JULY 1973

REPORT OF
NRL PROGRESS

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Fifty Years of Science for the Navy and the Nation

NAVAL RESEARCH LABORATORY
Washington, D.C.

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DEPARTMENT OF THE NAVY
NAVAL RESEARCH LABORATORY

Fifty Years of Science for the Navy and the Nation

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PREFACE

How does one capture the essence and drama of a half century of research? Of the contributions of ten thousand scientists and technicians working together? Of the dreaming and doing that lead to the launching of a submarine or a satellite?

A list of patent numbers, however important, would convey little meaning; a collection of report titles would be only slightly more enlightening. It takes the intertwining of men and materials, their reminiscences their frustrations, their flashes of foresight, and their long, long years of plain persevering to give life to real research in a real world of wars, rumors of wars, and the briefly interspersed respites of peace.

This issue of the "Report of NRL Progress" provides brief glimpses into the world of research through the lenses of the lives that have been part of NRL. The scope of NRL's research has reached far across the range of human interests and needs and is as varied as human beings themselves are different. In recording this historical perspective, it is hoped that humanity is reflected rather than just the material objectives of mankind's search. The Navy exists for the people, and naval research involves the interplay of some of mankind's best minds with mankind's highest hopes.

Selecting 50 writers to breathe life into 50 years of history is not an easy task, especially when ten times that number could have written on subjects of enormous fascination. It was hoped that the past and the present could be presented from the viewpoints of scientists and administrators, with sufficient coverage of eras and areas of research to provide both a perspective and a panorama.

A number of distinguished alumni could not respond because of personal commitments, and as a result some proposed areas of coverage do not appear. The reader who has been associated with NRL will construct his own mental list of omissions based on his own perspective and thus fill out this document to his personal satisfaction. Perhaps it is best that way; otherwise this report could never be completed satisfactorily.

The problem of how to arrange the essays in a meaningful order was resolved simply, and for the most part quite satisfactorily, by adopting a chronological sequence based on the author's year of "entering on duty" at the Laboratory. There is one exception. Dr. Berman's "View from a Vantage Point" appears last. Appropriately, that vantage point is the office of the Director of Research.

Very little editing has been done on the original manuscripts, and then mostly to supply a date, insert a reference, or construct a title when none was supplied by the author. If some duplication in reporting or similar scientific philosophies appear, it should be remembered that in spite of NRL's diversity, it has been an integrated and interdisciplinary laboratory, and its family, while large, is closely knit. But dedicated people will always have their honest biases, as well as strong convictions, and these cannot help but show through. It would be unrealistic and even undesirable to wish it otherwise. The creative mind is not easily catalogued.

As the Laboratory moves into its second half-century of research and service, it is a healthy exercise to reflect with a few of the many proud and dedicated people who have contributed to NRL as it is today. It is the people who are the muscle, bone, and blood of any corporate institution...that is the message of this special issue of the "Report of NRL Progress."

J H Schulman

JAMES H. SCHULMAN
Chairman,
50th Anniversary Committee

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The Laboratory in its first year of operation

The Laboratory today

Fifty Years of Science for the Navy and the Nation

On the second day of July 1923, the Assistant Secretary of the Navy, Theodore Roosevelt, Jr., delivered his dedication address at the commissioning ceremonies of the Navy's Experimental and Research Laboratory. This facility, soon renamed the Naval Research Laboratory, is on the Navy's Bellevue Magazine site beside the Potomac River in southwestern Washington, D.C. Roosevelt stood on the steps of one of the three buildings which then comprised the Laboratory and at approximately the same spot where two and a half years earlier the then Secretary of the Navy, Josephus Daniels, had broken ground for that building's construction. Daniels had an early concept of this facility, and in 1915, he enlisted the aid of Thomas Alva Edison, "the one man above all others who can turn dreams to realities," to chair the Naval Consulting Board of the United States and help implement the Laboratory's establishment. The board members and chairman reputedly had some of the most outstanding scientific minds in the nation; hence initial approval of the Laboratory's construction was practically assured. But there were severe differences of opinion among the board members as to what the facility should be, what it should do, and where it should be located; these differences had long delayed commissioning of the new Navy laboratory.

Roosevelt undoubtedly was aware not only of these differences but of an agreement among the Founders, which, as he had stated earlier before a Congressional committee, was that the Laboratory would be "an instrument whereby Navy men are encouraged to work constantly for advancement in Naval science." His unrecorded speech, therefore, may well have been tempered by a statement to that effect as well as by words dwelling on another point on which there surely was mutual agreement—that regardless of the initial guidelines, the achievements to be made at this facility for the Navy and the Nation would rest almost entirely upon the vision, resourcefulness, and dedication of the men assigned to it.

How rewarding would it be to those Founders if they could view the present NRL complex and staff? Instead of three buildings, they would see more than 100, most of them containing highly specialized laboratories and research devices. They would see a staff 200-fold larger than the original complement of 20 and would note that these people were applying disciplines of nearly all of the physical and related sciences to the solution of important naval problems. They would perceive contributions being made in fields unheard of when the Laboratory was commissioned. And they would notice how this Laboratory had grown so extensively from the modest beginning they had provided a half century earlier.

FOUR GROUPS OF SCIENTISTS ARRIVE

In April 1923, Dr. A. Hoyt Taylor moved three groups of scientists from the Bureau of Engineering, including men from the Aircraft Radio Laboratory, to the new Laboratory to form the Radio Division. Concurrently, Dr. Harvey C. Hayes brought in a fourth group which had been continuing, at the Annapolis Experiment Station, the underwater sound investigations it had conducted originally at New London, Connecticut, during World War I; in its new home it was called the Sound Division. Thus, 2-1/2 months before the commissioning of the Laboratory, the 20 people who initially comprised NRL's Radio and Sound Divisions had moved into their new quarters and resumed the same projects they had been working on for the Bureau of Engineering at their previous locations. Although the scientists were now under the direct administration of the Secretary of the Navy, they continued their research for the Bureau because they believed in its importance to the Navy and because the Bureau, having faith in the scientists, continued to fund their efforts. The Bureau was conscious of the many deficiencies in Fleet capability demonstrated in World War I and was anxious to overcome them through the application of scientific research.

It was not long before the Laboratory had demonstrated the superiority of a 50-kilowatt high-frequency (HF) transmitter over a 250-kilowatt long-wave transmitter in providing better communications between the Chesapeake Bay area and Balboa, Canal Zone. This demonstration overcame many of the prevailing prejudices against HF communications. It was quickly followed by successful HF transmissions over great-circle distances of about 10,000 miles from the flagship SEATTLE on her visit to Australia and

New Zealand in 1925 and the transmission of all required messages between Washington, D.C., and the rigid airship SHENANDOAH on the dirigible's cross-country flight in 1924. These and other early successes earned wider funding support for research that greatly increased the communications capability of the Fleet in the years ahead. They also led to related investigations that enabled the Radio Division, 16 years after its establishment, to make what Fleet personnel termed "the greatest radio discovery of the age," radio detection and ranging (radar), which enormously increased Fleet security and navigation capabilities.

In the meantime, the Sound Division was making great strides in underwater communications and detection. Working in a complex and nearly uncharted area, the Division undertook the development of a sound system that included ultrasonics to give both range and bearing by means of echoes from a target. The primitive state of knowledge in this area at that time is illustrated by the fact that there was absolutely no means of making quantitative measurements of underwater-sound intensities; evaluations of ultrasonic systems under development were made primarily by comparative measurements of obtainable operating ranges. Attempts to correlate theoretical and experimental results were frustrated by the large variable effects of then unknown underwater phenomena, such as stratified thermal and organic layers. These numerous unknowns provided a fertile field for the program of acoustic research that would follow.

The development of quartz-crystal transducers, vigorously pursued in the period 1924-1928, gave way to work on synthetic piezoelectric crystals, chiefly Rochelle salt, and concurrent research on the magnetostrictive type of transducer. The latter studies provided the technical basis for the development, by a Bureau-sponsored contractor, of a complete magnetostriction sound system which became standard equipment aboard destroyers. Continued work by NRL on the Rochelle-salt transducer resulted in the development of a new sonar system, known as the XBQ, which had many applications, particularly to depth sounding by surface ships and to listening systems aboard submarines. By 1935, several fairly adequate sonar systems had been developed, and quantity production of improved models got under way by 1938.

THE NEED TO EXPLORE NEW DISCIPLINES

Much of the work performed by the two charter divisions was done in a developmental context, so it greatly facilitated establishing the Laboratory's practical value to the Navy. The progress of the Radio and Sound Divisions was made in two established and relevant fields of research which the Navy was already supporting when the Laboratory was formed. It is clear now, however, that the scope of the scientific disciplines embraced by the two charter divisions was not nearly wide enough to adequately support the rapidly growing research needs of the Navy. The Division of Heat and Light was established in 1924, mostly on the basis of the intuition of the scientists and the Director's faith in them. It was the first of a number of divisions to be added to the new Laboratory. Each would make significant contributions to Navy strength through applications of its own scientific disciplines, and collectively those divisions would provide scientific support far greater than the sum of their individual efforts.

