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Military Operations Research Society (MORS) Oral History Interview

Paul K. Davis Interview (MORS)

Davis, Paul K.

Monterey, California: Naval Postgraduate School.

INTRODUCTION

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

Dr. Paul K. Davis is a senior principal researcher at the RAND Corporation. Paul was the MORS Wanner Award laureate in 1997 and was elected a MORS Fellow of the Society (FS) in 2012. The interview was conducted December 17, 2015 at the RAND office in Arlington, Virginia.

MORS ORAL HISTORY

Interview with Dr. Paul K. Davis, FS. Dr. Bob Sheldon, FS, interviewer.

Bob Sheldon: This is December 17, 2015, and we're here for a MORS oral history interview with Paul Davis. Paul, first of all, please give us your parents' names.

Paul Davis: Ruth Gledhill and Paul Davis.

Bob Sheldon: What kind of influence did your parents have on you?

Paul Davis: The narrow answer is that they had no influence on what I did, where I went to school, anything like that. But they had a broad influence. Both had some college, considered themselves educated, and expected their kids would go to college and then do well. There always were books everywhere, which everyone in the family read. They had no interest in or knowledge of science. Science is just what I decided to do and that was fine with them.

Bob Sheldon: Where did you go to grade school and junior high and high school?

Paul Davis: I grew up in a fairly small town in northeastern Ohio called Niles (16,000 people). We had lots of things to do: Boy Scouts and Explorer Scouts, YMCA, swimming pools, and a lot of things that you associate with "Happy Days." I walked to all of my schools and knew many classmates from first grade on (and some others from ninth grade on).

Bob Sheldon: Did you take an early interest in math and science?

Paul Davis: Math came easily and I didn't think about it. I was busy playing, and reading books that were more fun—historical fiction, mystery stories, adventure, biographies, war books (e.g., *Run Silent, Run Deep*), even books on ESP and flying saucers. It wasn't until high school that I became interested in math and science.

Bob Sheldon: Did you take a full range of math in high school, up through calculus?

Paul Davis: We had plane geometry, solid geometry, algebra, trigonometry, and a year of analytic geometry and calculus. Both my math teachers were retired military. My friends and I loved them. They were proud, patriotic, smart, and tough on us. And they loved math. I suspect that many retiring military officers would find teaching to be very rewarding (except for money). I taught calculus when I was in grad school and found it delightful.

Bob Sheldon: What about science?

Paul Davis: Biology, chemistry, and physics. Also, I got up at 6 a.m. to watch televised college-level courses, "Sunrise Semester," in chemistry and math. I didn't take them for credit, but I got the texts, which were more interesting than my high school books.

Bob Sheldon: Were you involved in any extracurricular activities in high school?

Paul Davis: I wish that I could say football and basketball because all of us in Niles were avid fans of both, but I was not suited for those after junior high. But there was tennis, driveway-level basketball, chorus, Boys State, Junior Red Cross, and other community activities. I also worked from junior high on, so that was part of my extracurricular stuff.

Bob Sheldon: What kind of work?

Paul Davis: First I was a newspaper boy. Back then, some of us would carry papers in bags and walk for miles. Also, lawn mowing, working in a department store, and even some selling (which I didn't enjoy). It was what I could do as a kid.

Bob Sheldon: How did you choose your college?

Paul Davis: Nobody in my family or school had a clue about where to go to college without money; and we had none. My principal urged me to apply to West Point, which would have been great, but I was not physically constructed for that kind of thing. Two close friends, however, went to Annapolis and the Air Force Academy. Anyway, I bought some books and applied to a bunch of schools without really knowing what I was doing in detail (there were no books on the top 50 schools back then that I knew of). As it happened, I had

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a lot of great choices and the question was where to go.

I ended up going to University of Michigan, which was a wise choice. Part of it was pragmatic: they gave me a full scholarship. Beyond that, visiting the university was like seeing into a new, big wide world. I never looked back.

Bob Sheldon: What year did you start at Ann Arbor?

Paul Davis: 1961.

Bob Sheldon: Did you declare a major when you started there?

Paul Davis: Chemistry, tentatively. I was in the College of Literature, Science, and the Arts. That included the sciences, but also English, philosophy, history, and so forth. Pretty quickly I became a confirmed chemistry major. That was because of the interest from high school, especially in chemistry theory.

Bob Sheldon: What did you take as a chemistry major?

Paul Davis: We had general, inorganic, organic, and physical chemistry, and lots of laboratories. I enjoyed those, but wasn't as good at them. I had to step back and think about the experiments before I walked in, whereas some of my friends were like natural chefs. If I didn't prepare well, I'd mess things up or break glass.

I also took a fair amount of physics and math, and also some liberal arts courses in English, philosophy, history, and constitutional law.

Bob Sheldon: Did you have any particularly notable professors in your undergraduate studies at Michigan?

