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Seymour J. Deitchman Interview (MORS)

Deitchman, Seymour J.

Military Operations Research Society (MORS) Oral History Project Interview of Mr. Seymour J. Deitchman

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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.

Mr. Seymour (Sy) Deitchman was the MORS Wanner Award laureate in 2000. He has served in private industry, in the Defense Advanced Research Projects Agency (DARPA) and Office of the Secretary of Defense (OSD), as vice president for programs at the Institute for Defense Analyses (IDA), and as a member of US Government and North Atlantic Treaty Organization (NATO) advisory panels. Since 1982 he has served as a member and then as a Consultant and Special Advisor to the Naval Studies Board of the National Research Council. During his career, he has conducted detailed technical analyses, managed research and development programs, been a corporate officer, and written six professional books as well as over fifty published papers and reports.

MORS ORAL HISTORY

INTERVIEW WITH Seymour J. Deitchman
September 12, 2008 and October 8, 2008
MORS Office, Alexandria, Virginia
DR. BOB SHELDON, FS, AND DR. YUNA WONG, INTERVIEWERS

BOB SHELDON: We're at the MORS office for an oral history interview with Sy Deitchman. First of all, when and where were you born?

SEYMOUR DEITCHMAN: January 15, 1923 in New York City.

BOB SHELDON: What were your parents' names and how did they influence you academically and professionally?

SEYMOUR DEITCHMAN: My father's name was Leo Deitchman. My mother was Pauline Deitchman, maiden name Levy. They came over as immigrants during the late 1890s or early 1900s. The way they influenced me was to instill not only a love of learning, but a must about learning. ''You're going to go to college. You're going to be a professional man. You're not going to make ladies' pocketbooks like I do.'' So that's where it all started.

BOB SHELDON: So that was your father's profession, making pocketbooks?

SEYMOUR DEITCHMAN: Yes. He first made shoes and then he made pocketbooks. He was a small businessman. He ran his own business.

BOB SHELDON: That was in New York City?

SEYMOUR DEITCHMAN: And cities near there, yes. Wherever he could find work.

BOB SHELDON: Where did you go to elementary school, junior high school, and high school?

SEYMOUR DEITCHMAN: Could I put in a caveat about memory?

BOB SHELDON: Okay.

SEYMOUR DEITCHMAN: I've checked against the records wherever I could so that I could remember what to tell you. But memory is an evanescent thing I'm finding, especially at my age, so be warned.

BOB SHELDON: Okay. We'll remember that.

SEYMOUR DEITCHMAN: I went to Stuyvesant High School in New York, which is an engineering preparatory school. Now I should note, I always wanted to be in aviation, from the time I was nine years old. I planned initially to spend two years at CCNY [the City College of New York] because that school was free at the time and engineering courses pretty much started out the same way at all colleges, with some humanities and basic physics, math and mechanics, before they specialized in the various branches of engineering. Then I had planned to take a year off and go to work on a ship in the engine room and travel a bit, and then come back and go to NYU [New York University] and study aeronautical engineering.

BOB SHELDON: Let me back up. At nine years old, you said you got interested in aviation. What happened?

SEYMOUR DEITCHMAN: That was shortly after Lindbergh flew the Atlantic and interested the country in aviation. That was the trigger. I declared I wanted to be a pilot, and my father said, ''No. That's too dangerous.'' So I said, ''Okay. I'll be an engineer, and build them.'' I had a much-admired uncle who was a civil engineer, and helped build the Triboro Bridge in New York. My father thought that still would be too dangerous because I might have to fly. But he learned to live with that. In fact, eventually I got him flying.

World War II intervened in my plan. So I started at CCNY, and then it was one step ahead of the draft board all the way, because

I was determined to get my degree. I managed to do what amounts to a five-year course in three years to get my Bachelor's degree in mechanical engineering. I've never worked so hard in my life. And I did have a couple a years of Reserve Officers Training Corps (ROTC), so I had an idea of what military training was like.

BOB SHELDON: Your mechanical engineering courses, did they include thermodynamics, heat transfer, and strength of materials?

SEYMOUR DEITCHMAN: Yes. Physics, hydrodynamics, and machinery design.

BOB SHELDON: Probably a lot of math, too.

SEYMOUR DEITCHMAN: Through calculus. I taught myself differential equations after I got out of school.

A recruiter came to school and he was looking for people to work at the Langley Laboratory of the National Advisory Committee for Aeronautics (NACA) at the time, which much later morphed into the National Aeronautics and Space Administration (NASA) as we know it today. That was like inviting me to paradise, with my love of aviation. As I was growing up I used to make trips down to Floyd Bennett Field where I would take pictures of the airplanes and watch them land and take off.

BOB SHELDON: Where is Floyd Bennett Field?

SEYMOUR DEITCHMAN: It's in the middle of Brooklyn now, but it was way out in the wild fields at the edge of New York at that time, and serviced both military and civil aviation.

The procedure when I got to NACA at Langley Field, near Hampton, Virginia, was that I was drafted into the Army and then assigned back there on inactive reserve. The plan was that when somebody with the requisite qualifications came back from service overseas, he would come and replace me, and I would go into the active Army and be sent overseas. Apparently no one with those qualifications appeared, because the issue never arose. I started at NACA by testing aircraft models for stability and control characteristics in the seven by ten foot wind tunnel. After 18 months or so, I got bored with wind tunnel testing, which I'll describe in more detail shortly. I tried to enlist in the Navy's V12 program, which meant that you got six months of training and you came out as an ensign or a lieutenant JG [junior grade], and got assigned to a ship. I ran into an interesting Catch 22, because the Navy was interested in taking me, but for me to get into that program, I had to resign from the Army Reserve. As soon as I would resign from the Army Reserve I would automatically go into the Army infantry. So I decided to stay put.

But I did transfer from wind tunnel testing, and went into the physics division at NACA Langley where I did some other work.

The seven by ten foot subsonic wind tunnel, where I started to work, was an archaic 1930s wind tunnel design, but it could test powered models and it mainly performed aircraft stability and control work. That meant measuring how the propeller slipstream and downwash from the wing-tip vorticity (too bad you can't see my gestures on the page) affected the stability and control of the airplane. If an airplane was stable, when it was disturbed it wanted to come back to its original orientation in the airstream; but if it was unstable, when it was disturbed it would go off track and into a spin or some other undesirable behavior.

At that tunnel, the idea was to measure the disturbing or upsetting forces, and later calculate the consequent aircraft trajectory, so as to correct the airplane design as needed. The model was mounted on scales and it had no automatic read-out. There were young women math majors, who were mostly from colleges in North Carolina and others nearby, who would sit at the scales and take readings whenever I gave the word, "Okay, now," because the airplane was in the right position and the wind speed was right. They were called "computers." And they would punch out the data on Frieden machines – if you remember the old calculators – and plot it up by hand. Then it was my job to interpret it in terms of the airplane design.

I learned a lot of aerodynamics, and I also learned to get along in an alien environment. I should note that here was a boy, born and bred in New York City, going down into what was then the Deep South, in a place peopled from the Deep South, and a totally different culture.

BOB SHELDON: This wind tunnel was in North Carolina?

SEYMOUR DEITCHMAN: No. It was at Langley Field, which is just outside of Hampton, Virginia. It's still there. And Langley Field is now

Langley Air Force Base, the home of the U.S. Air Force Air Combat Command Headquarters.

BOB SHELDON: Did you take a fluid dynamics course in college?

SEYMOUR DEITCHMAN: Yes.

BOB SHELDON: So you understood all the math and modeling.

SEYMOUR DEITCHMAN: Pretty much – I had to learn to extend it to the aerodynamics of the airplane, but that wasn't hard.

BOB SHELDON: Did you learn anything from your wind tunnel testing that you hadn't learned in college?

SEYMOUR DEITCHMAN: Not a lot professionally, although I hadn't studied any aerodynamics in college. I learned the basics of aircraft lift, drag, pitching moments, and other basic airplane aerodynamics. But I did enjoy the work a lot, and learning the aerodynamics, at the beginning. In line with learning to get along in an alien culture, let me interject an amusing incident.

One of the first things I learned was to tread gently in an unfamiliar milieu. The first job I got when I got to the seven by ten foot tunnel was to check the design of a gearbox that had to be built to power one of the models. The models used electric motors with the gearbox to the propeller. It had been designed by a young and very attractive woman who was very popular with the young males in the organization, a gal by the name of Annie Timberlake. I went through her design and tore it to pieces. That's when I learned you had to be very gentle with someone as popular as Annie Timberlake. It damn near got me thrown out of the place, but I redesigned the gear box to my satisfaction instead of bumping it back to her, and, yes, I learned that lesson well and early on: social networks affect how you do engineering.

In about 1945, we tested what became the F-84 Thunderjet, our first jet combat aircraft. The F-84 was used six years later in the Korean War. I was beginning to learn something about development time, which I hadn't appreciated before.

I got tired of wind tunnel testing, so I got myself transferred to the physics department where I played with supersonic flow and high-subsonic compressible flow, which were just coming into recognition at the time because airplanes in steep dives were finally beginning to go near supersonic speed. They never quite got there, but close. I devised a method to take Schlieren photographs in a shock tunnel. Schlieren photographs are photographs with polarized light that actually show the shockwaves attached to a body in a supersonic airstream, because the light gets refracted differently by air of different densities. We had a small tunnel with glass sides, so you could look through it, and I devised a method for taking photographs of the shockwaves so you could see how different forms disturbed the airstream in supersonic flow. That would come in handy later on.

So I worked on that, and the war ended.

An interesting thing came up there. The physics division had been headed by a man named Theodore Theodorsen, who was one of the pioneers in aerodynamics, a very famous person. Just before I got there, he had left to go to Brazil and set up the Aeronautical Institute of Brazil. The interesting thing is that now, 60 years later, we find Brazil coming up strong and building regional jet aircraft. So there's a dwell time, showing how long it takes to build an industry.

In 1947, I moved to the Cornell Aeronautical Laboratory (CAL) in Buffalo (which has since become the CALSPAN Corporation). That was a lab initially built around an 8×10-foot wind tunnel that had belonged to the Curtiss-Wright Corporation; the company was in Buffalo across the street from what became the Lab, during the war. After the war, Curtiss-Wright was going into the engine business and going out of the airplane business, so it gave the tunnel to Cornell University and they kept it in place and set up the Lab around it.

The Lab had the wind tunnel, and it had a full-scale flight test division and a theoretical aerodynamics division, materials and structures division, and a systems research division. It was quite large. I don't remember the staffing, but somewhere between 500 and 1,000 people.

BOB SHELDON: Were you working for Cornell University?

SEYMOUR DEITCHMAN: The Lab was owned and operated by the university, but it was run as a separate, independent entity. I wasn't conscious of those things at the time, but probably the Board of Trustees was on the University payroll.

I had two tours there, as I'll get to, but I thought first it'd be interesting to cover some of the significant work I did during that first tour. On one of them, we started wind tunnel testing an airplane that was designed by the Avro Corporation in Canada. Avro was an offshoot of Avro Aeronautics, Great Britain, which itself eventually got folded into British Aerospace.

They had one of the first jet aircraft right after the De Havilland Comet. I learned something from them about the sponsor's pressure to get the results he wants. The drag on that airplane turned out to be higher than they liked, so the airplane wouldn't go as far as they wanted or at the speed they wanted, and they kept trying to push us to lower the drag. They didn't believe the scales in the wind tunnel. (I should note that by now the model was mounted on a boom with electric strain gauges to measure the aerodynamic forces. We didn't have the young women working as computers any more.)

They kept pushing us, and finally they just gave up. And, in fact, they built that airplane, but it never went anyplace commercially.

Another thing we worked on, that I spent a lot of time on, was bomb separation from aircraft. It was under a US Air Force contract out of Wright Field [now part of Wright-Patterson Air Force Base]. The high-speed airflow under the wings, or in bomb bays, would upset the bombs; as they left the airplane, they would tip and the fins would damage the wings or the bomb bay internal structure. That was a real problem. They wanted to know, "How can we locate the bomb, or change the airflow around it, so that it wouldn't do that?" It was a fascinating study in the sense that it was three-pronged. We did wind tunnel testing with "live" bombs that I'll describe shortly; theoretical aerodynamics calculating the forces on the bomb, from which we could estimate the trajectory; and then full-scale flight tests measuring the forces, after which we actually did bomb drops into Lake Erie, measuring the trajectory as the bomb left the aircraft wing (we used an F-86 for the tests). I was the program manager for all of this. I had to put it together, so obviously I had progressed professionally.

The "live" bombs in the wind tunnel were interesting. I should mention that in doing this project, I met Dr. Al Flax, who was head of the Aerodynamics Division at the time. He became a lifelong friend. Al had made his name in aeronautics in the field of aeroelasticity, which is how structures interact with the airstream in flight. He developed the theory for wing flutter in three-dimensions. Wing flutter occurs when a wing becomes unstable in bending and starts flapping very rapidly and could break easily. Al's name will keep coming up in what follows because we interacted many times through the years, in many ways.

Back to "live" bombs in the wind tunnel. These were hollow plastic models of bombs that were filled with an emulsion of red lead and oil, to get the appropriate scale in the relationship of weight to the moment of inertia. There were scaling equations that eventually I had published in the *Journal of the American Institute of Aeronautics and Astronautics* (AIAA).

We would drop the scaled bomb models in the wind tunnel and watch their flight paths with high-speed photography, which had just come into common use. They would splatter on the tunnel floor and we had to mop them up, but that was no huge problem. Meantime, the Aerodynamics Division calculated the forces that should be on the wing and the bomb. Flight tests flew full scale bomb shells and measured the forces, and we got quite good correlation among the model, the theory, and the full scale measurements. Then we dropped the bomb over Lake Erie, and, indeed, it tipped and punched a hole in the wing flap of the F-86 fighter that dropped it. But the airplane survived.

Then we extended the tunnel work into release from bomb bays in the small supersonic tunnel; this was in anticipation of the B-58 bomber.

BOB SHELDON: What variables did you play with to fix the bomb drop so it didn't tip onto the wing?

SEYMOUR DEITCHMAN: I'm about to get to that. In fact, those tests showed that the bomb would have trouble getting out a bomb bay at all because when the bomb bay opened you'd get the flow curling around the front and it would hold the bomb up there and the bomb would tip and hit the airplane structure at the top of the bomb bay and sometimes just hang up there without leaving the aircraft.

Let me jump ahead in history now to complete this part of the narrative, and then I'll come back to the chronological flow.

Al Flax had become Assistant Secretary of the Air Force for Research and Development (R&D) when I went into the Pentagon in the Fall of 1963 to set up the R&D program for the Vietnam War, an activity I will describe later. I saw aircraft designs in reports that were passed around, and in particular I saw one I was sure would have bomb separation problems. I wrote Al Flax a memo that got the Air Staff all in a dither. I learned two things out of that. The first was that only people of the same rank could address an Assistant Secretary. And the second was that bomb ejector racks had been invented. Ejector racks pushed the bomb out so fast through the slipstream that the tipping was no longer a problem. That was my first lesson about being overtaken by events through technological advance. All that work we did on the aerodynamics of bomb separation - the equivalent of about $10 million worth of work today - didn't really matter any more.

Now, back to the Cornell Aero Lab (CAL). I worked for a long time on another issue that was overtaken by technological advances. I worked for several months converting the 8x10 tunnel to a transonic tunnel, and that meant shaping the tunnel throat correctly to get the transonic aerodynamic flow (about Mach number 1.2), in such a way as to keep the supersonic-subsonic transition shock wave at the exit from the tunnel throat from moving back over the model. You wanted to keep it downstream of the tunnel throat. The tunnel throat had to be shaped like a supersonic nozzle and be perfectly smooth. Something like a bolt head protruding a millimeter, or a small random variation in the throat contour, would set off the shock wave at the throat and keep the tunnel from going transonic. We had adjustment screws on the outside of the tunnel to get the nozzle shape right, and it was just a long process to get it – and keep it – perfect.

While this was going on, Al Flax came back from a stint as Chief Scientist of the Air Force with a new design for a slotted-throat wind tunnel, which meant the tunnel throat had an array of longitudinal slots from the subsonic to the supersonic part. The slots absorbed small perturbations in the flow so that you could shape the tunnel throat correctly and keep the transition shock wave where you wanted it, and it stayed there. That was another example of being overtaken by technological advance. I learned a lot about that kind of thing early in my career.

Eventually I got tired of wind tunnel testing so I left the CAL and I went to Bell Aircraft, which was also in Buffalo, on the other side of town toward Niagara Falls.

BOB SHELDON: What year was that?

SEYMOUR DEITCHMAN: Approximately 1953-54, because there's a landmark which happened in 1955-56, that I'll describe shortly.

There were several amusing and interesting things at Bell Aircraft. One was that Larry Bell, who set up the company and still worked there when I was there, insisted that he was going to punch the time clock. He was going to be a regular guy, which meant everyone who worked there, including the professional staff, had to punch a time clock. There went the flextime I had enjoyed at the CAL.

The aerodynamics department was in a very large open room with desks next to each other, a lot of hubbub all around. I talked to Paul Emmons, the Chief of Aerodynamics - a wonderful, friendly guy. I said, ''It's hard to get any work done here.'' And he said, ''Sy, airplanes started out being built in barns and they're always going to be built in barns.'' So I lived with that.

Another interesting thing was that German General Dornberger, who had been in charge of all of the World War II German rocket work at Peenemunde, was now at Bell Aircraft. He made a proposal for what was called DynaSoar (dynamic soaring) - a sub-orbital vehicle that would be a very fast bomber. Of course, that was overtaken by satellites and supersonic bombers. But just recently in *Aviation Week*, I saw that someone's trying to revive the idea. Some things never die.

The thing I put most of my time into there was designing the trajectory of a missile called the RASCAL. First, I ought to describe the computer that I was working with, because this was long before the days of electronic computers, or just maybe at the beginning of those days. This was a huge metal frame, about six feet long and three feet high and three feet deep, that was full of gears and levers and electric motors. You had to adjust the lever arms on all these things to do the calculations you wanted, all based on leverage calculations and gear ratios.

The RASCAL missile was very interesting because it was a big beast. Launched subsonically from an aircraft, it climbed to a very high altitude, went supersonic, cruised for about 100 miles and then went into a radar-guided terminal dive against a ship. The US never was interested in that missile, but it showed up as the Soviet Union's AS-4. That's exactly the Backfire-launched missile that became the main threat against aircraft carriers, because it could home in on the corner reflector made by the flight deck and the island. That's an interesting example of inadvertent technology transfer. How they got it, I have no idea.

Also, during that period, I went to the University of Buffalo (UB) at night and got my Masters in Engineering. My thesis, which I did partly on stolen time at Bell when the professional work was slow, was on the subsonic downwash field behind a wing due to the tip vorticity. Because there weren't good computers at the time, I devised an approximate method for calculating the downwash field behind a swept-back wing that didn't entail a lot of calculation. Swept-back wings were just coming into use for high speed aircraft, and this was a hot topic at the time for stability and control calculations.

At UB, I had to defend that thesis just like a PhD thesis. It was quite a big do as far as they were concerned. It was published in *The Journal of the American Institute for Aeronautic and Astronautics*, and I hope somebody used it at some time. I never went on for my PhD because the professional work just got very interesting and my wife and I were starting our family. I was starting to travel a lot and there was just no opportunity to go back to school.

BOB SHELDON: What was your master's degree in?

SEYMOUR DEITCHMAN: It was called an MS in Engineering and included a lot of work on theory of elasticity and some advanced math related to stress analysis of structures.