A principal strength of the Laboratory through the years has been its many mutually supporting disciplines. Thus, even when only the two charter divisions existed, both were conducting basic research and exchanging information on wave propagation. Later, when the Radio Division was investigating the eccentricities of the ionosphere and its influence on radio waves, Dr. E. O. Hulburt, head of the newly formed Heat and Light Division, furnished all of the early calculations concerning the ion densities and the refractive properties of the ionosphere. When the Radio Division discovered the "skip-distance" effect, which could not be explained by the prevailing wave-propagation theory, Dr. A. Hoyt Taylor of the Radio Division and Dr. E. O. Hulburt of the Physical Optics Division (a derivative of the Heat and Light Division) jointly published in 1926 a modification of the theory which laid the foundation for modern HF wave-propagation theory. These early interactions set the stage for the many interdisciplinary efforts that have enhanced Laboratory achievements ever since.

A principal difficulty encountered in getting the Heat and Light Division under way had been that it needed to conduct much fundamental research before it could help resolve problems of interest to the material bureaus. The Division's first important program was to undertake extensive measurements of the transmission and absorption of all optical wavelengths, including infrared and ultraviolet. These measurements were carried out through varying degrees of fog, snow, rain, and other atmospheric conditions and also through seawater. From the scientists' point of view, study of these physical phenomena and environments was indispensable to the solution by the Radio and Sound Divisions of many bureau problems to be assigned in the future. Thus the potential broad application of the results to many fields justified direct Laboratory support, independent of the Bureau of Engineering, for the work of the Heat and Light Division. Later, when the Chemistry Division was established, its head, Dr. F. R. Bichowski, made an informal stipulation that one-third of the money made available to it should be devoted to publishable research of a so-called "pure" type. Although rigid guidelines were never applied, the direct appropriations have been vital to the Laboratory in permitting a flexible and productive blending of basic and applied research.

ON THE EVE OF WORLD WAR II

When the United States became involved in World War II, the wisdom of creating the Naval Research Laboratory and the effectiveness of its contributions to the combat strength of the Navy would soon become apparent. On the eve of the conflict in 1939, the Laboratory was composed of the following Divisions: Radio, Sound, Chemistry, Physical Optics, Metallurgy and Thermodynamics, Mechanics and Electricity, and Interior Communications. Already, each had made significant contributions to the Navy; many of these would be tested quickly in a realistic wartime environment.

Among the contributions made by the Radio Division were a worldwide HF communications capability and radar for ranging, detection, and navigation. An important NRL invention, the Plan Position Indicator, greatly improved the tactical use of radar data. By December 7, 1941, 20 radar sets had been placed aboard selected ships, including battleships, cruisers, and aircraft carriers, as well as at key shore installations.

The Sound Division had contributed substantially to production models of sonar systems that had been installed widely in the Fleet and that would become the standard sonar equipment for the Navy's surface and subsurface vessels throughout World War II.

The Chemistry Division was already making progress in the development of antifouling paints for ship hulls and aircraft pontoons, and it was finding suitable noncorrosive additives for fuels and lubricating oils.

The Physical Optics Division had determined the best type of protective coloring for all classes of naval vessels and had written the first edition of the "Handbook of Camouflage," which became a "Bible" on the subject and was carried aboard all ships throughout the War. The development of low-reflecting films for coating glass surfaces of optical instruments had enabled the Navy to increase the efficiency of many instruments, including that of submarine periscopes, which rose from 50 to 85 percent.

The Metallurgy and Thermodynamics Division's development of nondestructive techniques for testing castings had done much to improve their manufacture and to reduce failures during operation. Now these techniques were being applied to the nondestructive testing of welding, which was largely replacing riveting in the gigantic shipbuilding program just beginning.

The Mechanics and Electricity Division had made precise analyses of the force-upon-impact and the penetrating power of various types of projectiles and of the resisting power and effectiveness of different kinds of armor for ships and aircraft.

The Interior Communications Division was testing electrical equipment to be used in the increasingly complex interior communications systems of Navy ships.

The examples cited above were only a few of NRL's many efforts that contributed to Navy effectiveness by the beginning of World War II and that gained Fleet-wide respect for the Laboratory.

CONTRIBUTIONS DURING THE WAR

With the onset of war, it became crucial to the Navy and the Nation that the Laboratory's scientific force gear itself to the rapid pace it would have to maintain. Some preparations for such an emergency had been made, but the Laboratory, like the rest of the Nation, was unprepared for the sudden onset of war. Its subsequent "growing pains" are reflected to some extent by the expansion, between 1941 and 1945, from 396 personnel to 2069 (about 4400 including students and transients), from 23 buildings to 67, from 1.7 to 13.7 million dollars annual expenditures, and from 200 to nearly 900 problems assigned to the scientific divisions. This rapid expansion was achieved only after tedious struggles to obtain from a very tight market the best-available scientific, engineering, administrative, and support personnel as well as essential materials and equipment. This unprecedented build-up resulted from the realization by the military and the Nation that World War II would be the most technical war fought to date.

Communications, Radar, and ASW

To their earlier contributions to the "finest Fleet communications in the world," the Radio Division added electronic navigation devices and improved radio communications equipment for air, surface, and submarine use. It launched the Navy into the technologically new electronic-warfare arena by quickly developing countermeasures to the unexpected German radio-guided aerial bomb which threatened allied shipping in the Mediterranean. The Division also developed a passive search radar for use by surfaced U.S. submarines to thwart radar searches which were being conducted successfully by Japanese antisubmarine-warfare (ASW) aircraft. NRL's expansion of radar capabilities to include the vital fire-control function received credit for saving a U.S. task force in the Second Battle of Savo Island in 1942.

The sonar systems that were developed by NRL before the War and that were installed as standard equipment on Navy ships during the War proved to be valuable contributions to Fleet ASW effectiveness. These systems were one of a group of scientific devices that Grand Admiral Karl Doenitz, commander of the German U-boat fleet, cited as the major reason for Germany's defeat in the battle of the Atlantic. During that battle, the Sound Division was not only improving these sonar systems but was applying its scientific understanding of underwater phenomena to the development of many other important devices. They included a sound generator that successfully countered German acoustic torpedoes designed to "home" on propeller sounds of Allied ships, three types of acoustical proximity fuses for mines and other explosives, and a powerful low-pitched sound generator that proved useful in mine sweeping. Sound Division scientists also pioneered the development and use of equipment which emitted very short ultrasonic signals to assist in locating small objects. This equipment proved helpful in guiding U.S. submarines through Japanese minefields and in safeguarding U.S. minesweepers.

Added to the many significant accomplishments made by the two charter divisions during the War were many more made by other divisions that had come into being since the founding of the Laboratory.

Chemistry

Working closely together, the Chemistry Division and the Army Chemical Warfare Service developed a variety of materials and devices that were used successfully by both the Army and the Navy during the War. They included chemical weapons, high-efficiency filter materials that were used in all Navy gas-mask canisters, and a defogging agent for optical surfaces to facilitate operations during adverse weather conditions. The Division also developed coatings that protected all types of military electronic equipment from rot, mildew, and rust in the humid tropic regions. The Navy's submarine-hunting capability was improved by the development of an antifouling film for sonar domes. Methods were devised to prevent the spoilage of jet fuels in storage, liners were designed and fabricated to permit storage of fuels in concrete tanks, and additives were discovered that provided adequate oxidation-stabilization and rust-inhibition characteristics to aircraft lubricants.

Many military men who survived the sinking of their ships or downing of their aircraft will remember the NRL-developed "Sea Marker," a fluorescent dye that brightly covered a small area of water near them and thus aided in their rescue. They will also remember the "Shark Chaser," a specially prepared chemical compound that discouraged sharks from attacking them. NRL chemists developed a method of introducing foam into the bottom of burning fuel tanks to extinguish fires. An NRL-developed "closed-cycle rebreather" was widely used in submarines during and after the War and saved the lives of many crewmen in closed atmospheres made unbreathable by fires. Among the Division's contributions to ASW were devices which, upon ejection into seawater, produced either hydrogen bubbles to confuse enemy sonars or ammonia bubbles that collapsed and made noises that diverted enemy acoustic homing torpedoes away from their targets. Many of the accomplishments of the Chemistry Division, cited sparsely here, provided industry with immediate or potential products for later commercial applications.

