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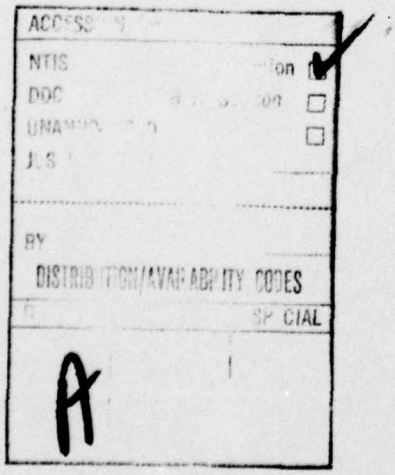
PREFACE

Compilation of Dahlgren's history began several years ago. This was a formidable endeavor in the scientific environment of the Laboratory where rapid technical progress has naturally taken precedence over formal documentation of historical events. Personnel records from the early years had disappeared; dates found in books, documents, and newspapers were not in agreement. However, after much original "digging" by Mr. Jack Brooks, Jr., and later by Mrs. Cynthia Rouse, the Dahlgren picture began to develop. Throughout all of this, it became apparent that the best resources for the history of Dahlgren were the people who played major roles in the operation of the Laboratory over the years. Therefore, it was decided to let the Dahlgren story be told by those participants.

The chapters in this book are, for all practical purposes, in chronological order with the exceptions of Chapter I and the overview of Navy Labs given by Admiral Withington in Chapter XIII. Some of the information, however, does overlap as did the careers of the individuals.

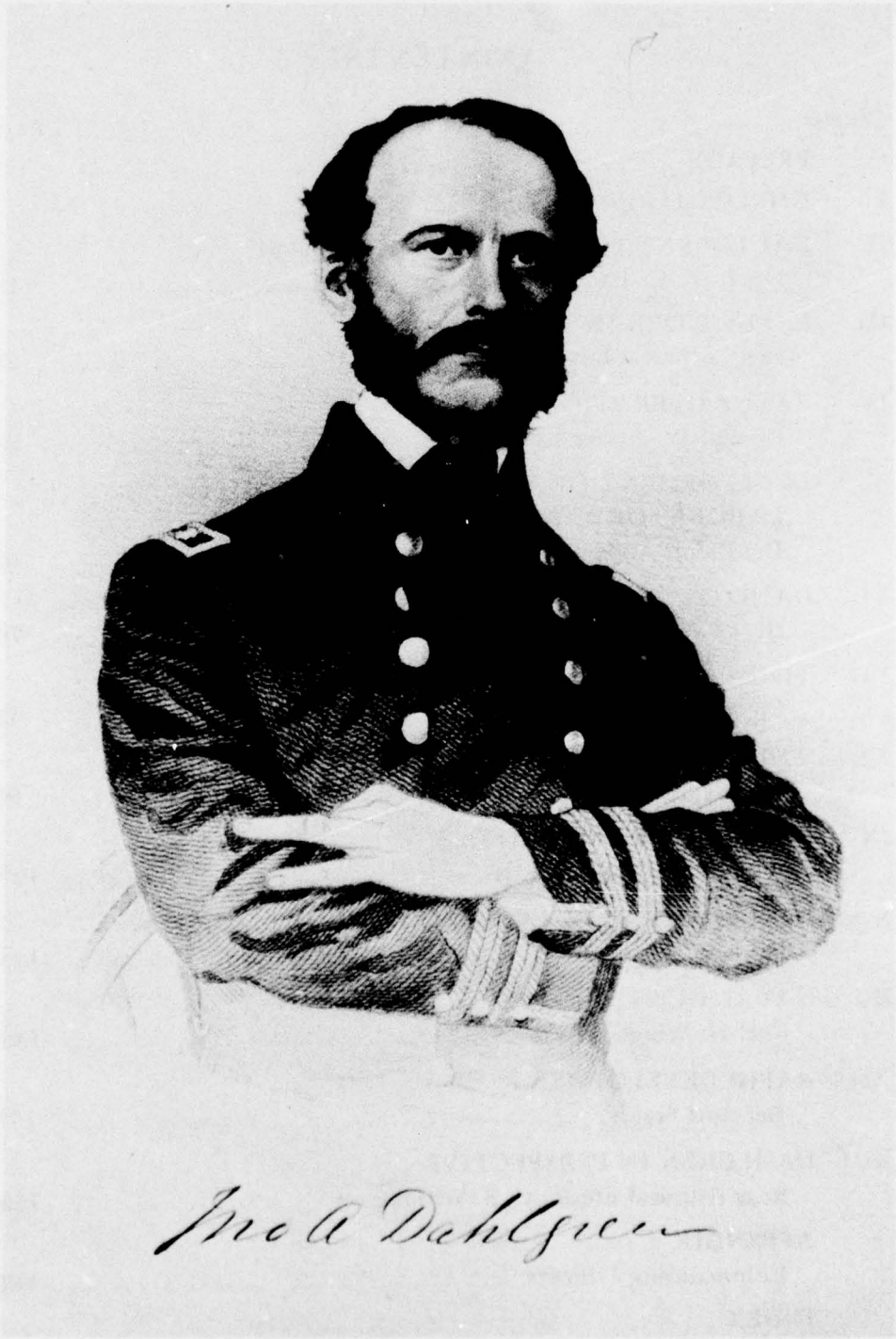
Chief credit for the data collection and interviewing must go to Mrs. Rouse who conducted most of the interviews, verified biographical and bibliographical material, and screened out a great amount of irrelevant and repetitious information. Thanks are also due to Mr. Bernard Smith for his suggestions and to Mr. Arthur Jones for his encouragement and invaluable advice during the project.

KENNETH G. McCOLLUM
June 1, 1977



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Rear Admiral John Adolphus Dahlgren, the "Father of Modern Ordnance."

CHAPTER I

Chronology*

By New Year 1918, the volume of proof and acceptance testing of ordnance continually being conducted at the Naval Proving Ground, Indian Head, Maryland, was causing intolerable safety hazards to Indian Head personnel and residents of the surrounding Virginia and Maryland communities. Private homes were endangered by shells overshooting the short range area. One residence near the Proving Ground belonging to a Mrs. Swann was hit by a shell that passed through the parlor and kitchen and emerged onto the porch. A more humorous incident occurred when a shell exploded near a cow grazing on a Virginia farm. The cow's owner, Molly Skinner, wrote the Navy Department claiming the frightened animal had refused to give milk since the incident. The affair was satisfactorily closed when Captain Lackey, the Commanding Officer at Indian Head,** purchased the cow for 30 dollars and had her transported by barge to a farm near the Proving Ground. Known thereafter as "Molly" by the Proving Ground personnel, the cow never gave another ounce of milk.[1]†

Following the Molly Skinner incident, a marine guard received a piece of shrapnel through his uniform sleeve. The torn uniform and shrapnel were submitted along with a letter of explanation to the Bureau of Ordnance. Within a month, orders from the Bureau temporarily halted all testing at the Proving Ground.[2]

The increasing hazards provided the opportunity for Rear Admiral Ralph Earle, wartime Chief of the Bureau of Ordnance, to push for the creation of a separate Proving Ground. On January 18, 1918, RADM Earle made a report to the House of Representatives' Committee on Naval Affairs concerning the state of the Bureau's various ordnance facilities, and he included a recommendation that a Proving Ground separate from Indian Head be located on a peninsula in Virginia adjacent to Machodoc Creek, a small tributary of the Potomac near Lower Cedar Point Light.††

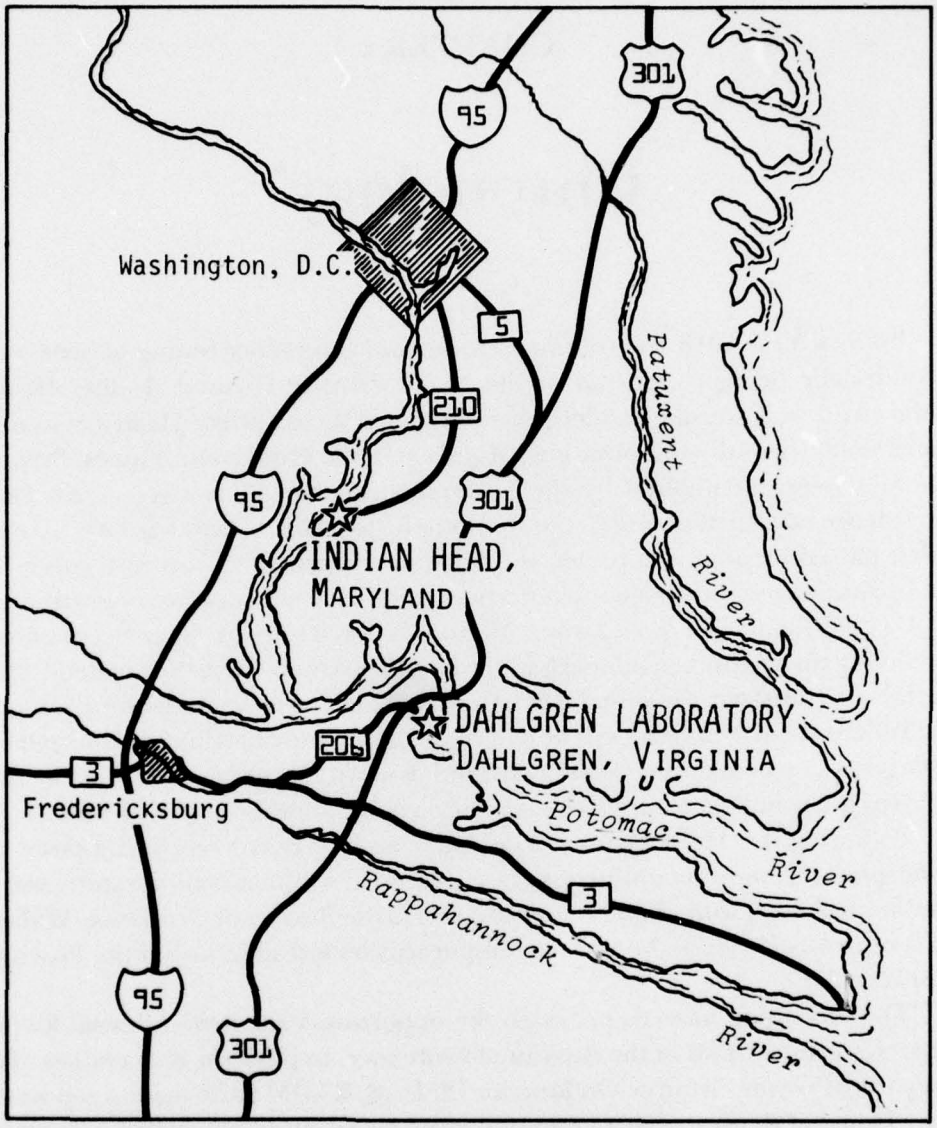
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*The information in this chapter preceding World War II was compiled by Mr. Jack Brooks, Jr., *former Historian for the Dahlgren Laboratory.*

**Captain Lackey, later designated Rear Admiral, presided over Indian Head from January 1917 until March 1920.

†*See references at the end of Chapter I.*

††RADM Earle had decided that this location suited the needs of the Bureau long before he officially suggested the site to the House Affairs Committee. In a memorandum dated November



Indian Head and Dahlgren locations

Following a discussion on the advantages and disadvantages of providing guns to the Allies, the committee chairman, Hon. Lemuel P. Padgett, referred to the bottom of page 39 of the Book of Estimates then under examination which provided for "increasing facilities for the proof and tests of ordnance

15, 1917, he inquired of the Chesapeake and Potomac Telephone Company their price for stringing a telephone line between Indian Head and Upper Machodoc Creek, the future site of Dahlgren. The phone company was cautioned not to "in any way disclose the Bureau's plans as such are merely under consideration."[3]

material, including necessary buildings, construction, equipment, railroad facilities, land, and damages and losses to persons, firms, and corporations resulting from the procurement of the land for this purpose, etc., $1,000,000." After noting the sum requested for the creation of a new Proving Ground, Chairman Padgett inaugurated the first official mention of the future Dahlgren Laboratory with the statement, "Tell us all about that item."[4]

RADM Earle was prepared to do just that. In response to the chairman's request, he inserted a statement into the record outlining the need for an additional Proving Ground. This statement was a classic summation of the arguments presented since 1910 by naval officers who recognized the inherent testing limitations of Indian Head.

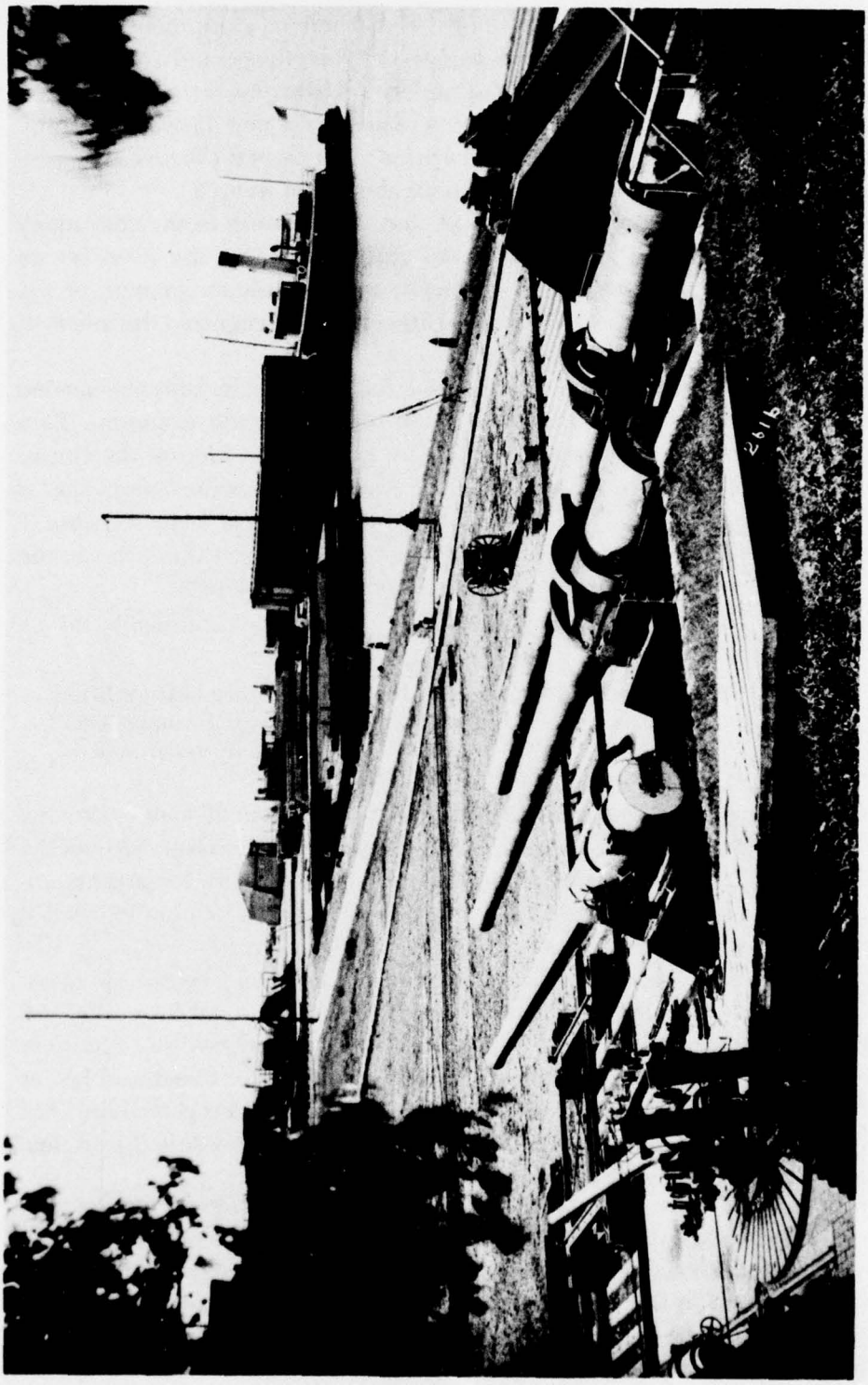
Throughout his testimony, Earle stressed that his estimates of funds needed for creating a Proving Ground were based on the strictest economy. To a question by a committee member as to why he wished to locate the future Proving Ground at the proposed site, Earle responded that the chosen site "is the only place that will give us proving grounds without large expense."[5] Inevitably, the question of how the firing guns would affect the farms in the surrounding area was brought up by Representative Littlepage.

> MR. LITTLEPAGE. Within what radius will the yolk of eggs be bursted by the firing of big guns?
>
> Rear Admiral EARLE. I can state from personal experience in that. I used to live at Indianhead, and we always were more successful with eggs right immediately under the guns practically, within 200 or 300 yards, than anybody was around the country. It does not seem to bother the hen at all.[6]

Summarizing his reasons for advocating the creation of a new Proving Ground, RADM Earle stressed the need for testing heavy guns fully without the limitations imposed by the range congestion at Indian Head. Regarding ammunition, he stated all ammunition should be tested "in the manner in which it comes aboard ship."[7]

Satisfied with the necessity of acquiring the new Proving Ground, Congress passed an act on April 26, 1918, authorizing the President to take over the land by Presidential Proclamation. In June 1918 by Proclamation of the President 1458, 994.3 acres of land were obtained between Machodoc Creek and Lower Cedar Point Light on the Potomac River. By Presidential Proclamation 1494 dated November 4, 1918, the Arnold Farm of 372 acres adjoining the original tract was acquired, giving a total of 1366.3 acres.[8]

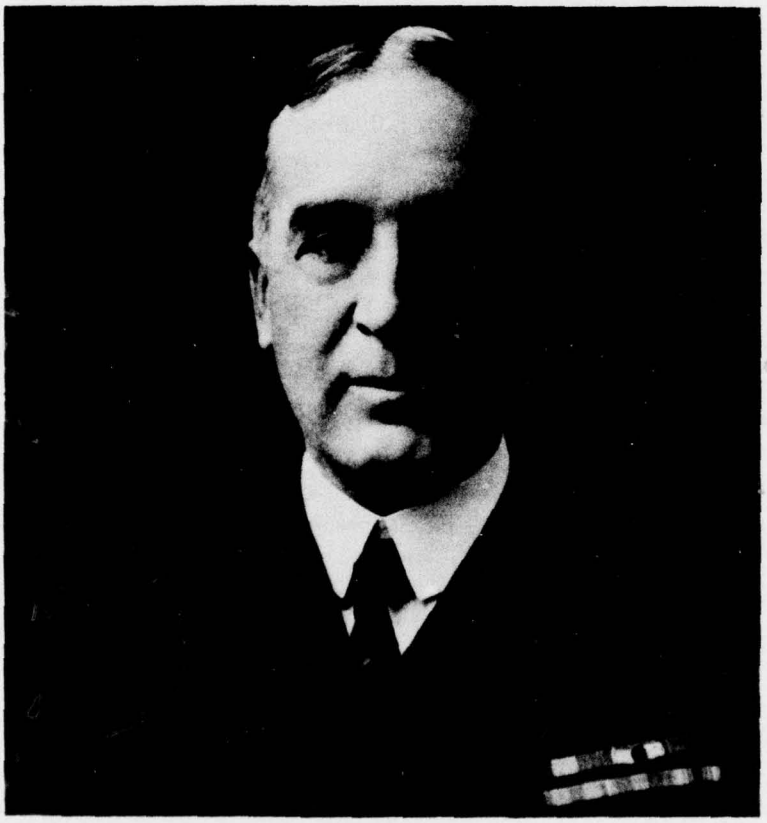
In December 1918, a recommendation was made to Congress for the purchase of Blackistone Island located about 30,000 yards downriver from the Proving Ground. Presidential Proclamation 1514 dated March 4, 1919, gave title of the island to the Navy, and formal possession was taken on June 18, 1919. The island was acquired primarily for use as a target for major-caliber projectiles which could be recovered for study.[9]



Main Battery at Indian Head, Maryland, November 1911.

On October 16, 1918, the Dahlgren "Lower Station" of the Indian Head facility* began its role as a Proving Ground with the successful firing of the 7-inch, 45-caliber tractor-mounted gun.[10] Army representatives present during the firing were favorably impressed, and an order was placed with the Bureau of Ordnance for 36 mounts—the exact number of 7-inch guns the Navy had available to turn over to the Army.

By the latter part of 1918, the new Proving Ground consisted of an administration building, inspector's quarters, Main Battery and Plate Battery bombproofs, eight officers' bungalows, 49 civilian bungalows, two civilian dormitories, sanitary sewers, and electric power lines along with storage and maintenance buildings. The Main Battery consisted of 12 emplacements which were laid along with two at the Plate Battery. Extensive dredging was also underway to provide a dock for barges and tugs, and material that was dredged



Rear Admiral Ralph Earle was Chief of the Bureau of Ordnance from December 1916 until May 1920. The Naval Proving Ground at Dahlgren was established primarily through his efforts.

*Dahlgren remained under the jurisdiction of Indian Head until July 1, 1932.

for the dock area was used as a land fill for the swamp and marsh areas that ran through the Proving Ground. Included in the early structure of the Proving Ground were aviation facilities consisting of a landplane hangar and seaplane hangar, each with a machine shop. A ramp was also provided for amphibious aircraft.[11]

In late 1918, it was decided that the new station should be named for a naval officer who had been prominent in the field of ordnance development. Included in the list was RADM John Adolphus Dahlgren, and in selecting Admiral Dahlgren RADM Earle stated:

> I chose Rear Admiral Dahlgren because I considered him the father of modern ordnance, for it was he who really pulled the Service out of a rut in ordnance in which the Service had been since the War of 1812, and built and advocated heavy ordnance.
>
> He did a great deal of his work at the Naval Gun Factory, was the first Chief of the Bureau of Ordnance, so that I considered it eminently fitting that an Ordnance and Gunnery building at the Naval Academy and our great Naval Proving Ground on the Potomac should carry the name of Dahlgren.[12]

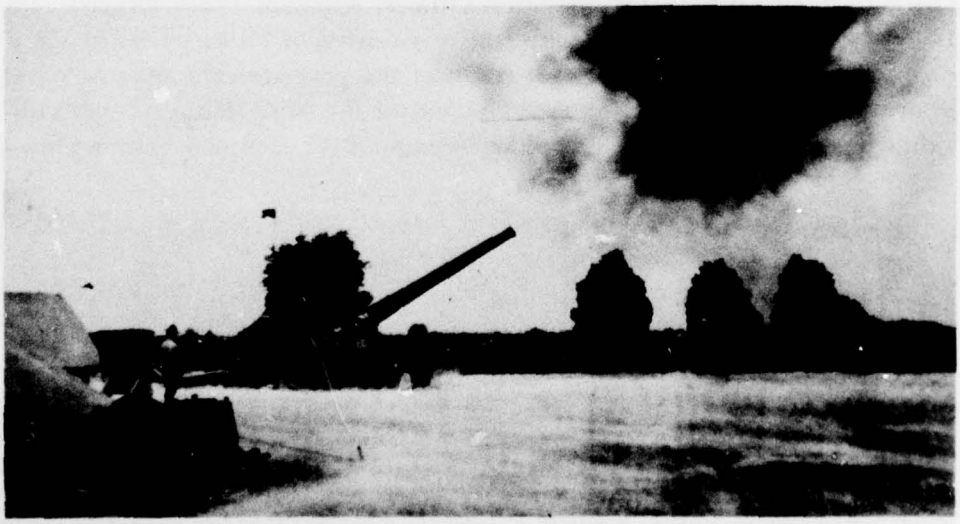
The Secretary of the Navy, on January 15, 1919, submitted the name to the Postmaster General who directed that the post office at the Lower Station be called "Dahlgren." Future official correspondence would be addressed to the Naval Proving Ground Lower Station, Dahlgren. In unofficial correspondence and in conversation, the Lower Station became known simply as "Dahlgren."

The new Proving Ground, removed from Indian Head yet under its authority, was assigned an Executive Officer to command under the direction of the Inspector of Ordnance at Indian Head. Initial organization consisted of Proof, Construction, Transportation, Aviation, Experimental, Supply, and Medical Departments. With the exception of Supply, each department was headed by an Officer in Charge who reported directly to the Executive Officer. Prior to January 1, 1925, an Indian Head officer handled all disbursements. After that date, Accounting and Disbursing were established independently at the Proving Ground.[13]

Construction at Dahlgren before 1919 had been confined to providing temporary housing for construction workers and personnel involved in proof, test, and range activities. From its inception, however, a permanent community was envisioned. As early as 1918 the Bureau of Industrial Housing and Transportation had drawn a housing plan for a projected community.[14] Preliminary drawings for an administration building of colonial design were prepared by Lieutenant C. W. Williams and Lieutenant W. C. Rehfuss, USNR, civil engineers attached to the Station. RADM Earle approved their plans on March 1, 1919.



*Rear Admiral H.E. Lackey, shown here as a Captain, was Inspector of Ordnance in Charge at the Naval Proving Ground, Indian Head, at the time the Dahlgren Station was established. He continued as Commander of both facilities until March 1920.**



The first shot fired at Dahlgren, October 16, 1918.

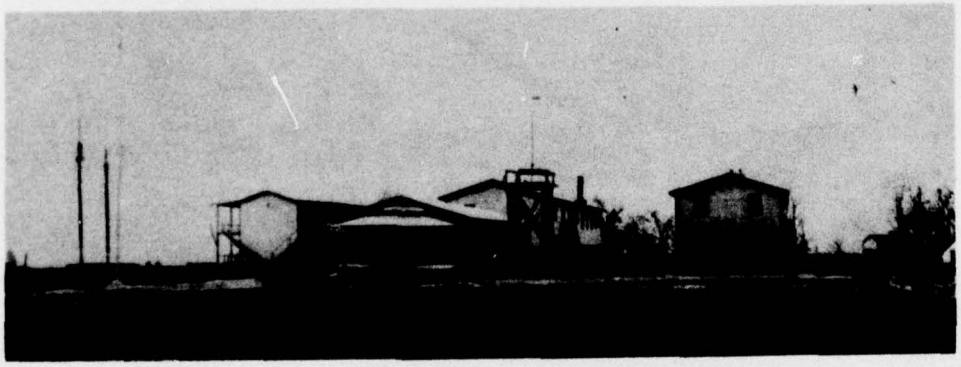
*In 1927, Admiral Lackey was in command of the Flagship MEMPHIS which brought Charles Lindbergh to the United States after his flight to Paris.

In the meantime, controversy arising from the design and cost of the Commander's quarters sparked an investigation of the necessity of maintaining the Proving Ground. The House Committee on Naval Affairs conducting this investigation heavily debated the need for proof-testing ordnance under conditions similar to those on shipboard. However, by the closing days of 1921, the issue was settled and the Dahlgren Proving Ground had decisively become a permanent fixture.

Even while Dahlgren's fate was being decided by the House Committee, an organizational structure and pattern of daily existence were developing at the Proving Ground that would remain largely unaltered until World War II. The primary area of professional activity centered around the Main Battery within which were sections that at a later date were to become independent batteries and departments. As the first permanent activity at the Proving Ground, the Main Battery furnished trained personnel to other ordnance activities as they developed.[15]

From 1919 to 1921, the Main Battery (later the Armament Department) was the sole ordnance materials testing unit at Dahlgren, including in its organization a Broadside Battery, a Fuze Testing Battery, and Ammunition, Velocity, Range, and Interior Ballistics Sections. The critical year 1921 also saw the submission of the first powder test report, dated March 10, to the Bureau of Ordnance.[16] After 1921, the Fuze Testing, Ammunition, and Velocity Sections were converted to other departments.[17]

The original personnel complement of the Main Battery consisted of four officers: Proof Officer, Experimental Officer, Assistant Proof Officer, and Battery Officer. The civilian complement consisted of 75 members in 1921, dropping to 25 the next year as a result of the government policy of naval disarming.[18] During the early development of the Main Battery, each individual assigned obtained a general knowledge of the complete activity; how-



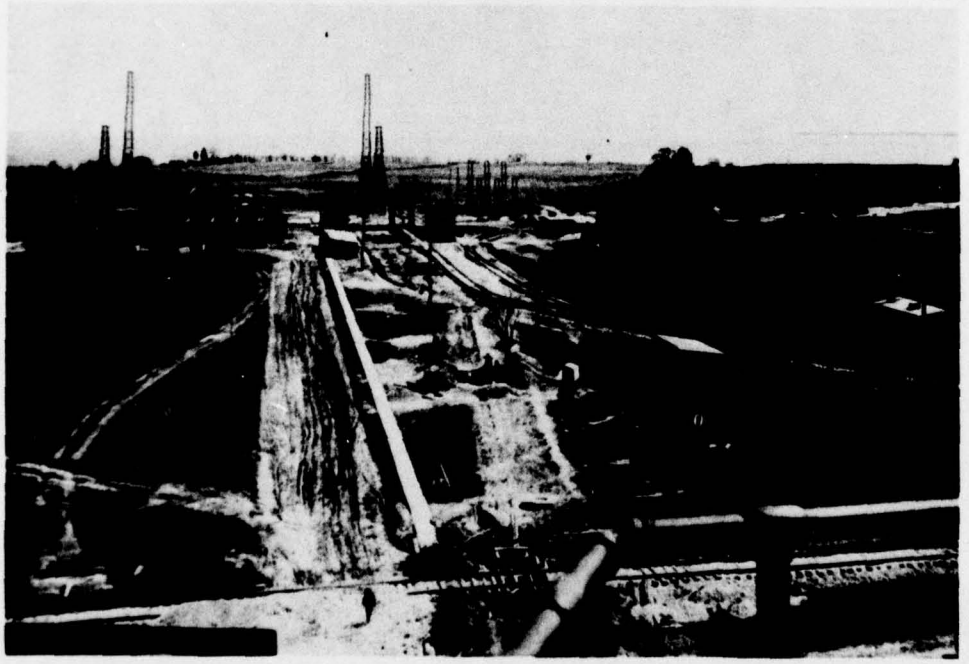
The first buildings constructed at the Lower Station were officers' barracks and a temporary office building.

ever, the gradual expansion of the activity led to ever-increasing specialization.[19]

Screen towers and rigging facilities for obtaining velocities of projectiles at up to 17° angles of fire were constructed in front of the Main Battery emplacements. The towers were to become the dominant physical characteristic of the Proving Ground. Additional facilities servicing the Main Battery were two gantry cranes, capable of handling 200 tons with trackage to service all major-caliber emplacements, and a locomotive crane with a 150-ton capability. A bombproof building at the Main Battery was positioned to allow for a powder stowage magazine, powder weighing room, constant-temperature powder stowage room, and office space. A small building also served as an ordnance workshop and storeroom.[20]

Expansion of the Main Battery facilities was begun on a small scale in 1926 and continued through 1935. During this time, two additional major-caliber emplacements for multiple gun mounts, one additional minor (or intermediate)-caliber gun emplacement, and two major-caliber gun parks were added.[21]

Maintenance of the river range at Dahlgren was formalized in 1920 with establishment of the Range Section, which at that time consisted of four individuals: a section head, an individual responsible for keeping records, and two



Construction of Main Battery at the Lower Station.



Results of first firing against armor plate at Dahlgren, July 25, 1921. The test was against 9-inch plate for USS INDIANA.

instrument men. Increased work loads led to the addition of two more men in 1932.[22]

Originally, there were 18 range stations on the Maryland shore and 19 on the Virginia shore. Some of these stations were not utilized in ranging guns but were necessary to tie in the survey. Because of difficulties experienced in landing range parties in winter, additional stations were added on the Maryland side.[23]

As a result of adverse weather conditions in 1933, five Virginia stations were added in 1936. Since then, all range activities have been conducted from the Virginia stations. Until after World War II, four lighthouses were used for range stations: Cobb Point Bar, Blackistone Island, Ragged Point, and Piney Point.[24]

Yardcraft range vessels manned by enlisted naval personnel assisted in range activities from October 16, 1918, when the first range boat, MURRAY, a Maryland Conservation Commission boat procured by the Proving Ground, landed the first range party. The complement of range vessels in 1922 consisted of two subchasers, a 40-foot motor sailer, a gig, and a large launch. Additional range vessels were not obtained until World War II.[25]

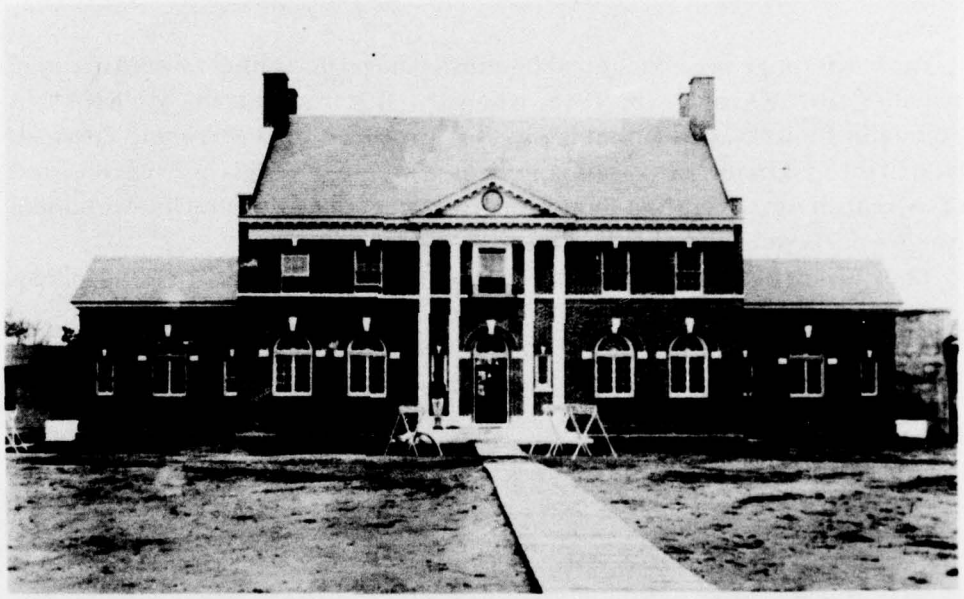
The nucleus of Dahlgren's future research and development (R&D) facilities lay in the Experimental Department. Since the establishment of the Proving Ground, an Experimental Officer had been among its group of officers and was in charge of the Plate Battery, experimental testing, and velocity readings.[26]



Lighthouse on Blackistone Island on the Maryland side of the Potomac, 1940.

In 1923, a civilian physicist was assigned to the Experimental Department with primary duties of analyzing experimental testing. The technical staff of the Experimental Department consisted of the Experimental Officer and Chief Physicist until 1935, when other professional personnel were added to assist the Chief Physicist.[27]

Dr. L. T. E. Thompson of Kalamazoo College, Michigan, was selected as the first Chief Physicist at the Proving Ground. His work in the Experimental Department laid the foundation for the military-civilian relation that was to precede Dahlgren's contributions to R&D in future Navy and other laboratories.



The Administration Building nears completion, May 1920.

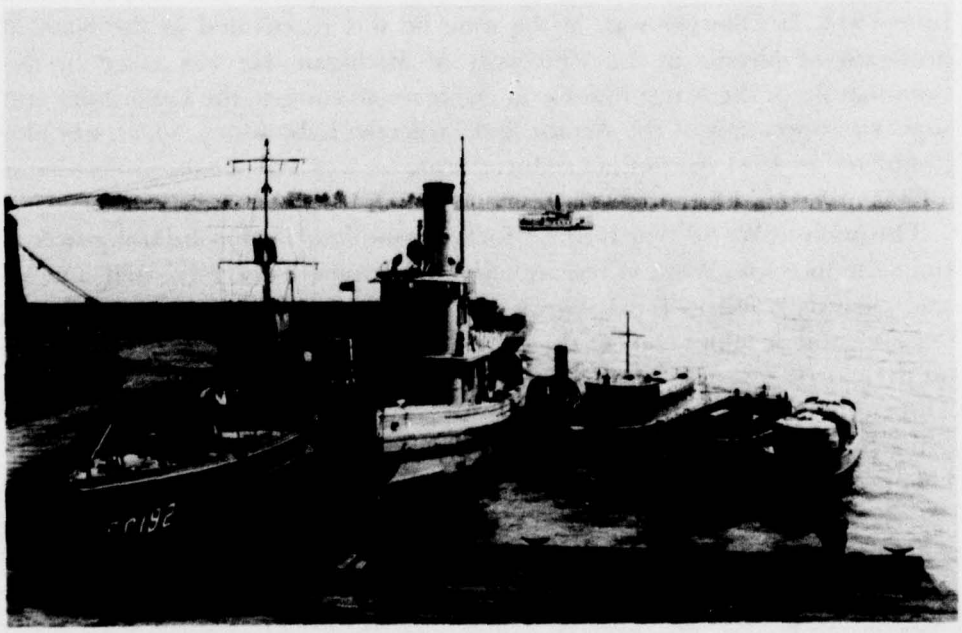


Bombproof building at the Main Battery, October 1921.

From 1924 to 1930, special ballistics studies and investigations conducted at reduced scale were performed at Dahlgren. These studies revealed that penetration results were closely similar at different scales for similar conditions of test. Impressed by the results of these tests, as early as 1927 the Dahlgren Proving Ground recommended that a laboratory be built for development tests and experimental work on armor, projectile, and assorted systems at small scale. Such studies were not only vital scientifically, but the economy resulting from their performance helped maintain the R&D programs of the Proving Ground during a period of severe economic restriction in military expenditures. Not until 1940, however, were funds for the creation of an Armor and Projectile Laboratory allocated by the Bureau of Ordnance.[28]

From 1927 to 1935, facilities for experimental work were extremely modest. However, an Experimental Laboratory was created and expanded between 1936 and 1940, and the Plate Battery evolved as a separate department. By the beginning of the war, the Experimental Department consisted of the Administration and Technical Staff plus the Velocity Section.[29]

Technical staff supervision was assigned to the Chief Physicist who acted as the Head Technical Assistant to the Experimental Officer. All personnel involved in bomb calibration, exterior ballistics, velocity measurements, armament tests, development work, and technical analysis of test results were included in the staff of the Experimental Officer and Chief Physicist.



Various boats used at Dahlgren for range support and transportation, April 1920.

Transportation problems due to the isolation of Dahlgren proved to be an early source of annoyance. Heavy test ammunition could be brought in only by water prior to World War II. After being unloaded at the Station dock, test ammunition was placed on railroad cars and hauled by a slow, cumbersome process to the old shell house area for storage. As roads to Fredericksburg improved after 1927, increasing use was made of trucks for transportation of small ammunition.[30]

During the 20's and 30's, the main mission of the Dahlgren Ammunition Department was the preparation of inert projectiles, explosive loading and fuzing, and supervision of the powder weighing rooms at the Main Battery. The expansion in naval ordnance development before World War II was reflected in the expansion of the Ammunition Department, and included in this growth were the technical changes in ordnance material testing. All projectiles fired at the Proving Ground before World War II were inert. Wartime use of high-explosive projectiles altered the duties of the Ammunition Department from inert loading and preparation of target practice ammunition to processing live wartime ammunition.[31]

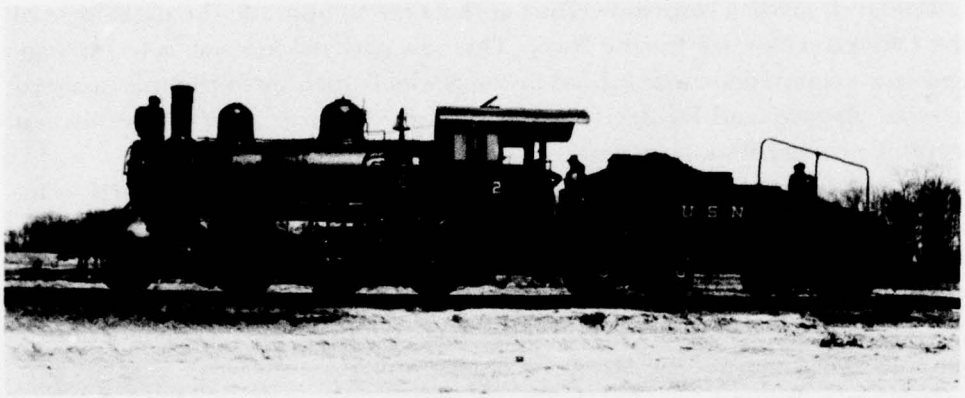
The entire nation had been expecting war, and the Dahlgren Laboratory was far from an exception. The summer of 1941 saw for the first time the influx of large numbers of professional employees into the ranks at Dahlgren. By and large, these were uniformed reserves that had been called to active duty, but their technical backgrounds added a new dimension at the Laboratory.

An excellent example of the new breed was Dr. Ralph Sawyer* who arrived in June 1941. Dr. Sawyer was, at the time he was reactivated in the Navy, a professor of physics at the University of Michigan. He was asked by the Commander of the Naval Reserve at Dahlgren to come to the Laboratory and supervise operation of the Armor and Projectile Laboratory, which was also completed in 1941 to conduct reduced-scale tests of armor and projectiles as well as other work in metallurgy.

Throughout World War II, the efforts in proof and testing at Dahlgren continued to increase. Work in research and development also increased, a trend that eventually led to the Laboratory's becoming primarily concerned with research and development. At this time, a small amount of work was also done on the closely guarded Manhattan Project for developing the atomic bomb. This led to the follow-on Elsie Project in which Dahlgren served as the Navy's primary test and evaluation agency for further work in atomic weapons.** The Laboratory continued this program until 1956 when it was taken over by the Atomic Energy Commission.

During the war, steps were taken to obtain large-scale computing devices for

*See Chapter V.
**See Chapter VIII.



Station locomotive for hauling heavy ammunition, April 1920.

processing ballistic data, and this led to the arrival of the MARK II Aiken Relay Calculator at Dahlgren in 1947. Even though crude by today's standards, this computer allowed much greater speed and volume of scientific calculations than had previously been possible either by hand or with desk-top calculators.

After World War II, the proof and testing of ammunition continued at Dahlgren as the Navy replenished its ordnance supply. However, the Laboratory lost a large portion of its professional staff as most of the reserve officers returned to civilian life. The few officers that did choose to remain at Dahlgren in a civilian capacity, together with the civilian scientists and engineers who stayed on, formed a competent nucleus that would in later years drive major research and development at the Laboratory.

The Korean War in the early 1950's again increased the proof and testing work at Dahlgren, and during the lull that followed the war, plans were considered for closing the Laboratory. On March 16, 1956, the following small article appeared in the Washington *Evening Star*:

> A cutback of 180 civilian employees is scheduled at Dahlgren Naval Proving Ground by June 30—a result of the new age of guided missiles, rockets, and nuclear weapons. . . . The proving ground's main mission is the testing of conventional guns, ammunition, and aircraft armaments.*

Fortunately, during this period the Navy began to consider the possibility of a large computing center and sponsored development of the Naval Ordnance Research Calculator. This machine, built by IBM for one dollar plus cost, was at the time the fastest computer in the world, and Dahlgren was selected as the site for its installation. With the increased computing capability came new scientific work, new facilities, and some influx of scientific personnel to operate and apply the computer facility.

---

* At the time of the proposed cutback, Dahlgren had 1510 employees.

Dahlgren made a conscious effort at this time to upgrade the usefulness of the Laboratory's work for the Navy. The new national interest in technology and space competition, stimulated by the Soviet launching of Sputnik, assisted to some degree, and Dahlgren was able to gradually draw in programs that were of primary importance to the Navy.

Since defense technology was shifting from guns to missiles, efforts to increase the scientific capability at Dahlgren were largely directed to new areas. In 1959, the Laboratory's responsibilities in the POLARIS Ballistic Missile Program continued to grow. A U.S. Naval Space Surveillance Facility was also established in the computer center and continued until 1961 when it became a separate command on the Dahlgren reservation.[32]

Many new diversified efforts at the Laboratory were directed toward electronics as well as gunnery; however, considerable proof and testing of guns and ammunition still continued, and rumors of closure again emerged. Fortunately, a major effort was already underway to acquire technical programs from the Navy's design and development organizations in Washington.

Under the guidance of Dr. Russell Lyddane and Mr. Bernard Smith,* Dahlgren enhanced its role in research and development of programs of major importance to the Navy. Dr. Lyddane succeeded in increasing the size and competence of the technical staff at Dahlgren, and by the time the Naval Proving Ground was officially retitled the Naval Weapons Laboratory in August 1959, over half of its work was in research and development.[33] A new two-million-dollar computer laboratory, approved in September 1962, increased the Dahlgren credibility, and by September 1962 the Laboratory had 1880 employees.[34]

During Mr. Smith's tenure, large numbers of young technical personnel were recruited to aid in weapons development. Management rotations were implemented to insure a flexible, well-informed management to promote new ideas. These efforts resulted in new programs that eventually led to involvement in complete weapons systems rather than isolated components of larger systems.

The Vietnam War in the 1960's again proved that guns are a vital part of modern warfare, and the proof and testing work at the Dahlgren range once again increased. However, this time it represented a small portion of significant research and development in other areas. The Dahlgren computer center increased in size and prestige with the expanding work on the POSEIDON and Transit Navigation Satellite Programs. New efforts in development of lightweight naval guns also emerged. Then in 1967, Dahlgren initiated the concept of guided projectiles fired from naval and marine guns. This opened new doors for work in fire control, electro-optics, ballistics, and guidance.

*Dr. Russell H. Lyddane was Dahlgren's Technical Director from 1956 until 1964. He was succeeded by Mr. Smith, who served in that capacity from 1964 until 1973.

