50 years...

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My Life at APL

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My life at the Applied Physics Laboratory was in two segments, totally different in substantive purpose but similar in spirit.

I received a Ph.D. in experimental nuclear physics from the University of Iowa in June 1939. After graduation, I had the good fortune to be appointed a Carnegie Research Fellow in Merle Tuve's laboratory of the Department of Terrestrial Magnetism (DTM) in Washington, D.C., one of several laboratories of the Carnegie Institution of Washington. My arrival there in September 1939 was more or less simultaneous with two other events of vastly greater significance: the invasion of Poland by Germany, which opened a worldwide military conflict later called World War II, and the announcement of the neutron-induced fission of the uranium nucleus. DTM immediately became an important center of fission research, which contributed to the development of the atomic (nuclear) bomb and to atomic power for civil purposes.

By mid-1940, Tuve, Larry Hafstad, and Dick Roberts foresaw the likelihood of U.S. involvement in the spreading European war. As a result, they looked for a way to apply their talents and experience to bolstering the efforts of the newly created Office of Scientific Research and Development to improve the technical capabilities of the U.S. Navy. In many respects, the Navy's capabilities were woefully inadequate for modern warfare, especially in defense against aircraft attack. As a result of consultation with similarly concerned British scientists and officers of the U.S. Naval Bureau of Ordnance, Tuve and his colleagues adopted the objective of developing a proximity fuze for projectiles of the Navy's large antiaircraft guns on combatant ships. The basic concept of a proximity fuze was to eliminate range errors, implicit in the use of time-fuzed projectiles. Pointing errors would remain, but preliminary estimates indicated that an improvement on the order of a factor of 10 in hit probability could be achieved if a fuze could sense the approach of its projectile to an attacking aircraft and thereby reduce a typical range error of at least 700 ft to zero.

In my first year and a half at DTM, I pursued low-energy nuclear physics research using DTM's pioneering 1-MeV Van de Graaff accelerator (now on exhibit in a museum of the Smithsonian Institution) in accordance with my fellowship plan. By the spring of 1941, however, I decided to join the rapidly expanding "war work" of the laboratory and developed a logarithmically sensitive photoelectric detector for a fuze. After an overall practical assessment, the photoelectric fuze was abandoned in favor of a radio-proximity fuze, based on preliminary design work in a British military laboratory.

The basic elements of such a fuze were a free-running Colpitt's oscillator operating at about 70 MHz and radiating a few milliwatts via a simple antenna, a two-stage audio-frequency amplifier, and a thyratron tube for triggering an ignitor. The loading of such a "soft" oscillator was modified by nearby reflectors of radio waves so as to change the frequency and, more importantly, the plate current of the oscillator. The latter effect was the one that was exploited in sensing the approach to an aircraft.

The electronics of the fuze, powered by dry batteries, were rudimentary. I built dozens of them with my own hands for test purposes. The real problem was obtaining vacuum tubes that would survive firing from a naval gun. The mechanical stress was composed of a maximum linear acceleration of about 20,000 g, a lateral (sideslap) acceleration of unknown magnitude, and spinup to about 300 revolutions per second, all within the barrel of the gun. Our starting point, long before the days of transistors, was miniature vacuum tubes, which were in small-scale production by the Raytheon and Sylvania companies for hearing-aid amplifiers. A typical tube had a glass envelope and was about 1.25 in. long and 3/8 in. in diameter. Our crude drop tests and centrifuge tests encouraged us to believe that a survivable tube might be possible.

It was on this hope that APL was founded in the spring of 1942. I was among the first group to be transferred from DTM to APL, thereby becoming a plank owner of a Silver Spring Chevrolet garage, converted to a laboratory. My job was to do whatever needed to be done but was principally devoted to improving the structure of the tubes by trial and error and shuttling back and forth to work with Ross Wood, an engineer at the Raytheon plant in Newton, Massachusetts. I potted and tested each fresh batch of tubes in a vertically fired gun at the Navy's Stump Neck, Maryland, firing range; recovered the projectiles with the help of a well-worn posthole digger and shovel; returned to the Laboratory for postmortem examinations of the tubes; and then shuttled back to Newton, Massachusetts, with suggestions for modifications. My most important contribution was the suggestion of a simple coil spring with a protruding V-shaped element for maintaining the tension and position of the fine tungsten wire filament. This feature, dubbed a mousetrap spring, overcame the last major hurdle in the tube development. Three colleagues and I prepared a patent application for the Rugged Vacuum Tube and, after relaxation of security restraints, were issued the patent in 1963. Some 80 million such tubes were produced during World War II. Upon issuance of the patent, I received a crisp $1 bill and a congratulatory letter from APL's patent attorney.