Physical Optics

At the beginning of the War, the Physical Optics Division, formerly the Heat and Light Division, disseminated valuable information it had accumulated through research over the preceding 15 years to scientists in industry throughout the United States. Application of this information by industry resulted in the development of radiometric devices that measured the radiant intensities of clouds, clear skies, ships, aircraft, snorkel exhausts from submarines, emissions from flame throwers, rocket-motor flames, and hot surfaces in ships' firerooms. The Division devised methods for testing models of a British-developed infrared detector tube that provided good images of infrared-illuminated objects, and it then inspected thousands of the tubes, which were used later by the Army during the invasion of North Africa and by the Fleet for secure intership communications. The finding by the Division that red light does not decrease the eye's adaptation to darkness, as does blue or white light, resulted in an immediate change throughout the Fleet to the use of goggles, flashlights, and other optical devices designed to receive or emit red light. The Division also produced self-luminous radioactive buttons, tapes, and other markers for identifying ladders, hatch openings, deck obstacles, and switches. Fluorescent materials excited by ultraviolet light and phosphorescent materials which glow for some time after the light stimulation has been turned off were developed for visual aids. Two examples are the fluorescent coveralls and signal flags that made Navy crewmen visible to aircraft pilots at much greater distances than had been possible before and thus facilitated the direction of takeoffs and landings. The new scientific discipline of electron optics was established and used by the Division to align some 90-million quartz crystals desperately needed for crystal oscillator plates during the War. The method involved the use of x-ray equipment and proportional counters to align the crystals by means of their x-ray diffraction patterns. This notable accomplishment was only the beginning of many subsequent applications of electron and x-ray diffraction methods to materials analysis, electron microscopy, and upper-atmosphere research.

Metallurgy

The Physical Metallurgy Division effected fundamental improvements in metals and metal products, which were of significant benefit to the Navy and to industry in general. Research by NRL metallurgists on welding techniques resulted in timely improvements in this new art, which was rapidly replacing riveting methods in shipbuilding. They developed better weld metal from core-wire and electrode coatings research, devised a test to determine the ductility of welds, and constructed an automatic welding machine. Improvements also were made on prewar techniques and instruments for nondestructive testing of metal castings, plates, and welds; for example, a device developed by NRL to generate beta rays was adapted to reveal (in a short exposure time) flaws in steel plates many inches thick. NRL metallurgists found that nickel was a very satisfactory substitute for tin in making bronze. They also demonstrated that an excellent quality of manganese bronze could be made by using scrap cartridge cases as alloy material, and they developed methods for improving the casting of alloy metals. This wartime reserach was valuable not only to the U.S. war effort but to the subsequent economy of the Nation and to the development of the science of metallurgy worldwide.

Atomic Energy and Electricity

Of all of the projects undertaken by the Mechanics and Electricity Division, one of the most significant was the investigation of atomic energy. It is a matter of record that NRL was the first U.S. Government agency to undertake the study of atomic power. In 1939, NRL scientists became convinced that a process for uranium fission, discovered by two German scientists, offered a potential method for obtaining atomic power, and they drew up a plan for its application to submarines, which contributed to the development of the first atomic-powered submarine. Furthermore, by 1942, NRL had constructed a small pilot plant that proved successful in separating uranium isotopes in small quantities. Later, a larger plant constructed at the Philadelphia Navy Yard under the auspices of the Bureau of Ships, became a branch activity of the Division. The NRL "liquid thermal diffusion process" for the separation of uranium isotopes was one of the three methods that scientists and engineers of the Manhattan Project used to obtain the vital chemicals which formed the explosive in the first atomic bombs.

The Mechanics and Electricity Division also started an investigation of precipitation static and coronal discharge which interfered with radio communications attempted by aircraft flying through snow, thunderstorms, and other foul weather. Because of the increasing stubbornness and significance of the problem, NRL created the new Aircraft Electrical Division in 1944 to solve it and to absorb most of the electrical work of its parent Mechanics and Electricity Division. The precipitation static problem was solved by the use of metallized wicks, about the size of small rope, with frayed ends, fastened to the trailing edges of aircraft wing and tail surfaces. The devices eliminated approximately 90 percent of the static caused by coronal discharge, and they soon were installed on aircraft throughout the world, increasing flight safety for aviators, military and civilian, and millions of passengers.

Interior Communications

Although the work of the Interior Communications Division was less scientifically oriented than that of the other research divisions, it nevertheless proved very valuable in the development and testing of electrical and other equipment used for communications and related purposes within ships of the Fleet. This equipment included voice communication systems and speakers, alarms, repeaters for transmitting information to dials and indicators from the bridge to the engine room, circuits for controlling gun batteries, and circuits for running lights and interior illumination. One offshoot of this work was the identification of a new field of investigation that involved the systematic evaluation of the effects of shock and vibration on ship structures and equipment. Through this work, methods were devised for producing superior shock- and vibration-resistant designs for these components.

POSTWAR RESEARCH

With the War over, NRL could look with pride on its performance during the conflict but could not affort to rest on the acclaim that performance received. It was time to reorganize and adjust to the new environment and challenges that lay ahead. The Laboratory was transferred administratively in May 1945 to the newly created Office of Research and Inventions, renamed the Office of Naval Research a year later, and expedient wartime planning was replaced in large part by long-range planning.

Although NRL had been looked upon primarily as an electronics laboratory, partly because of its spectacular successes in radio and radar, its administrators believed it could best serve the Navy and the Navy's material bureaus by exploring a much wider range of science and technology. Consequently, all divisions were placed on the same organizational level. New scientists and engineers were sought on the basis of highest quality rather than on the basis of what the limited manpower market could provide, which had been the case during the War. An important precept of the Founders—that the scientific and technical work be carried out by civilian personnel, while uniformed personnel performed the management and liaison functions—was firmly restated. These policies formed the basis for the postwar reorganization and provided a more balanced and effective research program for the long-term attack on problems in important and complex areas yet to be investigated.

Postwar research was conducted in a rapidly changing environment characterized by technological surprise and unprecedented expansion of scientific knowledge coupled with keen international competition. Researchers were presented increasingly difficult challenges to obtain knowledge of the undersea, sea-surface, atmospheric, and space environments, all of which would affect the Navy's future global operations. For example, the postwar development of powerful and sophisticated weaponry and nuclear powered submarines presented severe technical problems that had to be solved to conduct effective submarine and antisubmarine warfare; the surprise launching by the USSR of SPUTNIK 1 greatly accelerated NRL's space systems research and development efforts; the introduction to modern warfare of the new concept of low-level conflict as well as the Korean and Southeast Asian limited wars placed existing requirements in a new light and added new ones; and the sophisticated means developed by hostile nations for intercepting or interfering with our communications stimulated large Navy programs of countermeasures research.

These technological developments and others, made both by military and nonmilitary groups, had profound impacts on NRL's postwar research. The development of the electronic computer and modern data-processing techniques not only challenged NRL scientists to adapt them to Navy use but also to exploit their power in facilitating the conduct of research and expanding research goals to previously unattainable dimensions. In 1951, the invention of the junction transistor ushered in the new era of solid state electronics, which revolutionized the entire electronics field. NRL scientists took advantage of this challenge and significantly reduced the size, weight, and cost of electronic components while concurrently increasing their reliability. Other revolutionary advances such as the development of the laser, which provided coherent, spectrally pure, and powerful beams of optical radiation—added new dimensions to old techniques, made entirely new techniques possible, and opened the door to previously unanticipated areas of research.

Research expanded from both a fundamental and an integrated viewpoint. Where existing theories or scientific descriptions proved inadequate to explain newly observed phenomena, intensive investigations were carried out to refine them and to gain a more comprehensive understanding of their meaning. On the other hand, further emphasis was given to the development and operational integration of systems. Consequently, the products of separate but closely related research had to be brought to bear more effectively on relevant Navy problems.

New or newly emphasized fields of science and engineering were embraced in order to provide the comprehensive interdisciplinary capabilities needed to attack new types of problems as they arose. To meet these burgeoning obligations, the Laboratory procured many large and powerful research facilities. Today, NRL's 20 charter scientists would undoubtedly be amazed to see the Laboratory's Cyclotron, Linac, and Van de Graaff accelerators being used to examine the basic constituents of matter; observe the sophisticated acoustics research carried out aboard USNS HAYES and the deep-ocean search and monitoring operations conducted aboard USNS MIZAR; view the Nation's most accurate transducer calibration and acoustic measurement facilities at NRL's Orlando, Florida, facilities; fly on one of the Laboratory's several scientifically instrumented aircraft to obtain remotely sensed information about the sea and the atmosphere; and visit the Chesapeake Bay facility and take part in over-the-horizon radar experiments.

Research had become a very important and expensive national need. The Executive and Congressional branches of government scrupulously analyzed research management, planning, and justification. Section 203 of the Defense Appropriation Act of 1969 required that all funds expended by the Department of Defense for research and development be used for programs which have "direct and apparent relevance to military systems." Accordingly, the Laboratory undertook a broad and penetrating reassessment of the role, scope, and objectives of its research program. The results showed that the bulk of NRL research, although contributing significantly to the storehouse of scientific knowledge and to the welfare of the Nation, could easily be demonstrated to be of "direct and apparent military relevance."