The year 1969 saw the results of the technological efforts at Dahlgren formulated into a new mission statement: "To conduct a program of warfare analysis, research, development, test, evaluation, systems integration, and fleet engineering support in naval weapons systems, principally for surface warfare, and to conduct supportive programs in other warfare areas and fields of technology." This mission had been the dream of Dr. Thompson in the early days of the Proving Ground and had been pursued by all of the subsequent directors who believed in the utility of scientific research at Dahlgren.

The early 1970's evidenced technological progress at Dahlgren with items such as the 5- and 8-inch laser guided projectiles, EMPASS,* FLIR/laser** range finders for the MARK 68 Gunfire Control System, fire control software for TRIDENT, a rocket-assisted laser-guided bomb for use against coastal defense systems, and various electromagnetic vulnerability studies. The variety of this work is an excellent indication of the diversified expertise that had been accumulated at the Laboratory, and in September 1974 this competence in many fields was consolidated with the Naval Ordnance Laboratory, White Oak, Silver Spring, Maryland, to form the Naval Surface Weapons Center, which is the Navy's largest RDT&E center.

The merger of the Dahlgren and White Oak Laboratories has allowed the Center to utilize the varying expertise at each site to perform research and development work on major weapons systems such as AEGIS.† This program is under the general cognizance of the Combat Systems Integration Department at Dahlgren and promises to typify the versatile complex weapons systems of the future that will be developed at the Center.

*Electromagnetic Performance of Air and Ship Systems.

**Forward-looking infrared/laser.

†AEGIS is a fast-reaction, high-firepower, all-weather missile weapons system for use beyond the 1980's across the entire spectrum of surface warfare operations.

CHAPTER I

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CHAPTER II

Dahlgren's First Leading Scientist

Dr. L. T. E. Thompson

Dr. Thompson holds a BS from Kalamazoo College, Kalamazoo, Michigan, and an AM and a PhD from Clark University, Worcester, Massachusetts. He was instructor and then assistant professor of Physics at Clark from 1917 until 1919. From 1923 until 1942, Dr. Thompson was Chief Physicist at the Naval Proving Ground, Dahlgren, before moving to the Norden Company, and was Technical Director of the Naval Ordnance Test Station, China Lake, California, from 1945 until 1951.

Among the many other notable positions held by Dr. Thompson are Vice President of the Norden Labs Corporation; Vice President for Research, Norden Ketay Corporation; Consultant, Norden Division of United Aircraft Corporation; Consultant, Office of the Director, Special Projects, Navy Department; Member of the Ordnance Evaluation Group; Member, Advisory Group, Science Advisory Board, U. S. Air Force; Vice Chairman, Research and Development Board, Department of Defense; and Chairman, Technical Advisory Panel of Ordnance, Transportation, and Supply. He received the Civilian Service Award of the Navy Department in 1952 and the Distinguished Public Service Award in 1961. He served as a member of the Naval Weapons Laboratory Advisory Council from 1955 until 1966.

The following interview was conducted by Cynthia Rouse in Dr. Thompson's home in Scarsdale, New York, on December 8, 1976.

After World War I, most scientists who had been employed by the government to conduct military research programs returned to the academic community feeling that their tenure with the government had been a waste of time. Why did you, in 1923, choose to come with the government to such an isolated location as Dahlgren to continue your work in ballistics?

It's difficult to identify the primary factor influencing my decision, but people did have strange feelings about working with the military. That was one of the reasons why I felt it was so important to continue working with them. The military needed the kind of help that could come from people who had been



Dr. Thompson at the Dahlgren Laboratory, April 28, 1967.
(Thompson Road was dedicated in his honor at that time.)

working in the scientific world, and I can think of many other reasons a person would be interested in doing work that is important to the country and could affect the country. It was perfectly obvious in those days that we needed this kind of help for the military.

What personal goals did you set in approaching your work? What were your objectives?

In those days, I just wanted to get started with a program of work that would be significant in the military field, so my goals were to set up a framework that would be helpful, particularly to the Navy, in accomplishing what they were trying to do through the aid of the additional machinery that comes with a scientific environment. It was the appreciation of the importance of that framework in making progress that was lacking in the early days—not necessarily just at Dahlgren. It was a question of the military establishment as a whole not being certain that they needed the help. On the other hand, many of the individuals in the establishment were quite interested in the scientific tools that had to be used in order to get ahead.

What was the initial environment like at Dahlgren for conducting your work?

If you mean by environment the attitude of the people toward work of this kind, there was obviously a considerable need for a better understanding of what the work was about and what it was intended to do for them. I'm speaking now about the people in the military establishment in general—not just about Dahlgren. In the early days, people thought of this kind of work as something being imposed on them, not something that was necessary for them to get ahead. Later, they changed that view and became very much interested in getting scientific help. It worked out all right, but it took a little while. I don't mean it was done as a result of our work at Dahlgren, but the country as a whole became more aware of the need for the tools that the scientific establishment could provide to allow advancement in the military field that they depended on.

Since Dahlgren was under Indian Head at the time, did you feel any pressure from that area?

No, nothing in particular. There was always a great deal of back and forth discussion on common interests, but not many formal meetings.

Did you get much support from the Navy's Bureau of Ordnance and from Washington in general during the 1920's and 1930's?

The Bureau of Ordnance and other parts of the military establishment in that area were interested, but they were not as impressed with the importance

of the scientific work in the early days as they were later. That wasn't their fault. It was just part of a "growing up" process.

Dahlgren was once a major stop for postgraduate officers from the Naval Academy. Were you satisfied with the abilities and attitudes of most of these young officers who came to you for training?

The abilities were certainly there. They were able young men who had gone through the Navy postgraduate course and did good work. The kind of training they had to have before they came to Dahlgren was not always the kind that would be recommended now to get ready for that sort of program, but I think it worked out as well as you could expect under the circumstances—starting out from scratch. Primarily, what the younger officers who came to Dahlgren gained was that they had a chance to see firsthand how all the pieces fit together in the development of weapons for the Navy. I think it was worthwhile for them and for Dahlgren.

There were very few civilian professionals at Dahlgren in the early years. How did you find the working relations between civilians and the military?

It went all the way from one extreme to the other. In some cases, there were military people who were very sympathetic with the work that we were trying to do, and they did what they could to help us. But there were also many cases where people just didn't understand the importance of the work and thought of it as kind of an impediment. They weren't very cooperative. However, in many cases, they were quite cooperative with me, personally. The difficulty was that the atmosphere set around this sort of work was not especially developed.

There was a lot of work in aviation at Dahlgren during the 1920's and 1930's with tests of machine guns, bombs, and the very fine Norden bombsights. Did you support this effort and feel that Dahlgren was a proper place for such work?

It certainly was the proper place, but we had to improve the facilities for doing the work, and that was precisely what we were trying to do. Of course I had quite a bit more to do in my area with the other kind of work that was already in progress at Dahlgren just to develop the work as well as we could and try to increase the interest in it.

Can you give us the general background of your work in small-scale testing of weapons that eventually led to the establishment of the Armor and Projectile Laboratory at Dahlgren in 1940?

The more we got into finding out what the problems were, the more we realized that we had to do what we called fundamental work. We had to

understand what was going on in the field of ordnance, and in order to do that properly, we thought we needed testing at various scales. We needed to have a lot of small-scale testing because we could do a great deal of work in that way that we couldn't get done at all in large scale. Large-scale testing was so expensive, and in some cases there was a lack of interest.

Admiral Parsons was a great scientist as well as an outstanding naval officer. Can you describe your companionship with him at Dahlgren?*

That was one part of the experience at Dahlgren that was most encouraging to me and most satisfactory. He did what he could to encourage the kind of work I was trying to do, and I've never forgotten what that meant to me.

I understand you were very close friends.

Yes, it was very fortunate that he could be there at that time.

Can you describe some of the major problems you encountered during your stay at Dahlgren?

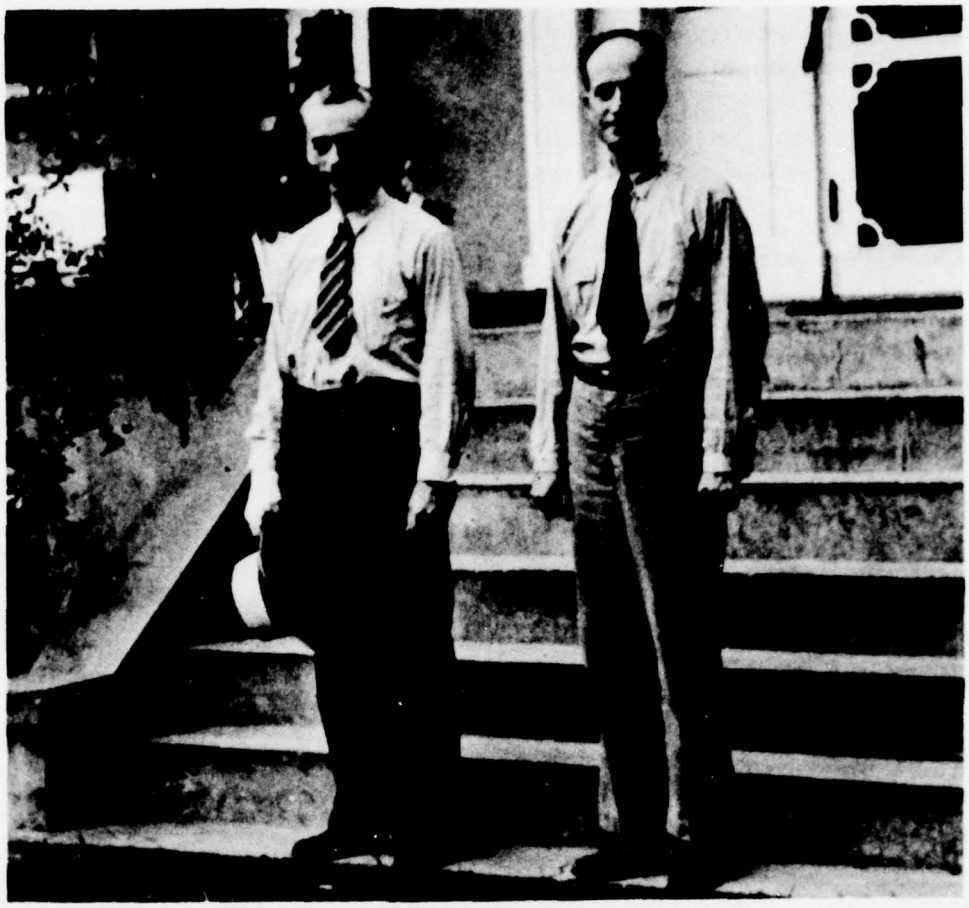
That was the time when the work at the Proving Ground was just getting started in the direction that eventually became quite prominent there. It was difficult in those days to get very much support, financially, for any large-scale work. That was the biggest problem. Funding.

World War II greatly increased the work at Dahlgren. When did you first feel the inevitability of war, and what was your reaction?

I'd always felt it was inevitable, but I don't think there was any abrupt occurrence that made it perfectly clear, except for the developing political situations which created the likelihood of there being trouble of this kind. One couldn't help thinking more and more about what was necessary to get ready. We had to be interested in the things that the military establishment lacked for dealing with a major conflict, and we had to build up the scientific foundations for supporting that type of thing.

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* Rear Admiral William Sterling Parsons served at Dahlgren between July 1939 and June 1943 as the Experimental Officer for the Naval Proving Ground. Admiral Parsons assisted in the development of the combat radio proximity fuze. He is also noted for his assistance in the development of the atomic bomb and was Weaponeer and Bomb Commander in the bomber that dropped the first atomic bomb used in warfare on the city of Hiroshima, Japan, on August 6, 1945. Admiral Parsons died in 1953 while serving as Deputy and Assistant Chief of the Bureau of Ordnance, Navy Department.



Dr. Thompson (left) and Admiral Parsons (then Commander), Experimental Officer of the Proving Ground.

Can you comment on how you approached general management problems in order to reach your objectives?

This is all part of the general area I referred to earlier. What do you do to get the best work from the people involved? It was the thing that was not so easy to do in the early days—getting a response that seemed adequate to meet the requirements of the facility. You couldn't do scientific work at a place like Dahlgren without having technical foundations that were necessary to understand the problems and then to get at them. We had to have a gradual building up in interest and in understanding of the problems that were necessary to solve.

There were many people in the old days who thought it was a waste of time to do a lot of looking into underlying research questions, and yet a good understanding of those underlying questions was necessary. It was perhaps one of the

most important things that was dealt with in the early days and probably still is. Anything that can be done to give younger people and others a better opportunity to understand how progress is made in the direction of new systems is extremely important. A lot of people used to think, and I suppose some still do, that you just decide you are going to develop something and the rest of it takes care of itself. You must, however, have built into the development the machinery for acquainting people with the kind of research that is necessary on the foundations of these new concepts. In those days, this was an overriding problem.

From Dahlgren, you went to the Carl L. Norden Company and then eventually became Technical Director of the Naval Ordnance Test Station at China Lake, California. In retrospect, had your expectations at Dahlgren been fulfilled?

Yes. I think, as far as my expectations were concerned, I knew it was a high uphill road because the support that was necessary for that kind of work had to be based on a very close understanding of the connection between the scientific program and the regular proof and test work that the Navy had to have from Dahlgren. So as time went on, the realization grew that we had to have the technical work and that it was being done better. In the early days, we used to be pretty discouraged because we thought that we weren't going to get support for the things that we thought were necessary, but that feeling gradually went away. We had lots of good support, as well as some that wasn't so good.

The fact that some scientific work was possible in the naval establishment led to improved work in the field. There were quite a number of naval officers at Dahlgren at the time who believed in what we were doing and did what they could to get a scientific program in perspective with the planning for the Proving Ground.

Again, had my expectations at Dahlgren been fulfilled? I would say, "Yes." I definitely would. I have no basis for an independent opinion on how much of this kind of work was necessary in the early stages in order to create a successful framework for developing a center kind of work, but I can say that what has come out of that is, I think, a very substantial confirmation that the Station was working in the right direction and made a contribution. This is a credit to the Station. I'm not saying this to set up a foundation for a feeling on my part or anybody else's part that the specific things we produced turned out to be extraordinarily good weapons systems, etc. That isn't the point. The point is that the merit of doing this kind of work was confirmed to a considerable extent by the results that came from Dahlgren early. Of course the work was continued at various other places, and finally the Naval Ordnance Test Station at China Lake was set up. That whole center was set up to do scientific work. The work at Dahlgren constituted the substantial foundations for the realization that a center like China Lake was necessary.



*Admiral Harold R. Stark, at the rank of Captain, was Commander of the Naval Proving Ground, Dahlgren, from November 1925 until September 1928 during Dr. Thompson's tenure as Chief Physicist.**

*Admiral Stark later served as Chief of the Bureau of Ordnance (1934-1937) and then as Chief of Naval Operations (1939-1942). He was assigned as Commander of the U.S. Naval Forces in Europe in 1942 and was in charge of all U.S. naval forces assigned to British waters and to the Atlantic coastal waters of Europe, including those assigned through him to the Allied forces for the Normandy landings in June 1944.

There were other experiences within the Navy that confirmed the value of the experimental and developmental work, but the fact that this occurred within the Navy itself meant a great deal to the chance of having it set up properly. I can remember the discussions that we had when we were getting ready to set up the center at China Lake. Of course, at that time, it was well accepted that the sort of thing that had been done at Dahlgren was a beginning. You have to have a starting point somewhere. It wouldn't have been as easy to get ahead at China Lake without the kind of beginning we had at Dahlgren. I'm not speaking specifically of a weapons program, but the kind of philosophy of how to do weapons programs.*

The evolution of the philosophy of how to conduct weapons programs had two major periods. The first one was the Dahlgren period, and I doubt if we would have had anywhere near as much success with those programs that came later if it hadn't been for the Dahlgren experience, although a great many people did not have that feeling or that interpretation of the early Dahlgren experience.

I can imagine how someone sitting here listening to this would think, "Why did it take so long to do it? Why didn't they get busy and do it earlier?" Well, there was, as I said, a framework that had to be built. We had people with various backgrounds and various ideas about what was necessary to do good work, and they didn't all agree. It took time to build this up, and it took the kind of experience that happened at Dahlgren and at China Lake later to create the foundations for successful programs in weapons work.

* This philosophy evolved into the concept of self-containment for a research and development facility. This encompasses the ability to conceive military devices, theoretically analyze their potential, make the hardware, test the performance, and evaluate the results all at one place in order to save time, money, and manpower.—Ed.

CHAPTER III

Early Work in Aviation

Rear Admiral Boynton L. Braun

Admiral Braun was appointed to the U.S. Naval Academy in 1917 and graduated in 1921. He served two tours at Dahlgren and was at one time Officer in Charge of the Dahlgren Air Detail. Admiral Braun performed gunnery duty aboard USS NEW YORK and ARIZONA before flight training in 1931 when he was designated naval aviator. He was then assigned to Torpedo Squadron I aboard LEXINGTON and was later designated Executive and Commanding Officer of Patrol Squadron 9 aboard WRIGHT. In June 1939, Admiral Braun was assigned to the staff on the flagship MEMPHIS in the Aircraft Scouting Fleet and then was given staff assignments with the Chief of Naval Operations and the Bureau of Ordnance.

In October 1943, Admiral Braun (then Captain Braun) assumed command of the carrier MANILA BAY and participated in action against the Japanese in the Marshall Islands, Admiralty Island, Hollandia, Aitape, and Marianas operations. He later commanded BENNINGTON in attacks and seizure of Japanese islands. Following this duty, Admiral Braun served as Commander of Naval Training Bases at Corpus Christi, Texas, until his retirement in September 1947.

The following interview was conducted by Mr. Jack Brooks, Jr., in Admiral Braun's home at Mathias Point, Virginia, on May 9, 1975.

Admiral Braun, you mentioned that you came to Dahlgren in 1929. Could you give me a little background on your career before that time?

I served on battleships in the Fire Control Divisions from the time I graduated in 1921 until 1927, and then I went to Postgraduate School and took aviation ordnance.

I think that some of the first postgraduate officers were still at Dahlgren around that time. Postgraduate School wasn't too old, was it?

No, I think the Postgraduate School started back in Annapolis when I was a midshipman. In fact, some of the postgraduate students later served under me down here at Dahlgren. It was just country through here then.



Admiral Braun controls a BG-1 at Dahlgren in 1935 during tests of a Sperry dive bombsight.

You were, of course, an Air Officer.

Yes.

This was in the early period when we were developing our carrier capabilities.

Yes.

Did you have any association with the early carriers?

I had been in the first heavy dive bombing patrol squadron based on a carrier, and I also had patrol plane duty for horizontal bombing. Most of my first six years, though, as I said, were in big gunnery with battleships in Fire Control Divisions.

That was something that Dahlgren was trying to assist in?

They had a lot to do with, of course, all the guns the Navy used, and at that time up until World War II, all the guns were tested right here at the Naval Proving Ground. The Dahlgren Air Detail in those early days, in the 1920's, was

working on an inertia catapult for catapulting planes off carriers, and they also had a radio-controlled airplane here.*

I think a lot of that work was carried on down here.

Yes, quite a bit of it was. Lieutenant Ballentine** was the Officer in Charge of the Air Detail at that time.

Later to be Admiral Ballentine?

Yes, that's right.

You mentioned being in fire control and gunnery after graduation. How did you get your wings? It seems that fire control and gunnery would almost be a career in itself.

When I was in fire control aboard the battleships, it looked to me as though the future was going to be in aircraft with their bombs. We had reached more or less the limit of long-range guns.

That was a rather heretical view for a Navy man, wasn't it–in light of Billy Mitchell† and his Army bombing?

Aerial bombing had its prospects, especially dive bombing, in the early days when they had nothing but the carrier LANGLEY.‡ He [Mitchell] came around with his airplanes in operations there with the battleships off the Virginia Capes with poor results. Airplanes came out and made a mock dive-bombing attack on the ships. The LANGLEY never saw them, never heard

*The first successful testing of a radio-controlled airplane took place at Dahlgren on September 15, 1924, under the leadership of Dr. A. Hoyt Taylor of the Naval Aircraft Radio Laboratory, assisted by Mr. C. B. Merrick, a capable radio engineer, who was assigned by the Head of the Radio Division, U.S. Naval Research Laboratory. An N-9 aircraft was used for the testing and was fitted with the Norden automatic control system and a radio-controlled system designed by Mr. Merrick.

**Admiral John J. Ballentine reported to the Naval Proving Ground, Dahlgren, Virginia, in June 1922, where he served as Officer in Charge, Naval Air Detail, until February 1926. In 1923, the then Lieutenant Ballentine conducted the original tests of the first bombsight designed by Carl Norden, a Navy consultant. While on duty at Dahlgren, Admiral Ballentine controlled, from the ground, the first airplane operated by radio control. Following various assignments with the Asiatic Fleet, he returned to the United States in August 1927 for another tour of duty at Dahlgren as Officer in Charge, Naval Air Detail, until June 1931. He retired from the Navy on May 1, 1954.

†Major General William [Billy] Mitchell, a famous aviator for the Army from 1914 to 1926, directed massive aerial bombing attacks against the Germans during World War I. In 1919, he came home from Germany as the United States' only flying general.

‡LANGLEY was the United States' first aircraft carrier. It was commissioned in 1922 as a carrier and converted and later reclassified as a seaplane tender in 1937. It was sunk by Japanese aircraft south of Tjilatjap off the south-central coast of Java on February 27, 1942.

them until they were right down on us. It made me realize then that they could fly over 100 miles to get to us and bomb us, and the battleships couldn't reach that far with a gun.

It seems that dive bombing, as a technique, was pretty much an American invention and contribution.

It was entirely.

The Germans get so much credit for it in the early part of World War II. Dive bombing, I think, was also practiced here at Dahlgren.

We had to do a lot of it here while I was Officer in Charge of the Air Detail. In addition to bombing with the horizontal bombsights like the Norden MARK 11 and the MARK 15, we were trying to bring out a dive-bombing sight. Norden was also bringing out a dive-bombing sight, but that came here for tests after I had gone.

Now let's go back a little. You flew off LANGLEY?

No, I didn't fly off LANGLEY. It was LEXINGTON.

The first LEXINGTON. Was that a converted cruiser?

No, a converted battle cruiser—LEXINGTON and SARATOGA. They took two battle cruiser hulls and just converted them into carriers.

These were LEXINGTON and SARATOGA that were serving in the Pacific.

That's right—LEXINGTON was lost at the Coral Sea battle.

I interrupted you earlier when you were talking about dive bombing. When did you take flight training? You mentioned that after Annapolis you went into naval gunnery.

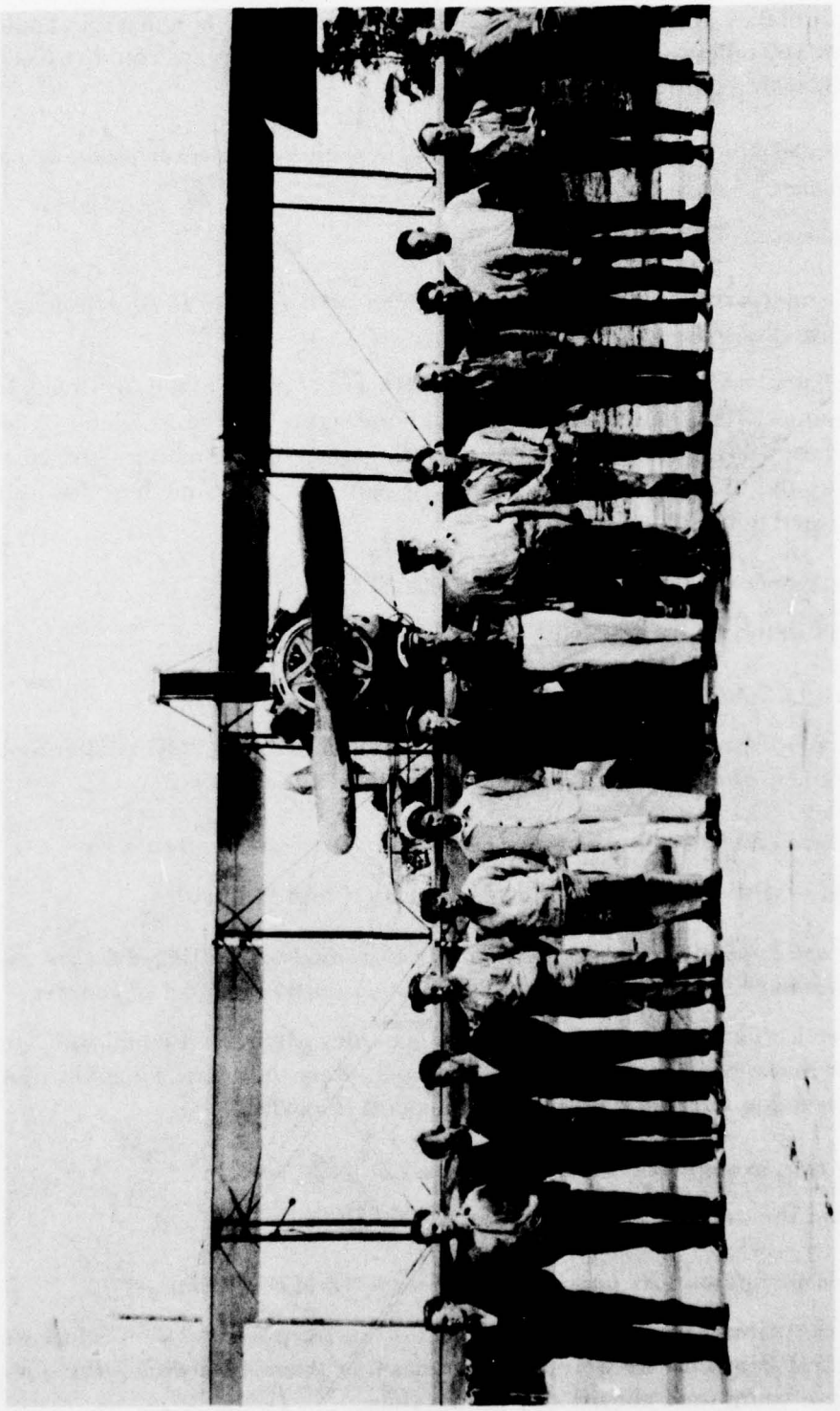
After I became interested in flying, I got rides whenever I could with any aviator that was available. This included catapulting off a ship. I actually took flight training in the fall of 1930 in Pensacola, Florida.

At that time, was it a short duration of training or fairly long?

About the same as right now.

Were the aircraft you were training in pretty much World War I vintage?

No, the primary trainers were built just for that purpose, and then before we finished at Pensacola we were put in modern airplanes for awhile—the ones that were being used aboard ship at that time.



Admiral Ballentine (directly under center of propeller) guided this N-9 seaplane on the first pilotless radio-controlled flight in 1924. To his immediate right is Mr. C. B. Merrick, who was instrumental in designing the control equipment.



Carl Norden with one of his bombsights shown installed in aircraft.

I believe our Navy at that time was accused of using bombing aircraft and various other types of aircraft in the Navy instead of one, perhaps, single aircraft up until almost the beginning of World War II. But this, supposedly, was their intent–to test various models to find a specific type.

The dive bombers had to be specifically designed because the wing structure of an ordinary airplane wasn't strong enough to take the stresses of dive bombing and pullout. They were always trying to bring out something new which was designed for that purpose.

Did you have any occasion to fly Helldivers? They were, I think, the first aircraft with a canopy that closed.

Yes, I flew Helldivers. I think Grumman brought out the FF class, which was the first that had a canopy put on there as such. On the earlier planes, we just had a windshield in front of us. It was wide open.

When were you first assigned to Dahlgren?

I came here in the postgraduate course in September 1929. The course was about 6 or 8 weeks long. Then I remember I left in late October.

This was a course primarily in gunnery.

It was a course in the first Norden Bombsight when the MARK 11 was going through tests here. They [the bombsights] were just about to be sent out to the Fleet, so on my PG tour I became very interested in the bombsight. We had, during our postgraduate tour, what we called a "Cook's tour" about our third year. We visited various government installations which handled or developed certain types of equipment. We went up to Radford Arsenal, New Jersey, for machine guns. One of my tours was in New York with the Norden Company. I got interested in their tests and in building the bombsights and balancing the gyros. And there was a question of keeping them properly balanced—how do you do that? Just as I finished my postgraduate tour, I was sent back here to start the first bombsight school. We had to teach mechanics how to maintain the bombsight. Then we also had to teach pilots how to use it and operate it in the air, so I was sent back here to start the first school. With my experience in the PG tour and also with my work at the Norden Company in New York, I guess they assumed I would be the logical one to start the school, so we started from scratch.

This was in 1930.

Yes.

Was there a formal name for the school?

Just Horizontal Bombing with the MARK 11 Norden Bombsight.

Which was a function of the Naval Ordnance Section?

That's right. All of it was under the Bureau of Ordnance. Of course, the Dahlgren Proving Ground was directly under the Bureau of Ordnance.

So you more or less moved to Dahlgren in [June] 1930 when it was in the more primitive state.

I stayed here until late October. I think I left about Halloween. There wasn't any information on the bombsight, and all I had were a few blueprints from the Norden Company. I had to write up the first pamphlet on the Horizontal Bombsight, the MARK 11. I would write it up, and I wasn't given any clerical help. My wife* was good with the typewriter, so she'd type a couple of stencils and then we made a mimeographed pamphlet. I had to make the sketches. The pamphlet was not only on how to operate the bombsight, but it gave the

*Mrs. Braun is the former Miss Mary Lewis Ashton of Hooes, Virginia.

rudiments of electricity and electrical circuits to the mechanics. Some of them didn't know anything about electricity, and since this was entirely an electrical problem, we had to start from scratch to teach them the circuits. So she [Mrs. Braun] turned out the stencils, and we had the mimeographed copies made and gave them serial numbers. Then they had me go up to the Navy Yard in Washington where they had printing facilities, and they turned out the first pamphlets on the bombsights.

This was the result of your research and work at Dahlgren.

That's what I had to turn out with the help of my wife. I couldn't have gotten along without her because she typed the thing up. It turned out to be a pretty good size book.

You mentioned your wife. I imagine this had to be a desolate assignment for her.

I had a roommate aboard ARIZONA who they wanted to send here as an Ammunition Officer, and he said he wouldn't come down here—nothing but country, and he wasn't going to bring his wife down here. When I got here, I loved it. I liked the country and fresh air. I met my wife, a local girl, while I was on my PG tour here.

How long was your last tour at Dahlgren?

I was ordered back here as Assistant Officer in Charge of the Air Detail in 1934. Lieutenant Boone,* who's now Admiral Boone, was in charge, and in 1935, he was transferred, so then I fleeted up to Officer in Charge of the Air Detail. I was here 3 years from June 1934 until June 1937.

Then you have good familiarity with Dahlgren in the 1930's.

Yes.

Did you know Admiral Parsons?

Yes, we went through Postgraduate School together.

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*Admiral Walter Frederick Boone (with the rank of Lieutenant) was Officer in Charge of the Naval Air Detail at Dahlgren from June 1933 to June 1935. He received numerous decorations and medals while on successive aviation assignments from April 1926 to December 1945. While on special duty at the American Embassy, London, England, from 1939 to 1942, he witnessed the latter stages of the Air Blitz. In 1958, Admiral Boone became U.S. Representative of the Military Committee and Standing Group of the North Atlantic Treaty Organization and remained in that capacity until his retirement from the Navy in 1960.

He later became the so-called "Atomic Admiral."

He also worked with the sensitive fuze.

The VT fuze, sometimes called proximity fuze.

When I came back here in 1934, the next series of horizontal bombsights made by Norden was starting through—the MARK 15.* It was called the pickle-barrel sight because of its accuracy.

Testing of the MARK 15 was conducted at Dahlgren.

That's right. We had to drop eight bombs with every bombsight that came through and make adjustments when the bombsight failed to meet the prescribed standard for accuracy.

And you did the dropping in the area now called Pumpkin Neck?

No. The Air Detail office was down at the seaplane hangar, and we had a target right off there. Later, we put one up the river because the bombsights were coming through in such numbers we had to have several planes testing at one time dropping bombs.

Much of the credit for teaching and instructing the bombsight mechanics goes to Mr. C. C. Middlebrook. He had been a Chief Electrician's Mate in the Navy and was assigned to the Naval Proving Ground at the time Mr. Norden commenced his work here. He was an outstanding and capable man, and the meritorious work performed by the bombsight mechanics afloat is an excellent testimonial to the perseverance and ability of Mr. Middlebrook. Every bombsight which went to the aircraft squadrons was given a rigid and thorough inspection by Mr. Middlebrook before it was shipped.

Did you ever meet Dr. L. T. Thompson?

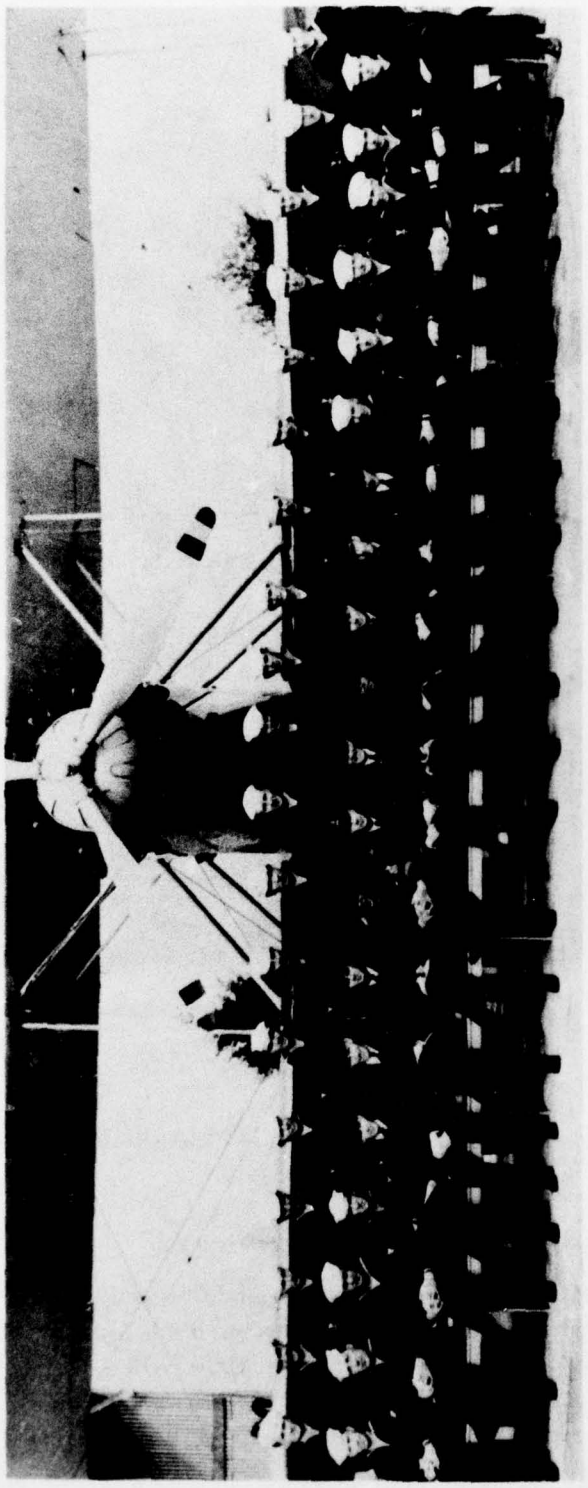
Yes. Dr. Thompson was here as Senior Physicist when I was here. In fact, he was here when I went through the postgraduate course, and he was still here when I left in 1937.

He was our primary civilian scientist.

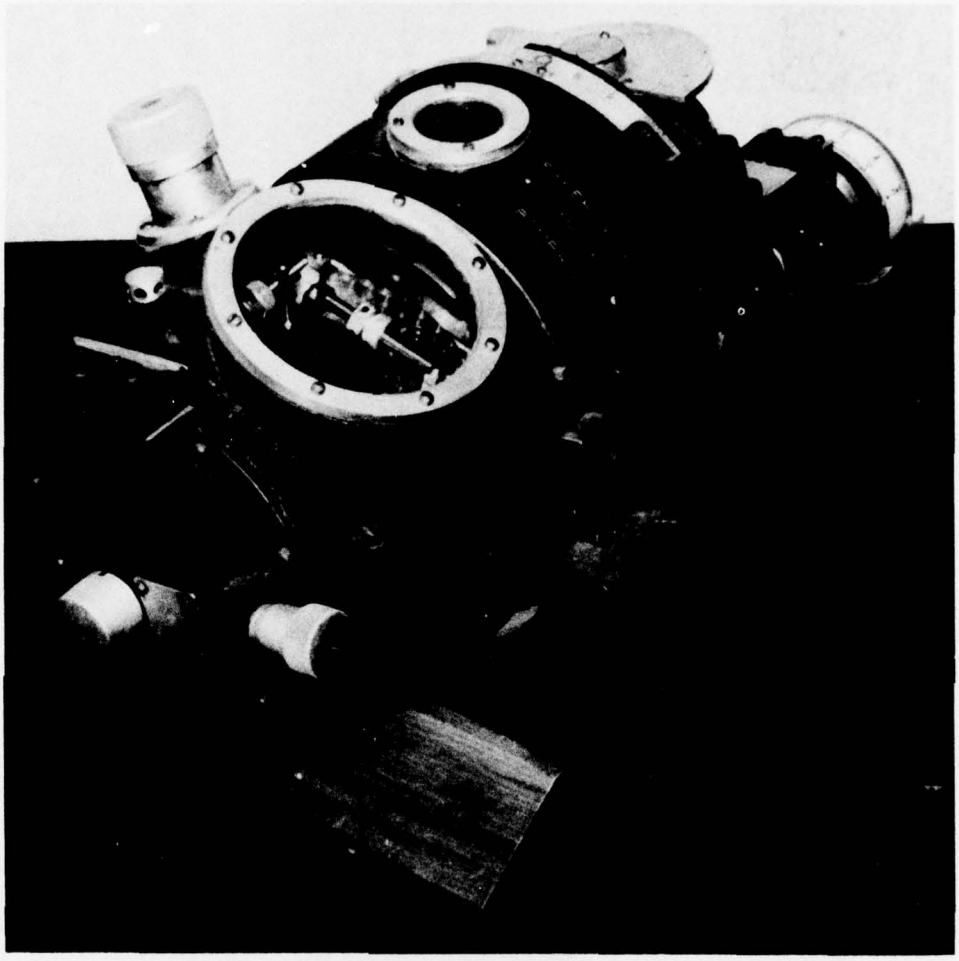
He was really the brain down here for the mathematics work because the naval officers—we never had a genius in mathematics and physics that gave us the knowledge that he had, so we depended on him for a lot of the analysis. Of

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*The Naval Historical Center's archives offer extensive coverage of the Norden Bombsight in *The Navy's Mark 15 (Norden) Bomb Sight: Its development and procurement, 1920-1945*, April 1946, by LCDR Robert V. Brown.



Admiral Braun with other members of Dahlgren Air Detail in front of a TG-2 sometime between 1934 and 1937. Directly under the center of the propeller on the front row is Lt. Norman Ellis, Assistant Officer in Charge of the Air Detail. Admiral Braun, then Officer in Charge, is seated immediately to Lt. Ellis' right.



MK 15 Bombsight designed by Carl Norden.

course they didn't have computers in those days. The arithmetic computation all had to be done by longhand.

What was the billeting like down here? Were the quarters good?

The first BOQ where I stayed when I was a bachelor was a little long frame building that has been made into apartments now, but it was comfortable. Then when I came back on duty in the summer of 1930, when we started the bombsight school, we had to live with Mary's [Mrs. Braun's] family out near Owens. When we came back in 1934 for duty, we received quarters which we thought were wonderful.

You didn't really find it inconvenient living in Dahlgren?

No indeed. I loved the country.

I think the Potomac was your primary connection with Washington, wasn't it?

For material, yes. All the big guns that came in had to be brought down on a lighter, and then they had two oversize tugs that made trips back and forth to Washington by Indian Head. Any passenger or express packages went on the tugs.

Also, wasn't there a practice of taking the officers' wives to Baltimore or Washington to go shopping?

They had one car each week which went on shopping trips. When Dahlgren was really opened in 1918, all they had was a little country store here, and they couldn't handle the people. They had a cracker barrel which the cat slept in—things like that. So people, both military and civilian, who were assigned to Dahlgren and lived on the base took up some funds to start their own store. The store was where it is right now. Of course, it was small compared with what we now know the commissary to be. They expanded their wares all the time, and it was good shopping. But the women wanted clothing and things used for sewing, so there would be one trip a week to Washington. Sometimes, the Captain or some officer would have to go up for a conference. When the car went, they were permitted to take one or two wives with them. They would get a



Seaplane ramp and hangar at Dahlgren in April 1920. The biwing seaplane in the hangar was protected by the latest in canvas doors.

list of everything wanted on Station by the other wives, and then they would do the shopping and would come back that afternoon in an official car. Also, until the Dahlgren Store handled the items most housewives wanted, there was a trip to Fredericksburg once a week. One wife would take a list for everybody and one or two wives would buy the stuff and bring it back. Those early days were right interesting because the roads in Virginia were pretty bad then.

That's what I hear.

They could bog down in nothing flat. In fact, I think that one of the reasons they started changing the time for getting license plates from December to March was that people would lay up their cars down here, and they objected to paying a license fee for a whole year when they couldn't use their cars for 3 or 4 months because of the roads. But then about this shopping trip to Fredericksburg—the roads were bad. Nobody knew when the car would get through. They had some mules down there, and they always kept a team of mules hitched up. Bob Pulliam, who just died a few years ago, was hostler here for years. The Air Detail had carrier pigeons. In those days, planes didn't have any radios to speak of, so they would transmit messages by carrier pigeons. They put a capsule on the leg of the bird, and if they wanted to send a message back to the home base they would put a message in this capsule and let the bird go.



Shelton's Store at Dahlgren in May 1927.

While you were in the air?

Absolutely. This was communications from the aircraft to the ground base.

Did you have the pigeons in the cockpit with you?

We had little cages that fit in there, in cells so you could pull one out at a time. When the shopping trip was going to Fredericksburg on these uncertain roads, they never knew whether they were going to get through. There were two bad stretches between here and Fredericksburg where people would usually get stuck. One was Deep Bottom, which is just off of Route 301 from Dahlgren. The other was up on Peppermill Hill.

That one I'm familiar with.

When people would start to town on the shopping trip, they would take at least four carrier pigeons with them. When they got stuck at Deep Bottom, they let a pigeon go on an air detail. We had to stand there and grab the bird as soon as it got back. When the message said "Deep Bottom," old Bob Pulliam would get one of these mules and ride out there, probably a half-hour trip. If the pigeons didn't come back in 25 minutes, they knew they'd gotten by Deep Bottom. So the next place would be about 45 minutes to Peppermill Hill, and they would stand by to receive a pigeon. If no pigeon came back then, they would be on their way to Fredericksburg. It would be 5 or 6 hours before they had to be ready again.

With dirt roads, I imagine the Fredericksburg trip would be pretty much an all-day affair.

Yes. They didn't start paving around here until the early 1930's.

What were the personalities of your younger air officers? One has visions of spirited young men who had time off with their airplanes to do flying on their own.

No. Commanding Officers when I was here did not permit cross-country flights. In other words, they could not take an airplane away on non-duty.

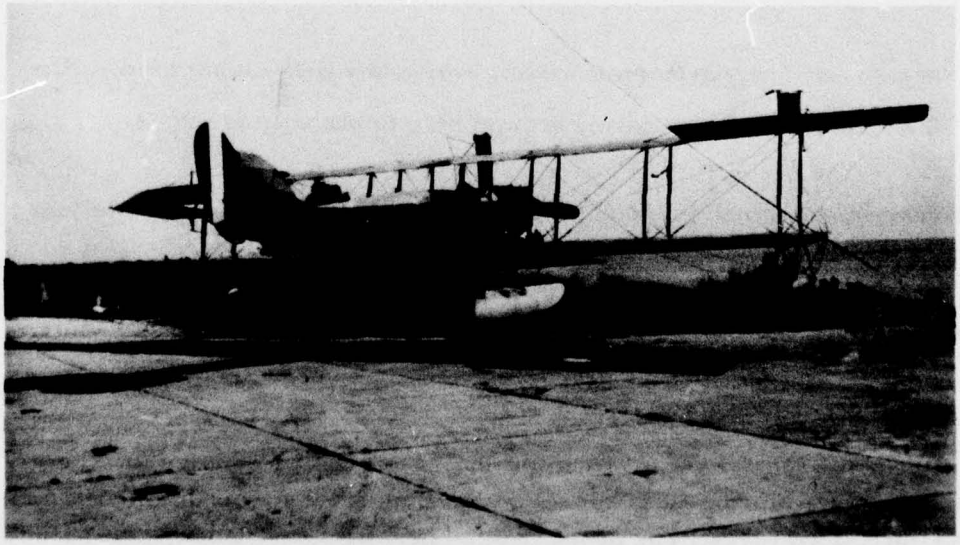
The age of barnstorming was coming to a close.

Well, they didn't have any barnstorming here when I was here. I understand that sometime during the 1920's some young pilot took a plane up and flew into a bunch of wild geese. The people around here were very much incensed and orders came out that from then on you had to stay away from the geese, ducks, and swans. Couldn't disturb them.

It would seem that Dahlgren was pretty far removed from civilization for younger officers.

A lot of them didn't like it. Of course, they always liked to take a car and see how fast they could get to Washington. One young fellow made it in 3 hours one time. They said he was going to kill himself. He later did in an airplane crash, but not here.