Paul Davis: Michigan did well by its undergraduates. A philosophy professor, Charles Stevenson, liked teaching undergraduates because they hadn't yet confronted the issues that he had devoted his career to. He told us that if we had some great deep thoughts as we wrote our papers, they were "ours," even if a Greek philosopher had had the same thoughts long ago. That was comforting and I remember it today. A more famous professor, Abraham Kaplan, had studied under Rudolf Carnap and Bertrand Russell, and in the Far East. He epitomized breadth and taught a comparative political philosophy course, which was deeply insightful. As Kaplan said of himself: "I am by training a positivist, by inclination a pragmatist, in temperament a mystic, in practice a democrat; my

faith Jewish, educated by Catholics, a habitual Protestant; born in Europe, raised in the Midwest, hardened in the East, softened in California, and living in Israel." One chemistry professor (Seymour Blinder) was very impressive to me and a small group of theory-oriented friends. I also had an excellent organic chemistry professor. The first semester was all theory and it was wonderful. We used a textbook by Donald Cram and George Hammond (UCLA and Cal Tech). Cram later got a Nobel Prize. You can understand a great deal of organic chemistry from the theory. But then came all the stuff that you couldn't understand. That was the art, the alchemy. It didn't do much for me.

Bob Sheldon: What were your thoughts coming up on graduation in 1965?

Paul Davis: I thought about a lot of things. I took the Law School Aptitude Test and got the application for Harvard. My roommate, who later went to Harvard, always insisted that I should have gone that route and would have loved it. I also looked at biophysics. I applied to several schools and was accepted, but when I got accepted at MIT (the Massachusetts Institute of Technology) and read up on some of the things that they did in the Department of Chemistry, I liked that and decided to go there. I certainly did not do a detailed decision analysis, but the decision felt good and it turned out well.

Bob Sheldon: What was your specialty at MIT?

Paul Davis: Within the department, there was inorganic, organic, and physical chemistry. And within physical chemistry there was chemical physics and physical chemistry. I gravitated into theoretical chemical physics, into what's called "statistical physics" or "statistical mechanics." The first year of classes was broad and some was great fun: kinetic theory of gases, spectroscopy, and statistical mechanics. Also, teaching freshmen. Some of my best courses, however, were in the physics department. The graduate-level quantum mechanics course was profound and inspirational. I also sat in on lectures in general relativity by Steven Weinberg, who later got a Nobel Prize. Beautiful stuff.

Bob Sheldon: Did you go there intending just to get a master's degree or did you apply for the PhD program?

Paul Davis: In chemistry, a master's degree was a consolation prize, so I never got a master's.

Bob Sheldon: How did you choose your dissertation topic?

Paul Davis: I chose a professor, Irwin Oppenheim. After enjoying his first course, I asked whether he could take me on as a student. He would give frightening verbal tests to scare us off, and draw curves pointing out that most people who try to contribute to his kind of work had a net negative effect on the world. But if we didn't scare off, then he would take us on.

He would try out a problem on a student, and—if it resonated—that became their dissertation topic. If it didn't, then he would try a different one. The first problem I had was appropriate for a pure mathematician who didn't really care about the physics or chemistry. I had neither the mathematical brilliance nor the interest to appreciate it, so he changed gears and suggested a problem that I spent the next several years on. It was also deeply mathematical, but close to fundamental physics and chemistry. It required using a version of quantum mechanics that related well to abstract classical physics. It traced back to Eugene Wigner in the early 1930s. There was, let's say, a "significant learning curve," but after that the theory was beautiful and remarkably intuitive.

Oppenheim died last year and his old graduate students got together in Cambridge for a celebration of his life. He was a brilliant scientist, fine advisor, and an exceptionally broad human.

Bob Sheldon: How long did it take you to finish your PhD?

Paul Davis: Five years; most of that was dissertation.

Bob Sheldon: What was your dissertation topic?

Paul Davis: Stochastic Theories of Energy Transfer. It was about vibrational relaxation in gases and liquids. The liquids part was an add-on. I didn't realize that was coming. I thought I was done when I finished the gas part. Instead, I was told "That's great. Now do liquids and you're done." From his point of view, there had been some hints on how to go about the problem for gases, but for liquids there was no clue. So solving the problem was entirely on the graduate student. He once told me that he picked dissertation problems such that, if he personally focused on a given problem, he could crack it in six months. He figured a good student could do it in a few years. That

was a bit humbling to hear, but accurate for my cohort of students.

Bob Sheldon: How much of your dissertation work was in the lab versus theoretical?

Paul Davis: Mine was strictly theoretical, except that I had to look around to see whether there was experimental confirmation of what I was coming up with. There wasn't: the more meaningful experiments hadn't been done yet. Some required unusual isotopes with special characteristics. That was a bit disappointing, but I had no doubt about the dissertation being correct.

Bob Sheldon: Finishing up your PhD, what were your job plans?

Paul Davis: Academic work. I took a two-year postdoc at the University of Chicago and found myself bored. It wasn't as interesting as at MIT, but more importantly, I was looking at the folks who were a year ahead of me trying to get real jobs. They were going to mediocre schools, or to very good colleges with no research. I concluded I wouldn't be satisfied with either.

A friend then called and asked me to apply to the Institute for Defense Analyses (IDA). Reading the Enthoven-Smith book, *How Much Is Enough? Shaping the Defense Program, 1961–1969*, on the airplane, I concluded that there was important work to be done in defense analysis. I then met the folks at IDA and thought, "These are really interesting people." So the next thing I knew, I had taken a job. I had also gotten a more lucrative offer in the chemistry industry, but I had turned a corner.

Bob Sheldon: What year was that?

Paul Davis: 1972.

Bob Sheldon: Who did you interview with at IDA?