ESTABLISHMENT OF FOUR AREAS OF RESEARCH

At the close of World War II, the number of NRL's research divisions had increased to ten. A major part of the postwar management effort was directed to integrating the research without destroying the individuality of each research division's interests. A position was established for a Director of Research, and, in the 1950's, several Division Superintendents were designated Associates to the Director of Research. It was soon evident that a stronger integration and direction would be needed for the Laboratory to be adequately responsive to the advanced research management policies being adopted by the Department of Defense and for it to exploit the mutual support that could be provided by divisions having strongly related scientific disciplines. Consequently, the Research Department was restructured through several phases into the present organization, which encompasses the four areas of Electronics, Materials and General Sciences, Space Science and Technology, and Oceanology; and their 17 divisions and several special laboratories and groups.

Descriptions of some of the postwar activities of these organizations are given below under the technical Areas in which they are now grouped.

Electronics Area

In the postwar period, NRL quickly joined the revolution in electronics caused by the introduction of solid-state physics technology. Interest in this new field coincided with an unprecedented demand for the application of electronics to military systems. Basic and applied research and development programs were directed to better understanding and exploiting the electronic properties of solid materials, electron beams, and microwave radiation. The subsequent attention given to high-power microwave devices led to major contributions in the development of equipment for the improvement of communications, radar, and electronic warfare. Miniaturization techniques were developed which reduced the size, weight, and cost of components of microwave and other systems while at the same time increased their reliability. High-power traveling wave tubes were developed, and metal-oxide semiconductor transistors were hardened against radiation damage to improve their reliability for operation in the space environment.

Shortly after the War, Navy radars, so essential to victory in that conflict, had become outmoded. Rapidly increasing military requirements of the postwar period dictated that a vast array of improvements and innovations be made. Due in part to NRL's postwar research and development efforts in radar, the Navy's capabilities in surveillance, navigation, and weapon control were improved greatly in such areas as Fleet air defense, strike warfare, ocean surveillance, and antisubmarine warfare. Some of the major postwar radar developments that can be mentioned involved monopulse tracking, airborne weapon control, satellite surveillance, airborne early warning, and over-the-horizon surveillance. Important advances also were made in detection techniques, high-resolution methods, the understanding of sea clutter and propagation media, and the definition of target characteristics.

The need for improved communications became much more demanding in the postwar period. More information had to be transferred more rapidly, reliably, and securely. HF radio communication, which NRL had introduced to the Navy long before World War II and which, since then, continued to serve as the Fleet's primary means of communication, was significantly upgraded in capacity, reliability, and security by devices and techniques developed by the Laboratory after the War. These developments included shipboard broadband HF antennas and transmitter multicouplers and the techniques and equipment for providing a common base for worldwide dissemination of time, time intervals, and frequency. To improve the security of Fleet communications, NRL developed, in the immediate years following the War, an on-line electronic encryption system. This achievement was closely coupled with the Laboratory's role in the development of stabilized-frequency, single-side-band trnasmitters and receivers for communication circuits. NRL also developed the processor which permits current naval platforms to be adapted readily to receive broadcasts transmitted through the Fleet Satellite Communication System. Other research has resulted in major contributions to worldwide systems for the identification of friend or foe and to navigation systems.

With the nuclear submarine and the subsequent expansion of its role and importance, more positive control of submarine operations was required. At the lower end of the electromagnetic spectrum, therefore, NRL's attention was directed largely to providing effective radio-wave propagation underwater. Resulting developments have greatly increased the capacity and reliability of submerged-submarine communications.

Electronic warfare essentially began in World War II with the successful thwarting of German radio-controlled aerial bombs and Japanese ASW search radars. In postwar research in electronic warfare NRL helped the Navy to lead the way in the highly competitive programs conducted by many nations. Early developments included a still widely used superheterodyne receiver for detecting and recognizing threat signals and the pulse deception repeater, which is the backbone of modern defensive electronic-warfare systems. NRL played a major role in the development of the Navy's worldwide HF direction-finding system, and it made a significant contribution to the concept of automatic threat recognition and preprogrammed reflexive response systems. In addition to satisfying long-term electronic-warfare needs of the Navy, NRL assisted in solving such urgent problems as devising and installing special electronic defensive devices on Fleet units operating in Southeast Asian waters. In fulfilling its responsibilities as the Navy's lead laboratory for exploratory development in electronic warfare, NRL had conducted an extensive electronic-warfare program which includes the use of unique facilities to conduct complex, computer-assisted simulations of potential battle situations.

Among NRL's contributions that have been of great value to civilian as well as to military interests is the recent development and use of high-resolution imaging radar for mapping oil spills and the application of over-the-horizon radar to the measurement of weather effects at distances of 1000 miles or greater.

Materials and General Sciences Area

During the years of reorientation and growth following the War, major advances were made in methods of investigating and increasing the knowledge of materials. Larger numbers of researchers at the Laboratory looked more deeply into the basic properties of matter in efforts to understand and exploit the atomic-scale structures and phenomena which determine the macroscopic characteristics and behavior of materials. This trend was made possible largely by a growing arsenal of increasingly more powerful probes, techniques, and facilities. They included the electron microscope, a variety of diffraction, interference, and scattering techniques, new research approaches made possible by the revolutionary invention of the laser, more powerful theoretical methods, and the sophisticated design, control, collection, and analysis capabilities afforded by electronic computers. These developments have substantially affected and shaped virtually all of the programs of the research groups comprising today's Materials and General Sciences Area.

The many contributions to Fleet capability made in chemistry during the War were augmented during the postwar period by a number of productive advances in new fields and in exciting new directions in well-established disciplines. Advanced investigative techniques have provided new understanding of combustion phenomena and reaction kinetics across a broad front ranging from fire suppression to chemical lasers. Studies of the surface characteristics of various materials and the synthesis of new compounds have led to the probing of the mysteries of structure-property relationships in search of substances that will serve to improve coatings, adhesives, and lubricants and that will satisfy a host of other practical needs.

In metallurgical research, the Laboratory has retained its position at the forefront of fracture mechanics testing, principles, and practice. Efforts to increase the strength and toughness of new alloys have been made across a broad technical front that ranges from investigations of microscale radiation damage and holographic studies of crystal growth to the formulation of scaling laws for fracture testing and welding techniques.

NRL scientists working in the new field of solid-state physics have probed the fundamental secrets of optical, electrical, magnetic, and structural phenomena of crystalline and amorphous solids. They have explored excited-state and far-infrared spectroscopy as well as new detectors and information-storage systems. These research programs have led the way from fundamental understanding of solid-state phenomena to new and improved applications.

In optical sciences, increased emphasis has been placed on programs oriented directly toward satisfying existing and anticipated Navy needs. Laser phenomena, which have been at the core of this interest, have stimulated studies of light generation, propagation, and interactions across a broad range from the fundamentals of quantum mechanics to the identification of system needs.

Fundamental investigations in nuclear science have been supplemented in recent years by interdisciplinary studies that have drawn heavily and profitably on the postwar investments in NRL's specialized techniques, experience, and facilities. In ion-implantation and radiation-effects studies as well as in preparations for a neutron cancer therapy program, NRL nuclear scientists have blended their special talents and techniques with those of many other disciplines pursued at the Laboratory to open powerful new investigative avenues.

Space Science and Technology Area

Shortly after World War II, NRL was one of several institutions that undertook programs of upper-air research with the aid of captured German V-2 rockets. On October 10, 1946, an NRL team was the first to send a spectrograph aloft by rocket and thereby to obtain the solar spectrum below the wavelength limit set by ozone absorption. During subsequent rocket flights, NRL recorded the entire solar spectrum to the x-ray limit and contributed significantly to understanding of the structure of the ionosphere. The Laboratory performed about 80 high-altitude experiments aboard V-2 rockets and later conducted investigations by means of Viking, Deacon-Rockoon, Nike-Asp, Aerobee, Aerobee-Hi, Black Brant, Superchief, and Blue Scout Junior research vehicles.

The launching by the USSR of the first man-made earth satellite, SPUTNIK I, stimulated considerable growth in the U.S. space program, including Navy space research. NRL scientists sought to explore and better understand space science and technology in order to meet present and future Navy needs, particularly in the areas of communications, navigation, environmental monitoring, and surveillance.

Communication satellites have greatly assisted the Navy in meeting its worldwide responsibilities. Satellite communications experiments have been conducted successfully between destroyers during ship maneuvers, during takeoff and landing operations aboard an aircraft carrier, and between shore stations and Navy aircraft. Satellites also provide unique capabilities for navigation, and the Navy has used Transit satellites to good advantage for many years. Recently, the Department of Defense has been evaluating an improved navigation system, the Defense Navigation Satellite System, which is designed to meet the ever-increasing navigational requirements of all the Services. The Navy's candidate for this system is a constellation of Timation satellites developed by NRL.

