

INTRODUCTION

Ervin Kapos was one of the founding directors of MORS in 1966. This article includes both an interview of Mr. Kapos as well as his remarks at the MORS Heritage Session at the 71st MORS Symposium at Quantico, Virginia, 11 June 2003.

MORS ORAL HISTORY

INTERVIEW WITH ERVIN KAPOS

July 27, 2004

Office of Naval Research, Arlington, VA
DR. BOB SHELDON, FS, INTERVIEWER

BOB SHELDON: Today is July 27th, 2004 and I'm here at the Office of Naval Research to interview one of the founding directors of MORS, Ervin Kapos. Tell me where you were born and raised.

ERVIN KAPOS: I was born 21 June 1931 in Transylvania. I spent the first seven years of my life in Romania. Then twelve years in Cyprus, where I went to an American mission school, the American Academy.

BOB SHELDON: Back to Transylvania, had your parents lived there their whole life?

ERVIN KAPOS: There, and thereabouts.

BOB SHELDON: What are your parents' names?

ERVIN KAPOS: Leo Kapos. (Pronounced more nearly like Kah posh.) And Elizabeth Barta Kapos.

BOB SHELDON: Did you go to school in Transylvania?

ERVIN KAPOS: No. I spent a year or two of kindergarten there in Transylvania, and one year of primary school elsewhere in Romania (Braila). Then we moved to Cyprus.

BOB SHELDON: What was the reason for moving to Cyprus?

ERVIN KAPOS: World War II was looming, and my father was a very liberal and humane person. He didn't see the environment in Romania as being conducive to raising his boys there. So he decamped in 1938.

BOB SHELDON: How many siblings did you have?

ERVIN KAPOS: One. Ten years older.

BOB SHELDON: What year did you leave Romania?

ERVIN KAPOS: We left there in 1938, and I lived in Cyprus until 1950. As I said, I attended an American mission school there until 1948. Then in 1950, I came to the United States. I have lived here ever since.

BOB SHELDON: What kind of courses did you take at the mission school?

ERVIN KAPOS: Well, what was normal for primary and secondary school.

BOB SHELDON: Did you have lots of math?

ERVIN KAPOS: Had lots of math. Had lots of science. Had lots of humanities. We put in a full school day.

BOB SHELDON: Any particular teachers you remember that had an impact on you?

ERVIN KAPOS: Oh, boy. Actually I remember all of the teachers. And they all had their various and sundry impacts.

BOB SHELDON: Tell me about your math and science teachers.

ERVIN KAPOS: My math teacher was a fellow named Mavrides. I don't know what his precise educational background was, but he was a good teacher. He advised me that I should study whatever I wanted, because I was a fairly talented kid and would do well at whatever I chose—but not to major in mathematics. So, I majored in mathematics. [Laughing]

BOB SHELDON: And your science teacher?

ERVIN KAPOS: It was Mavrides again. I don't remember much of the science, except that it was basically high school physics.

BOB SHELDON: Then you left Cyprus in 1950?

ERVIN KAPOS: '50 and came to the United States.

BOB SHELDON: Did your whole family come?

ERVIN KAPOS: No, I came by myself. I came to go to college, and I ended up at Indiana University.

BOB SHELDON: How did you choose Indiana University?

ERVIN KAPOS: That's a good question. For some reason there was a veritable trail of predecessor students from the American Academy in Cyprus to Indiana University. That was about how it started: I got into that trail. They had a famously good foreign student program, so I went there.

BOB SHELDON: What was your major?

ERVIN KAPOS: I shilly-shallied for a while, but it turned out to be mathematics.

BOB SHELDON: Applied mathematics or theoretical mathematics?

ERVIN KAPOS: As an undergraduate those two things amount to the same thing. Then I went on to graduate school at Indiana University, and I went in for theoretical mathematics, abstract algebra and things

Military Operations Research Society (MORS) Oral History Project Interview of Ervin Kapos

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like that, with an extra sprinkling of Applied Mathematics.

BOB SHELDON: Any notable professors from your Algebra studies?

ERVIN KAPOS: There were a number of them. There was an algebraist named Professor McKenzie. On the applied side, the one I remember best was my advisor, Professor Eberhard Hopf. And there were others, of course, each of whom had an impact.

BOB SHELDON: How was your move from Cyprus to Indiana? A big change in lifestyle?

ERVIN KAPOS: Well, it was. But then again it was not. Remember, I had gone to an American school, and so I'd gotten part of the culture. I think I fit in rather easily. What was different about me from most foreign students was that I had to work my way through school. By the time I had paid my first term's fees and my first month's room and board, I had left exactly twenty-four dollars. [Laughing] So, I had to get ready to work, and I got ready to work. I had a variety of jobs, from selling jewelry in the downtown credit jewelry (where I was a big hit because of my funny accent), to typing book lists for the university bookstore, to typing W-2 forms for the university payroll department. On and on. Then I got a job running projectors for the audio-visual department. I finally got a research assistantship in the geography department, and an editorial assistantship in the history department. So, quite a varied menu.

BOB SHELDON: So you were not just interested in mathematics?