Paul Davis: The managers were Bob Fox and Bill Schultis. Bill was an excellent systems analyst who would see through the fog to the essence. The hiring committee included Roger Molander, Philip Selwyn, and Fenner Milton—all of whom later had distinguished careers in defense research. It was an impressive bunch of people. A lot of decisions get based on whether you like the people.

Bob Sheldon: What kind of project did they assign you initially?

Paul Davis: The Navy was just beginning to field the F-14, which was like the F-35

today—controversial, expensive, very cutting edge. OSD (Office of Secretary of Defense) didn't really know what the airplane could and couldn't do (the Navy kept information close hold). My job was to do an assessment for OSD on how we should look at the F-14 and anticipate what upgrades might be needed down the pike. So I spent time with the NAVAIR (Naval Air Systems Command) people, learning the rudiments of air defense and so on, trying to be structured and candid about what the airplane could and could not do, what the limitations were, and what might therefore be needed later. So, this was where I first learned systems analysis.

Bob Sheldon: Did you have to learn how to study air-to-air combat and air-to-ground combat?

Paul Davis: My project was more elementary, things like, "What does it take to maintain combat air patrol at various distances?" "How big an attack on the air fleet could be defended against?" The worry was attack by Soviet bombers, which were getting standoff missiles. So it was that level of analysis: not very hard, but interesting, technical, and consequential.

I then started working on a DARPA (Defense Advanced Research Projects Agency) program that had to do with satellite observations of rocket exhaust plumes. We and the Soviets would launch missiles, which could be tracked with satellites and their exhaust plumes observed, perhaps suggesting things about missile characteristics (I worked out a theory for estimating throw-weight from such data). The program had a lot of experimental work as well as theory. IDA was advising at a high level. Hans Wolfhard, a respected scientist who had emigrated from Germany after World War II, was in charge and was also my mentor. I was able to work with people who were doing outstanding aerodynamic, chemical dynamics, and empirical work, but who were unable to see the whole because they were so deep into their own work. The program was fragmented, but—as sometimes happens in a DARPA program with first-rate people—it fell together.

Bob Sheldon: Was it classified compartmentalized?

Paul Davis: It had been, but it became just classified.

Bob Sheldon: Who did you brief the results of your analysis to?

Paul Davis: The program manager at DARPA and the head of the office of Strategic Technology. In a way, however, the whole community of people that had been contributing to the program were my customers, because the IDA job was to pull things together. People were depressed because they couldn't understand the empirical observations that had contradictory conclusions. But I was able to put together a remarkably simple model, which was motivated by work with the individuals about how to simplify what they had done, and then putting this together in a formula model that made sense. Given the simplifications, it was supposed to be for explanation and communication. However, it also fit the data extremely well. When I briefed the model and how it integrated the various strands of work, the many contributors to the program were really excited (and relieved). That was a high for me.

Ironically, the boss at DARPA had concluded the program was a failure. And then we came in with this report where everything fell together. That discombobulated him for a while.

Bob Sheldon: How many rocket launches did you have data on?

Paul Davis: Probably tens, between American and Soviet launches. We also had experiments from aircraft and shock-tube laboratory experiments.

Bob Sheldon: What was your next project at IDA?

Paul Davis: I did some work for NASA, but then in 1975 I went into the government. This was back in the days of strategic arms control, which seemed to me deeply worthwhile despite my strong pro-defense attitudes. I took a risk and went to work for the Arms Control and Disarmament Agency (ACDA), knowing that I couldn't stay long or I'd be tainted for future defense work. The very name "Arms Control and Disarmament" made me nervous because I had never believed in total disarmament (or even eliminating nuclear weapons). But ACDA was a big player in the analysis that underlay the strategic talks. Each study had a rep from OSD, JCS (Joint Chiefs of Staff), State, CIA (Central Intelligence Agency), and ACDA. ACDA was a small agency, so a young guy could get into the action fast.

Bob Sheldon: Was that part of the State Department at the time?

Paul Davis: It was administratively attached to the State Department, but was a separate agency by legislation.

A lucky thing was that, at the time, scientists could inform policy because some of the issues were, e.g., how can we limit and verify the throw weight of the next-generation nuclear missiles, how can we verifiably define a MIRV (multiple independently targetable re-entry vehicle), how would we expect the Soviets to exploit loopholes in language or actually cheat, or is Minuteman really vulnerable? Later, the issues become more political.

Bob Sheldon: When you were at ACDA, was Al Lieberman, FS there?

Paul Davis: Al headed the operations research division. I was in a different group that was closer to policy and strategy. We cooperated. I remember liking Al very much and admiring his integrity (some of the higher-ups didn't like some of the results of good analysis).

While housed in ACDA, I was writing interagency papers for the National Security Council (NSC). I would have a co-author from OSD, Joint Staff, CIA, or State. There were fierce, even emotional arguments. But people came to respect each other. I can remember being at a table over in the Old Executive Office Building when one of the military people accused one of the civilians on the other side of the table of being a communist. But we had the proverbial beers together afterward. By the way, those of us who interacted with Soviet scientists came also to respect them highly. There is no substitute for face-to-face meetings with adversaries (as well as allies).

I also did some work on far-out space issues, involving antisatellite systems, possible arms control in space, etc.

Anyway, in 1977, I was asked to go over to OSD Program Analysis & Evaluation (PA&E) (now CAPE, Cost Assessment and Program Evaluation) to work in the strategic nuclear analysis shop on both force planning and arms control.

