Memoirs of Tom Willey at NADC, Johnsville, Pennsylvania

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After five years at NADC, I was to begin in 1956, a series of special studies, which got us involved with the intelligence community. These had to do, in a very small way with the debate then underway between the Air Force and Navy about the Navy's mission in atomic warfare as delivered from carriers. Martin Landau and I also did a special study for the Air Warfare Research Department (AWRD) on one phase of a feasibility study for this proposed ballistic missile submarine program later to be called Polaris. These studies were well received and t was soon promoted to GS-12. They were the first that would eventually lead to my reassign reclassification from Physicist to Operations Research Analyst.

In late 1957 or early 1958, reorganization was accomplished at NADC, which abolished the Armament Lab (where I had been) and formed a new ASW Lab. The division that I had been in, with Frank Dyer its head, was split up. Dyer, Joe Brown (my former supervisor), Martin Landau, Jack Brundage, Ed Sweeney and Millard Mitchell were reassigned to AWRD. Jim Luckman (a Navy Reserve Captain), myself, Tony Greco, Isadore Zaslow and Ron Doray (a fresh-out) were assigned to the new ASW Lab as their Analysis/Special Studies group.

There was then a lot of special training. I went to the ASW tactical school in the Norfolk, Virginia area for a two-week training session with mid-career Naval Officers, and a one-week stint at sea off Bermuda in the new ASW carrier task force ALFA.

A special part-time study got me involved with Bill Lyons and our radar department folk who were considering the possibilities for application of some new high-resolution radar technology in the ASW world. Much more would come of this, but not for me for a number of years, At any rate, the radar folks were very pleased with my study of potential applications, Technology opportunities and submarine operating tactics and procedures.

But the new organization was not a happy one. One of the Technology Division heads was most disappointed and angry that Homer Huey, formerly Armament Lab

director) was made ASW Lab Tech Director instead of himself. Seeing that others saw this as unfair and inappropriate, he organized it campaign to oust Huey. This caused all sorts of friction, poor morale, slack discipline, etc. Our little group under Luckman were outsiders in all this ferment and tried to stay out of it and remain neutral.

The Navy is nothing if not discipline oriented. Captains made decisions and gave orders intended to restore discipline and smooth operations in our R&D ASW Lab. Finally, the Chief Troublemaker was brought up on charges having little to do with the real issues. This was the only time in my 36-year career in civil service that I saw a GS15 charged, convicted and fired. I was to see others downgraded, or given jobs intended to make them want to leave or retire, but never an outright GS 15 firing. At this time others in the conspiracy were persuaded to leave or were reassigned and new tough discipline supervisors were installed in the rebellious divisions. The other side of the case saw Homer Huey reassigned to a staff job, and the long service ASW type Russ Mason brought in from the New London tab to head up the ASW Lab. Thus I learned much about how the system could or couldn't work and the perils of fighting City Hall.

Soon after these organizational doings, LCDR Hal Cody took over one of the divisions in the ASW Lab, and Luckman and group were moved into that division. It was at this time that I was extending my activity in support of Lyon's radar work. I was to receive substantial and sincere encouragement from both Cody and Lyons and my career often crossed paths with both of them in later years. Luckman, finally fed up with the nasty politics of the ASW Lab left for the FAA in 1963 and I never saw him again. Ron Doray, our 1958 fresh-out, was a bright, ambitious young man but sadly lacking in social skills. He consulted me for advice about his ongoing courtship of Gertrude. I don't think he really needed my advice, but at last the two were married and we were invited to the wedding.

In 1960, 1 spent a considerable time doing a study of ASW Helicopter effectiveness and the possible effects of new technology and tactics for ASW Helicopters. The Navy was interested at this time, but chose to go with the small drone helicopter DASH program for destroyers. The unmanned Dash machines proved to be far less useful than hoped for - its reliability and maintainability were both much less than advertised, and its ability to drop weapons accurately at modest distances from its parent destroyer was poor. My detailed studies of manned helicopters in the ASW setting would form the basis for later expansion at the time I became deeply involved in the beginning of the LAMPS helicopter

program in 1968-69. My study, completed in early 1961 was well received by both BUAER and AWRD types.

Luckman put Tony Greco, Izzy Zaslow and myself to work on a BUAER funded feasibility, study awarded to several avionics contractors. We were overseeing the basic work that would eventually become the P3-ANEW ASW Avionics system that put NADC on the map with the VP-PE community. But I did not continue with this program because AWRD selected me for promotion to GS 14 to head their Polaris system vulnerability study program. AWRD was suffering from industry hiring at this time. Several of their high producers as well as ex-armament types Joe Brown and Martin Landau had left with little advance warning.