ERVIN KAPOS: No. I was not. And in fact, fairly shortly I became research assistant at the Institute for Educational Research there, and I went from being a research assistant to being a research associate. But I also had a fellowship in mathematics, which ultimately turned into a teaching fellowship.

BOB SHELDON: What courses did you teach?

ERVIN KAPOS: Freshman courses.

BOB SHELDON: Did you continue for a PhD?

ERVIN KAPOS: I did, but I didn't finish. I'm what's known as an ABD [All But Dissertation].

BOB SHELDON: When you left Indiana University, what was your first regular job?

ERVIN KAPOS: It was in the Operations Evaluation Group, the OEG, which was then

operated by MIT under a contract with the Navy. That was in 1958.

BOB SHELDON: How did you happen to interview with them?

ERVIN KAPOS: They published an ad in *American Scientist*, which struck me as very snotty. They essentially said we can't tell you what our name is, and we can't tell you what we do, but send us your resume and we'll tell you why you're not good enough to work for us. That's a very loose translation. And you know, when you're that age, something like that is a challenge. So I wrote them a letter and said hey, look, I'm much too good to have a resume—however, let me tell you a bit about me. And apparently this impressed the powers that be. The fact that somebody could write a letter in English. And a few days later here comes the recruiter to interview me, and a few weeks after that, I came to be interviewed in Washington.

BOB SHELDON: Who was the recruiter?

ERVIN KAPOS: It was Jack Robinson. He was the Jack-of-all-trades, you know, he recruited, he ran document control for the Group, and all kinds of things. As I say, I came to Washington for an interview and was hired.

BOB SHELDON: What was your initial project?

ERVIN KAPOS: It was an interesting project. It was to optimize the employment of aircraft carriers in the western Pacific for general war. And that pretty much occupied my first year.

BOB SHELDON: How did you approach that problem?

ERVIN KAPOS: In pieces. In other words, I took a look at the AAW [air-to-air warfare] and the ASW [anti-submarine warfare] and strike warfare and so on, and I don't recall that I had any particular methodology in mind. I just examined the problem from all sides, and then eventually suggested the solution.

BOB SHELDON: What was the solution?

ERVIN KAPOS: The solution was to operate the carriers in single-carrier task groups, because that would maximize survivability and it would not greatly handicap strike operations as those were visualized at the time.

BOB SHELDON: Who did you brief the results to?

ERVIN KAPOS: The Commander of the 7th Fleet.

BOB SHELDON: Did they accept them?

ERVIN KAPOS: I'm sure they did. I did not brief them myself. The OEG rep on the 7th Fleet staff briefed them. And yes, as a matter of fact, they did accept the recommendations.

BOB SHELDON: That's good to have your first study taken seriously.

ERVIN KAPOS: Yes, it was—although it was not the only factor bearing on that decision. Especially considering the vicissitudes that the study went through—or rather that I went through because of the fact that I did not finish the study before I went off to my first field assignment and had to come back to finish it. It was okay.

BOB SHELDON: What was your first field assignment?

ERVIN KAPOS: It was in San Diego, at COMOPTEVFORPAC, which was a new command at the time.

BOB SHELDON: Did you go out to sea with them?

ERVIN KAPOS: No. OPTEVFOR is an operational test and evaluation command, and they are shore-based—one went to sea only to observe individual tests. But I did get a sea assignment for my second tour, which was consecutive to the first one, also in San Diego. I was assigned as OEG rep to First Fleet, and COMFIRSTFLT was put to sea just about the time I arrived. In fact, I arrived just in time to have my files transferred to the flagship.

BOB SHELDON: Did you find your sea legs easy?

ERVIN KAPOS: Yes, at least I don't remember any particular problems.

BOB SHELDON: Then you came back and you finished up your study?

ERVIN KAPOS: I came back on TAD and finished up my study during my first assignment. That was the nature of discipline for not finishing up before I left the home office.

BOB SHELDON: Was your study published as a report or as a set of briefing slides?

ERVIN KAPOS: It was published as a report.

BOB SHELDON: How many pages?

ERVIN KAPOS: Probably seventy.

BOB SHELDON: It was a substantial report then. Did you have team partners?

ERVIN KAPOS: No, I was the only one.

BOB SHELDON: Did you have peers review it?

ERVIN KAPOS: Actually, we did not have peer reviews. It was reviewed by the higher-ups. I had a team leader, who was Ralph Beatty.

And he had a division director over him, who was probably Jim Hartzler. Whether my study also got reviewed by the associate director and the director, I don't know.

BOB SHELDON: Name some of your peers that you worked with then.

ERVIN KAPOS: The same year that I came to OEG, George Haering joined; he subsequently became a big shot in strike warfare. I also remember Marc Nerenstone. But the others don't come readily to mind, although there were eleven of us in that entry "class". The one member of the group of roughly the same vintage whom I remember is Phil Depoy. We are friends to this day.

BOB SHELDON: How did you find your work pace at OEG?

ERVIN KAPOS: The work pace was just a normal office existence. Except we tended to work nine hours a day, nine and a half hours a day, just according to individual tastes. The work pace in the field was really quite different because in the field you, of course, integrated into a Navy command. You worked whatever hours the job demanded, very rarely less than eight, and quite often more than twelve. At sea, of course, there's not much to do except work, and so there it got to be fourteen hours a day. It does good things for productivity I assure you.

