ago. He once had a company, Ketron. He brought in people, gave them stock, gave them too much and they fired him. So John taught me a lesson: it's a benevolent democracy. Anyway, I was committed to trying to sell it, and in 2000 I started looking and offered management the option. They could raise some money—not that I was outlandish—but I knew they would be lower than other corporations would bid for it. But they were way low *(Laughs)*. It just seemed not appropriate. So I had a change of policy that we'd go outside, rather than me just try to grow it again. I looked around, and had one of the local companies here in Washington look for potential buyers. We had some come in on their own, when the word got around. They started with 17 buyers—eventually they narrowed it to seven. We had a few others, from the ecological players here in town and ended up with, surprisingly, one in Ann Arbor. It was the company that used to be called the Environmental Research Institute of Michigan (ERIM). It was the old Willow Run Laboratories from the 1950s that they spun off for a dollar from the state and then they had this multispectral sensing company. They created two companies, a for-profit ERIM and a not-for-profit ERIM. They sold the for-profit a number of years before we got together to Veridian. Then they had all this cash *(Laughs)* and there I was and they needed projects of a research nature. They made an offer, we negotiated. I got the offer I wanted and sold it in May 2001, so it's now been four and half years. That's why I said, in 2000 and maybe it was more like 2001 *(Laughs)*, it became my age of freedom. The company changed its name; it's now called the Altarum Institute. It's a nonprofit. Some people I know are on the Board: Jacques Gansler is on their Board, a good friend. He's the Chairman of the Board now. It was a good sale. There wasn't any stock involved because they're a nonprofit. We knew we were going to consummate this earlier than May, probably six months early. We were working towards getting all the paperwork and all the arrangements and the incentives and bonuses, just to keep the good people on board. I became unemployed *(Laughter)*. Not true, and they actually wanted me to work for 18 months; the new CEO, who came from General Motors, wanted me to stay on and advise him. When anybody asks me to advise them, I always tell them, "Don't ask for what you may not like." *(Laughter)* And sure enough, his style of management was different from mine, and after six months, we agreed to part ways. It was fine.

They owned the company, they should run it the way they want to run it, and I became free. I sold VRI in 2001. I've continued my extensive work with the Army Science Board. When I got off in 1992, Peter Cherry was on the Board and when he got off in 1998, either Paul Kern or Walt Hollis asked me if I'd come back, so I went back on the Army Science Board in 1998. I've done a bunch of studies with them over the last six years. I just got off the Board again; it's a six-year tour or two three-year tours, or something like that.

We did some interesting stuff with the Army Science Board, and I grew up with them, starting in 1972, so I really have a great allegiance to the Board. I've since then become a member of the Board on Army, Science and Technology, the BAST, which is a National Academy Board; it's chaired by George Singley and John Miller now. Glenn Otis asked me to accept the offer to join the Board. Malcolm O'Neill is on the Board. These are retired generals, and a bunch of fairly senior technology people are on the Board. The Air Force has an analogous board, the Air Force Scientific Advisory Board and the Navy has an analogous board, as part of the Academy. So I became a member of the BAST.

I provide strategic advice to the University of Michigan Comprehensive Cancer Center. I'm on their advisory board to the Chairman; same with the Kellogg Eye Center. What I bring is not a lot of knowledge about cancer, but I'm interested in cancer, obviously. I bring OR thinking, which although those of us in the business don't think it's that special, it is quite different from the rest of the people in the world. I bring a different thought process to that. I sit on the Community College Board in town. I sit on two startup boards, and I sit on the OSU and University of Michigan Engineering External Advisory Boards.

The reason I call it my age of freedom, I'm free to pick and do what I want to do. I work on studies that I want to work on; I turn down those I don't want to. I go wherever I want to go. *(Laughs)* I'm free, which is very nice, and I don't have to manage, I don't have to market. The only one I'm responsible to is me and my family. I created a number of endowments when I made money selling Vector, not as much as some people think but enough to keep me happy the rest of my life. I decided to give a bunch away, so I established a scholarship at OSU, to mimic the same fellowship I went on in 1960. Of course, the original has disappeared.

As you probably gather, I do a lot of pro bono stuff and some of it is done for the National Academy. I do studies for them. I've sat on peer review committees for the National Academy to get people into the Academy. I'm just finishing up one study looking at operational testing at the DoD. For evolutionary acquisition, what did the testing look like? We're just finishing a study on that. I did another study, just finishing, on how to bring more engineering into healthcare delivery. Not just into better technology, but how to make the delivery system better. There's a report coming out on that soon. The National Academy has three pieces: the National Academy of Engineering, engineers go here; the National Academy of Sciences, biologists go here; and then the Institute of Medicine (IOM), where all the medical people go. They're all commissioned studies. I got a call from somebody high up in IOM, who asked me if I would join a study they're going to start on looking at how to restructure the mental healthcare delivery system in the country.