It might be interesting to note some of the bureaucratics of a university, which probably haven't changed much in all these years. When I was at Langley Field, a friend and I would borrow a room in the local high school at night and we went through a textbook and taught ourselves differential equations. I wanted to get credit for the course at UB, and I volunteered to take the exam to show that I was qualified. They said, ''No. You can't do that. You have to take the course.'' Well, I didn't take the course, but nobody noticed, and I got my master's degree anyway. That's the kind of thing that will keep showing up as we go through this oral history: I have fought the bureaucracy throughout my whole career.

I also spent a lot of time at Bell, when there was spare time, looking through *Methods of Operations Research* by Morse and Kimball, and I became interested in operations research. I finally realized I was going nowhere professionally, so after about 20 months at Bell, I talked to Ira Ross, the President of the Cornell Aero Lab, and went back there into the Systems Research Division.

BOB SHELDON: What year did you finish your master's degree?

SEYMOUR DEITCHMAN: 1955. The first thing I did there was work on air traffic control. General Edward Curtis was a friend of President Eisenhower. Air traffic control had turned into a god-awful mess. If I recall, there were something like 80 agencies that had something to do with qualifying airplanes to fly and managing traffic control. Ike wanted to do something about that. He asked General Curtis to pull together an organization that would study the problem and tell them how to straighten it all out.

At just about that time, when transport airplanes flew transcontinental – (this was DC-6s and DC-7s, Constellations, and other propeller-driven aircraft) – they would circle the Grand Canyon to show everybody what a wonderful sight it was. Two of them came together over the Canyon, and about 180 people were killed. That really emphasized the need to make some order out of the air traffic control mess.

There were three outfits involved in the Curtis study. CAL concentrated on forecasting where aviation was going; Airborne Instruments Lab, which was in New York, did the traffic measurement and worked on the traffic control aspects of communications and radar;

an economic research organization in Cambridge, Massachusetts projected the traffic demand. These outputs were all put together to make up the final report, which Gen Curtis covered with a brief summary.

Among other things that I worked on during this study was the question of supersonic transports and what they might do to the air traffic control systems. I looked into that issue and concluded that there would be supersonic transports, but at a price. At the time, it appeared that the only way you could get transatlantic range was to use a boron-based fuel called ''zip fuel,'' because it had a much higher heating value than kerosene-based jet fuel. Then you could get the long range, but the boron in the fuel would have made an environmental mess because it would dump combustion products consisting of boron oxides like borax powder into the atmosphere, and that would have settled to the surface and been a dangerous pollutant.

We had talked to several of the aircraft companies about supersonic transport prototypes. I projected that the prototype would be flying in the early 1970s and it would be so expensive to fly that you'd have to charge a very high fare. Therefore, there weren't going to be enough of them to worry about; they would fly above the subsonic traffic enroute, and around airports they'd fly subsonically anyway, so they would fit in with the rest of the traffic.

Then the Concorde came along in the early 1970s and bore out my forecast exactly. I should have gotten out of the forecasting business then.

What I didn't anticipate was the turbofan engine, which made the Concorde possible. That engine was efficient enough that it could use ordinary jet fuel. That made the Concorde possible, because it never would have flown with the boron fuels. The environmental impact would have been too severe. So there are things you don't anticipate in technological forecasting – you might be right in the large but for the wrong reasons in detail.

That work led to the Airways Modernization Board, which later became the Federal Aviation Agency in 1958. It didn't become an ''Administration'' until 1967 when it was folded into the Department of Transportation. Thus the air traffic control work at CAL did lead ultimately to what is now known as the Federal Aviation Administration (FAA), and we continued a lot of work for them.

Some of the significant outputs that I worked on included: showing the Agency that they could locate their air traffic control research center at Atlantic City, New Jersey where it still is. The reason was you had the busiest airway in the world, between Florida and Boston going up the East Coast, and they would be alongside of it so they could observe that traffic. They could see what happened if they changed the methods of en-route control and things like that, without interfering with the traffic. So that was good.

In another project, they were searching for a location for Dulles Airport. We looked at the traffic flow in the Washington, DC area and found that traffic at an airport in the location that was initially proposed would interfere with traffic at both Andrews Air Force Base, Maryland and Washington National Airport traffic. We recommended the site at what is now Herndon, where it is today.

At the same time, we started working for the Army on Army air mobility. It was sponsored by the first group of Army aviation enthusiasts led by Colonel Williams starting in 1956 or so. They were running into a problem. There was one twin-rotor helicopter with the rotors set longitudinally at both ends. I think it was the H-21. The French had used it very extensively in their war in Algeria.

The Army wanted to develop a more advanced twin-rotor helicopter which turned out to be the CH-47 Chinook. They were having a problem with it because they couldn't get the hot-day performance that they wanted. For hot-day performance, the specification was it had to be able to lift its full payload at 95 degrees Fahrenheit temperature and 6,000 feet altitude. I did a study of the geography on the whole southern Eurasian perimeter, which is where we thought we'd be involved in warfare. It turned out you almost never ran into those conditions. The critical conditions occurred where we are now, around the Iraqi Desert where 100 degrees and 3,000 feet would be critical for helicopter performance. Changing the specification was much easier on the design of the aircraft, so they were able to go ahead with it; successive versions of the Chinook are still Army standard transports.

They also asked us to look at a utility helicopter, the Bell UH-1, and we told them that it wouldn't be cost-effective. That turned out to be the Huey of Vietnam fame, and it's in service in some areas still. Showing, again, that in cost-effectiveness analysis you can be right some of the time but not all of the time.

Our work helped the Secretary of Defense, ''Engine Charlie'' Wilson, to issue the Wilson Directive dividing aviation between the Army and the Air Force, which still is in effect. Basically, what he did was give the Army rotary-wing aircraft and give the Air Force fixed-wing aircraft. There are some exceptions: the Army can have some small transport aircraft, and the Air Force operates some utility as well as search and rescue helicopters, but in the main that's the way it's set up now. Some of the issues in the decision were: What does it take to maintain those aircraft? What does it take to sustain them? What does it take to buy them? What do you have to know? We helped them with their decision.

I also did some work on bombing accuracy, which I'll come back to later because it's been a continuing interest. At that point I concluded that with the weapons available at the time, the only way to get good accuracy was with dive bombing. That was going to change, as I'll indicate later on.

Another significant thing happened professionally. Mark Kac, who is the father of probability theory and was at Cornell University, came up to the Lab and gave a course on Fourier transforms, which I took. I don't remember anything about Fourier transforms, but I remember one of his dictums about making assumptions.

His example was the ''snowplow problem.'' The snowplow problem is the following: it started snowing at 6:00 a.m. in Ithaca and I want to have the streets cleared by noon; how many plows do I send out? And that's all you know. He illustrated how you can make reasonable assumptions, and write the differential equations for getting the streets clean. That, I've never forgotten: If you don't have enough information, make some reasonable assumptions, and that has stood me in good stead through the remainder of my professional life (and personal life, too, I might add).

In 1960, the opportunity arose for what was supposed to be a year in Washington on loan to a relatively new organization called the Institute for Defense Analyses (IDA). At that time, that organization was built mainly around the Weapons System Evaluation Group (WSEG), which worked with the Joint Chiefs of Staff, performing operations research and systems analyses (ORSA) for them. IDA had just set up a new organization, the Research and Engineering Support Group, at the request of Herb York, who was then the Director of Defense Research & Engineering (DDR&E). (And, parenthetically, I got to know Herb pretty well too when he later became an IDA Trustee.)

At any rate, he had asked for technical support on research, engineering and related ORSA matters. At first the way the IDA organization outside of WSEG worked was to borrow people from industry. IDA had what was informally known as a ''CAL'' slot. The opportunity came up to go there because the current incumbent was returning to CAL, and I heard about it. I agitated to go because I was ready for a change again. We moved to Washington. I was interviewed by Charlie Townes, who was then Vice President for Research at IDA and later won the Nobel Prize in physics for the invention of the maser and the laser. I passed muster there and was offered the appointment.

Interestingly enough, the first task I got was to look into machine translation of language, for the Director of Research and Advanced Technology within the Office of DDR&E in the Pentagon. That's something we're just beginning to learn how to do now, over forty years later. The issue, then as now, was how to handle semantics and idiom. Semantics and idiom go together, and they're different in all languages. When you finally have a big enough memory, you can begin to translate one into the other if you set it up such that the machine knows what to look for. And finally, we're getting there.

There was some machine translation of U.S. tech manuals into Vietnamese during the Vietnam War. I never learned how well that worked, i.e., whether the Vietnamese could understand the manuals, but that was a practical attempt to deal with it.

Among other things during that first stint at IDA I worked with three social scientists. One was the late Jesse Orlansky, a human factors psychologist from Dunlap & Associates in

Connecticut, who arrived at IDA the same week I did, also on a one-year assignment. We both stayed on for a long time, except for my stint in the DoD, into the 1980s and beyond. Another was the late Joseph E Barmack (JEB), a psychologist who eventually became head of the psychology department at CCNY. The third was social psychologist Alex Bavelas, a brilliant guy with a fiendish imagination. To illustrate that, I'll mention an experiment he had set up (outside of IDA) that's worth describing for the interest of it.

He had somebody plant a rumor in a rail car in New Haven, on the New York, New Haven, and Hartford Railway. It was done just by having a couple of people seated side by side and talking to each other in a loud voice. It was an outlandish rumor; I can't remember what it was, but of topical interest. Then he had people stationed all around Grand Central Station in New York listening to what people were saying when they got off the train. That rumor had gone through the whole train in the 40 minutes it took to go from New Haven to New York. That says a lot about what one can do in psychological operations.

Be that as it may, I learned from these three people about the social sciences and human factors engineering, a result that will have an effect on later adventures in my career, as will be seen.

Then, during the year that I was assigned there, IDA reorganized and it was decided to establish the Research and Engineering Support Division (RESD) with a permanent staff, and I was invited to stay at IDA. John Kincaid, the Division Director, came to me and said, ''Sy, I like the way you work. You're a self-starter and that's what we need, so would you stay on?'' There followed a lot of soul-searching and asking what would I do if I went back to CAL, questions to which I didn't get a satisfactory answer, so I decided to stay. Thus, my one-year leave in Washington became 48 and still counting. We bought our house in 1961 and we're still in it.

During the first year at IDA I had also been doing some work on tactical warfare, looking at the tactical warfare budget and the emphasis in it and where it might be changed. This was now 1961. Harold Brown was DDR&E, which at that time was, according to law, the Number 3 spot in the DoD. The Acquisition Executive (AE) hadn't yet been separated out, so Harold was Chief of Research, Development, Test, and Evaluation (RDT&E), as well as the AE.

We were just getting started in Vietnam and Congress appropriated $120 million for ''limited war,'' whatever that was, and Harold wanted to know how that money should be spent. He set up the Limited War Task Group, which came to be chaired by Luis Alvarez, and I was named the Executive Secretary of the Group because I had been working on limited war kinds of things at IDA.

(As a slight amusing aside, Daniel Ellsberg was on that panel, and I got to know Dan well enough that when the *Pentagon Papers* were broken loose, I said to myself, ''I bet Dan did that.'' I turned out to be right.)

Most of the panel went to Vietnam, because the war was just breaking out, and they wanted to see the place at first hand. I went with four other Group members to Europe and the Middle East. Then I decided, since we were going to the Middle East (Teheran), to take a couple of days off and go to Israel, which I did at my own expense. There was a Marine colonel along with us who wanted to go there too, so we went together. He knew Israeli Col Yariv, who had been the Israeli military attaché in the U.S., and he told him we were going to be there. Yariv arranged a meeting with General Rabin, who was then the Vice Chief of Staff.

The Israelis are very blunt. We came in, sat down in his office, and as usual in Israel, there was a table with a big bowl of fruit in front of his desk. After inviting us to partake of the fruit, he said out of the blue, ''So you're here to study limited war. Who do you think you're going to fight in this area?'' I mumbled some words about people with Soviet weapons. And he said, ''No. I'll tell you what you're going to do. A war is going to start between us and the Arabs and you're going to try to interpose yourselves. And I'll tell you, I'll have it over with before you can get here.''

The fascinating thing to me was that he was describing to me the top-secret plan I had been briefed on at the Middle East Command in London. I didn't tell him that, but he had reasoned that that would be the only approach we (the U.S.) would reasonably be able to take. And, his prophecy about having it over quickly came true in the 1967 war (I came to understand afterwards that he was the architect of the Israeli strategy in that war).

We came out of the task force effort recommending that there be a big increase in R&D on items related to tactical warfare and limited warfare because there had been very little until then (although I found out later, when I had to pull the R&D program for Vietnam together – see below) that much of the ongoing R&D program that had been started for other reasons could be adapted to yield outputs useful in the Vietnam War). That report is what led ARPA, (the Advanced Research Projects Agency) to start Project Agile, which was a tactical warfare R&D program aimed primarily at the developing war in Vietnam. I made myself very unpopular with the first director – an associate ARPA deputy director who had come to ARPA from the Marines and the intelligence community (as I recall hearing) named Bill Godel, because I asked him a couple of questions at the first meeting we had in which he was looking for IDA help in setting up Project Agile.

He wanted to make a spraying apparatus to put on airplanes and helicopters to distribute Agent Orange because they'd been told by Sir Robert Thompson, who had been chief of the counterinsurgency in Malaya, that part of protecting the population against insurgent attack was to clear the brush away from the roads and the villages to keep the insurgents from hiding there. (It turned out in the long run that the Malaya and Vietnam situations were totally different, but that's a separate issue.) Godel wanted to spread Agent Orange for the purpose, and I asked him, ''Does anybody have any idea how that's going to affect people, because it's going to be all over them when you spray it.'' He didn't like that. He said, ''I've got a war to fight and don't bother me with that kind of crap.'' The second question was, that he was pushing the Colt AR-15 rifle (the lightweight assault rifle had just appeared at the time) for the Vietnamese because, he said, the big M1 Garand rifle (the then-standard U.S. infantry rifle) was too heavy for them to handle, and its kickback was hurting their shoulders when they fired it. I asked, ''Well, the Vietcong seem to be doing quite well with captured ones – aren't they Vietnamese also?'' He didn't like that either.

However, I was wrong about the AR-15, similar to my assessment of the Huey, because it became the M16 assault rifle, the main U.S. infantry assault rifle. The irony is that the Vietnamese Army didn't get it until 1968, and this conversation took place in 1961 or 1962.

From the Alvarez Committee, I went more deeply into the limited war studies that I've mentioned, and based on my observations about the developing war in Vietnam I wrote a paper developing a variant of the Lanchester equations for guerilla warfare. The classic Lanchester equations consider two sides fighting symmetrically. If both sides are fighting in the open, this leads to the Square Law in which the relative fighting strength of the two sides compare according to the square of the number of people on each side. If the two sides are in hiding so their people are invisible to each other, the equations lead to the Linear Law, in which relative fighting strength is simply proportional to the number of people on each side.

In counterinsurgency, you usually have one side in hiding, but the other side in the open, which is the usual sort of guerrilla ambush situation, and nobody had written that up. I wrote that paper, which I've been told since that time has been considered by the OR community to be a classic, although I had no idea it would be viewed that way at the time.

I also wrote my first book during my time at IDA in the early 1960s. The book was called *Limited War and American Defense Policy*, and in it I tried to wrap up what I was learning about the U.S. trying to fight limited wars. One of the points I made in the book was that ballistic missiles didn't hold a candle to reusable aircraft in terms of the cost-effectiveness of firepower that can be delivered over a period of time. That's a key point, and I will come back to it later on. Also, I had looked at the Tacnuke [tactical nuclear weapons] problem in context of the relative deterrent value of NATO IRBMs [intermediate-range ballistic missiles] and aircraft, and my conclusions were of interest to Secretary McNamara, who quoted them as part of the basis for the strategy he was developing to deal with the NATO-Warsaw Pact standoff.

But the main significance of the book in retrospect was that I had worked out a very simply stated strategy for counterinsurgency: you have to defend the population, you have to go after the insurgents, and you have to change the conditions that induce the population to help the insurgents. That is now Gen Petraeus' policy in Iraq and, I believe, in Afghanistan (not that I think Gen Petraeus used my recommended strategy, but the approach has to be obvious to anyone who thinks seriously about the problem). However, I was told by Paul Gorman –whom I knew in the Pentagon well before he became the four-star Commander in Chief of our Southern Command when it was in Panama, that ''This is still the classic on counterinsurgency.'' Paul hauled out his copy of that book and made his comment at a counterinsurgency strategy meeting at IDA in the early 1990s (which, interestingly, included Ollie North among others). It was nice to think I had created something that would last. And thinking about it now, I have to say that although the strategy is simply stated, it can be fiendishly difficult to carry out.

Along the way, also because of my previous experience with Army aviation at CAL, I became involved on the periphery of planning for the tests of the first helicopter mobile division, which at the time was called the 11th Air Assault Division. I had met Colonel Harry Kinnard, who'd been in the Pentagon and was planning those tests, and who later became commander of the Division.

That division went into a lot of operational testing and exercises in the Carolinas, leading to its final organization, tactics and equipment. They made a lot of changes from the original design along the way. It was sent, in 1965, to Vietnam by Secretary McNamara, as the First Cavalry Division (Air Mobile), under the command of then-Major General Harry Kinnard. I was privileged to visit them at their headquarters at An Khe in the central highlands of Vietnam.

BOB SHELDON: Was this just before the Ia Drang campaign?

SEYMOUR DEITCHMAN: Yes. I was there just before the Battle of the Ia Drang Valley. But the Division was just getting started in its new environment.

What was fascinating to me, in addition to the huge field full of helicopters that they called ''the parking lot,'' was a mountain called Hong Kong Mountain, right in the middle of their quite large area of occupation. General Kinnard said, ''On top of that mountain, it's full of Vietcong. By patrolling, we keep them up there. They're not bothering us, and that's easier than trying to root them out.'' That was a good illustration of the complexity of the Vietnam environment.

As a result of working with the Alvarez group, the work I had been doing at IDA, and the book I had written, I was invited to join Harold Brown's DDR&E staff as his special assistant for counterinsurgency. What he asked me to do was to build and coordinate the R&D program to support what was now a full-fledged war effort in Vietnam.

I started working in OSD in October of 1963, and I reported to Harold through John McLucas, who was the Deputy for Tactical Warfare Programs in ODDR&E. When I came in and first talked with John McLucas I commented that I had done a lot of reading about the French war in Indochina. I said, "This is a war about people, and I think we don't understand the enemy very well at all. We have to do some research on how to deal with the population of the area that we're trying to defend. And that means doing some work in the social sciences." John responded by saying "That sounds interesting. Let's go see what Harold thinks."

Harold said, ''That's fine. Do it. But I want you to build the R&D program to support the war, and get that ARPA bunch out of the Services' hair,'' referring to the fact that there had been a lot of cross-service and ARPA interference at the time. Every organization was building up its own R&D programs. I had my assignment, and also the freedom to try to build the program as I thought was necessary.

That tour in the Pentagon lasted two years, and I thought now it would be useful to cover some interesting things that occurred during the tour.

BOB SHELDON: Let me back up to your first trip to Vietnam. What was your sense of how things were going in the country? Did you get to observe some of the operations and get a first-hand feel?

SEYMOUR DEITCHMAN: A quick answer to the question, and I'll come to it more later. ARPA had a field unit in Vietnam. When I was in DDR&E, I would visit that field unit. When I was head of Project Agile, it was my field unit. I would talk to the people there. It was run by a senior U.S. colonel, and he had a total of about 30 U.S. military people and 30 Vietnamese counterparts of equivalent rank. We had a couple of majors and several captains and lieutenants and enlisted people.