Bob Sheldon: While at ACDA or OSD did you get to know Lieutenant General Glenn Kent?

Paul Davis: I didn't know Glenn until later, at RAND, where we spent many, many happy hours talking, debating, and sometimes working through logic or math. I doubt that anyone ever "won" a debate with him, but I held my own and we had a good time. While I was in OSD, an important player was Major General Jasper Welch. He had succeeded Kent as head of Air Force Studies and Analysis (AFSA). There was some OSD-AF tension, of course, but also good cooperation among analysts. We were all building programs and protecting the nation. It was the Cold War with serious things to worry about.

Oddly, some of the issues have come back. For example, our big study in 1978 was whether to modernize the ICBM force or scrap it, and whether to have a Triad. The same issues have come up again 40 years later.

Bob Sheldon: How long were you there at OSD?

Paul Davis: After a year or so doing strategic nuclear planning and thinking, "I really understand this stuff," the head of PA&E, Russell Murray (a terrific analyst and leader), insisted that I become a senior executive and take a division to do something completely different. The division was called Special Regional Studies (I'm not sure why). We were actually looking at the Persian Gulf region. The NSC, Zbigniew Brzezinski in particular, had asked the Department of Defense (DoD) to look at US problems in that part of the world. So I was put in charge of the division that did that, under Paul Wolfowitz, the Deputy for Regional Programs. So I became a strategic analyst in a different sense. I had to learn about ground warfare, strategic mobility, etc. Wow, the ignorance of a civilian when first looking at ground warfare! Fortunately, I had some young colonels assigned to the office, and they would patiently tutor me on things such as why armies don't move nearly as fast as one might think, or how army officers look at a map and think out what forces would be needed for a particular situation. So I had a lot to learn, but I could think and write; apparently, I had a sense of strategy. I also benefited from working for Paul Wolfowitz and often with Andy Marshall, the Director of Net Assessment, both of whom I have always greatly respected and liked.

That was a great period because we were filling a vacuum. The US had no military capability in the Persian Gulf. So we did a big study (the "Wolfowitz study") about different things that could happen, no one of which was

expected. We were doing capabilities analysis and we ended up recommending more sealift, more airlift, expeditionary forces, base improvements, and even a new command. All of this "happened." The command evolved into USCENTCOM (United States Central Command). The capabilities we programmed were used in the first Gulf War.

I've done both building and cutting; it's more fun to build. To do so, however, requires organizations and people pulling together. There were fine innovations from the Marines, particularly General P.X. Kelly, who later became Commandant, from mobility folks, and others. We would stitch such ideas together as part of a capability-building strategy. We worked closely with the Under Secretary of Defense for Policy, Robert Komer (he was my "second boss" since I did both program analysis and strategy work).

Our success was partly luck. In 1980, the political winds changed and DoD got more money. The things that we'd studied and suggested were on the shelf and ready, so they could be moved into the budget almost overnight. How often in a career do you get that?!!

Bob Sheldon: Were you doing this during the transition from the Carter administration to the Reagan administration, when the plus-ups came in?

Paul Davis: There was a big shift in the Carter administration itself, from continuing decline to badly needed real growth as of FY 1980. The Reagan administration threw more money at the problems. In their heart of hearts, the incoming officials probably wanted to kill everything from the previous administration (standard foolishness during transitions, for both parties). But the things that we were recommending—even if from the previous administration—were attractive. The new officials liked them and wanted to do more.

Bob Sheldon: Did you travel to the Mideast to look at any of those basing concepts?

Paul Davis: No, my staff officers did, but I did not.

By the way, the best story about that period was that in our analysis we had no notion about being able to predict the future. We were talking about vacuums to fill with capability. So we literally had a scenario in which Saddam Hussein

invaded Kuwait. Everybody outside our group said, "That will never happen!" We had a scenario where the Soviets would invade Iran, even though they hadn't done that in decades. "Ridiculous." We had an Arab-Israeli scenario, and so forth. We were looking for common patterns of what capabilities made sense. In the early part of this period, the conventional wisdom in the rest of the Pentagon was that *conceivably* some capability against Saddam Hussein might be useful, but probably not. The Soviets? Impossible! But then the Soviets invaded Afghanistan and, *overnight*, conventional wisdom flipped and senior leaders focused entirely on the Soviet threat to Iran. But the war that actually occurred 10 years later was when Saddam invaded Kuwait. This shows the goofiness of conventional wisdom and the strength of analysis that transcends scenarios. If you do the right analysis, you can do smart things without knowing how the capabilities will be used.

Bob Sheldon: Were you still working in that office in the early 1980s?

Paul Davis: Through 1981. For much of the last year, I headed regional programs more generally. Just like today, there were cosmic issues for the Secretary of Defense. The US has a bunch of possible threats. How should the DoD prepare for all the possible threats? Could we assume that wars would be separated? How do you pay for things? I think we did a reasonable job. I certainly respected the people who were making the decisions and understanding the issues. Under Carter, this was Secretary Harold Brown and his Under Secretary for Policy, Robert Komer; early in the Reagan administration it was Frank Carlucci, the Deputy Secretary (he later became National Security Advisor, and then Secretary). There were many good officials, in all three administrations during which I was in government (Ford, Carter, Reagan).