My new supervisor, the head of the Systems Analysis and operations research division of AWRD was David MacInnes. Dave was both well experienced in studies and a gifted leader of this type of work. Thus in January 1962, t took over the task leadership of the Polaris vulnerability project in AWRD.

The nature of the task was to play "devils advocate" to use the church term, for the Polaris/submarine Project office, SP- I in Washington. Out studies were to examine the Polaris submarines and their ballistic missile missions as if we were Soviet Technical analysts or spies. Such work is sometimes called t4eam since we were loyal workers doing our best to simulate the enemy. Anyhow, we were denied much highly classified stuff that might have been helpful since "spies" would not have such access. Our output was to be advisory studies of were and how the system might be vulnerable to USSR surveillance and/or attack and, of course, how to fix such problems.

The project had a 4 or 5-year history with lots of files full with information and speculation but no hard analysis and no results. Two of my staff of 4 soon left seeking more obvious opportunities elsewhere. This was a disguised blessing, since it enabled me to secure Bob Fosko and Bernie McHugh for my team. With the help of our Washington overseer, CDR Napier, and Bernie McHugh we zeroed in on the potential threat posed by the very large Soviet fishing fleet. They had put over 1000 fishing boats out to look for our first Polaris submarine sortie.

At this time, everything we did, was covered by a blanket of heavy security overlaid by a very strict need-to-know. However, we learned much about the skill and art of ocean fishing-commercial fishing - some of the techniques going back centuries. There were long lines, bag nets, trawling, bottom dragging and that ancient relic of

sail days the drift or gill net. We learned much about the fishing vessels themselves - typically a few hundred GRT in size. Our studies covered sources as open as the encyclopedia and as closed as special friends in the intelligence world. We postulated a scheme based on some knowledge of how the missile submarine navigated for accurate missile launch and some relatively new ASW Technology applied to the fishing boats. This scheme, if carried out at length and given the proper mathematical-statistical analysis, had the potential to allow Soviet interdiction of Polaris operations in the North Sea. Our study was presented to SP who immediately called in other experts. After careful grilling, the study scheme was evaluated as unlikely and highly creative but definitely a potential threat to Polaris. After a review in the Pentagon by the top submarine admiral and his OPNAV staff, the Admirals were clearly frightened enough to forbid any circulation of this work and gave the edict that this particular topic was too sensitive to permit civilians such as our team to continue work on the fishing fleet threat. Clearly we had come close to their operational problems. We were forbidden to discuss this study with anyone regardless of clearance or position. I don't think the Soviets ever hatched a scheme similar to ours - their military officers like ours who took over our work are typically too traditional in thinking to have concepted the highly creative schemes we had postulated. Eventually, all written and report material of ours on this topic went to the shredder.

Our reward for this outstanding Work wag twofold: silence about the fishing fleet scheme and considerable recognition at SP as esteemed members of their team. Our overseer, CDR Napier got an outstanding toward his promotion to Captain (in 1966) and we were frequently called into RADM. Levering Smith's office as expert consultants. Sometimes this meant reviewing the work of other activities and I'm afraid we ruined a few reputations this way because more than once we exposed efforts or sloppy work that could not support the conclusions of its authors. Some of our special analysis - that were sometimes prompted by Kremlin speeches - were used by RADM. Smith or VADM. Gallantin (the Chief of SP-1 at this time) on the SEC NAV or the JES to deflate claims of Polaris vulnerability by the Soviets. Eventually, I would receive a special citation from the SEC NAV for outstanding technical support to Polaris - but it was vaguely worded and did not refer to either the fishing scheme or the EMP work. Such citations from SECNAV were very rare at NADC and the details of our work were still unknown.

The EMP work was another major effort on out part. In 1962 a U.S. nuclear weapon explosion high over a Pacific Island damaged electrical systems 900 miles away in Hawaii. The culprit was an Electromagnetic Pulse (EMP) - a high voltage

burst of energy similar to a lighting strike, but much more intense - the result of the bomb detonation. The Air Force (AF) was very upset by the 18MW disruption - it seemed that the then new Minuteman ICBM might be destroyed or at least put off course by such a nuclear warhead generated pulse. A special committee reporting high up in the DOD was appointed to examine this closely. The chairman was the prestigious Dr. Edwin McMillan, a Nobel winner in 1951, and at that time head of the UCAL, RAD Lab. Many University professors expert in physics, electronics, radiation effects, etc. were on this committee. I did not know of this committee when RADM. Smith set us to work on an examination of this new threat to the Polaris system and its missiles. Polaris missiles at this time were of much shorter range than Minuteman and therefore their burn time (the vulnerable period) was much less. There were also differences in the guidance electronics which favored Polaris - but the main point was how could the Soviets explode a warhead at just the right time near enough to the launch when they knew neither the time or place of Polaris launch in advance.