I would talk to them about how the war was going and I would talk to the researchers, the R&D people. I would get a much better idea of what was happening in that war than the canned briefings – the command briefings that all visitors, including me, would get. That way, I think I had a much more realistic view of how the war was going. The command briefings tended to be optimistic, predicting how soon the war could be won, and such, but from what the Field Unit and researchers said, it appeared it would be much more difficult for the Vietnamese to succeed. There was a lot of incompetence in the Vietnamese officer corps, and much corruption, and the Vietcong pretty much called the shots on when and where the fighting would take place. I'm going to get to more of that later.

Let me deal with the bureaucratics first. When I got into the Pentagon, the first thing I did was set up a coordinating committee that included representatives of the Services and ARPA. In the group, we gathered together all the things the Services and ARPA said they were doing in R&D for Southeast Asia for the war. I also drafted memos for the Services regarding work I thought needed to be done. Usually, these memos were prepared for signature by Harold Brown to the assistant secretaries, according to how the bureaucracy worked. Among other things, that set included my memo on social sciences.

At that time there was a social scientist in OSD, Rains Wallace, a psychologist who later headed the psychology department at Ohio State. His job was mainly training and education and human factors engineering. We became good friends and he and I worked together quite a bit; this will come up in context of Project Camelot, which I will get to shortly.

DDR&E, by that time had $250 million in what Congress called an emergency fund to allocate for R&D on the war, and it was up to me, really, to figure out how it was to be spent. I would make recommendations to Harold and he would, most of the time, decide, "Okay, that's a good expenditure. Let's do it." Through my coordinating committee, the Services knew I had access to that money, and typically, they would start trying to load on all kinds of extraneous or impossible projects.

One was, for example, a bombing system for the B-52 to bomb from low altitude. My response to that was, "You're not bombing from low altitude in Vietnam and you have all kinds of strategic money to do that. No. You can't have that money."

Another one that came from the Air Force was that they proposed putting a satellite with a huge dish to keep on station over Vietnam. It would reflect sunlight down over the Mekong Delta region and light up the battlefield so starlight scopes could be used all night – it would be the Air Force's artificial moon. It turned out when we did the costing and looked at what the capability to keep on station at that time was, it would have been a gross waste of money.

At this point, I'd like to pay tribute to Gene Fubini. He came to the Pentagon from Airborne Instruments Lab, the same organization that I had worked with during the Curtis Study. He was the principal deputy DDR&E. He had a fantastically brilliant mind and was extremely blunt. He would ask difficult questions when the Services would come to brief him on programs. If they couldn't answer the questions, he would just say, "Get out of my office and come back when you know what you're doing," and things like that made him famous throughout the DoD system. Years later there was a tribute dinner to him that included everybody he had ever worked with, and the festivities were replete with stories like that. But the point was, he trained the R&D system to a level of rigor that it would not have achieved otherwise.

As an example of how he and I interacted: I would send memos up to the front office saying we needed to do certain things. And I'd get back a note that said, "What's wrong with your memo is XXX." I finally said to myself, "I have to make it with this guy or get fired. I have to keep pushing.'' One of the things I said after I came back from one of my trips to Vietnam was, ''We've got to do more operations research there. They don't really know how the enemy is functioning, nor how we are performing against him.''

He called me into his office and said, ''Why are you saying that?'' He gave me a half-hour grilling on why we needed more operations research in Vietnam. I thought, ''Well, he doesn't believe it, but okay.'' After I'd been there about six months or so, I arranged, (with their help, of course) a very large meeting that included Harold, Fubini, and John McLucas, all the Services' assistant secretaries for R&D, and all the military R&D chiefs. I had the Services and ARPA come in and show all their programs. We had an open discussion about all of it, so everybody knew where the gaps and overlaps were and we could start working on that together. As part of that meeting (which was a key part of trying to keep the service and ARPA programs from falling all over each other) Gene commented in annoyance at some service presentation that ''Sy Deitchman sends me these magnificent memos about this subject, and you guys are totally unresponsive to direction, etc., etc.'' That was how I knew I had actually made it with Gene.

Another interesting anecdote about the learning process along the way. ARPA, the Air Force, and the Army were all working on beacons to guide aircraft to a drop zone. The aircraft, usually C-47s, would come in very low to avoid enemy fire and drop their packages of supplies out the back door. I thought, ''Why do we have to work on three different beacons? Let's have one.'' So I said, ''Okay, we're going to have a fly-off.'' And we did. The Air Force beacon won the fly-off, and that's the beacon we went with. The price we paid for being careful was that the beacon came a year later than it might have otherwise, while it was needed in the field ''now''.

Finally, I ran into the arcanities of the requirements process. I found that things the Services and ARPA worked on weren't going to Vietnam to meet what we knew were urgent needs. And why not? I arranged a large meeting at HQ, Commander-in-Chief Pacific Command (CINCPAC), in Hawaii, and invited all the people I had worked with on my coordinating committee, and representatives from Military Assistance Command Vietnam (MACV), and CINCPAC; CINCPAC was really running the war and was in overall command, including command over MACV.

The question before the meeting was: ''Why aren't things being delivered out there?'' It turned out ''There's no requirement. You offer us something, which we have probably told you informally we need, but we have no way to take it because there's no requirement.'' It was a purely bureaucratic thing. We fixed that by having MACV set up a requirements section and we worked very closely with them, so that when they knew something was in work, they established a formal ''requirement'' for it so that the Services or ARPA could send it out there and MACV could accept it. That got it done, and it may have been the most important thing I did in my two and a quarter years as Special Assistant, Counterinsurgency (COIN).

Ironically, it reduced the time from idea to the field from seven years, which I had been clocking, to somewhere between two and four years. While in many cases that was still too long, that little bureaucratic change made a huge difference.

Now let me go on to some of the technical things. We were pressed to originate new things, and I very well remember a conversation I had with Harold Brown. We were looking at a whole bunch of things the Services had proposed, and most of them were weapons. And he said, ''Where's the greatest need?'' I said, ''It's finding the enemy.'' He said, ''Well, there's hardly anything here on that.'' I said, ''Right, because there are no new ideas.'' I think that's still true, actually.

There was a serious weapon need in one case, however, and that was to respond rapidly to ambushes. The U.S. Army Limited War Lab developed a variation of the Claymore mine that would fill the bill. The Claymore mine at the time was a circular thing about a foot across filled with explosive, somewhat domed-shaped on top, and with dozens of pellets embedded, each of which was about a half inch in diameter and a half inch long. The charge was attached to the side of the vehicle, and was designed to blow those pellets out in a fan shape, so that when trucks (usually the lead truck in a convoy) came under fire from an ambush, the enemy might get off the first shot, but then the Claymore would go off and that would be very effective in silencing further enemy fire. That weapon was one of the things that led to this whole issue of the requirements process, because, having been told informally by people in MACV that it was needed, we developed it and offered it to them and they said, ''Well, we have no requirement for that.''

I was also, interestingly enough, midwife to the gunship, which is one of the Air Force's main weapons now. A couple of majors from Wright Field came to my office one day and they said, ''We've got this idea but we can't get anybody on the Air Staff to be interested in it. Let us show you a film.'' They set up a movie projector and projected on the wall in my office. It was a DC-3 - a military version, the C-47 - with a 7.62-millimeter Gatling gun shooting out the side window.

They put the airplane into a tight circle with a steep bank so they could point the wing down at the target, and then fired that gun. They had done that over water outside of Eglin Field, and there was a big wooden platform in the water for them to shoot at. When that stream of fire crossed that platform, it totally disintegrated. And my first thought was, ''Wow. What a way to support villages that come under attack.'' I'd been in Vietnam on top of the Rex Bachelor Officer Quarters (BOQ) in Saigon and watched the flares drop and knew there was a fight going on at a village. There had been no good way to beat off the Vietcong attackers, who usually could overrun the village, and then would kill the village chief and terrorize the villagers into being Vietcong supporters. I said to myself, ''Boy, that's an ideal weapon for that.''

I told Harold about it, and he said, "Okay. Let's see the film." After he saw it he had lunch in the Blue Room where the higher level officials - assistant secretaries and up - usually would go for lunch. General LeMay, then Chief of Staff of the Air Force, was there. Harold told LeMay about the gunship experiment and the fact that the inventors couldn't get any attention to it in the Air Force. LeMay said, ''Well, hell. Let's get it out to Vietnam and try it.'' Those two majors got their chance and that was the beginning of the gunship. It has gone through I don't know how many generations now, but it's still a primary Air Force weapon, used by the Special Air Warriors [now Air Force Special Operations Command]. We keep hearing about it being used in Iraq, and I think it was used in Somalia just last year or so against some rebels there. I helped that go.

In another adventure, Paul Gorman, who I mentioned earlier, was a lieutenant colonel at the time. He came to me asking for my help because the Chief of Staff of the Army, General Harold Johnson at the time, had wanted a timetable, a schedule for winning this war. Paul asked for my help in crafting it. I said, ''Paul, you know you can't have a schedule because the Vietnamese have to be able to shape up to do certain things. You can have a sequence of events, but you can't have a schedule.'' He said, ''The Chief of Staff wants a schedule. I'm going to give him one.'' We see that kind of thing happening today, in Iraq and Afghanistan. Is there nothing new under the sun?

In another area, the Army had a need for $40 million to improve its uniforms, weapons, and the soldier's pack. As one example of the problems that needed fixing, pocket fasteners had changed from clips, where you'd put a button through the buttonhole and just rotate it, to Velcro. Velcro in the jungle at night was deadly because it made a noise, and, in fact, the static electricity when the Velcro was pulled apart would give off a little flash. If the troops were there in ambush watching an enemy patrol go by, then they couldn't move to engage the enemy. They couldn't get ammunition out of their pockets. That's just one example of a whole slew of things like that, needed to make the soldier's job easier and safer, which added up to $40 million.

At that time, I knew Charlie Poor, Deputy Assistant Secretary of the Army for R&D, quite well. I had met him when I was still at CAL doing some of our aviation work and he was at a high level at Aberdeen Proving Ground. I went to see him and I said, ''Charlie, if you just took the uncertainty in the cost projections for Nike Zeus –'' which was the Army's antiballistic missile (ABM) system at the time - ''you could cover this.'' He said, ''We can't touch that; it's sacred. Doesn't matter that there's a war on.'' There's another lesson in bureaucracy that carries into today's world.

Then came Project Camelot. Now, having learned a lot about the social sciences from my friends at IDA, working with Rains Wallace in OSD, and having Harold Brown's mandate to work with the social sciences, I wrote a memo to the Services that said, ''We want to encourage this kind of work. Tell us your ideas and we'll see what can be worked out.''

There was an Army FCRC (Federal Contract Research Center) called the Special Operations Research Office (SORO), whose job was PsyWar [Psychological Warfare] and things like that. I had just been to Panama and talked with General Porter, who was Commander of the U.S. Southern Command (USSOUTHCOM), and told him that we were starting this program in the social sciences to learn more about what motivated people to support rebellion against their governments, and how to deal with the issues. He said, ''Well, it's awfully sensitive to try to do that kind of research in this area.'' I said, ''There won't be any research of this kind going on in your area without our passing it by you first and getting your comments.''

About two months later – in the summer of 1965 – my wife and I had just taken our kids to camp and we were looking forward to our first summer by ourselves since the kids were born. When I got home, I got a call from Colonel Sullivan. He was responsible for the Army Research Office's social science program, among other things.

He said, ''Have you seen the *Washington Star* today?'' I said, ''No, I haven't.'' ''Okay, take a look.'' Walter Pincus, who's still writing for *The Washington Post*, had an article about a State-Defense feud because the newspaper in Santiago, Chile had published an article saying ''the U.S. Army is planning an invasion of Chile under the name of Project Camelot.'' That was how we learned that people in SORO had contacted some of their friends at the University in Santiago without letting us know. That blew Rains' and my efforts to use the social sciences to help in counterinsurgency sky high.

Our summer was destroyed because the State Department made a big play to take over all overseas research that the Defense Department was doing. The media had a ball. They were playing up the State-Defense feud and making fun of the social science research. Congress held hearings. Congressman Dante Fascell, who headed the House Armed Services Committee (HASC), called me and some other people in OSD – from ODDR&E and the Office of the Assistant Secretary for International Security Affairs (ISA) - to testify before his Committee. It was just a god-awful mess.

BOB SHELDON: What kinds of questions did they ask you?

SEYMOUR DEITCHMAN: ''Why are you doing this? Why do you have to?'' At the time, H. R. Gross was that day's curmudgeon on the HASC. He said, ''Well, what do you guys know about military things, that you can presume to try a thing like this?'' We tried to explain it to him, and I had commented about having retired military people working at the organizations that were supposed to do the work. He said, ''Oh, so you're taking advantage of the military guys, huh. I'm glad to see you had sense enough to consult someone with military knowledge, but they're not in the military now.'' There was lots of such needling.

Congressman Sensenbrenner, who is still there and was in those hearings at the time, led me into a trap. He said, ''I read an article in this morning's paper to the effect that women and children are supplying the Vietcong when they're in battle.'' I said, ''Yes, the guerrillas go forward and the women and kids bring up their ammunition and food while they're up in the front lines waiting to ambush some of our patrols, or engaged in extended combat.'' And he said, ''So, when we're fighting the Vietcong, we're fighting women and children.'' ''Well, yes – what choice is there?''

In fact, we see that happening today when the insurgents in, say Iraq, or Hamas in the Gaza Strip deliberately draw fire on women and children because they understand our chivalry-based rules but they don't feel bound to fight by those rules. They see the propaganda value of that asymmetry for themselves. Al Qaeda and the Palestinians learned from the Vietcong.

But I'm digressing. Camelot was a total disaster. The social scientists had a very hard time of it. Congress kept pressing us to know about the work in the social sciences. Senator

Fulbright, when he made his ''arrogance of power'' speech, referred to that as an example of this Defense Department's arrogance of power, thinking they were going to do work outside the ''traditional'' military concerns.

I offered the State Department money for them to do some of that kind of research. At the time, there was a rebellion in Angola, and the question was, what should be our position on that. I said I'd transfer the money to them if they would do the research on how the U.S. should respond to that. They (their Bureau of Intelligence and Research) said, ''Oh, heavens, that would be too sensitive. If it even got out we were thinking about it, it would be a disaster.'' This came out further in subsequent hearings, which I'll mention now, for continuity of the narrative.

This argument about the social sciences went on for two or three years. In FY68, Senator Mansfield attached an amendment to the Defense Authorization Bill saying that any research undertaken by the Department of Defense had to have a clear and obvious relationship to the DoD mission. That's still in effect, if anybody wants to enforce it – and, of course, the DoD mission is looking very different these days, after 9/11. Along the way, Mansfield and Fulbright had an argument with Senator Gaylord Nelson. Nelson said, ''The Defense Department will tell you this research has to be done. And if they don't do it, nobody will do it.'' But Fulbright wouldn't have any of that, nor would Mansfield, so the Mansfield Amendment passed. That view of the military mission is interesting in view of current events and the accepted DoD nation-building efforts in Afghanistan and Iraq.

Another interesting happening during my tour in the Pentagon: I had some indications that Secretary McNamara read the trip reports that I wrote on return from my trips to Southeast Asia. The typical trip was that I would spend two weeks or sometimes three weeks in Southeast Asia, with a week in Vietnam and then a couple of weeks in Thailand – where ARPA had a very large field unit, as I will describe in due course. I would then come back and spend the day on the beach in Hawaii, probably at Fort DeRussy where it was quiet, or else outside the Hilton Hawaiian Village Hotel.

I would talk my trip into a little recording machine, and when I got back my secretary would transcribe it and I'd finalize it. I knew it would go to Harold Brown, and after him Johnny Foster, and the Director of ARPA – this, no matter whether I was in OSD or ARPA - and I had some indications that Secretary McNamara was seeing those reports.

When I would go to Vietnam, I was always invited to stay in General Westmoreland's guest house, and I would have breakfast with him. I'd get his views on how the war was shaping up.

On one trip out there, in October, 1967, he was jubilant because they had just used a new ammunition called Beehive, which was a flechette–loaded shell [a shell loaded with hundreds of pointed steel projectiles] that could be shot out of 105 or 155mm cannon. He said, ''We caught a Vietcong battalion in enfilade – (meaning they were caught endwise so that the whole battalion was exposed to the fire). They were absolutely decimated, 100-percent casualties. At this rate, we're going to have them negotiating on our terms by spring.'' From what I understood of the Vietcong and North Vietnamese and their willingness to sustain casualties, I just didn't believe it.

I didn't believe it to the extent that I didn't even want to put it into my trip report, and I didn't, because I knew McNamara would see it. Then Westmoreland came back and briefed Congress on the progress of the war, and he said the same thing there. The next February we had the Tet Offensive, and that turned the whole thing around because he had led people to believe we were about to win it hands-down; and all of a sudden, they (the enemy) were a good fighting force. The public asked, ''What happened?'' I think that was the turning point in U.S. support for the war.

After two years working in OSD, I had done the job that was assigned and the program was running smoothly. John McLucas had left and Tom Cheatham, who had been at Grumman Aircraft, replaced him as the Deputy Director for Tactical Warfare Programs. Tom wasn't really interested in the counterinsurgency. He spent a lot of time pushing the F-14/ Phoenix program, which was a clear conflict of interest because he was from Grumman, builders of the system. He then said to me ''You're wasting your time on this Vietnam War stuff. Why don't you be my speech writer?'' I said, ''Thanks very much,'' and I went back to IDA.

After that, the counterinsurgency work was pulled out from Tom Cheatham and it was set up as a separate Deputy DDR&E for Southeast Asia Matters under Len Sullivan, who was a good friend at the time. He was a very capable guy who did a great job running the DoD R&D program in support of the war effort.

Meantime, back at IDA, I got involved in the JASON summer study related to the Vietnam problems. JASON was a group of physicists and chemists then, the top people in the country, a self-organizing group that a few young scientists were from time to time invited, by the JASONs, to join. As an indicator, several of them eventually won Nobel prizes in physics. The JASONs had organized themselves to work with DoD on defense problems. (There's a book about their history, by Anne Finkbeiner, called *The JASONS*, published by Viking/Penguin press in 2006.) IDA was their administrative home at the time, and the way they worked was to get briefed on defense problems, at two or three meetings during the Spring, and they would decide what they wanted to work on during the summer vacation, at a six-week summer study. They made some very important contributions to the national defense.

The JASONs had been deeply troubled by the whole Vietnam War. Under the leadership of George Kistiakowsky (who had been President Eisenhower's Scientific Advisor, the first to hold that position), physicist Jerrold Zacharias, and a well-known economist, Carl Kaysen, they decided they wanted to see if they could do some work on the Vietnam War that would help stop the bombing of North Vietnam. Since I had just come back to IDA from the Pentagon and knew a lot about the country's war effort, I worked with them.

They got an okay from McNamara to work on that problem. The idea was to see if we could close the Ho Chi Minh trail by setting up an instrumented network including sensors and attack aircraft to attack the North Vietnamese logistics trails through Laos so that they couldn't support the war in the south. Rather than go into that effort in detail here (because there's been a lot of misinformation around about it), I'll just note that I have just had published what I consider to be the definitive article on it in *The Journal of Military History* of July 2008. Why don't I just leave that as a reference that describes the whole operation.