Because navigational aids provided by satellites are affected by changes in the ionosphere, as are other radio transmissions, ionospheric monitoring is an important part of the Navy space program. The ionosphere is influenced by solar-radiation processes and thus cannot be comprehended fully without a thorough understanding of solar interactions with the earth's atmosphere. For a similar reason, the successful use by the Navy of the important radar technique of over-the-horizon surveillance is highly dependent on knowledge of ionospheric conditions and hence of solar physics. The Navy's Solrad satellites, developed by NRL, monitor the x-ray and ultraviolet output of the sun for purposes of predicting solar disturbances and their potential interference with the operation of military systems. Other environmental-monitoring satellites provide data on global weather patterns that are important to the Navy as well as to many civilian organizations. The Navy also is interested in the potential use of space platforms for monitoring oil spills and other pollutants and for monitoring the ocean-air interface to determine wave-height and wind-field distribution. For several years, NRL has been developing techniques to provide these capabilities.

One of the Navy's most critical challenges has been the development of equipment and methods for conducting surveillance missions aboard aircraft and spacecraft. Research and development performed by the Laboratory have contributed significantly in this area.

In recent years, NRL has developed many complex instrument packages for space flight. They include the ultraviolet camera carried to the moon by NASA's Apollo 16 spacecraft and the solar-monitoring instruments designed and fabricated for NASA's Skylab mission.

Furthermore, NRL has carried forward a variety of space-related programs through investigations in plasma physics. For example, it has studied in great depth problems concerning ionospheric modeling and high-altitude explosions. NRL's mathematics and operational research, which is supported by well-established and growing computer facilities, also has been directed to problems involving space operations—the determination of satellite orbits, for example—as well as to a wide range of other subjects, including ocean surveillance and naval warfare.

Oceanology Area

Soon after the end of the War, a sobering new light was cast on the Allied victory in the Battle of the Atlantic. It was discovered that Allied antisubmarine-warfare capabilities, which had been a key factor in that victory, would have been no match for the German Type XXI submarine had it become operational before the War ended. This realization along with Navy projections concerning postwar advances in submarine design stimulated new NRL research on antisubmarine warfare. This work led to the development of the 10-kilohertz long-range active search sonar in 1950 and to active sonars operating at even lower frequencies in later years.

As these advances were being made, needs were increasing for more accurate and rapid calibration of transducers and for the establishment of improved standards of acoustic measurement. These requirements were met in large part by the Underwater Sound Reference Laboratory at Orlando, Florida, acquired by NRL in 1966, where equipment and techniques were developed for calibrating underwater sound transducers in simulated deep-ocean environments. The Orlando laboratory also developed a family of 20 types of standard hydrophones and wide-range sound sources which currently are in use throughout the Navy and the Nation. This standard acoustic instrumentation, which covers the frequency range from 1 hertz to 2 megahertz and can withstand a hydrostatic pressure as great as 10,000 pounds per square inch (equivalent to an ocean depth of 23,000 feet), serves to bring reliability and comparability to underwater sound measurements made throughout the Navy.

In retrospect, the early recognition by NRL's charter Sound Division that its scientists must turn to the ocean itself for answers crucial to naval military problems can be seen as the harbinger of the establishment of the Oceanology Area in 1967. As far back as 1924, when NRL invented the forerunner of the continuously recording echo profiler, isolated groups at the Laboratory experienced various needs for knowledge of the sea. These needs have increased ever since. The numerous technical advances wrought by the War provided marine scientists with new ways of looking at the ocean. They soon discovered many details that had "escaped" observation during the years of ocean study conducted up to that time. Sea mounts, rift valleys, abyssal plains, turbidity currents, deep scattering layers, seawater microstructure, internal waves, and many other features and phenomena could now be examined in considerable detail, and their effects on naval operations could be carefully evaluated. These new horizons, coupled with the realization that rapidly advancing technologies would soon enable our Navy to operate in any nook or corner of this major portion of the globe, presented formidable challenges to the NRL oceanographic community.

In response to these challenges and in recognition of the need to balance its continuing attack on underwater acoustic problems, NRL undertook major programs in ocean sciences. Although most of this work was of a fundamental character, its implications with respect to practical considerations quickly became apparent. Scientists who had developed a method for determining trace quantities of dissolved gases in small volumes of irradiated solutions applied the method to the measurement of gases in seawater and developed a standard shipboard method of analysis which marked a major advance in chemical oceanography. Since then, this tool has enabled NRL oceanologists to make several important discoveries concerning atmospheric phenomena, including the determination that the ocean is a natural source of atmospheric carbon monoxide comparable in magnitude to man-made sources. NRL measurements made on a monthly basis for almost a decade of the water-vapor content of the atmosphere at high altitudes have provided a first look at an environmental baseline that has important impact on man's future high-altitude travels and how those travels might in turn affect the earth's climate. Pioneering measurements were made of temperature gradients at the air-sea interface, which established a new basis for investigating heat-exchange phenomena having important effects on the thermal structure of the upper layers of the ocean.

Toward the end of the War, NRL was leading the way in the study of the effects of shock and vibration on ship structures, and it continued to do so in subsequent years. This work led, in the early 1960's, to the development of the only currently available means of predicting the response of a structure to noncontact underwater explosions. Since 1967, this method, called the Dynamic Design Analysis Method, has been strongly endorsed by the Naval Ship Systems Command. Techniques devised by NRL for determining the fracture resistance of materials designed for use in the deep ocean have had significant impact on undersea operations. NRL engineers also made contributions to glass and ceramic technology that have moved the Navy closer to the day when transparent capsules will be designed that will enable men to travel to the greatest ocean depths and to remain there for long periods of time.

The sudden increase in interest in deep-ocean technology witnessed in the 1960's is often cited as having been precipitated by the loss of the nuclear submarine THRESHER in 1963. Long before that loss, NRL ocean technologists had been developing the capability to probe large portions of the ocean bottom at great depths with sonars towed by ship. In 1964, NRL scientists and engineers aboard the Laboratory's research ship USNS MIZAR, which towed an improved underwater platform containing sonars and television and still cameras, located and photographed the hulk of THRESHER. They went on to assist in locating and retrieving the H-bomb lost in the Mediterranean Sea in 1966, to find and photograph the lost nuclear submarine SCORPION in 1968, to find and help retrieve the research submersible ALVIN in 1969, and to locate and photograph the lost French submarine EURYDICE and to monitor the scuttled ship LEBARON RUSSELL BRIGGS and her cargo of deadly nerve gas in 1970.

This NRL capability has also helped open up a new era of ocean study in which details of the deep ocean floor can be accurately mapped and related to the major forces which form them.

NRL's 50 Years of Radio-Electronic Research

L. A. Gebhard

Dr. Louis A. Gebhard attended Georgetown University and George Washington University. His career at NRL began in 1923 with his appointment as the Head of the Radio Transmitter Section. His work has been in radio communications, navigation and countermeasures, and from 1945 to 1968 he was Superintendent of the Radio Division. Dr. Gebhard was a recipient of the Presidential Certificate of Merit in 1946 and presently is a Consultant to the Electronics Area of NRL.

Dr. Gebhard has the distinction of being NRL's only 50-year employee.

The past 50 years have been an era of tremendous scientific progress, particularly in radio-electronics. In this field, the Naval Research Laboratory has continuously played a major role. This progress has had a major impact in advancing the operational capability and combat effectiveness primarily of the U.S. Navy, and in addition that of the other military services of the United States and of its allies. Industry and the national economy generally have also been greatly benefited by this progress.

During this period, there has always been the need for NRL to convince the operational Navy of the value of its work through actual demonstrations; positive proof had to be given that the advances made would provide performance substantially superior to that of existing facilities before the Navy would agree to relinquish previously accepted methods (proved useful through years of use) and adopt new and untried methods. This convervatism, while it represents an inconvenience to the scientist, assures that changes made are sound and provide an actual advantage to the military and to the nation. Although progress is an essential to any modern military establishment, military commanders must not take lightly a decision which will bring about great changes in existing equipment, with resulting substantial difficulties attending alterations in military operations.

NRL developed the technological basis for high-frequency communication, which is today the mainstay of the Navy in radio communication. The Laboratory led in the development of the very high, ultrahigh, and superhigh frequency bands, making available a tremendous increase in the number of communication channels at a time when a great need was felt for such an increase, not alone for military use, but also for civilian services such as television. This interplay of radio communications channel development and increased need for channel space has been a continuing problem, with no indication of abatement.

NRL provided the first satellite-communication system, which has presaged the present-day extensive utilization of satellite communications for all purposes. Major contributions to radio navigation have resulted from Laboratory work, as a result of its early direction-finder effort and later through the worldwide radio-navigation system known as Omega.