Bob Sheldon: So you were there until 1981. Where did you transition to next?

Paul Davis: I got a telephone call from Steve Drezner, a Vice President at RAND who wanted to take me to lunch. As a lark, I said sure, so we had lunch. He gave me all sorts of reasons why I should come to RAND. I hadn't considered leaving but thought to myself, "I've been at this a few years now and have had a good time. Maybe it's time to do something different." I

also recognized that the work in OSD, while consequential and sometimes heady, was not building deep intellectual capital. So I went to RAND. I considered another job offer, but RAND was the most attractive. Doing so opened another era in my life with lots of very interesting, very different problems, and good colleagues.

Bob Sheldon: So you were used to living in DC area, where the real estate's high priced, and you went to Santa Monica where it's even higher-priced. Was that a shock to your system moving out to California?

Paul Davis: When it came time to move and buy a house, the interest rates were 17 percent. Another shock was the change of climate. My wife was initially ''not real happy''; we moved in August, which is not the best time to move to the Los Angeles area. We discovered, however, that it was really pleasant. Now she probably wouldn't want to move to anyplace cold.

Bob Sheldon: Did the move to California disrupt your family's life?

Paul Davis: Not too bad. Our daughter was only 11 or 12, not in high school where I think moves are particularly hard.

Bob Sheldon: What did you start out working on there at RAND?

Paul Davis: The first big project was for Andy Marshall, the Director of Net Assessment. In 1980, a Defense Science Board had postulated the need for an improvement in wargaming and analysis that would go all the way from conventional to nuclear. They had a grandiose description. The DoD decided to do it, with personal approval of the Secretary of Defense. Andy Marshall was put in charge. There was a competition before I got there and RAND won the competition with an innovative scheme of building a simulation that included artificial intelligence models optionally, so you could either have people-in-the-loop or models-in-the-loop. I arrived on the scene and was recruited to be in charge. My deputy was Jim Winnefeld, a retired admiral, wise man, and gentleman who just died last week. He was the father of Admiral Winnefeld, who was Vice Chairman of the JCS from 2011 to 2015.

The result, the RAND Strategy Assessment System (RSAS), was a terrific effort technically and analytically. We had models making cosmic decisions for the Soviet Union and the United States (and their allies), and for third countries; we had joint combat models for everything from little contingencies to multitheater conflict that might escalate to general nuclear war. By about 1987 it was all working and I moved on, although the project continued under Bruce Bennett. This had required an enormous effort by a large and very talented team.

As intended, we deployed the system into government. It was used for some years to run Title 10 wargames, for education at several military schools, and so on. That said, it was not really used well for what Andy Marshall had had in mind initially. We did some of the highly classified analytic wargaming of nuclear war within RAND, as well as the escalatory transitions from conventional war, but there was no real market for such things in the DoD. The various offices represented on the project's steering group were far more interested in conventional conflict and conventional models. The more innovative features of the RSAS fell away over time.

Ultimately, the government was just not a suitable place for something as sophisticated as the RSAS. Despite heroic efforts, it was too complex with a lot of PhD-level work behind it. Within the USG (US Government), it was typically used for relatively standard scenarios. When we'd train up a sharp young officer in the Pentagon, who understood the system, he'd be promoted and shipped out. As for the most innovative aspects, with the national-command-level and military-command-level models, the USG lacked the interest and, bluntly, was unable easily to accommodate the need to have versions that represented contrary-view images of the Soviet Union. We did such work within RAND for Andy Marshall, but it didn't really deploy well. The Soviets then turned belly up and interest in the system went away in the early 1990s.

Aside from the Soviets going away, Andy had concluded that human gaming was a more efficient way to get at many of the issues (e.g., escalation dynamics and even some aspects of nuclear war fighting) than was complex simulation. I had reached the same conclusion; I found the complexity to be too burdensome and turned back to simpler methods.

Nonetheless, we had used the system for sizable studies for the Under Secretary for Policy and Andy Marshall. The most influential

aspects were recommendations for conventional arms control in Europe—first, for NATO to take a hard line on having the Soviets asymmetrically reduce forces and, later, to redirect emphasis to strategically important "operational arms control" that would reduce the likelihood of surprise attacks, rather than confidence-building measures of a more cosmetic character. As it happens, Gorbachev took unilateral actions and the issues became moot.

The combat model from the RSAS became the Joint Integrated Contingency Model (JICM), which is still used today within DoD. In RAND's usage of it, we included "soft" aspects of war (e.g., qualitative factors to represent low fighting effectiveness for given equipment), aggregate effect of maneuver rather than unrealistic attrition battles, and—most important—multiscenario analysis exploring the consequences of the numerous key parameters. This introduction of exploratory analysis was unique and laid the basis for the much more sophisticated uncertainty analysis that we have today. Key figures in developing JICM were Bruce Bennett and Carl Jones.

Bob Sheldon: What next?

Paul Davis: I moved on to other things after the RSAS had fallen together in 1988. In particular, I began worrying about the post-Cold War era and rogue-state problems. As I mentioned earlier, I also wanted to use methods and models that skimmed the cream of what we had learned, with much less complexity.

John Arquilla (now at NPS, the Naval Postgraduate School) and I did a very interesting study for the Joint Staff that anticipated much of what Saddam Hussein did in 1990–1991. It involved what I call simple "cognitive models of the adversary." During the 1990 crisis and 1991 Gulf War, we briefed and had other exchanges with General Schwarzkopf and senior leaders in the Pentagon based on this work.