BOB SHELDON: How many weeks did you spend at sea?

ERVIN KAPOS: I was with First Fleet for twelve months. We were technically at sea all the time since we were aboard a flagship. The flagship itself was at sea a week or two a month. And then at the end of my tour, we went off on a circuit of Southeast Asia, because we were the contingency commander for Southeast Asia at the time, and so that was about eleven or twelve weeks. I came back after my two assignments, which ran from 1959 to 1961.

BOB SHELDON: Before the Vietnam War lit up?

ERVIN KAPOS: Yes. And once we came back from Southeast Asia, I jumped ship. My relief came aboard in Hawaii and I jumped ship and came to Washington.

BOB SHELDON: What was your next project?

ERVIN KAPOS: I came back here with something of a reputation for understanding Command and Control, because the NTDS had been evaluated in task force operations when I was at OPTEVFORPAC. I did not really under-

stand Command and Control, but I had an interest in it and so I began to gravitate towards it. I became the OEG Scientific Analyst (as they would call it), essentially on local field assignment, to OP-35 which was the Command and Control division of OPNAV, and then I was loaned out to NAVCOSSACT, the Naval Command Systems Support Activity, which has long since gone, as sort of an acting technical director. It was a new command, just being established. So I played at being a technical director for two or three months. They eventually got a real one in place, because I certainly was not a real one. Then I returned to OEG and, after considerable discussion with the then director who was not at all sure he wanted to go that way, was given the job of organizing a Command and Control team. This was sort of unprecedented for OEG where teams came in place with names already. I did not appreciate at the time how wonderful an opportunity it was. But it was. And I built a team of five people. We did Command and Control analysis of various types. From tactical to not exactly strategic, but at least operational.

BOB SHELDON: How did you approach the problem?

ERVIN KAPOS: Well, there were problems and problems. For example, one of the projects we did—and these are all done by individuals on my team, not by me, and not by groups; group projects were not in fashion at the time, at least not in that place—was to look at the factors that affected the effectiveness of electronic warfare systems in defending ships against missile attack. This turned out to be a legitimate command and control project, because decision delays turned out to play a major role in the effectiveness of the devices. Another one dealt with the utility of satellite communications and what kind of satellites the Navy should want. We looked at CPXs [Command Post Exercises] and analyzed results of CPXs, with a view to determining what information flows characterized crisis operations, and then we transitioned from CPXs to real-world crises such as the Cuban missile crisis and the Dominican Republic crisis. We had quite a diverse program—but diversity has its penalties. I mean, some say your impact is necessarily very localized.

BOB SHELDON: Who posed the problems for you to study?

ERVIN KAPOS: There is no simple answer to that question. For the most part, a consensus

just evolved between ourselves and the client about what was to be done, as in the case of the EW analysis. The satellite study was requested by the management of the Center for Naval Analysis, to patch a hole left by a program that had run its course. The CPX analysis was more or less self-initiated, except that we needed the real data from the field and so we had to convince the command in the field that it was for a worthy cause. It was a peculiarity of OEG at the time that we were pretty much left to select problems. The director, of course, had to approve them and there was a process for doing that.

BOB SHELDON: What kind of decisions did the studies impact? Were they acquisition decisions or operational decisions?

ERVIN KAPOS: Both actually. It was not always easy to see the impact. You made your little input and hoped for the best. Except that toward the end of my time, I discovered a Naval officer who carried around with him in his briefcase all of the stuff that we had published from my program—all of the communications analyses—and who could tell me more about the results than I could remember myself. He would pull out the right piece of paper to influence a decision on the table.

BOB SHELDON: He was your marketing guy?

ERVIN KAPOS: Yes, and he did a great job.

BOB SHELDON: How long did you spend at the OEG?

ERVIN KAPOS: I was in OEG for fourteen and a half years. I had a year in OEG Washington to begin with. And I had two years in the field. Then I had four years and a bit with the Command and Control team—which in itself is unusual in length, but it took that long to develop a program, to run the program, and to develop a successor and all of that. Then I went to CINCPACFLT, as the senior OEG representative. By now the war was in full swing. And I did combat analysis with CINCPACFLT.

BOB SHELDON: What kinds of combat analyses?

ERVIN KAPOS: Here again, we had a team of five people. And different individuals did different pieces of analysis. I did strike analysis and aircraft attrition analysis, and things of that nature, and I did oversight of the work that everybody else was doing. Somebody studied air-to-air combat. Somebody else studied the effectiveness of strike support, and

so on. The problems obviously shifted over time.

BOB SHELDON: Can you remember any particular strike studies during the Vietnam War that you were involved with? The impact of those studies?

ERVIN KAPOS: The impact was immediate, because we put out essentially weekly or bi-weekly analysis reports. And they went to the people on the front lines. As for an example of a strike study I was involved with, probably the best example was the analysis of the Rusty Nail strike series against the Hanoi/Haiphong area in December of 1966—an isolated series of strikes that required us to work pretty much day and night to get an assessment of how they had gone.

BOB SHELDON: What kinds of recommendations came out of those?