Later on, that plus all the other work I'd done in OSD got me onto the President's Science Advisory Committee's Vietnam panel. I'll touch on that again later. Also, I was on the advisory panel for the Defense Communications Planning Group, which was the cover organization that was implementing what we called at the time the air-supported anti-infiltration barrier.

Just to make a note for the record though, the JASONs were very disappointed because they had hoped that mounting a major attack on the trail system, according to the system we designed during that 1966 summer study, would replace the bombing of the North, but it didn't. It augmented it. Also, it wasn't implemented as planned, because it was supposed to be a reconnaissance-strike system, but all the Air Force used it for was reconnaissance, and they responded with strikes as the aircraft were available. They didn't want to dedicate aircraft to the one effort, and that's what changed what the JASONs who worked on the system had hoped would happen, or at least, kept it from working as designed, for sure.

Also, part of the system was diverted to help the Marines besieged at Khe Sanh. I agreed with Bill McMillan, who at the time was General Abrams' scientific advisor, (Abrams had succeeded Westmorland) that the sensors and the capabilities would be essential to help avoid another defeat like that which did in the French at Dien Bien Phu a decade and a half earlier. At Khe Sanh we did have it as a recce-strike system, in part because by coincidence there was a Marine captain who had visited us during the summer study, where we told him what we were working on. He knew, when those sensors arrived, what to do with them, and that helped turn around the battle. Thus, it was the air strikes and artillery response that went along with that that finally caused the North Vietnamese to break off that siege, avoiding a major defeat for the exposed Marine force. According to a later Senate report, it probably saved hundreds or thousands of American lives. All this didn't become clear until after the war, however.

BOB SHELDON: Did the JASON group set up shop at IDA?

SEYMOUR DEITCHMAN: That was their administrative home, and IDA furnished meeting places. The Vietnam summer study took place in a dorm at the University of California, Santa Barbara, where preserving secrecy was a continuing headache, while the preliminaries took place during spring breaks at a girls' school in Massachusetts. Now JASON is associated with MITRE. They left IDA as an administrative home back in the 1970s, I think.

After the summer study I was asked to go back into DoD and head Project Agile in ARPA. Johnny Foster was now DDR&E, and he was the one who asked me. I don't remember whether it was Jack Ruina or Bob Sproull who was head of ARPA. Charles Herzfeld, who I knew quite well from my stint in ODDR&E, was deputy director then, and he became Director about the time I went into ARPA, in late fall of 1966.

Agile was ARPA's R&D program focused on the Vietnam War, started after the Alvarez Task Group's work that I described earlier. It had had a succession of military leaders, and Johnny and ARPA wanted someone with a civilian engineering background to head it, and I had just the right background because I'd been working on the Vietnam problems. Agile had three field units. It had the Vietnam field unit that I described earlier. It had a field unit in Thailand, RDFU-T (for R&D Field Unit, Thailand), which included about 30 U.S. military, 30 Thai military, a large number of U.S. contractors, and a large secretarial and support staff. Altogether, that came to a research lab of about 500 people.

What was done there included R&D on weapons and surveillance techniques, and communications techniques to help the war effort in Vietnam and also with insurgencies in Thailand's northeast and northern provinces.

BOB SHELDON: Where were the 500 people located? Were they in the DC area?

SEYMOUR DEITCHMAN: No. This field unit was housed in its own building in Bangkok. There was another small field unit in Beirut, Lebanon. Its purpose was, through research, to try to learn something about the area. That one had some interesting background. On a visit to Israel, I got to talk with General Bar Lev, the Israeli Chief of Staff, after whom the Bar Lev line defending the Suez border with Egypt was named. In response to his question I told him what that field unit was for, and I stressed the accommodations that had to be made between the Lebanese Christian and Muslim communities. His response was, "What are they worried about? We'll protect them," which shows the insensitivity of the Israelis to the subtleties in the Arab communities that they really needed (and still need) to be sensitive to.

In another instance, Dr. Herzfeld, when he was ARPA Director, was testifying before Congressman Mahon, who was head of the House Appropriations Committee, and dual-hatted, as head of the Armed Services Appropriations Subcommittee. Mahon wanted to know why we had that Lebanon office and Herzfeld said, "We have to learn something about the area; it is important to us strategically." Mahon said, "We have no interest in that area. I think you ought to stop that." This was in about 1968 or 1969.

BOB SHELDON: This was the Lebanon office he was talking about?

SEYMOUR DEITCHMAN: Yes. And it finally got disestablished in about 1970 or so, soon after I left ARPA.

There was also a low-level insurgency in Northeast Thailand which had been stimulated, I think, by the war in Vietnam. The North Vietnamese had been training the Pathet Lao, who had taken over half of Laos. They were also training what came to be called communist terrorists, abbreviated to CT, who would come into Northeast Thailand and agitate in the villages, attack police posts, and that sort of thing. We were also helping the Thai, trying to defeat that insurgency.

Now, a few of the outstanding things about Agile. At any time, I think I had 25 or 30 projects going. I think I've already mentioned Agent Orange and the M16 rifle that Agile had done. They had also put a gunner's shield on the M113 armored personnel carrier, which was the standard infantry vehicle in Vietnam. It had been built with a machine gun mounted on top, but no way to protect the gunner and the other side got onto that very quickly.

They were doing a lot of work with a vertically looking infrared scanner on a B-57 bomber, which could pick out Vietcong encampments at night and things like that. That turned out to be the forerunner of the forward-looking infrared sights that we use now. At the time, it wasn't used very much because the J2 of MACV felt, "That's an R&D thing, and I can't be bothered with that. I've got a war to fight," which was really too bad because it could have helped him fight his war. There would have been an ambiguity, though, because you would have to know where the villages were and be able to separate them from what were Vietcong encampments outside in the woods.

Also, there was work on jungle communication. It turned out that the dense vegetation at ground level was very absorptive of very high frequency (VHF) radio waves so patrols or outposts under attack couldn't get a message asking for help to go very far. Bobby Kennedy, who was then attorney general, had headed what John Kennedy had set up as a Special Group, CI, (for Counter Insurgency). At that time, we had just gotten our first space shot to Venus. It was also the beginning of our participation in the war in Vietnam, and we were hearing about Special Forces camps being overrun because they couldn't call for help, because their VHF communications weren't working. Bobby Kennedy famously remarked, "I don't understand why you can communicate across 25 million miles of space to Venus and you can't communicate through 5 miles of jungle."

ARPA had started a very large program on that, and eventually found there was a treetop mode where the foliage was less dense and did not absorb all the radio waves. The Special Forces and other troops who had to be in that dense vegetation were given a means to shoot an antenna up into the treetops so they could communicate. That was a very useful Agile output.

There was work done on vehicles that could move through rice paddies, and other rough or furrowed, ploughed terrain. Another problem was tunnel detection, because there was a whole network of tunnels underlying the area just outside of Saigon where the Vietcong were hiding and used as supply depots and everything else a military base can do. They were in Zone C, which was northwest of Saigon, a very large area. We found ways that helped detect individual tunnels, but we could never find enough to work out the entire network, and that's what a lot of the B-52 raids outside of Saigon were for – simply trying to uproot the whole area through which the tunnels were laced. There was also the Rural Security Systems Program (RSSP) in Thailand that was designed to help the counterinsurgency in Northeast Thailand. It was just getting started when I got there.

I continued all these programs that were ongoing when I got to ARPA, and I started some new ones. One was a quiet aircraft. Now we have the Predator UAVs [unmanned aerial vehicles], but at that time there was nothing like that. The Vietcong would make cuts in roads and lay mines at night, so our forces had to find them. I said, "There has to be a way to make a quiet aircraft."

I don't remember how we got to Lockheed, but they took a Schweitzer glider and mounted a muffled Volkswagen engine on it. It had a long propeller shaft with a very slow-moving propeller out front so you didn't get the noise-making shockwaves off the propeller tips. That could fly at 500 to 1,000 feet altitude and the Vietcong wouldn't hear it. From that aircraft, using the original starlight scopes for low-light level viewing, we could see them planting the mines, and then call in the ground forces to either chase them or at least dig up the mines.

We also extended the work with sensors. For one, we made up what was called a patrol seismic intrusion detection system. There were four small seismic detectors with an enunciator box, and patrols could put them out around their bivouac for the night. If anybody was coming in, they would hear them and they could get ready for them.

I started something that I called Small Independent Action Forces (SIAF). The complaint had been that the soldiers had a 90-pound pack and then they were overloaded; there had to be a way to lighten their load and make a patrol more efficient. Now we had in our field units, Brits and Australians working with us, who had been through fighting insurgencies, both in Malaya and in Borneo. They were very helpful in how to set up a patrol. What we did was to describe the patrol as a system. Their weapons were part of the system, their communications were part of the system, and the observations were parts of the system. They would all work together just as in ongoing patrols, but much more effectively and efficiently, while the soldier's load could be lightened considerably.

What we didn't count on were the human factors aspects. We lightened the solders' load but then they found they were used to carrying 90 pounds. After we lightened the load, they filled it back up with cans of Coke because that was quick nourishment in the field. Nobody was going to give up his gun. They were willing to have a primary communicator, but they found they'd better all be cross-trained in how to use that equipment, and have more than one set, just in case. And so on. That was about the state of it when I left ARPA; I know they continued the work, but I don't know what it came to afterwards.

We also found out, through our international dealings - I won't mention the country because I don't know if they still have it classified –a way to make a three-wire barbed wire fence which, in fact, we could use in our own southwest now. The wires were attached to sensors on the posts, with the posts maybe 100 feet apart, and if somebody either tried to stretch the wire or cut the wire, it wouldn't matter; you'd get an indication, and you'd get a readout. You'd know where that sensor was, so you could go directly to that spot.

I thought that was interesting enough so I bought about ten kilometers of it to put along the fence on the DMZ in Korea. This was where I learned that taking shortcuts and getting rid of milspecs doesn't always work. We put the sensors on the existing fence along the DMZ, but they were invented in a place that doesn't have freezing cold winters. In Korea, everything froze. The only sensors on the fence that worked were the tin cans that the South Korean Army tied up along the fence in bunches because if anyone disturbed the fence, they would rattle like hell. You keep learning lessons.

Now for some things for which there was no service home, like the Rural Security Systems Program in Thailand. We helped the Thai set up that system, which included protection for villages and civic action to help the villages improve their economies by getting pure water, employing their young men in things like auto repair, and an evaluation system to know how it was working.

They first had to get their police and their military on the same radio wavelengths because they couldn't call in help if a village was struck. Then, they couldn't detect the CT (communist terrorists) coming across the river, so we set up what came to be called ARPA's Navy, which was a set of Thai operated patrol boats with radars. But then we were cautioned by Ambassador Leonard Unger – a very nice guy who was very supportive – who said, ''Look, that river is not a boundary. It's a highway. The same peoples are on both sides of the highway, and you can't disrupt that culture by interdicting normal traffic across the river or we'll have even more trouble.'' The issue was, how do you do that? Again, the social sciences came into it.

In another part of the Thailand effort, ARPA had an anthropologist by the name of Bob Kickert up in the far northwest provinces of Thailand. The problem up there was that the hill tribes would practice slash-and-burn agriculture. What they were slashing and burning was teak, which was Thailand's main export. The idea was to try to get them to do agriculture in a different way, on fixed fields, and trying to get the government to help them do that. It never really succeeded. The government was always at war with those tribes, but we tried.

For the U.S.-oriented parts of the Agile program, I had concluded, finally, that we shouldn't start working on anything unless we had advanced assurance from the Services that they had established a requirement for it. There was a lot that we did – same problem I saw from the DDR&E side – that the Services just wouldn't accept. So what was the point? Then Eb Rechtin (Dr. Eberhardt Rechtin) – independently when he became director – made that ARPA policy. We wouldn't do any work with the Services unless they first said they would accept the results if successful.

The issue came to be, how does our research affect policy? At least on the social and political side, not much. I've just given the example of the Thai government with the hill tribes in the northwest. Another example has more profound implications.

Henry Kissinger was now President Nixon's National Security Advisor, and he was trying to ease us out of the Vietnam War. Among other things he pulled back the U.S. Special Forces who had been working with the Montagnard tribes in Central Vietnam, and put the ARVN, the Army of the Republic of Vietnam, in charge of their units. The Montagnard units were not reporting to the U.S. Special Forces now, but to the ARVN. By this time, I was out of the DoD but I was on the President's Science Advisory Committee (PSAC) Vietnam sub-committee. I sent Kissinger a memorandum, saying "There is active hatred between the Montagnard and the ethnic Vietnamese. If you put the Montagnard under the ARVN, that's going to end in a disaster." I never got an answer to it. I know he got it because I found that out through informal channels.

But, in fact, that's where the North Vietnamese broke through and came down to Saigon. Ban Me Thuot, which had been defended by the Montagnard, was attacked by the North Vietnamese and the Montagnard said, "Well, hell. We don't have to defend those ARVN guys," and they just melted away and the North Vietnamese came through and reached Saigon shortly thereafter. So how does research affect policy? I suspect that by that time, Kissinger and Nixon had decided we had to leave Vietnam, win or lose, so the memo I sent Kissinger, based on years of research by Gerry Hickey, was overtaken by events (OBE) before it was written, let alone sent.

Another example. By this time, Igloo White, which was the sensor network along the vehicle part of the Ho Chi Minh trail that had resulted from the JASON 1966 summer study, was in operation and with Loran D, which was a pretty accurate navigation system. We found that a 500-pound bomb landing within the CEP (circular error probable) of Loran D would destroy a truck. As part of the PSAC Vietnam Panel I had recommended that the Air Force, instead of using 2,000-pound bombs, which were too big and very expensive, could use 500-pound bombs and could buy several times as many for the same price.

John McLucas, who by then was Secretary of the Air Force, came over to talk to us. I never challenged him on it, because clearly the Air Staff was insisting, but he said, "The Air Force has no requirement for 500-pound bombs." Again, the requirements process trumped common sense.

In fairness though, I have to point out that R&D products do affect policy where the "powers that be" want the results the R&D offers. For example, for all the uncertainties in the application of the "electronic battlefield" in Laos, it did help win the battle of Khe Sanh when it was deployed there. General Westmoreland took advantage of a tool that was presented to him, although not in the way the inventors of the tool expected.

I also need to make a comment about being in "the system", and believing data selectively, because we see a lot of that happening today.

When I was in ARPA, we engaged a friend of mine – Alfred Blumstein, who's a top-notch operations researcher, now at Carnegie Mellon, – to help us understand the data that we were getting from Vietnam. He came to me one day and said – this was about six months after we had gone in with U.S. combat troops – "Sy, look at this data on enemy-initiated incidents. They've recovered from the escalation." I said, "No, that can't be right," and I gave him ten reasons why that couldn't be right. But now that I look back on it, of course, he was right. Which just goes to show, and I was known as a skeptic in the DoD system, that you get captured by it. We see that kind of thing happening today in many parts of the government, especially with respect to the wars we are in.

In the fall of 1969, I felt I had done what I could and I left ARPA and returned to IDA. That was after a more extensive job search because I figured with all the people I knew in the defense industry, and my reputation, I could write my own ticket. In fact, I got job offers with a lot more money doing very specialized things; for example I was offered the presidency of a company making surveillance equipment. In the end, I decided, no, I'd rather be doing analytical work "close to the throne". I went back to IDA, in fact at a pay cut, because Al Flax who was then president of IDA, was making sure it didn't look like they were luring me away from the Defense Department.

At the time, there was generally a lot of agitation against the Defense Department because of the Vietnam War, and there was a feeling among some of the staff in IDA, that, "Hey, we need to work on civilian things. All we do is war, and this is no good.'' Al Flax asked me to take over an office of civil programs that he had just set up, and help build up that office.

They were working mainly with the Department of Transportation on mass transit, and were looking at high-speed rail for the Northeast Corridor. They were also trying to start work on environmental issues, like pollution control, especially water pollution. They had a contract with Post Office department helping them evaluate mail-sorting machines.

As an aside, we showed the Post Office that the very fancy letter-processing machine they were thinking about buying would not be cost effective, and that they were doing fine with what they had and ought to have stayed with it. The Assistant Postmaster General for R&D at the time didn't like that answer. He cancelled the contract. This raises the question again: does research affect policy?

As another bit of trivia, we also made a proposal on reducing energy use to the predecessor of what's now the Department of Energy. They were incredulous that we were assuming oil was going to reach $20 a barrel. This was 1969-1970, when it was $10 a barrel or less. They wouldn't even consider our proposal, because they thought that with assumptions like that we weren't smart enough to do the work we were proposing to do.

I also learned some interesting things about Japan's high-speed train in the process. They didn't have an existing infrastructure to build on, so they built their whole system from scratch – the tracks, the cars, the wheel trucks to suit the new track gauge they concluded they needed, and the kite or trapeze (I'm not sure of the right word for it) that rides the overhead rail for the electric power. The whole thing was built as a total system. I realized that we in the U.S. were handicapped by having an existing infrastructure because what we were trying to do in this country was to use the Pennsylvania/New York Central railroad tracks to run a high-speed rail system between Washington and Boston. And the rail bed was just not up to it. In fact, it's only now 40 years later that we started the Acela Express train, and it's still not as good or as fast as Japan's train, or even Europe's high speed railways for that matter.

I had another interesting interaction in the civil work, where we contacted the FAA. Al Flax and I both knew Dave Israel from Lincoln Lab, where he had been very active in air defense. He had become Chief Scientist at the Defense Communications Planning Group, which worked on the anti-infiltration barrier and from there he had become Assistant Administrator of the FAA for R&D in the early 1970s.

Al and I went to talk to him about undertaking a study on how to transition from what was then the grid-like map on the ground that the airways were following, from one vertically broadcasting radio beam to the next, to a direct origin-to-destination navigation system because that technology was just beginning to come in.

We were doing a study at IDA on NAVSTAR (Navigation Satellite Timing & Ranging), which became what today we know as the Global Positioning System (GPS). There was also a system around, whose name I forget, that was a point-to-point navigation system just put out by Sperry. We said, ''It's time to look at how would we implement such a system?'' Dave Israel said, ''Look, I've been brought here to implement the Alexander report. I'm not going to touch anything that goes beyond that.'' He was referring to Ben Alexander who was a well-known systems engineer. His report, after a committee he chaired, assessed the air traffic control system and recommended putting transponder beacons on airplanes so that their radar returns could be tracked more easily, but made no fundamental changes in the airway system. That set air traffic control back probably 10 to 20 years at least, and it's another good example of someone being captured by ''the system.'' We're just beginning now to get caught up with the GPS technology in air traffic control, and that was stimulated by the successful use of GPS in the 1991 Gulf War, indicating how long it takes to adopt a new technology for peacetime applications.

But we did get a contract to explore mid-air collision avoidance systems. I had a long-term interest in that from the days when we were working with the FAA at CAL. In fact, I had published a paper on mid-air collision avoidance in which I demonstrated that you could avoid collisions by one airplane going up and the other one down if they were on a collision course, but there was no way they could be sure they wouldn't get on another collision course if they changed the two aircraft tracks with horizontal maneuvers.

BOB SHELDON: Did that evolve into TCAS?

SEYMOUR DEITCHMAN: Yes. The IDA team evaluated several mid-air collision avoidance systems that the FAA was thinking about, and the work ably done by Jim Bagnall and Bob Turner helped the FAA decide that they would authorize TCAS - the current Traffic Alert and Collision Avoidance System.