The early 1990s was mostly about rethinking defense planning for the post-Cold War era. I organized and edited a big 1994 RAND book, *New Challenges for Defense Planning,* that had 23 chapters by senior RAND staff about how things were or should be changing. One of my own themes was that DoD should shift to a system that seriously confronted uncertainty. This meant more scenarios, more focus on capabilities, and a complete rethinking of the building blocks used by the Services (those had been corps/division, wing, carrier battle group (CVBG)). This was radical back then, and also threatening.

The late 1990s were even more exciting. That was the period that old-timers like yourself will remember. This was a period of transformation—people like Vice Admiral Art Cebrowski—he was very influential in the Joint Staff and President of the Naval War College. He later headed up the Office of Transformation. It was a period in which the Joint Staff was very forward looking (Joint Vision 2010, etc.). Some people could see that we were in a period where technology was changing things, but you had to have all the other things too. You had to have new concepts of operation and new ways of organization. Andy Marshall championed this view. A few senior military leaders, like Cebrowski and Admiral William Owens, were the heroes leading the charge from inside the Pentagon (sometimes with a degree of exaggeration and hype). Other "pushers" were outside (I remember fondly two Defense Science Boards in 1996 and 1998).

All of this was up my alley, as I was thinking about implications for defense planning, and methods for doing so. One study in 1996 was prescient: we had a baseline option of continuing the existing force structure, but we had others that moved with degrees of aggressiveness toward new-era forces with precision fires, what amounted to network centricity, and new "building blocks" (brigades, squadrons, and differently configured naval battle groups). We paid for these with reduced manpower, but maintained enough structure to deal with multiple overlapping contingencies (we did not make the mistake of assuming that air power could do everything). Most senior officials were interested only in the baseline force, but there were exceptions such as Admiral Don Pilling and Secretary of the Navy Richard Danzig, who clearly saw the need to be transforming, although incrementally. The Air Force was also moving along smartly to introduce the new precision fires, which it recognized was changing matters drastically.

We also urged, in 1996, that the DoD take a portfolio-balancing approach to defense

planning. Two-war capability was a criterion within that, but there should be a higher-level construct. The eventual DoD version, in Secretary Bill Cohen's first Quadrennial Defense Review (QDR), was the Shape, Respond, Prepare Now strategy. It was much easier than previously to understand the role of the Navy and why it merited the investment it had long gotten; it was also recognized that the ''Respond'' component required worrying about *numerous* conflicts; and, finally, the ''Prepare Now'' component was a (timid) first step toward transformation.

Later, in the 2000s, I tried to codify a lot of my thinking about ''capabilities-based planning,'' and to substantially develop the methods for both exploratory analysis under uncertainty and strategic portfolio analysis. I did some of that work with Russell Shaver, a great analyst (now retired), some of it for the Under Secretary of Defense for Acquisition, Technology, and Logistics (Michael Wynne and then Kenneth Krieg).

The basic idea is to invest in a variety of capabilities with a variety of objectives in mind, such as maintaining or promoting US security interests in various regions, for the near, mid, and long term. The issue becomes how best to ''balance'' the portfolio of investments to address adaptively the many risks and opportunities that may arise. Secretary Robert Gates used the language of rebalancing well.

Today, capabilities analysis and strategic portfolio analysis are routine in parts of DoD. I was asked about whether they are taught yet in schools. I've taught them in the Pardee RAND Graduate School but they're not standard in many curricula to my knowledge. Portfolio analysis, of course, originated in finance and generated Nobel prizes. However, finance is easier because fewer variables are at issue (long-term capital accumulation and buffering against the risks of short-term disasters) and there is a wealth of relevant empirical data. In contrast, national security planners don't know the probability distributions for the many possible challenges to arise in the decades ahead. Further, they don't have something as simple as profit-making as an objective.

In the 2000s, of course, I also found myself doing a lot of terrorism-related work, including a 2002 study for the Director of DARPA about whether there could be a deterrence component of strategy against al-Qaeda and a late-in decade review with Kim Cragin and other colleagues on what social science told us about terrorism and counterterrorism. That was very interesting and provocative work, from my perspective.

Bob Sheldon: What have you done more recently?

Paul Davis: Well, I continued the analytic work relating to counterterrorism, particularly in work with colleagues Eric Larson and Angela O'Mahony. Some of that was qualitative but structured, involving what I called the factor-tree methodology, to include qualitative validation with fresh case studies; some of it involves a new uncertainty-sensitive approach to social-science-informed *causal* modeling. The word ''causal'' is important. To inform strategy we need causal modeling, not just the ubiquitous and often modest and less-than-useful statistical correlations.

A few years ago, I did a broad strategic study with Peter Wilson that described what we saw (and see) as a ''perfect storm'' of challenges for the US. We called many things right, but—like a lot of people—we did not anticipate Mr. Putin becoming a real problem again, and we certainly did not anticipate ISIL (Islamic State of Iraq and the Levant). Humility is important in this strategic-thinking stuff. I recall looking back to our 1994 book a few years ago. We got a lot of things right, but—yikes—we sure missed some other things. I did not see al-Qaeda coming.