Presently, our study was ready for review. We were to present at a special meeting at an Air Force base near Los Angeles. Levering Smith introduced me to the McMillan committee with the challenge to find the flaws in my presentation. Momentarily I thought my SP backers were offering me up as a sacrificial lamb to this stem-looking quarrelsome group. I never finished my presentation that day - those professors hectored and badgered me for over four hours trying to find a technical flaw or hole while the SP officer sat silently in the audience. Finally I was excused - shaken and exhausted, but unbowed. Bob Fosko assured met that I had not stumbled and Dr. McMillan later told me I had done well and that he would ask SP if I could do a brief special study for their committee. At any rate, I passed the test for the Polaris people and their program was given the green light partly as a result of my presentation.

It was the article on p. 67 of Aviation Week magazine of July 28, 1997 that caused me to write about my SP-Polaris work. That magazine article recited some of the EMP history (now declassified) and called for renewed efforts in hardening satellite and military electronics. At the time of our presentation (1964) and for ten-fifteen years afterward everything about the ENV induced problems was very hush-hush, especially the possible vulnerability of MM and ICBM missiles. But we were used to being silent about our work. Part of the problems with Polaris vulnerability were the paranoid fears of the Navy that the Air Force would take the whole program away from the Navy since the air Force thought it "owned" strategic warfare. Indeed, at one time General LeMay tried hard to take the whole thing -subs,

operations, missiles and R&D away from the Navy, but wiser heads prevailed. These lessons about ownership and the need for silence were to stand me in good stead years later as a successful ghostwriter for absolute some very senior persons in the Navy. Most of our best work went to the shredder long before it might have been declassified and some of it appeared with other names as authors.

Later, I was to go with Gallantin and Smith to Livermore Labs where I met the famous physicist Edward Teller. In service to SP I was also to meet Dr. Peter Waterman (later acting ASN, R&D), Dr. Probus (later DNL), and George Levine, a prominent Westinghouse engineer (and NYU classmate) concerned with Navy work. All would appear as friends again later in my long Navy R&D career.

But there were problems back at NADC, and after my presentation to the SECNAV, and CNO the management at NADC decided I would be of more use elsewhere than for SP to pull some of the other NADC project chestnuts from the fire before they burned to a crisp. Technically and career-wise these had been the best years of my life, although it took me years to recognize them as such.

Over the next several years I had several interesting assignments. One was preparing the Navy's objectives and desired results for the P-13 project as part of the Air Force's Manned Orbiting Laboratory (MOL). Their own projects, P-1 through P-12 were not all that convincing of scientific or military benefit so my study report on P-13 ocean surveillance was a real hit with them. But nobody at NAVAIR or NADC management seemed to care about the Air Force fortunes with their space program. Another task was assisting Bill Lyons with the get-well program for the E2A (later to be the E2B). Again I found myself in the ASN R&D office talking to a special assistant - Dr. Waterman. Later, with the technology support of Bill Lyons and his radar folk, l was to prepare a study report of Navy AEW requirements, which was the precursor to the E2C program. The E2C updated (Airborne Radar) still flies today.

I also got involved in the ASW radar program again - this time Ed Koos was the leader of the work for the radar technology operations at NADC. Several studies were done and I did a presentation or two for them at high Navy command levels. At any rate, the NADC program bested its competitors, and was sent to production for the S-3 ASW aircraft as its periscope detecting radar. Bill Steuteville was my right hand man in support of Ed Koos' work. This basic radar design (now updated) still flies today in ocean surveillance and ASW roles. But Ed Koos was

too creative-oriented for the new management in the early '70's and went to NRL to work where his sizeable talents were much more appreciated.

Somewhere along the way in the '60's I had gotten involved in helping the Moored Sonobouy System project folks with their program. Royce Knouse was its leader at first and later the late Jim Brown. Bernie McHugh, whose creative work on Polaris had been so valuable, was my right hand man in support of this MSS work. He and I did special studies and helped evaluate contractor proposals on a part-time, sometimes basis during the late '60's and much of the '70's. The program was eventually cancelled after nearly fifteen years of R&D due to incompetent management and leadership in Washington. Fully operable MSS equipment would have been a god-send to the British in the Falkland Islands war in 1982.

My most noteworthy major program success at NADC was with the LAMPS destroyer helicopter program. It was through my personal efforts at the very start of this program (1968) that NADC would later come into prominence in the LAMPS development to the tune of budgets of tens of millions per year (1970's). But by then, I was just an expensive GM-15 study advisor for the program. Our earliest study work was well done and quickly demonstrated to NAVAIR and OPNAV that NADC had the knowledge and technology skills for the program. I was helped immensely in this work by Bob Fosko, Tony Mickus and others in my, by now, growing analysis group. But to do LAMPS justice requires a long chapter by itself, so I must stop here.