I said, ''I know nothing about mental health'' and just jokingly said, ''I'm not going to be a case study either.'' *(Laughter)* They said, ''No, we need a systems engineer.'' There are going to be some 13 psychiatrists, three or four psychologists, and one or two public health people, who have all been working in mental health and substance abuse for years. ''But we need somebody with a different thought process.'' Well, he was right *(Laughs)*. The study was 15 months long and the findings briefly got released to the press at the Jimmy Carter Center in Georgia. The report will be out in early December. So that's done, but it was a very difficult study for me in a sense that I kept bringing OR thought processes to this and they think differently.

If I say to you, look Bob, you're at A; and to get to B, you do this and then you get to B; and then you do this to get to C; and once you're at C, then you've got an integrated system. They don't think that way at all. Just as sort of an aside, the mental healthcare delivery system is almost separate from the rest of the healthcare delivery system in this country, for lots of reasons, both in public and private sector. I was pushing for more integration, and since the report's not out, I can't tell you some of the interesting findings and recommendations. But that was a study that I just finished. We have one more meeting to figure out what to do to get it implemented. I had recommended, as part of our report, we have an implementation strategy. That is, just telling somebody to do something doesn't get it done. They didn't do that. There were 13 reviewers of the study; the Academy has all those studies heavily reviewed before they go out. Four of them recommended – what's your strategy for implementation? We don't have it; so we have to create one, eventually. I'm just starting a new one, this one's coming out of the Air Force Studies Board, part of the Academy, and that's why I'm in town; the first meeting's tomorrow. I'm not sure I know much about it, other than it has to do with aircraft, stealth, and some things. It's going to be code word stuff, so that's all I know about the study and I'll find out more about it tomorrow. But that's some of the pro bono work that I enjoy. But I picked those that I want to work on. I probably got asked to do three times as much and turned most of them down, because they just don't look interesting.

Interesting things are those that I can contribute to and when it's done, it's going to be useful to somebody. I have been, over this five-year period, presenting some keynote talks in forums and other places and presented some memorial lectureships at various schools throughout the country. And I've been doing some consulting in the defense industry and some in the health industry.

Let me wrap it up with an overall comment. I could talk about OR and military OR, with lessons learned and what students ought to do and all of that. I don't want to do that. What I really want to do is to have people recognize you don't have a career without a lot of other people. Careers aren't solos—maybe if you're a singer—but not in OR. Even though I did the initial theoretical work on the differential models, the growth on those depended on a lot of people. Many academicians mentored me. I had mathematicians to bang ideas off of when I started getting into equations that I couldn't solve, and neither could they. You get help along the way, all the time, as an academician, and you make friends and you get a lot of mentors who help you in various ways from how to teach ethics in analysis and a bunch of other stuff. I can't overstate the importance of all of the support that came from guys like Bob Farrell, Peter Cherry, Dave Thompson, George Miller, Allen Weintraub, and many others, on the growth of the whole differential models of combat. These are used, not only in the Battalion, but the VECTOR Series; as they go from VECTOR 0 all the way to VECTOR 3; there must be eight or nine versions, for different regions of the world. That doesn't happen with one person; it happens with everybody contributing. I directed and managed, but a lot of ideas came from others and we implemented as a team. It comes from having great clients. I have to tell you, all of my clients understood what I was doing and they were willing to contribute. I'm talking about senior people like DePuy, Thurman, Otis, Maddox, Foster, and Kent. Wilbur Payne was a major influence on my career, in his sardonic ways. I value all of our sessions where we taught each other so many things. All of that work contributes to having your career. My clients, who were not only clients, but helped me do some of the work, and even when they were working for me, they've been friends, my whole life. It's been a wonderful career, full of great intellectual things, full of great joys, full of lots of sadness when you do the wrong things, and full of a lot of learning, which is what I always have to do. It never stops. I hope this history is useful to some other people who get into this business. It's such a great business.

Bob Sheldon: You mentioned your relationship with other people like Saul Gass. Is your relationship with him via INFORMS?

Seth Bonder: It was professional through ORSA, before INFORMS. I think it was Jack Borsting and myself who started the visiting lectureship program, and Saul was part of that. I met Saul through ORSA. He and I presented papers at various conferences together. For one of them, an OR-themed applied mathematics conference organized by Saul for the American Mathematical Society (1979), I presented and wrote a paper, ''Mathematical Modeling of Military Conflict Situations,'' that appeared in the proceedings Saul edited.

Yes, I've known Saul for a long time, but it's all through ORSA. Comradeship. What is nice in my career is that I've had that comradeship through ORSA. A lot of them were academics, so in part, they related to my academic side. But I had a separate group of academicians who are part of my career and we were a separate group of comrades.

Obviously, growing Vector, with all of the people in Vector, who were like family, and we ran it like a family, are among my comrades. The clients of Vector were among my comrades. Then you go to the Army Science Board and it's a whole new group of people. And MORS is another group of people.

So I had ORSA, MORS, the Army Science Board, the National Academy of Engineering, Vector and the teaching part. They've all been pieces of an integrated career. I'm not sure how I did all of it, probably not well *(Laughs)*. When I look back on my life and wonder if I've contributed something, I think: ''Yes, I did.'' And, you look back on all the friendships which are everlasting, they just go on.