We've done a lot of different things at RAND over the years. Most recently, I've been working on information fusion with Walter Perry, John Hollywood, and David Manheim (a PhD student). The part that makes it interesting is when you're trying to combine very different kinds of information, qualitative and quantitative, highly uncertain, speculative, some of it literally dishonest, some of it deliberately deceptive. Then what are the methods for doing so that have some foundation in information science? This has been mind stretching and, yes, fun.

Bob Sheldon: You've written a lot of reports at RAND. What are your favorite ones, either the ones you enjoyed the most working on, or where you thought you had the most impact?

Paul Davis: The most important has been the continuing theme of planning under uncertainty—confronting uncertainty without hand-wringing.

Within that, I like the phrase "planning for adaptiveness" or, in a more complex formulation, planning for flexibility, adaptiveness, and robustness (FARness). My work on this began back in the 1980s with "multiscenario analysis" when it was radical. Today there is a momentum for doing such things, much of captured by what some RAND colleagues call robust decision making.

The 1994 book that I mentioned apparently had background impact, but it's hard to trace it. I know, however, that two derivative issue papers in 1996 and 1998 (done primarily with David Gompert) were influential within OSD and the Joint Staff. They affected both policy-level and analytic-level language. The first, *Adaptiveness in National Defense*, included the transformation options I mentioned earlier, and had a first-cut approach to strategic portfolio analysis. Some of the ideas were reflected in the Shape, Respond, Prepare strategy of the first QDR and, much later, in the now-common terminology of "balancing" and "rebalancing" the DoD portfolio, as discussed articulately by Secretary Robert Gates. Taken together, these things contributed to DoD's move to capabilities-based planning, although DoD's implementation has been much more bureaucratic and much less uncertainty sensitive than I had hoped.

The 1998 issue paper, *Transforming the Force*, urged exploiting the new technology. It recognized that it was possible to reduce force structure and come out with more capability. It included a crude diagram arguing that from 1998 until around 2012, the US could enjoy an era in which everything would be dandy. Thereafter, things would be much more dicey because of proliferated technology and China having ascended. We're now in that dicey era.

As a single report on defense planning, my 2014 monograph pulls together my previous work and adds new features relevant to analysts and analysis. This is *Analysis to Inform Defense Planning Despite Austerity*.

It's too early to tell, but I hope that the social science work we've done has impact over time.

The idea of uncertainty-sensitive computational models based on qualitative factor trees was, I believe, a significant innovation. But we'll see. If we know enough, such models can be useful for description, explanation, and seeing broad patterns even though we can't, a priori, confidently estimate the factor values or the rules by which factors combine to create effects. Humans deal with such matters routinely in social settings: we avoid getting into danger zones by not pushing "hot buttons"; we take tentative steps that we think will be positive, but then reassess based on reactions and events. We know that random things (moods, distractions, or events we don't know about) can change results. We recognize that we may read the situation wrong and will need to adapt.

Bob Sheldon: Some people erroneously think that in the field of operations research (OR) you have to have a degree in OR to be effective. But historically some of the most influential people in OR have started with degrees other than OR. What was it about your chemical physics background that allowed you to see problems clearly and do useful analysis?

Paul Davis: I think that being a good analyst is a matter of built-in talents plus a certain kind of mind plus experience with "hard" problem solving. Without naming names for fear of leaving someone out, some of the first-class analysts I have known over the decades have come from physics, economics, engineering, mathematics, chemistry, business administration, and, yes, operations research. If we include "softer" but rigorous forms of analysis, the list includes law and political science. The people involved have had multiple talents rather than just what the name of their degree would suggest.

I don't have deep credentials for academic operations research. If you ask me about queueing theory or optimization, I'll point you to specialists in those tools. I see them as "just tools." Many of the most important problems for analysts are at a level where problems are unstructured and the notion of optimization is silly (something I once heard General Larry Welch point out at a MORS meeting).

Bob Sheldon: Let's backtrack. When was your first involvement with MORS?

Paul Davis: Around 1980, when I was in the government. It probably had to do with the

strategic mobility working group; I gave a paper and ran the working group at some meeting. Over the years, I've spoken or otherwise participated in a lot of the special meetings. Those did not always exist. You, Bob, remember it was an initiative to create more of the special meetings and have greater depth, sometimes with written papers.

Bob Sheldon: It started after MORS went from two symposia a year to one.

Paul Davis: I thought that was good. MORS has changed over the years, experimenting to see what's useful.

Bob Sheldon: One of your most valuable contributions to the MORS special meetings is serving as the synthesis chair. That's a challenging job. How do you view that when you walk into a special meeting, trying to synthesize and pull it together and make sense of it?

Paul Davis: I don't have a formula, but you have to listen. Normally, though, it requires more than listening—you usually have to do some conceptual construction, because stapling things together doesn't usually do much and only sometimes are the pieces just lying there, ready to put together. Sometimes, diagrams help, such as those Stuart Starr, FS, often used well in meetings I recall.

Bob Sheldon: You were recognized by MORS with the Wanner Award in 1997. What was your reaction to that?

Paul Davis: I was pleased and surprised. I don't know what actually instigated that, although I imagine that it was the methods for planning under uncertainty.

Bob Sheldon: More recently, in 2012 you were recognized as a MORS Fellow for your lasting contributions to the Society. Your reaction to that?