NRL's work resulted in the origination of radar in the United States, with its resulting revolutionary influence on the conduct of warfare. Radar has added tremendously to the safety and control of civilian aviation and to the safety and guidance of ships on the seas. One NRL development associated with radar permitted friendly forces to distinguish targets so as to identify friend from foe (IFF). NRL pioneered in the radio remote control of guided missiles, beginning with the first flight of a radio-controlled unmanned aircraft. The Laboratory was a recognized leader in the development of electronic countermeasures, both during and since World War II.

NRL developed techniques that greatly advanced the precision of standard time utilized in the present worldwide system of radio standard time transmissions. Associated with the time techniques are those concerning precise frequency, through which the Laboratory has made possible the discipline involved in interference-free radio channels on a global basis. To provide effective means for command and control, NRL researchers pioneered in the field of systems integration, providing the Navy with its first electronic tactical data system. This system provided for electronic means for the generation, storage, display, and utilization of critical tactical data from the several electronic systems and the automatic interchange of such data between component ships of a task force. NRL's development in systems integration was followed by further important contributions to subsequent electronic tactical data systems.

NRL has built a remarkable record of scientific achievement during the past 50 years. Impressive though this record may be, however, the Laboratory has the talent, know-how, and opportunity to make a record of even greater achievement during the next 50 years.

NRL Beginnings

E. O. Hulburt

Dr. E. O. Hulburt received his Ph.D. in physics from John Hopkins University in 1915. From then until 1924, he taught physics at John Hopkins, Western Reserve, and Iowa. He next joined NRL as a physicist and Superintendent of the Physical Optics Division, positions he held for 25 years. His wide-ranging research interests, which included optics of the sea and sky, terrestrial magnetism, aurorae, the ionosphere, and radio propogation, took him on many expeditions. Dr. Hulburt served as the first Director of Research of NRL from 1949 until his retirement in 1956.

I remember very vividly coming to the Naval Research Laboratory to enter on my new position there as Superintendent of the Optics Division. It was June 1924; the Laboratory was nearly 1 year old; I was 33 years old. The Laboratory consisted of four white buildings in a sizeable field of the old Bellevue farm on the reed-fringed banks of the Potomac River. The Bellevue house, a large pleasant country house, was separated from the Laboratory by an ancient orchard. There were barns and various outbuildings scattered about.

Although Bellevue is in the District of Columbia and hence the City of Washington, the city had not yet spread out, and Bellevue was quite remote and surrounded by almost untouched Maryland country. My wife came over by train from Baltimore to look at my new surroundings and at the station engaged a taxi. When she told the driver where she wanted to go, he said, "Yes, lady, I know where it is, but you better take me by the hour."

For the fourth time in my life I found myself in a large suite of empty rooms in which to set up a laboratory to work on problems as yet unspecified. Our Director, Captain E. G. Oberlin, gave me $40,000 for fundamental pieces of equipment, a princely sum in those days and much above what I had been accustomed to in universities. I was tremendously excited. Here was the entire Navy, the sea, and the sky to play with.

While waiting for the new equipment to come I found out that Dr. E. Hoyt Taylor, the Head of the Radio Division, was in the midst of discovering and investigating the marvelous properties of short radio waves which reached thousands of miles over the surface of the earth with very little power. The short waves had interesting peculiarities, skipping over regions near the transmitter and coming down to earth far away. These skip distances I recognized as phenomena of an ionized, magnetized region of the upper atmosphere, and the collaboration between Dr. Taylor and myself led to the discovery and measurement of the ionosphere.

I am sure that Captain Oberlin was as excited as we were at the prospect of a capable civilian research laboratory in the Navy. This called for a new venture in military-civilian relationships. A civilian organization in the naval hierarchy was a novelty and quite out of step with the usual military organization. Actually the plan had been specifically recommended by the Naval Consulting Board (of the 1914-1918 War), of which Thomas Edison was chairman. The board realized that unless a research laboratory were given proper status it could not attract competent scientists and would be a failure.

Captain Oberlin recognized clearly that naval officers were not necessarily scientific research men and that scientists were not necessarily naval officers. Although they were all working to the same end, their training was completely different; each had his own important job to do, and it was definitely not his business to try to do the other's job. This attitude led to mutual respect, appreciation, and cooperation.

Thus in the very beginning Captain Oberlin set the pattern for the Laboratory, a pattern that has been scrupulously adhered to ever since. Whatever success the Laboratory has had has been largely due to this design. We all owe a great debt to Captain Oberlin's sturdy personality and wisdom in giving the Laboratory such a good start and in guiding its development for many years.

The Beginning of the Division of Physical Metallurgy

R. F. Mehl

Dr. Robert F. Mehl came to NRL from Harvard to establish a division of physical metallurgy (1925-1931). Gaining recognition as the "Father of Gamma Ray Radiography" was the beginning of a long, illustrious career in research and education. Dr. Mehl is the author of over 200 scientific publications, he is a Fellow of the National Academy of Sciences, and currently he is a Professor of Metallurgy, Emeritus, Carnegie-Mellon University.

Two years after the Naval Research Laboratory was commissioned it was decided that a division of physical metallurgy should be initiated. At the time the technical force numbered 40 men, employed in the Radio Division under Dr. A. H. Taylor, the Sound Division under Dr. H. C. Hayes, the Light Division under Dr. E. O. Hulburt, and the Ballistic and Photographic Division under Mr. H. H. Moore. The Assistant Director, Captain E. G. Oberlin, and his advisors decided that this new division should engage not in applied research but primarily in pure research. Accordingly, Captain Oberlin wrote to a number of universities asking for nominations for the superintendency of the new division of "one founded in the fundamentals of metallurgy" (to quote from a letter to Professor D. J. Demarest of Ohio State University). One of these letters reached Professor Albert Sauveur, professor of metallurgy at Harvard University, who recommended me. At the time I was a National Research Fellow (postdoctoral) at Harvard. I had been awarded a Ph.D. at Princeton University, under Professor D. P. Smith, who in turn had been a student of the famous Professor Gustav Tammann of the University of Gottingen, Germany, one of the great pioneers in applying science to the study of metals.

The new division was housed on the first floor of the original laboratory building. Some equipment was already available: a tensile testing machine, a Brinell hardness tester, several rather large induction melting furnaces, a micro-metallograph, *etc*. I had done some research at Harvard in the field of x-ray diffraction, a new and exciting field in metallurgical research, and purchased one of the new General Electric x-ray diffraction apparatuses, and in pursuing research in this field wrote Nobel Laureate Arthur Compton, at the University of Chicago, for a new doctorate in this field; and thus Dr. C. S. Barrett was appointed as a member of the staff of the new division.

Thus at the beginning the staff of the Division was comprised of Barrett and a laboratory technician, Donaldson,* and me. In time Mr. Keyser was added and a few years later Messrs. Gow, Briggs, and Gezelius.

Barrett and I undertook a series of studies on the atomic-crystallographic mechanism of phase change in metals and alloys, employing photomicrographic and crystal structure methods. The first paper on this subject was published in 1930 under the general title "Studies upon the Widmanstatten Structure." My interest in this subject stemmed from my studies with Professor Sauveur at Harvard, who was very much

*A. R. Donaldson retired as Head of the Metallography Section in 1965, after 40 years of NRL service.

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interested in this structure as it appears as a beautiful geometric pattern in steels. At the time it was known only that in some alloy systems a new phase appeared on cooling, and in low-carbon steels, as plates parallel to a crystal plane in the high-temperature phase. Barrett and I demonstrated the basic principle that the mechanism of transformation, involved in all heat-treating operations, depended upon a crystallographic interaction between the lattice of the parent and that of the new phase. These studies laid the basis for a complete theory of age hardening, one of the chief methods for the heat treatment of alloys.

This work continued during my 4-year tenure of this post. I employed a series of graduate students during the summers; from Yale (F. N. Rhines and D. W. Smith, in 1931); from M.I.T. (O. T. Marzke, in 1930, ultimately to become Director of Research of NRL); and from Minnesota (H.S. Jerabek, 1931). These workers returned to their universities, continuing their work for their doctoral theses.

Other research projects were undertaken: aging in iron and steel, recrystallization in cartridge brass, and especially on gamma-ray radiography.

As a high school student in Lancaster, Pa., I had procured some uranyl acetate. Knowing that uranium emitted alpha, beta, and gamma rays, I, and a schoolmate also interested in science, placed the uranyl acetate on a card with a key underneath it and then a photographic film, producing a clear-cut radiograph. Although we were invited to do fundamental scientific research—the Widmanstatten structure was an example of this—we wanted also to do work of practical importance to the Navy. Remembering the early work on uranyl acetate, and a special graduate course on radioactivity at Princeton, we thought it might be interesting to see whether gamma rays from radium would supply a useful practical inspection method, particularly since such a method would be fully portable, requiring only a small amount of radium (or small bulk of radium emanation) and a photographic film—all in contrast to the bulky and stationary apparatus required for x-ray radiography. And, moreover, the low absorption coefficient of gamma rays might allow radiography of far thicker sections than possible with x rays.