Now, when I was here in the 1930's, in addition to the MARK 15 Bombsight, we had a low-altitude attachment which went with the MARK 15 Bombsight and was intended to be used for bombing submarines. You could get down low where the bombsight could still compute the dropping angle to release the bomb. You could also use it for navigation. Excellent for navigating. Then, in addition to that, we had the stabilized bombing approach equipment which went with the bombsight. That was an automatic pilot which I always thought was wonderful. While I was here, I was in on the testing of it from the beginning. We had to write up the pamphlet on low-altitude bombing tactics. Then when I left here and went to the Fleet, I went to the Patrol Plane Squadron. We had the MARK 15 Bombsight, and that, with the stabilized bombing approach equipment, was a wonderful pilot. All you had to do was make corrections to the changes in the course and you would stay right on the course.



In 1924, Admiral J. J. Ballentine, then a Lieutenant, guided this N-9 seaplane in the first radio-controlled flight.

Did your air group at Dahlgren have the responsibility for testing various aircraft?

Yes, every airplane that had a machine gun or bomb rack was sent down here, and we had to put it through tests. With all planes, we had to see that the angle of fire for the guns was such that you wouldn't shoot up the wings and see if they could actually handle the gun in the air. For fixed guns in fighters, we had to fire them and test them. Any plane that was used for dive-bombing tactics had the "grasshopper legs" on it which kept the bomb from being thrown into the propeller. The bomb was right in this little part of the fuselage when you released it; and if you were at too steep an angle the bomb would hit the propeller. In order to prevent that, these "grasshopper legs" would come down and keep the bomb out far enough so it would clear the propeller. We had to test each of the planes for dive bombing. We had to go up to 15,000 feet to get into the no-lift terminal velocity dive. We would have to do every kind of maneuver we could do when we released the bomb to see if we could hit the propeller. Nothing ever happened.

You never had any accidents?

Never had one hit the propeller.

That seems like a good way to terminate your career rather quickly.

It would. If it took your propeller off, you would lose your power and lose control. They never had any trouble that I heard of.

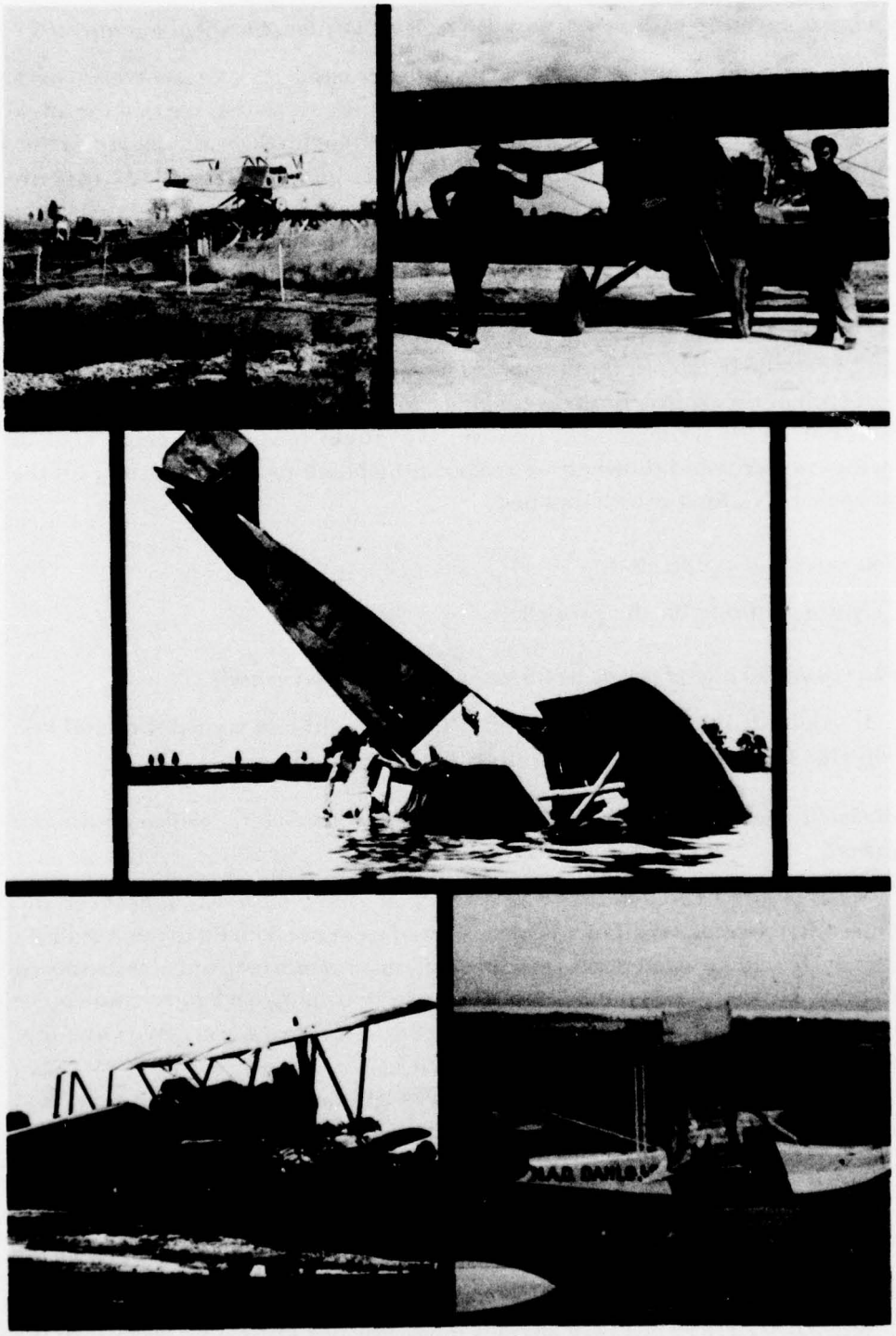
At that time, was there ever any talk among the Navy professionals of possible trouble with Japan?

Well, before I came here, we knew back in 1932 that it was almost certain. Once when we started off on a cruise, a few of us were taken up to the Admiral's office. He said he wanted us to take every item an armored plane was supposed to have. He said, "You don't know when you're coming back here, and you've got to have your stuff ready in case we got into a war in a hurry." We knew that was coming. There was no question about it.

In addition to the bombs and the machine guns, we also had to test the float lights which we could throw out of the plane. We had parachute flares, and we had to take those up and drop them to see how long they would burn. They had to burn a certain length of time or the whole lot was rejected.

That would seem to present something of a hazard to local fishermen. Did you ever have a flare or bomb drop too near a fisherman?

No. They were always very anxious if we dropped something because of the nice silk parachutes. They wanted to get that silk. We had one case of fire where

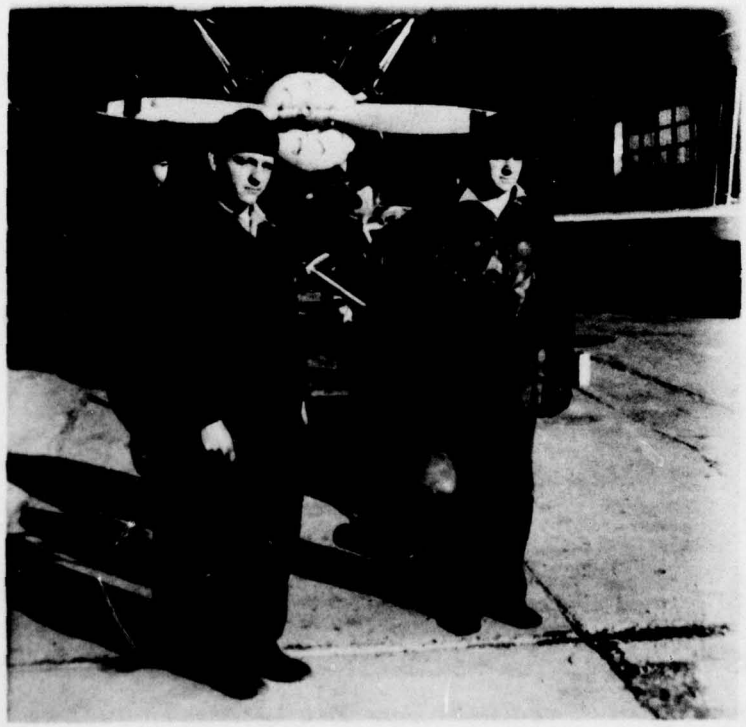


Various aircraft used in tests at Dahlgren during the early years of aviation.

someone released a parachute flare and didn't abide by the rules on where he was to drop. He was supposed to drop them off this wide part of the river here. He went up the river a little farther, and one of the flares drifted over an abandoned sawmill. It went down while it was still burning and set the mill on fire, as well as a lot of timberland around there. Of course from then on, they had to comply with the orders. They had to be over water and we had to know what the wind was before we dropped so that the flares would burn out before they could reach the shoreline, but we never hit anybody. I've heard stories that folks were nearly hit by falling bombs or a bomb would fall in somebody's yard from a premature release or a hangup, but I never heard of anybody getting hit or hurt.

Dahlgren has an exceptional record in that regard. In earlier days when Indian Head was testing its weapons, it had a pretty bad record.

I do remember as a kid I read an article in *Popular Mechanics*, which I always enjoyed reading, where Indian Head had fired a 12-inch gun down the river, and the projectile landed over in the town of Quantico. That was one of the things that led to this Station here. This Station had 30,000 yards down the river



MK VII smoke screen tank at Dahlgren, December 26, 1934.

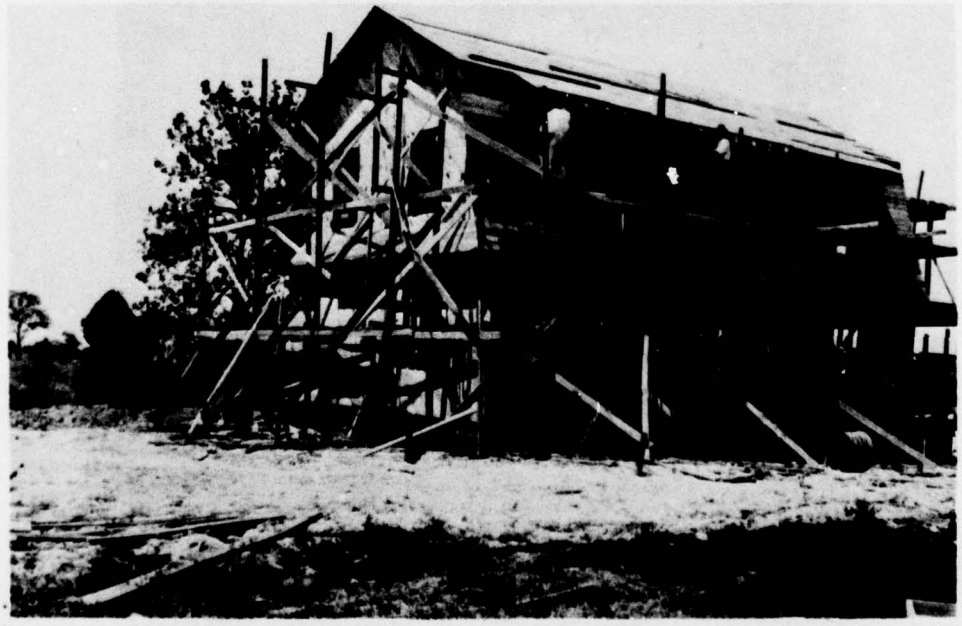
and they didn't have to worry about that. At Indian Head, they had a bank of dirt and the guns were supposed to fire the projectiles into this bank so they couldn't get over into Virginia. One of these projectiles went crazy and skipped over the top of this pile of dirt, and that's the one that landed in Quantico. Nobody was hurt. Of course, Dahlgren, since I retired, fired a gun down the river one time and the rotating band came off. The projectile lost its trajectory and landed in somebody's back yard way down the river.

As you say, the danger at Indian Head was the reason Dahlgren was created. Also, once Dahlgren was developed and heavy ordnance was tested here, it seems that the people who were living at Indian Head were not happy about having to move to Dahlgren.

A lot of the original workers here had been at Indian Head for years. They were the foundation stock, so to speak, of the early Proving Ground.

I understand that they began to complain about the construction at Dahlgren. There was a controversy over the Captain's quarters. There was some discussion on whether it was too opulent for a naval station at that time.

They thought they spent too much money for what they called the "Arcadian bungalows" down there. I think they were supposed to be limited to $10,000,



Construction of officer's home at the Dahlgren Proving Ground in 1921. The house still stands and is in use today.

and some of them ran over that. I think the primary cause of complaint from the people around here was that it took their working men, the laborers, off their farm work. They were offered jobs at Dahlgren which were relatively easier than farming and paid more. As Dahlgren expanded in the amount of work it did, it took more people, and people wanted to live near their work. Naturally, houses sprang up here and there, and it became more congested. Finally, it got so congested that they were almost hesitant about dropping even dummy bombs in the water around here. There were boats everywhere.

Who was the Officer in Charge at Dahlgren in the period you were here in the 1930's?

Captain Leary* was here when I started PG into the summer of 1930. Then when I came back in 1934, Captain Schuyler** was in command. When he left, Captain Furlong† came. Captain Furlong was relieved in 2 years by Captain Mike Robinson.††

How would you describe these men?

Leary was a go-getter. Everybody had to work and do their job because he showed no mercy. But I'd been with him before aboard ship, and I thought the world of him. Schuyler was the studious type, and Furlong was a very good organizer and a good leader. I liked him. When Captain Robinson took over, he was another one who was good. I have no bad memories of any of the Commanding Officers. As far as I was concerned, they were all very good.

Were any of them adverse to the air function?

In those days, there was more or less some hesitation about the value of the air part. Some of them wanted to lay down rules, but after we'd explain what it was all about, we had no trouble.

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*Vice Admiral Herbert Fairfax Leary (with the rank of Captain) was Naval Inspector of Ordnance in Charge at Dahlgren from October 1928 until May 1931. He obtained the rank of Vice Admiral in 1941. In 1942, he assumed duty as Commander, U. S. Naval Forces, Southwest Pacific, and received many citations for gallantry not only from his own country but foreign countries as well. He retired from the Navy in 1946.

**Rear Admiral Garrett Lansing Schuyler (with the rank of Captain) was Inspector of Ordnance in Charge at Dahlgren from May 1931 until July 1934. During his naval career, his principal achievements were in science and development of ordnance for naval vessels and aircraft. He retired from the Navy in 1947.

†Rear Admiral William Rea Furlong served at Dahlgren from July 1934 until May 1936 as Inspector of Ordnance in Charge. He attained the rank of Rear Admiral in 1937 and retired in 1945. He saw action in World War II during the Japanese attack on Pearl Harbor when his flagship, USS OGLALA, was strafed, torpedoed, and finally sunk. Following that attack, he assumed the duties of Commandant of the Navy Yard, Pearl Harbor, and during that assignment was awarded the Legion of Merit with Gold Star.

††Captain C. R. Robinson was Inspector of Ordnance in Charge at Dahlgren from June 1936 until December 1938.

I was wondering if they wanted to, perhaps, subordinate the air function.

We had to play second fiddle, that's true; but we usually got what we wanted.

Do you remember any men you served with here at Dahlgren that you later were associated with in the war or in your future career?

I was later with Admiral Leary during the war. The others I saw after the war.

It took quite a philosophical battle, as well as political, to convince the Navy back before World War I of the necessity of having proving grounds for testing naval guns. The idea at that time was that the naval ship was the platform from which to conduct tests. Was there a feeling on the part of Commanding Officers that being assigned to Dahlgren may have been the end of a career? It wasn't shipboard duty, in other words.

No. Dahlgren was really considered a stepping stone for anybody interested in gunnery. They all went higher up. No, I don't think there was any feeling like that at all in the Navy. They considered this an ideal Station. If you were interested in gunnery, you were interested in what was going on at Dahlgren, the Proving Ground.

So that means Dahlgren was recognized for its value.

It still is. I'm sure the work at Dahlgren still stands high in everybody's mind in the active Navy.

Following your Dahlgren experience, where were you assigned?

When I left Dahlgren, I went to a Patrol Plane Squadron on the West Coast. We were scouts. We could fly for over 24 hours.

Were you on the West Coast when the war began?

No, I happened to be in Washington in the Bureau of Ordnance when we actually got into war.

I noticed a picture of Admiral King here. Were you very familiar with him?*

We are both from the same hometown.

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*Fleet Admiral Ernest J. King commanded several aeronautics facilities within the Navy Department and was Commander in Chief, United States Fleet, and Chief of Naval Operations from 1941 to 1945. In 1945, the title was changed to Chief of Naval Operations. He directed all activities of the Navy, Marine Corps, and Coast Guard in conjunction with the U.S. Army and our allies. He was relieved of this prestigious position by Fleet Admiral Chester W. Nimitz. He died in 1956 while serving in an advisory capacity in the office of the Secretary of the Navy.

I forgot to ask you where you were from.

I'm from Lorain, Ohio. Admiral King's family lived across the street from my mother when she was a bride. When his mother died, my mother sort of looked out for the three King children until their father could make other arrangements to get someone to take care of them. There was always a strong tie between Admiral King and my family. He used to come down here. He was very fond of Mary's cooking, and he liked Mary, too. He would come down here twice a year and spend a long weekend.

Was this after you retired?

Yes, after I retired.

How long have you been at Mathias Point?

I came here in October 1947.

Admiral Braun, for his service aboard MANILA BAY, was awarded the ribbon for and a facsimile of the following unit citation:

NAVY UNIT COMMENDATION—USS MANILA BAY:

"For outstanding heroism in action against enemy Japanese forces in the air, ashore and afloat. Boldly penetrating enemy infested waters, the USS MANILA BAY and her attached air squadrons defied determined Japanese air and sea opposition to strike fiercely at hostile warships, aircraft, shore batteries and other installations vital to the enemy. As one of the southern carrier group which received the brunt of Japanese land-based plane attacks in the historic Battle for Leyte Gulf, the MANILA BAY fought her guns gallantly against enemy dive-bomber and suicide planes, at the same time, sending out her own aircraft to strafe and bomb Japanese Fleet units. During the Mindoro assault, she aided in covering the operations successfully despite active hostile airfields surrounding the Sula Sea. Although crashed by two Kamikaze planes at Lingayen Gulf, the MANILA BAY was brought back into action by the heroic efforts of the ship's company and her air group and, within thirty-six hours had resumed her mission of launching her aircraft in support of our landings. Her notable record of service and combat achievement reflects the highest credit upon her courageous and skilled officers and men and upon the United States Naval Service."

CHAPTER IV

Range Operations

Donald W. Stoner

Mr. Donald W. Stoner, a native of Gettysburg, Pennsylvania, received a BS in electrical engineering from Gettysburg College in 1931 and an MS in physics from the University of Michigan in 1934. From 1931 until 1935, he was an instructor of physics and electrical engineering at Washington and Lee University. Mr. Stoner came to Dahlgren as a junior physicist in 1935 and was Head of the Engineering and Evaluation Department prior to his retirement in 1968.

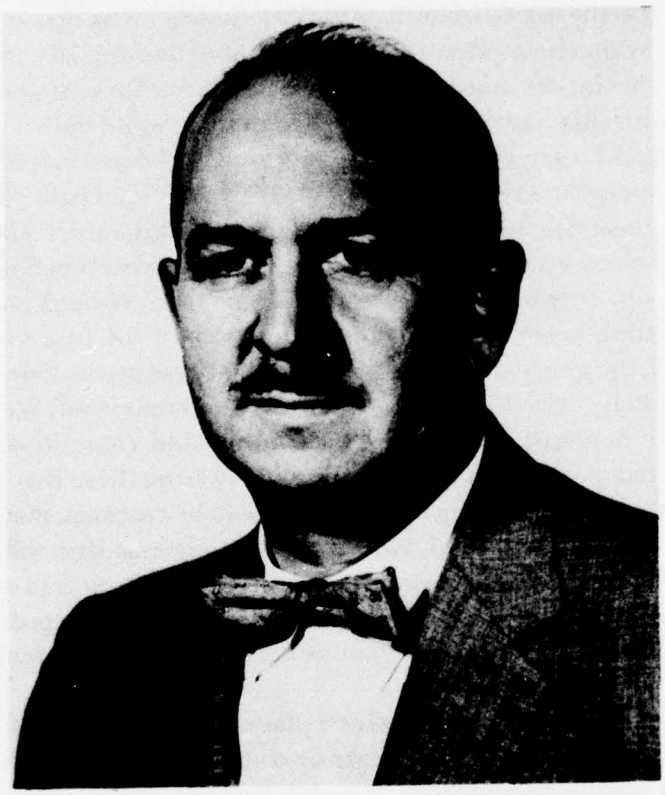
The following interview was conducted by Cynthia Rouse in Mr. Stoner's home in Lottsburg, Virginia, on September 14, 1976.

Can you give us a general background on why you came to Dahlgren in 1935? With your excellent educational background, there certainly must have been other opportunities for you.

I had about 4 years of experience teaching physics and electrical engineering at a college, and I was interested in getting into something nearer to industrial research. Also, at that time which was still during the years just following the Great Depression, the colleges were not paying very much, and I could actually earn more money working for the government. Then, too, I liked the idea of working in the ordnance area.

Could you relate some of the early work you did on bombsights and fuzes from 1935 to 1941?

During that period, Dahlgren was running all sorts of tests incident to the development and refinement of the MARK 15 Horizontal Bombsight which was known as the Norden Bombsight and was developed by the Norden Company. Also, Norden was working on a bombsight that was for a diving airplane, and there were various tests to be conducted on that. Most of these tests involved theodolite stations that would track the airplane during the approach just before release, observe the time of flight of the bomb, and



Mr. Donald W. Stoner joined the Dahlgren Laboratory in 1935 and was closely associated with the range operations for over 30 years.

determine the range the bomb took. Out of this, information could be gained that would tell how the bombsight was working, so far as the inherent errors in the system were concerned, and also they could calibrate the bombs in terms of aerodynamic performance. Different bombs all had a different ballistic coefficient that was determined by these drops, so there was a lot of work on calibration of bombs.

Then there were experimental bombs that were brought down, and objects, mines—all sorts of things to be dropped. Also during that period, the Navy was developing some fuzes for bombs which were to be used against ships, and, of course, they were primarily concerned with the ship's deck. The ship's deck that you see looking down on the ship is normally the weather deck and is constructed of relatively thin steel on an armored ship—maybe not over ½ to 1 inch thick. Then down under that is heavier steel which might be the armored deck or you might go through one more deck before you get down to the real

armored deck to the thick armor that would definitely break up a bomb if it did not detonate by that time. There were a lot of ships, like cruisers, that wouldn't have for their armor deck more than about a 2-inch thickness of steel, and there were some ships that had even a little less than that. So we built a deck target. Against this deck target, we were testing inert-loaded bombs that had the fuzes that were supposed to select the action. The fuze, if it hit a light plate that the bomb could penetrate, would not function, but if it hit a thick plate that was capable of breaking up the bomb, the fuze was supposed to function before the bomb broke up. It was called a selective-action fuze. We did an enormous amount of testing of such during the development of the fuze for what they called, I think, the general-purpose bomb, or light-case bomb. Our deck target was about 1200 feet north of the Terminal Range Bombproof. We'd stand up on top of the bombproof while the airplanes would come in and we took pictures. We had a whole set of cameras, ranging from those that operated at about 120 frames per second up to the 16mm, built by Eastman, that could go as fast as 2000 frames per second. Now they have cameras that will take up to 16,000 or 18,000 frames per second. For winter tests, we had to keep the cameras warm all night with electric lights so they'd be ready to go first thing in the morning. If they got cold, the film would get brittle and break up in the camera.

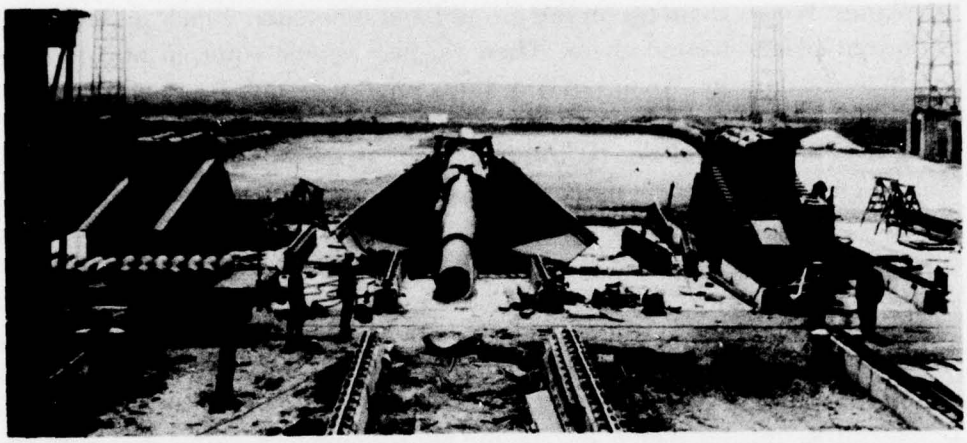
I had the job of arranging the tests and planning the photographic coverage and, in fact, handled most of the camera operation myself with remote-control switches back on the bombproof.

The bombs had small charges that produced smoke puffs which came out of ports on the side, and we could tell when the fuze operated by when we first saw the smoke and flame coming out of these ports. Then, with the cameras, we could tell by the time delay how long it took for the bomb to detonate after it struck the top deck. Sometimes it would go right on through and impact on the second deck, and we would have to determine how much of the bomb crushed or how long it was after impact on the second deck before the bomb functioned.

This was most of the bombing stuff. Of course, when we got to armor-piercing bombs, then, instead of dropping them, most of the development work was done by shooting them out of a gun and into the armor plate because they were so expensive. Of course, we did drop some armor-piercing bombs.

After your work with bombs and bombsights, you moved into general ordnance and ballistics in 1942. Can you describe the environment surrounding the range operation during the war years and your later career, perhaps highlighting some of the significant events?

In 1942, the early part of the war, I was working in the Experimental Department as an assistant in a group of assistants to the Experimental Officer.



Inspection of a 14"/45 MARK II-1 gun that exploded after the second round was fired, May 1924.

The bulk of the range work moved very fast into acceptance work for the production of projectiles, propellants, and fuzes. That got to be the big firing load, but there was experimental work—it had to come along. Mostly, we looked over the armor tests to determine the quality of the armor, reviewed the tests, and determined whether trends in the armor quality were bad or good. Especially, we wanted to identify the bad traits and eliminate them quickly from production armor that was being produced to go aboard ship.

Armor came in different sizes. If it was for battleships, it was pretty thick. Deck armor would be around 5 or 6 inches thick. Turret armor could get up to maybe 12 or 14 inches thick. We were testing projectiles, too. The trick with the projectile was to build one that would, hopefully, defeat armor, and the idea with armor was to build it to defeat a projectile.

Then early in World War II, a lot of reserve officers came in, and we had to get people working on armor who hadn't known anything at all about it. They didn't know the terminology, and there's a great deal of unique terminology in armor work. There were things like the plate is dished or it's penetrated or it's perforated or you throw a button out of the plate or it spalled—all sorts of terms such as these. The old-timers got together some good pictures and then said, "This is what we mean when we say 'button,' and this is what we mean when we say this and that." We put out some information for instructional and reference purposes. All of these reserve officers, only a couple of months earlier, were working in offices somewhere, and they had never even seen the Navy.

Light armor got to be very important. They needed light armor for landing craft and for airplanes and ran a large number of fragmentation tests against

airplanes. We set them up on the ground and detonated 5-inch projectiles at different places around them. Then we had people come in and help us evaluate the damage. We did an enormous amount of static fragmentation tests against airplanes and came to the conclusion that a relatively small amount of weight in the order of maybe 150 pounds of armor built into the pilot's seat would give the airplane a much greater protection and wouldn't hamper the airplane insofar as performance was concerned. Out of this thing came a big business for light armor in airplanes. We received all kinds of requests like, "What kind of armor ¼-inch thick can be put on an airplane to keep out 30-caliber machine gun bullets?" The answer, of course, was "None," and they didn't like this. This thing finally got into a big business of qualifying manufacturers for airplane armor production.

Of course, during World War II, the Main Range was always going strong. Now and then we would get interested in finding out how long after impact on water a fuze functioned, and we used some sound techniques for doing that—using hydrophones. I remember going downriver one time, when we wanted to get fairly near the target, within 500 yards of the point of impact. We got our boat in position, and one of these reserve officers (we used to call them "90-day wonders") who had suddenly become a firing officer fired a program which involved some illuminating projectiles that were notorious for coming apart. They were impacting at a safe distance pretty well where they were supposed to, and then one of them apparently came apart, didn't make the right range, and landed so near the bow of our boat that the range men couldn't see a splash anywhere. On the boat we knew something was near because the noise that went through the boat sounded just like wood shattering. In fact, I was in the chart house at the time, and everybody came out there looking for the hole. Then I started looking for holes, and the next thing I saw was two of the crew who had been down in the front end of the boat getting a little shuteye. They were real big guys, and there was a hatch about 2 feet wide that both of them tried to hit three times and finally one backed off and let the other one get out because they were sure they were sinking. I was thinking, "Well, to hell with this camera gear I have here, I know I can swim to shore." It turned out that we weren't hit, but I'd say the projectile surely didn't land more than about 6 feet from the hull.

Then one day, Captain Hedrick* went downrange on a boat just to see how things were going when one of the young firing officers dropped a 6-inch loaded and fuzed projectile awfully near the boat. Fortunately, it was a dud, but that young man had a few words put on him by the CO.

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*Captain David I. Hedrick was Commanding Officer at Dahlgren from April 1941 until June 1946.

You mentioned Captain Hedrick. Who was the Dahlgren Commander when you came to the station?

The Commander of the Station when I arrived was Captain Furlong. The man who preceded him was Captain Schuyler. He was something! I got there shortly after Captain Schuyler left, and they were still talking about things that he did. One of the things that they talked about was Schuyler and the grass fires we had that started out in the fields in the fall and early spring when everything is dry. We were always having grass fires, and sometimes they were pretty bad. They'd get in the swamps where we have all the cat-o'-nine-tails, and they would be real hard fires to handle. But when we had a grass fire, as a rule, all the fire trucks would get out there, and people took wet burlap bags and shovels to try and beat out the fire. Back in the early days of the Station and, in fact, for many years after I arrived, anytime there was a fire, every able-bodied person on the Station, civilian and military, turned out to fight the fire. We were expected to do that because we didn't have a fire department. Just like aboard ship, everybody turned out to do what they could to fight a fire.

Anyway, the story they liked to tell was about a big fire out north of the Plate Battery. It was spring, and the ground was pretty wet. There were times out there, I remember, when we had some holes in the ground where bombs had gone in. In the spring, the water level in those holes was even with the surface of the ground, and it stayed that way until it dried out in late spring or early summer. Well, it seems when they had the fire, Captain Schuyler went out, too. He was the "Fire Chief" of any big fire on the Station. He ordered the fire truck into the field to a place where he thought it should be to fight the fire. Well, the driver said, "If I go in there, Captain, I'm going to get stuck. I won't be able to get in there and get out," but the Captain ordered him to get in there. So he went in, and the fire came along, and before they could put it out, the fire burned up the fire truck.

Is it true that five foremen, per diem workers, ran the Station when you came here?

Well, I would say that all depends on your point of view. When I arrived in December 1935, there were civilian professionals, Dr. Thompson and myself, and some physicists named Lipnick, Riffolt, Barker, who was a mathematician, and Scott, who was a mathematician. Then there were two ballisticians, but they were not professionals. They called them ordnance engineers. They were good men, but they worked up through the ranks. Roger Dement had the range operation, and Milton Dement was the ballistician. Aside from that, all the rest of the employees on the Station were per diem employees. Of course, they reported to the foremen. To that extent, the foremen did actually run the Station so far as most of the employees were concerned, but the foremen did not run the professional end of it at all. The professional end of it was pretty



First impact at the Proof Battery, August 4, 1941. Top row (right to left): CDR H.D. Hoffman (Proof Officer), LCDR W. S. Parsons (Experimental Officer), LT. H. S. Harnly (Plate Fuze Officer), CAPT D. I. Hedrick (Inspector of Ordnance in Charge), LCDR C. A. Buchanan (Plate Battery Officer), H. S. Jenkins, C. B. Peters, Ed Worrell. Front row (right to left): H. Chinn, J. B. Rawlette, F. M. Marshall, W. A. Rose, J. H. McClanahan, A. A. Rogers, E. B. Williams, J. F. White, Ch. Gunner Krupa (standing), W. F. Jenkins, A. G. White, W. W. Clare, L. V. Morgan, G. B. Elliott, I. M. Fritter, George Garner, F. B. Staples.

much all in what was known as an Experimental Department under the Experimental Officer. The Experimental Officer was also responsible, in those early days, for all those operations at the Plate Battery, which was the Terminal Ballistics Range, where all the projectile firings against armor plate were conducted.

The foremen, of course, carried quite a lot of heft, and most all of these per diem people had to recognize that a foreman was boss. He had a lot of authority, and he was allowed a lot of latitude. There's the old saying, "If you're going to give a guy the responsibility for getting a job done, you've got to give him the authority that goes with it." He had the power to give and to take away, and if you didn't leave a certain amount of that with him, some of those char-

acters he had to work on—I'm telling you, they were pretty rough characters—would be impossible to manage.

I remember riding to Washington, shortly after arriving on Station, with a couple of men who worked on the Plate Battery and who were living over in the civilian dormitory. We went across the Potomac on a motor launch to where we kept the car. Then we could drive on up to Washington. When we were halfway up, they stopped somewhere to buy some "hooch." Lipnick and I were just two young physicists, and we were to meet them down at the Gun Factory at 5 o'clock Monday morning. I remember coming back. Oh boy, they'd had the most wonderful weekend you've ever heard of. They wrecked four beer joints, beat up seven or eight guys, they'd been in a couple of fights that they hadn't won but claimed they were real good ones. They had a great time. There were quite a few of those characters working around Dahlgren at that time, so if they say the foremen ran things, you had to let the foremen run things because they had some pretty strange animals to get the work out of.

You mentioned Roger Dement. He was in charge of the range crew there?

He was Head of the Range Section. He also handled all the diplomatic operations between the Proving Ground and the property owners down along the river where we placed range stations. We didn't buy the land; we just obtained permission to use the land. Sometimes we had complaints that our range vehicles going in and out were tearing up the roads. Then maybe we'd get a couple of loads of gravel to the owners or something of that sort. Also, we had people who went down to man the range stations who would occasionally do things that annoyed the property owner. These are the kinds of things Roger took care of.

What significant part did Mr. Benjamin Tubman play?

Mr. Tubman was mainly the power out on the Main Range. We called it Main and Broadside after the main battery of the big guns on a ship and the smaller guns that participated when a broadside was fired. That whole group out there was under Ben Tubman. He was a good foreman, and he kept things going pretty well out there.

Ammunition was a different outfit. Ammunition was under Ben Carpenter. Of course, Public Works had in the early days a Navy person known as the Construction Officer. He was normally a warrant officer, and then he had a foreman at all times. I've forgotten who the foreman was.

Melvin Reynolds?

I think there were a couple of foremen in Public Works. Mel Reynolds was electrical and plumbing. At that time, a man was an apprentice, and then he

became a journeyman. Then when he became a supervisor, he became a leadingman, and then went to a quarterman. From quarterman, he went to a chief quarterman. The foreman was above the chief quarterman. They were the old ratings. Back when I was there, a foreman was normally a man that had at least 100 or 150 men. Now a foreman can be a person who has 12 to 20 men. The foremen had important jobs looking after all these people.

You were in charge of range operations for a number of years. Did you encounter any funding problems?

I had the Dahlgren range operations during a long period when very little was being spent on development in guns or ammunition. In fact, there was a period when I think the current wisdom was that guns and ammunition were obsolescent. You could more or less just look forward a certain number of years and say there just wouldn't be any more guns. Ordnance would be all bombs, missiles, and rockets. It was during that period in which they were developing TERRIER, TALOS, and TARTAR missiles and other such surface-to-air weapons. They were doing a lot of work on development of unguided rockets, too. But there was a tendency to say, "Well, the gun testing facilities, the instrumentation, and what goes with it are more or less not the place to put any significant capital investment." There were years starting immediately after World War II when there was money available, and we did get some good new instrumentation during those first couple of years. After that, there was a long period during which money for gun range improvement was virtually nonexistent. That's the time when they were building up more and more of the type of rocket testing that was going on at NOTS in China Lake, missile testing at White Sands, and all sorts of facilities for testing the newly developed ordnance of that kind.

There was a long period when we had to live on a pretty skimpy diet. I would say that it wasn't until probably during the Korean War that some money became available because they needed material. During the Vietnam War, the Department of Defense found that they had to revive a lot of the things that they had decided to lay away and forget about. For instance, there was a trend to put nothing but rockets and missiles on airplanes, and they found very quickly that they had to have guns on airplanes. There was a problem because we had practically disbanded at Dahlgren all the people we had working on aircraft machine guns. When Vietnam came along, fortunately we still had at Dahlgren a few people that knew something about aircraft machine guns. They were worth their weight in gold—people like Clif Samuels who knew how these guns operated and, when they didn't operate, knew what to look for. When the Fleet came in and said, "Nothing is working right," we could send him out and find what was wrong.

Not only that, but they found they got interested again in gun ammunition for ships and ship guns for shore bombardment. They found they were using up a lot of ammunition, and it's amazing how much of that ammunition was reconditioned or reworked stuff that had been left over from World War II. There were projectiles that had never been intended to be used with certain types of fuzes. When explosive was put in them, it was for use with a kind of fuze that wasn't what they wanted over there for the current application. Then they would try to rig these up in some way so they could use them with the fuze that they weren't intended to be used with originally. This kind of thing, using up these pieces assembled in a little different way from the way they were designed to be put together, produced some troubles. Then we had to hurry up and find out why they blew up a turret or why they were blowing up guns or doing things of this sort. We were very busy during that time.

Also, the Navy began to buy a lot of rockets, and there were some components they had to buy, and some ammunition they had to buy. However, the Navy was going to use Army 8-inch projectiles because the Army had a lot on hand, and we had to use them up instead of buying new ones. Then, they wanted projectiles that were not intended to be used against ships at all but purely against people, so we had to figure out what to do to make an 8-inch projectile very good in that regard. It was thought to be especially nice if we could use those old Army projectiles. There were many tricks of this sort. During the Vietnam War, we were just completely occupied with things of that nature and finding out why things weren't working right. Of course, we had a lot of production stuff come through—mainly small ammunition, but none of the 8-inch.

I would say during that period, then, quite a lot of money was made available—practically all we needed to gear up for the acceptance work and experimental work, too. There was a long period there, prior to Vietnam, when the range was more or less just left standing and we didn't put any money or any capital investment into significant improvement of the instrumentation or equipment. Nobody felt there was going to be much use for it.

Can you describe some of the primary types of ordnance under development and tested during your career and relate their significance to the Fleet?

Along with the development of bomb fuzes, there was also the development of a series of what they called GP, or general-purpose, bombs. They were heavier than the light-case bombs and were the bombs that would, hopefully, take care of armor up to maybe 1 to 1-½ inches thick. They were versatile bombs because they had enough explosive in them so that if you had a miss and the bomb didn't go off on the plate, additional fuzes would cause the bomb to detonate at a certain time after water impact and produce a mining effect

alongside the ship. We tested a lot of those bombs in drops against a deck target—one that had various thicknesses of lighter plate and then some 2-inch plates as well.

In other work with bombs, we had, on several occasions, reports that dive bombers operating at steep angles were having the bomb tails, after release, flip up and knock holes in the undersurface of the wing. To solve the problem, we put a bomb out on a wing rack and then in another bomb case mounted a camera. I went over to sick bay and got a roll of adhesive tape and a lot of string and cut that into pieces about 6 inches long. I took the adhesive tape and stuck lengths of string all around the surface of the bomb mounted on the rack. We then had the pilot make a dive, without releasing the bomb, and took pictures to see what the airflow was like around the bomb. It was easy to see that the airflow was coming down on the nose and up on the back of the bomb, so what we needed was a little L-shaped piece sticking down on the tail so the tail couldn't flip up.

We also had problems with what were called flexible guns in airplanes. The fixed guns are built so that when you aim the airplane, you aim the gun. Then, in some airplanes there was a man in the rear seat, and he had a gun that would pivot all around, and this was called a flexible gun. They were interested in windage effects on flexible guns. We rigged up all sorts of camera techniques for firing tracers, photographing them, and seeing how far they drifted.

Of course there was a lot of work done in figuring out how much yaw a projectile or a bomb had. On one of the first jobs I had, we had a report around 1935 or 1936 from Admiral Nimitz in the Pacific Fleet that in battle practice he had seen projectiles coming down and hitting the water flat and bouncing at battle range. That would have been very bad because they wouldn't have penetrated armor, so we set up a program, fired projectiles, and took high-speed motion pictures of them. The projectiles weren't yawing that much at all. Then we received another report and had to get some more pictures. That time we found out what was really wrong. The projectile was an armor-piercing projectile with a large combination windshield and cap on the front end, and what happened was that when the projectile went into the water, the cap broke off, and it was the cap that came out of the water, bounced a couple of hundred yards, and entered the water again.

There was a lot of work with the guns, too; for example, the original development of the 3"/70 gun. We did the experimental firing and tried to get a gun that was known to have high velocity and a reasonable operational life. There was a lot of just getting pictures and devising means of producing sharp photographs of projectiles coming out of the gun to see the condition of the rounds and if they had full rotation. Had they taken their full spin or had the rifling been stripped off the rotating band? How much did a gun jump when it fired? How do you measure jump?

I also remember when the Bureau of Standards came down and wanted us to fire a rubber duck out of a 5-inch gun. What had happened was that a couple of commercial pilots were flying along, fortunately on automatic pilot, when they were rudely awakened and found their windshield had been broken out and they had a wild duck in the cockpit. Well, the Bureau of Standards came down with partially cured rubber balls to fit in a 5-inch gun that we were to shoot at low velocities up to about 250 feet per second against certain windshield material to see if it would withstand impact of a wild duck. We called this our "rubber duck project."

I was in the Experimental Department for a long time, and anything crazy that came to Dahlgren went to Experimental. I was a physicist, too, and anything that came to the Station that they didn't know where else to put it, they said, "Give it to the physicists; they can do anything." The first job they gave me when I came aboard was to go downrange and, with an old camera, get up there within 150 yards of the point of impact of 14-inch projectiles to take pictures and see how much the projectiles were yawing. That was awfully near the point of impact, and if it hadn't been for Captain McLaren back there supervising the firing, I would never have gone down on that job. Captain McLaren was Experimental Officer, and I had enough confidence in him to know that he would see to it that things were done right up there. What they did was fire three shots and plot those, and then we'd come in 150 yards from the mean point of the impact of those first three rounds. The nearest one that came to us, I think, was about 75 yards. The projectiles impacting were beautiful. They had dye in them, and then when they hit the water, the cap would break and the dye would come out—a beautiful orange and green and white.

I also recall one time when somebody made a mistake and shipped some 50-ton armor plates on a barge, not on railroad cars. If they had shipped them on a railroad barge, we had a tidal bridge that had rails on it and the train could have run right out on our railroad and taken the plates up to where the big cranes could unload them. However, these they put on a plain barge, so to get them off, we had to run one of our big cranes and a locomotive and a flat car all out on our dock and then lift the 50-ton plates off the barge and put them on the flat car. I remember Commander Davis coming in and talking this over with Dr. Thompson, and the question was, "Would this collapse the dock?" They didn't know whether the dock was strong enough to withstand the load, so they said, "This must be a job for a physicist." Anyway, what happened was Mr. George Jones, our civil engineer, and I went down to the dock, got in a rowboat, and went all around under the dock poking penknives into the pilings and timbers to see how rotten they were. The trouble was they had three sets of piling in there. When the first one looked like it was bad, they didn't take it up. They just put in the second one and let the first one stay there for what it was worth. Then they put in the third set of piling, so we had three sets of piling. We

had to look at all the beams and compute this all out from the strength that you'd allow and finally came up with a conclusion. They didn't want any technicalities. All they wanted was "yes" or "no." I remember we came to the conclusion, "Yep, you can do it. It will be okay." They went and unloaded the plates. Now, physicists normally don't do that sort of thing, but in that day, a physicist was supposed to do everything or anything that came up that was technical.

What part did Dahlgren play in testing weapons confiscated from Germany and Japan after World War II?