ERVIN KAPOS: One recommendation that came out of an analysis done by Frank Shoup, who was one of my colleagues there, was on penetration and egress tactics. He suggested that they fly over 8,500 feet with at least two or three thousand feet of clear air beneath them, to be able to see and avoid surface-to-air missiles, because evasion turned out to be the most effective tactic against the SA-2 missile of that day.

BOB SHELDON: Where were you physically located then?

ERVIN KAPOS: At CINCPACFLT headquarters in Makalapa. It's on Oahu, on the fringes of Pearl Harbor, in Hawaii. I was there for thirteen months. Then I came back. Having been in the business of combat analysis, I was commissioned to organize the Southeast Asia combat analysis division in order to match up with the Southeast Asia combat analysis group in the Pentagon. I did that. I had twenty analysts at that time continuously analyzing air, surface, and support warfare. And again, we had a very fruitful time of it. The most avid consumers of our analysis were the Navy headquarters types, of course. But we also reported to the operational commanders on matters of interest to them. I did that for thirteen months—plus five, because I was to be relieved by Phil Depoy, and he was finishing up his thesis. But after thirteen months, I became director of the Marine Corps Operations Analysis Group, while also keeping the Southeast Asia group for about five months—something that I don't think the Marines were really privy to.

BOB SHELDON: This Marine Corps analysis group you headed, did that evolve into MCCDC at Quantico?

ERVIN KAPOS: No. MCCDC is the Marine Corps Combat Development Command and it is a command. The Marine Corps Operations Analysis Group, or MCOAG as we called it, was just what its name implies—an analysis group. We had tasking from various parts of the Marine Corps. But the fact was that the Marines were busy fighting a war, and so I made it my business to swing the program over to combat analysis. We continued with one project which was being done for Headquarters, which was to do a Leontieff model of the Marine Corps' resource allocation, and that turned out to be very useful. Otherwise, we pretty much concentrated on combat analysis. After thirteen months—thirteen months seems to have been a common theme in my career at that time—as the head of the Marine Corps Operations Analysis Group, I returned to OEG, and was there for about four years.

BOB SHELDON: How many people were in OEG then?

ERVIN KAPOS: About forty-seven when I started and about a hundred and twenty when I finished.

BOB SHELDON: What was growth due to? More interest in studies or growth in the military?

ERVIN KAPOS: It was nothing very subtle. It was partly organic growth in OEG, which was rather anemic at the time. We also picked up the ASW Tactical Analysis Groups during my tenure, as a matter of fact more or less simultaneously with my accession, and that was another forty or so analysts. We also established a military component: for the first time CNA began to get military officers—and even some enlisted personnel with appropriate qualifications, partly as a result of an initiative of mine—and I got my share of those. One of the things I started in OEG was something we called "Red Side OA", which required the personal support of the CNO, then Admiral Zumwalt. Under it, a military-civilian team in OEG reconstructed Soviet naval tactics—yes, Virginia, that was back in the days when the Soviets were the red side—based on all the data that was available from any source, and we produced detailed research memoranda on the results (more or less under the rubric of know your enemy). But we also produced a "bridge manual", called Red ATP-1, which was given

the widest distribution in the fleet and had quite an impact on the specificity of threat awareness. Red Side OA was run by a colleague of mine called John Pierce.

I was in OEG for four years plus some, and then I left and entered the private sector. I went to work for a tiny little outfit called Ketron, Incorporated. John Kettelle owned it, and he recruited me because I was looking for some useful obscurity at the time, having decided to leave OEG. I worked there for eleven years, and grew it from a tiny little outfit, grossing maybe half a million dollars to one grossing maybe twenty million. I spent three years as Vice-President for Washington, and then four years as Executive Vice-President for the whole company. Then I had four years as President of the company. We supported really several sectors. The Philadelphia operation, which was located at Valley Forge, had grown its business on the Naval Air Development Center nearby. They were supporting the technologists and the logisticians there. And they gradually developed their business into the non-Navy and civil side. They had a Vice-President of their own—Dave Knies, a very capable fellow. The Washington operation, which was mine, had a clientele that looked an awful lot like OEG's clientele. The work was an awful lot like it as well. We did a whole gamut of things—combat analysis, intelligence analysis, acquisition "analysis". It was a very fruitful sort of existence. I have nothing but fond memories of it. Then at the end of 1982, I quit and started Kapos Associates, which I ran until the year 2000.

BOB SHELDON: How many were in Kapos Associates initially?

ERVIN KAPOS: Five. We started kind of first class, with an administrative person, and a financial person and three technical people. Then we grew rather rapidly for a while.

BOB SHELDON: What was your business market?

ERVIN KAPOS: Navy, initially. We worked for ONR and we worked for OPNAV on the Navy's Surface Warfare Plan, and then things just sort of "grew". We did the war games for the Atlantic Fleet Tactical Command Readiness Program—LANTFLT TCRP to its friends, of whom there were not many by the time the program expired 17 years after it was started during my years with Ketron—and we got into terrorism studies long before that was fashionable. So it was a natural thing that we began to do terrorism gaming on a joint and

interagency basis, which was our first major foray into that world, although to be sure we had had some joint work long before that. And then it was just as natural that we did a program of DoD-wide war games on Y2K—remember Y2K?—and took that interagency wide. All of that was very rewarding. Then my company was acquired by another somewhere along there, in the fall of 1999, and in the fall of 2000 I resigned. After all those years in and around the private sector—42 by then—I allowed myself to be recruited by the Chief of Naval Research, Rear Admiral Jay Cohen, to come to the Office of Naval Research as something new for ONR, the head of an Operations Analysis Program. And here I am—where you are talking to me—running an analysis program that is oriented partly toward an outside clientele, the fleet, and partly toward an inside clientele, ONR itself. It is going quite well, although some of the practical problems of getting projects started and keeping them going are uncomfortably reminiscent of what I experienced in the private sector.