Under Tuve's leadership, everything was done in parallel. For example, suitable batteries were being developed on the assumption that tube development and production would be successful and vice versa. Cost was irrelevant; achievement was everything. As a result, by the summer of 1942, we were getting small quantities of preproduction fuzes that had a consistent performance of better than 50%, the remainder being duds or premature firings. At this point, the responsible officers of the Naval Bureau of Ordnance authorized full production and issuance to combatant ships of the Pacific Fleet, which were gravely threatened by dive bomber and torpedo bomber attacks by the Japanese.

Three of us from the Laboratory—Neil Dilley, Robert Peterson, and I—were commissioned as Lieutenants (junior grade) in the U.S. Naval Reserve on 6 November 1942. Thirteen days later, we sailed from San Francisco on the troop ship USS *Republic* with 5920 highly classified rounds of proximity fuzed 5"/38 projectiles. We arrived in Nouméa, New Caledonia, headquarters of ComSoPac (Commander, South Pacific), and fanned out under the general direction of Commander William S. ("Deke") Parsons to distribute the ammunition to destroyers (my assignment), cruisers, and battleships and to instruct gunnery officers on the characteristics of this exotic new ammunition.

I continued as the APL/Naval Bureau of Ordnance representative in the South Pacific until August 1943, when I returned to the States to advise APL on practical experience with the fuze's successes and shortcomings, and its sometimes reluctant acceptance by gunnery officers. One of our major problems was an excessive percentage of duds, which I found to be caused by dead B-batteries. Delivery of the fuzed projectiles to combatant ships by surface transport typically required several months, during which elevated temperatures in the holds of cargo ships and naval vessels had a devastating effect on the useful lifetime of the multiplate B-batteries. APL was engaged in developing a "reserve battery," a liquid electrolyte battery that would be activated upon firing, but it was not yet ready for service. Meanwhile, we had thousands of dead and dying batteries in the Fleet. I succeeded in advocating the air shipment of fresh batteries to the South Pacific and in devising practical procedures for replacing batteries at ammunition depots. I then requested a second tour of duty in the South Pacific to set up rebatterying stations. In March 1944, I arrived at Nouméa again, this time by air, with the first load of fresh batteries. With enthusiastic support by ComSoPac, I proceeded to set up rebatterying stations, first at Nouméa; then at Tulagi; Brisbane, Australia; Eniwetok; Ulithi; Manus; and Espíritu Santo. At these ports of call, ships could exchange their proximity-fuzed projectiles for ones with fresh batteries.

On my previous tour, I had established a cordial relationship with Rear Admiral Willis A. Lee, one of the Navy's leading gunnery experts, a task group commander, and type commander for battleships in the Pacific Fleet. He requested my assignment as his assistant staff gunnery officer, and I again reported to his flagship, the USS *Washington*, on which I had served for several months on my first tour of duty. During the Battle of the Philippine Sea, I was with Rear Admiral Lee on the bridge of the *Washington* during heavy attack by Japanese aircraft. All of the attacking aircraft were successfully shot down, and we were not hit, but I regret that I was unable to pinpoint any case in which I was certain that a proximity-fuzed projectile did the job.

In November 1944, I left the *Washington* and returned to the States, where I served the remainder of the war as a Bureau of Ordnance liaison officer to APL. One of my assignments was reading, excerpting, and summarizing action reports from combatant ships and attempting to assess the utilization and overall effectiveness of proximity-fuzed ammunition against aircraft attack. The haze of battle is far from a controlled laboratory experiment, and it was very difficult to arrive at an objective quantitative summary. Nevertheless, gunnery officers identified many episodes of proximity-fuzed rounds being responsible for aircraft hits. In the end, I thought that APL's direct contribution to the Pacific Fleet might reasonably be claimed to have saved at least several U.S. warships and hundreds, perhaps thousands, of American lives. An additional contribution, of unknown magnitude, may have been to persuade the Japanese to surrender sooner than they otherwise would have. The matter should be revisited by a dispassionate military historian. APL's contribution to World War II in the European Theater is another story, of which I have no direct personal knowledge.