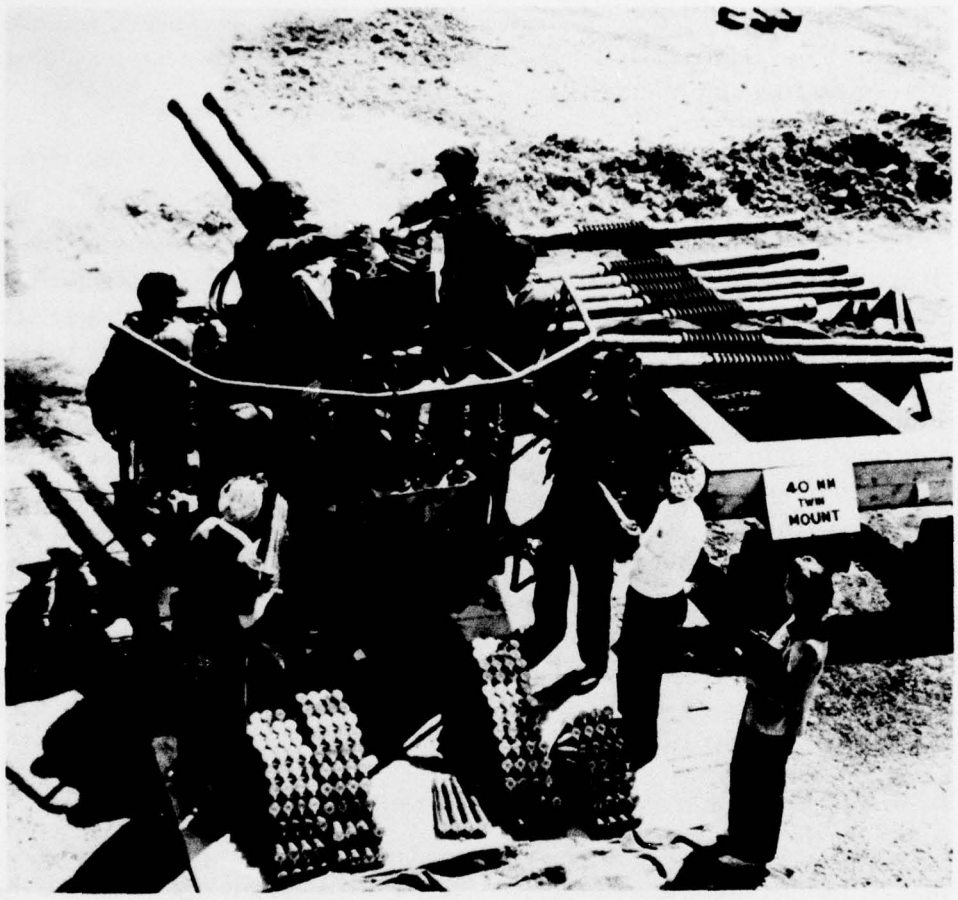
There were some items that came in to be examined by Dahlgren, but, as I recall, there wasn't a whole lot done in that regard. It seems to me that I vaguely recall them bringing in a Japanese so-called wire-wound gun, a big gun. How it tested out, I don't recall. There were some things coming back from Germany. At the time, I was working mostly in instrument development, and they sent a lot of electronic stuff back to us, but it wasn't anything that amounted to very much. There was a lot of money and effort spent on analysis, writing reports, etc., but, frankly, I don't think we learned a great deal.

Did you feel at any given point that the mission of the Laboratory was changing to put more emphasis on R&D rather than just testing ordnance?

Yes, indeed. After World War II, we made a very deliberate attempt to develop R&D and worked at it very hard. In fact, there was a long period after World War II when there was very little proof and testing. There was a big reduction in the force of ordnance workers after World War II, ordnance mechanics and that kind of job, and it was then that we attempted to build up the experimental and development work. After World War II and clear up to Korea and Vietnam, there was a very deliberate effort on the part of the technical staff at Dahlgren to build a research and development capability. It's nice to look back and see that the effort paid off, but without a deliberate effort in that direction, it never would have happened.

Do you recall any problems with the local community because of the testing work at Dahlgren?

None of any great significance. We've had people come in and say the gun blasts or explosions had burst out the windows or knocked down their plaster. A certain amount of that went on every now and then, but we had no great problems. One thing that saved us a lot of problems, I think, was the fact that the Station, about the time of World War II or just a little before, bought quite a bit of land up north of us—the Hooe farmland. We were then hassling with



Women assisting in range operations during World War II.

Virginia about where they would put the road down here. They wanted to run it straight down through the reservation, but we made them put a curve in it to keep it a little farther away from our test area. I remember I insisted at that time that we buy the land 300 yards north of the road, not that we'd ever use it for anything, but if we didn't buy it, it would fill up with commercial hot dog stands, gas stations, etc. Just as soon as we would have had everything fixed up well, we would get complaints from those people about the noise, and fragments, and so forth. So we might as well get both sides of the road and control it; otherwise, we'd have that nuisance coming back on us. That nice one mile we own there has kept us free of commercial business, and saved us a lot of trouble. The people living around the Main Gate have been there so long that they don't even hear the noise anymore. When they put Highway 301 in there, part of the agreement was that the Naval Proving Ground, at that time, had the right to

close the road for up to an hour at a time, if necessary, because of fragment hazards from Dahlgren tests. The State agreed to that. That was part of the agreement in giving them the right of way.

What do you consider to be the primary problems in maintaining a range such as Dahlgren's?

I think the primary problem is maintaining the authority to operate a range. It becomes more and more difficult to have a place where you can conduct the kinds of tests that Dahlgren conducts on the river. In other words, there's the problem with the commercial aircraft overhead, the airspace. At the time I was there, we had to coordinate very closely with the Civil Aeronautics people. At times, with certain tests, we had to inform them well ahead and have them put out notices and route the airplanes around Dahlgren. Then you've got the people building more and more up and down the banks of the river. The people over at Cobb Island, Maryland, were complaining about our tests even when I was there. They said, "There shouldn't be this kind of stuff done in a place like this. You ought to take this someplace else." You know, it's like, "Where do you put up a nuclear power plant?" Obviously, someplace else, and *it's the "someplace else" business that in the long run could be your major* problem. Dahlgren is excellent as a test facility in that it is so near the R&D activities that can use it.

Is there a real need to maintain a range over water?

I can see that in certain electronic ordance work the range over water may be the only range that will permit you to know definitely whether or not the equipment is going to work at sea because the kind of reflection you get from water is likely to be remarkably different from the kind of reflection you get from land. I do think there is a very distinct advantage in having a range over water for types of electronic ordnance or if you're testing ordnance that discriminates between water and a target automatically. When you test over water, you're testing over the element which you would be using in operations against surface ships.

Can you remember any serious accidents that occurred on the range during your career?

About the most serious accident I remember was during air tests we were running during World War II. We had a practice bomb being dropped from an airplane at a high altitude strike an airplane at a lower altitude. The airplane was disabled. The pilot got out, but the copilot didn't. There was also a case of some oystermen working out in front of what we call the machine gun range. The two men out there tonging up oysters in front of the range brought up a 40mm mortar shell that exploded on the boat and tore them up pretty bad.

Did it kill both of them?

No. They survived. Actually, so far as other accidents on the range, there were some accidents with explosives. We had a man killed on Main Range, not on firing, but on a personnel ejection test for an airplane. Martin-Baker, a British firm, made a telescoping tube containing propellant that throws the pilot out of the airplane. We were running some tests on these, temperature conditioning and so forth, and one fellow did something wrong. The thing fired, hit him in the chest, and killed him.

A Lieutenant was killed way back by a fragment. Then someone, more recently, was killed by armor. Actually, considering what happened on that range, it is amazing the accidents were as few as they were.

I remember the day they put the 16-inch projectile ashore near Nomini. The probability of that happening is extremely small, but that day, it did.

I also remember one time the Antiaircraft Fuze Battery was firing a test. The firing officer looked all around and everything was fine—a beautiful blue-sky day. Then they fired, and here came an airplane across the range and triggered what we called a radio fuze—VT fuze. The plane reeled and flopped a little and then came in and made a landing on our field. It was an Air Force trainer that had blundered across the range. Normally, the firing officer would see him, but he managed to come over the guy's shoulder, and he didn't see him until it was just too late. The pilot had an observer with him. Neither one of the men had a scratch, but the airplane looked like it had been sprinkled with a pepper shaker.

There were a lot of close things like that during the war. I remember once going to the Plate Battery with Russ Lyddane. We were going out there to do something to the left of the Plate Battery where they ran fragmentation tests. We were measuring something up there. We stopped and talked with the firing officer and said, "We're going out front, and when you're ready to fire, call us in, and we'll get out of the way." That was fine. We were out there working and all of a sudden we heard an impact on the plate and a 5-inch round going "whr . . . whr . . . whr. . . ." So, we got on the phone and said, "Hey, what goes?" They said, "Oh, we forgot you were out there."

There's a certain amount of that type of thing. There was a time when I was out with Commander Kiland, who was Experimental Officer, and we were going to detonate, I think, a 500-pound bomb. We had the bomb all set up for detonation. Back about 1000 feet at the bombproof of the Plate Battery was a fellow who was holding up the firing line. That was to be the end of the line to detonate. He held that line up to show that it was not connected to anything. You don't connect to the firing squib on the bomb unless you see him holding that up. Well, we had a guy back there holding the line who was not very bright. Just out of luck we had a man from Ammunitions with us who was taking care of the line to the bomb. He put the wires on his tongue and said, "These taste like

they've got electricity on them." I said to Kiland, "Maybe we'd better test it." We got a firing squib, put it behind something and connected it, and it went "Whoo!" And we were getting ready to hook it up to a 500-pound bomb! When we went back to find out what was going on, there were two lines back there. The guy back at Plate Battery was holding up the wrong one. You come away and you just say, "Well, I suppose the Lord has been using a little extra time taking care of people today." I know a lot of that kind of thing happened that I never knew about, but there were extremely few serious accidents, and I think we were very fortunate.

Were there any people who were particularly significant in developing and maintaining the Dahlgren range?

For years, Roger Dement looked after the range, and it was after he retired that the Range Section, which had been under him, came over as one of my branches—we called it a branch then—in what was then the Ballistic Instrumentation Development and Services Department. I would say that, up until that time, Roger had pretty well taken care of the range, seeing that we got new stations put in where they were needed, and so forth. Then, when we got into some bombing work, Bill Kemper put in some special stations down there for bombing, on which they could use the Askania theodolites. Then we found some interesting things happening. They started using the theodolites for airbursts of antiaircraft projectiles and then checking them with surface projectiles. Then we made a very interesting discovery. The positions on the range as computed for the old stations and the new ones that Kemper put in did not agree. They were systematically off by something like 5 or 7 yards. So we went back and checked on this.

We used the Coast and Geodetic primary survey points in putting in the range. National Coast and Geodetic Survey has a series of assigned triangles that they use to cover the whole United States, and that's the way they get the distances to positions of everything in the United States. Locally, when they put in the range, they started with the Coast and Geodetic points that they had located with their long-range operation. Dahlgren set up a 90-foot tower above each one of these points and then they did all their triangulation at night when the refraction, the heat wave in the air, is least significant. So they started at those stations to triangulate and determine the positions of all the stations established along the range. It turned out that the Coast and Geodetic Survey made a new survey of the United States sometime after our range was established, and they referred to these as datums and gave them a year number like there might be a 1911 datum and then there could be a 1923 datum. It turned out that Kemper put them on an up-to-date datum, and the old stations were all on the old datum, so things got a little confused there.



Present Main Range at the Dahlgren Laboratory.

Captain Ruckner, who was then the Ordnance Officer, gave me the job of writing the letter to the Bureau of Ordnance requesting $15,000 or $20,000 that the Coast and Geodetic Survey wanted to give us a new survey of the whole range according to the latest datum. We got that accomplished, and I don't know if anything new has been done to the range since that time in locating precise positions.

Were there any other people besides Roger Dement and Dr. Kemper?

I would say that in the early days, Roger was pretty much the prime person for looking after the range. His number one helper was Lloyd Payne. There was a period when the amount of range work got so small that you really couldn't justify having the people in the Range Section full time. There just wasn't enough work for them. Then we combined the Range Section's operations with the general instrumentation operations which at that time were under Dee Ross.

What enticed you to remain so long at Dahlgren rather than seek employment elsewhere?

I enjoyed the work at Dahlgren. During the time I was employed at Dahlgren, I can say that I had an exceptionally wide variety of jobs and a wide variety of areas of work. It wasn't like working at the same job all the time.

There was something new coming along continually, and I was getting different assignments. When I came in, I was working mostly in armor and bombs. Then I got into instrumentation, and working with instrumentation got me into everything. What got me into instrumentation originally was in the bombing work. I just said, "The instrumentation is deplorable. Something has got to be done about it." I started doing something about it. Other instrumentation was also deplorable, and they wanted me to do something about that. The first thing I knew, we were just busy doing more and more instrumentation to take care of more and more things that just had to have something better.

I stayed in instrumentation for many years until I got back to what we called W Department, Weapons Development Evaluation, which included the instrumentation development and range operation—everything except the Terminal Range, which was under Terminal Ballistics. Then when they went through the last reorganization which changed the Dahlgren structure from three labs to five departments, I had the Engineering and Evaluation Department. For awhile, I also had the Cartridge Division. I was a great guy for picking up new business. Then I'd get too much and had to give some of it to somebody else. That's what made the place grow and amount to what it is—being ready to do things, picking up new business, and following through.

It's nice to have new business that will improve the scientific prestige of the Laboratory, but we also had to make sure that the new business was going to be of a direct and clear-cut service to the Fleet, not just something to entertain scientists interested in research. I used to tell my people, "There's only one excuse for being at Dahlgren. You're here to serve the Fleet, and any day that you do a day's work and don't serve the Fleet, you don't do what you're here for." Dahlgren has got to keep this in mind. It is not a place for scientists to walk around in their caps and gowns and conduct nothing but very interesting investigations. If they want to do that, they belong in a university. But Dahlgren is for the Navy, and it's to support the Fleet. You've got to continually keep that in mind.

CHAPTER V

Development of the Armor and Projectile Laboratory*

Dr. Ralph A. Sawyer

Dr. Sawyer was born in Atkinson, New Hampshire, on January 5, 1895. He received his PhD in physics and mathematics from the University of Chicago in 1919 after serving with the Army in World War I. Following graduate work at the University of Chicago, he was appointed Professor of Physics at the University of Michigan and held several teaching and research positions before being recalled to active duty as a Lieutenant Commander with the Navy on March 25, 1941.

Dr. Sawyer reported to the Naval Proving Ground, Dahlgren, on June 14, 1941, and supervised construction of new physics equipment. In December 1944, he was designated Officer in Charge of Laboratories at the Proving Ground where he remained until August 1945. The following interview with Dr. Sawyer was conducted by Jack Brooks, Jr., at the University of Michigan on May 22, 1974.

Why were you assigned to Dahlgren in 1941?

Leonard Loeb** had suggested to me, since he was getting the A&P Lab started at Dahlgren, that he wanted spectrographic equipment for quantitative analysis, and he hoped that I would take a commission and come down there to advise them since I knew something of that type of work. So I took the commission and reported to the Naval Proving Ground in June 1941. Loeb was there then, and he was Commander of the Naval Reserve. The A&P Lab had been built, and he was ordering equipment.

*The Armor and Projectile (A&P) Laboratory was constructed at Dahlgren in 1941 to conduct reduced-scale tests of armor and projectiles. These tests were to determine the composition of armor and the relative effectiveness of different projectiles and armor.

**Captain Leonard B. Loeb was Commander of the Naval Reserve at Dahlgren from February 1941 until the summer of 1941.



Naval Weapons Laboratory Advisory Council meets with NWL officials, 1959. Front row (left to right): Dr. H. L. Hazen, Dr. H. K. Ihrig, Captain A. R. Faust, Dr. R. A. Sawyer, Admiral W. A. Kitts III, Dr. L. T. E. Thompson. Back row (left to right): Mr. C. B. Green, Dr. R. H. Lyddane, Captain V. V. Utgoff, Mr. R. A. Niemann, Mr. D. W. Stoner.

The A&P Lab seems to be the fruition of a lot of work Dr. Thompson did at Dahlgren.

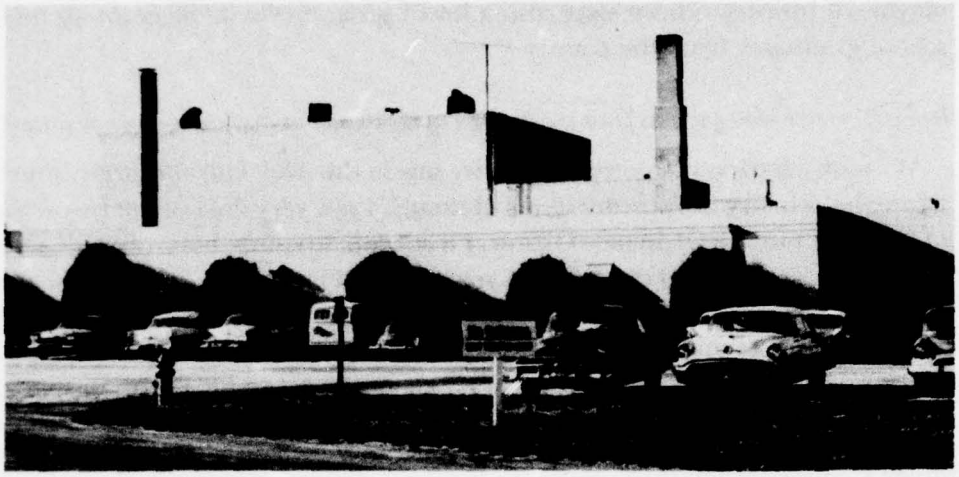
Yes, Thompson was the Chief Scientist there until 1942, and he had gotten the idea from measurements that penetration and armor quality could be tested in small scale and scaled up. He persuaded the Navy, through the Chief of Ordnance, to build a small laboratory which would do small-scale work to save using heavy guns and heavy armor for all the test work.

That was a critical point in Dr. Thompson's career. His development of the small-scale testing came when proof and testing was losing favor with the government officials. After World War I, there was no great allocation of funds for that type of testing.

Yes, Dr. Thompson was very active.

Let's get back to your early work at Dahlgren.

Well, I was sent back to the University of Michigan shortly after I arrived at Dahlgren. I was doing work in the spectrographic field. Then the war broke out and I received orders to come back to Dahlgren. I reported on the 16th of



Armor and Projectile Laboratory in the 1950's.

February 1942, and Loeb was gone. So was Thompson. We lost him to the bombsight people.

The Norden Company?

Right. He went to Norden as Director of Research.

I understand that you were de facto Chief Scientist, or Technical Director, as far as responsibilities were concerned at that time.

That's right. I was Chief Scientist then.

What size staff did you have in 1942?

I think the staff consisted of about eight or ten people when I arrived. There were a couple of mechanics, a secretary, and about a half-dozen officers.

You had been sent back to work on spectrographic equipment?

Yes, I was very active at that time in spectrochemistry in steel. I developed a process by which we could analyze steel samples very quickly. We could take a sample out of the steel foundry and have an analysis in 30 seconds. The Navy wanted a set of my equipment, and that's what really brought me to Dahlgren.

Did you have any civilian scientists working for you?

The staff was primarily military—these fellows all came in as reserve officers. I remember that around 1945 I had one chemist, one civilian metallurgist, and

maybe 70 officers. There were also a lot of girls, maybe a dozen or so high school graduates from the county.

Did you receive your projects from the Bureau of Ordnance or did you go your own way?

We took a look at the potential that we saw in the A&P Lab and pretty much wrote the primary mission ourselves. Actually, I saw very little of the Inspector of Ordnance or the Ordnance Officer. I'll tell you how little I saw of them. One of them—they changed every 2 or 3 years—called me in for some reason, and he said, "Commander, I want to tell you about precedence in the Navy." He said, "Sometime I might be away and you'll be in charge or we might eventually get down to where an Ensign is in charge. He might get an idea in his head and order an air conditioner for his office." He said, "I want to tell you, Commander, if anybody has an air conditioner here, it'll be first the Inspector and second myself." I said, "Yes, sir," and came back to my office, which had been air-conditioned for 2 years. It had to be because we had a big metallurgical laboratory, and you have to have air conditioning to do metallurgy.

Did he ever find out that you had air conditioning?

No. I didn't tell him, and he'd never been in the place. That's how much they bothered me down there.

Captain Hedrick was in charge then. Why do you think he left you alone? Did he respect your position or did he have a lack of interest?

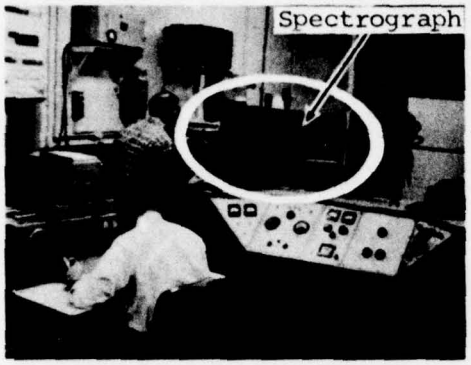
I think he had a good opinion of me, but he felt that Dahlgren was a proof and test facility and that they were there to test ammunition, projectiles, and armor—not to do research work. He felt that the steel companies should do the research work.

While you were at Dahlgren did you get the feeling in your work in the A&P Lab that this type of research was going to grow and become more of a factor at Dahlgren and in the Navy?

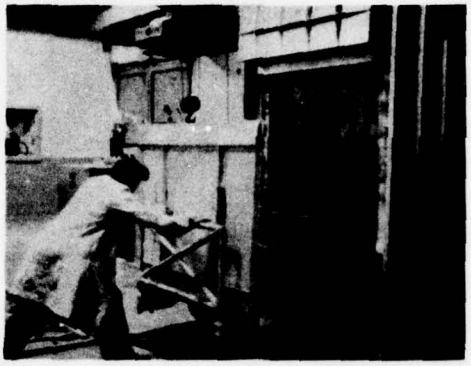
One thing I can tell you right off. We developed the process of case-hardening STS armor—that's special-treatment steel used in thin sheets for splinter shields on 5-inch guns and for airplane protection.

What do you consider to be the most significant achievements of the A&P Lab under your supervision?

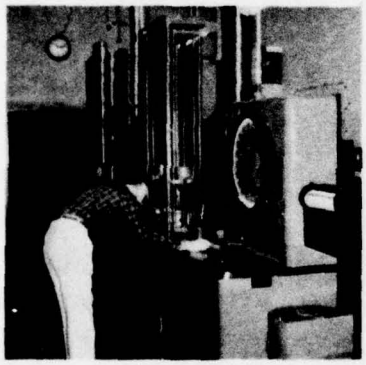
Of course there was the STS armor which was the homogeneous light armor. Optimum ballistic properties were obtained in homogeneous light armor by



Spectographic equipment (including Dr. Sawyer's original spectrograph).



Car bottom heat-treating furnace.



Mechanical testing equipment.

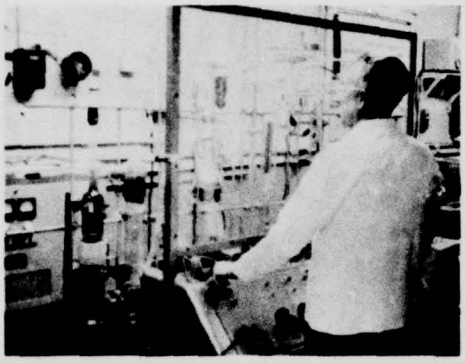


Hardness testers.

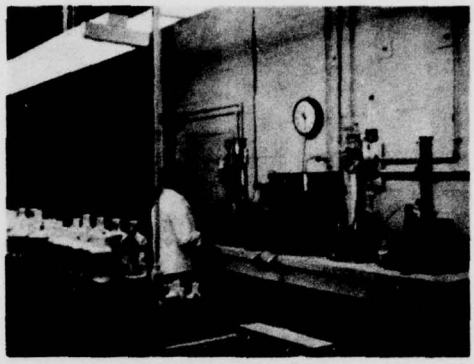
A&P Lab test equipment.

quenching the armor to specific hardness. We also did some work in heat-treating nickel-chrome steel and made it possible to treat heavy plates for uniform structure, regardless of plate gauge.

There is a building story in which you might be interested. We were short of space in the A&P Lab, and I asked Captain McLaren if we could get any additional square footage. He called me over and said, "You need more space, don't you, Commander?" I said, "Yes, sir." "Well," he said, "you've got a builder there, don't you?" I said, "Yes, sir, I have. He used to be a contractor. He worked as a carpenter for me." He said, "You have an electrician, too, don't you?" I said, "Yes, there's one attached to my place." He said, "I haven't disapproved any orders for materials you have sent over, have I?" I said, "No, sir." He said, "Don't let me catch you building anything."



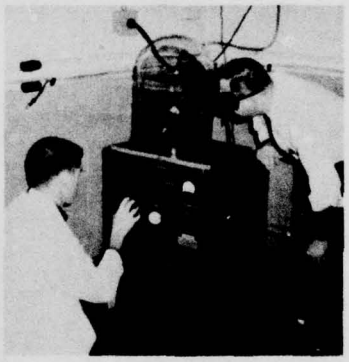
Vacuum fuze gas analysis apparatus.



Carbon determinator.



Metallograph.



Vacuum metal-deposition apparatus.

Research apparatus used in A&P Lab–1955.

In other words, that was an unwritten agreement.

That's right.

When you left the Navy, did you return to the University of Michigan?

Well, not quite. I had agreed with Dr. Thompson that when I was released I would go out to Inyokern, California, for a year as a division director to help them get started. They had my name on a house out there, and I was all packed and set to leave. I was going to leave on a Monday and had my car loaded, but before I left I received word that I was being given a temporary job in Silver Spring, Maryland, and would be transferred to Inyokern later. So I was out at Silver Spring and had planned to leave in 3 days for Inyokern when I got a telephone call stating that the Chief of Ordnance wanted to see me. I hurried down there, and he said, "We'd like to have you go out to Bikini as Technical Director." I said, "Look, I've got a job at Inyokern and I'm leaving next

Monday." He said, "We'll take care of that." So I said, "I'll have to ask my wife, and I'll come back this afternoon." So I talked with her and she agreed to go. I went back and told him I'd take the job, and he said, "Good, we've got the plane standing by. You'll leave for Los Alamos at 4 o'clock." I had luck and we were socked in by fog until the next day. Things were moving very fast, and I was out in the Marshall Islands for most of 1946. I was Technical Director of the first atomic bomb test out there.

CHAPTER VI

Dahlgren's First Director of Research

Dr. Charles C. Bramble

In 1912, Dr. Bramble received a PhB degree from Dickinson College. He was awarded an AM in mathematics in 1913 by Dickinson and received a PhD from Johns Hopkins in 1917. Dickinson later presented him with an Honorary Doctor of Science degree in 1948.

Dr. Bramble was appointed Assistant Professor of Mathematics and Mechanics at the Postgraduate School of the U.S. Naval Academy in 1919 and eventually became Senior Professor of Mathematics and Mechanics.

Dr. Bramble came to the Naval Proving Ground, Dahlgren, in 1942 as Head of the Exterior Ballistics Section. At that time, he was sharing his time with Dahlgren while still maintaining a teaching position at the Naval Academy. Dr. Bramble was employed full time at Dahlgren in 1947 when he was appointed Head of the Computation and Ballistics Department.

In 1951, Dr. Bramble was selected as Dahlgren's first Director of Research. He held that position until his retirement in January 1954.

The following interview was conducted by Cynthia Rouse in Dr. Bramble's home in Annapolis, Maryland, on January 31, 1977.

You had contacts at Dahlgren beginning in the early 1920's. What was your connection there then?

My first contact with Dahlgren was in 1924. In those days, there was no bridge across the Potomac. I used to call up, and they'd send a boat over to Morgantown, Maryland, for me. When I came down, it was just for general interest in ordnance problems while I was teaching ordnance courses at the Naval Postgraduate School. The courses included ballistics and gun design, both exterior and interior ballistics.

Naturally I was interested in the current problems in those areas, so periodically I would get in touch with Dr. Thompson, who was at that time the Senior



Dr. Charles C. Bramble was Director of Research at Dahlgren from 1951 until 1954.

Scientist at Dahlgren. It was a very informal contact, but that was my way of maintaining a live interest in current ordnance problems and the research that was going on. I also did the same sort of thing with the Army Proving Ground at Aberdeen.

When the national emergency [World War II] came on and the decision was made to move the ballistics work out of Washington from the Bureau of Ordnance to Dahlgren, the Postgraduate School was requested to transfer me to Dahlgren, but the Head of the Postgraduate School wouldn't agree, so they compromised by sending me to Dahlgren 4 days a week. That was the beginning of the ballistic work and the beginning of the Computation Laboratory because, at that time, there were only two mathematicians employed at Dahlgren. They were at about a GS-7 or GS-9 level. That was back about 1942, and there were also a couple of women at the GS-5 level.

That was a start. That work, organizationally, was under what they called the Ordnance Officer, who was second in command.

Did you have Admiral Withington as one of your students at the Naval Academy?

Yes, Admiral Withington, Admiral Parsons, Admiral Turner Joy,* Admiral Hussey, Admiral Blandy, Admiral Schoeffel, Admiral Vieweg. The Ordnance Group at the Postgraduate School usually consisted of only about eight or ten officers, and they had more courses with me than any other faculty member. In fact, they stayed at the school longer than most of the groups. They stayed there 1-½ or 2 years, so I got to know them very well. We had very pleasant relationships, a good many of which continued through the years.

By the way, I first came to the Naval Academy in 1917. I was in the Department of Mathematics for 2 years. That was during World War I. Then the Postgraduate School opened up under Captain King, later Admiral King, in 1919, and I was appointed to the faculty there at that time. I remained on the faculty officially until I went into uniform in 1942 and still continued then. After I was out of uniform in 1946, I remained at the Postgraduate School as a civilian on the faculty until 1947.

And that's the time you transferred to Dahlgren?

Yes. There was a question of whether I'd stay in the regular Navy, as the Proving Ground was interested in my continuing the work that I had started during the war. You see, we started the Computation Laboratory. I decided to come back as a civilian, and as things turned out, I'm happy that I did because in 1947 my family and I moved to Dahlgren and we occupied quarters 508 for 7 years.

You mentioned Dr. Thompson. Could you tell us something about his work and the man himself?

He was technically classified as a physicist, but he was the Chief Scientist, or the Senior Scientist, although there was no title of that sort at the time. There were two or three other scientists who came later as civilians, I think about 1934. But there was just a very small handful of civilian scientific personnel. The rest of the people were essentially all trained on the job at the batteries—Main Battery, Machine Gun Battery, etc.—so the actual highly trained technical competence was rather scant in those days.

Dr. Thompson had a very pleasant personality and really got along very well with people in general. He was very diplomatic in dealing with the naval personnel in command, and he also developed quite a bit of "clout" in dealing

*Rear Admiral C. Turner Joy was Commanding Officer at Dahlgren from June 1946 until August 1949 and was advanced to the rank of Vice Admiral in August 1949.

with the Bureau of Ordnance through the years. He got to be well-known and respected.

One of the reasons they were interested in having me come to Dahlgren was that Dr. Thompson was leaving. I had the advantage when I came into this work that the personnel who were in command positions in the Bureau of Ordnance were ex-students of mine and were practically all personal friends. For instance, Admiral Schoeffel, Admiral Hussey, and later on Admiral Burke. That was always helpful in expediting our programs. Anyway, Dr. Thompson left to go with Norden Laboratories because Norden Laboratories had the mission of establishing the Indianapolis Ordnance Plant for the Bureau of Ordnance, and he went to Indianapolis and was there during the war. Later, as an outgrowth of that and Dr. Thompson's ideas, the Bureau of Ordnance started the laboratory at China Lake.

What were some of your major duties when you were attached officially to Dahlgren in 1942?

When I was detailed on temporary duty to Dahlgren, which was really a steady job, I was instructed to develop a facility for producing the Navy range and bombing tables. At the time I went there, there were only two desk-type computers in the place and two mathematicians to operate them. I immediately saw that we were going to need more desk-type calculators, so I put in a request to get five more. I came back to Annapolis on Thursday evening, and when I returned to Dahlgren on Monday, I found that the order had been changed to two for ballistic work and two for the so-called velocity measurement work. I was somewhat appalled by that, and I think it was difficult to impress command with the necessity for this kind of work, but it was just a matter of a very few months before we were up to about 50 desk-type calculators.

Shortly after I arrived, we put in requests for mathematical talent from the Naval Reserve. Then we brought in some Wave officers who had mathematical training. I immediately put the mathematicians to work studying ballistics. I gave them some lectures on ballistics and set up the computational procedures. They taught the Wave officers what to do to manage enlisted Waves who worked the desk-type calculators, and we soon had the laboratory going.

How did Dahlgren's involvement with the Aiken Relay Calculator come about?

Later, we began to see the first development of what we might refer to as large-scale program calculators. We saw that Aiken had developed the MARK I, which was electromechanical, at the Harvard Computation Laboratory, and also Bell Labs had developed a relay calculator for use in design work in connection with Bell Telephone. We saw the need for getting large-scale

computers into ballistic work, and we developed a contract with MIT to have them operate for us the Bush Differential Analyzer. They ran a great many of our basic trajectories for the firing tables and bombing tables, and they sent us the output. We polished it up and developed the rest of the subsidiary data that was essential to the firing tables—all the so-called differential effects. Then we saw the need for having a large-scale calculator of our own.

I had some consultations with Bell Labs, with Harvard Computation Laboratory, MIT, and Princeton. As a result, through the Bureau of Ordnance, a contract was given to the Harvard Computation Laboratory for the development of the MARK II Calculator. That was the first one [large-scale computer].

Incidently, about the same time or maybe prior to the MARK II, I had consultation with Dr. Eckert at the Nautical Almanac Office, who later went with IBM. IBM developed a couple of small relay computers. They were about 4 or 5 feet long, 4 feet high, and a couple of feet wide. They built three of them, and they placed two of them at Aberdeen and one with us. They called it the 799 Relay Computer, and they had us experiment with it to see what we could actually do with it in the way of practical computation. We used that to do a lot of our computation work with IBM cards before we got the MARK II.

Then Aiken developed the MARK II for us and shipped it down. What a time we had getting the bugs worked out of that! The programming was rather difficult. There were lots of failures, inaccuracies, and a great deal of difficulty in troubleshooting, but we got a lot out of it, nevertheless.

That program was followed by a research program through the Harvard Computation Laboratory to investigate the feasibility of a fully electronic calculator. Aberdeen, at the same time, had developed an electronic computer called the Eniac. Of course, I was also in consultation with them and made frequent trips to Harvard and kept in touch with Aiken.

As a result of the research contract with the Harvard Computation Laboratory, we were able to have the MARK III developed. The MARK I was a relay calculator. It was completely based on relay and tape—punched tape. The MARK III was electronic with a magnetic drum memory and magnetic tape.

The next development was the NORC [Naval Ordnance Research Calculator]. Rather let's say the next development at Dahlgren was the fact that the Bureau decided to locate the NORC at Dahlgren. That really put the Computation Laboratory in "big-time" calculations because the NORC was so far beyond anything else at that time.

About that time I was approached by Norden Laboratories. The decision [to get the NORC] was pending when Norden approached me to come with them, and I talked with Admiral Parsons and Admiral Schoeffel about it, and they proposed that I stay at Dahlgren until the NORC situation was fully firmed up. The NORC was delivered very shortly after I left in 1954. That was really, I think, one of the turning points in the history of the Laboratory.

In 1951 you were appointed as Dahlgren's first Director of Research.

I was functioning, in a sense, as a Technical Director, but I was not so designated until 1951.

This was a period of heavy proof and test during the Korean War. What did you see as your mission at that time?

The mission of the Station was to do research on weapons systems, to carry out test work on systems under development, and also to perform proof work for production. Down there in the early days, you'd hear guns all day long because they used to test so much ammunition.

Can you tell us something of the work of Mr. Riffolt, who succeeded you as Director at Dahlgren?*

Mr. Riffolt came to this country from Sweden. He came to Clark University, and he was originally an instrument maker and a very skillful laboratory technician. He was more of a technician than an engineer. I'm not saying that in a derogatory sense at all, but that was the situation. He worked with Dr. Thompson at Clark University, and he was influenced to come to Dahlgren by Dr. Thompson.

Nils Riffolt was really the predecessor of the A&P Laboratory. His strength was in the field of physical and engineering measurements. Of course, that's a field in which the Swedish engineers are preeminent. His interests were in the technical operational details within the laboratories. I don't think Mr. Riffolt really had much concern with what we might call administrative control. He was the type that either worked in a laboratory or sat thinking at his desk.

Were you optimistic about Dahlgren's future when you retired in January 1954?

Yes, I really was. I was optimistic because they were getting the NORC. I felt that was really going to give them a very solid foundation for a growing Computation Laboratory, and I felt the Computation Laboratory was very essential to the Bureau of Ordnance. I thought that we developed an excellent staff there at that time.

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*Mr. Nils A. M. Riffolt, a native of Sweden, came to Dahlgren in 1925 and worked with the interior ballistics program. In 1942, he was placed in charge of the Experimental Gun and Interior Ballistics Laboratory. From 1954 until his retirement in 1956, Mr. Riffolt held the position of Director of Research.

CHAPTER VII

Times of Crisis

Dr. Russell H. Lyddane

Dr. Lyddane received a PhD in physics from Johns Hopkins University in 1938. He then taught physics at the University of North Carolina before coming to Dahlgren in 1941. From 1941 to 1946, he was a physicist at the Naval Proving Ground, Dahlgren, and from 1946 to 1948 he served as Head of the Armor and Projectile Laboratory. Dr. Lyddane directed terminal ballistics research at Dahlgren from 1948 to 1954, at which time he was appointed Assistant Director of Research. He was selected Technical Director of the Laboratory in 1956 and served in that capacity until 1964 when he joined the General Electric Company in Schenectady, New York, as a member of the company's Engineering Service Group.

The following interview with Dr. Lyddane was conducted by Cynthia Rouse in Dr. Lyddane's home in Schenectady, New York. on January 24, 1977.

When you came to Dahlgren during World War II, what was the work environment like?

When World War II came along, the proof and test burden at Dahlgren went way up. There wasn't as much proportional time to do experimental work or any kind of R&D work, although there was more absolute time. The Navy had recognized quite early that it needed competent professional technical help, and of course you had that at Dahlgren when Dr. Thompson got there. He started building that almost single-handedly, and by the time the war came, he had a small staff which I joined near the beginning of the war.

Then the big influx of professional people came with the reserve officers. These, of course, were young men who had, while they were in college, gone to Navy ROTC or joined one of the few Naval Reserve units, and when war came, they were called to active duty. In these ranks were many engineers, physicists, metallurgists, and other technical personnel who were very fine candidates for firing officers.

The burden of proof and test, as I said, was enormous. This was Dahlgren's mission during the war. There was, in fact, very little thought given to other



Dr. Russell H. Lyddane was Dahlgren's Technical Director from 1956 until 1964.

areas throughout the war, partly because there just wasn't time. While the war was on, one wasn't thinking about after the war; one was thinking about just getting the war over with—then we'll think about what we are going to do next. But it was perfectly obvious what was going to happen, and it did happen. When the war was over, the proof and test work went down to almost nothing. This was Navy-wide, and that was when the difficulties of the Naval Gun Factory started.

The Gun Factory did a tremendous job during the war in turning out millions of rounds of 20mm projectiles, etc. At the end of the war, nobody wanted any 20mm projectiles. The only thing anybody could do was experimental work—R&D; and if you couldn't adapt to that, you were doomed. That's what doomed the Gun Factory.

However, Dahlgren retained—as one very major factor—the ballistics table work that it had been doing during the war, which was the genesis of the computation and exterior ballistics work. Historically, Dahlgren had the re-

sponsibility for the Navy range tables, gun tables, and later bombing tables. This went on, and the Bureau of Ordnance supported the mission which led to the development of the digital computer.

There were only two groups of people in those days who were interested in large-scale computers. One was the astronomers—they never had any money—and the other was the ballisticians. As soon as the ballisticians got money at the beginning of World War II, the computer was developed. The time was about right, but money was also needed. If the war hadn't come along, we wouldn't have anything like the computers we have now.

So here we were at Dahlgren with a rather healthy, although not terribly large, ballistics and associated computational operation. We had also a fair amount of development work in the gun area because between World War II and the Korean War there was a fair amount of money put into that. You know, "We've been through the Manhattan Project, and we've been through the development of radar, and we've been through all the wartime weapons development. Now let's put some of the real technical push and money into improving the more or less conventional weapons like guns and ammunition and explosives and see what we can do." We did.

Then along came the Korean War and the guided missile. It became increasingly obvious that the missile was the place to put money, and money began to get short in the areas in which Dahlgren had a real advantage in the sense of an established competence as well as facilities. However, with the Korean War, up went the burden of proof and test again. So proof and test was competing for the same facilities as the experimental work, and proof and test had to win. It always does. "You've got to fight this war, so we'll be here tomorrow to do our R&D on a reasonable kind of time scale."

Those were the principal reasons Dahlgren began to get into difficulties, and the difficulties kept increasing in the early 1950's. We probably hit our low watermark about the mid-1950's. It became ever more apparent that Dahlgren was going to have to find something else to do because its old sources of funds were simply drying up. For example, there just wasn't any money to go out and develop a better rotating band for a projectile.

Let me go back to World War II for a moment. As I said, Dahlgren had a small civilian staff, probably on the order of a dozen professionals with a good number of reserve officers plus 40 or 50 USN types and a very large staff of blue-collar workers, because proof and test makes blue-collar employment rather than white. You have one firing officer and a tradesman staff of 20 or 30 people to support him in getting the test set up. There were lots of riggers and ordnancemen and all the other kind of trades work to conduct one of these proofs, and the test facilities in those days were really not so enormously different from what we have now. About 1942 we built the addition onto the Ordnance Office [Building 218]. Practically all the technical work was done

there. The only major thing missing in the way of facilities was the Computation and Analysis Building [the present computer facility].

Did you come to Dahlgren as a Naval Reserve Officer?

No. At the time, I was on the staff at the University of North Carolina. I'd taken my Doctor's Degree about 3 years earlier in 1938, and I began to consider doing something during the war to help the country. I went to some of my friends who directed me to Dr. Thompson at Dahlgren. I had an interview with him and he offered me a job. I came to work on the first day of July 1941.

In those days, the civil service procedures were very complicated and slow, and it wasn't possible to offer people civil service jobs, so what they did was offer me a contract job which amounted to the same thing as a civil service job with no permanence. I had a year's contract at about a GS-7 salary, and by the end of that year, of course, the war was in full swing and they weren't about to let us go.

There were about three or four of us who came in about the same time, and so they converted us to civil service and we stayed. I did experimental work during the war. Dr. Thompson was called the Experimental Physicist, and I was on his staff. Largely, we were doing studies of one kind or another, and I remember the first job we did was to study the solenoid chronographs. Those were the devices we used in those days to measure projectile velocity.

That was quite a long study, and there were others—bombsights took a lot of effort. I also remember the flap we got into about Tarawa. The marines went ashore on Tarawa and discovered to their consternation that the bombardment preceding the attack had not ruined the Japanese the way they thought it was going to do. The Japanese had built themselves coconut-log and sand fortifications. These were astonishingly effective. They retired into their fortifications, and when the bombardment stopped and the marines came ashore, the Japanese came out full of fight. They were supposed to have been completely destroyed.

So the Fleet said, "What is wrong with our ammunition? Why have you done this to us? Please explain, and tell us what we can expect the next time?" So we went through this period of designing and building coconut-log encasements and firing 5-inch projectiles at them. It came out exactly as you'd expect. It is not difficult to stop a 5-inch projectile.

Then we had also at the beginning of the war what was first called the Reduced Scale Laboratory, later the Armor and Projectile (A&P) Laboratory. I had a fair amount of interaction with some of the people there, notably on the question of understanding light armor and how you should heat-treat and design and choose light-armor material for best protection. We used to have quite a bit of trouble with that. The Bureau of Aeronautics had a standard form

they would send down. "We have a new airplane going out. What's the best ¼-inch armor we could put in it to protect the pilot against 30-caliber ammunition?"

Well, unfortunately, this was a silly question, because there wasn't any. It's better to leave it out than put in an inadequate piece of armor which simply takes up weight and doesn't protect the pilot against anything. So for a while, Dahlgren was writing letters back saying, "The best thing you can do is use such and such material. It isn't any good at all, but it's the best you can do. It will stop a 30-caliber bullet if you fire it from 2 miles away, but those are not the attack conditions." Finally we put a stop to it. We wrote one letter back and said what I just said—that it is absolutely useless. Don't put anything on it. Take it off. Save weight.

Another problem for Dahlgren came with the attack on North Africa. The French Fleet started to come out, and this made the Navy very nervous because they didn't want to get into a battle with the French for several reasons. However, MASSACHUSETTS had to fire at a French destroyer and put a 14-inch projectile squarely through the stack. There was a 14-inch hole in the stack of that destroyer, and that was all. The projectile didn't go off. "Why didn't it go off? What kind of bad ammunition or bad fuzes are you giving us?"

Well, you see it was an armor-piercing projectile. They were loaded for bear. They were loaded for the French Fleet. They fired an AP projectile at this poor little destroyer that didn't have a plate on it thicker than a quarter of an inch. It didn't do the projectile any harm at all. In fact, the projectile didn't even know it hit anything. Other than deceleration, an armor-piercing projectile has nothing to act as a signal to the fuze that it has hit something, because the fuze is carefully hidden inside where it can't suffer damage. You have to get it through a rather thick plate of armor without getting it hurt and then let it function. All it can feel is the deceleration. If it hits a 3-inch plate of armor, it gets a real whack. That starts its processes going, and it goes off. If it hits the ¼-inch armor, the projectile doesn't even slow down enough to give the fuze a shock.

These were some of the flaps that had to be answered, because it was perfectly conceivable from the Fleet's point of view—and I'm not blaming them for ignorance or malice or anything—that there was a bad fuze there. It had been known to happen.

Everybody was quite conscious of the terrible situation we ran into with torpedo fuzes at the beginning of the war—largely because Congress had been too damned tight to provide any money for testing torpedoes. All of our torpedoes had been designed and built without fully testing them. If you tested them, what you did was run a dummy with no warhead. If you tested the motors in the torpedo, you could recover the torpedo and refurbish the motors and use it again. You could afford to do that. You could not afford to blow up a torpedo. It's a very expensive device. We paid for that very heavily in the first months of

the war. We had fuzes that just did not work. So everybody was conscious of that, and nobody wanted to see it happen again.

Do you have anything else you would like to say about how the technical focus evolved after the war?