X-ray radiography was, of course, well known and widely practiced in both medicine and engineering. Indeed, shortly after Röntgen discovered x rays in 1896, Professor Albert Sauveur, then a very young man in Belgium, had taken an x-ray radiograph of a weld. While at Harvard I became acquainted with Dr. H. H. Lester at nearby Watertown Arsenal; we became close friends, and I came to know the fine work he did in x-ray radiography. All this is background. It surely was no great mental feat to think of using gamma rays instead of x rays in radiography.

As it happened, at about the same time the Navy was encountering serious difficulties with steel castings used in naval construction. If one wanted to improve steel castings, it was important to study defects in steel castings, as x-ray radiographers had long since done, now with the added advantage of greater penetrability and greater portability.

The Navy readily agreed to undertake this research work, and a project was set up. We discovered that the Howard Kelly Hospital in Baltimore had 5 grams of radium and regularly pumped off radioactive radium emanation, radon, the first disintegration state of radium, sealing this gas in glass capsules 1/8 inch in diameter, and employing it in treating victims of cancer. Arrangements were made to have this source of radon available to us. Indeed, our first radiographic work was done in the basement of the Howard Kelly Hospital.

Dr. Charles S. Barrett and I began the work. We were shortly joined by Dr. Gilbert Doan, professor of Metallurgy on leave from Lehigh University, and as time wore on by Charles Briggs and Roy Gezelius. It was great sport. We had many questions to find answers to: how long would the exposures have to be and the companion question of how much radon would be required; what would be the maximum thickness of steel that could be radiographed in a reasonable time; what degree of sensitivity could be attained—that is, how small could a defect be and still register as an indication on a photographic file; *etc.*

Impatiently chasing these goals we radiographed an odd collection of handy objects: a small complicated steel casting, full of blow-holes; a bronze valve, a monkey wrench; a lathe tool rest; a 10-inch-thick iron casting; a pile of steel plates 12-1/2 inches thick with slots of various depths machined into one plate in an attempt to appraise sensitivity; miscellaneous welds; *etc.* Great sport. The thing worked! Contrast on the films was less than in x-ray radiography, but the method was less sensitive to changing section thickness in a casting; sensitivity was apparently no less than that of x-ray radiography; successful radiographs were taken through 12-1/2 inches of steel. And we hungrily hastened into publication, first in 1930 in the Transactions of the American Society for Steel Treating, now the American Society for Metals.[1]

All of this work was reported by NRL to the Assistant Secretary of the Navy on June 14, 1930. And then, on August 21, 1930, the Navy Department brought to the attention of NRL the case of the new cruiser USS CHESTER. On initial trials, of turning and maneuvering, the sternport casting cracked; the ship was drydocked in the New York Navy Yard and the 15-inch-long crack was welded shut. The casting cracked again, and the ship drydocked at Norfolk. The Navy wanted to know whether NRL could offer service in this connection by employing the new method of gamma-ray radiography. We agreed. Indeed, though the fate of the CHESTER was bad luck for the Navy, the opportunity it offered these young researchers was most timely. Dr. Barrett and I, equipped with some radium emanation in a capsule, and with the largest x-ray photographic films we could find, set sail for Norfolk in November 1930.

The sternpost casting bore the bending moment of the rudder. It was a shell-like casting, weighing 9 tons, with a wall thickness in the most part of 1-1/2 inches. Penetrability was thus not the dominant problem, but the portability of inspection equipment was, for the ship lay in drydock (a not very dry drydock!). We placed the capsule of radium emanation in the middle of the U-shaped, shell-like casting and attached 24 films on the outside surface. The developed films showed that the casting was fearfully defective, exhibiting long hot tears, blowholes, and spongy metal. This work was reported to the Assistant Secretary of the Navy in the same month. As a result the casting was scrapped. It was subsequently cut up, and the defects revealed were compared with the radiographic results, with happy concurrence. Then, quickly, this method of nondestructive testing was established.

A preliminary manual for gamma-ray testing was shortly thereafter written by Dr. C. S. Barrett and issued to interested groups in the Navy. The Navy purchased half a gram of radium sulfate in August 1931, and NRL provided facilities for service to the Navy yards and other interested people; a lead box was made to carry the radium safely. The use of the method was demonstrated at the Philadelphia Navy Yard in February 1932; radiographic inspection of castings from the sister cruiser USS NORTHHAMPTON was performed, including turbine castings and keel knuckle castings; cooperative studies were made with the Westinghouse Corp., General Electric, Bethlehem Steel, and others. Many of these results were shown in a series of further publications. I left the Naval Research Laboratory in the summer of 1931, and the radiographic work devolved chiefly on Charles Briggs, who thereafter made steel castings his lifelong specialty. I went on to become assistant director of research at Armco, and shortly thereafter became director of the Metals Research Laboratory at the Carnegie Institute of Technology. Dr. Barrett joined me there in 1933. Barrett stayed with me for many years, finally becoming professor at the University of Chicago, where he had earned his doctorate in 1928. He has become a world-famous scientist, full of honors, including a membership in the National Academy of Sciences, and author of a book, "Structures of Metals," which, translated into a number of other languages, has become a classic. He is currently Distinguished Visiting Professor at the University of Denver.

Dr. Gilbert Doan returned to his professorship at Lehigh University and became head of the department of metallurgy. Charles Briggs remained at NRL for some years and then became Secretary of the Steel Founders Society of America, from which post he only recently retired.

Further work comprised mostly gathering experience, studying the question of sensitivity for small defects, and extending the early work on welds. Other people carried on the work. In time radioactive cobalt became available as a result of the development of atomic energy, and this replaced radium and radium emanation.

A formal project was initiated by the Bureau of Engineering on steel castings, with a view, of course, of improving them. Mr. James Gow began this difficult work (a large foundry and a large staff would have been required fully to have implemented such a program); it was taken over by Charles Briggs (who was, as noted, also involved in the very pertinent gamma-ray radiography program) and Roy Gezelius. The project

[1]*Trans. Am. Soc. Treating* 18:1192 (1930); presented to a meeting of that society October 1930 in Chicago.

has been continued through the years, culminating in the beautiful research of the present Superintendent of the Division, Mr. Pellini, on directional solidification of steel.

These were the major items of research through the first 4 years of the Division. There were smaller ones. Dr. Robert Canfield of Johns Hopkins University had devised an excellent method to measure the coefficient of internal friction—of such importance to the study of fatigue in metals, and upon invitation continued this work at NRL. Such work seemed important to the Navy in view of frequent fatigue failures in naval equipment, and in view of the work by McAdams on corrosion fatigue at Annapolis. The Sound Division, developing sonar methods for the Navy, became interested in alloys showing magnetostriction, requiring the attention of a metallurgist.

Radar — A Retrospection in Perspective

R. M. Page

Dr. Robert M. Page accepted an appointment to NRL in 1927 and devoted his entire professional life to its projects, rising to the position of Director of Research in 1957. Among his many honors is the Presidential Award for Distinguished Civilian Service, received in 1960, in recognition of his pioneering work in radar. He retired in 1967.

The generic term "radar" has been loosely applied to a very broad spectrum of radio technologies which embrace the unmodulated continuous-wave (cw) bistatic system investigated by NRL in 1930-1934, and later employed by NRL in the Space Surveillance (SPASUR) System; the unmodulated cw monostatic system used by highway police to measure automobile speed; the frequency-modulated cw system developed by the Radio Corporation of America (RCA) for the radio altimeter; the "broad brush" bistatic system developed by the British in 1935-1939 for coastal defense and adapted to airborne use by the Royal Air Force (RAF); the "fine brush" monostatic system developed by NRL in 1934-1938 for shipborne and airborne use; and the ultra "fine brush" microwave systems developed during the war, 1941-1945, by the Radiation Laboratory of the Massachusetts Institute of Technology (MIT) in cooperation with American industry and NRL, and suitable primarily for gun fire control and navigation. These six systems represent six different technologies, aimed at six different objectives, and were achieved in six different and relatively independent development efforts. This report is concerned primarily with the contributions to radar by NRL and some of its leading personnel, and with the relationship of those contributions to other radar developments.

In September 1930, L. A. Hyland discovered that aircraft in flight could reflect radio-frequency energy in sufficient amount to be observed in a radio receiver in a bistatic cw configuration, and he suggested that the phenomenon be employed for the detection of aircraft by radio. The resulting project immediately set up by the Bureau of Engineering marks the beginning of radar research at NRL and in the world.

Hyland's cw method proved that small aircraft could be detected at distances out to 40 miles or more, but it was not suitable for use aboard ship. Some time in 1933 Leo C. Young suggested that the short-pulse method, which, if successful, would be better adaptable to shipboard use, be substituted for the cw method. In March 1934, the author was assigned to the task of developing the short-pulse method. This marks the beginning of pulse radar research.