The final result of the civil program work was that I concluded that a Federal Contract Research Center (FCRC) like IDA couldn't compete. The staff had a different mindset. It didn't know how to prepare a winning proposal. Our cost structure was wrong because the overhead didn't allow for preparing proposals, and we couldn't stand the expense. There was no profit stream to absorb it. I recommended to Al Flax that we just drop it, and the ongoing projects were just absorbed into the divisions' work programs as we had them at the time.

I realized subsequently that Al probably knew all that, but that was his way of putting me on ice for two years because there was no way I could work at IDA on defense work without interacting with people that I had worked with in the DoD.

BOB SHELDON: So IDA stayed primarily with defense work?

SEYMOUR DEITCHMAN: Yes. There was still a trickle of work for civilian agencies, particularly continuing the FAA work for a while, but mainly, IDA worked for defense agencies – and mainly for OSD or defense-wide agencies, because we couldn't work for the Services when much of our business was evaluating service systems and activities.

To recap, Al Flax had put me on ice for two years in terms of dealing with the DoD, by asking me to head the Office of Civil Programs. At the time, because of the agitation about the Vietnam War, people were wanting to get into civil programs with less concentration on warfare programs. After two years, I had concluded that IDA was not going to be able to compete in that environment. Its structure was not constituted for competition, especially with commercial firms, so the office was disestablished.

BOB SHELDON: Explain again the reasons it wasn't competitive with commercial firms.

SEYMOUR DEITCHMAN: The overhead structure was wrong. We didn't have a profit structure where we could put profits into preparing proposals. The people didn't know how to prepare proposals that would compete in the commercial area because IDA was a protected organization; it had been working for the government and was paid by the government. But there was some civil work as I described previously.

About 1972, I was appointed Assistant Vice President for Research. Al Flax had held the Vice President for Research responsibility in addition to being President and Chief Executive Officer (CEO) of IDA.

My responsibility was to oversee the quality of the research program and help get it organized; I dealt with sponsor relations and reported on those things to Al Flax and the Board of Trustees of IDA.

I later became Vice President for Planning and Evaluation and continued the same kind of work. Then under retired General Andrew Goodpaster, former NATO commander who became President in about 1982, I became the Vice President for Programs with the same responsibilities, except now the division directors reported to me rather than directly to Andy Goodpaster, the CEO. And explicitly, he assigned me the full powers of the CEO at the times when he was absent.

Then in about 1984-85, General Goodpaster retired from the CEO position, and retired General William Y. Smith took over. He had been Deputy Commander-in-Chief Europe (DCINCEUR) under Andy Goodpaster when Goodpaster was Supreme Allied Commander Europe (SACEUR), and the Trustees elected him to be President and CEO. We then reverted back to the previous mode with the division directors reporting directly to him rather than to me. But all through it, I essentially operated in pretty much the same way. There wasn't a lot of difference in how I functioned through all those management changes.

BOB SHELDON: Tell me about some of the research programs you had under you and how you impacted them.

SEYMOUR DEITCHMAN: I'm going to get into that. But first, I want to cover some management innovations that I'd instituted. Throughout this interview, I have separated management from substance because I think it's easier to cover all of it that way, without rambling.

When I came back to IDA I found a steering committee operating, which included the division directors and the corporate management. It met every week on Monday morning, and the division directors reported on task orders pending, other management actions that had to be taken or were pending, and the outcomes of management actions that had been taken. So that minimized surprises. In fact, it's an approach I'd recommend for the federal government's cabinet secretaries and the President of the U.S. right now, if they don't do something like that.

Another responsibility was riding herd on report reviews. There were three levels of reports: papers, studies, and full reports. Every one of them was reviewed at a level determined by the level of the report, and then released by management. Papers were reviewed within the divisions and could be released by division directors. Studies were in between, usually reviewed from outside the division, but a division director or the Vice President could release them. Full reports could be released only by the President, and they had to be reviewed by people from outside the division that wrote them. If they were inter-divisional studies, then we might get reviewers from outside IDA. The President released them, usually after I reviewed them and recommended release. There weren't many full reports because those were reporting on big efforts, and I'll describe some of those shortly. I also reviewed some of the lower-level reports. I sampled them as a quality control check.

I also found that the semi-annual briefings for the Board of Trustees led to a sharpening of projects because for the ones we had to brief to the Board, we reviewed in advance and we made sure that they weren't going to cause the Board to say, ''What are you guys doing here?''

BOB SHELDON: How many people were employed by IDA at the time?

SEYMOUR DEITCHMAN: It was a total of about 500 people, with a professional staff of about 200 or 300. A large fraction of those had PhDs and masters degrees.

BOB SHELDON: When you say a division, about how many people were in a division?

SEYMOUR DEITCHMAN: Twenty to 30, maybe 40. It depended on which division. There were six divisions. We reorganized after Andy Goodpaster came in; he and I put our heads together and decided external conditions demanded a reorganization of the place. After that, as I recall, there were four divisions, a cost analysis group, and the computer group that was responsible for operating our large mainframe computer.

There was also a lot of Congressional pressure on FCRCs, that later came to be called FFRDCs, Federally Funded R&D Centers. Those were IDA, Center for Naval Analyses (CNA), Research Analysis Corporation (RAC), MITRE, Analytic Services (ANSER), and the Aerospace Corporation. This pressure had come about because Congress was unhappy with some of the FCRC behaviors – because as they said, ''You people want to act as though you have a lot of profit to put into these things when we're protecting you and guaranteeing your income.'' There were two particular examples that they kept coming back to. One was Ivan Getting's boat. Ivan Getting was President of Aerospace Corporation. When he moved from the East Coast to take over that presidency, he had a large boat to ship between the coasts, and shipping it cost about $3,000, which he charged to overhead. The other example was that IDA invited Elliott Montroll, who was at the IBM Thomas J. Watson Research Center in Yorktown Heights, New York at that time, to be Vice President for Research. Montroll was very well-known for his operations research work on ground traffic control, and things like that. IDA had helped him sell his house and buy a new house in Chevy Chase, Maryland. At that time, it was an expensive house. I think it cost about $200,000 or $250,000, so that would be an order of magnitude higher in cost now.

Congress said, ''You guys can't do this. You're a protected organization and you can't act as though you're profit-making.'' As part of my duties I always had to help DDR&E justify our existence. This led to what came to be what I called, the Gabriel File. I called it that because we used to do something like that in ARPA when I was there. The name was coined because we were like the angel, Gabriel, blowing our own horn. It was our brag sheet because we didn't have a bottom line to show, so we had to have some way of showing that our organization was doing some good.

"Was our work being used?" was the issue for evaluation. The criteria were that we needed independent evidence of utilization. For example, if we could find a Congressional Report or similar, that said, "IDA recommends the following changes in the Airborne Warning and Control System (AWACS) and the Air Force needs to implement them" or some such thing, and then we could show that the Air Force did it, that could be a claim of influence by our work. That indicator applied to about 30 percent of the work that we did. It was up to the divisions to gather the data, project by project, and show us at the end of each year whether their work was being used and how it was being used.

I also introduced the idea of what came to be called Psi graphs. The division directors used the play on words between Psi and Sy, somewhat in derision, because these graphs gave them a headache every month. IDA's divisions had become rather lax about the money and on-time performance of tasks and this was causing management problems with our sponsors in ODDR&E. Each month I wanted each project to plot money versus time on a linear graph, and I wanted one sentence on the status as well as the percentage complete so that I could track it. If money spent departed from the straight-line graph or there wasn't progress and they had spent money, then I could raise the question "What's happening?" It was a very useful management tool.

Also, we found working with Lieutenant General Glenn Kent, who was Director of WSEG, an altogether interesting, strengthening, and searing experience. The staff never got used to his style. He'd get hung up on a word. For example, "analysis" instead of "evaluation." Someone briefing him on a WSEG project would say evaluation and he'd say, "No. You should have said analysis." Or in a briefing, graphs should be plotted one way rather than another. I kept trying to tell them, "Grant him the point," but no, they kept arguing. These reviews of WSEG projects would take forever; in retrospect I can only call those reviews nit-picky. But, as I said, it was a style, and it had a purpose. It was his way of getting the analysts to think carefully about what they were saying, and to avoid sloppy or confusing language when talking to the DoD.

A lot of time and adrenaline was spent at IDA discussing how to deal with General Kent. I noticed that this was discussed by General Larry Welch in his oral history. What it amounted to was that Kent's style stressed the importance of how language is used in setting up and conveying analytical results. That really was quite important. But at the time, the frustration of arguing with him overrode that lesson for the IDA staff. I never could get it across to them.

I want to comment on the value of WSEG. We had a division set up especially to work with them, which meant that projects were worked on jointly in an integrated way by military and civilian analysts. That meant the military point of view was deeply embedded within the project and the military people absorbed the analytical point of view. It was a unique product, and it's never been duplicated anywhere. It was really too bad that WSEG was disbanded in the late 1970s for many bureaucratic reasons within the Joint Chiefs of Staff (JCS). I think something important was lost in their loss.

The last management thing I want to comment on was General Goodpaster's definition of "strategy." He said,"A strategy is defined by three questions: "What do you want to do? How do you want to do it? With what?" Period. I have always thought that was elegant, simple, and it applies to any situation. I think of it in terms currently used, of the so-called "surge strategy" in Iraq. People call that a strategy, but it really deals only with the "with what." Nobody ever really articulated "what do you want to do", or "how do you want to do it". All they talk about is the "with what". I noticed that's going on in this election campaign (2008) and the arguments about how the war is going. [And, post-script – it seems to be going on in part in the Obama Administration's review of Afghan War policy. The main arguments are about whether to add more troops, and people in Congress and the media say "we need a strategy first," when they mean we need to resolve the questions of objectives and how to achieve them. But I have the feeling, from comments in the media and by the President himself, that the President is indeed following the Goodpaster definition in working out a war strategy for Afghanistan.]

Now the technical highlights. After I talk about several aspects of IDA's work, I'll talk about personal work I was doing on various advisory groups. Remember, too, that IDA had several dozen projects going at any one time, so what I will mention includes a selection of memorable events and outcomes from a long list of work that was always under way.

First, modeling and simulation. Jerry Bracken and Bruce Anderson at IDA built the TACWAR [Tactical Warfare] simulation model. It gradually evolved and became a standard simulation that I think is still used in analyses of conventional warfare. They did a magnificent job in getting that model on line and in use.

There was, however, great reluctance on the part of the staff to test the models and simulations against real life. For example, I was after them to test it against the Israeli Sinai campaigns because those were classic campaigns on which there was a lot of data, and TACWAR exactly fit that kind of conflict - two armies facing off. There was a forward line of own forces and breakthroughs of the enemy lines, just exactly what TACWAR was designed to look for.

We may have done such a test, after all. I can't remember. But it was very difficult to get the simulation model builders to think along those lines – basically, they didn't want to, and I never figured out the reason why. Maybe they were afraid the model would show up as not being quite adequate.

At the same time, we were asked by the Joint Chiefs to evaluate the performance of U.S. systems in foreign military operations. That led us to gather histories of conflicts that had just passed, like some of the Sinai campaigns and the Falklands War. Just to comment on some of the main results of interest in these evaluations: We found that tank kills that were claimed for infantry weapons and air-to-ground weapons in desert war had been greatly exaggerated. If someone shot at a tank, he said he killed it. But the data from film showed, ''No, it wasn't killed – it kept on maneuvering,'' and things like that.

In the Falklands War, it was found that tracers in anti-aircraft fire were very important in keeping aircraft away from the ship targets. It spoiled the pilots' aim by making them aware that they were being shot at and knowing where the shots were headed. Also, command and control in the Falklands War was exercised by Margaret Thatcher personally. The fact of communications between the fleet in the South Atlantic and London led to problems, because when ships' communications antennas were turned towards London for communication, they were losing radar visibility in the direction the threat was coming from. So that was an important factor in ship losses in that conflict. That conflict also showed the importance of keeping intermediate bases for long-distance deployment. For example, for the Brits to get to the Falklands, Ascension Island was absolutely essential. If they didn't have that base, they couldn't have gotten there.

In another area IDA had worked with WSEG and the Services on air combat testing. There were two programs, Air Intercept Missile Evaluation (AIMVAL) and Air Combat Evaluation (ACEVAL), which, respectively, tested different angles for off bore-sight air-to-air missile guidance, and multi-formation combat from one-on-one to four-on-four in different combinations.

Because the Navy and USAF both participated, it was F-14s and F-15s against F-5s, which were used to simulate the MIG-21 threat aircraft. The Air Force and the Navy simply didn't want to accept the low exchange ratios they were getting because of the tactics they were using. I don't want to describe the tactics here because I'm not sure whether both the U.S. tactics and the tactics that the threat used have been declassified yet.

But what the outcomes of the tests showed was the importance of having off-boresight air-to-air missiles. They also showed that maneuvering and guns remained important for close-in engagements. The Air Force had been wanting to take the guns off the aircraft, and that would have been a great mistake.

BOB SHELDON: Let me back up to your historical analyses. Did you compare notes with the Trevor Dupuy Institute when they were gathering similar data for the Army?

SEYMOUR DEITCHMAN: Yes. In fact, at the time, Trevor was working for IDA, I believe, in one of the divisions, or at least he had worked for IDA just before that. But anyway, we maintained a connection with him. And if anything, he helped contribute to those histories.

There was also an interesting lesson on being cautious in making judgments about new systems. There was an IDA study for DoD of a new USAF proposal for what they called NAVSTAR at the time. It's what became GPS. We found – the report said – that other methods of navigation, like Loran C and D, already existed that were just as accurate as NAVSTAR was then projected to be. Therefore, we didn't need this expensive new system. That was the conclusion of the report.

What we didn't foresee were the potential other uses for the system, like weapon guidance, and the drastically lowered cost of GPS as it proliferated into the civilian economy. Not only were there more military applications than we had foreseen, but extensive civilian use as well, as in air traffic control and civilian aviation navigation, as well as the personal uses that are now appearing, as in automobile navigation systems. We were just dead wrong in that because we couldn't see far enough ahead and didn't see enough of the potential other uses for that kind of navigation system. That just says, be cautious when you make judgments about brand-new system concepts; caveat your results.

I retired in 1988 from the vice presidency of IDA. I had to retire, because under ERISA (the Employee Retirement Income Security Act) a corporate officer could be required to retire even though employees generally couldn't. But I was ready, and the reason was that I was finding that my memory wasn't as good as I wanted it to be for effective management. In earlier years I could retain in my head, conversations from months ago and would know exactly who said what, and I would know my calendar for a month in advance or more. As I was getting older (and by then I had reached age 65), I couldn't do that any more. I noticed that the division directors would often say to me ''but you said ….'' and I couldn't remember what I said to confirm in my own mind that they were right. So no matter what ERISA said, I had felt it was time for me to retire from management.

I did stay on at IDA as part-time researcher, and I'll come back to some of what I did then. But first I want to mention a backhanded compliment that I got for IDA. It was a compliment via a complaint, and it just shows the integrity of the organization and how we had been running it, and that's why I want to mention it. Chuck Bernard was then the Deputy Director for DoD Tactical Warfare Programs (DDTWP) for land warfare programs. He was the brother-in-law of Tony Battista, who happened to be a much-feared staff chief of the House Armed Services Committee during the 1980s. He didn't mind going into the Defense budget and changing it unilaterally, line-by-line, and then writing a report saying, ''You must do this,'' and the committee backed him up. (I don't remember who was chair of the committee at the time.) As an example of what had happened in that kind of environment – Jim Wade and Chuck Bernard came into ODDR&E together. Jim Wade was the principal Deputy DDR&E. The two of them put their heads together and overturned NATO's whole air defense structure just after it had been finally decided after years of negotiation among the nations.

Part of that scheme was a line of defenses along the eastern border. Chuck and Jim developed a scheme that involved the Missile Experimental (MX) Intercontinental Ballistic Missile (ICBM) first stage, which was supposed to be loaded with runway-busting sub-munitions, to be launched from West Germany against the Soviet Bloc airfields in the east if they launched the expected air attack on NATO as their opening gambit. That MX launch would break up the airfields and the Soviet Air Force would be put out of commission after its initial sortie because the missile would be launched during their attack and they would have no bases to come back to. In addition, instead of Hawk, which had finally been decided was to be the main air defense missile along NATO's eastern defense line, Wade and Bernard wanted to introduce a ground-based version of AMRAAM, the Advanced Medium-Range Air-to-Air Missile, which was just being introduced at the time but is now standard in the USAF.

I thought it was inappropriate to introduce that scheme just after NATO had spent decades agreeing on the existing air defense plan. But, much to my horror, they asked IDA to look at the cost effectiveness of their proposed system. And we did. Among many conclusions, one was that the Germans would never allow an MX first stage to be launched from their territory, because it would look like a branch of the U.S. strategic forces. Another was that cruise missiles with runway-busting sub-munitions would be more cost effective than the MX first stage in taking out the East German airfields anyway. We stuck to that and had many arguments with Chuck Bernard about it.

That's background. When I was leaving the vice presidency in 1988, I was going around saying goodbye to all the people we'd worked with, and I talked to Chuck. He said, ''You guys drive me crazy. I could go to any beltway bandit and tell them what I wanted in a study and they'd come up with the answer I was looking for. And you blank-blank-blanks, I couldn't budge you an inch once you reached your conclusions.'' From the way he said it, I think he secretly harbored a great admiration for that quality. I took that to be one of the greatest compliments IDA had ever had.

BOB SHELDON: When was that MX analysis?

SEYMOUR DEITCHMAN: They were in office about four years. I retired in 1988 so it was like the 1984 timeframe.

Now I'll come to my work outside of IDA.

Dr. Eb Rechtin, who had been Director of ARPA after Charlie Herzfeld, eventually became Principal Deputy DDR&E. After Rechtin moved up to ODDR&E he was the U.S. member of the NATO Defense Research Group, which essentially tried to coordinate R&D among all the NATO nations.

About 1970, he asked me to help start a NATO panel on operations research under the DRG (Defence Research Group of NATO). The first question it was going to examine was the perennial one during the Cold War: Can NATO technological superiority overcome Warsaw Pact mass? The answer was, ''Yes, but …'' We got a lot of interesting results along the way that I'll comment on now.

But let me first follow the pattern I've established in this oral history – I'll deal with the bureaucratics first. The first thing to talk about, vis-à-vis NATO, is that it is a bureaucracy to end all bureaucracies. I'm sure it still is, but the point is there were people there who had nothing to do but be bureaucratic. It was very difficult to get nations to share information, so you had working groups galore and they'd share little bits at a time and they'd meet regularly. In a sense, it was a way of keeping the alliance together, but it also turned into a way of stopping anything from happening.

BOB SHELDON: Did you work on a panel called AGARD (NATO Advisory Group for Aeronautical Research and Development)?

SEYMOUR DEITCHMAN: Yes. I'm going to comment on that later. I went to the first meeting of a new panel called DRG Panel 7, on the applications of what NATO called operational research. The staff director of the panel, a French official by the name of Pierre Naslin, with whom I became quite good friends, said, ''Okay. Let's sit down and set up your working groups, etcetera.'' I declined, and he was shocked.

I had the Defense Science Board (DSB) model in mind and I said, ''We should just do studies with the people at hand on the Panel, plus others who the people at hand might bring in. And when the study's finished, we disband and we go on to the next study.'' He said that was unique in NATO experience. He'd never heard of that approach, but he went along with it. He later agreed with me that it was an effective way to work.

BOB SHELDON: Why was that more effective than the working group model?