Yes, I did carry that part down to about the Korean War and the aftermath thereof. After the Korean War, we had another decline in personnel because we had taken on a lot of blue-collar help to do the proof and test, and this dried up again as it does after every war. We were losing people, and money was getting quite scarce because the R&D projects that we had been supporting ourselves on before the Korean War were drying up. Our budget went down, and our employment went down year by year. Finally, it got to the point where we had to do something.

We had at that time three sources of strength. The first, most obvious and most indispensable, was the Computation Laboratory, which now, owing largely to Dr. Bramble's efforts, was about the finest computation center in the country and was recognized as a resource—not as much as one would like because the resource really consisted not just of a big machine but also of a staff that knew how to get something out of the big machine. This was the message we had to carry around. In those days, people were altogether too inclined to think that a computer was a black box that you carry into the laboratory and it had a button on it you pushed and it answered your questions. I can't tell you how many times I pointed out to various Admirals that what we had was a facility not only of machinery but also of people. This was an operating entity which could produce valuable answers and needed to be supported in order to do so.

The second item was the A&P Laboratory, which was a very well-equipped metallurgical and ordnance laboratory capable of doing it all. It was originally set up to do research on armor and armor-piercing projectiles. The concept dated back to about 1936 and its construction to about 1941. There it was, and the same techniques, the same expertise that you apply to design a better armor-piercing projectile can also be used to design better warheads. We chalked ourselves up with confidence in the warhead area. After all, if the Navy was going along the route of guided missiles, why couldn't we do the warhead work?

It was quite clear that what we needed was to get a greater, broader responsibility from the Bureau of Ordnance. The first prerequisite for that would be to get the recognition from the Bureau that we could play a role in the increasing technological arena of weapons development.

The various Navy laboratories were actually, of course, rivals so we didn't get any help at all from them, and we had to fight our own battles for our share of the sponsors' resources. The principal sponsor was the Bureau of Ordnance. In

presenting our case, we had to muster our resources and tell them what our resources were.

The third resource, to continue the list, was the fact that we did have by this time a quite competent technical staff. We had a good cadre of scientists and engineers. We had built this up painfully over the years by inching and pinching. Every time we hired a professional, we had to get rid of, by attrition, one or two blue-collar people so we'd have money to pay them. We *had* to increase the technical staff, and we took every step we possibly could. We starved our people for scientific aids. Our engineers and scientists said, "I need somebody to do this data processing work." Well, "I can't give you anybody to do the data. I can't give you an engineering aid, because that's one billet I could put a professional in, and what we're going to live or die by is not how many engineering aids we have, but how many engineers we have." I went through that speech I can't tell you how many times.

Dr. Bramble recognized this problem as well as I did, and so did Riffolt. It was a slow process cutting back the blue-collar staff when we got out of the proof and testing business and trying to increase the professional staff. This was a very difficult problem. Everybody spent a great deal of time on it, because in the early 1950's the economy, as far as weapons development was concerned, was booming. There were defense contractors all over the place who were stockpiling engineers like cordwood because if they got a big project, they were going to need them very badly in order to complete the project. They put them on the staff and gave them make-do work until projects came in, because when a project came in, it was too late to try to go out and hire. It was a very wasteful procedure, but you couldn't blame them for it. It was the only way they had to live. It made it extremely difficult for the government to compete against its own contractors, and we had a terrible time recruiting.

What problems did you have in classifying professional positions?

Anybody in the Navy in those days could tell you that one. I used to meet with the senior scientists of the leading laboratories, and the main topic that would get voices raised was, "Let me tell you what they did to me." The difficulty was that the Area Wage and Classification Office [AWCO] had people who were doing the position classifications who had no responsibility whatsoever toward getting the work done. They were optimizing things that we didn't particularly care about.

It was a constant fight with AWCO, which had position classifiers who were not only not technical people themselves, but also too far removed from any responsibility for getting the work done. Management does need enough authority with proper checks and balances which are applied by statutory considerations. They do need enough leeway to not be completely frus-

trated by the fact that, "I can't pay this man enough to keep him on my staff, and I need him desperately." This was the problem we were always confronted with in some degree or another. We were competing with the defense contractors who could pay more. We couldn't even get what we thought we were legitimately entitled to out of the people who were doing the classifying. We would make representations to them, and they would say, "Sorry, that's the way the law is." We knew perfectly well it wasn't. We knew what the law was. We'd read the law too. It wasn't the law but their interpretation of the law. It is the usual problem you get into when you have a bunch of administrators who are completely divorced from responsibility.

I don't feel any particular animosity toward them, although they caused an enormous amount of trouble. I can understand why they did it. What we were principally annoyed at was the system that allowed this to happen. That problem was fought on a great many fronts. It was fought all over the Navy. The final happy resolution came when we were allowed to add the position classification function to our own Personnel Department. I watched it very carefully because AWCO said what would happen if you let everybody get everything he wanted. It wasn't true.

Waldo Beck* was our first classifier. He did a very conscientious job, and he was quite aware of the trouble that could arrive if he were too permissive. All of our difficulties went away when we had someone who was as interested in the success of the Naval Weapons Laboratory as I was.

What time frame are you talking about here?

About 1960, I would say. That was the time we had won the fight, but the fight had been going on since 1948 or so. It was a long fight and an extremely frustrating one.

You have already touched on this, but can you give us some more of the highlights of Dahlgren work in the late 1940's and through the 1950's?

There was considerable growth in computation and ballistics. That was in increasing our machine capability, learning how to use computers, and really acquiring more responsibility from the Bureau. One other thing that I should note is that after the war, the Bureau began to find it more and more difficult to retain technical competence in the Bureau itself. In the older areas, they had some people who very often had come up as draftsmen and after years of service were then sort of limited engineers. They did a good job. However, they were not the kind of people on whom you could build the technical

*Waldo H. Beck came to Dahlgren as a Position Classifier in 1955 and is presently Head of the Wage and Classification Division in the Civilian Personnel Department.

competence needed for modern development projects nor was the climate at all good for increasing that staff by recruiting from the professional field. Congress was not willing to believe that anybody in Washington really worked. They *were* willing to believe that somebody out in a field station might conceivably do some work sometime, but they knew those guys in Washington were not doing anything. Any time the departments tried to increase their Washington staffs, they ran up against a stone wall. The lesson became perfectly clear. Technical competence was felt to reside with either the defense contractor establishment or with one of the field laboratories.

So this was the other factor that came into this business that the several laboratories were now competing for. "Put the new capability at my place, not his." The point I'm now making that I didn't make before is that there was this new shift of responsibility out of Washington and into the field, so we were trying to compete for this, and we were competing under some difficulties. We were small and we were old. We were an old established activity. That may seem to be an advantage, and it was in some regards. However, in some regards, it was a considerable disadvantage because there were too many people around who knew us "back when." You know, "When I was at Dahlgren as a JG in the early 1930's"—this is an Admiral talking—"when they didn't have anybody, it was a very remote, primitive, picturesque isolated spot. My goodness, you wouldn't think about putting anything modern and new there. Hasn't that been closed yet?" I suffered through that many times. None of it was true anymore.

How did the Fleet Ballistic Missile [FBM] Program begin at Dahlgren?

It started when the JUPITER missile development was under consideration. The Navy did not want to be left out of the ballistic missile picture any more than the Army did. Here again, we had a real under-the-cover fight. The Air Force said missiles were its prerogative.

The big development money now—we're talking about 1955 or 1958—was all in ballistic missile development. The Army had picked up Wernher von Braun, and he was down at Redstone Arsenal, anxious as hell to continue the work he had done on the V-2 and inching and pinching and scraping and getting a dollar together here and there for little pieces of work. So this was the real big opportunity.

One of the results was the Navy planned the most cockeyed scheme I'd ever heard in my life which was the JUPITER missile. The JUPITER was going to be a liquid-fuel missile about like the ATLAS, which was the technology of the day. It was smaller but a big beast, and it was going to be launched from the deck of a merchant ship.

The idea of launching from the deck of a surface ship something as fragile as a JUPITER was preposterous. For example, you couldn't ship an empty

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ATLAS missile around the country. You had to pump up its tanks. The walls were so thin that it took pressure in the tanks to keep the thing from collapsing from its own weight. It was extremely delicate. Not only that, but imagine launching it off a pitching ship in a North Atlantic sea. It's incredible, but the Navy was desperate.

About the same time, three developments came along that saved the Navy's bacon, and the Navy was smart enough to seize the opportunity. First of all, the Atomic Energy Commission [AEC] came up with miniaturized warheads. That took an enormous load off because as soon as the warhead size went down, the size of the entire missile went down. When the size of the entire missile went down, you could start thinking about the second development, solid propellants. You couldn't launch an ATLAS-size missile with solid propellants. Nobody had the technology. But now we're talking about the POLARIS size compared with the ATLAS size. Now you could start talking about solid propellants, and that's a whole different ball game. You could carry those things around like rocks. You didn't have to worry about their being mishandled.

Draper* came up, at the same time, with the third development which was his very-high-accuracy gyro. This gave us a chance for the kind of guidance system we wanted. Those developments made it possible for the Navy to start thinking about a POLARIS kind of missile, and it wasn't very long before the submarine launch got tacked onto it. That solved a lot more problems. A merchant ship carrying ballistic missiles around would be about the most vulnerable thing you could possibly think of. You couldn't hide it. You couldn't protect it. It didn't take very much to destroy it. But submarines were and still are notoriously hard to find. That was when the Navy started the POLARIS business in the Special Projects Department with Red Raborn** in charge—a man for whom I have enormous respect. He did a magnificent job on the POLARIS Program.

The first connection I remember Dahlgren had with this was when we had a big session down at Redstone when the JUPITER Program was first started, and every Navy activity that went down there put on a pitch, "Why I should be in the new Navy Ballistic Missile Program."

I learned a lot there. It was, as I recall, one of the first of that kind of presentation that we had to do. I had an opportunity in the morning to watch what everybody else did, and that taught us a great deal about what not to do. For example, there was one Technical Director of a Navy laboratory who came down there and talked at great length about what a fine school he had on his

*Charles S. Draper is an American aeronautical engineer who did research and development on fire control, flight control, and inertial guidance systems for the Air Force and Navy and was a consulting engineer to many aeronautical companies and instrument manufacturers.

**Vice Admiral William F. Raborn held overall responsibility for the FBM Program from its inception through the development of the POLARIS.

installation and showed pictures of his elaborate housing. That did not exactly fly.

I didn't get on until late in the afternoon, and I was so damned tired of hearing these guys go over the same thing which all of them had. You know, everybody's got a comptroller, everybody's got a fine personnel department, everybody's got this, and everybody's got that, and everybody insisted on talking about it. So when I got my chance, I got the only round of applause. I said, "We've written all of this up. It's here in this brochure. You all have copies of the brochure, and I hope you read it. We've got everything that everybody else does." There was no immediate result from that exercise, because von Braun was quite hard to get along with. Fortunately, the JUPITER project aborted, and we had, by this time, impressed sufficiently on people that we had a very fine ballistics capability, and that was where we got into the Special Projects. That was our real contribution to the POLARIS. As you know, it went on and got bigger and bigger. As we demonstrated that we could do useful things with our capability, that was the best thing of all. Once you get a foothold in one of these areas, then the thing that you have to do, obviously, is deliver. Proof of performance is one of the best inducements to get a sponsor to entrust work on projects, and funds, and responsibility to you. So this was one of the means by which we expanded the Laboratory little by little, and we were always conscious of the fact that we did, after all, have a rather small operation, in comparison with our rivals.

We had lots of people at Dahlgren, but our technical staff was not as large as we would have liked it to be. Sponsors have a habit of looking at you and saying, "If so and so quits, what will happen to my project?" Now if you can convince him that you have enough depth so that there are three or four other guys you could put in place of someone who leaves, then he has a great deal more confidence that he can entrust his project to you and not find himself facing his superiors with a story of well-intentioned failure. One always in these cases has a step-by-step struggle, and as you gain a little momentum, it becomes easier to gain the next bit of momentum. But getting off dead center is the tough part.

The Naval Proving Ground at Dahlgren became the Naval Weapons Laboratory in August 1959. Was this conversion to a laboratory done at the initiative of the Bureau of Ordnance or Dahlgren?

That was us. Very much. The emphasis of that came from Dahlgren. We had to change the name because of our stereotype heritage. Oh yes, the Naval Proving Ground. It's not the Naval Proving Ground anymore, it's the Naval Weapons Laboratory. It's greatly changed. I remember we did quite a lot of thinking as to what to change the name to. We didn't want to be the Naval

Ordnance Laboratory because there already was one. You know, NOL, White Oak, and NOL, Dahlgren. We did not have sufficient personality of our own or sufficiently established credibility to be another NOL because the next thing somebody would be saying was that we were a field station of the big boy in Washington. NOTS we thought about, the Naval Ordnance Test Station, but that wasn't really what we wanted to convey because we didn't want people to call us a test station. We weren't a test station anymore. This was all part of our big campaign which was to convince our sponsors, notably the Bureau of Ordnance, that we were in business, should stay in business, and should be regarded as a factor in the world of today and not yesterday.

Things got quite bad. It was about 1956. So we started a considerable campaign. It was very much of an all-hands maneuver, and all of our top technical people were concerned—Niemann, Jones, Cohen, Stoner, Overman, Rossbacher, Meyers, and so on. I took pains to make sure they were all engaged, because this was not the kind of thing that could be brought down from on high.

We had two things that we really needed to do. One was to get opportunity from the sponsors to demonstrate our capability, and this I had to do. I was going to be the front man. At least I had to take the leading part. But the more important part was what happened when they grudgingly gave us something to do. Then these other guys were going to have to come through and demonstrate success. Then I could go back and say. "Look at the success we've had. Now give us more." We spent an awful lot of time organizing our approach, and I remember the first broadside in our campaign.

We asked for an audience with Admiral Withington who was then the Chief of the Bureau. He gave us a good long time. He brought all of his staff along, and he listened. I made a pitch, and when I got through, Admiral Withington said, "That's the best presentation I've ever heard."

We tried to be as honest with him as we could possibly be. Here's Dahlgren, and first of all, "What's Dahlgren's situation?" Well, we described its history very briefly. "And what does Dahlgren have?" This was very clearly organized along the three lines I mentioned previously. "What were our strengths? What did we have, and what could we do?" The last section was, "What can the Bureau do?" For example, the Bureau can go on with Dahlgren the way it is. We don't recommend this because we're sliding downhill. We are just getting a little bit less able each year as we go on with inadequate support, without a real mission, without the kind of support that is needed to keep a technical staff viable and producing. You would do better not to follow this course.

"What else could you do?" Well, you could make us a field station of NOL, White Oak. This is a possibility. We'd talked about it. "Would this be satisfactory?" We said it would be a way of keeping the field facilities operating, but we didn't think that if you did that you should keep Dahlgren as a separate entity. Abolish it and make it a pure and simple outlying field site for NOL.

Don't have any technical staff down there because you cannot run a pure test station—not today.

People say "RDT&E" as if it is all one thing. It isn't by any means, but a proper mix of RDT&E is essential for a technical installation like a Navy laboratory or a proving ground. You need people like Allen Hershey* around, even though he might not contribute anything at all to the direct mission. He's doing basic research, and this rubs off on the other people in the Laboratory. You need a few people like that to keep the intellectual tone what it should be to promote a healthy professional climate. A lot of people on the staff are proud and happy to know that Dahlgren is doing basic research. They don't want to do it themselves. It's not their talent, but its existence helps their morale and their professional competence.

We tried occasionally at Dahlgren to run a separate test installation, and there were managerial reasons why this made a nice organizational entity. Let's make a pure test operation. It would die on the vine. You couldn't persuade good people to go there. The work would not be sufficiently stimulating. If you mixed it in with the other kind of work, development work, and gave these guys some responsibility, some sense of belonging to the development program rather than being there to run tests, you found that it was a great deal healthier, and it worked a great deal better. So we were not at all enamored with the idea of being an independent test station because that would not work.

We told Admiral Withington that the third alternative was to support us—find us something to do. Here is what we are good for. You can tell us what projects you can nudge our way. The fourth alternative was just to close us down.

He adopted the one we wanted him to adopt. He started asking his staff, "Okay, what can you give Dahlgren?" He asked right there in the meeting. We got our first project started right there because one of the Captains on his staff was at the time worrying about HERO—what later became HERO [Hazards of Electromagnetic Radiation to Ordnance]. He said, "This Hazards Program—I've got to put that somewhere. You know, it wouldn't be a bad idea to put that at Dahlgren. We'll consider it." And he did. That was our showdown, so to speak, and Withington was convinced that we had something, and he supported us.

But now, you see, this gave us nothing more than an entree, a chance to present our case. Then we had to send people up to the Bureau and beat on the doors of the individual project officers who had programs to sponsor and convince them that this was the way of the future. You'd better get on board. It took the entire senior staff an awful lot of time in Washington to get this thing moving. And, of course, the more you get it moving, the more you demonstrate

*Dr. Allen V. Hershey is a senior member of the Warfare Analysis Department.

that you can perform, the more funds you get, the more people you can hire, the more solid an organization you can build, the more results you can produce, provided you don't make the kind of error that some of the other Navy laboratories made. They played a great deal of "scientist versus naval officer." That is not any way to build an organization which gets its bread and butter from the Navy.

We always took the attitude that the Fleet was, after all, our customer, and if you are going to stay in business, you'd better worry about and respect your customer. If you despise your customer, you cannot possibly succeed. You'd better find another business to go into.

CHAPTER VIII

The Manhattan and Elsie Projects

Wesley W. Meyers

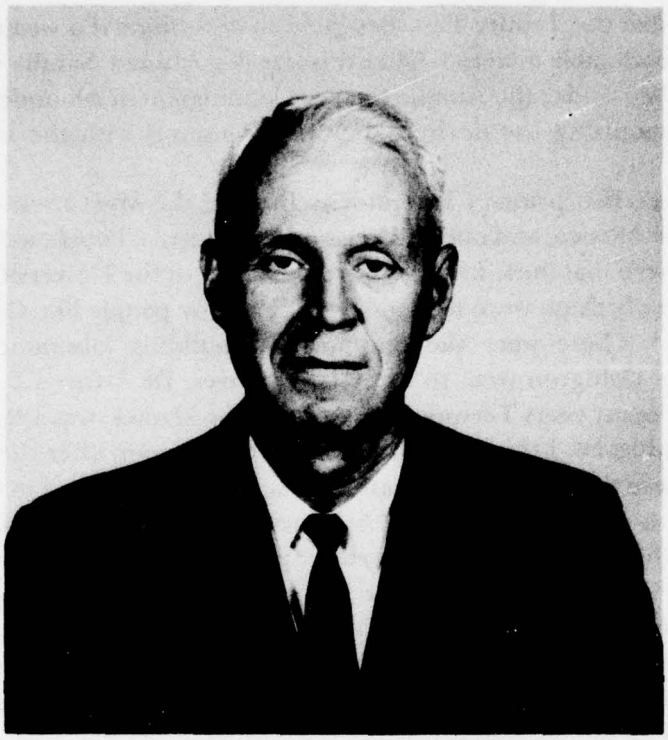
Mr. Meyers received a BS degree in chemistry from Illinois Wesleyan University in 1937, after which he was employed as a metallurgist until he was called to active duty with the Navy in 1943. Naval Reserve officer duty brought him to Dahlgren in 1944, and he remained in a civilian capacity after being released from the Navy in 1946. From 1946 until 1956, he served as Head of the Plate Battery Division and Special Projects Division. From 1956 until 1968, Mr. Meyers was Head of the Development Division in the Terminal Ballistics Laboratory. He then served as a division head in the Engineering Department and later in the Armaments Development Department before his retirement in December 1974.

The following interview with Mr. Meyers was conducted by Cynthia Rouse at the Dahlgren Laboratory on December 1, 1976.

Can you relate the events leading up to the Manhattan Project and Dahlgren's participation?

The Manhattan Project was undertaken by the government in the 1940's primarily to determine if fissionable material could be obtained to make an atomic bomb and if there was enough of this material available. The material under investigation was uranium, of which the predominate isotope is uranium 238. This is not a fissionable isotope. Actually, the chemical composition of natural uranium, as mined and refined, is approximately 99.3 percent U-238 and .7 percent essentially U-235, which is a fissionable isotope. The big problem, of course, is to separate the fissionable isotope and get enough of the enriched fissionable material to make atomic weapons.

However, this was only one of the aspects of the Manhattan Project. There were many others, but they were all divided into bits and pieces scattered here and there. Very few people had the overall picture of what was going on. They



Mr. Meyers was the Head of Dahlgren's Special Projects Division for test and evaluation of nuclear devices.

knew they were working on something that was highly classified, but they didn't have the overall perspective.

Dahlgren had several of the small pieces. I think Dr. Kemper mentioned determining aerodynamic properties of a shape which could be dropped from an aircraft.* At that time, I was working in the Light Armor Division, and we tested peculiar little pieces of elliptically shaped armor plate that we were told were highly classified and which I'm now sure were parts of the armor that went around an atomic device. We didn't know that then. We just knew we were doing something that was very urgent and that we couldn't talk about it. There may have been other pieces here and there at Dahlgren that I didn't know about.

I believe that later on, when they determined that atomic devices could be built, the first atomic weapon device was built under the Manhattan Project. I'm not too sure about that, but the device that evolved was carried out to Alamogordo, New Mexico, and set up on top of a 100-foot tower and tested.

*Dr. William A. Kemper, former physicist at the Dahlgren Laboratory, was interviewed, regarding the Dahlgren history, by Jack Brooks, Jr., at Dahlgren in January 1975.

This was called the Trinity Test, designed to determine if a weapon could be built from fissionable material. Once this was determined, Sandia Corporation was established under the Atomic Energy Commission in Albuquerque to take care of weaponizing the device. They had contracts with the University of California.

There were two primary laboratories built in the West then—one at Los Alamos, New Mexico, and one at Livermore, California. People were told when they went there that these facilities were functions of the University of California, but actually these were the laboratories run by people like Oppenheimer and Teller.* These were the real weapons-building laboratories. Several people from Dahlgren went to those laboratories. Dr. [Norris E.] Bradbury, who was for many years Technical Director at Los Alamos, was a Naval Reserve officer at Dahlgren. I think Dr. Bradbury left there soon after the first atomic device was tested in Alamogordo, and I believe he participated in building the two devices used in Japan. Admiral Ashworth,** who was a Commander when he was at Dahlgren, I believe was also the Chief Weaponeer on one of the drops in Japan. The reason these people were pulled out of Dahlgren was that Dahlgren was considered to be the Navy's primary gun laboratory, and the first atomic device used in Japan was a gun-type weapon. They needed somebody experienced in naval gunnery to help put this device together because it was essentially a gun.

There are several ways to build atomic weapons, and this is no secret. One is to physically separate, by a safe distance, two subcritical masses of fissionable material. These must be assembled quickly and held together, at least for a short period of time, as a critical mass that starts a certain fissionable process and the atomic reaction. One way to do this is to shoot one subcritical mass at the other—the gun against the target, so to speak, or a projectile against a target. Of course there have to be all sorts of associated safety devices, but this was essentially the first type of weapon.

Another way to create an atomic device is to take a critical mass and make a hollow sphere out of it. Because of the space it occupies from the hollow in the center, it interacts subcritically. Then this mass is assembled by being driven inward and is called an implosion device. Instead of explosion, where things go out, implosion makes things go in. A layer of explosive is placed on the outside of the hollow sphere. The major factor is to have a sufficient number of

*Dr. Edward Teller, called "the father of the hydrogen bomb," worked on the development of the atomic bomb at Los Alamos, New Mexico, with Dr. Robert J. Oppenheimer, the wartime director of the Los Alamos laboratories.

**Vice Admiral Frederick L. Ashworth served at Dahlgren in 1944 as Senior Aviator and was bombardier in the bomber that dropped the second atomic bomb used in warfare on Nagasaki, Japan.

detonation points and enough symmetry in the time that you create an explosive detonation wave driving toward the center which also has to be symmetrical. This drives the whole mass inward at the same moment, and everything arrives at the center. The assembly of this sphere in the center becomes a critical mass and is helped some by the pressure generated by the explosives. Then the whole critical mass starts a chain reaction which continues until it blows itself apart.

You mentioned people from Dahlgren going to other installations and working on the Manhattan Project. Were you one of these?

No. There were several times when people were called out of Dahlgren, and one was when the determination was made by the government to deploy atomic weapons. After the war was over, a large number of people from Dahlgren went to observe the atomic tests in the Pacific where arrays of naval ships were set up and atomic devices were exploded both under the water and above the ships. These people then determined the damage to the ships to see how they would survive. Also, Dahlgren people were sent to Japan and Germany after the war to take a look at what was left of their armament and analyze how their weapons and armor differed from ours. A fairly large number of people from Dahlgren went on those projects.

Admiral Ashworth was the bombardier for the second atomic bomb dropped on Japan. He was Senior Aviator at Dahlgren in 1944. Did he have anything to do with the Manhattan Project at that time?

He probably had something to do with the shapes that were dropped from the aircraft on our bombing ranges. He was Head of the Aviation Armament Department, and they took care of all this.

Can you describe the "Sewer Pipe" bomb addressed at Dahlgren?

As I said previously, one of the ways to achieve an atomic reaction is by assembling two subcritical masses in what is called a gun-type weapon. That was what people referred to as a "Sewer Pipe" weapon. I think only one has ever been detonated, and that was the first one in Japan. But the Navy—and I think the Navy is probably the only activity which has undertaken development of atomic weapons outside the Sandia Corporation and the Los Alamos and Livermore Laboratories—in the late 1940's and early 1950's undertook the design and development of two bombs. The first one was called the MARK 8 and the second was an improved version of the MARK 8 called the MARK 91. These were gun-type weapons, and they were designed to penetrate hard targets and detonate underground. Dahlgren became the primary test and

evaluation activity for the design and development of these weapons. This was done to simulate, with guns, launching of a bomb from an aircraft at 50,000 feet. Guns were modified so that they could accelerate these devices up to the terminal velocity that would be achieved by free-fall. The guns had to accelerate the devices but were not to exceed a certain G-load level because the bombs were not designed for high-G acceleration forces. Several guns were developed that had a multitude of charges. As the device would go down the barrel, these charges would be set off resulting in relatively slow acceleration but achieving the required terminal velocity. These guns were installed in the battery areas at Dahlgren.

This was after the war?

This was after World War II but during the development phase [Elsie Project] of atomic weapons.

They evolved from the Manhattan Project?

This was a follow-on to the Manhattan Project, but Manhattan, of course, had essentially been dissolved, and the AEC had taken over. However, Dahlgren was the primary test and evaluation facility for these weapons because of our guns, and we could set up big targets. Some of those old targets are still out in the test area—large concrete targets 30 or 40 feet thick. They were too expensive to dismantle after they had been built, so they're still out there. The old test butt used to catch the projectiles is also still out there. We had to fire all the projectiles inland because we couldn't afford to lose them out in the water—some of them did have natural uranium components. About the only differences between the final version of the weapon and the version we tested here was that we used either natural uranium or depleted uranium, primarily the U-238 isotope, whereas the weapon designed to function would use enriched elements. Also, we tested a few other little devices which are probably too classified to talk about. We did lose one device out in the water. It bounced out of the target area. We finally had a diving team come in to find it, and we retrieved it.

We brought all the recovered devices back into the Butler hut which is now the Dahlgren Mail Room [Building 492] and which was at that time a very secret type of facility. We had a big chain-link fence around it, and it was equipped with all sorts of security features and elaborate alarms so that unless you actually knew what you were doing when you went in, you'd trip an alarm. Immediately, marines from the local barracks would surround the place, and an investigation would be started to see what caused the alarm to go off. These were marines armed with machine guns—a spectacular operation at times.



Large concrete targets used in the Elsie Project at Dahlgren.



Side view of test butt used to catch Elsie projectiles.

Everybody was curious about the building, but you had to have a special clearance to enter.

Security must have been a big problem.

Security wasn't too much of a problem because people were pretty well impressed with the need for security. When we got the Q-clearance, which is what it was called, we were given a stern lecture about what would happen if we violated security. So far as I know, there was never a security violation of any type at Dahlgren during this time.

Was the Q-clearance used only for atomic weapons?

That's right. There were not too many people involved, but there were various types of clearances. We had numbers assigned. I've forgotten what mine was—N-144 or something like that. In the Navy, I think I was the 144th person cleared. This was out of the whole Navy.

The design and development were managed out of the old Bureau of Ordnance which had a special section set up in Washington. To walk into that place, you had to have a special clearance. There were all sorts of security devices up there, too. The group that worked in the Butler hut at Dahlgren had access to that place. There were probably not over 10 or 15 Dahlgren people who were cleared at that time. There were two junior naval officers—a fellow named McDowell and a young Lieutenant named McFadden. The people involved in the assembly, disassembly, and measurements were people from an instrumentation group headed by Nils Riffolt. We also had a photographer who was cleared, and we had a small group of technicians.

Out in the battery areas, anybody who was involved in loading guns or the recovery of the test device had to be cleared, but they only knew that they were working on something very secret. They knew that they were not supposed to discuss even the size, shape, or length and diameter of whatever it was they were working with.

Whenever we had certain types of nuclear materials here and whenever we were disassembling any type of weapon, we had special people called the "health people" who were assigned from Los Alamos. Their main function was



Butler building previously used for Elsie Project.

to keep a constant check to detect any possible radiation hazards. The uranium isotopes had to be handled very carefully, and there was only one incident that I recall where we had some of this stuff get loose. Then the function of the Los Alamos people was to clean up the mess to get rid of it, and they knew how to do it.

The one radiation spill was caused by a Los Alamos man who picked up the device that he had dropped. It had fractured and was emitting radiation when he got some on his hands. He had to go through a special cleansing process to get it off. I think it was U-239 that was involved. It's hard to deal with and is a very insidious type of material. If you let it loose in the middle of a table, it will spread across the table and down the legs, across the floor, and up to the ceiling. You have to stop and clean it up. There's a way to do that. Actually, we kept buckets of axle grease around and anybody suspecting a leak was to jam the material down in the grease, and the grease would hold it.

When we were getting ready for tests, we'd close up the area. We would actually condition the devices—temperature-condition them—and we'd have to get them up to the battery in a hurry. When we went up the road, we went pretty fast. I had a marine chase me all the way from the Butler hut to the test site one day, and he accused me of going 60 miles an hour. I told him it was faster than that. It was 65. It was as fast as the jeep would go. He said, "Well, I'm going to take you in." I said, "You're going to go right over there and go in that building and stand there and not make another peep." Of course I got away with it because the regulations said when tests were underway, the person in charge had absolute authority to do whatever he considered necessary, and speed was necessary.

Did the marines have Q-clearances?

The marines were not cleared. They could only stand outside the building with their guns. We had alert tests to see how quickly they could get there from the barracks, and they really got there fast. Generally, it would not be more than a minute from the time the alarm was tripped until they had guards all around the building.

Unless you had a special clearance, you were not even supposed to come inside the gate in front of the building. We had Captains here, Officers in Charge, who didn't have the clearance to come into the building, and there were other significant people who could not come into the building. One was the Fire Chief. He actually came to the door and knocked one day and said, "I'm coming into the building." We asked, "What are you coming into the building for?" He said, "I've got to inspect it." We said, "Chief, you can't inspect the building." He replied, "I'm the Fire Chief. I've got to know what's going on in here if this place catches on fire." He was told that if it caught on fire to watch it

burn and not let the fire spread. He was told that nothing would blow up, but that he was not to come inside the fence—just let the building burn.

The marines you mentioned were sent here from Guadalcanal.

I think so, but that had nothing to do with the Manhattan Project or even the Elsie Bomb aspect. Incidently, the MARK 8 and 91 were called Elsie Bombs. This was a code name. The marines sent here during World War II, when we had marines as our Security Patrol, were sent here for sort of a rest. As I recall during our program, the marines had been out on Guadalcanal during the fighting, and they'd been overexposed to gunfire and were battle-fatigued. In those days, marines manned the sentry posts at various locations around the Dahlgren Station 24 hours a day, and one of the sentry posts was across the street from Building 111, which was then called the Proof Office. The Main Battery was about 300 or 400 yards away, and we were doing lots of firing. Every time they'd hear the gunfire, some of those poor guys would jump into the ditch alongside the sentry box. They had been conditioned so that when they heard a gun go off, they'd try to take shelter. This was just a natural reaction. It was really cruel to put those poor people down here. I don't know how they stood it.

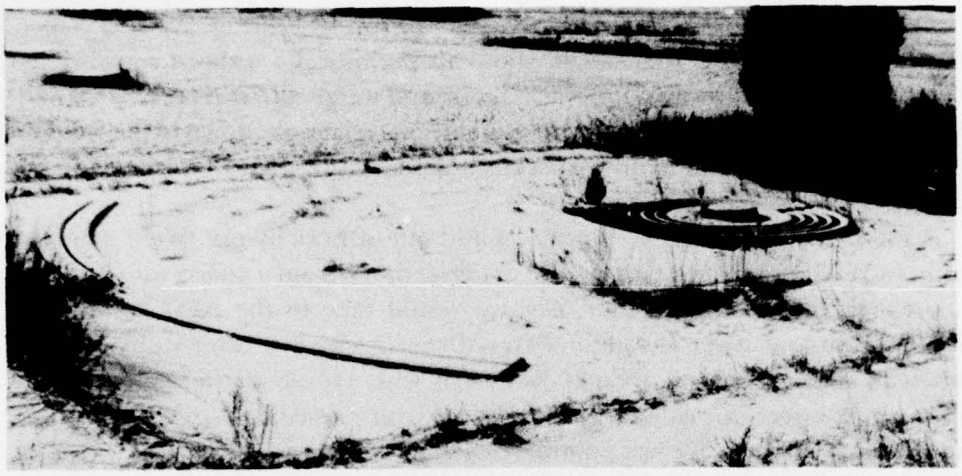
Were there any accidents involved? Did the marines ever shoot at anyone?

They were armed, and as I mentioned before, some of them had battle fatigue. When we went down there at night, as we did a lot in those days, we had to first get a key from a marine to get into the Proof Office and then get a key out of the safe that would let us into the Butler hut. The marines would hide in various places in the dark, and the usual procedure for us was to go down and honk the car horn and then get out and stand in the headlights. Pretty soon some guy would come out of the darkness with a .45 in his hand. Of course, he wasn't supposed to have his gun drawn, but that's what would happen. It would make you nervous.

There was one incident during a change of guards when one marine shot another. They had to go through some sort of procedure where a marine being relieved would hand over his gun—a .45 automatic—to the marine taking over. The marine being relieved was supposed to pull back the ejector to show that the gun wasn't loaded, but this guy had loaded the thing and inadvertently pulled the trigger. It was pointed right at the forehead of the marine relieving him and killed the man.

Were there any other significant incidents?

There was one. After the MARK 91 design was firmed up, the Navy decided that they had a lot of excess MARK 8 atomic bombs that they were going to get



Pivot mounting for the Elsie gun still intact.

rid of. At that time, we still visualized that we would be active as a design and development activity because we had many requests to consider other designs for atomic devices. When these MARK 8 bombs were declared surplus, they asked us if we wanted them, and we decided to take them. They had to be shipped across the country from Albuquerque on a special train which had marine guards. At that time, the Dahlgren railway was still active. When the train finally pulled into Dahlgren, there were probably a half-dozen passenger cars for the marines to live on and a couple of freight cars full of atomic bombs. They parked the train, and of course everyone in the area was curious about why the train was there with armed marines. The bombs were unloaded and put in the magazine area.

Did the marines know what they were guarding?

I think they did because the officer in charge of the marines, when they got here, was looking for somebody to turn the material over to, and I told him, "You can give it to me." He hardly believed me because he wanted to turn them over to some military type, not a civilian. I finally convinced him that he could be relieved of the responsibility.

I guess information about them is downgraded by now.

The weapons are no longer supersecret, I suppose. At that time, they were classified SECRET-RESTRICTED DATA. This is more secret than SECRET by quite a lot—sort of like TOP SECRET. Later, I think they were downgraded to SECRET. Anything with SECRET-RESTRICTED DATA was guarded very carefully.

Of course, we were held strictly accountable for everything. We had inspection teams from AEC that would show up periodically without warning, go through the books, go through the ledgers, and weigh all the materials we had. We had to account for any type of fissionable material right down to the fraction of a gram. They would really inspect us and make sure that we were maintaining all security clearances.

A lot of the material was shipped in and out of here by rail. We'd meet the train in Washington and pick up the material there from a guard who brought it through on the train. A lot of it we would take to the AEC building in Washington and make the changeover there. Some of it came in by air, and when an airplane landed, we'd know exactly where it was all the way across the country. We were informed when it took off from Los Alamos or Albuquerque and when it stopped. We were notified when it stopped in Chicago for refueling or takeoff. When it appeared in the sky here, we'd go meet it.

Uranium, you know, is very heavy material. We would get shipments of mostly natural uranium components, and those little boxes were 8 inches square and a foot high. We'd go to the plane and take some sailors along to help us unload the materials. We'd ask them to pick up that little box over there and set it in the truck. It was a small box. The guys would go over and grab hold of it, and they'd think it was nailed to the floor. Those little boxes would weigh 100 pounds.

You said everybody wanted to get into the Butler hut during the war. Did you ever have trouble with unauthorized people trying to get in?

There was a lot of curiosity. People wanted to know what was going on. Actually, the marines were supposed to keep a special eye on that, and they had floodlights all around. Every once in awhile, the marines would be tested by what they called the "Invasion Team." Not too many people knew things like this really went on here. Those teams were usually EOD people out of Indian Head. They'd come down the river, and nobody would know when this type of activity would take place, but they would come down the river by boat and land out on the shore. They would attempt to hang a handkerchief on the fence around the Butler building, and they succeeded several times—getting in and putting a handkerchief or flag up on the fence and getting back out without being detected. When this would happen, they'd notify somebody that they'd made an attempt or succeeded.

It's a wonder the marines didn't shoot them.

They would have shot at them had they seen them. That was part of the game. This was done a time or two just to check security. They were very fussy about security in those days, particularly regarding atomic weapons.

Who were the other people involved significantly in the project?

There was the Head of the Terminal Ballistics Laboratory, who was here most of the time during the Elsie Project and was a naval officer, Commander [Ben] Sarver, who later was Admiral Sarver before he retired. The first civilian division head in the beginning of the Special Projects Group was Don [L.] Winchell. He left here in, I think, 1953 and went out to Los Alamos. Then I took over. I was Plate Battery Division Head before he left, and then I was shifted from Plate Battery Division Head to Special Projects Division Head. As Plate Battery Division Head, I was in charge of all the tests on the atomic weaponry going on out in the battery areas.

We spent quite a lot of time in the Butler hut on the special projects. We finished up the work on the MARK 91 in 1956 when the testing and evaluation were finished and the design was finalized. After 1956, the atomic weapons work at Dahlgren came to an end, and as far as I know, the Navy has not worked on the design and development of atomic weapons since that time. That was sort of an unusual case. I'm sure the Navy has done bits and pieces, and, as Dr. Kemper said, a lot of the calculations for people working at Oak Ridge, Redstone, and Los Alamos were done here in our computing facility because we have one of the best capabilities in the nation to do this type of work. This continued, I'm sure, after 1956 until other facilities got their own big calculators and didn't have to come here.

That was really the time when the first major reorganization of Dahlgren took place and Dahlgren evolved from a naval proving ground concept into the naval laboratory concept. After 1956, after the work in the Special Projects Division tapered off, I was then assigned as Development Division Head in the Terminal Ballistics Laboratory.

How many hours a week did you work on the atomic projects?

There were long hours involved—about 60 hours a week. Among other things, we were trying, and were requested at the time, to consider new designs for these types of weapons—designs for devices that would not exactly penetrate deeply but would be capable of impacting a hard surface like an aircraft runway and not ricocheting off. They were to come to a skidding halt on the runway and lie there until they detonated. Of course when you drop an atomic weapon from an airplane, you've got to give the guy who dropped it an opportunity to get out of the area because of the large area involved and the things that result. These devices have delayed fuzes.

Admiral Parsons dropped the first one.

That one was retarded by a parachute. It dropped very slowly, and they had

calculated what the safe separation distance should be, but, as I understand it, they were right on the edge. The aircraft received a big dent in the side.

Some of those devices were as long as this room, 6 feet in diameter, and weighed an awful lot. They used one of the big bombers, and when they released that thing from the airplane, the wings would flap. The wings were tilted way up in the air and when the load was released, the wings would flap and the plane would jump about 10 feet.

CHAPTER IX

Community Relations

Harold M. Gouldman, Jr.

Mr. Gouldman is a lifelong resident of the local area surrounding the Dahlgren Laboratory. He is a 1940 graduate of Marshall Wythe Law School of William and Mary College and was the Legal Advisor for the Laboratory from 1949 until his retirement in December 1976. Consequently, Mr. Gouldman was in the mainstream of relations with local county and state officials, as well as dealing with other federal agencies on behalf of the Laboratory.

This interview was conducted by Cynthia Rouse in Mr. Gouldman's office at Dahlgren on July 27, 1976.

Can you relate your first encounter with Dahlgren and give some of the circumstances surrounding that event?

I grew up here. My father was the Civilian Personnel Director, and he came here in 1919. I've been living on or near the Station ever since then.

Where were you born?

I was born in Potomac Beach.

When did you come to work here?

My first job here was as the Navy Personnel Officer in 1945 while I was on active duty with the Navy.

What about your civilian career?

I started here as a civilian on March 1, 1949.

What did you do between 1945 and 1949?

I was called on active duty with the Navy in January 1942, and I stayed in the Navy until January 1947. After I was released from active duty on January 17,

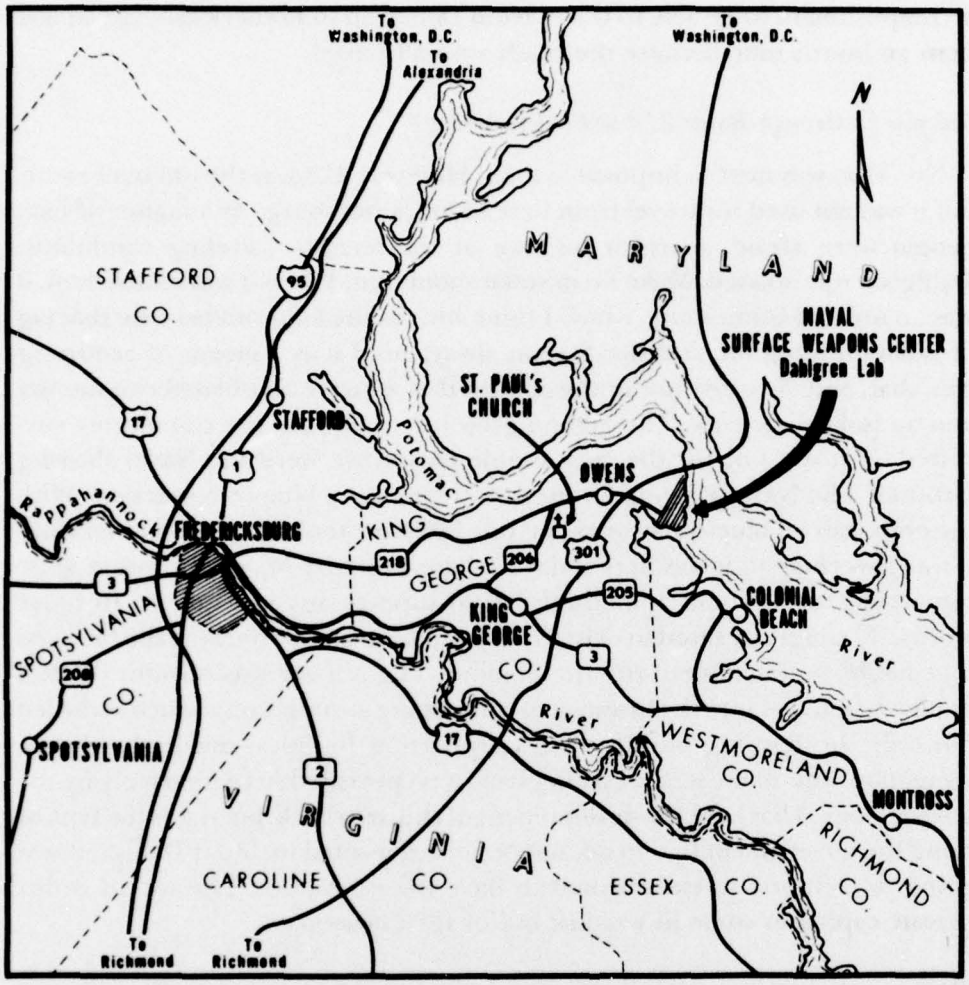


Mr. Harold M. Gouldman, Jr., was Legal Advisor for the Dahlgren Laboratory from March 1949 until his retirement in December 1976.

1947, I was appointed to what was then called Trial Justice of King George County on March 1, 1947, and I stayed there until March 1, 1949.

We'd like to get your viewpoints on the Dahlgren scene and its relations with the local and Northern Neck communities during your career.

As you well realize, I'm a native, and I've seen a tremendous change, not only here, but everywhere—particularly here. When I was growing up on Station, things certainly weren't as they are today insofar as the residential community life is concerned. We did have a golf course and tennis courts, but that was just about it. There was a baseball diamond; they didn't play softball then. We didn't have any highways. Highway 206 [to King George and Fredericksburg] wasn't built until 1932. Route 301 wasn't built and opened until 1940 when the Potomac River Bridge [to Maryland] was constructed. Of course, 301 had to be



Community surrounding the Dahlgren Laboratory.

constructed in Virginia to provide Virginia access to and from Maryland. The only route that we had in and out of here was through what today is still known as Route 614, which takes you up to B Gate and across 301 and comes on out at Owens [Virginia] and then snakes back and forth across present 206. Just beyond St. Paul's Church [in King George County], we have Peppermill Stream, and that was the "bug-a-boo" of travel, particularly in the winter months. It was impossible to get up or down when weather was bad. I recall the tales of my parents and others who were able to find an automobile that would go up Peppermill Hill in high gear. Of course, it's very difficult to envision that problem today, but that was quite an accomplishment then. It was also quite an

accomplishment to be able to travel from Dahlgren to Fredericksburg* in less than an hour's time because the roads weren't paved.