BOB SHELDON: Let's turn to the history of your involvement with MORS. When did you begin to attend the symposia?

ERVIN KAPOS: It seems to me that I attended two or three symposia beginning about the 3rd MORS—perhaps all three of the 3rd, 4th, and 5th Symposia, or at any rate two of them. These were all out on the West Coast, and I was there myself on my first two OEG field assignments, as I mentioned. Then the next MORS I attended was the 9th, which was held at Fort Monroe in Virginia in (I think) the spring of 1962, and that was where I first began to participate actively. I was in the Command and Control working group, and I remember tangling with Herman Kahn on the subject of Command and Control—if trying to have a discussion with Herman Kahn can be called tangling with him. Perhaps as a result, I found myself on the Board of Directors later that year. About 1965, several of us on the Board (I will not identify the others to protect the guilty) began to explore the possibility of having MORS merge with ORSA, with the classified symposia to be run under contract with that organization. However, this idea did not find favor—which is code for having been universally hated—and so we contented ourselves with being the charter members of the new Military Applications Section of ORSA, but I am afraid I did not ever become really active in

that organization. I continued on the MORS board, where I was elected Vice President but did not accede to the presidency, and with MORS activities generally.

BOB SHELDON: What were these activities?

ERVIN KAPOS: They included presenting a significant number of papers on my own and with co-authors, serving as Command & Control working group co-chair and chairman, serving as Contributed Papers chairman and, I believe, as General Sessions co-chair. At this point, I went off on another OEG field assignment in 1966, to CINCPACFLT, and so on. In any event, I remained active in MORS until perhaps the 40th MORS, not attending every symposium but the vast majority. Then I fell off the wagon and did not participate in any further symposia—until I agreed to attend the Heritage Session at the Quantico meeting.

BOB SHELDON: How have the Symposia evolved in recent years?

ERVIN KAPOS: For one thing, the number of attendees is startlingly larger, to the point where I suspect it may soon become necessary to start limiting it, if it does not level off. Secondly, at least based on a very small sample of papers that come my way for information, the Working Group papers are of a much higher quality than they used to be. I presume that the General Session papers have similarly improved in quality. Mind you, there are still a number that make one wonder why the author bothered, but I suspect that those will always be with us. All in all, the hard-working folks who put together the Symposia these days have every reason to feel pleased with themselves.

BOB SHELDON: Any other last comments?

ERVIN KAPOS: I don't think so. Anything else would be belaboring the point.

The following transcript is from the MORS Heritage Session at the 71st MORS Symposium at Quantico, Virginia, 11 June 2003.

First speaker: Ervin Kapos

I stand before you in truth both proud and humble because this is my first official introduction as a fossil. Actually, being a fossil is more complicated than you might suppose because it occurs to me that I am not sure whether to be humble about my pride or be proud of my humility. I don't propose to resolve that on your time, but I do have to think about it.

I probably have the flimsiest claim of any of the speakers to be here. I was the director of the Marine Corps Operations Analysis Group of the Center for Naval Analysis for thirteen months, thirty-five years ago. For five of those thirteen months I was also doing my previous job as director of the South East Asia Combat Analysis Division of the Operations Evaluation Group. So, a lot of that time, of those five months, I was unconscious. The rest of the time I probably wasn't paying as much attention as I should have been, because I have relatively few recollections.

I was told to reminisce. So we will do a little basic reminiscing. Then I would like to talk a little bit about my perception of how analysis around the Marine Corps has evolved. I am tempted to be a smart-aleck and say "not much", but that would not be fair. It is not a question of evolution onward and upward so much as it is a question of some sea changes, most of which have been more good than bad. And, by way of full disclosure, for most of these intervening thirty-five years I have not been much of a direct participant in Marine Corps matters or Marine Corps-oriented analysis. I have been mostly an observer. I have mostly been a leech, sucking off other people's experiences, and so there is very limited first-hand content to any of this. People are used to listening to me with a grain of salt; I suggest two grains today.

Just to cover the territory before my Marine Corps Operations Analysis Group (MCOAG) tour briefly, I started in this business almost forty-five years ago on August 10, 1958, which if we reckon really quickly is forty-four years and nine months and one day and, by now, roughly six hours—but who's counting? I joined the Operations Evaluation Group in the days before there was a Center for Naval Analysis around it. I went through the requisite hazing during my first year of doing a couple projects I was incompetent to do, and then I went off to the field. I spent a year at the Operational Test and Evaluation Force in the Pacific and a year on First Fleet staff. Then I came back to Washington and established a Command and Control analysis team in the Operations Evaluation Group.