SEYMOUR DEITCHMAN: The working groups tended to get set in their ways and meet for the sake of meeting. They'd have an established agenda and they would come back and keep working on that same agenda. They might not ever finish a study because they would just go on forever working on the same question. Every time there was a meeting, that study would come up and they'd talk about it some more and then go away. In the way I had proposed, we would start a study, we'd finish it, write our report, and go onto the next one, which might very well involve different people. I think it was a lot more effective, and certainly more responsive to the questions asked.

BOB SHELDON: Did you have to travel to Brussels for most of your NATO meetings?

SEYMOUR DEITCHMAN: All of them, except for a couple we had in places like the SHAPE (Supreme Headquarters Allied Powers in Europe) Technical Center (STC) in The Hague, Netherlands. Most of the work on these studies was done by the U.S. and U.K. It involved myself and Wilbur Payne, who was a famous U.S. name in operations research. He was then Deputy Under Secretary of the Army for Operations Research, as a matter of fact (and while we were working together he left that spot and became head of analysis at the Army's White Sands Proving Ground) and Douglas Andrews of DOAE for the U.K. DOAE was Britain's Defense Operational Analysis Establishment, which was privatized and renamed under Margaret Thatcher. I don't remember the name of the new setup.

There were some inputs by the STC, and occasionally by Germany and their use of the IABG (Industrieanlagen-Betriebsgesellschaft GmbH). IAGB was Germany's analog of IDA. Panel 7 met twice a year and reviewed inputs and results, so it was a group effort because we passed the reports around via the NATO system and then received and incorporated everyone's comments at the semiannual meetings.

Also, we drew on work that was done in our home countries and organizations, and used resources like our modeling and simulation capability at IDA, when corporate money could be set aside as internal research money for such purposes. Before I discuss some substantive results, let me comment on how I like to work on such problems. I like to use simple models based on simple equations or sums and differences, like combining sorties per day and weapons per sortie and CEP variations as parameters; things I could put on an Excel spreadsheet and treat as parametric studies, because I found that was the best way of learning about important variables.

While at IDA, I could steal time working with TACWAR. I always had a staff assistant who had helped gather the data for the psi graphs and analyses of IDA's program performance. First it was Anita Shriner, who eventually moved to China Lake, and then Evelynn Putnam, who was fluent in Russian and had a mathematics background. They welcomed the break away from the administrative kinds of work.

The U.S. and U.K. studies agreed on the relative cost-effectiveness of the different kinds of weapons, but TACWAR showed that much more force would be needed to stop the canonical Warsaw Pact assault than the U.K.'s battle group model, so the issue became ''Why?'' We spent a day with Wilbur Payne at his White Sands headquarters, with me on the telephone to Washington and Doug on the phone to London, having our respective modelers running the simulation models as we changed parameters in real time to find out what was happening.

Finally we found that the answer was in the implicit assumptions about how we use the Army. The Brits had the new Chobham armor, which is essentially a layered armor and very effective. They would dig their tanks into hull defilade, so nothing was showing but the top of the turret and the gun. In their battle group model, the attacking Soviet forces would come through and the virtually invulnerable tanks would just keep taking pot shots at them. That was the British model. In the U.S. model, the armor is held back in reserve, and when the enemy starts coming through, the reserve armor counterattacks, so that the real parameter of concern got to be how long was the exposure of our tanks to enemy fire? It turned out in the models that the Brits were essentially having a turkey shoot, so their forces were much more cost effective than ours were, and that's why it required so much more force on our part, because we would be losing tanks depending on the percent of time the tanks were exposed to enemy fire. Subsequently, we spent a lot of time trying to track back through field exercises to see what percent of the time tanks actually have been exposed to enemy fire. We never got any good results. Approximately 25 percent of the time was the best we could arrive at, but that made a big difference in the results from our models.

What this exercise really showed was the importance of running more than one model, because the differences between their results help to bring out the models' implicit assumptions and their implications for the results.

I got started, also in this same set of studies, at looking at TACAIR (Tactical Airpower) effectiveness, accuracy and costs. That was the beginning of a long period of study that I did in that area that I'll summarize briefly here to maintain the continuity of the narrative. I carried these studies on at IDA under various tasks, past the time I was in the NATO DRG Panel 7.

The results of the Panel 7 studies and several analyses I did afterwards, even through the period after I had retired from the vice presidency of IDA, showed that it is more important to work on key elements of what we now call precision engagement, including accurate target acquisition and precision weapon system guidance. Doing that is more cost-effective than working on the design of the weapons themselves, and more important even than advancing the platforms, in the long run.

It showed that the driving cost factor was aircraft attrition because when you lose an aircraft, it's a huge cost that overwhelms the added cost of precision target acquisition and weaponry. Precision weapons and standoff weapons that could strike with precision from standoff distances that greatly reduced aircraft attrition were much more cost-effective than improved bombing systems for close-in attack, or even improving the attack capability of the aircraft themselves. It also showed the importance of using precision weaponry and standoff to take out the defenses before attacking targets of high tactical or strategic value, and to allow close air support aircraft to close with the enemy in direct support of ground forces.

The Air Force was resistant to the idea because they were focused on weapon costs. We'd say, ''Well, this guided weapon is going to make your total system much more cost-effective.'' And they'd say, ''I can't afford that laser-guided bomb. It costs too much,'' Even though that bomb would save aircraft by requiring fewer sorties to kill a target, thus reducing the huge cost factor of aircraft attrition. They were comparing the $40,000 for a laser-guided bomb with the $3,000 for a dumb bomb. But that was not the comparison to make. I never could persuade them until they got the GPS-guided Joint Direct Attack Munition (JDAM), and that brought the cost of a guided bomb down enough that they were willing now to take on precision weaponry.

There's an interesting case study. I commented earlier that I wasn't sure that studies and advancing technology affect policy. This is a clear case where the studies didn't have a lot of effect but the technological advance did, because that advance made the guided weapons cheap enough so that the Air Force would buy them in quantity. They responded not really to logical analysis, but to the advance of technology.

Another result of the Panel 7 work was a fallout from the work on the, ''Can technology beat mass?'' question. We looked at implementation costs of all measures, including the advanced armor, and improved TACAIR weapons and aircraft, and concluded that the results we foresaw could be achieved with about a three percent increase in NATO defense budgets. That was in one of our reports.

I couldn't trace it directly, but to me it was an interesting coincidence that when Jimmy Carter became President and Harold Brown was his Secretary of Defense, a few months later Carter called for a three percent increase in the NATO defense budgets. Bob Komer was Assistant Secretary for International Security Affairs at that time, and I knew him quite well from our interactions when he headed the pacification program in Vietnam. I asked him whether the President's three percent budget proposal was our Panel 7 three percent, and whether the Panel 7 report was where they got it from. He said, ''Sy, you could never prove it, but probably.'' So there's an example in the other direction.

Also, closely allied with the Panel 7 work, Al Flax asked me to complete an AGARD report, looking ahead 20 years to where aerospace technology was going. The study had been done but the leader dropped out due to illness, and Al Flax was the U.S. member and chair of AGARD at the time. As an incidental observation, that was the position Von Karman, the father of modern aeronautical engineering, held first, so it was a position of great honor. Al certainly deserved it for his professional contribution to three-dimensional flutter theory and a lot of other theoretical aspects of aeroelasticity.

The AGARD panel had looked at space and aeronautical systems, and the panel's results led to the clear conclusion – and that's the way I wrote the report – that the future of aviation and space lay in information technology: target acquisition, weapon guidance, command and control, space communications, and observation from space. But it also pointed to where aeronautics was going, including unmanned aircraft.

BOB SHELDON: When did you write that?

SEYMOUR DEITCHMAN: I was still dealing with Panel 7, so it probably was before Andy Goodpaster became President of IDA, around 1980.

It also showed there was no easy way to overcome the sonic speed barrier for commercial aircraft, which echoed the results of the Curtis Study that I mentioned earlier.

Finally, in connection with NATO-related work, I was asked in 1982 and 1983, respectively, to give the keynote addresses at a DRG Symposium on Operations Research and at the 25th Anniversary celebration of TTCP (the Technical Cooperation Program, which included U.S., U.K., Australia, New Zealand.). In both cases, using examples appropriate to each occasion, I stressed the issues of why implementation of good technological ideas and programs always takes longer and costs more than originally estimated and promised.

The reasons were the unknown-unknowns, ''unk-unks,'' that show up as you go from concept to concrete system, especially in system integration, where the parts probably have never been joined together and you run into all kinds of compatibility problems and second-order effects. And there are always institutional constraints that are rarely accounted for in the cost and schedule projections: where the budget will be, because that's a political decision; and the conservatism of the military, which in many cases is justified because they have to win wars and especially if you consider that you're going to be on the defensive, they want the tried and true, not the untested and uncertain. There was also legislative competition in the budget for other things, and you would never know how that would turn out. You could lay all this out and show why everything took longer than you wanted, cost more than you wanted, and sometimes didn't get implemented.

That NATO-related work was one whole area of my non-IDA endeavor. In the late 1980s, I was asked to be on the Strategic Defense Initiative (SDI) Scientific Advisory Panel –I don't remember the circumstances that led to that invitation, but it emerged from my being VP of IDA, and it emerged from one of the Beltway think tanks that was helping the SDI office with technical analyses. Basically, the SDI project leaders would brief the panel on ideas and progress, and we would comment. They could use the advice, or not, as is always the case in such things. One idea that was floated, called for putting hundreds (as I remember it) of guided interceptor warhead stages in an orbital belt around the earth, to be operated in conjunction with a satellite launch warning system. Then the appropriate interceptor in the belt could be called down to intercept the launch at boost phase. The issues, of course, were, first, that it would be difficult to exercise command and control over the entire system, and worse, someone else might capture it and bring the interceptors onto friendly targets from orbit, and then what would we do? At any rate, there was a lot of argument about that, and thank goodness, as far as I know the idea was abandoned. Overall, my recollection is that in the offense-defense equation the problem of decoys was the driving issue. Whatever scheme anyone thought about to pick the warheads out from a cloud of warheads and decoys, the offense could always find a way around it. I think that remains a critical issue, and when I heard the Bush administration was going to deploy the system I wondered whether the issue had been resolved. From reading *Aviation Week*, which is my only source on the SDI, I think not yet. So, why do we deploy? Well, I'm not in ''the system'' any more, so I can only wonder.

That lasted only a couple of years, right across the boundary between when I retired from the vice presidency and went part time. Another area I got involved in, and this one has lasted over the long term, was that in 1982, Eb Rechtin invited me to join the Naval Studies Board (NSB) of the National Academy of Sciences. He was then Chairman of the NSB. As an aside, Eb, who died a couple of years ago, was responsible for getting me involved in the two fascinating long-term extracurricular activities that most enabled me to continue my professional contributions along with the management activities I had undertaken at IDA.

I'm still affiliated with the NSB, but on a much-reduced scale. Mainly, I now act as internal reviewer of selected and important panel reports before they go out to the wolfpack of external reviewers, whose critiques the study group then has to take account of in order to get the reports published by the Academy. (Most of the time, they do make the reports better.)

From the start, however, I contributed to many studies as the integrator and overall report writer, as well as making technical contributions in my areas of technical competence. Many were multi-volume studies, in which case I would write the summary report, usually called the "Overview" volume. At this point I ought to comment that I have never believed it worth being on a panel unless I could make a significant contribution. I enjoy writing, and I can take a broad view of systems, so all that turned out to be helpful. I enjoyed exercising those talents, if you want to call them that.

With the NSB work and the IDA work, I had to drop out of the NATO activities. I had made my contribution, and in any case the experience stood me in good stead for future efforts.

Now, a quick summary of a selection from many projects I worked on with the NSB, not necessarily in chronological order.

The first study I worked on was right after I joined, in about 1982 – a study of the future of naval aviation. We decided, in what turned out to be a continuing theme, that the future of naval aviation would be determined by information technology, including target acquisition, communication, navigation, weapon guidance, rather than by aeronautical advances, although the latter would be helpful (stealth technology hadn't yet come to the fore, but clearly, that would have been an exception – if we could talk about it at that time.) So for me, this reinforced the conclusions reached in the NATO work.

But the reader shouldn't draw the conclusion that I, alone, was responsible for those results. This was the considered opinion of a group of key people who were expert in all the areas. For example, the study was chaired by Bob Frosch, who had been Assistant Secretary of the Navy for R&D. And it included Ivan Getting, who had been the President of Aerospace Corporation and was famous for inventing the first radar-controlled naval antiaircraft gun during World War II, and many others of that caliber. I was reflecting their results, and I agreed with them and found them interesting.

And I mustn't fail to mention Lee Hunt, who was the Director of the NSB at the time. Lee helped the studies happen by assembling the distinguished participants, working out the task statements, managing the budget, and liaising with the Navy in many ways. In essence, as the National Academies' Board Director via the National Research Council (NRC), Lee was the impresario who kept the program running. The Board Chairman, usually but not necessarily a member of the National Academies, presided over the meetings, of course, and was generally responsible for overseeing the quality of the technical outputs and reports of the Board panels and studies, in accordance with the Academies' review procedures and standards. In simple terms to explain a complicated management system, Lee was the NRC's internal manager and the Chairman was the manager of the activities of the outside experts drawn from Industry and Academia.

Lee and I are still fast friends. [Sadly, as I review the final draft of this history in mid-October, 2009, I must report that Lee died of complications from diabetes just a few days ago – a great loss to the country and to his many friends and of course, his family.]

BOB SHELDON: How many people served on the NSB?

SEYMOUR DEITCHMAN: The NSB was usually about 30 people and still is. According to the National Academy of Sciences' rules, members are supposed to serve no more than two 3-year terms, but in a few cases the Board has found ways to keep some of us associated with it in various capacities for longer times. For example, I'm now called a senior advisor, and work with the Board as an unpaid consultant. They keep my clearance and invite me to their meetings, which I appreciate very much. In general, study panels tend to draw in additional qualified people from the outside, and, as I'll note in a moment, can become as large as 100 or so people.

We did two studies, ten years apart, of where the Navy would be going in the future (more accurately, where technology would take the Navy in the future). The first was called Navy 21, and that was done in the mid-1980s. The next one was TFNF, Technology for Future Naval Forces, and that was in the mid-1990s. These were very large studies, on the order of 100 people in each. The TFNF study led to a nine-volume report, and I wrote the Overview volume summing up the results of all the separate studies, of air warfare, surface warfare, undersea warfare, information warfare and command and control (actually, command, control, communication, computers, intelligence, surveillance, and reconnaissance [C4ISR]). And, there was a volume on modeling.

In the first study, we anticipated the importance of information technology, target acquisition, and also undersea warfare, and steps that have to be taken to mitigate the vulnerability of carriers. Early-on at the beginning of this narrative I mentioned the RASCAL missile that I had worked on at Bell Aircraft that showed up, at least in performance, as the Soviet AS4. This could easily home in on that great corner reflector that was the island on the deck of the carrier. That was a continuing problem we had to deal with in the study.

In the second study we looked at the possibility of using tactical ballistic missiles in place of ship-borne artillery, and thought that they could be made more cost-effective with the vertical launch bay that was now being installed on ships to launch anti-aircraft missiles. But the Navy was hooked on the Extended Range Guided Missile (ERGM), which was in work and intended to be fired from the guns. When we looked at the magazine size and the rates of fire of the two modes, we found the vertical launch bay with ballistic missiles would be far more cost effective, but we could never persuade the Navy of that.

In another interesting exchange, we said the Navy should have a high-altitude, long-endurance aircraft that could orbit over the fleet so that the fleet (or, more accurately, a task group) would have the analog of a continuously available observation satellite and communication satellite. One of the carrier admirals, when we briefed that, said, ‘‘That's never going to happen, because a system like that would have to be joint system and the Navy won't be able to own it to control it, so we don't want it.'' But now they're working on integrating Global Hawk into the fleet.

Admiral Pilling, who was then the Vice Chief of Naval Operations (VCNO), said, after we briefed him on TFNF, ‘‘You've showed the Navy its future,'' which we took as a great compliment.

There was a big issue in both studies about modeling and simulation. A modeler by the name of Tim Horrigan had promulgated what he called configural theory. His main point was that in running models, and particularly naval mine warfare models, the models weren't designed to remove the ships and the mines that were sunk or exploded, respectively, from the sequential event cycles in the models. Thus the results weren't an accurate representation of the true outcome of such an exchange. That had been a huge fight in the Navy for many years. So, modeling at many command levels from the narrow tactical ones through the major force-on-force models became one volume of the report. We thought we'd put it to bed, but in talking with friends, I hear that it's still being argued.

We did a study called Carrier 21, looking at future aircraft carriers and where technology would take carrier design, and concluded that the best course for the Navy would be to stay with the Nimitz Class carriers. That was not popular with many members of the panel, who wanted something big, new, and different. But we found that they wanted something big, new, and different because aircraft were projected to grow in size. That never happened. The proposed A-12 attack aircraft, which was the driver, was cancelled because it was overambitious in design and therefore overly costly. It turned out also that the dry-docks on both coasts could only take a Nimitz Class carrier as their maximum size vessel for construction or overhaul. If there were a bigger carrier, the whole infrastructure that deals with the carriers would have to be rebuilt.

Edward Teller had planted the idea with Congress of creating a floating island built on the same technology, with vertical spar buoys, that oil platforms are based on. The island would be built in sections that would be hooked together, and the Navy would float this island at a few knots to every place where a base was needed, like the Arabian Sea, where you didn't want to put an airbase on land for fear of violating sovereignty. We put that idea to rest by showing how much it would truly cost, and how difficult it would be to operate, and how putting the sections together and trying to float them around the world would be rather more than anybody understood how to do at the time. It would be just as vulnerable to attack as any land base, and so would need the same elaborate defenses that the land base would need – that hadn't been accounted for in the original cost estimates, designed to sell the idea.

BOB SHELDON: Is that similar to the current sea-basing concept?

SEYMOUR DEITCHMAN: No. That's different. I'm going to get to that. But first, I'll note, we did a command and control study that basically extended our appreciation of the importance of space systems. In fact, I should say that Ivan Getting headed a space panel as a standing sub-panel of the NSB, and that panel made many useful contributions in alerting the Navy to the importance of space systems for the Navy and urging them to exercise their representation on the Air Force planning committees. The Air Force has been the lead service for DoD space systems.

I also was vice chair of a mine warfare study, which was the third in a series that the NSB had carried out, not counting the mine warfare contribution of the undersea warfare sub-panel of the TFNF study. There were many warnings to the Navy about the potential for mine warfare to put the Navy out of business in critical theaters of warfare. In fact, we showed that during World War II, U.S. mining of Japan's Inland Seas was on the verge of putting the Japanese out of business even before the atomic bomb was dropped on them. We found significant gaps in the Navy's mine warfare programs, on the offensive (mining) and defensive (mine sweeping) sides. We recommended ways to fill those gaps which would have been costly and which would have required more specialization in the field than the Navy has been comfortable with.

But my observation is– and it's my purely personal interpretation of their behavior – that, at least today, the Navy views mine warfare as passive and defensive, and therefore, not in keeping with the ethos of offensive warfare. It could be a problem for us, because instead of specialized mine warfare systems, the Navy is planning a defensive mine warfare module for the new littoral combat ship, in addition to some helicopter-based anti-mine systems currently in inventory or in work. But we could face serious mine warfare threats from the outside. For example, the Iranians have threatened to mine the Straits of Hormuz. Under fire, it would be very difficult to clear those mines. And it's possible that someone like Al Qaeda could arrange for a mine or two to be dropped in, for example, New York harbor. Imagine the havoc that would cause.