Did you go through Route 218 to Fredericksburg?

No. That was next to impossible then. Highway 218 was the old mail route, but it was not used for travel from here to Fredericksburg. As a matter of fact, people were afraid to use it because of the terrible traveling conditions. Dahlgren was isolated. Make no mistake about that. When it was established, it was an isolated community, which I think hurt in the long run because that tag of isolation hung on, and this Station always used it as a means of acquiring this, that, and the other under the context that we were an isolated community and an isolated activity. The Station grew internally, but the community surrounding it did not for the very simple reason we were the Naval Proving Ground. The Naval Proving Ground, you see, was a blue-collar station. With the emphasis on blue-collar work, private investors took the position that since it was government, it was here today and gone tomorrow, and if it were gone tomorrow, there would be nothing left to support any investment. In other words, if Dahlgren ceased to exist, what would remain to continue the business that might be established off the Station? The answer was obvious, so this Station had to get into the business of developing a community which included not only the housing but all of the supporting facilities—medical, schools, recreation, you name it. Everything had to be provided here internally by the government. That held the development of this area back, but it was the type of thing the government had to do, not because it wanted to, but if Dahlgren was going to continue to exist, it had to have these facilities. The incentive for private capital to come in was just out of the question.

What was the initial reaction from local citizens regarding the buildup of an ordnance facility in the area?

Quite negative, particularly during World War II. You see, King George County was a rural county and still is, but it was quite predominately rural back in 1918. There were no means of livelihood in the county other than farming. Also bear in mind that in the early 1920's, the automobile industry was just beginning to emerge, and so far as ownership of cars was concerned then, not too many people owned them because they simply didn't have the wherewithal to buy them. Jobs weren't available because there was nothing that produced the jobs. When Dahlgren came, it provided a steady payroll. As a blue-collar station, the payroll went to the local people because they were the ones who were coming in here as employees. They didn't have to rely on outsiders. The

*A distance of about 28 miles.

shift to hiring outside employees didn't come until we got into research. Then we had to have professional and technical people who simply were not available locally. That interrupted a pattern of life that King George had enjoyed for decades—I suppose really from its very beginning. That disruption was resented because local people found that hired hands and local help were becoming scarce. They resented the loss of that labor force. They also resented the fact that the Dahlgren employees got a substantial wage as wages went in those days, and there was a steady payroll. They resented the fact that a buildup of homes started in the county. I can remember time and time again riding through the county with different people and hearing "Dahlgren built this and Dahlgren built that." Of course, what they were referring to was the payroll and the people who were then able to become homeowners.

Then when World War II came, the labor force here expanded tremendously. That really broke the bubble. There was simply no local help available in King George County, basically, and it never returned after the war. Then, of course, things did take on a radical change. Tom Hunter, who was a local lawyer, was also the "Cavalier." He wrote a column in the *Richmond Times Dispatch* called "As It Appears to the Cavalier." It was a daily column, and during World War II, Tom had a special article on Dahlgren in which he said the conditions were so intolerable here that every time a new employee came through the main gate, one had to jump off the bridge into the creek to make room for him. He was an outspoken critic of Dahlgren from that standpoint, but it was much like the story in *Gone With the Wind* after the Civil War. This typifies the transformation that took place in King George County.

Another example is St. Paul's Church. St. Paul's Church is an Episcopal church, and I'm not saying this critically, but it is a fact that the Episcopal Church in its old days was known typically as a family church. This was quite true with respect to St. Paul's. There were old families, generation after generation, in this area of King George County who were members of St. Paul's Church. That was all the membership they ever had, and it wasn't until the 1930's when Dahlgren became fairly settled and then during World War II that new blood was fused into St. Paul's Church. The whole atmosphere and the membership of the church changed, and then the church started to show some progress because it was getting new money in considerable amounts. Today, it is doing quite well.

The Laboratory did have a big economic impact on the county.

There's no question about that. It still does. If this Laboratory were pulled out and no other industry took over, then King George County would be a blighted area. It would have no economy. Dahlgren is the economy. But the amazing thing is that it has become the economy not only of King George

County, but also of surrounding areas. Colonial Beach [Virginia] has always been the center of population for the Station. Now, admittedly, that happened primarily during the blue-collar race in the 1920's and 1930's, but we've had a number of professional employees who have migrated down to Colonial Beach in the past two decades and have become permanent residents. In the early days the Colonial Beach employees, because of the inadequate highway travel, commuted daily by a boat that was operated by an old boat captain named Captain Bruce. He used to run that boat back and forth every day, summer and winter, and brought a large number of employees up.

How long did it take to get here by boat?

I don't really remember. I know that my father used to come by boat when he was living in Potomac Beach before we moved up here. I don't remember this, but I remember him telling about it. One day when the Potomac River was frozen in the winter, he got halfway here when he heard the whistle blow for noon. So he decided that he was just wasting his time. He was just breaking ice trying to get up here, so he gave up, turned around, and went back. I think it took about half an hour or 45 minutes, depending upon the weather conditions.

You have been in the mainstream of legal actions surrounding Dahlgren for a number of years. Can you give us a general perspective of the types of problems you've encountered, the political personalities involved, and perhaps some of the unique situations you've witnessed?

The Station never had a Legal Officer, as such, until I was appointed as a civilian. When I was here as a military personnel officer during World War II, I also had the collateral duty of being a legal assistance officer, but that was strictly for military personnel and I had no impact on the civilian population. I came here when Admiral Joy was the Commanding Officer, and he was the first flag officer that we had in that position. He was followed by Admiral Kitts* and Admiral Duke.** Then Duke was succeeded by Captain Byrne,† and we have never had flag rank since.

From my own personal standpoint, I tried to operate on the basis that this Station was a part of the community. I never looked for any laws or regulations which we could use as a means of saying, "It doesn't apply to us because we are

*Rear Admiral Willard A. Kitts III was Commanding Officer at Dahlgren from September 1949 until June 1951 and was advanced to the rank of Vice Admiral in June 1951.

**Rear Admiral Irving T. Duke was Commanding Officer at Dahlgren from July 1951 until June 1952 and was advanced to the rank of Vice Admiral in 1957.

†Captain James F. Byrne, later designated Rear Admiral, was Commanding Officer at Dahlgren from June 1952 until June 1956.

federal," but I sought ways and means by which the federal government could make itself a part of the community by complying, as far as practicable, with the laws, philosophies, and policies of Virginia, and most particularly with the County of King George. As I mentioned before, there was a decided cleavage between the people of King George County and this Station. It was nothing but an outgrowth of jealousy. That's all it was. It was not until we got the schools merged and the dependents on this Station going to King George High School that we found a lessening of that feeling. It took a full generation to accomplish that. The kids see it from a different light. They grow up together associated in the school system, and then they become the future residents, and it was through that transformation that we saw the lessening of that divided feeling. You still see some evidence of it here and there, but I was personally acquainted with it. I grew up with it.

My idea, as I said, was to try to make Dahlgren a part of the working community as opposed to setting ourselves aside and saying, "We are the United States Government. We'll do it our way, and you do it your way." I tried to extend it not only to the county level, but also the state level by maintaining contacts with our state agencies and working with them and asking for their assistance on matters of mutual interest. I feel, and I always have felt, that if all of our federal activities could and would operate with that philosophy we would have less feeling of separateness between the state and federal governments, but they don't do that. One of the major reasons we were able to do this was that we were and are basically a civilian station; therefore, we are not typical. The typical military activity is dominated by the military personnel who are here today and gone tomorrow. Therefore, there is little relationship ever established between the military and the surrounding community, and that's understandable. However, I do not agree that it must be that way.

You mentioned the Dahlgren students going to King George High School. Who objected to this mostly? The school board?

There were no objections to the merger of the school systems as such. We have to go back to the 1920's, and I think that was about the date that the first school was established here on the Station. At that time, we were the Naval Proving Ground under the management authority of the Navy Bureau of Ordnance, which made funds available to this command for the operation of elementary and high schools. Where we are now was where the first school was conducted—in this building [Building 246]. One half of this building was devoted to the school and the other half for medical purposes—strange as it may seem. We provided education for grade one through high school. Then in 1922, the first Dahlgren school building was opened. That is today what is known as the Library and Chapel Annex, and it was during World War II that

the brick elementary school building that we have today was constructed. It was also during that same period that the present dispensary was constructed. The funding of the school continued through BuOrd, but as we approached the 1940's our high school population became too large on the Station and too costly. We didn't have the facilities to take care of the numbers. An arrangement was made whereby the high school was closed and the dependents [high school students] went to the school in King George County.

Then in 1951, Congress passed a special bill known as "Federal Aid to Impacted Areas." Those were the areas which had a significant number of children in their enrollments who were dependents of military and civilian personnel from activities in those areas. The philosophy was that military personnel, not being taxpayers in the area, were creating a burden on the state school systems, and this was the way the federal government was attempting to ease that burden—by making revenues available. In addition, it also required that all schools on the federal establishments be operated by the United States Office of Education, so that cut the funding off from BuOrd and put us under funding from the United States Office of Education. It was the intent that there be no local school operations within the continental U.S. on federal property and that the children there go to the local schools. The government was going to pay the local school systems for accepting.

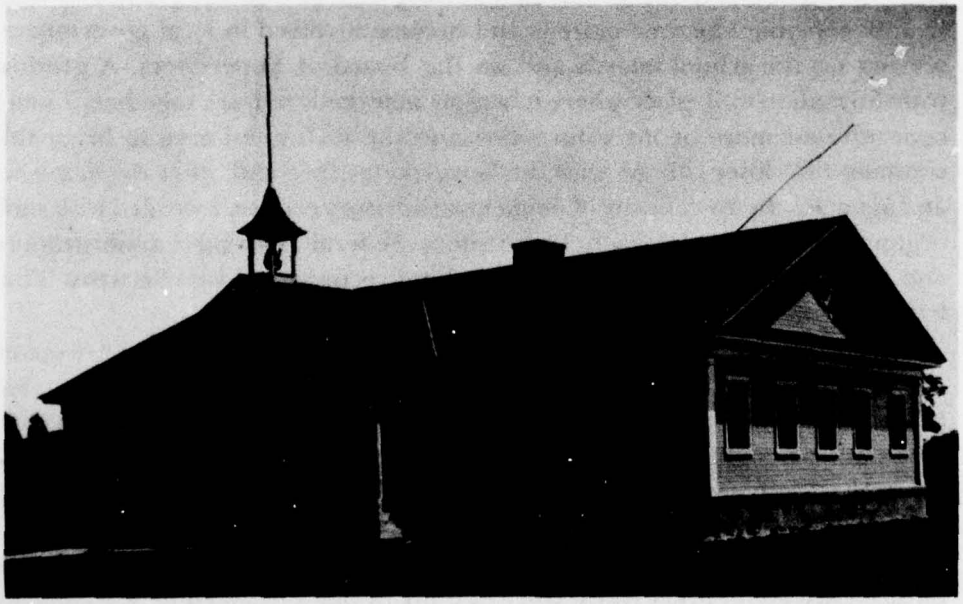


First private school and class at Dahlgren.

Now that caused a fight. The feeling here on the Station was that King George did not have a good school system, and Dahlgren people weren't about to send their children to such a system. Therefore, the Station was arguing for a high school and elementary school to be operated on Station, and the United States Office of Education was saying, "That's ridiculous," and the County of King George was sitting back saying nothing. The situation wound up in a compromise with continued operation of the elementary school as a federal school and the high school students going to King George with special funding under this bill. That created a problem for awhile, and that is understandable. Our children here on the Station had been led to believe that King George County had a bad school system and that we had a good system on Station. They didn't appreciate having to go out of here. As a result, they went up to King George with the "chip on the shoulder" attitude that they came from a superior school system. I suppose some of that is still carrying over today, but I don't think as significantly as it did then. Time changes things. This was the best break that, I think, ever came about in bringing the two communities together—the consolidation of this school system—and it took the kids to do it.

Did you go to the county schools?

No, I didn't. When I grew up here on the Station, we had a high school, but it was neither accredited nor certified, and therefore the students were not



First Dahlgren school building constructed in 1922.

considered eligible for admission to college. As a matter of fact, I think I was the first one to make this type of break. My folks sent me up to Arlington County, Virginia, where I lived with my aunt, my mother's sister, and went to Washington and Lee High School in Arlington County. In the following year, virtually everyone in my class went to another school. Some of them went to King George by moving out and living with somebody. Some went down to Montross and Warsaw. A very good friend of mine went with me and also lived with my aunt and went to Washington and Lee. We both graduated that year. That was the prevailing practice. Some went to high school in Washington, D.C. That continued until arrangements were made for students to go directly from this Station to King George High School without the necessity of moving out into the county.

Were there any other unique situations?

When I first came here, we had some expansion problems in programs and other areas that caused opposition officially in the area. I felt it was because the local people and the state people didn't know what we were doing and didn't have enough appreciation of what we were doing. We couldn't live in a little world of our own; we had to work with them. So I started a program to get first the Board of Supervisors of King George County acquainted with not only who we were, but also what we were doing. One thing led to another, and I attended the board meetings and served as a command representative on matters of mutual interest between this command and King George County. Then, eventually, our personnel became citizens and became involved in local government, serving on the school boards and on the Board of Supervisors. A gradual transformation took place where it became automatic to work together. Then I concentrated more of my efforts down in the Richmond area to bring this command in closer contact with the Governor's office and other departments and agencies. In my capacity of being in an advisory service, I worked with such organizations as the Highway Department, Federal Housing Administration, and Small Business Administration, and indirectly with Social Security. That led to others.

Then in the 1960's it became evident that something had to be done to create some housing developments in the county. That's when I got involved in what today is known as Bayberry Estates. At times I think I'd like to forget about it all. One tremendous mess is what it wound up to be, but now, of course, things look very promising and hopeful for the development of a nice community. But it was government-needed and it was government-backed. The government wasn't putting up any money, but the government was insuring loans. I think it induced just about every rascal you can think of that professed to be a contractor to come down here because they all had the idea that they were going to get

rich overnight out of this program. Then it got to the point where you couldn't believe anybody or anything they said, and that program floundered. It eventually did get on its feet, and, as I say, today it seems to be a growing development—not exactly as we hoped for and anticipated, but certainly I think it still has a great deal of potential.

Then other developments started up in the county. When we first got into it, we could not induce any local interest from the governmental standpoint of the county, and we had to work primarily with Richmond, basically with the federal housing officials. I think I got to know almost everybody in the Federal Housing Administration during that time.

Obtaining proper facilities at Dahlgren has always been a problem. Have you been involved in the various military construction dilemmas over the years?

I have been involved in some. We have a much more sophisticated and better qualified and better organized Public Works Department today than we've had here before. Public Works is well staffed to take care of these problems, but I go back to the days when we were trying to get the computer lab, and we had to have the program approved by the Department of Defense. For some reason or another when the appropriation bill came up in the House of Representatives, certain elements in there said we had too many computer facilities that were duplicating efforts throughout the nation, and they just wiped it out. It was reported—I'll never forget this—it happened that particular morning, it was Saturday, we were painting the interior of our house.

I was living in Potomac Beach. Because the painters were there, everything was a mess. My wife and I went down to the diner in Colonial Beach to eat breakfast, and I grabbed the paper. I was sitting there waiting for my breakfast when I looked and saw this article in the paper about our Laboratory having been knocked out of the appropriation bill. That was the first information that I had about it, so I immediately got on the phone and called Russ Lyddane. He didn't know anything about it. I asked him if he could get Captain Sellars.* He said, "Well, Captain Sellars is out on the golf course." I said, "Russ, I'll be back home in about 20 minutes." Russ went out on the golf course and found Captain Sellars, and just as I returned to the house, the phone rang. Captain Sellars was in his office, and to say he was upset is putting it mildly. He had not been informed of this action by anybody in Washington, and nobody knew about it until I told them. It was just a little tiny article buried in the *Richmond Times Dispatch*. So we went to work and that's why I contacted various officials and told them what had happened and what it meant, and to make a long story

*Captain Robert F. Sellars was Commanding Officer at Dahlgren from August 1961 until June 1964.

short, the appropriation was reinstated and was approved. We got the computer lab.

The restoration of that bill was primarily due to the efforts of two people in Congress—Willis Robertson, who was then our United States Senator, and Howard Smith, who was our Representative. Willis Robertson never once—and I saw the correspondence on this—asked anybody to do anything on this problem just because the Laboratory was in Virginia and his constituents were here. But he wrote to Senator John Stennis and said, "You and I were recently on a trip down to such and such a place in Florida and we were on such and such a submarine and it was there they were working on this POLARIS. The programming for this is at the Naval Weapons Laboratory at Dahlgren and that's the reason they need this facility, and you and I know what this means to our defense effort." That was the basis upon which they got approval. We have tried to keep certain sources informed of the technical aspects of programs for which we need specialized facilities as opposed to trying to exert any political influence or stressing what certain things mean to the economy of the local area. I think thus far we have been fairly successful.

Can you recall any specific individuals, either military or civilian, who you consider produced major effects on either the Laboratory or the community during their tours here?

I don't think there's any question about it. The first name that comes to my mind is Admiral Joy. Admiral Joy, I think, was one of the finest men, personally, that I've ever seen here as a Commanding Officer. I'm not derogating from others either, but he had a special talent. To me, his special talent was organization. He understood it, he knew it, and he practiced it. He was great for delegating authority. He got jobs done. He wasn't interested in the petty stuff unless he got a complaint about somebody not doing his job, and he'd call them in and say, "We got you in that job because you were supposed to be able to do it. If you can't, you say so, and I'll get somebody else in it. If you don't produce, I'll have you removed, and I'll have somebody else that will produce." And that was it. It was as simple as that. Then he was followed by Admiral Kitts. Admiral Kitts was a fighter up in Washington. Of course, he had his promotion to Admiral, so he wasn't afraid of going up there and stepping on somebody's toes.

Admiral Kitts was the one responsible for getting the Bank of Dahlgren established off the Station. Until that time, we had no banking facilities either on or off the Station except in King George or Colonial Beach. He went up there and told them he was going to have a bank. They wouldn't give him any help, and he came back and told us, "We'll do whatever is necessary within the law, but we're going to get a bank here," and he did. He got that group of people who were stockholders in the Bank of Westmoreland to create an independent bank off the Station and built what is today the Bank of Dahlgren. As

soon as they got that accomplished—it opened while Admiral Duke was here—then the Station was able to convert its payroll to check as opposed to cash.

People were paid in cash?

People were paid in cash unless they requested a check, but most people wanted cash. That's the way the Station had operated all these years. Again, change. People oppose change as a basic rule. We couldn't convert to check because we didn't have the check cashing facilities. We could only do it for those who made a special request for it. As soon as we got the facilities available, we were able to convert to check.

I also, personally, hold a high esteem for the administration of Captain Sellars and Ball* as a team. They had an outstanding relationship with the employees. They were down to earth. They were wholesome, and they could be serious, and they could sit down and talk with you on any subject. They could be humorous. I found from the working relationship that, as a team, they probably stood out. I can't speak from the technical standpoint because I'm not involved and never had been involved in the technical operations of this command. There are probably technical personnel who would take exception to that, and that would be understandable, but I'm looking at it strictly from just the day to day routine of working with them as the Commanding Officer and the Deputy Commanding Officer.

Your entire career has been at Dahlgren. Do you have any regrets about not moving elsewhere, perhaps for better job opportunities?

No. I think any of us could leave this area if we wanted to and get higher grades and higher pay, but to me it isn't worth it. I realize I'm obviously biased in my evaluation of the area, having grown up here, but I wouldn't trade living in this area of rural Virginia for all the tea in China. I went to school up in Washington. When I went to school in Arlington County, it was rural, believe it or not—rural enough so that I could ride a bicycle 3 miles to get to school without any fear of heavy traffic, but you wouldn't dare undertake such a thing today. I used to ride a bicycle in the District of Columbia, on any occasion, without any fear whatsoever. The traffic, we might say, was heavy for those times, but it wasn't fast moving traffic, and it was no problem at all to get around via bicycle. I spent a lot of time over at the Capitol, over at the Library of Congress, in the House Office Building, and the Senate Office Building just browsing around, meeting people, talking with them. Insofar as living up there now or in any other metropolitan area, I don't want any part of it. I feel that this

*Captain George G. Ball was Deputy Commander during the tenure of Captain Sellars and was Commanding Officer at Dahlgren from July 1964 until September 1964.

Laboratory is unique in the opportunities that it can offer to young people in the way of career employment and providing them the luxury of living in this area. I don't think I have to emphasize that point because you can go anywhere else and tell people what you do and where you live, and there's an immediate envy that crops up. They would like to have such an opportunity. It took the local school authorities, I will say this without question, some time to recognize the fact that the potential in the technical and professional fields was here to stay at this activity and that their educational processes were going to have to be geared to them. I'm not suggesting that they've accomplished it yet, but certainly it's a far cry today from what it used to be.

CHAPTER X

Development of Computer Technology

Ralph A. Niemann

Mr. Niemann obtained an AB in mathematics from DePauw University in 1941 and received an MA in mathematics from the University of Illinois in 1942. He came to Dahlgren from Harvard University in 1947 and served as Head of the Warfare Analysis Department from 1955 until 1970. From 1970 through 1972, he was Assistant Technical Director of the Naval Weapons Laboratory and then returned as Head of the Warfare Analysis Department where he is presently employed.

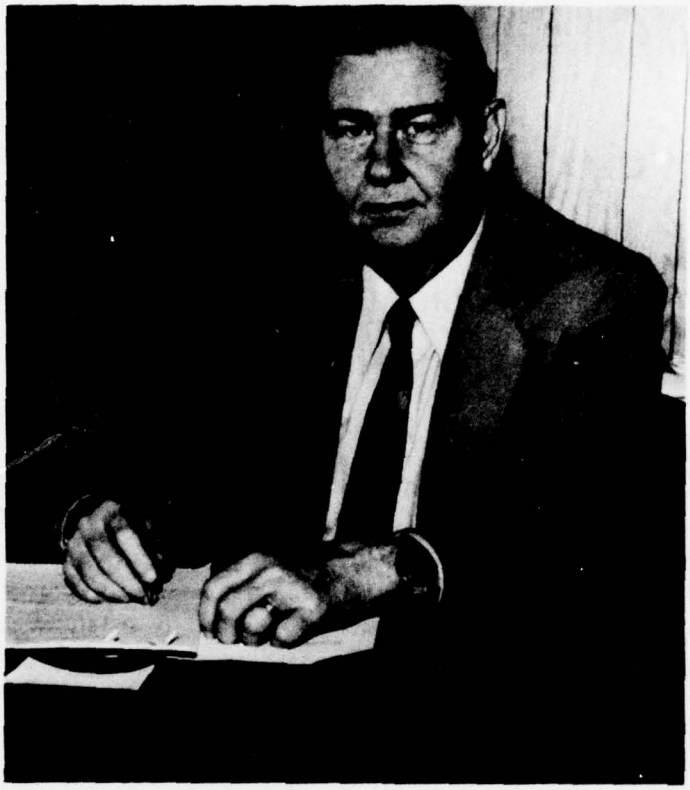
The following interview was conducted by Cynthia Rouse in Mr. Niemann's office at Dahlgren on October 26, 1976.

Were you here when the first computers arrived at Dahlgren?

Actually, I came here with the first computer—the first large-scale computer. There were so-called computers here when I arrived, but they were IBM card-feed devices. They weren't the same type as modern computers. I had been at Harvard and worked on what was called the MARK II Computer, which was later changed to the Aiken Relay Calculator. Then I was on the staff there and came down here with the computer, along with the people who built it and a few others who were hired to do programming.

What were the first computers put into operation here and what was their purpose?

We had a couple of smaller card-feed computers which were used mainly for producing ballistic tables for the Navy—bombing tables, rocket tables, projectile tables, and that kind of thing. This was sort of a center for producing that data for the Fleet, and it still is. That's what prompted the idea that with modern technology advancing, computers could be built. That's what prompted the Navy to go to Harvard and try to get a computer built to do so-called manual



Mr. Ralph A. Niemann served as Head of the Warfare Analysis Department from 1955 until 1970 and from 1972 to the present.

labor, you might say—the things you would normally do with a desk calculator. I think during World War II there were about 60 Waves here just using desk calculators, so with the advent of computers, a tremendous step forward was made in being able to do a lot more in a shorter period of time.

Was it difficult to get operators and proper service at that time?

Well, we had the fortunate situation in that the operators that helped build the computer at Harvard—I guess at least 90 percent of them—rather understood that when they got the job up there that they would transfer to Dahlgren when the computer was completed. So they actually came with the computer, and the technical expertise needed came right with it. We didn't have any problems initially with getting operators.



MARK II Aiken Relay Calculator used at Dahlgren in the 1940's.

How many people came down?

I guess there must have been about 15 in total. Right now, there are only two of us left—myself and Bill Burke.* Bill Burke works on the CDC 6700 and is the Head of our Operations Group.

Where were the original computers housed?

There was a T-shaped addition built on the back of Building 218 which is next to the Main Range. That addition was specifically built for housing the two computers in the plan. One was the first one I came with, the Aiken Relay Calculator, which was housed on the second floor of 218, and then later in 1949 or 1950, the MARK III Aiken Dahlgren Electronic Calculator was sent here.

*William G. Burke is Head of the Computer Operations Branch in the Warfare Analysis Department.

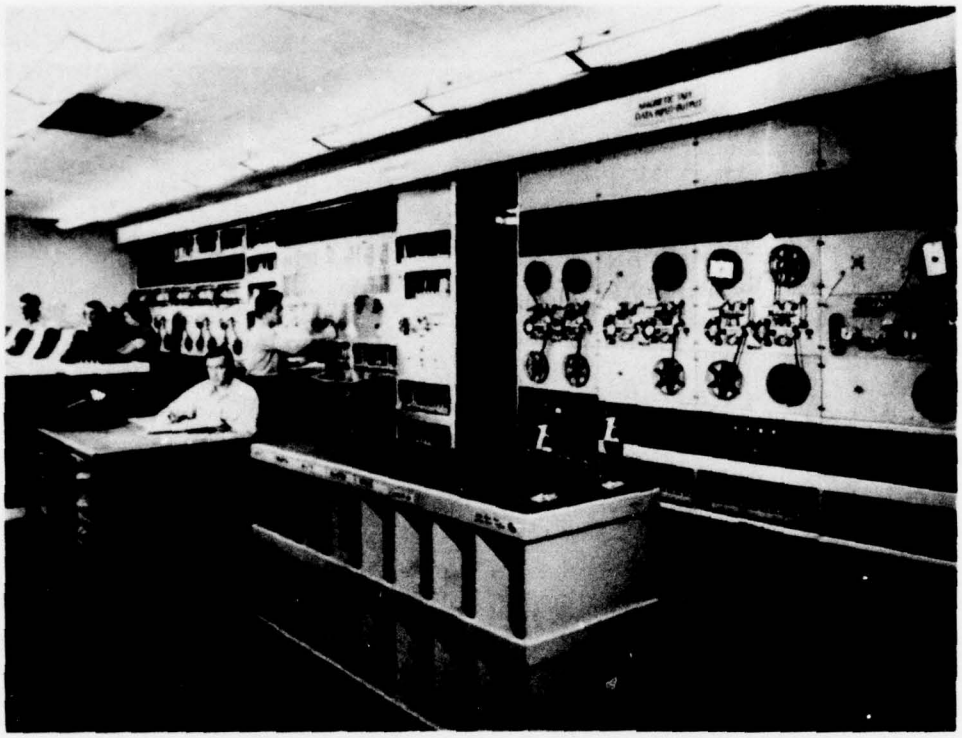
Can you describe the evolution of the computer at Dahlgren from those early days to the present and the changing work environment that prompted the evolution?

Well, I think initially the two computers at Harvard were, you might say, on the frontier of the state of the art at that time. They were sort of experimental computers, and at that point, you couldn't rent big computers from any commercial manufacturer—IBM or anybody else. They just didn't have them for rent, so whenever the Navy needed a computer, they had to get money appropriated and buy the computer. The basic need for the computer is obvious. Of course, once you get a tool like that, then you see other things you can do with it.

As time went on we did bigger jobs with computers and that required more computers. In the mid-1950's, UNIVAC and IBM also started renting big computers and that changed the whole nature of the computer industry, as well as the Laboratory, in relation to computers. Originally, the Bureau of Ordnance had the concept that there would be one computer center in the Navy because they couldn't afford more than that, or so they thought. It would be some big computer complex, and all the other Navy activities that needed scientific calculations would have to come to that central place to get them done. This held up for a while because nobody else could rent computers, and industry couldn't get the Navy to buy any more than those that were at Dahlgren, so when IBM started renting computers, large-scale computers, then other labs also started lobbying for computers. The key thing was they could pay for computers on rental out of project money. They didn't have to get appropriations from Congress as long as they had enough project work. Whenever they used the computer, they charged the project, and that money went toward paying the rental for the computer. That caused a big expansion, both in the Navy and in the government in general, of the use of big computers because they could be rented from commercial organizations.

So now that we had these big computers, we were looking for work in the Navy that required that kind of computation capability. It wasn't hard to find because there was one key thing. The problems were already here, and the engineers and mathematicians were looking for ways to solve them faster and cheaper. If you got a computer, tomorrow you could put a problem on it and try to solve it. In the business world, it was a little different. Once a business had a system working, the manual system working, if they wanted to install a computer, they had to keep two systems going for a while before they could switch from one to the other. For that reason, I think the biggest use of computers was in the scientific area, initially. Of course, they were sponsored by the Defense Department.

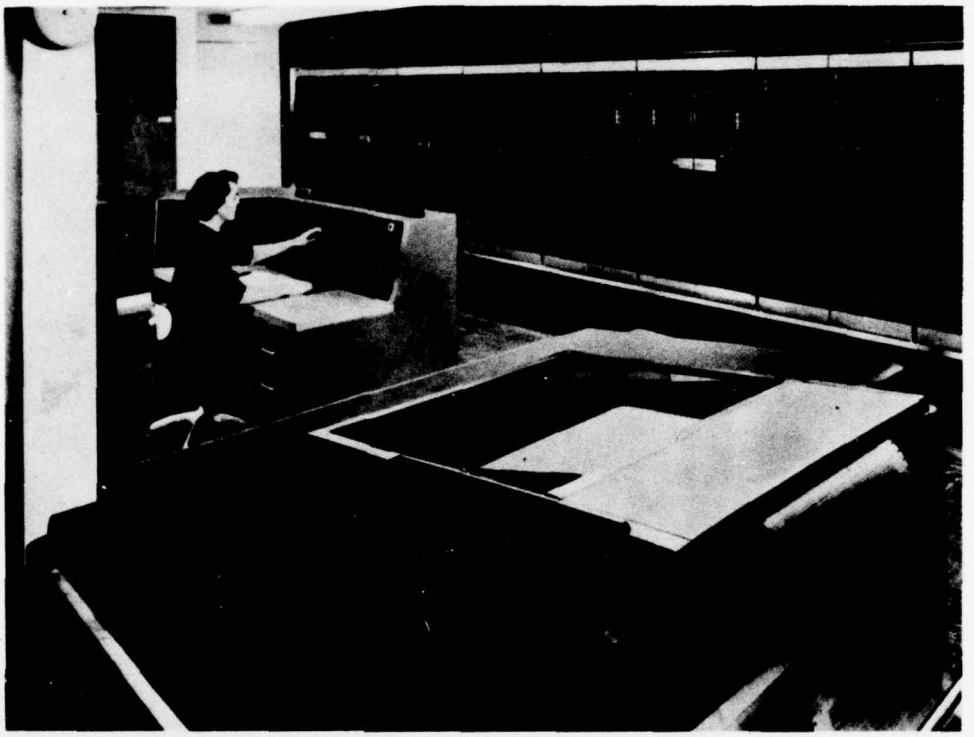
As we moved from around 1950 on, commercial computers became available. The question was still raised as to whether commercial computers were right for the Navy's computing center; however, there were several of us who



MARK III Aiken Dahlgren Electronic Calculator.

felt that technology was moving along rapidly, and we felt that in order to get the best computer for our needs the Navy had to sponsor development. Around 1958, there was a committee put together to look at Navy needs, and they surveyed a lot of installations. They came to the conclusion that we should talk to some computer manufacturers like IBM, UNIVAC, and maybe Burroughs. They came to the conclusion that the way to get the best computer was to sponsor a development, and they chose IBM. A contract was made with IBM to build a computer to meet the state of the art for one dollar plus cost, so the Navy paid IBM one dollar plus the cost of building the computer which turned out to be the NORC, delivered here in 1955.

That was a significant step forward in speeds of computers. Back at that time, NORC was the fastest computer in the world. It was built at Watson Lab in New York, and they did some things with the NORC that were way beyond what was considered the state of the art. For instance, magnetic tape is one of the storage devices on computers. At that time, most of the commercial organizations were using 100 bits per inch on tape. On the NORC, IBM went to 500 bits per inch. Even the engineers at IBM in Poughkeepsie and other parts of the company



Naval Ordnance Research Calculator (NORC) delivered to Dahlgren in 1955.

didn't think it would really work, but the people at Watson Lab who were sponsored by IBM and some different engineers who hadn't built the other computers thought it would work, so they put it on the NORC, and that was one of the significant improvements in computer technology at that point. When we got the NORC, it was about 100 times faster than anything we had before, so that opened areas of problems that would have been too costly or too time-consuming on smaller computers. We started looking at areas like war gaming. We got involved in simulations of amphibious forces and things like that which required quite a bit of computer capability and a lot of analysis and time.

About 1955, Sputnik went up, and the United States was quite embarrassed that they were behind in the Space Program. We put forth a lot of effort to try to do something about that. The Naval Research Laboratory [NRL] launched their own satellite called Vanguard, which was about as big as a grapefruit, but it was successful. Dahlgren was involved in that, some of the analysis of the orbit trajectories and so on. That led eventually to the Naval Space Surveillance System, which is now a separate command on the Dahlgren reservation but was then part of this department. We got it started with NRL and did the computation on the NORC because it was a capable, fast machine.

As time went on, we actually rented two IBM 7090's for the Space Surveillance System. They were still on Station until recently over near the mess hall.

Then came along, in late 1959, the POLARIS Ballistic Missile Program for the Navy, the Fleet Ballistic Missile Program. Having had experience in exterior ballistics, we had some expertise to go into that program, which required a lot of computation. So the NORC was used for the Space Program, the POLARIS Program, and war games.

As we moved into the 1960's, there was a new program being developed which is now the POSEIDON, and the computing required there was, I guess, an order of magnitude over what we required on the POLARIS Program because it was a more complex projectile and eventually had multiple reentry vehicle warheads on it which necessitated a lot more computation and faster computers.

About that time, we got the IBM STRETCH computer. They built about eight copies of it; we had one, the Weather Bureau had one, and I've forgotten where the others were. We had one here for about 10 years before we got our CDC 6700. About two-thirds of the work load on those computers was for the Fleet Ballistic Missile Program, and then we kept getting further involved in the Transit Navigation Satellite Program. Those two programs essentially used up about 75 percent of our computing time. As time went on, those kept drawing more and more. Today, we have the TRIDENT Program, which is a follow-on to POSEIDON, and we have a larger responsibility in the Space Program—the satellite geodesy. Again, today, that comprises about 75 percent of our computing requirements. The other 25 percent is for exterior ballistics tables for rockets, mines, and bombs. And then we do a small amount of work for the other departments at Dahlgren.

Besides yourself, and you mentioned Bill Burke, who were some of the key people involved in building up the computation efforts at Dahlgren?

I think there's one more key person—Dr. Cohen,* who is still here. I can't say enough about Dr. Cohen's important contributions. Other key contributors were Bob Ryland, now in the Electronics Systems Department, Gene Gleissner, now Head of the Applied Mathematics Department at the David Taylor R&D Laboratory, and John Walker, who recently retired. Dr. Bramble was the key person, originally. He interviewed and hired me at Harvard in 1946 or 1947. Anyway, he was working for Dahlgren at that time. Before that, he was teaching at the Naval Academy Postgraduate School in Annapolis, and they persuaded him to come here and take over this department. At that time, they were just

*Dr. Charles J. Cohen came to Dahlgren in 1944 as a Lieutenant (jg) with the Navy. He returned as a civilian mathematician in 1947 and is presently Associate Director for Research in the Warfare Analysis Department.

doing exterior ballistics computation. Dr. Bramble came, I think, during the war on a part-time basis and later took a full-time job. He represented the original push behind the computation center. I think they promised him a computation center here in order to get him to come here. He didn't particularly want to leave Annapolis, but the Bureau of Ordnance was doing a lot of computation work by hand in Washington. They needed a computer facility at some place like Dahlgren, and they persuaded Dr. Bramble to head up the department. Dr. Cohen was here during the war, too, in the Navy.

There's an interesting historical note that Bill Burke was involved in at Harvard. They had trouble one night troubleshooting the one computer, so they looked around and finally found there was a real bug in a relay. They took the bug and pasted it on a piece of paper and said they "debugged" the machine. The interesting thing is that everybody in the computer industry uses that term now when they are checking out a computer program; they call it "debugging" the computer program. Bill has still got the sheet of paper with the bug on it. He's still got it down there in his office.

Was there any resistance to establishing a major computation center here?

Yes, there was a lot of resistance. In fact, there was a major decision around 1949 between us and what used to be the Naval Ordance Laboratory at White Oak. After we got the first two Harvard computers, there was a group at White Oak who claimed that they had large requirements for computers to do hydrodynamic calculations, and so forth. Independently, they made an extensive effort to try and get the big computer at White Oak. That was finally resolved in sort of a verbal agreement with Dr. Bramble.

During the war, Dahlgren constructed a building called the Spark Range, which was a long, narrow building. It's not here anymore, but it looked something like a tube. There was a similar facility at White Oak. So when we got into the argument about who was going to be the computer center, the Bureau of Ordnance decided that they would close down the Spark Range here and let White Oak have that responsibility. They put the computers here, and White Oak wasn't to get the big computers. However, White Oak continually met with commercial concerns to see what new things were coming along that would be better than the computers we had here. When they discovered IBM was considering electrostatic storage, they wrote a letter to the Bureau of Ordnance saying they'd like to enter into a contract with IBM to build a computer with this technology. So the Bureau, instead of agreeing to that, appointed a committee made up of people from White Oak, Dr. Bramble, and several other people who went around surveying industry, and that's what brought out the NORC computer, which got delivered here in 1955. There was a continuous tug of war between White Oak and Dahlgren on whether or not they were going to get computers.

Acquisition of military construction is never very easy. What were some of the major problems in getting construction approved for your present facility?

You're talking about this building [Building 1200].

Right.

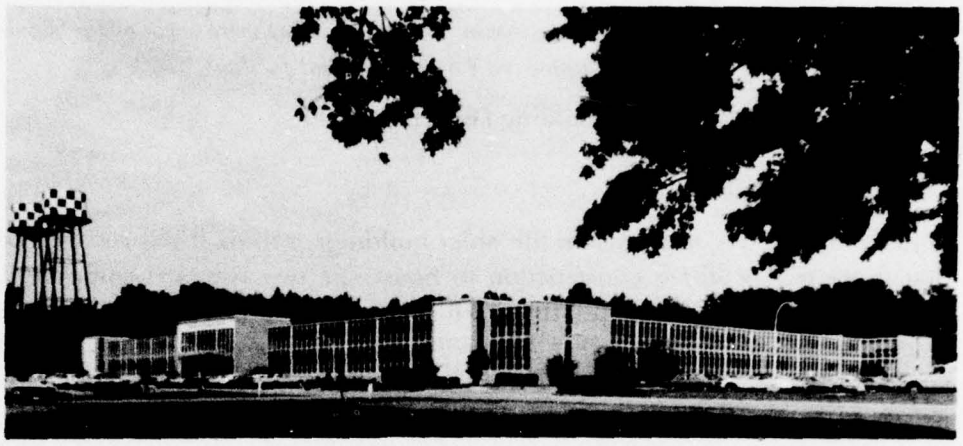
Just a couple of comments on the older buildings. I think it was recognized that there was need for construction to house the two Harvard computers. There was no other place to put them. The Laboratory didn't have any suitable place, and the computers needed air conditioning. In fact, at that time, the only way you could get air conditioning was for equipment—it couldn't be done for people. People would walk around sometimes sweltering, wearing shorts, perspiring. Every now and then, we would go back to the computer area and cool off.

In the case of this building, the problem we had was that all during the late 1940's, and in fact almost every year I've been here except for the last 10 or so, there were pressures or indirect threats about closing Dahlgren. I heard it when I first came here, and it just kept persisting. Around the end of 1959, it was almost a reality, and everything here was in bad shape except for the computation and analysis facility. We had new modern programs building up. We had computers and things that were on the upswing. The rest of the Station had gun and ammunition programs and a lot of miscellaneous things. It was after the Korean War. Some of the Advisory Committee that knew the situation said, "Dahlgren is out in the sticks. You can't get any professional people to work there. After all, the educational system is no good. They're not close to universities. Who would want to work at Dahlgren?" That was the general attitude, and some inspections were made here and recommendations were generated to close Dahlgren. That stayed in the chain for a long time. It was three or four years, almost 1964, before we finally got the building through, and some of those things were still bouncing around in the Navy Department. Every time McNamara* or somebody decided to look at base closings, Dahlgren was one of those mentioned. That finally stopped about 1964.

We moved into this building in 1964, and the reason we were able to acquire it was that, when Captain Simons** was here around the end of the 1950's we had the Public Works Department to help. Our department was expanding, and a few other things were expanding about that time. We needed office space. Most of the buildings constructed previously were very special-purpose things on the range and on the waterfront. They weren't good office buildings. For air conditioning, we had to go through several inches of concrete. Lighting wasn't

*Robert S. McNamara was Secretary of Defense from 1961 until 1968.

**Rear Admiral Manley H. Simons, Jr., was Commanding Officer at Dahlgren from October 1957 until August 1959.



Present computational facility at the Dahlgren Laboratory.

good, and we needed new floors—all kinds of things. So when the Public Works Officer added up the costs of all the requests he had for renovation, it came to about a half million or maybe $800,000. He said, "You know, it would be better if we built a new building than it would be to keep renovating those old buildings." So that kind of sparked things, and Captain Simons, being somewhat of a salesman, said, "With these space programs building up now, we ought to go out and try to get a new building." We had the POLARIS Program at that time, and we had the Space Surveillance Program. We were doing some work on the Transit Navigation Satellite System, which APL* developed. Those three things were right in the forefront at that time. They were modern programs that were expanding with a lot of glamour attached to them. We used that as backing, and we went in with the request that we needed a building like this one for housing the computers and people. It would be more efficient. We kept pushing it, and we finally sold it.

Once the building was constructed, then the issue about closing Dahlgren sort of went away because when people would come down, they'd see a new building. They'd figure things were going good, and maybe Dahlgren shouldn't be closed. I think, also, the Navy began to realize that maybe the real estate here was the best they had on the East Coast. Land was getting scarce, and there wasn't too much interest in going out and buying new land. The government already had a lot of land, and many people were saying they were taking on too much land. So I think all of these things kind of came together at one time, and we were just lucky to get the building when we did. Of course, we've

*Applied Physics Laboratory, Johns Hopkins University.

added on to it several times since. We've added on to the computer area about three times, and we've got a request in now to add some more on the back. We were told we couldn't build on, but we kept doing it. I think it's like when you build a house. If you can't afford to landscape it completely, you have to plan. If you go ahead and landscape it completely and have to keep digging stuff up and moving it around, then that's a problem. But we had a plan, and every time we incremented, it fitted into the overall picture. That's the reason we were successful in doing it.

What have been some of the significant management problems you have faced, and how have you dealt with them? For example, training programs, personnel acquisition, etc.

There were several. I guess the first one was when we moved the people down here from Harvard. They were on the Harvard payroll. It was a contract with the Navy, so they weren't civil servants. Once they moved here, they got switched over to civil service. In the civil service standards, there wasn't anything about computer work at that time. What they tried to do was put the people into positions that they thought were similar, and they called them laboratory technicians. The thing that bothered us and the people themselves was that they'd only been working for the government a few months. If there was a reduction in force, they'd have to compete with lab technicians in shops somewhere else. If they got bumped out, we'd end up with old lab technicians who didn't know anything about computers. So we made an effort—I did personally and several people helped me—to try and get the Civil Service Commission to establish a job series for computer engineers and computer technicians. That took a long time, and we hit some bumpy roads along the way. In fact, one time they actually ordered us to change all the computer people over to per diem. I called the senior civilian in the Bureau of Ordnance, Mr. Werthheimer, and told him it was going to cause a lot of problems if they did that, and he contacted the AWCO, which was the Area Wage and Classification Office promoting the change, and got it stopped. That was a significant management problem we finally solved, but it took an awful lot of time, and there were many people who weren't willing to listen.