After a few years of running that, I went off to CINCPACFLT during the war—Bruce Powers joined me there I believe—and then came back and established a South East Asia Combat Analysis Division in OEG. I ran that for thirteen months, but on a Monday I was told that, as of that day, I was the Director of the Marine Corps Operations Analysis Group. (As it turned out, I continued to run SEACAD in parallel with my MCOAG duties for a further five months, until Phil DePoy was available to relieve me.)

I was innocent of any Marine Corps background. What I knew about the Marine Corps was essentially what one picked up in the course of normal professional activity around the Navy, which was not a lot. I had been out drinking with the Fleet Marine Officer a couple of times, and that was probably more useful. However, I was at no disadvantage relative to Headquarters, Marine Corps because they had about the same amount of notice as I did that I was showing up; they knew my name and that was about all. There was a Deputy Chief of Staff for Research, Development & Studies, at that time Brigadier General (later Lieutenant General) Lou Metzger; and he had a Chief Scientist, Al Slafkosky, now retired. They were the people I reported to in the Marine Corps.

I was in the business of learning about Marine Corps analysis because, if I knew about the Marine Corps what one picked up casually in the course of normal activity, I didn't know anything about Marine Corps analysis. The Marine Corps Operations Analysis Group was a few years old at that point, having been founded as a division of the Operations Evaluation Group, not then of the Center for Naval Analysis for the simple reasons that there was not a Center for Naval Analysis. With the establishment of the Center for Naval Analysis, it ultimately became an independent group. Its first director in its OEG days, and continuing on under CNA, was Russ Coile, retired now but still active professionally though not in military OR; and I was the second. The group was some twenty people strong, plus or minus two. There were a couple of active duty Marines attached to us, not analysts but bravely doing their utmost. At some point, I seem to remember some guy named Major Al Gray showing up to see if there was anything he could do to help—

maybe I just dreamed it. In any event, he went away relatively quickly, as there were presumably more useful things for him to do.

The program was sort of interesting. At that point, there was a small group down here at Quantico, helping with evaluations and experiments and things like that. I think five people. And there were maybe three or four people up in CNA headquarters, doing Headquarters-Marine Corps-ish small analyses; let's call them chores for lack of better word. And over 50% of the group was doing a giant input/output model of the entire Marine Corps, a Leontieff model. This took me aback a little bit. I didn't recall reading about Leontieff models in "Search and Screening", which is what all of us kind of grew up on. I do not think even the Naval Postgraduate School teaches Leontieff models, although I will not insist on that. But, existence proof, there was at least one project in which people actually tried to do something with a Leontieff model.

Three things were striking about this. First, this was a very big project for the normal scale of things at that time. The second was it was not clear that it was going anywhere, at least not to me. The third was that in 1968, at what might be reasonably called the height of the Vietnam unpleasantness, there was no combat analysis going on. There was no operational data flowing into MCOAG. There were no analysts out collecting anything. There was nothing being done. I thought this was sort of dramatic, particularly since I had just come from running the South East Asia Combat Analysis Division, which had been set up at the express behest of the Chief of Naval Operations. So after a short of period of mutual acclimatization, four or five days, I went back to the Deputy Chief of Staff for Research, Development & Studies, and proposed cutting the input/output model effort in half, and taking the effort that was freed up and putting it into Combat Analysis.

This got a bit of attention for a couple of reasons. First, there were an awful lot of rice bowls tied up with a project of that size. I had been in the trade not quite ten years at that point and I did not understand about rice bowls. (If anybody would like a short course on rice bowls, do come and see me: I have had 35 years since then to get to understand them.) So

all of the owners of rice bowls rose up in rage. The second reason why this got some attention was that it clearly entailed putting analysts in the combat zone; I was not proposing that somebody feed us data and we would analyze it. That didn't seem reasonable, particularly in view of the need for analysts to understand the environment in which the data was generated. I was proposing that we send some people out, get some data, do some analysis forward, and also do some analysis back here. And this is a point I would like to emphasize. This was considered to be revolutionary at the time. It's not revolutionary any more; it is accepted as a *modus operandi*, which is a heck of a good thing. You know how we are inclined to grumble about how things don't progress? If you would like an existence proof for real progress, the fact that this is a normal *modus operandi* right now is not a bad existence proof.

The answer was absolutely not. After a couple of weeks, it turned out to be relatively not. And after a couple of months, we had a couple of people in Vietnam. I am not going to pretend to you that we accomplished very much; in fact arguably we didn't really accomplish anything except establish a proof of principle, if you like. There was some analysis done proving the point that it could be done in-country; and there were some resources pulled out of Mr. Leontieff's pocket and put into the applied operations analysis pocket. But I was only there for thirteen months. If I had the makings of somebody who would revolutionize Operations Analysis in the Marine Corps—and that's a big IF—I didn't have the time. But let's not poormouth me: I did make some changes. I was relieved by Bob Gigliotti, and I went back to be director of the Operations Evaluation Group. After a few years of running that, I went off into the private sector, where I spent the next 28 years before abruptly turning into a fed two and a half years ago.