Then, we worked on a regional conflict study that dealt mainly with the Marines. Again, it dealt with precision weaponry and precision engagement. Those were recurring themes. But we also stressed the importance of combat in built-up areas. This was about the time of the Black Panthers. You couldn't use the term ''urban warfare'' because it sounded like you were going to be fighting the Black Panthers. So I invented the term – I'm going to take credit for that one –of ''combat in built-up areas.'' And I note that that has taken hold in all the Services. I've seen it very recently in Marine technical manuals.

General Krulak, who was then Commandant of the Marine Corps, read our report on this study– (there were many chapters), and said, ''The only important chapter worth reading here is the one on fighting in built-up areas.'' That's the Marine orientation, and we've seen that in Baghdad, among other places.

I also worked – in the late-1990s – on a study of experimentation for the Navy, in which we reinforced the idea that was growing in the DoD, of ''spiral development.'' You do some development on a system; you put it in the field and observe how it works; then you develop further and you keep spiraling up to an advanced system. We showed the Navy how to integrate experimentation into the spirals so they could determine where to go with the next turn of the spiral.

Another study was Post-Cold War Conflict Deterrence, which the Navy asked for because they were thinking about the future roles of the SSBN (Strategic Submarine Ballistic

Nuclear) force after the Cold War ended. This was done in the mid-1990s. I took the responsibility for asking General Goodpaster, who was then Chairman of the Atlantic Council, whether he would chair that study, and he agreed and asked me to be his deputy on the study. Among others, we had many of the people on the panel who had crafted the Cold War deterrence theory and policy, like Tom Schelling, Wolfgang Panofsky, Paul Nitze, Dick Garwin, Glenn Kent, and Paul Wolfowitz (who later became DepSecDef). Among other things, we propounded the idea that nuclear weapons are a different order of magnitude of weapon from other weapons of mass destruction, because unlike the others, say, chemical or biological, they deliver instant destructive power and are much harder to counter because the time to do so is expected to be so short.

The study concluded that nukes would be useful only to deter the use of nukes by others, so that we could reduce our nuclear arsenal significantly. Wolfowitz, who had a hand in undertaking the Iraq War and subsequently in expressing concern over Iran and the North Korean nuclear programs, was present. During his term in office none of his actions reflected anything he might have taken away from the study. Subsequently, we – that is, the country – also seem to have forgotten the deterrence idea completely. I want to come back to that afterwards.

Then in later studies, I backed off from being the chief integrator. I got to the point where I said to myself, ‘‘Been there, done that,’’ because that integration involves a tremendous amount of argument with people on the panel who have their own ideas about what should be said and how you should say it. After twenty or so years of that kind of negotiation it begins to wear, and I think you lose your effectiveness. I decided I didn’t want to do that any more.

I worked on a brief study the Board did of sea-basing, and helped to write that report. The idea was that if we could base a land and air combat force at sea we wouldn’t have to depend on foreign bases, and all the issues of sovereignty and alliance influence on how we use the base. The Navy hoped to be able to base a whole expeditionary force at sea. Well, it turned out not to be that easy. They would need a new kind of ship, which would look like an aircraft carrier, but no island on the deck because you would have to land an aircraft about the size of a C-130J to go to shore with air cargo. And underneath that flight deck, it would look like a conventional commercial warehouse where you could pull out containers from wherever they happen to be with the materiel you needed next, and run them up to the deck and get them on the airplanes. It was a whole new kind of ship which would be quite expensive. Then there was the issue of offloading extremely heavy cargo like tanks and trucks that would overload an air delivery system, onto lighters at sea, where there would likely be a practical limit of being able to do it in sea state 3 (waves 2-5 feet). And, it might involve enforced landing against opposition, and then having to defend the landing area as a base ashore, anyway.

We calculated that at best, with the limitations on sea-basing that our study brought out, the Navy might be able to support a brigade on shore, so they could use sea-basing if they could win the war with a brigade. Maybe we could win some wars with only a brigade. It depends on how fast the country can react to a building situation.

The NSB also worked on a study of network-centric warfare, to which I contributed peripherally, where the idea that it’s not the weapons and platforms that should be the center of attention, but it’s the whole combat network including C4ISR, logistics, and all the other activities that go into a military campaign, and the weapons and platforms are just part of the network. That concept was proposed by Vice Admiral Art Cebrowski, who died just a few years ago, and it has taken hold strongly in the whole Defense establishment.

BOB SHELDON: Let me back up to that sea-basing study. Do you recall the important variables you worked on in this sea-basing study and its effectiveness?

SEYMOUR DEITCHMAN: Yes. We looked at the various cargo needs, the largest being Class 3 (fuel) and Class 5 (weapons and ammunition); tonnages to support various size forces in different kinds of combat; necessary delivery rates; sizes of containers (the ones we looked at would be smaller than a standard commercial shipping container – 8 by 8 by 20 feet, if I recall);

speeds of ship movement; numbers of ships in the supply train; and aircraft and ship designs. It was a very thorough review, even though it was done in a three-day workshop. The people on the Panel were very knowledgeable, and the Navy and Marines were very helpful in providing the necessary data.

BOB SHELDON: What year were you working on that sea-basing study?

SEYMOUR DEITCHMAN: The report was published in 2005, so the work was probably done in 2004.

That was the last study I worked on formally. Now, as I indicated earlier, I'm in the mode of being an internal reviewer. There were a couple of reports worth mentioning. I'm not sure whether they've been published in final form yet. One was on the Navy's role in the Global War on Terror, and the other is what Admiral Mullen, when he was CNO, initially called the Thousand-Ship Navy, but which came to be called Maritime Security Partnerships. That dealt with how nations could get together to deal with things like piracy in the Straits of Malacca or, for example, that we have been seeing lately in the Gulf of Aden off the coast of Somalia.

Now let me backtrack to the period 1988 to 1996 and describe some of the things I did at IDA after retiring from the vice presidency and going part-time. There were two ongoing DSB studies being supported by IDA that I had a chance to make some contributions to. One, chaired by Mal Currie, who had been DDR&E, was on the possibilities for economic and technical cooperation between the U.S. and other Pacific Rim nations. The issue was how we could cooperate in advancing technology without giving away our technological advantages. I don't remember a lot about the study, except that a key question, as an example, was the assistance we would give to the Japanese in developing their new fighter, which is now their standard fighter and looks much like an F-16. The issue at the time was what the Panel thought the U.S. could get in return for helping Japan develop that fighter. We thought they were ahead of us in the technology of large-scale composite structures and we could gain knowledge in that area, but it turned out subsequently that they weren't that far ahead of us. But that was the exchange that was finally agreed.

The other DSB study to which I contributed had to do with assured access to space. My role in it is hazy in my memory; mainly, Bob Hermann, the chairman, had hoped I could contribute to it the way I had contributed to the NSB studies when he was NSB chairman. That turned out to be not so easy in the very different DSB environment, since the DSB members essentially like to do their own writing; they prepare the summer study briefings for the SecDef themselves, and then the staff in the Pentagon writes the report afterwards.

But that study primed me for the NASA study I'm about to describe. A task came in to IDA from NASA under a more general contract IDA had with them, on how they could assure manned access to space within their budget constraints. It dealt with the costs of a potential successor to the space shuttle and a new expendable booster, all in support of the Space Station that was just getting started. I was asked to lead that study for IDA.

I learned a lot of things about NASA's world, and one of them included how NASA planned their earth observation systems, which I found to be quite a revelation, and I still marvel at it. I don't know if they still work that way (from a few hints I've had reading *Science* and *Aviation Week* magazines, I think they still do). Namely, in DoD, if we wanted to build a system to do something, say to observe what's transpiring in the earth environment from space, we would set out the requirements for what we want, including the instruments, the "bird," the launcher, etcetera, and we would ask people for proposals to meet those requirements. NASA didn't work that way. They would tell the R&D and academic community, "We're going to have an earth observing satellite. Send us your proposals for instruments you want to put on it and we'll pick and choose among those proposals." (Of course, they didn't say it quite that way, but that was the sense of their approach).

BOB SHELDON: That is a different way of doing business.

SEYMOUR DEITCHMAN: Yes. I had trouble coming to terms with that; I still do because it says the government can't plan what it wants to do. It depends on what the university and research contractor environment antes up for you. I don't think that's a sensible way to work because you can't be sure you'll be able to get the job done.

The Challenger disaster happened while we were working on this project. An interesting thing we learned along the way was that NASA could have used kerosene-powered rocket boosters instead of the solid fuel boosters to help launch the Shuttle, which would have avoided the Challenger kind of accident. Kerosene rockets actually would have had a higher specific impulse than the solid fuel boosters they were using. They didn't want to do it, as nearly as we could tell, because the technology wasn't advanced enough and so they thought it wouldn't look right for a high-tech organization like NASA to use such retrograde technology. We also learned that the main purpose of the Space Station, as given by an early 1990s committee chaired by Norm Augustine, was to learn how humans could live in space for extended periods so they could incorporate that in the planning to go to Mars. That stated purpose of the Space Station appeared to have been lost when the cost of going to Mars became impractical, but the Space Station has continued and we've been trying to find earth-oriented uses for it ever since.

The main results of our study were that if NASA wanted to save money on the shuttle program, they shouldn't fly it as much. Much of the use was to send cargo to the space station, and they could use unmanned boosters for that. We also found that the new expendable booster they wanted to build would not be cost effective, compared with using an existing booster. Their main problem was that there was no way to reduce the cost of a man-rated booster, if they wanted to fly people up to the Station as an alternative to using the Shuttle. There are so many precautions you have to take if you're going to carry people, with redundancy, recovery and rescue preparations, extra checkouts, and on and on, that there's just no way you can work the cost down much below where it is now. That wasn't our conclusion; the NASA people at Cape Canaveral told us that. However, the Headquarters people resisted all these conclusions at the time of our study. But in practice, they've been following them. They are reducing the cost of the shuttle by flying it less and in fact, the main reason it's being taken out of service five years earlier than they'll have a replacement is budgetary. They need the money to build the replacement.

The expendable launch vehicle they now use is an evolved expendable launch vehicle, not a new one, which we thought would cost too much. It's based on the Atlas ICBM. We had recommended using Titan IV from the ICBM program, since that part of the program was being discontinued. Titan IV turned out not to be quite as good as the Atlas, because it had some unreliable qualities that would have been difficult to overcome, although it would have been more powerful.

They haven't yet found a good program to succeed the shuttle because of the cost factors I've noted, and the Ares plus Orion manned space flight program intended to replace the Shuttle will bear out what we predicted about a new program. We did this study which wasn't accepted by the sponsor; another instance of where you can be right but unloved for it.

Another thing I worked on was to see whether one could quantify the military value of training. Earlier in this discussion I mentioned Jesse Orlansky. That was an area he was working on, and he asked me to get involved with him. The issue was, ''What measures of effectiveness (MOEs) could be used for the purpose?'' We finally settled on using warfare models and comparing their results with the results of actual exercises, at least, up to Platoon or Company level, for purposes of calibration and to set the values of the ground force effectiveness parameters. We got data from the Army Research Office, who got it from field training exercises, and compared that with results from combat models like TACWAR. The MOEs turned out to be the time and resources it would take to win a battle and how much troop training you would have to expend in order to achieve those results. I still have the reports we wrote on that topic.

I also contributed to the IDA history of ARPA/DARPA's chief technical accomplishments, which was published in 1991. It was a three-volume study that detailed the history of most of the successful ARPA and DARPA projects from the organization's inception. My contribution, in addition to describing a few of the projects where I was especially knowledgeable, was to describe the particular management structure and operational concepts that led to success. Basically, they were that you had an urgent national problem and you set up an organization that was free to run with it with very few of the bureaucratic constraints and delays that characterize the normal operations of the DoD. They obviously had to stay within budgetary constraints and they couldn't do anything illegal, but Congress gave them a lot of leeway and let them run outside the usual service and OSD bureaucratic structure, of having to have a "requirement," having to budget years in advance in exquisite detail, get a whole string of approvals up the line – all the things that slow defense acquisition to a crawl. I wrote all that detail into the final volume of the three-volume report. Obviously, having been in ARPA helped, because I could bring in a lot of inside understanding of how the ARPA (now DARPA) system worked.

I look now at other organizations people try to start that are supposed to be ARPA-like. For example, one just started in the intelligence area, and I think there is talk of starting one in the energy area. I don't think the conditions exist to make those organizations as successful as the original ARPA, but that's a purely personal conclusion.

Finally, other work I did outside of IDA after my final resignation from IDA in 1996. I joined the Atlantic Council right after I retired from the VP position, while Andy Goodpaster was Chair of the Council. It was a great learning experience in terms of seeing how NATO operated at the top political and diplomatic levels, which had been beyond my ken in the Panel 7 work, although I had an inkling of it then.

BOB SHELDON: What is the Atlantic Council?

SEYMOUR DEITCHMAN: The Atlantic Council of the United States is a non-profit organization that was organized, I think, to exchange ideas about and within the Atlantic Alliance, and to consider matters that concerned the Atlantic nations, but it eventually got to be broader than that and deals with national security issues in the broadest sense – diplomatic, economic, as well as military and defense. It's still functioning.

BOB SHELDON: It includes some of the NATO countries?

SEYMOUR DEITCHMAN: No, it's a U.S. organization that deals with the NATO countries. It sets up lectures and meetings with distinguished players in international affairs, and undertakes related public affairs activities, all having to do with promoting trans-Atlantic cooperation. As part of that, in addition to it being a learning experience for me on how things operated at the political level in NATO, I had a chance to hear from world leaders. For example, General Shalikashvili came in and talked with the Council before he became Chairman of the Joint Chiefs, and General Wesley Clark came in and gave a talk before he became SACEUR. I had written a book by then called *Beyond the Thaw* –this was in 1990, after the Cold War ended – that dealt with the issues of where do we go from here. The book formed the basis of an "occasional paper" that the Council published. That was a big deal, as they sent their papers to all members of Congress and other high levels of Government – 3000 copies, I was told. The paper amounted to an executive summary of that book. That happened because I had asked Andy Goodpaster to review the book and then he asked me to write the Council paper.

As a matter of interest, one of the things that I was able to show, in connection with discussing the future of the Defense budget – at the time, about 1991, 1992, after the Cold War had ended – was that the costs to renew our infrastructure were going to be astronomical, to the point where the numbers looked beyond belief. For example, I estimated it would take on the order of two trillion dollars to upgrade and maintain the deteriorating parts of our highway infrastructure, including especially all the bridges in the country. That seemed like an impossible amount of money at the time, and yet we're seeing numbers like that entering the budget in all seriousness now.

This might be an appropriate place to interject another illustrative anecdote about the genius of Gene Fubini. We had stayed in contact after I left the Pentagon. I had been meeting Gene about once a month, at his request, especially after I became Vice President for Programs at IDA. We would chat for an hour or two about affairs of state, the world, and the DoD, as well as work going on at IDA. Gene was retired by then, and I think the contact helped him feel he was keeping his hand in, and I certainly got some good advice about many puzzling matters. I had given him a copy of *Beyond the Thaw*, and the next time we met, he dismissed it, in his typical blunt fashion, with the comment, ''You don't know anything about history.'' Well, that was a puzzler, because I fancy myself a history buff and the book is salted with historical references. I asked him what that was all about, and he commented that with Tito gone the next big conflict in Europe would be about the breakup of Yugoslavia, and I didn't even mention that in my book. He was right, and prescient.

To continue my story, I maintained my affiliation with the Atlantic Council sporadically. Then in 2006 I became involved in a study of post-conflict reconstruction. The issue, spurred by the Iraq and Afghanistan experiences, was how to get a country rebuilt after it has been torn up by conflict, and how the Alliance can do that. One of the models of how to do it that resulted from this study was the Provincial Reconstruction Teams (PRTs) in Afghanistan. One conclusion was it would take a lot more resources than anybody would bargain for. It involved thinking about how to provide security; how to get the local people to do a lot of the work, rather than our doing it for them – all problems we're facing in Iraq and Afghanistan today. It's all very difficult to accomplish, and it's hard for us not to just take over and say, ''Hey, let me do it.'' The Afghan PRTs serve as a good model.

I did some work with the Office of Technology Assessment (OTA), which had been set up during the 1980s to help Congress assess the potential impact of new technologies that looked like they were going to overwhelm the country, and how to establish policies to deal with their effects. There was computing, there were ballistic missiles, there were guided weapons and the questions were, ''What does all this mean?'' OTA did many studies in civilian areas as well as military, undertaking projects at the request of members of Congress, including the Senate and the House. To do a study, they would assemble a group of experts and provide administrative and technical support and guidance. I worked with them on the question of where TACAIR technology was going and whether ballistic missiles could take over. And again, in a theme I didn't originate but that just keeps coming up, was the issue of how information technology is going to drive everything and how to manage it.

When Newt Gingrich became Speaker of the House, he abolished OTA; my personal opinion was he didn't like the answers that OTA was giving Congress. The way the OTA system worked, someone in Congress would ask a question. Then OTA would gather the appropriate experts, and the experts would get their thoughts around the problem, arguing around the table the way advisory panels do, and then the staff would write a report and the experts would review it to make sure that their thoughts were accurately reflected in it. Newt Gingrich didn't like some of the results that were coming out and he said ''I don't need these people second-guessing me.'' He disbanded OTA and eliminated the authorization and appropriation track that had set it up. That was really too bad; I think an organization like that could be very helpful to Congress (e.g., on stem cell research, climate change, and alternate energy), and I may have seen a press report lately that there is thought being given to resurrecting it.

I also did some work with the National Defense University (NDU) Institute for National Strategic Studies (INSS). I helped work on a major study called ''Globalization and National Security.'' There were inputs from both inside INSS and many on the outside, including people like then Undersecretary of the Navy Jerry Hultin, Bob Oakley, who had been Ambassador to Pakistan, and Robin Pirie, who at one point was the Acting Secretary of the Navy and had also been a Vice President at IDA. I had helped recruit Robin to the position of VP Planning and Evaluation when I was VP Programs, and we'd become good friends. Another participant I knew quite well was Paul Davis from Rand, with whom I had worked extensively on the NSB. All told there were about 50 people on the project, and they produced nearly as many papers, all of which were integrated as chapters into a two volume work called *The Global Century.* My chapter, based on what I had learned working with the NSB, was entitled Military Power and Maritime Forces, in which I laid out where I thought the Navy and Marines would have to develop over the next half-century. One of the main issues I foresaw was that the elaborate Cold War base structure that we had built would have to shrink, making the maritime forces all the more important as the spearhead of American power abroad.

The two-volume set was published in 2001, edited by Dick Kugler of the INSS and Ellen Frost, who was then with the Institute of International Economics and who had been Deputy Assistant Secretary of Defense for Economic and Technical Affairs. She had also worked for the U.S. Trade Representative, so she was very knowledgeable about many aspects of globalization outside the narrow military ones.

It was a very broad ranging couple of volumes that ranged over all the military, economic and diplomatic matters. (You could probably get them from INSS.) In my chapter, I reflected some recurring themes in addition to what I noted before, that I will get to very shortly. Basically, all these efforts helped my thinking evolve on matters that I think apply to defense and national strategy today, as will appear from here on.

After the NDU work, and with the NSB work pattern still evolving, in about 2005-2006, I got involved with Ray Hemann, President of Advanced Systems Research in Pasadena, on a task for Andy Marshall. I had met Ray through the NSB work, and he and Lee Hunt (mentioned above) invited me to work on the study for Andy Marshall. What Andy wanted to explore was the economic exchange between our high military costs when we go after terrorists – people like Al-Qaeda – and the costs to Al-Qaeda and other terrorists, such as those in Somalia, of attacking us with terrorist acts, or weapons like the rockets the Palestinians, Hezbollah, or similar organizations might use to attack U.S. and allied forces.