I think the thing that helped eventually was that when everybody started using computers, then there was a lot of pressure in Washington to do something about the job standards. As long as we were the only ones complaining, they didn't pay much attention. In fact, some of the personnel people in the Navy, the old-line personnel people, have a "shipyard attitude." I've always felt the titles they used on the ungraded employees were pretty bad, like leadingman, snapper, and so forth. I asked one of the old Navy personnel men one time, "How would you like to see your kid on television and have somebody ask him what his dad did, and he would say, 'He's a snapper'?" That really made

him mad because those were traditional titles that craftsmen had some feelings for. To us and some other people, they didn't mean a thing. I think we did get the classification problem straightened out to where people finally became known for what they were doing and not for some side issue that they just did not fit into.

In programming, there were two or three of us hired. Two of us were hired at Harvard, and a couple at Dahlgren. There wasn't such a big problem in the beginning because the computers were pretty slow, relatively speaking, and we could keep up with them, but as the staff expanded, we had to figure out how to get programmers from colleges because at this point, no one was teaching programming. None of the colleges had any computers, so we made a decision to hire mathematicians and train them to do the programming. I think that has been a blessing to us in two ways. The mathematicians are flexible enough now so that they can move to other jobs. We could have hired people who were not mathematicians and tried to teach them to program. In some business applications, we did that, but by hiring professional mathematicians we were able to train them on the computers. They also had a career ahead of them, and they could move to other kinds of work such as in weapons systems.

At that time, we didn't have any educational program here. We didn't have college courses being offered on Station, but the professional people that we had felt a definite need to stay up to date with what was going on, technologically. I remember even organizing groups of people that would take certain textbooks and each person would read a chapter and give something like a book review. We'd meet after work and talk to each other and explain what was in that particular chapter. That's the way we tried to stay up to date. Now, we have a fine educational program, and we don't have that problem.

I guess one of the management problems that we haven't solved completely is that computers are used for all sorts of programs, but there is no single office in Washington we can go and talk with about getting money to do R&D in the programming area. The money that keeps computers going comes from various projects. The project people want work done, but they don't want to sponsor any research on computers or programming languages. There is a lot of efficiency to be gained by improving our methods, but you have to have money to do that. We did work it out to some extent because in our computer hourly rate we have a surcharge, sort of an overhead, that we use to pay people to do some of these things which benefit everyone. This type of work takes place in our Systems Group, DK-70. They do things to improve programming systems and languages and determine the most efficient systems or methods. We still don't have a central office in Washington to support that kind of programming, but we are seeing an organization get started now called the Computer Science Technology Program, and I think that NAVELEX will eventually have that responsibility. So we see some things changing.

Where do we go from here in computer sciences at Dahlgren? What do you see as our role in future Navy R&D?

Well, interestingly enough, there's a lot of good things coming along in computers. We always seem to think that the latest computers are the last major construction in the field, but now we see computers in which the whole computer processor is as big as the end of your little finger with these large-scale integrated circuits. They are called microprocessors. The industry has done a good job in dropping the price down. A few years ago, a microprocessor cost $200. Now you can buy some of them for $20. You see the results of some of these things in pocket calculators, wristwatches, and things like that. The same thing could be, we think, applied to Navy computers in the Fleet to make them much smaller, much more reliable, and much more flexible.

I think the next big push will be to take the modern technology and put it in the Fleet. The Fleet, right now, is using computers designed in the mid-1960's or maybe early 1960's. It hasn't gotten the modern technology into the computer. That's the danger in standardizing. If you standardize on some computer, it stays in the Fleet forever. When new things come along, you can't get them in because they are not standard. I think there's a recognition now that we are at the point where we should start applying the new technology in the Fleet system.

It's hard to tell, in general, how things will be. I think we are still going to have big computers, but there's another mass of smaller computers that people are using called minicomputers. They're cheaper and, in fact, a minicomputer about as big as a desk can do as much as some of those like the early MARK II computer that took up several rooms. That's how much they have miniaturized everything. Part of it has been done through the Space Program and part of it with research in semiconductors. There are predictions that everybody will have his own computer, one in every house. The truth is probably somewhere in between that extreme and the extreme that you would only have a central computer.

We have also seen, since 1970 at least, the third-generation computers. In those, you have the ability to interrupt, electrically, what's going on and do something else. This is called time-sharing. You can have a computer, and we have this feature on the 6700 here, which has a console somewhere in another building, and you dial over the telephone line and you get in the computer, do some work, and get your answers back. That's a significant change over the early computers. We can dial a computer in California that we have arrangements with and can actually use their computer with our programs to do a certain amount of work. Around here, we have about 20 remote consoles in different parts of the Station, and the 6700 is downstairs. We also have people at Yorktown using the 6700 some, as well as some contractors who work for the government dialing in here and using the computer. Then we have a small

contractor who has a console in her house. She dials in here to use the computer. So that's the big difference in the use of computers.

Getting a little more technical, there's another concept coming along called "virtual" computers. The hardware is not much different from the computers we have today, but it is designed in such a way that through a computer program, you could make that computer emulate another computer. The person using it may think he is working on another computer. One advantage to the Navy would be that the Navy has aboard ship many different computers, and if you had a virtual computer working with all the others and one of the others went down, the virtual machine would take over the work of the one that went down while it was being repaired. For example, if a computer that is directing a radar goes down, then the virtual machine takes over and directs the radar. One virtual machine, through programming, senses when another computer goes down and takes over. It can exercise the same coding that the other computer has without destroying the problem. That's an interesting concept that's coming along, and we've got some people working on it right now, but it will take some development to perfect it.

I assume the speeds of computers will keep going up with all these small shifts, but there's a limit to the speed of light for transmitting an electrical signal. If you reach the speed of light, you couldn't get any faster on a computer; however, we're not there yet.

How far are we from it?

Well, I'm not sure, but I felt around 1960 when we got the STRETCH that there wouldn't be much improvement in the speed of computers. Now we have the 6700, and it is a lot faster, so it's hard to tell where technology will take us in speeds.

Dahlgren should be proud of the fact that we've had a very outstanding computer organization here. I think it hasn't always been appreciated by everybody, but when we hear from people in other laboratories who use our system and our programs, they're generally very complimentary. The capability hasn't come by accident. For instance, the decision to hire mathematicians as programmers in the mid-1940's was the key decision in developing our capability. I think there's a good future for them.

The programs we have now are good ones. Obviously, once you get large programs using computers, there are generally follow-on programs, and they all require more computing than we had before. Another advantage of having programs like POLARIS, POSEIDON, TRIDENT, and the Transit Satellite System is that they require large computers, and they pay most of the cost. People in the other departments at Dahlgren with small computing problems couldn't afford to have a big computer

program, but they still have the big computer facilities because they are paid for primarily by other programs. They still get charged an hourly rate, but if it weren't for the bigger programs they'd have to use something much smaller. They probably couldn't do as good a job in a lot of cases. They've got a real first-class computer capability to use even though they can't afford it themselves.

CHAPTER XI

Naval Guns

Carl H. Wingo, Jr.

Mr. Carl H. Wingo, Jr., received a BS in mathematics from Wofford College, Spartanburg, South Carolina, in 1949 and later did graduate study in mathematics at American University. He has been employed at Dahlgren since 1951 and is presently Head of the Engineering Integration Division in the Combat Systems Integration Department.

The following interview was conducted by Cynthia Rouse in Mr. Wingo's office at Dahlgren on September 30, 1976.

In order to set the stage for a discussion on guns, can you tell us when you first came to Dahlgren and what opportunities you saw here then?

I came in March 1951, and the opportunity I saw then, as a young man, was the opportunity to get into scientific work. That opportunity was not very much available anywhere else to the normal college graduate. Of course, the Navy always interested me. It posed a spirit of adventure, and I was always historically interested in war and weapons of war. Putting all those things together drew me here.

Were you employed somewhere else previously?

Yes, I was an athletic director for the city of Spartanburg, South Carolina. I got that job upon graduation from college, but I had applied for a job at Dahlgren prior to that. It took quite a while for it to come through. As a matter of fact, it took a couple of years. Employment was then almost like it is now. I can see a lot of similarities in the restrictions and in the ways we are retreating back to control from Washington. Many very difficult things that we had to live with then are coming back to us now—freezes on hiring, control of position descriptions, things we thought we had put behind us are right back with us again.



Mr. Carl H. Wingo, Jr., has had a long-standing excellent relation with naval gunnery at the Dahlgren Laboratory.

What was the scientific environment like at that time regarding proof and testing of guns and ammunition?

Well, scientifically, testing has really come into its own in R&D, and proof and testing is a completely different kind of testing. I wouldn't even describe proof and testing in the same scientific terms as I would R&D. Very little has changed here in the approach to proof and testing over the past several decades, and the scientific requirements in proof and testing are not very great. I've never known, for example, a "lot" of ammunition to be rejected here. So that kind of says very little for the actual testing itself. As long as it stays together, about the only requirement is for it to hit the ground or the water somewhere. However, in the last 10 years, remarkable things have happened at this Station in R&D testing—things that I don't think are recognized very widely, probably not even in our own house. We probably have the capability to do things here in the instrumentation and testing of ammunition in R&D that can't be done anywhere else in the world.

What were some of the major gunnery items tested here during and just after World War II?

In gunnery, there wasn't very much. The major work load in gunnery after World War II was, in fact, proof and testing until the mid-1960's. The latter part of the 1960's, during the Vietnam War, was when the change came here. There was some new gun development that was started in the early 1960's—the lightweight 5"/54 MARK 45 mount and the 8"/55 MARK 71 mount. Both were brought here for testing in the latter 1960's. That's the most significant thing that happened here in the period we're talking about until the guided projectile came on the scene.

At what point do you feel that the pendulum began to swing from purely proof and testing at Dahlgren to increased emphasis on research and development?

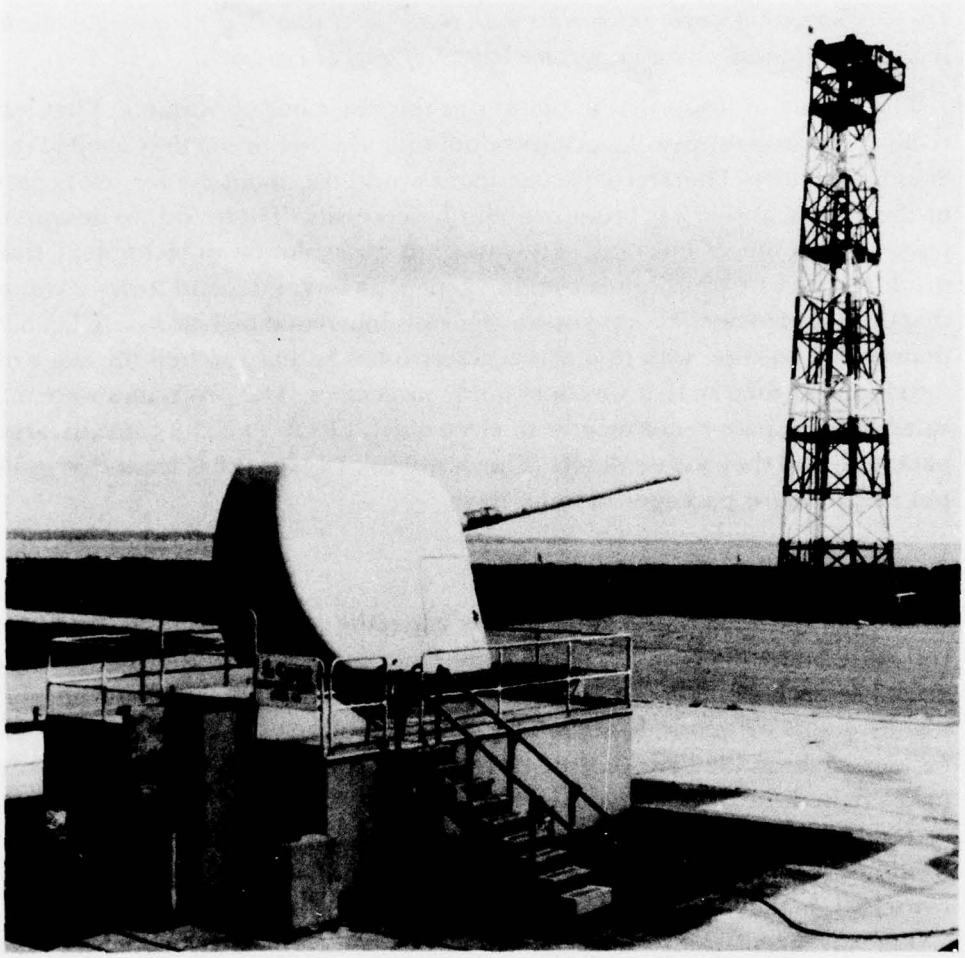
The real point came when this Station decided it wanted to get into weapons development, and again that was the latter part of the 1960's with the leading effort being the guided projectile. Up until then, we were just meandering around aimlessly under the continuous threat of closure. The change in our mission in the late 1950's saved us long enough to get our feet on the ground in the latter part of the 1960's. If we hadn't had a change in mission from a proving ground to a naval laboratory, we never would have survived long enough to have gotten started in the late 1960's into weapons development. If we hadn't gotten into weapons development, we wouldn't be here today.

What effect did the Korean War have on the gun work at Dahlgren?

There was a tremendous increase in proof and test. There were probably more rounds fired in the Korean War than in World War II. As a result of that, the Navy exhausted all of its supply of ammunition. It didn't have any left, and even after the Korean War was over, we continued firing for long periods of time—just proofing ammunition to restock the Navy supply. It was a tremendous increase in our work load—barrels, propellant lots, and projectile lots all had to be proofed here. That was the main impact on this place. After we did that, the question arose as to whether we were needed anymore, and that put us in the period where we were threatened with closure. It's amazing that it could have been forgotten so quickly that a place like Dahlgren is needed to do that kind of work for the Navy. We had to learn that all over again in the Vietnamese War, and we are beginning to forget it again.

I understand that a lot of old guns that had been removed from service were needed again in Vietnam.

That's right. We had to go back and introduce the battleship NEW JERSEY into service for the third time in the Vietnamese War. It was taken out of service



5"/54 MARK 45 installed for testing at Dahlgren.

after World War II. It was a brand new battleship at the end of World War II, and it was taken out of service with most of the others and put back into service before the Korean War. After the Korean War was over, it was parked again, and then by the time of the Vietnamese War, by the time we decided we were going to fight enough to need a battleship, then most of the people that knew anything about that kind of naval gunnery work were gone. We had to go back and get some of them out of retirement. Most of the ones that had not retired still worked here. We were lucky in that regard, so this Station had a significant part to play in the introduction of that ship back into the service. Most of the guns that were used in that war, however, were the 5"/38's which were first introduced into service in 1933 and installed on our tired old ships dating back to World War II.

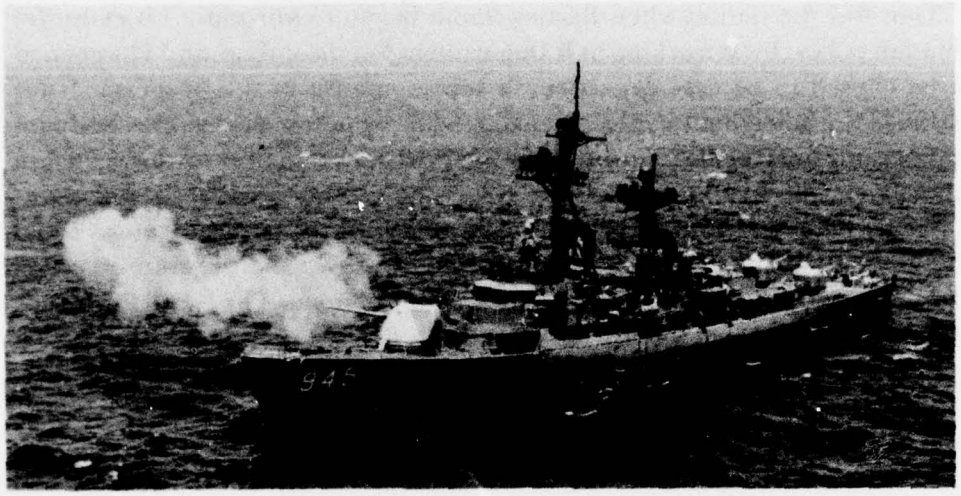
The development of shipboard missiles must have had a retarding effect on gun work. When did this occur and what was the impact, if any, at Dahlgren?

The impact of it was that it almost put this place out of business. That was really the reason for people seriously doubting whether or not they needed the Station anymore. That started occurring, I would say, about the very early part of the 1950's, and it was brought about by necessity. There was no design in putting guns out of business, but it was just an evolution in technology that produced threats which guns couldn't cope with anymore, and it was obvious that there had to be some kind of weapon developed that had more intelligence than bullets to cope with that threat. Electronics hadn't reached the stage of development then so that we could guide projectiles. The projectiles were too small for the space requirements of electronics. Electronics still came in large packages, and they were still soft. They wouldn't survive the G loads that guns put on guidance packages in those days.

Could you explain that?

Yes, I'm speaking of the setback force when the projectile is fired. It's like if you were suddenly accelerated to 2600 feet per second, you'd feel bad. So guidance packages felt bad. The only way electronics packages could survive was by a soft or gentle launch. The only possibility of doing that was with rockets, so the guided missile evolved from that, and that was the solution to the problem in those days. Of course, as years went on, missiles began to get more costly, and there was a period when money didn't enter into the picture and we would have bought them at any cost. In fact, we did, but now we are very cost-conscious, so that's caused a lot of reconsideration of the use of guided missiles.

The advancement of solid-state electronics, which means you can package them in very small packages, and their capability to survive large G forces presented the opportunity for guiding projectiles. We had an attempt in the early 1950's to guide projectiles. That's not very widely known and should probably go into the historical account of the work at this Station. Our part in it then was only testing. The Naval Ordnance Laboratory at White Oak was the developing activity for that program, and it was called the Angled Arrow, but the weapon didn't use the guidance concepts that we're using now. We're using missile concepts now. In fact, the guided projectile is really a gun-launched guided missile, but the Angled Arrow was truly a guided projectile. It just didn't present the Navy with the solution it was looking for. It could have worked. Some of them did work, but the Navy came up with missiles, and they challenged the imagination of the decision makers to the point where they just forgot about guns. That was essentially the end of guns, including the Angled Arrow concept. It went down the tubes with everything else until the mid-1960's when this Station really revived guns.



MARK 71 8-inch lightweight gun firing from USS HULL.

Guns versus missiles must have caused a great deal of controversy in Navy policymaking at one time. Can you identify any changes in naval warfare tactics that resulted from this issue?

The controversy didn't come until the guided projectile began to challenge the missile because there was no controversy when the missile took over from guns; that wasn't controversial at all. The gunners just simply went away and crawled in their holes. The tactical change that was brought about by the introduction of missiles was the change from an offensive to a defensive Navy. That's when we began to be defensive in nature, and it almost completely inundated the Navy. We lost almost any capability to conduct offensive strike warfare.

Can you tell us how the guided projectile concept was conceived and relate some of the initial work in this area?

As I rather implied before, the guided projectile really led to the reawakening of naval gunnery and was the leading effort in this Station's thrust into weapons systems development. The concept, as we know it today, is vastly different from the Angled Arrow concept so that you can call it a different development entirely. The present concept was born here at Dahlgren in about 1967. It happened right down in Building 198, the old Machine Gun Battery. In those days, that's where the Armament Officer resided, and the Chief Engineer for the Armament Division had his office there with the Division Head in that building. Jim Kirschke was the Armament Officer, and David Sloan was the Chief Engineer.

That was the period when Barney Smith began to introduce his executive rotation policy. I was working in K Department* in those days, and I happened to be chosen to be that department's representative in the program. I was assigned the billet that David Sloan had, and he went somewhere else. Suddenly, I became not a ballistician anymore but the Chief Engineer working for Jim Kirschke in the Armament Division.

Well, he was concerned about naval gunnery. Instead of using me as Chief Engineer, he assumed those duties himself and asked me to organize an effort to see if there was any future in gunnery, in our opinion. The Naval Ordnance Station at Indian Head was working frantically to try to get into the R&D end of gunnery in those days, and Jim wanted to know if there was something that Dahlgren ought to be interested in. Is it worth anything to the Navy? Lots of questions were on his mind.

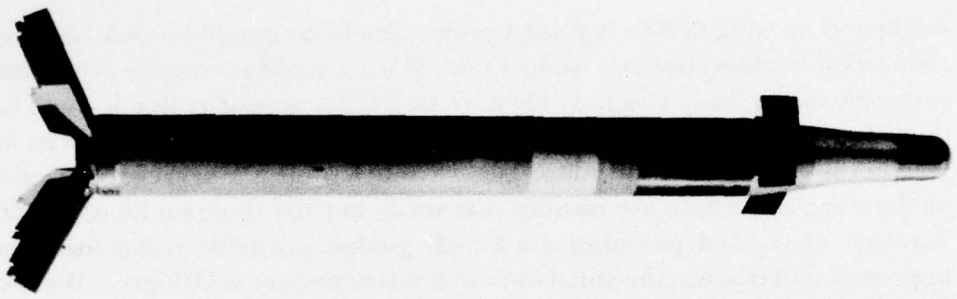
So we organized a little study group. Roy Shank was the main assistant, and we used the other resources that Jim had available in his group and set out to investigate the area of naval gunnery. When we got through with that, we concluded that naval guns were not worth pursuing unless we could improve the intelligence in the bullet. We absolutely had to do better with the bullet or we could forget it. The concept of the guided projectile came out of that. We didn't know how to begin, and we didn't really know whether the whole world would start giving us the big laugh, so what we did from that point was to introduce this idea to the rest of the world and see if we could get some opinions from people who had been working in missile guidance.

We came up with the idea for the first Naval Gunnery Conclave. At that conclave, we brought together experts from all over the United States in just about every area that involved naval gunnery, but we did have a group that addressed guidance for projectiles. That group was headed up by a Mr. Sheppard from the Applied Physics Laboratory at Johns Hopkins, and he had a lot of people that were interested in the guided projectile concept working on that committee with him. We worked night and day for about 3 days, and they heavily endorsed the pursuit of guided projectiles. In fact, they said that technology had indeed advanced well enough for us to have a chance to do it.

After we got that endorsement from the experts, we started charging as hard as we could. The first guided projectile that worked was fired at Barbados, West Indies, with a lot of the initial work done by Jerry Bull, a very controversial gentleman that works for Space Research Corporation in Vermont. He had attended the Naval Gunnery Conclave and had been doing some very early work in high-altitude probes using guns. So he was capable of launching electronics from a gun. We asked him to join us in this venture, and he was eager to do it. Under contract, he provided us with the first guidance package that was successfully fired in this program.

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*Warfare Analysis Department.



5-inch guided projectile developed and tested at the Dahlgren Laboratory.

Before we got to the next step in the normal evolution of that process, that would be getting into developing the controls to go along with this sensor, we discovered that Texas Instruments had a gentleman who worked for them named Weaver Lafferty. He was trying to sell guided projectiles to the Army based on the concept that was used in guiding Navy low-drag bombs. That concept was in the early stages of being introduced in the Vietnamese War, and it was the first guided bomb that was developed using lasers for guidance. Lafferty was trying to sell that concept for guided projectiles, and he believed that he could actually engineer that weapon to the point where it could be fired from guns. He didn't come to the first Naval Gunnery Conclave, but there was a representative from Texas Instruments there. Maybe by that route he learned about the work that was going on here.

Warren Kitterman* came on the scene at NAVSEA in those days and was in on some of the early decisions as to what we should do with guns. He was in the Marine Corps in those days and met Lafferty somewhere. Warren came up with Lafferty, and we got together with him and heard what he had to say. We decided very quickly that he had the fastest way into guided projectiles. We abandoned the effort that we had going with the people from Space Research Corporation because we would have to, in our opinion, redo a lot of things that Lafferty already had done.

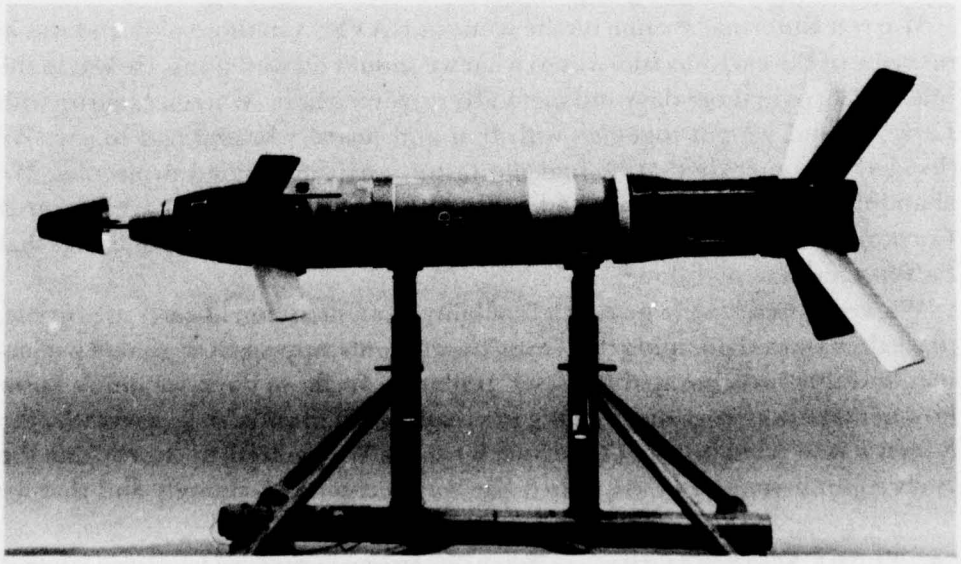
We were interested in getting a feasibility demonstration as early as possible, so we then started pursuing the Texas Instruments' approach to guided projectiles and that incorporated the laser guidance. In those days, we didn't know how to make that work on anything less than 8-inch rounds, so that was why the 8-inch was selected instead of the 5-inch round. We realized, of course, that the Navy's gun inventory in the 8-inch size was declining alarmingly and that we

*Mr. Warren P. Kitterman is presently the 8-inch Gun Systems Engineering Integration Manager at Dahlgren. In the early days of the guided projectile effort, he was a Lieutenant Colonel in the Marine Corps.

might end up with an 8-inch guided projectile and no gun to shoot it, but that didn't slow us down because we had faith that we could eventually evolve the projectile into a smaller round. There was even the possibility that it could be done for a 3-inch round, but there's a lot of doubt in people's minds as to whether it would be worthwhile to guide a 3-inch projectile because of the size of the warhead. There are missiles that small, but still there's a lot of doubt. Anyway, we started pursuing the 8-inch guided projectile using the laser approach, and that was the initial work that was done here at Dahlgren. We had that thing flying satisfactorily within 18 months after we started work with Lafferty. The first successful demonstration was done on March 12, 1971. This was the first one that worked, and we had fired, I think, a couple before we got one to work.

Can you describe some of the ideas behind the development of lightweight guns?

One of the ideas, at least, was light weight itself. The mounts were entirely too heavy. That was one of the big complaints from the people supporting missiles. They were saying we can't have guns, not only because you can't hit anything with them, but because they're so big and heavy. Another idea behind lightweight guns was to bring guns into action again by giving them modern electronics and reliability. Primarily, the reliability factor in the lightweight gun itself was the main driving force behind it. It was a very successful development. The 5"/54 MARK 45 weighed about 50,000 pounds as compared with its



8-inch guided projectile with canards extended.

predecessor, the MARK 42, which weighed 150,000 pounds. We lost half the firepower, but we know how to get that back now. We can build a 50,000-pound, 5-inch mount now and not lose that firepower, so it was a tremendous development, in my opinion. The mean rounds between failure went up considerably, bringing the solid-state electronics into the control systems and getting rid of some of the old switching. This, coupled with improving the hydraulic systems, and so on, has led to considerable improvement in reliability. We're still not anywhere close to where technology should allow us to be, and that's why I think we should develop some new lightweight guns. The lightweight guns that we have now are based on the very late 1950's technology. For example, the MARK 71 gun, which is the new 8-inch gun, has not even been introduced on the first ship yet. It has been almost 15 years since that gun was started.

The Naval Gunnery Improvement Program, which you headed, represented a major effort at Dahlgren in the 1970's. How did this program originate and what was it all about?

The Naval Gunnery Improvement Program was a collection of many individual efforts which had been going on for some time, most of which date back to the Naval Gunnery Conclave. The only thing that does not is the electro-optical sensor system [EOSS] which was really, in my opinion, evolved due to the early work of a young naval officer named Robin Battaglini. Another officer, Pete Orvis over at OPNAV, supported that effort, although he didn't give us a lot of support in naval gunnery. Battaglini and Orvis kept the electro-optical sensor work alive long enough to get it into the Naval Gunnery Improvement Program, although both of them were gone by the time it actually became known as the Naval Gunnery Improvement Program. In those days, GIP, as it was called, included the electro-optical sensor system, the development of the HIFRAG round, and the radar effort which was known as Command Detonation. Pete Orvis was big on that. In fact, HIFRAG, Command Detonation, and EOSS were three of the things that Orvis was behind very strongly. The velocimeter* became part of the program, although it has been a "stepchild" all the way through. It never had a dollar appropriated for it, but it was carried through the entire program.

The first velocimeter effort that I know anything about began when I was in Jim Kirschke's division in 1967. That was one of the first things we did. We asked the University of Virginia to give us their opinion on whether the velocimeter was worth considering, technically, and they decided that it was. They actually did some of the early work. As far as I know, that's where the velocimeter started.

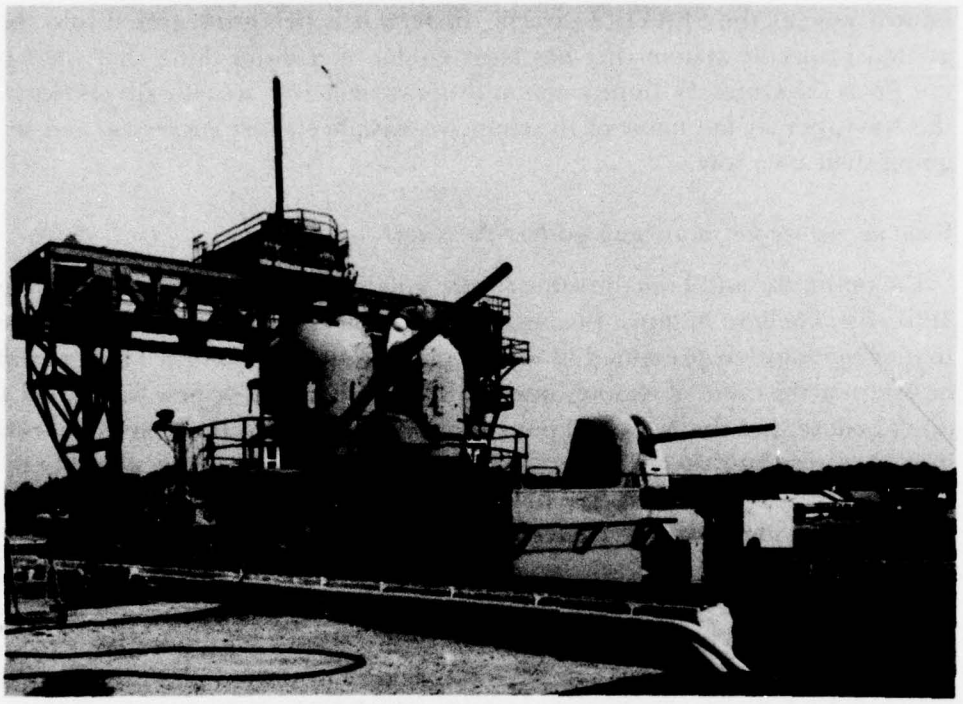
*An electronic device for measuring initial velocity of projectiles.



Tank struck by 8-inch guided projectile.

The Command Detonation fuze was so controversial that it was cancelled within 3 months after we started. The fuze required a fairly significant redesign of the fire control radar. Airborne Instrument Laboratory had redesigned the fire control system, and when the Command Detonation fuze was cancelled we had a fairly significant radar effort which had other spinoffs of importance to the Navy. It was decided then that the radar would continue in spite of the fuze cancellation, and that got to be known as the 53 Echo Radar Development with the major objective of improving the Navy's ability to counter low-flying targets.

The 5-inch guided projectile was in the Gunnery Improvement Program when it was first established, but the controversy and politics of having the guided projectile associated with the Gunnery Improvement Program were so great that it was finally splintered off into a program of its own. The improvement of the 5"/54 MARK 42 mount was part of the program, and we had a lot of trouble with the MOD 7 mount in the Vietnamese War. Part of the Gunnery Improvement Program was to upgrade that mount, insofar as possible, to the technology of the period, so it became known as the MOD 10 mount with greatly improved reliability. That has been a very successful part of the pro-



Australian velocimeter mounted on base of gun barrel for testing at Dahlgren.

gram. There have been a significant number of MOD 7 mounts replaced with MOD 10's, and whether the Gunnery Improvement Program lives or dies, that part of the program is going forward and may, in fact, be completed right now.

The 53 radar itself has been demonstrated successfully both here at the Land-Based Test Site and at sea. The concept seems to be a good one, but the fact that it's tied to the upgrade of a very old radar may retard that particular part of the program to where it will never evolve very much further. I would hope that the concept itself will find its way into other developments.

The velocimeter, as I said, never got the first dime, and the effort that was carried on at this Station was financed by other means. In fact, there was a velocimeter produced which eventually found its way to a ship, but the Navy has never really supported it, and for some reason it's an unexplainable issue at this point. In fact, the Australian velocimeter has found much more support than our velocimeter. It's likely that that one will be the one the Navy will eventually use, although it doesn't have anywhere near the capability that the Navy's own velocimeter has.

The HIFRAG Program is, I believe, ready for introduction to the Fleet now. The electro-optical system is evolving into a more advanced system which is

known now as the SEAFIRE system. In fact, it is being integrated into the guided projectile system that has laser guidance. I don't think that what is known as the Gunnery Improvement Program will ever actually see service in the Navy, per se, but many of the elements have been very successful and are going their own way.

What do you see for future gun work in the Navy?

I'm optimistic and I see the same future gun work for the Navy that I saw in 1967. I still believe in guns. I believe that the Navy needs them, and I believe that technology has presented us with major opportunities which I believe will be useful in the event of another war. I think that there will be new Navy guns. I don't believe that the Navy will put ships in retirement in the year 2020 with guns that were built on technology of the 1950's. I think somebody will wake up and say, "We can't do that. We have to put up-to-date modern guns on our up-to-date modern ships." I have more faith in the system than that, and I believe that the new CSGN, the new strike cruiser which the Navy is now setting its sights on, will have a modern gun that's based on technologies of the 1970's, not of the 1950's. That's what I see for the future Navy. I see guided projectiles playing a strong part, but I don't see the demise of the simpler approach to things. I think that's one of the attractive parts of the gun, the fact that you can shoot both simple and inexpensive as well as sophisticated weapons from it. It's no longer limited to "dumb" bullets, but we still need them. We don't seem to have learned that war can just as well be fought with a rock most of the time. You don't need atomic bombs every time you shoot a weapon, so most of a war is fought with very primitive means. If we take away our primitive means, then we have to escalate to the more sophisticated things right off. We don't want to do that. A lot of times, we don't know what we're shooting at anyway, so we don't want to throw away our whole treasury when we're shooting at things that we can't see or locate. That's the advantage of the gun, and I don't think the Navy will view that any differently. I think there will be very strong support from the Navy for an offensive as well as defensive capability, and guns will play a strong role in that. We're going to have new guns to do that.

CHAPTER XII

Rapid Development

Bernard Smith

Mr. Smith was born on May 15, 1910, in New York City. He received a BA in physics from Reed College in Portland, Oregon, in 1948, and in the same year gained employment with the Naval Ordnance Test Station, China Lake, California. In 1961 he was promoted to Chief Engineer for the Bureau of Naval Weapons where he remained until he fulfilled a 1-year tour as a civilian scientific officer with the Office of Naval Research in London, England. In August 1964, he came to Dahlgren as Technical Director. Mr. Smith retired from Dahlgren on June 29, 1973.

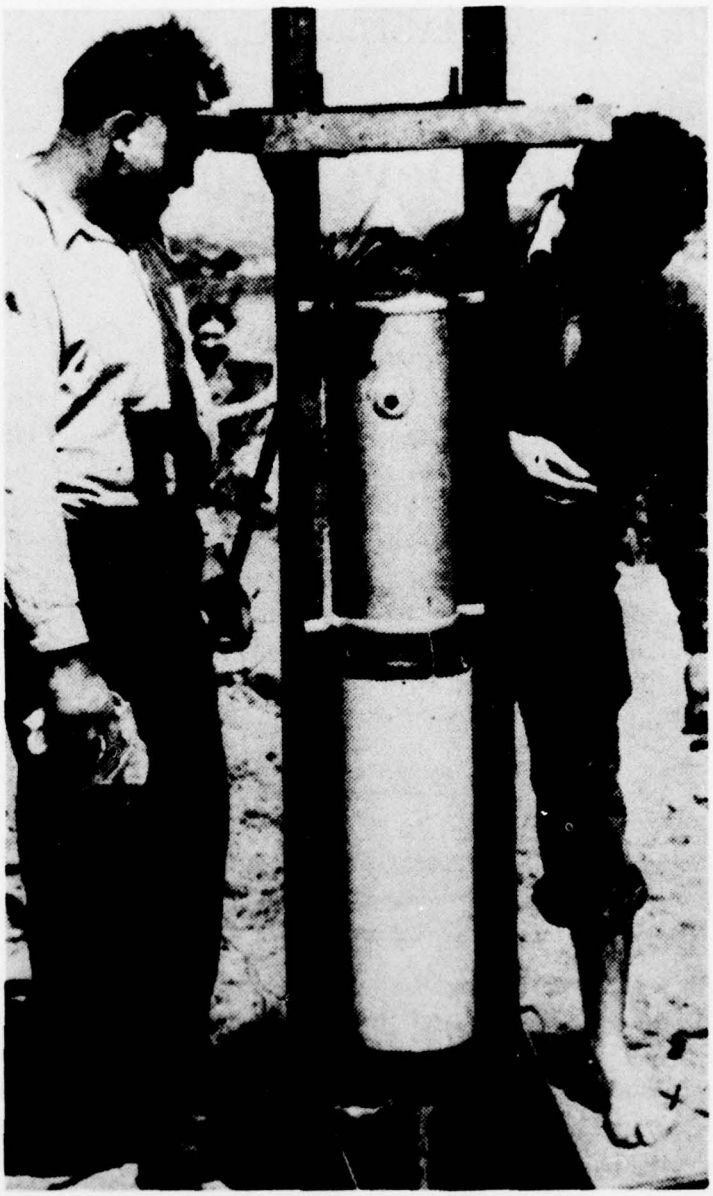
The following interview with Mr. Smith was conducted in his home in King George County, Virginia, by Cynthia Rouse on June 8, 1976.

In 1932, you conducted the first public firing of a liquid-fuel rocket in America. You were 22 at the time, which seems a very young age for such an endeavor. How did you build up to this feat at that young age?

If you will recall, those were the years of the Depression, pretty much the depth of the Depression, and there really was nothing very much to do in New York where I lived. It struck me at that time that maybe there's a better world. The thing to do is get off this planet and look for another one. The only way to do it is with rockets. That's really what started me off.

Your first association with the Navy was at the Naval Ordnance Test Station in California in 1948. There's a large gap between 1932 and 1948. What did you do between those years?

Most of those years, with the exception of college years, were spent as a mechanic, a welder, a blacksmith, a locksmith, doing odd jobs. Just prior to my going off to college, I was a welder in a shop, sort of working into the managerial area and deciding after a while that the best



Bernard Smith (right) sets up ARS Experimental Rocket No. 3 for a public launching in 1932.

short cut was to go to college. So, one day I sold my tools and my house and went off to the first college that would admit me and that happened to be Reed College. After I came out, there was a question of employment in 1948, and I first applied to a number of universities to do graduate work. They didn't like my age. That was pretty late to start

doing graduate work. The Naval Ordnance Test Station at China Lake, right in the middle of the Mohave Desert, was in need of professionals, and I guess they were ready to take anybody, so they took me. That just about fills up that gap.

You came to Dahlgren as Technical Director in August 1964 after serving in numerous impressive positions. How did you view your assignment at that time? What did you see as the mission of Dahlgren, and how did you plan to achieve and expand that mission?

By the time I was appointed at Dahlgren, it was already established as a laboratory, and having had most of my experience in the Navy at a laboratory like the one at China Lake, I thought that was what I wanted to get back to. Research and development of any kind were always fascinating to me, especially in ordnance. I was most familiar with rockets, guns, etc. I had a familiarity with that for a long time before I worked for the Navy. I thought I could help the efforts here with what I knew, with the ideas that I had, and with the experience that I had in management, to look at all aspects of it. But I wasn't exactly prepared for what greeted me after I arrived. It wasn't more than 6 months after I arrived when there were some hints that Dahlgren was to be closed. I didn't like that. I didn't think I should have come to Dahlgren to officiate at a decent burial, and that's when the challenge appeared. I can tell you more about that later.

Had you been to Dahlgren before 1964?

Oh, yes. Even when I was at China Lake I had interactions with Dahlgren on a number of weapons. Weapon A was one of them. That's when I first got to know Dr. Cohen. There was also a shorter-range version of Weapon A. I think it was in 1950 or 1951 when I first came to Dahlgren to look at the firing results of Weapon A, and I talked to Dr. Cohen sometime in those years. Then I had other interactions with Dahlgren when I was Chief Engineer of the old Bureau of Weapons. I had quite a few of those, so I was conversant with what went on at Dahlgren before I took the job.

What was your view of Dahlgren in the early 1950's when you first came?

Well, I thought that, really, it wasn't going to go very far if all they were going to do was ballistics and making range tables. I felt that while we needed that kind of service for weapons like Weapon A, there were limitations to it. The Laboratory looked like it had certain kinds of work established for it on guns and ammunition and something on rocket trajectories, and it was on the tail end

of bombing. All the bombing work was going to China Lake. I doubted if they knew themselves where they were going.

You were here at Dahlgren during the transition period when Dahlgren shifted from a proof and testing laboratory to a research and development laboratory.

No, the decision to change it from one to the other took place in 1959, and I arrived in 1964.

So the transition had been made.

A certain amount of it had been made, but it still clung to its primary role, which was the proofing of ammunition. When I arrived, that was about 50 percent of the work. When I left, it was less than 10 percent. That's a big change in a period of over 9 years.

Was there any major reorganization during this time?

There was some reorganization before I arrived. I think Russ Lyddane was involved in that. I'm not sure I would identify it as a major reorganization. I knew that I had to make a reorganization, but I also knew that making it on paper was worse than useless. You can make an organization chart on paper and distribute it and say, "Now here, attach this or attach that," and there's no assurance at all that it's going to work. What I did was study the situation for about 4 years and made sure that the key people—and I had to identify them—would be comfortable with changes and wouldn't be too attached to organization charts that already existed. So, I had lots of people attempting to do things that they thought made sense, that they believed in. They had quite a bit of freedom to do those things, and pretty soon organizations began to build up around them in an evolutionary way. I knew the time was right for a formal reorganization when quite a few of the younger enterprising men came to me and said that we're disorganized, and that we ought to get organized. So what actually happened was that we formalized what had begun to exist, and then it was very easy to get acceptance of the new organization, which was really a revolution. The revolution had taken place in an evolutionary way. It's one of the few organizations—and I give myself credit for this because I did it deliberately that way—it's one of the few organizations in which we got acceptance of a reorganization without promoting anyone.

Did you see any trends in the surface Navy that brought about significant changes in the Dahlgren mission in the 1960's?

Yes, there were changes taking place all along. Some of them were almost by osmosis with nobody really engineering them, just people coming to conclu-

sions and waking up to the way it's going to be. One of them was the change in the approach to fire control. There was the need for new sensors that would give us an advantage and the need—this was recognized at Dahlgren, I think, more than anyplace else—of constantly having to change the approaches to ordnance in order to insure military surprises, never really having to say you have the only way. I've always felt we had to watch out for changes because, if we get prepared to fight in one way that preempts all other ways, the enemy would choose to fight the other way so that our real strength was in (a) a quiet willingness to accept change; and (b) in making the change revolutionary, if necessary, without becoming addicted to that change because you made it. The strength of the naval resource at Dahlgren is the understanding—it isn't understood by all the employees—that the way to keep ahead in military technology is to constantly generate new additions, new combinations of things, at such a high rate that the enemy can never catch up with you, even if you have no secrecy or any restrictions. You just open up all the doors and let them come in and give them all the drawings and everything else; but always stay ahead of them so fast that they're kept busy trying to catch up but never really doing it. Now let them make those mistakes thinking they've got the way; then they're in a trap. That, I think, is the most important change that took place; even more important than the new pieces of hardware that were being generated. To get down to specifics, we knew we had to make bullets smarter. Trying to do this almost sounded impossible—putting guidance in a bullet and a number of other things, some of which are classified and can't be in this history.

Dahlgren is heading more and more in the direction of systems work now. Did you anticipate this in the late 1960's?

Oh, yes. I knew that we had to get ready for it. There were very few places in the Navy where this was actually being accomplished. In Washington, there were headquarters groups that thought they were doing it, mainly because they were told it was in their position descriptions and their organization charters. That didn't automatically make them equal to the job. It takes a lot of hard work, a lot of experience, a lot of education, and a lot of equipment to have that competence; and if you don't do it for a year, you're dead on the vine. Those people in Washington hadn't been doing it for many, many years, but it was hard for them to believe they weren't capable because they had the official assignment. There was some of it being generated at China Lake, some systems capability, and I think at White Oak, and a few other places in the Navy. I think we've gone farthest at Dahlgren in recognizing what it really takes. It's not only the breadth of the techniques—the mechanical, the electronic, the electrolytic, the optical—all those techniques have to be put together. But it's also the ability to generate the concepts and take them through the early feasibility stages,

understanding what that process is and what the mistake-making process is. That's what it really is, and being able to identify the winners and reject the losers, no matter how dear those babies are. Some of the ideas were no good. They were clever and really represented a lot of intelligence, but they were solving problems that didn't exist.