The point of this recitation is that, ever thereafter, I did not have very much to do directly with analysis in the Marine Corps, which is why I am limited in the amount of reminiscing I can do. To be sure, in later years, after my tour in OEG, I was involved with some Marine Corps things. We did some analysis and, by the way, looking just on the instant at

the overall complexion of the things I was involved with, they turned out to have been rather war gaming heavy. And there is, I think, a useful object lesson hidden there, too, an evolutionary lesson—let me get back to that in a moment. Sticking to the chronology for the moment, I did get to do some analysis and then some war gaming with colleagues on counterterrorism—not anti-terrorism, counter-terrorism. The Marines had established the notion of the special operations capable Marine Expeditionary Unit, the MEU(SOC). It turned out that there was a rather delicate interaction between an already deployed MEU(SOC) and another joint mission that would deploy on the occurrence of an event, and so the Commandant, who was by now General Gray, decided that this interface needed to be explored—defined before somebody got killed—and that war gaming (he had been exposed to some of my games when he was in the Fleet Marine Force) would be a good way to do it. It was fascinating stuff.

Now for the point I would like to make about war gaming—I might as well make it now as later—is this. A war game, of course, is not analysis, we all know that. Do we? I hope we do. I mean, it's one iteration of a Monte Carlo process that you would like to have 500 iterations of. Well, you're going to get three if you're good; you're going to get three if you're lucky. And you will get five if you're both good and lucky, because that is the way of the world. So the analytical potential in any rigorous sense of war gaming, I will assert, is pretty limited. It is nonetheless a very good way of exploring structures and processes, and getting enough definition for vague problems so that you can begin to model them more rigorously, whereas you couldn't perhaps do that without war gaming. The Marines have, over the years, turned more and more heavily to war gaming. That's a good thing. They have gone through various iterations of heavily model-supported games, to not at all model-supported games; closed games and seminar-type games. It doesn't much matter, because it is all in support of analysis—as long as the analysis gets done.

In my present practice, in the Office of Naval Research, I view war gaming as what I call the 'analytical stiletto' for people who view

analytical studies as a blunt instrument that somebody is about to wield on their heads. Even those people can be made to be receptive to games which are relatively concrete, they're participatory. And what one doesn't need to make a big fuss about is that, if they are set up on some reasonably rigorous quasi-analytical basis, they can provide very useful insights about certain processes and maybe a basis for hypotheses about outcomes. It does not feel like analysis at all. You're not being hit over the head. It can be inserted between your ribs—the analytical stiletto.

Then in 1986, I got the opportunity to do an analytical study on, hold your hats, Marine Corps General Officer Requirements. That was an interesting thing to be willing to subject to Operations Analysis. Was it the millennium? No, in 1986 it was not the millennium, but you certainly could confuse it for the millennium. The story on that was that the Navy, sensing a Congressional mandate coming, decided to start looking at the number of flag officers it needed. And somehow—the how having a lot to do with who knew somebody, who knew somebody, who knew somebody, who knew me and thought I could do it—I got the opportunity to set up that study.

There didn't seem to be any precedents, and I was not given the time to do a whole lot of homework. So I went ahead and invented a methodology, which seemed to go down well. When I briefed the then-commandant, General P.X. Kelly, on the study, he said, "What would it take for you to do this for the Marine Corps?" Now remember the Navy paid for the methodology. So the hint was, keep it cheap. So for a price that was approximately one-eighth of the Navy study, I did a Marine Corps analysis. Why is one-eighth important? Because the normal Navy/Marine Corps ratio in flag/general officers is four to one, and so we have that to show that the study was a real bargain. The results went down quite well. Both with the Marine Corps and with the Congress, and I was very happy and went back to sleep. Ten years later, I got to do it again. No good deed goes unpunished!

The interesting thing is that not much happened in the wake of the study in 1986–87; but in 1996–97, the Marines marched up on the Hill

with the analysis—with their own judgment superimposed, which was to scale back their request from what I had recommended—and they came away with twelve additional general officer billets which, on a base of 58, was not too shabby. There are guys out there that think I am really neat for having pulled that off—which I really didn't do—and some of them are still on active duty, after all it has only been six years. But this was interesting stuff to do; I think more importantly, it blazed some interesting trails in willingness to submit even unlikely issues to rigorous—how rigorous can I be?—quasi-rigorous analysis. (Let me explain quasi-rigorous. It is not as good as rigorous, but is better than pseudo-rigorous.) So my work blazed some trails in willingness to submit these kinds of pretty sensitive, almost social, issues to quasi-rigorous analysis.

A bit later, I was doing some war gaming for the Navy, actually for OSD and then for the Navy, on Y2K. Remember Y2K, the disaster that didn't happen? Maybe because we prevented it from happening? Well, I produced some games. The Marine Corps participated in a DON game—because of proportional representation, some of them were invited. And their conclusion was, "Gee, this might not be bad for us." So, we ran a couple of Y2K games. One of the participants is the excellent landlord of this place where we are meeting, Lieutenant General Ed Hanlon, who came away with a good taste in his mouth. This is terribly important. War gaming can get you lots of enemies who will be more determined than your admirers typically; so you can use two admirers for one enemy!

And that, until very recently, has been just about the sum total of what I have done as an analyst around the Marine Corps. Not very much is it? I told you right at the outset, I have the flimsiest of claims to be up here.