Meanwhile, the most important event of my life occurred. I met my future wife, Abigail Halsey, in a minor bumper-to-bumper automobile collision as we were both en route to APL. An English literature major from Mount Holyoke College, she was then a mathematician and data analyst working on APL's Mark 57 fire control project, intended to reduce the "other" major errors in the antiaircraft problem, namely, the pointing errors. We were married on 13 October 1945, and now, after more than 51 years of marriage, have five children plus their five spouses and six grandchildren in our squadron—all credited to APL.

On 1 January 1946, I was promoted to Lieutenant Commander and was transferred to the ready reserve in March, with two months of accumulated leave. I was re-employed by APL during my leave and began the second segment of my life at the Laboratory. Actually, I had been spending more and more of my time at the Laboratory and less time "downtown" during the previous several months. Henry Porter had learned of a prospective Army program for assembling and firing a number of German V-2 rockets, which had been captured at the Peenemünde plant in Germany. The primary purpose of the program was to gain military experience in handling, firing, and tracking this rocket, then a much larger one than any U.S. rocket. The Army Ordnance Department also invited military laboratories and contractors to use the payload capacity of about a ton for scientific instruments. An informal panel of scientists was organized under the leadership of Ernie Krause of the Naval Research Laboratory (NRL) to plan such utilization. Tuve had indicated that he would support the development of such a program at APL, and in early 1946, he assigned me to represent the Laboratory, even though I was still in uniform. Tuve gave me a free hand in undertaking a program of high-altitude research, broadly related to the guided missile developments that by then dominated APL's activities. Our initial group included kindred spirits Howard Tatel, Bob Peterson, Lorie Fraser, John Hopfield, Clyde Holliday, and Shirley McCullum, our indefatigable secretary. Others were added later, and we had access to excellent machine shops and electronic shops.

While at DTM, I had learned a lot about what we knew and did not know about cosmic rays, the ionosphere and upper atmosphere, the solar spectrum, and the production of ozone in the atmosphere. I was bursting with ideas as to how advances could be made by traversal of the atmosphere to altitudes on the order of 150 km and by observations of cosmic rays and the solar spectrum from above the appreciable atmosphere.

After I again became a regular employee of APL, I became its member of the V-2 Upper Atmosphere Rocket Research Panel. About a year later, I was chosen as its chairman to succeed Krause, who left NRL for an industrial job. This panel, later called the Upper Atmosphere Rocket Research Panel, and still later the Rocket and Satellite Research Panel, always unofficial, had the central role in planning the national upper atmosphere research program, allocating payload space on available rockets, sharing techniques and results, and achieving world leadership in upper atmosphere physics, cosmic rays, and solar spectroscopy. The Panel continued its functions until the creation of the National Aeronautics and Space Administration in October 1958.

We had ample financial resources, left over from wartime budgets, so to speak. The wartime spirit of achievement, irrespective of cost, still prevailed. Also, the Army's schedule of V-2 firings was rigorous, and the "warheads" would be filled with desert sand if we were not able to supply our instruments on time. The pace of the work was intense. NRL assumed responsibility for supplying the basic nose cone structures, within which all groups mounted their instruments, and the radio telemetry and command systems. Physical recovery of photographic film and other data in armored cassettes was also considered feasible. The initial plan was to fire all V-2's nearly vertically from the White Sands Proving Ground near Las Cruces, New Mexico.

Our APL group supplied cosmic ray instruments for the first three V-2 flights on 16 April (vehicle failure), 10 May, and 29 May 1946. Unfortunately, no scientific results were obtained because of a variety of technical failures of the hastily assembled equipment, including the destruction upon impact of our magnetic wire data recorder. We supplied instruments for seven subsequent V-2 flights during the period from July 1946 to February 1949. All of these flights were successful. Our principal results were determination of the intensity of the primary cosmic radiation and its attenuation in the atmosphere, the successful recovery of high-quality spectrograms of the ultraviolet spectrum of the Sun, and a large number of detailed photographs in the visible and near infrared of large areas of the surface of the Earth and its cloud cover. The latter, due to Clyde Holliday, were the forerunners of the now enormous field of satellite reconnaissance.