You take it from there to some prototypes and on and on until you have a model that you can describe for production that will work when it's produced as described and which then can be given out to industry. Industry, of course, likes to usurp all that. They say they can take the whole process themselves, but that has always been a mistake. Whenever we have generated the models ourselves to at least the first prototypes, the Navy has always been better off, always been able to write better contracts, and the bidding was better. The bidding was more honest. That's one of our functions, to always be educated far ahead of the rest of the world, including the industries to which we give contracts. If they get the education and we don't, we're at their mercy. That's the way it has always worked out where that hasn't been guarded against. There are too many [in civil service] who are willing to take the path of least resistance and not do this work themselves and give it to contractors. Pretty soon they're trading with contractors. The civil servant cannot monitor contracts if he doesn't know more than the contractor. It's impossible.

What type of funding problems did you encounter with the SYSCOMS in the 1960's, if any?

Really not very many except in the one area of military construction. That's where I failed. We didn't have any trouble getting the programs, but we were short of housing. I don't mean private dwellings. I mean offices and buildings for putting things together and making things. We were short of that—laboratory space—and I think the former Director of Naval Laboratories felt that as long as we were getting the programs, we weren't in trouble, and that it was those labs who didn't have the programs that needed the military construction to create the facilities that would get the programs. Well, I've lived long enough to know that it doesn't work that way. When you have the programs financed, you can then justify the military construction money for providing the plant for those programs. In fact, if I were sitting in the approval chain and anybody came to me for new military construction money and didn't have the programs, they wouldn't get a penny, but it didn't go that way. It's a good example of how reason sometimes doesn't work and that there was something else working, and I don't completely understand what it was. Now, the new Technical Director has been more successful and maybe some of the groundwork was done in my time. It might have helped. He's very kind. He gives me credit for that groundwork. But during my time, there was very little

construction work in those areas. So that's the only area where we were really short. That's why we had to have 100,000 square feet of relocatables, which is a poor investment, in order to have working space. You know those darn trailers down there. They were a joke, but we had to have them. That was the only way we could get any work space for the expansion. Otherwise, we really didn't have any money problems.

What was the military-civilian relation at Dahlgren during the 1960's? Are there any base commanders that come to mind that particularly affected this relation?

Well, commanders of bases are people, and just like people, there are good ones and bad ones. I learned a long time ago that the relationship of the Technical Director and the Commanding Officer of a laboratory is something that should be kept between those two men, and if they have any differences, they should not be settled by either one of them trying to get edicts from higher authority to settle the problem he has with the other fellow. In fact, I never let the issue of who the hell is the boss around here come up with any of the commanders I worked with. Now some of them felt that they carried all the responsibility and I was just another employee to them, and there was no use arguing that point as long as they let me do my work. There were some that recognized that it was a partnership and that really the technical work belonged to the Technical Director. Some of them frankly faced that. They said, "That's the way it should be; you are in charge of all the technical work, and you carry the responsibility." That's the way Washington recognized it, by the way. But some of them [base commanders] lived in dream worlds. The reason they were appointed to the position [they felt] was that they were either scientists or familiar with the R&D system or the Navy Procurement System, and everybody else at the Station was an underling. Well, you can't redo history, but Dahlgren would have been even further ahead if about half those commanders were replaced with the other half. About half of them really didn't do anything. An outstanding one was Admiral Chase.* He came here as a Captain, and he saw immediately what I was trying to tell him—that we have to go in a certain direction which requires plowing a lot of young professionals into the system at a very high rate. We were going to need them and a different class of officers coming on board, not those who were taking their last tour before retirement or mustangs who never really were professionals, but career officers who still had a career to make, even after they left Dahlgren, and whose careers could be enhanced by the right duties at Dahlgren. He [Admiral Chase] always took me with him when we went to the recruiting officer so we could explain what we had to have in the way of officers. Even though he wasn't here a full term, the

*Admiral John Dawson Chase was Commanding Officer at Dahlgren from July 1968 until July 1969.

biggest expansion in professional personnel took place when Johnny Chase was Commander. He gave me carte blanche. Steve Anastasion* also saw what needed to be done, but he more nearly wanted to do it himself, but that's alright. Well, I would say they were the two most outstanding ones.

What were your feelings in the 1960's concerning civilians being housed on the Station?

That makes sense. It still does.

To have them housed on the Station?

The experience I had was that employees who live on the Station somehow get much more interested in their work. I'm talking more of professionals. I expect a professional to believe in his job and believe in what he's doing and want to do it. It's a convenience to live on the Station when you're working on a problem, a challenging problem, if you want to come back after dinner. In those years, we were getting a tremendous amount of effort the government never paid for just because the men were interested. I don't know whether it led to happy homes or not. Maybe it didn't. During some of those years in the 1960's and the early 1970's, you could go to those labs after dinner and be astonished at how many people were working there that never put in for overtime. Most of the professionals were doing what they really wanted to do, and we were gradually getting rid of those who were just taking a salary. You don't go very far in an organization, especially in an R&D organization, when you have too many people like that. They put in their 40 hours. They know what the minimum is. They study the regulations to make sure what the minimum is going to be, just sweating it out until retirement time. We encouraged a lot of early retirements to get them out. We probably had less than our fair share at Dahlgren.

What did you see happening at the Laboratory after the Civil Rights Bill was passed in 1964? What impact did this have on the environment at Dahlgren?

Practically none because we had started all this long before. I was real lucky in that Mike Sellars, the Commander that hired me, and one of his predecessors had pointed out to the residents of this county that the growth of Dahlgren is beneficial and that the Station couldn't grow if it couldn't receive anybody, black or white, in the local restaurants, in the motels—they had to be integrated. This was long before the civil rights business came. When I arrived, anybody of any race or color could go to any place here, at any restaurant, any motel, and it wasn't any problem. What we did was to use every part of the civil rights movement that was favorable to build up the personnel and morale among the

*Captain Steven N. Anastasion was Commanding Officer at Dahlgren from July 1969 until January 1972.



Mr. Smith (left) with the late Wernher von Braun at a 1966 banquet honoring space pioneers.

blacks at Dahlgren. We had very good results with the professionals. Actually, I think the statement can be made that the black professionals really didn't get any special privileges. They had been working themselves into their positions through their own struggle and their own preparation. We were always able to knock the wind out of anybody's argument about discrimination among the blacks by saying it certainly isn't true among the professionals because the average grade, only a few years after I came, of black professionals was higher than the average grade of white professionals. We were almost up to the numbers game, too, where there were 8 or 9 percent that were among the professionals. That was getting close to the 10 percent of the black population of the nation.

The bigger problem was among the nonprofessionals where the whites and the blacks both seemed to be willing to accept a relationship that really worked against the blacks. There we had to take a positive action and use reverse discrimination, but we didn't do it by lowering any standards. What we did, and I think it's still true, was to make sure that the disadvantaged blacks would get priority on any training or any excused time for training. We actually got complaints from the whites that we were discriminating against the whites. But we had to do that. A number of blacks hadn't even learned how to read and write. We had to start some of them from that—to teach them how to read and

write. But you can always look back and say there were things you could have done, in retrospect, that would have made a situation more successful, but I think we did much better than most other communities. We've been looked at as a showcase by the equal rights people in Fredericksburg and Washington. Washington had sent people down to us to see how we did it. It's only right that a federal activity attempt this. I don't know what you can say about private business, but certainly the government, as a whole, has that kind of responsibility, and we're agents of the government.

You were Technical Director at Dahlgren during the Vietnam War. This conflict certainly must have had a tremendous effect on the work at Dahlgren.

We never had any trouble recruiting as many young people as we wanted—the best ones, the cream of the crop, out of colleges and universities—never had any trouble. It indicates to me that what the newspapers and the media were reporting was grossly exaggerated. A lot of us felt that if you're going to do any negotiating with that bunch [communists], you would have to have a position of strength. If you didn't, you would lose your friends, and we've been losing them.

Did the work increase at Dahlgren during the war?

It increased. We had quite a bit, but it hasn't gone down since we had this so-called phony peace. Dahlgren is still as active as ever.

Employment was higher at that time, wasn't it?

Employment on the gun line, proofing load, was higher. But as that went down, it was replaced by other programs coming in. Dahlgren skills weren't going to be overlooked by headquarters.

So there really wasn't much of a "gearing down" after the war?

Only in one area, the proof and acceptance. The other areas increased as that went down. In fact, a lot of the people that we thought we might have to lay off in that area were transferred and retrained in other areas. Through that whole so-called RIF, there weren't more than 50 discharged. Out of 3000, 50 are peanuts.

Do you remember any serious accidents that occurred in weapons testing at Dahlgren?

There was one. I guess we attribute it to a personnel error, where somebody was killed testing some ignition [of ammunition]. I think it was somewhere around 1969 or something like that.

What was your philosophy of management disciplines at Dahlgren in the 1960's?

I think that it was suitable up until about the 1960's. Management always took great pride in doing very well what Washington asked them to do. One of the problems during my time was that people weren't being rewarded for what they were asked to do. Many of them didn't like that. Some of them couldn't even understand what I was saying, that it wasn't going to be a question of slavishly doing what you're told to do. Professionals would have to think about what ought to be done or what makes sense to be done and not assume that the people in Washington or even the management at Dahlgren had a monopoly on brains. Real rewards would come from sticking to what you believe to be right, even against external resistance. As it turns out, that's what the Bernard Smith Award is for. Every year it has been awarded to just those kinds of guys who believed in what they were doing. They didn't get much support. In fact, in some instances, there was resistance against what they were doing, but they stuck with it and came through with a valuable product. We tend to forget those kinds of people. I'm glad that the award was established for them.

Dahlgren now seems to emphasize heavily professional development. Was this always the case or can you relate some factors that led up to this?

No, it wasn't always the case. The only place where it really was pushed was in K Lab when I arrived and that was through Ralph Niemann. He knew that professional development would be the answer. He and a few others I appointed worked with the Personnel Department and really worked up the Dahlgren University. That has had a tremendous payoff. The best part of it is that if you have that kind of thing on the premises, professional work itself reveals the need for the training; and if it can be obtained on the site, that's a real good closed loop. Most universities are teaching things something on the order of 5 years behind the times at best. At the Dahlgren University, we have closed that gap to maybe a year or two. Technology advances so fast that you have to get courses organized and changed at a very high rate. Universities don't want to do this. It takes them so long to get a course established.

How do you feel about management training?

I think it is a good thing to do, even though the results are frequently disappointing. We've had many cases where we poured management courses into some of the managers for years and made absolutely no change, just a complete waste of time. In some instances, it has made a change, but I often wonder if the people who took those courses didn't have it inside of themselves in the first place and that we really don't know how to teach management. We think that we're doing something progressive and beneficial when we give these



Bernard Smith (left) hosts Astronaut Scott Carpenter at Dahlgren in March 1966.

courses and sometimes a person is improved. We attribute it to cause and effect, but there may not be any relationship. It's hard to take the risk of not doing it. I don't think it does any harm, and frequently when you give those courses, it isn't the instructor who is really doing much; it's the student managers talking to each other and exchanging ideas that probably has the most value because they wouldn't be doing it in a work situation. So, I think it is a plus, although the benefits are not as great as teachers of management courses might think or the salesmen for management courses might think.

You seem to have promoted executive rotations during your tenure. How did you arrive at this policy?

Well, I did it on a small scale every time I had a supervisory job. It all comes out of the notion that a man practicing good management, as one of his first acts, makes sure that his replacements are trained. Some people don't believe in that. They think that if they can keep themselves indispensable, no one can replace them. They get more security out of it. That kind of guy shouldn't be promoted. An indispensable man isn't ready for any promotions because he can't be spared for any promotions. I've always found it to be a good practice to make sure, as soon as possible, that replacements were under training to take my job. I found I was always free to say "yes" when I was offered another without hurting the organization that I was leaving because my job was ready to be filled with trained men. Well, how did I do that? By letting them take my job once in a while. I'd go away and come back after a visit to Washington or an extended course that I was taking and see what they did, and in this way I had them face those kinds of problems I was facing and gave them a better understanding of what my decisions were when I got back on the job. That always worked for me, so I tried it on a larger scale and said, "Why just have them rotate to my job? Why don't they rotate to each other's job—all of the department heads?" Then nobody is indispensable. Any one of them can fill any of the other jobs, including mine, and you know they all got higher jobs out of it, and one of them became the Technical Director.

*Jim Colvard.**

He not only made Technical Director of the Laboratory, but of the Center. Two of them are essentially Technical Directors of the two labs. All came out of that system. So I think it's good. Then it was picked up lower down the line. Division heads began to rotate. The departments began to practice that with their division heads and division heads with branch heads. This made for a very flexible organization, no member of which was too closely identified with his parochial organization, and all this bickering that I used to see at China Lake with department heads plotting against each other just disappeared because each guy knew that if he was plotting the downfall of another department, that's the next assignment he would get—that other department.

*Mr. James E. Colvard came to Dahlgren in 1969 and has held positions as Head, Advanced Systems Department; Head, Surface Warfare Department; and Assistant Technical Director. In July 1973, he became Technical Director at Dahlgren and is presently Technical Director of the Naval Surface Weapons Center.

Now we've already mentioned Ralph Niemann and Admiral Chase as making outstanding contributions at Dahlgren. Are there any other significant personalities that come to mind?

Of course, Jim Colvard and Chuck Bernard,* and Lee Clayberg** is showing that he has outstanding qualities. A number of men have left. Art Jones† was really very good. He undertook to do things that were, strickly speaking, out of his training, and he showed the flexibility of being able to learn. Some of them [the men] were flashes in the pan that showed a peak. Maybe that was when they were trying very hard. I used to try to tell all the young people when I interviewed them that, contrary to what they might hear from other people, if they really wanted to move ahead in the organization, they'd better not relax, just stay tense all the time. It's a full-time peak effort, moving all the time, which means the adrenalin has got to be pumping. It's too bad, but all the junk we get from the psychologists talking about ways and means to get people to reach "a good adjustment" with no problems is completely wrong. The human being evolved as a problem solver, and that's his primary function. If he doesn't have problems, he improvises or invents artificial problems, games or something. He's constantly out of adjustment and that's normal for a human being. When he's got a lot of problems, he's functioning.

Did you ever have a chance to become acquainted with the former Technical Directors–Dr. Bramble, Mr. Riffolt, or Dr. Lyddane?

Dr. Bramble and Russ Lyddane. Who's the other one you mentioned?

Mr. Riffolt.

No, I didn't know him. I heard about him.

He must have been there in the early 1950's.

The one I knew best was L. T. E. Thompson. I put him a cut ahead of the others. He became Technical Director of the Naval Ordnance Test Station where I spent 12 years. A lot of the methods that I've used at Dahlgren were really taken from "Tommy." He understood the research and development game, I think, better than any other Technical Director. That's what I tried to

*Mr. Charles W. Bernard came to Dahlgren in 1969 and is presently Associate Technical Director at the White Oak Laboratory.

**Mr. Lee A. Clayberg came to Dahlgren in 1962 and was former Associate Technical Director at the Dahlgren Laboratory. He is now Head of the Electronics Systems Department.

†Mr. Arthur L. Jones came to Dahlgren in 1942 and was Assistant Director of Analysis, Computation and Analysis Laboratory (now Warfare Analysis Department), from 1964 until 1970. From 1970 until his retirement in 1972, Mr. Jones served as Head of the Engineering Department and as Head of the Surface Warfare Department.

get at Dahlgren because I saw what he was trying to do at China Lake, and it sounded absolutely right. His philosophy was to have a research and development activity in which you could think of an idea, do some analysis on it, whatever is necessary, make some drawings, get some people to make them for you, put it in the shop tonight, have it finished tomorrow morning, take it out to be tested, and by the end of the second day, you get results to tell you whether your idea was worth a darn—all in one place. You don't have to ship anything; you don't have to negotiate a contract to have this made or this tested or this analyzed or anything like that. That's what I tried to do at Dahlgren, and to a large extent, I think that I was able to do that. Of course, not by myself. I had good people who all understood what the goal was—to be self-containing so that you can work fast. You make your mistakes very fast, and that's how you beat the world.

Dahlgren has merged with the former Naval Ordnance Laboratory [NOL]. Do you see any significant impact that this will have on the Dahlgren work environment?

I think Dahlgren was headed in the right direction in systems before that merger. I think the problem would more nearly be how to get NOL to make the adjustment from a very formal organization to an informal one. It's losing a little formality with Chuck Bernard there now, which is a good thing, but prior to the merger, it was highly organized. Everything had to go up through the chain and come down before it could be approved, sanctioned, financed. It was much easier going at Dahlgren. I was criticized more than once for running this loose organization. "You don't even know what the hell the organization is!" No, I don't want to know what the hell the organization is, and I don't really care. It certainly isn't the chart. At Dahlgren, far more attention was paid to objectives and goals than to organizational structure. You really have the best kind of organization when there's no discipline required and no one needs to resort to the authority of the organization to get the work done or as an excuse for not getting it done. When somebody has done the wrong thing, you let him into your office when he comes to tell you what he's going to do to fix it. Don't let him in to alibi, to explain how he goofed off and it isn't his fault and the organization is to blame. Just tell him to forget it and work on the problem instead of the alibi, and he'll be much more impressive. It's a hard thing for people to learn, to give up trying to explain their failures. I sincerely hope that the merger will not introduce the kind of formality that will cause Dahlgren to lose its easygoing ability to admit mistakes and go on to the next thing.

Was this merger discussed during the 1960's?

Oh, yes, it was discussed from a different angle, of how Dahlgren was going to be absorbed by White Oak. That was the topic of conversation in 1966.

Were there any proposed mergers with other installations?

Panama City was supposed to be part of the merger with NOL and what used to be called USNAVUSL* up in New London and NUWRES** in Newport, but we at Dahlgren never made any proposals at any time to absorb any other activities. As a matter of fact, it was Admiral Mark Woods,† he's on the Advisory Board now, who proposed one time we take over Indian Head. I felt that would be getting too deeply into industrial tendencies for which Indian Head was really created, and it would be diluting our effort to get into that. The merger the second time around made more sense, more a merger of equals, but there's nothing really to be gained out of it. There are more people required—coordinators. I'm not saying they're useless, but you've got a Technical Director and a naval officer now in addition to what was existing before consolidation. I would not have done it that way. I would have taken an evolutionary approach of maybe 4 years, on that order, of people exchanged between them. They're doing some of it now, but I'd have taken a much longer time and then call on the people who have been rotated to prescribe a reorganization. Then it's their organization, but now they don't have anything to do with reorganizing. They're not responsible for reorganizing. Well, I've written articles and preached on this a long time ago that unless the people who are being reorganized have a good part in the reorganization, nobody needs to be surprised if reorganization fails. Most of them do because they're masterminded from the top by those who don't understand the process. You can't reorganize into competency or efficiency or even into honesty. You've got to work on the problems of "feeblery," incompetence, and so forth. You've got to identify the individuals and really get them straightened out or fire them. That's unpleasant, but it's better than to reorganize them.

In retrospect, would you do anything differently as Technical Director?

I'd have been a little kinder to certain people who were really caught in traps, and I'd have been a little tougher on some of the others who were schemers and contrivers. Those are the things I would have done differently, but you really don't know about a person until after some history has transpired. I don't claim to be somebody who can interview a guy for a half hour, size him up, and tell you what he's going to do. Even after the year's probationary period, it's hard to tell what a man is going to do. You need at least that, but for some of the men, I needed something longer than that. They're pretty clever in being sea lawyers and knowing all the rules and regulations and how to avoid them and give the

*U. S. Naval Underwater Systems Laboratory.

**Naval Underwater Weapons Research and Engineering Station.

†Admiral Mark Woods was Commander of the Naval Ordnance Systems Command (now Naval Sea Systems Command) from 1969 until 1972.

appearance of progress when no progress is being made. It was just impossible to have a big enough police force. It would be wrong to have one to check on everybody. That would be self-defeating anyway. You have to trust—do a lot of trusting. Some people let you down. If I knew then what I know now about some of those people. Some of them who are self-effacing and didn't toot their horn or didn't alibi, some of those suffered at my hands, but time has vindicated most of them. Anyway, they'd have come ahead a little faster, I think, if I knew then what I know today about some of them. That's what I'd have done differently. The other things, the technical decisions, who knows whether they'd have been better or worse. You'll never find out whether they'd have been better decisions. You can identify decisions that were wrong. If you cannot identify the replacement decisions at those times, almost anything else would be wrong.

CHAPTER XIII

Dahlgren in Perspective

Rear Admiral Frederick S. Withington

Admiral Withington was born in Rutherford, New Jersey, on November 1, 1901. He was appointed as Midshipman to the U.S. Naval Academy in 1919 and commissioned Ensign in June 1923. His selection for the rank of Rear Admiral was approved by the President on December 29, 1949.

After graduating from the Naval Academy, Admiral Withington served a brief tour with the Navy's Bureau of Ordnance before being assigned to the battleship WEST VIRGINIA in December 1923. He remained with WEST VIRGINIA until 1928 when he was enrolled in Ordnance Engineering at the Naval Postgraduate School. From June 1936 until June 1939, he performed staff duty aboard WEST VIRGINIA, CHICAGO, and WINSLOW before spending a 3-year tour with the Bureau of Ordnance in Washington.

From May 1942 until November 1943, Admiral Withington served as Gunnery Officer aboard INDIANA and as her Executive Officer until October 1944. During this time, INDIANA operated in the Southern Solomons and participated in the New Georgia campaign before transferring to the Central Pacific in August 1943 where she joined a carrier group in air strikes against Marcus Island, occupation of the Gilbert Islands, and bombardment of Nauru.

In June 1944, Admiral Withington was assigned as Chief of Staff and Aide to Commander Group TWO, Fifth Amphibious Force in the Central Pacific, and was awarded the Legion of Merit for outstanding service.

On November 28, 1944, Admiral Withington reported for duty again with the Bureau of Ordnance and was appointed Officer in Charge of the Naval Ordnance Laboratory in October 1945. From January 1947 until August 1948, he returned to sea duty as Commander of MISSISSIPPI and later MANCHESTER. In July 1949, he reported to the Chief of Naval Operations and served first as Assistant Director and later as Director of the Atomic Energy Division.

Admiral Withington was assigned as Commander Amphibious Group THREE in November 1952 and reported as Deputy and Assistant Chief of the Bureau of Ordnance in December 1953. In November 1954, he was nominated Chief of the Bureau of Ordnance for a term of 4 years, and in April 1958 he

became Commander Naval Forces, Japan, where he served until 1961, receiving a Gold Star in lieu of the Second Legion of Merit.

Admiral Withington was transferred to the Retired List of the U.S. Navy on April 1, 1961, and later continued his service with numerous Naval Advisory Boards. The following interview with him was conducted by Cynthia Rouse in his home in Washington, D.C., on November 5, 1976.

When you first went to the Bureau of Ordnance after graduating from Annapolis in 1923, were you aware of the work that was going on at the Dahlgren Proving Ground?

At that time, a large group of Ensigns, myself included, were ordered to WEST VIRGINIA and COLORADO—new battleships. Neither ship was yet in commission, and the Navy didn't know what to do with us, so they gave us what we called a "Cook's tour" of a good many naval activities, including the Naval Proving Ground at Dahlgren. The skipper at that time was Captain Stark, who later became Chief of Naval Operations. There was very little there because the Proving Ground had only been shifted from Indian Head a few years before. The Captain's house was in existence, the Plate Battery was started, and the Gun Battery had been placed, but none of the fine facilities that exist there now—certainly not the computer center—had even been thought of, let alone placed. So, in that respect, I was very much aware of the work at Dahlgren.

You took ordnance engineering at the Naval Postgraduate School in 1928 and spent some time at the Navy Yard during this tour. Did you have any contacts with what we now know as the Navy Labs?

We spent the third year of our Ordnance Postgraduate Course on a long tour of various naval activities. I was a fire control postgraduate officer, so my major stop was at Dahlgren where I spent an interesting and eventful 5 months. I also went with other fellow students to Indian Head, Newport, the Army facilities at Belvoir and Aberdeen, the Ford Instrument Company, and several others, but my major stop was at Dahlgren. We lived in a field hand's shack across the road from a former farmhouse which was occupied by the Supply Officer. It was east of the Plate Battery, and it had been hauled in from the woods and set up on bricks. It was impossible to heat. I spent one Thanksgiving Day stuffing the windows with newspapers so we wouldn't freeze to death.

Captain Leary, who was then Commanding Officer, used to come out and walk around the place worrying about us. He didn't have any money to help us, but he helped just by at least being aware that we were there. I was allowed to bring my family because of the length of my stay as a student. The students who



Admiral Withington (at head of table) with Bureau of Ordnance Staff, June 1957.

were there for a shorter time did not bring their families. This was an eventful and happy experience, except for my family problems with a pregnant and not very well wife. I had a very long, interesting, and fruitful connection with Dahlgren in 1930 and 1931.

I have one story about Dahlgren that might be of interest. During our residence there, in postgraduate days, the art of dive-bombing was first being developed. Our child was playing with our dog in the road between our house and the house across the street when a 500-pound water-filled bomb fell about 100 feet away, splashing the child and dog with dirt and uprooting an apple tree. The pilot had to let the bomb go or crash with it, so he let the bomb go, knowing that it would not be in the target area. Fortunately, there was no damage done. The pilot was not very happy about this and neither was I, but such things do occur during the development of new techniques with weapons, and they will continue to occur.

In the earlier days at Dahlgren, one of the primary activities concerned naval aviation. There was a short airstrip which was adequate for the planes at that time. Unfortunately, the airstrip was not adequate in length to support modern

jet aircraft, and the activity no longer exists, so the direct connection between Dahlgren and aviation also no longer exists. I fear that this is not for the general good of the Navy.

In May 1934, you returned to the Navy Yard to work in the Naval Gun Factory until June 1936. Was there any detectable buildup in testing or research and development at that time?

At the Navy Yard, which was really the Gun Factory at that time, there was also a considerable effort in fire control. The Navy Yard was building torpedo directors and gunfire control directors. My job was to see that these rather complicated instruments got built. I know that there was considerable development involved in putting together the components which came from commercial contractors and assembling them with the large castings the Gun Factory made to hold the machinery of the torpedo directors and the gun directors. This was before the time of radar, and the only way to measure range was with the optical range finder. There was very little research and development, as such, in the Navy at that time.

There was a very small research activity in the Navy Yard called the Navy Mine Unit. One man was there—Dr. Duncan. I think he might have had a half-dozen helpers, and he was responsible for developing all the mines for the Navy for many years. This little activity was the seed of the present Naval Ordnance Laboratory at White Oak. But there was very little real activity, certainly not at the Navy Yard, in research or development.

From June 1939 until March 1942, you were again with the Bureau of Ordnance serving in the first Fire Control Section. At that time, was there talk about a west coast test facility for ordnance–what we now know as the Naval Weapons Center at China Lake, California?*

Yes, I was aware that there was an activity being generated in California centering around rockets and their testing. My classmate, McLaren, was concerned very directly in the initiation and development at that place, and through my friendship with him, I was aware of that activity. I've forgotten how many hundreds of square miles are in the Station—almost all desert. This was an enormous undertaking and has proven through the years quite successful.

During the pre-war period, there was a type-desk organization, not the present organization where research is separate from production. The Fire Control Section, which I was in, was responsible for development, production, and other things. Fortunately, through NRA and other sources from President Roosevelt, who smelled the war coming, we had lots of money, more money

*The China Lake facility was started in 1943.

really than we could spend wisely. So three or four of us in the Fire Control Section were working 60, 70, or 80 hours a week, much of the time at home, trying to do everything ourselves. We had no help from the large civilian staff—only very slowly were a few reserve officers being called in. Lewis Strauss, one of the first, later became a Rear Admiral in the Reserve and then the Chairman of the Atomic Energy Commission. He is now deceased, but was one of the most distinguished Americans I've known. The Head of the Fire Control Section was Commander Finley France, now deceased. Since we were concerned with fire control, our connection with Dahlgren was not, in any way, direct. Of course, we knew about the strenuous activity going on there, proving the guns that were being bought in larger and larger quantities, checking out ammunition, and so forth. Dahlgren was heavily loaded and did very important work throughout the war.

After your harrowing duty in the Pacific during World War II, you came back to the Bureau of Ordnance in November 1944, and in October 1945 you became the Officer in Charge of the Naval Ordnance Laboratory. I believe the facility had been moved to White Oak, Maryland, by then. What were your thoughts about the mission of the Laboratory at that time?

This was a very challenging detail for me. I had been at the Bureau of Ordnance only a few months in the Fire Control Production Office when Admiral Hussey detailed me to NOL as Officer in Charge. NOL was still a "stepchild" in the Navy Yard with offices and buildings here and there. The prime facility at White Oak was then under construction and unusable. Before I left the Laboratory, the two houses, which were intended for the Commanding Officer and the Executive Officer, were occupied by myself and Dr. Ralph Bennett, the Technical Director. We commuted, during the rest of my stay as the Officer in Charge of the Laboratory, from those houses to the Navy Yard every day. This took about 45 minutes each way—not a very convenient system.

During World War II, the Naval Ordnance Laboratory had a group of distinguished scientists and engineers who came from the collegiate world, from industry, from everywhere. Ralph Bennett himself was a distinguished scientist from Massachusetts Institute of Technology. At that time, he was in uniform as a Captain in the Naval Reserve. Incidentally, as a civilian appointee, he was the first man to earn $10,000 a year in government service. I believe his rating was GS-16.

My problem, fundamentally, was to retain as many of these brilliant men in the service as we could, and we were not successful with very many of them. In this effort, we developed a mission for the Laboratory which has been copied almost verbatim at the Naval Weapons Center, China Lake. We tried to spell out in simple words what the relation should be between the military comman-

der, his few military assistants, and a large civilian staff under the technical directors. I am still proud of this statement we generated which remains essentially in effect without any change at all. I think this is the single biggest accomplishment I've achieved on any shore duty assignment.

I have really cherished the broadening knowledge I gained at NOL in the civilian community, and I've been better for it ever since. You don't work a laboratory by being a commander, believe me. That's really my whole philosophy, and I hope that my present and active successors will hold the same belief. You don't command a laboratory, you persuade it.

In your 2 years as OIC at White Oak, did you perceive any changes in the Navy Lab concept in which you were involved?

Prior to World War II, there was little or no contact between the civilian scientific and engineering world and the Navy, except through a few contractors such as Ford Instrument Company, the General Electric Company, and a small number of others. Each side was suspicious and uneasy in the company of the other. It is of interest, however, to know that prior to World War II Professor Einstein, then in residence at Princeton, was a consultant to the Bureau of Ordnance. During the war, the Naval Research Laboratory had already been in existence, but the Office of Naval Research then came into being. This was, and still is, the primary contact between the scientific world and the Navy and was a great help in breaking down the barriers of suspicion.

There has been a good deal of criticism of the Office of Naval Research through the years for furthering and funding projects to particular professors and particular universities and colleges about such things as the love life of the mouse. There have been very searching questions asked by people like Senator Proxmire about such things. A few silly things are done, but you can never tell what a good scientist is going to come up with in the way of an idea that may be useful. You've simply got to bet on a good man and hope for the best.

The work done by NDRC* also broke down many barriers. Dr. Tuve, who developed the Navy's influence fuze for rotating projectiles, was one of its most distinguished members. Under NDRC, many others were involved. Some of these, of course, were at NOL, and a few of them were at Dahlgren. One of the most distinguished members at Dahlgren was Dr. Bradbury, who later became Technical Director at Los Alamos in the Manhattan Project. Also, Dr. L. T. E. Thompson spent many years at Dahlgren and later became Technical Director at the Naval Ordnance Test Station in California.

There were many changes in Naval Laboratory concepts. I was involved more or less directly, being the Officer in Charge at White Oak. The Navy

Laboratories really came into being during this period, immediately after the war. The largest laboratory, geographically and also in numbers of people, was at China Lake, but NOL settled down with a considerable complement of civilian scientists and engineers. Indian Head had its first stirrings of interest in doing something besides making powder. At Dahlgren, there was also considerable activity.

You were assigned to the Chief of Naval Operations in June 1949 and eventually became Director of the Atomic Energy Division [Military Applications Division]. What was the role of the Navy Labs in your work then?

There was very little connection between the Navy Laboratories and the Atomic Energy Commission which was then, and still is, the manufacturer of nuclear weapons for the services. The Military Applications Division—I don't know what it's called now—spent most of the money which Congress appropriated for weapons. Most of the money appropriated for the AEC was spent by the Military Applications Division, and this is still true. The major laboratories supporting the AEC were, and still are, at Los Alamos, Sandia Base, and the University of California. Since that time, another one has been developed near Berkeley at Livermore, California.

In December 1953, you were assigned as Deputy and Assistant Chief of the Bureau of Ordnance and later became Chief of BuOrd in November 1954. Since the Navy Labs were under you at that time, what was your philosophy regarding their tasks in supporting the Fleet and possible future consolidation of efforts?

I was hopeful, and I remain hopeful, that the Navy Laboratories can be of crucial importance in supporting the Navy. Essentially all of the production of ordnance material for the Navy is done by civilian activities. Much of the research and development is also done by Navy-supported civilian activities, such as the Naval Ordnance Laboratory at Penn State, the ordnance activity in Seattle, and the Applied Physics Laboratory at Johns Hopkins, which was the birthplace of the antiaircraft missiles—TARTAR, TERRIER, TALOS, etc.

We did not, in my day, foresee the future consolidations of Navy Labs that were to take place. We were too busy trying to attract and maintain adequate staffs at the several laboratories and test facilities. We were hopeful that the laboratories could set the pace for ordnance and its developments and provide guidance to the contractors. We were able, especially at NOL, to do a certain amount of pilot production of such things as fuzes, and the same thing was true at the Naval Ordnance Station at Inyokern. The question of consolidation came up after my time in the Bureau of Ordnance.

The United States was just coming out of the Korean War while you were Chief of the

Bureau, and Dahlgren was easing up a little in proof and testing. Did you get involved in any movements to close the Dahlgren Proving Ground?

This is a very good question. Dahlgren was in danger of dying, and I speak advisedly. I was probably the principal figure in fighting for its survival. I believe this to be true, but I'm not sure, that the rented computer facility was started at Dahlgren at approximately that time. The fastest computer in the world was built for the Navy and was installed at Dahlgren. I believe it was called the NORC. This was around 1955 and for that time was a great achievement.

Dr. Bramble was associated with Dahlgren and with computers at that time, and he was also my teacher at Postgraduate School in mathematics. He was a great mathematician in his own right and taught me to realize how little I know about math. I believe he was intimately associated with the NORC and other computer developments at Dahlgren.

The obvious answer to continuing the Dahlgren Proving Ground was to diversify its activities and to find good people, and I did my best to help. The computer facility greatly assisted in this respect.

There has always been pressure to reduce costs in defense spending. Was there any difficulty in promoting the Navy Lab concept in light of the highly paid scientists and engineers that would be required to perform the work?

There was, and still is, a large group who maintained that government laboratories, per se, were not justifiable and that all research and development could be done either by industry or in the universities. The Air Force, more or less, largely followed this concept. The Navy Lab concept has been difficult to support through the years because of its costs. Then, too, the quality of personnel in the Navy Labs has not always been the best. The employees get old on the job. They cannot be removed under the civil service system, and they are not as bright after 30 or 35 as they used to be. Their ideas don't come forth. I believe in the Navy Labs, but there are defects as well as good things about them.

The big money, of course, does not go into the laboratories. It goes into the purchase of things like the SST prototypes at a couple of billion dollars each. The big money is in the development, not in the research. Parenthetically, the load at Dahlgren in testing is always going to be there. The right people are there. Some don't care for this role. They'd rather see somebody else do it, but nobody else can do it because they don't have the facilities. They don't have the range down the river.

Do you see an advantage or disadvantage in promoting scientific work in the Navy Labs versus private industry?

The scientist is a very interesting man. If he's good, he's going to produce ideas and results, and they may not always be of any interest as far as weapons

are concerned. I remember years ago at NOL we were concerned about mine firing mechanisms, and we came up with a magnificent new method for measuring blood pressure precisely. You never know what a scientist is going to turn up. By the same token, you never know either whether he's awake or asleep when he sits behind his desk with his eyes closed. This man may be in a government laboratory or he may be in private industry; but if he's working for you and he's good, you're just plain lucky. The scientist is also a very temperamental man, as I learned at NOL. One of them said he hated to come to work because he didn't like to get up in the morning, but once he was awake, he was a damn good man.

I don't think it's wise to say that we should attempt to achieve leadership in science in the Navy Labs or in private industry or in the colleges and universities. You ought to try to promote it wherever you can, and certainly you ought to try to keep the quality of the scientific staffs in the Navy Labs at the highest possible level.

A certain amount of cross-fertilization is possible by transferring between Naval Laboratories. For example, Barney Smith spent many years at the Naval Ordnance Test Station in California before he became Technical Director at Dahlgren. This cross-fertilization proved to be very advantageous to Dahlgren. I have the highest opinion of him.

Can you recall any significant people who had a particular impact on the development of Navy Labs?

My list would, of course, not be all-inclusive. I already mentioned Dr. Bradbury, who worked during World War II at Dahlgren before going to Los Alamos. Dr. Teller, the inventor of the hydrogen bomb, has also had a real personal influence on Navy Laboratories. Dr. Thompson is one of the giants in the field. His assistant, in my day as a postgraduate student, Mr. Riffolt, was the scientific staff of Dr. Thompson at Dahlgren. They were it, and they did an enormous amount of work between them. One of the finest men who, unfortunately, recently died at too early an age, was Dr. W. B. McLean at the Naval Ordnance Test Station. He was the developer of the SIDEWINDER missile for which he received a $25,000 award from President Eisenhower. One of the greatest men to ever support naval ordnance was Dr. Lauritsen under whose watchful eye the Naval Ordnance Test Station was established primarily in a large, naked part of the United States where rockets could be tested without killing anybody. He is, also, unfortunately gone. Dr. Bennettt, the Technical Director at NOL in my day, is now, after leaving the government service and being employed by Martin-Marietta and General Electric, more or less retired and operating only as a consultant. Mr. Browning at Indian Head was a great man. He tried his best to make Indian Head effective in research and development.



William McLean (left) receiving Presidential award for SIDEWINDER Missile.

One of the stars at Indian Head on the staff in my day was Dr. Atanassoff. He shot off sparks, so to speak, mentally. Not directly on the staff in my day, but during World War II, was Dr. Ellis Johnson who discovered and remedied the fatal defect in our torpedo exploders which made it impossible for them to go off when they hit Japanese ship holds. He later became the Director of the Army's equivalent to the activity at Norfolk—Operational Development Force. Dr. Weller at NOL was a very valuable member of the staff. I mentioned that Dr. Einstein had been a consultant to the Bureau of Ordnance before World War II started. Another very valuable consultant to the Bureau of Ordnance was Dr. John von Neumann, the great mathematician who was very important in the development of the modern computer.

Rear Admiral Parsons was also a brilliant scientist rather than naval officer, although he had been in uniform throughout his life. He was one of Dr. Oppenheimer's right-hand men at Los Alamos in the Manhattan Project, and was one of the gentlest, ablest, brightest men I've ever known and a great scientist in his own right—a most unusual and useful man.

I could mention others, but I think these men are representative of some of the finest civilian talent which has ever served the Navy.

How long did you serve on the Advisory Board for Navy Labs?

In 1961, I joined the Advisory Board at the Naval Ordnance Test Center at China Lake and spent 5 years there. Later, I was on the Advisory Board of the Naval Propellant Plant at Indian Head, Maryland, and the Advisory Council at the Naval Weapons Laboratory at Dahlgren. Lastly—I just left this one—I was on the Ordnance Advisory Committee under the Naval Research Advisory Council which was concerned with the work of Naval Weapons Laboratory, Dahlgren, and NOL, White Oak.

The Dahlgren and White Oak Laboratories have now merged to form the Naval Surface Weapons Center. Did you see a foreshadowing of this while you were on the Advisory Board?

We did see a foreshadowing of this merger while I was on the Board. I am hopeful that the merger was a good idea and that it will be successful.

In light of our heated competition with the Soviets in the arms race of the 60's and 70's, did you see any changing roles for the labs while you were on the Advisory Board?



Dahlgren Advisory Board, 1966. Front row (left to right): Dr. B. McMillan, Dr. C. C. Bramble, Dr. N. E. Bradbury, Dr. J. W. Johnson, Dr. L. T. E. Thompson, Dr. J. D. Nicolaides, Admiral F. S. Withington. Back row (left to right): Captain H. D. Allen, Mr. B. Smith, Captain J. E. Thompson, Captain W. A. Hasler.

I've given considerable thought to this question, and I'm afraid the answer is "No." The Soviets build very simple, functional equipment. I remember, particularly in my day, going down to Dahlgren to look at a captured 50-caliber machine gun package which was then part of the Soviet aircraft armament. This thing looked so simple that it was almost impossible to imagine it would work, but it did. It was put together with such clearances that any child could assemble it. Instead of clearances in the thousandths, the clearances were in quarter inches or something of that sort. When the plane came back from a mission, the whole assembly, the quadruple machine gun assembly, was dropped down to the ground and a new one hoisted up by the same simple method. As a matter of information, the Russian MiG fighter which was recently landed by a defector in Japan was not nearly the beautiful, magnificent, gleaming, expensive aircraft we expected to see. The material of which it was made looked like old scrap iron. The only thing really fine about it was the engines. It obviously cost half or less what one of our fighter planes would cost, and I'm afraid of our position relative to the Soviets because we overwrite specifications and have done so for years. They obviously strive for simplicity in every sort of a weapon system or ship or airplane or submarine that we know of.

While on the Advisory Board, I asked the Technical Director at NOL how they could justify the prospective $1,000,000 expense of a torpedo. He said, "The target for the submarine costs $100,000,000." I didn't think this was a very good answer, and I still don't think so. The other day, I listened to General Brown, the present Chairman of the Joint Chiefs of Staff, who is not too optimistic about our position relative to the Soviets and thinks we're falling behind. I'm sure we are in the arms race. I asked him if any serious work was being done to reduce the costs of everything we have in the military—ships, tanks, missiles, airplanes, rockets. He said, "Oh, they are so expensive, but we just have to have them and have to pay the bill." But if the Russian fighter is available, such as the one we recently saw in Japan, in numbers two or three times the numbers we can afford, who is going to win the war? The answer, in my opinion, is fairly certain. We are not. I'm sorry to end on such a pessimistic note, but I would hope that the laboratories, specifically the Naval Surface Weapons Center, would fight constantly to simplify specifications, to use standard materials, and to reduce the frightening costs of our weapons. I also hope that it will be possible for them to develop the new lightweight 8-inch gun and produce it at a cost we can afford.

The last action I performed for the Navy Department after retirement was to head a large group that studied the missile system now called AEGIS. I've been informed that the fire control system alone, without any missiles, costs $60,000,000 per ship. This is something I'm afraid we simply can't afford, so what good is it?

APPENDIX

Commanding Officers

Naval Proving Ground/Indian Head Lower Station (Dahlgren)

Jan 1917—Mar 1920	H. E. Lackey	Commander, USN
Mar 1920—Jun 1923	J. W. Greenslade	Captain, USN
Jun 1923—Sep 1923	C. C. Block	Captain, USN
Sep 1923—Nov 1925	A. C. Pickens	Commander, USN
Nov 1925—Sep 1928	H. R. Stark	Captain, USN
Oct 1928—May 1931	H. F. Leary	Captain, USN
May 1931—Jul 1932	G. L. Schuyler	Commander, USN

Naval Proving Ground/Dahlgren

Jul 1932—Jul 1934	G. L. Schuyler	Captain, USN
Jul 1934—May 1936	W. R. Furlong	Captain, USN
Jun 1936—Dec 1938	C. R. Robinson	Captain, USN
Dec 1938—Apr 1941	J. S. Dowell	Captain, USN
Apr 1941—Jun 1946	D. I. Hedrick	Captain, USN
Jun 1946—Aug 1949	C. T. Joy	Rear Admiral, USN
Sep 1949—Jun 1951	W. A. Kitts III	Rear Admiral, USN
Jul 1951—Jun 1952	I. T. Duke	Rear Admiral, USN
Jun 1952—Jun 1956	J. F. Byrne	Captain, USN
Jul 1956—Sept 1956	R. D. Risser	Captain, USN
Sep 1956—Aug 1957	G. H. Wales	Captain, USN
Aug 1957—Oct 1957	R. D. Risser	Captain, USN
Oct 1957—Aug 1959	M. H. Simons, Jr.	Captain, USN

Naval Weapons Laboratory

Aug 1959—Mar 1960	A. R. Faust	Captain, USN
Mar 1960—Aug 1961	T. H. Morton	Captain, USN
Aug 1961—Jun 1964	R. F. Sellars	Captain, USN
Jul 1964—Sep 1964	G. G. Ball	Captain, USN

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Sep 1964—Jul 1968	W. A. Hasler, Jr.	Captain, USN
Jul 1968—Jul 1969	J. D. Chase	Captain, USN
Jul 1969—Jan 1972	S. N. Anastasion	Captain, USN
Jan 1972—Aug 1972	J. H. Burton	Captain, USN
Aug 1972—Jul 1973	R. F. Schniedwind	Captain, USN
Jul 1973—Sep 1974	R. B. Meeks, Jr.	Captain, USN

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