At the beginning of 2001, I joined the Office of Naval Research, as Director of a new Operations Analysis program. ONR had never had an Operations Analysis program. Arguably it still doesn't, but there is hope. Within ONR is a segment of the program which is Marine Corps-oriented: there is a segment of the 6.1/6.2 basic science and technology program, and then there are two Future Naval Capabilities

programs—FNCs are more applied, focusing on technologies that can transition into acquisition—which are Marine Corps-oriented.

I have a charter to evaluate FNCs. Who wrote me that charter? Nice thing about being the first in an office, I did it and nobody said no. Here is where the analytical stiletto came in. The so-called Littoral Combat FNC was just forming, and I said, “How about a utility analysis, a military utility analysis?” And they said, “Give us a break, we are not even started yet.” I said, “Well, how about a game to define your problem set?” “Oh, that is not such a bad idea.” The next step was, “How about an analysis of the game?” “Well, all right.” That didn’t come out too badly. So pretty soon there was another game. I must give the credit for the initiation of these games to the Marine Corps people in ONR because it took their being willing to take the risk to do them. Pretty soon there was another game to define the Marine Corps interest in Sea Basing. If you don’t speak from Sea Basing, look up CNO’s Sea Power 21, the Sea Basing pillar of that nice “evolutionary” concept of what has been done with the Navy/Marine forces for a very long time. But nonetheless, it produced a good game, also a good analysis, which is about to be published.

So we are making progress in bringing war gaming and analysis into closer confluence, which I think is important. I mean, I would expect analysis to define what needs to be gamed because it can’t quite analyze it yet, and I would expect the gaming to lay the foundations for analysis that you couldn’t quite do earlier—and so on in a rolling cycle, which is a matter of perpetual employment. It should go on forever.

I think things are going that way, and I think that is very healthy. There are issues of what things cost: are war games expensive relative to analysis? Are they cheap relative to analysis? I don’t want to get into that because that depends a lot on how you do either one or both, and I have nothing really useful to say on that subject.

Meanwhile we’ve had a Gulf War and we have had another Gulf-ish war, although we don’t call it that, and we have had a war in Afghanistan. And the Marine Corps has systematically deployed analysts, uniformed guys

and civilian people from CNA, from MCOAG. It is taken for granted. This is a really good thing. We have evolved a long way. We have a good way to evolve yet. And I would like to go ahead and sling a couple of ideas—not darts, but a couple of ideas—out there regarding things that we could stand to do better.

First, the analysis that is done on these combat operations tends to be focused on very tightly defined needs that somebody defined. There is not very much looking at data to discover problems and solutions for those problems. The focus tends to be over-defined and perhaps overly focused on Washington needs, take it for what it’s worth. But, of course, the Marines have very limited studies and analysis resources. The Marine Corps studies program is very tightly defined. I mean it is sliced and diced into little blocks, and over-specified. I think that, I hope that, things will continue to evolve in the direction of letting the business of answering pre-defined questions yield to some extent to just messing around with the data in a discovery mode. These two things will not, need not, become equally important, but I would like to see it be something other than 100% and 0%. What’s the right ratio? I don’t know, cut your own. Three to one is not bad as far as I am concerned, because you’re not talking about a huge block of resources and there is a question of critical mass on the lower side, on the smaller side. But I don’t care what the ratio is. What I do care about is that there should be a philosophical viewpoint, if you like, that we are going to divide up the pie in this fashion. Look! Yes, we understand the problems well enough so that we can—we, the non-analysts can—define them down to a gnat’s eyelash for the analysts who don’t understand. Yet, the problem is to discover what we don’t understand. That is so obvious it’s hardly worth stating, but it is important nevertheless to realize it.

A second observation has to do with the dominance of the database. My goodness, but are we in love with databases—big structured databases. This is a love affair, which means that we throw away data because they don’t fit the pre-conceived structure of the database. Once we have thrown the data away, there is not much discovering to be done. I would like

to see big structured databases happen second, not first—after the data is well enough understood so that it can be properly annotated as to conditions and so on, and therefore can be flexibly used. As it is, stuff is piled in as soon as it is gotten. I think it is pretty untidy. The Navy learned this lesson with the old Fleet ASW Data Analysis Program, FADAP, and has re-learned it periodically. It is a lesson that I think necessarily has to be re-learned periodically. But, again, you can't go out there and collect everything—the vacuum sweeper approach, the Hoover approach, to data. I know that. But when you do collect data, don't store it in structured databases before you have tried to do analysis with it, some analysis with it. Structure your database after you understand the character and the uses for the data. There is, right now—remember, I watch at a distance—there

is a kind of tyranny of the database, which I would very much like to see broken.

Finally, experimentation and the role of analysis in that. The Marines have a pretty good experimentation process at the Marine Corps War Fighting Lab—where they have a hefty representation of both uniformed and CNA analysts. (Footnote: I tried to break in there when I was a contractor and didn't succeed, so there must be something wrong.) And they have a kind of an SOP for how do you define a problem and how do you set up an analysis. It seems a little simplistic; it is almost at field manual level. But then I looked at this and I asked myself, "How would I do this better?" The answer is, "I wouldn't." It's a pretty good way of doing things. The Navy is not anywhere near on as solid ground. And so that has been, I think, pretty close to exemplary.