Meanwhile, because of the limited supply of V-2's and the potentially great expense of reproducing and firing them, we needed a much simpler, less expensive rocket for the continuation and expansion of scientific studies at high altitudes. With Tuve's backing and the Bureau of Ordnance support, I initiated such a development at the Aerojet Engineering Corporation and the Douglas Aircraft Company in early 1947. The Navy's Office of Research and Inventions (later transformed into the Office of Naval Research) joined in supporting the development on behalf of NRL. My overall specification was to deliver 90 kg of payload to an altitude of 100 km. We called the rocket Aerobee—the "Aero" for Aerojet and the "bee" for APL's Bumblebee program. The first powered flight of the two-stage Aerobee was made from White Sands Proving Ground on 24 November 1947. The summit altitude was only 58 km, however, because of command cutoff of the thrust of the second stage as it drifted out of the range safety grid. The second flight on 5 March 1948 was both technically and scientifically successful in all respects, reaching a summit altitude of 113 km.

Our subsequent Aerobee flights were interleaved with V-2 flights and were made at the rate of about six per year during the early period, with a 100% success rate. In 1949, our APL group conducted two successful Aerobee flights from the USS *Norton Sound* off the coast of Peru near the geomagnetic equator, and in late 1950, we conducted two successful Aerobee flights in the Gulf of Alaska, also from the *Norton Sound*. These flights were a vital part of our latitude survey of the primary cosmic radiation and the Earth's magnetic field in the ionosphere. In other flights at White Sands Proving Ground, we determined the altitude distribution of atmospheric ozone and extended our earlier work in solar spectroscopy and high-altitude photography of the Earth's surface. The Aerobee, in its original version and in successively upgraded versions, became the primary U.S. vehicle for high-altitude research. As of termination of the Aerobee program in 1985 after 38 years, 1037 Aerobees had been flown for a variety of scientific investigations in upper atmospheric and ionospheric physics, cosmic rays, and astronomical research.

This early work with high-altitude rockets laid the foundation, both technically and scientifically, for the competence of U.S. laboratories to pursue scientific research with satellites and deep space missions to the Moon and planets beginning in 1958.

By 1950, despite the many successes of our small research group, there was an increasing level of grousing at APL about the "5-percenters"—those of us supported by the Laboratory's overhead on missile programs to conduct pure research loosely related to APL's hard-nosed missions. As part of this feeling, Dr. Gibson, then director of the Laboratory, assigned me the additional duty of supervising the residual proximity fuze effort. I was well qualified to do so, but by this time I had no further interest in peacetime improvements of the fuze and began to look for a different job, preferably at a university. Soon thereafter, I was offered an appointment as professor of physics and head of the Department of Physics at my Ph.D. alma mater, the University of Iowa. I accepted, and after winding up my ongoing obligations at APL, left for my new post in late December 1950.

I carried with me the "we can do it" spirit of APL. It has served me well in an intensive space research program during the subsequent 45 years.

SUGGESTED READING

Baldwin, R. B., *The Deadly Fuze—Secret Weapon of World War II*, Precidio Press, San Rafael, CA (1980).

THE AUTHOR

JAMES A. VAN ALLEN received a Ph.D. degree in nuclear physics from the University of Iowa in 1939. He joined the Carnegie Institution of Washington and was among the first group of researchers to be transferred to the newly founded Applied Physics Laboratory of JHU in the spring of 1942. At APL, he worked on the development of radio proximity fuzes for antiaircraft projectiles. He then served as a gunnery and ordnance officer in the South Pacific Fleet and at the U.S. Naval Bureau of Ordnance. In 1946, he returned to APL, where he founded and supervised its High-Altitude Research Group in the conduct of numerous scientific experiments with V-2 and Aerobee rockets. Dr. Van Allen was appointed a professor of physics at the University of Iowa in 1951 and has been a principal investigator for innumerable scientific projects on Earth and lunar satellites and planetary spacecraft. He has published 260 papers and 2 books, won numerous national and international awards including the National Medal of Science (1987), and has been a member of the National Academy of Sciences since 1959. In 1958, he discovered the radiation belts of the Earth. He continues to be active in space research at the University of Iowa. His e-mail address is james-vanallen@uiowa.edu.