That proved to be a non-problem because we have to pay for our defense budget and the forces it buys while the other guys get their weaponry from the states that support them, so other than their manpower and support costs they operate for free. But there would also be a concern if we became involved with a state like Iran, whose long-range weaponry consisted of ballistic missiles. As an MOE, I considered the cost per person in the armed forces, and I could compare various countries and non-state actors that way.

It turns out the U.S. has, by far, the highest cost per person in the armed forces in the world, but I could show through many examples that it was cost-effective to spend that money for what we have come to call precision engagement. I'll note two examples here.

One is the bombing of Dresden in World War II, compared with bombing of Serbia over Kosovo. Dresden was destroyed because at that time we didn't have precision bombing and there were manufacturing targets scattered throughout the city. There was no way you could be sure of hitting any one of them without destroying a large part of the city around it. So we just took out the whole city.

In Serbia, with precision navigation and weapons, we took out just the targets we wanted to hit, and the few mistakes, like the Chinese Embassy, resulted from misidentification of the target, not the technology of the engagement (and incidentally, note the importance of IT (information technology) in TACAIR, again). Another good comparison came from a look at the U.S. and the USSR in Afghanistan. They used essentially World War II methods to try to take the country. They had helicopters, but they didn't have precision engagement in the way we understand it. They were there nine years and essentially were driven out. It took us three months and we conquered the country. It is true that was helped by alliances with local tribes, but I think we can be certain that our demonstrated military capability, including our ability to take out targets with precision without destroying the country, helped persuaded people that they should be aligned with us.

That was the kind of analysis that went into our report. Basically, we told Andy that the comparison he was looking for was a false lead, and that the U.S. approach to warfare was cost-effective by several measures.

We never heard from him about whether he found our report on this useful, but I expanded on the ideas that were in it as to what precision engagement imposes on the structure of the armed forces in comparison with the armed forces of other nations. Since I still had some connection with the Atlantic Council, I asked them whether they would publish it as an ''occasional paper.'' They agreed, and at the suggestion of the Council's president, I enlarged the paper to cover what we'd have to do to better prepare the armed forces to deal with what has come to be called asymmetrical warfare – what happens after we conquer a country's organized armed forces, as we saw in Iraq, and as we're seeing in a resurgence of internal warfare in Afghanistan now. That paper came out in October, 2004.

Here are some major themes that have emerged from all this work.

One is the effect of the diffusion of technology. IT is becoming global and we have to work much harder to maintain the U.S. technological lead. It also constitutes a growing vulnerability. Our armed forces and our whole society are increasingly dependent on IT. You can expect that opponents will focus on denying us our ability to observe, to navigate, to communicate – so our strength becomes our vulnerability and we have to look out for that. I don't know the extent to which we are.

For example, if a nation hostile to us had one nuclear weapon and wanted to use it most effectively, they would explode it at the right altitude in space. I don't know whether we have built our civilian communications, navigation, etc., systems to be able to withstand the electromagnetic pulse from that explosion.

Another theme relates to why we should use the military for peacekeeping, nation-building, and post-conflict reconstruction. That was something the Bush Administration was very negative about when they came in, and that left us less prepared to deal with what we've actually run into. I'm glad to see that the current administration has turned that around. In my view, the reason you want to use the military for that kind of thing is that they're trained, they're disciplined, they know how to marshal resources in a hurry and organize the use of those resources. They can fight if they have to, so they can provide security at the same time they're doing the reconstruction. That can get them started until the civilian help arrives.

I think our nation paid a very high price for not going that route in Afghanistan and Iraq. For example, it took years and many contractors to rebuild the road from Kandahar to Kabul. I think that could've been done by our SeaBees or the Army Engineers, in a few months, and what a difference that would've made in getting the country together. They could have protected themselves while doing it. Now we have the Provincial Reconstruction Teams doing that kind of thing, and while that's appropriate for the long run we still have to provide protection for them, so it uses a lot more resources for the same task.

Another lesson – our enemies play by different rules designed to counter our chivalry-based rules. They don't protect their women and children. In fact, they put them in the line of fire so we'll create civilian casualties and our media will play that up. They do this deliberately so as to create dissent about our prosecution of a war within our own country. For reasons like this, we're not, by nature, attuned to long wars. Our enemies know that, so they know that they don't need to win battles against us, all they have to do is keep the war going and we'll tire of it and want to go home. I came to that conclusion during the Vietnam War and it still, to me, seems to be true. You see the signs of all this when, say, insurgents in Iraq start shooting at our troops from the middle of a crowd in order to get us to shoot back, and indeed, in the fact that we have to fight insurgencies at all to support our interests overseas. And, it has become an issue again in the Afghanistan War, under President Obama. Insurgencies take a lot of time to resolve.

I want to backtrack a minute to make another note about this. When I talked about going to the Air University and misusing our air power because the enemy set up his guns in the temples and shot at us from there, and we shot back and thereby created more enemies.

BOB SHELDON: That was Hue City?

SEYMOUR DEITCHMAN: Many places all over Vietnam. It's been the same thing in Iraq and it is now the same thing in Afghanistan. That's another lesson. We don't understand that we're fighting an enemy that doesn't play by the same rules that we do.

Another theme: We have to work with other nations but there aren't any others that spend the cost per person in the military that we do.

We have unique capabilities and if those capabilities start to spread, that will enhance our vulnerabilities, (as I commented earlier). On the other hand, we hear a lot of talk now about how building conventional forces is passé because we need to be able to fight asymmetric warfare, meaning we are fighting against terrorists and insurgents. But we need both and that's something that's not commonly recognized, because if we leave an opening, that's where our enemies will come in. We have only to look way back to the Korean War and note that we demobilized after World War II and left ourselves wide open. That's when the communist forces moved, and that kind of thing could happen again. In fact, I could argue that the reason we are coming to be on good terms with China is that they have come to the fact that we will protect Taiwan and we have the capability and the resolve to do it. As soon as we let that go, it will become a whole different ballgame.

Finally, I've been giving a lot of thought to deterrence. It seems to be gone from our lexicon but to me, the essence of deterrence is that the side that would attack you perceives that in your counterattack they will lose much more than they have to gain by the attack itself. As an example, think about the jihadists. When our current presidential campaign was just starting and we had many people tossing their hats into the ring, Representative Tom Tancredo from Colorado said, ''Well, if they blow up a nuke among us, we'll drop one on Mecca.'' The jihadists may not care, but the nations harboring them would have to think about whether we might actually do that. If there were a nuke blown up in the U.S. that killed a few hundred thousand people, and if it was also decapitating so that the most responsible elements of our government might no longer be in charge, then there would be no telling what we might do. We would hope the harboring nations would figure that they'd better restrain the jihadists.

To reinforce that, we have shown in many instances, like Korea, the Gulf War, Afghanistan – even Pearl Harbor – that attackers mis-estimate how we'll come back. Also, when we talk about it being intolerable for Iran to have a nuclear weapon I haven't heard anyone raise the notion of deterrence. Deterrence worked perfectly well between us and the USSR for 40 years or more – why can't we do that vis-à-vis Iran? I remember reading that Rafsanjani, who had been President of Iran either one or two Presidents before Ahmadinejad, said, ''If we had a nuclear exchange with Israel, then we would be damaged but Israel would be obliterated.'' Well, if we put Israel under our nuclear umbrella, we have enough under the sea in one of our SSBNs to obliterate Iran. Why don't we use that instead of just saying a nuclear-armed Iran would be ''intolerable.'' Intolerable means we have to do something about it right away – do we really want to go to war with a country the size of Alaska that has 79 million people, many of whom are, underneath it all, friendly to the U.S.?

I'm troubled about the lack of discussion about deterrence, and I keep writing about that wherever I can. For example, I had a long letter in the National Academy of Sciences journal, *Issues in Science and Technology*, about this very thing. Some people fear, and I think Senator McCain mentioned it during the election campaign, that if we let Iran have a nuclear weapon, then we'll have a nuclear arms race in the area. But when we put Japan under our nuclear umbrella, Japan didn't build nuclear weapons. They know they're protected. Similarly with, South Korea – and we have extended the nuclear umbrella over countries that have nuclear weapons. If the argument would be made vis-à-vis Israel – they already have weapons, so what value is our nuclear umbrella? Well, so do France, so do Britain. But we still extended our nuclear umbrella over them during the Cold War. A gap in our ability to do this is, as Larry Welch has pointed out, that while we and the Russians each had a pretty good appreciation of what motivated the other, we don't know that much about Iran. In fact the study of post-Cold War deterrence by the NSB recommended that the conditions and motivations of new players in the deterrence game need to be studied. That was a recommendation to the Navy, but to my knowledge, no one has followed up on it. This is now my crusade, if you want to call it that. We've got to start thinking about deterrence in these new contexts.

Now I've reached a close but I can't end it all without noting the honor of having won the MORS Wanner Award in the year 2000. To me that was a way of saying ''congratulations on having made some useful contributions to what we (the people interested in defending the nation) do.''

BOB SHELDON: One of the things some of my mentors have tutored me on is the book *Thinking in Time* by Neustadt and May, and I think you've read it. The book is replete with examples about historical anecdotes being used poorly to make current-day decisions. Based on your experience from writing in the Vietnam era and the insurgency and now applying that to the current day, how would you say what you learned back then compares to what we would apply now – what's the same and what's different?

SEYMOUR DEITCHMAN: Let me first say what I think is different. The enemies are different; they think differently, they operate differently. The North Vietnamese were trying to unify the country under their rule; they weren't trying to come to the United States and strike us here. Their activity in the United States was propagandistic. I would read classified things that sounded just like what I was reading in the *New York Times* about what the North Vietnamese were saying in their anti-U.S. propaganda. Beyond that, the Vietnamese – or, rather, the Vietcong - were following the Mao Tse Tung model of insurgency. Start with subversion, grow into guerrilla units, and from there into organized units. The North Vietnamese were already a full-fledged army, growing out of their war with the French, and they were helping the Vietcong and eventually invaded the South themselves with regular army units.

What I think is the same is that we have a tendency to underestimate our enemies. Example one: In Vietnam, we had all of Bernard Fall's books, which were required reading in the U.S. Army. They told about how difficult an enemy the North Vietnamese were, and yet I remembered when we went in there, the feeling was, ''With our technology, we won't be caught like the French. We'll overcome that enemy easily with it.'' It didn't work. Next example: Iraq. We went in there thinking, ''These people are going to be friendly, they'll welcome us.'' At least that's what we were told. We didn't recognize the internal schisms between Sunni and Shia. We did understand a little bit about the Kurds but thought that one would be under control because we had a sub rosa friendly relationship with them even during Saddam's time. In Afghanistan, we created our own diversion by invading Iraq, thinking the job in Afghanistan was finished – we never thought about how the guys we beat might try to come back.

Again, we underestimated the opposition. We're underestimating the opposition now, I think, vis-à-vis Al Qaeda. I don't know what's happening in the classified world, because I've had no briefings on that; but I think, at least from all the public statements that came up in the presidential debates and since, in some quarters the thought is, well they're essentially beaten, they're falling apart. I think we're getting into a much more dangerous situation because there's going to be a lot more freelancing among the jihadists, inspired by Al Qaeda in the background. Just listen to Zawahiri's occasional rants. We don't know where they might strike next. There's a book that I have read, called *Lightning Out of Lebanon,* which is about Hezbollah penetrating the United States, mainly for fundraising purposes. It describes how a Hezbollah cell was broken up by the FBI in Charlotte, North Carolina. That is mainly what the book is about, but it describes activity in other areas. When you read that book, you realize in spite of all the constraints, how easy it is to penetrate this country. So again, I think we're in the business of underestimating our opposition and that seems to be a U.S. characteristic.

BOB SHELDON: In Vietnam, you were working with a team of social scientists. Now several years later, we're trying to use social scientists again for the irregular warfare fight. What's the same and what's different?

SEYMOUR DEITCHMAN: What's the same is, I think, the sensitivity of the subject. As soon as a social scientist breaks out and does something that the media might think is newsworthy, that would make a news article. The media will jump on it, mainly as something to make fun of or to be snide about. Either vis-à-vis the government, because why didn't you understand what this guy is saying, or vis-à-vis the social scientist because why did you probe into that sensitive area? The sensitivity of what social scientists might find and how that would strike the media, and how that might backfire on policy, I think, is much the same.

What's different is that there's more recognition on the Hill that we need that kind of work. In fact, when I read in *Science* Magazine that the DoD is going to start what is now called the Minerva enterprise, I wrote to the man who was named as the person responsible for it in OSD and told him that we had a similar experience and mentioned my book. We've had several exchanges, including with Yuna here, and with Pauline Kusiak, who I think is now the Program Manager on that. One of the people that I talked with, who learned about my recent activities and expressed an interest, is Representative Ike Skelton's Staff Chief. We had lunch together and he indicated his boss approved of the effort. That's totally different because at the time of Vietnam, the Congressional staff was totally hostile to the whole notion of the DoD using social scientists to study what they viewed as things within the State Department's domain. Finally, I've learned since that Minerva was actually started by Secretary of Defense Gates. It's not just ''me'' being tolerated in an initiative, as back in the Vietnam Project Camelot days; now it's policy at the highest levels.

Another thing that I think helped Project Minerva get started without negative fanfare is that there was a presidential election going on at the time and the media were focused on that so they weren't looking for malfeasance on the part of social scientists. That's just an accident of timing, which has been very favorable. I hope it'll do some good.

When they start getting into subjects that are sensitive, that could get to be a whole different thing. I'm trying to think of an example that could rouse people up. Suppose, as a hypothetical example, that it was found out that DoD-supported social scientists were studying Taiwan, in Taiwan, to find out under what circumstances Taiwan would be willing to be absorbed by Mainland China, and that became public. That would raise a storm. The situation, to me, is fragile, even with all the positive aspects I outlined earlier. Project Camelot, the effort started back in the Vietnam days, blew up when social scientists, not appreciating the sensitivity, broke out of the bureaucratic constraints I had tried to impose and went public. That could happen again on sensitive subjects.

BOB SHELDON: There's often tension between OR analysts who want to quantify everything in nice, simple terms and social scientists who generally use qualitative methods. Based on your experience with these issues, what kind of advice would you give?

SEYMOUR DEITCHMAN: Use the simple OR terms but allow the social scientists to hedge them around with conditions and references to circumstances and possibilities where they might or might not apply. In other words, you have an MOE but then you say, well, this applies in these circumstances but not necessarily in those, and so forth, and that's what the social scientists add.

I had an exchange like that with my friend, Jesse Orlansky, whom I described earlier in this oral history. I had quoted Lord Kelvin in one of my books – in fact it was the book on Camelot in which Lord Kelvin said, in paraphrase, ''When you can express something mathematically, then you begin to understand it. And when you can measure it, you can then understand it.'' And Jesse, the psychologist said, ''That's a bunch of nonsense.'' He didn't agree with that at all because he felt you couldn't describe the complexities of human behavior in such terms. My way through this is, yes let the OR people do their thing, and the social scientists can help them understand the implications of what they're doing in human terms. The OR people express the MOE and then the social scientists put all the caveats around it.

BOB SHELDON: What has been your involvement with MORS over the years?

SEYMOUR DEITCHMAN: Sporadic. I came to some of the symposia, and was a member on and off. I received the *Phalanx* and enjoyed reading it, then had a lot of other things to do and I didn't attend as often.

YUNA WONG: I was just curious as to your thoughts on modeling and simulation in irregular warfare?

SEYMOUR DEITCHMAN: I had commented earlier that I wrote a paper on ''A Lanchester Model of Guerrilla Warfare.'' I think if you can do the modeling correctly – and they have to be stochastic models because the way guerrillas (and humans in general) operate everything is going to be statistical – the work ought to be done, for two reasons. One is, when - and if - you finally get a model, it's going to help. But the key point is, in trying to build the model you'll learn a hell of a lot about both your opponent and yourself. So by all means, I think that should be done. Irregular warfare is broader than guerrilla war. Guerrillas in warfare are in many ways an organized military unit, if you will, in a place. They operate under cover and they operate clandestinely and so on. But irregular warfare is broader. It also includes the jihadists and international terrorism and some of those kinds of things. All this is in my lexicon; I don't know what the official definitions are.

YUNA WONG: Because you have such extensive experience in so many past national security challenges, what should MORS be trying to do going ahead?

SEYMOUR DEITCHMAN: I think a useful thing that can be done in today's environment would be to spread the notion that you raised earlier, that you can do modeling – that there is value in the process, and I would try to get military people to understand that. When people think about models they usually think about simple exchanges, which don't work very well in the kinds of warfare we face now. We need to get people to think about extremely complex models that are statistically based. We can vary parameters statistically, so that we can learn, or characterize, the nature of the different kinds of warfare that we're talking about now. MORS should be very well placed to encourage that.

I had a hint that one of the candidates for President or Vice President early on – it might have been Wesley Clark – said something like, ''Well, we've run our models and this is the way we come out of it.'' I don't know what models they're talking about. If it's a simulation model like TACWAR, it's useless vis-à-vis the kinds of warfare that we seem to be having to fight today. That might be one of the most important areas to work in.

Another is to learn about dual purpose armed forces. You have to have the people trained for combat against organized armed forces because those are all over the world and we don't know where or how we're going to get involved. Then how do you use those people to do the work of what's now called asymmetric warfare? I don't like that term, by the way, and I'll come back as to why. How do you get them to do the work of asymmetric warfare, which involves everything from local business economics all the way to fighting guerrillas and just maintaining law and order in a community. All that will come in play. I don't think we fully understand how to use our military in that context and still have them be a military that can fight more conventional wars that might come up again.

Why don't I like the term, ''asymmetric warfare?'' Let's go back to the Cold War. We were a strong naval power, and when John Lehman came in as Secretary of the Navy he came up with a Maritime Strategy that said we're going to send five carriers into the Norwegian Sea and bomb the hell out of the Soviet Union, and he turned our Navy in that direction. How did the Soviets counter that? With submarines and aviation. That's asymmetric warfare and the term asymmetric warfare applies to a lot of things. When it's applied to the warfare between us and the jihadists, that masks the possibility that that's not the only asymmetric warfare. I would call warfare between us and the jihadists anti-jihadist warfare and that's explicitly descriptive.

But to continue, I think MORS can do much to raise the national consciousness about deterrence and its value. My personal opinion is that if Iran wants to make nuclear weapons, we'll never stop them. They know how to design the weapon now. They are gathering the material and even if we bomb the hell out of their centrifuges, they'll find a way to make a weapon anyway. They'll get the material from North Korea, if necessary. But deterrence has to work and I think the U.S. has forgotten the language of deterrence and MORS can really help bring that back. You could even have a MORS session about deterrence, to get people thinking about it. Assume countries like Iran, North Korea, and others have nuclear weapons, or Pakistan turns hostile and they've got nuclear weapons already. How do you deter them? We've just given up thinking about it.

BOB SHELDON: At Ft. Leavenworth, they have what's called Human Terrain Teams - cultural anthropologists. Any comments on that from your perspective?

SEYMOUR DEITCHMAN: I don't know enough about what they're doing. But I instantly understood what they meant with the term human terrain. We deal with physical terrain and now thinking about the ''human terrain'' is, to me, a step very much in the right direction. It might even lead us not to underestimate our opposition, for a change.