Paul Davis: I was really honored. But one of the things that bothered me is that, although I've contributed to MORS in many ways over the years, I have never paid my dues by doing some of the burdensome things, like being a President or Vice President. I'm not very good at those things anyway, but in terms of service, I hadn't done as much as some. But when people added it up, the substantive contributions apparently made it okay. We do what we can.

Bob Sheldon: I know you're familiar with the person we consider MORS Fellow Number One—Clayton Thomas. What are your thoughts about Clay?

Paul Davis: I met Clay in the 1980s and had good conversations with him over the years, but regrettably never had a chance to work with him directly. He exuded wisdom and thoughtfulness. I also recall that he would quietly ask pointed questions, but they were intended to be helpful.

Bob Sheldon: One of your roles at RAND is you teach classes. How long have you been doing that?

Paul Davis: Off and on since 1982. Lots of different courses.

Bob Sheldon: What kinds of courses?

Paul Davis: Technology and policy analysis. Defense planning. Terrorism. In more recent years, I've taught a course about complex policy problems that require modeling. For a long time, I taught that with Dick Hillestad. It includes things like ''How do you construct systems models?'' ''How do you deal with qualitative factors and values?'' ''How do you think about detail?'' ''How do you deal with uncertainty?'' I have the students apply the course's lessons to a problem of their choosing, which sometimes becomes their dissertation project. I also teach some brainstorming methods, to help with both divergent and convergent thinking.

Bob Sheldon: How many students do you typically have?

Paul Davis: From four to 24. There's been no pattern. It's a relatively small school and participation depends on what other courses are being taught at the same time.

Bob Sheldon: You taught yourself systems analysis. Have you read the systems analysis literature developed at RAND by Quade and others?

Paul Davis: Yes, with admiration.

Bob Sheldon: Did you have to relearn some of the systems analysis while working at RAND? Did it change your outlook?

Paul Davis: I had picked up or discovered the more important things. When I looked at the books in the 1980s, I found that they added structure, depth, and subtlety, but I already had the ideas. Perhaps what impressed me most was better appreciating that what seemed to me obvious wasn't so obvious to others. That's why the early course in systems analysis had to be created. Some people are better at such things

than others. Russ Shaver (now retired) was a favorite colleague at RAND for many years; he was a great systems analyst with strong, non-linear, creative intuition and, in addition, the ability to be rigorous. As far as the Quade books, I give them as suggested readings. They are better for people who have gotten their hands dirty and encountered the problems, and are ready to ponder them seriously, than for someone who is just starting.

Bob Sheldon: What other books would you recommend for our MORS readers?

Paul Davis: I recommend Nobelist Daniel Kahneman's book, *Thinking, Fast and Slow,* for graduate students. Another book, first pointed out to me by a British student (an MD obtaining a PhD at RAND), is perhaps best for someone who has just been through an American operations research degree. It is a book by Rosenhead and Mingers, *Rational Analysis for a Problematic World Revisited: Problem Structuring Methods for Complexity, Uncertainty and Conflict.* It's about what might be called "soft OR" and is an antidote for overly mathematical attitudes about analysis.

Bob Sheldon: Do you have advice for analysts and MORS?

Paul Davis: In my 2014 book I urge recognition of a new professional responsibility for analysts: Instead of merely listing analysis assumptions, analysts should: (1) routinely show how results vary with all key assumptions and disagreements, (2) routinely assess options for FARness [flexibility, adaptiveness, and robustness], showing the value of affordable hedges even in periods of austerity when hedges may seem like luxuries, (3) do this comprehensibly to aid policymakers in converging on decisions and actions.

I believe that, without exaggeration, embracing the admonition would be "revolutionary" to the way DoD analysis is performed.

Bob Sheldon: A question from Dr. Yuna Wong: What are your thoughts about how DoD analysis has evolved over the years?

Paul Davis: Well in some respects, badly in others. Big, detailed models are extraordinarily important when they are valid (as in work for NASA, for training simulations of combat, or for technical, engagement, or even mission-level work). When they are not really valid or have highly uncertain inputs (all the "scenario variables"), detailed models can be counterproductive for framing and informing strategic decision making. They get in the way of good uncertainty-sensitive thinking. The intent should be to develop capabilities that will prove effective for a wide range of plausible scenarios and circumstances therein. These will not usually be optimal for any particular case.

The solution necessarily involves models at different levels of resolution: we need the detailed models as well as the simpler ones. DoD often doesn't do a good job of having and dealing well with families of models. It also was enamored for a long time with standardized kinetic campaign models and databases, squeezing out serious discussion of uncertainty, and squeezing out agility and creative thinking. The recent rediscovery of human gaming is welcome. I hope it affects future approaches to modeling as well.

Bob Sheldon: Another question from Yuna: What would be your advice for younger analysts who want to learn more about this career field or develop themselves professionally?

Paul Davis: For civilians, an interesting job in government is valuable even if for a limited time. There is no substitute for having worked with people who have to make real decisions while dealing with complex problems and organizations. You come to admire and appreciate the many civilian officials, military leaders, and experts. Military officers, of course, learn these matters routinely.

Crosscutting is good (for some of us). If you have friends and tasks that have you doing different things over time, with different approaches, you will either discover that it doesn't suit you or you will discover that you love it. Either is okay. Specialists are fine. If we go to a doctor for surgery, we want a specialist. But you need to know whether you're a crosscutter.