Shipboard operation was presumed from the start. The most difficult condition imposed by shipboard operation was monostatic configuration, theoretically possible with the short-pulse method. However, with no isolation from a nearby transmitter radiating many kilowatts of pulse power on the frequency and pulse length to which an extremely sensitive receiver was tuned, the receiver problems were formidable to say the least! For this application the most obvious and the most fatal weakness of radio receivers of that day was blocking by the transmitted pulse. Solution of that problem was the *sine qua non* of radar, so it was attacked first. A solution was conceived, and all the components of a short-pulse radar system were assembled and tested in December 1934. This system was very crude, but it was really pushing the art, with a transmitter generating powerful 10-microsecond pulses on 5-meter wavelength and an unprotected receiver only ten wavelengths removed from the transmitter. Echoes were received from flying aircraft, but they were not as pretty as desired due to ringing of the receiver circuits after each transmitted pulse. However, the first major receiver problem had been solved. The receiver was immune to blocking from the transmitted pulses! In fact, it was the absence of blocking that permitted the ringing.

The first 8 months of 1935 were devoted to solving the remaining receiver problems and building a new receiver. It was a one-man effort. Mathematical solutions to short-pulse amplification were obtained, and sets of pulse receiver design curves were constructed. Not only the width but also the shape of the receiver pass band were critical. No receiving tubes on the open market were capable of meeting the stringent requirements of low capacitance and high mutual conductance. Development of the acorn tube had just been completed under Navy contract by RCA, and that tube was found to be ideal for the purpose. Electrical circuits were designed to fit the mathematics, and a receiver wiring diagram and specification were completed. Mechanical construction was designed to implement the circuits, complete shop drawings were made, and commercial components were purchased. The receiver was assembled, wired, and tested, and inadequate coil form insulation was replaced with a new low-loss material just made available. The finished receiver was completely nonblocking and nonringing, free from spurious pulse transients following the transmitted pulse, and had a stable gain-bandwidth product orders of magnitude greater than that of any other receiver then in use. It was capable of amplifying a 5-microsecond pulse from input noise level to over a hundred volts for cathode ray deflection without observable pulse broadening.

In September 1935 Mr. (now Dr.) Robert C. Guthrie was assigned to the project to build a new transmitter and assist in setting up the system for test. This doubled the manpower on the project. An existing large directive antenna array on 10 meters wavelength set the frequency of the system. A breakthrough on the transmitter resulted from the suggestion by Mr. (now Dr.) L. R. Philpott, with whom the author frequently consulted, that a self-quenching, or "squegging" oscillator with an inductive storage capacitive discharge keying circuit be tried. With the use of those circuits Guthrie built and installed a new transmitter. The remaining components were built and assembled into a 5-microsecond pulse radar system which was first operated in April 1936. Success was immediate and almost startling. Small aircraft were observed out to 25 miles, the limit of the indicator, with excellent resolution and clarity.

Reference is now made to parallel efforts in England. In February 1935, while the new 5-microsecond pulse receiver was being designed at NRL, Watson Watt in England calculated the radio-frequency energy that might be reflected by an aircraft and concluded that it would be sufficient for observation in a radio receiver at distances useful for military purposes. His conclusion was verified in a preliminary test made in June 1935. The configuration of the test was bistatic, as was that of Hyland in 1930. Pulses were transmitted, but the pulses, being of the type used for ionosphere probing, were so long that the receiver was illuminated simultaneously by energy received directly from the transmitter and by reflection from the aircraft. The experiment was therefore a practical duplication of Hyland's 1930 experiment, and, as with the Hyland experience more than 4 years earlier, an exuberant reaction to the observed reflection led to the establishment of a research project for detection of aircraft by radio.

The imminence of war and the threat of the Luftwaffe put great pressure on England to find quickly a means of long-range warning of the approach of hostile aircraft. There was no time for research. The hastily erected coastal chain of radar stations had to tolerate very large antennas, very long pulses, relatively long wavelengths, and bistatic configuration, but they met that military necessity. The problems of short-pulse radar and monostatic configuration being solved at NRL in 1935 were not even faced in England until the RAF later tried to put radar in aircraft, but that is another story.

The spectacular success of 5-microsecond radar at NRL in April 1936 put immediate pressure on the Laboratory to reduce the size of the antenna from 120 feet square for the transmitter alone to something less than 20 feet square for both transmitting and receiving antennas combined without loss in either sensitivity or directivity. This required great increases in both frequency and power, when increase in either entailed sacrifice of the other. The task was so great that it was decided to break it down into two steps and to augment the staff from two to three engineers in order to pursue both steps at once. So in June 1936 Mr. A. A. Varela was added to the staff and assigned to work on a transmitter for 1.5 meters wavelength, while Guthrie took the intermediate step from 10 to 4 meters.

A significant breakthrough occurred when Dr. A. Hoyt Taylor suggested that since transmission and reception occur at different times, it should be possible to use the same antenna for both transmitter and receiver, thus gaining a factor of four in sensitivity. Acting on this suggestion the author devised a transmit-receive (T-R) switching circuit based on impedance inversion switched by the transmitted signal at the grid of the receiver input tube. The device worked well on Guthrie's new 4-meter radar. This radar was successfully operated in October 1936, observing aircraft at ranges out to 40 miles. At 1.5 meters, however, the receiver input grid was too inefficient for switching high power, and Mr. Young suggested the use of a gas discharge tube instead of the receiver input grid for the switching function. This worked very well, and such a T-R switch was used on a crude 1.5-meter radar system tested at sea on the destroyer LEARY in April 1937. This first 1.5-meter radar worked well on targets at moderate range but lacked the power necessary for required longer ranges.

Two more events finally showed the solution of the problem of high pulse power at high frequency. One event was the marketing of a new transmitting tube by the Eitel-McCullough (Eimac) Company, the type 127, which was found to have a short-wave limit just under 1.5 meters at normal operating voltage but was capable of pulse operation at ten times normal anode voltage, under which condition it became very efficient at 1.5 meters. The other event was the invention at NRL of the ring circuit oscillator, which made possible the use of any desired even number of tubes in one oscillating circuit without loss of efficiency or frequency over those of a simple push-pull circuit.

All these advances were combined in a new 1.5-meter, 3-microsecond radar system. Completed in June 1938, this radar developed 15 kilowatts of pulse power with a six-tube transmitting oscillator and had a single antenna about 17 feet square serving with a T-R switch. It tracked a small airplane to a range of 98 miles. The electrical design of this set was frozen for a service prototype which was completed by the Laboratory in December 1938 and designated Model XAF. It was installed on the battleship NEW YORK and service tested in Fleet Problem XX from January to March 1939. It was so sensationally successful that a contract was rushed through to have it copied and produced commercially as Model CXAM. The first six units were delivered to the Navy in June 1940, and sixteen more quickly followed. Some went down at Pearl Harbor, and some served throughout the war in the Pacific. Used for both early warning and fighter direction, they were responsible for saving many ships.

Having achieved a successful radar system for capital ships, the radar research group at NRL, then numbering half a dozen full-time engineers and growing rapidly, embarked on several follow-on projects, including identification friend or foe (IFF), Plan Position Indication (PPI), precision tracking, and still shorter wavelength radar, all of which are beyond the scope of this report. The main emphasis at that time was on radar for destroyers. Cooperation with Eimac on pulse tube design produced a 1.5-meter tube, the type 327, capable of nearly 100 kilowatts of pulse power and with normal life expectancy of over a thousand hours of service. A four-tube oscillator using these tubes and a new NRL-designed all-metal broadband curtain array antenna about 6-feet square were used in the laboratory-model XAR radar, the service prototype for the SC-1 series, of which 600 sets were produced by the General Electric Company.

Further cooperative effort by Eimac resulted in the fabulous tube type 15-E, a transmitting miniature scarcely 1-1/2 inches in diameter and 2 inches in length, which produced a pulse power of 100 kilowatts at 60 centimeters wavelength and which had a service life of many hundreds of hours. With the use of only two of these tubes for the transmitter, a 60-centimeter airborne radar was developed at NRL; it was produced by thousands in contracts with several manufacturers.

Reference is now made to the RAF efforts to adapt radar to airborne use in England. The attempts to reduce wavelength and pulse length and to increase high-frequency power produced discouraging results. At the time the XAF was demonstrating spectacular successes in the U.S. Fleet, the RAF had reduced wavelength for aircraft to 1.5 meters, the same as the shipborne XAF, and pulse length to about 20 microseconds, or seven times the XAF pulse length. The greatest problems, however, were insufficient power and inability to use the same antenna for transmission and reception. The resulting equipment required three large antennas on the aircraft, which earned for it the name of flying Christmas tree. The RAF interceptors were so slowed down by carrying these antennas that they were unable to overtake the German bombers. The magnificent defense of London against the onslaught of the Luftwaffe was due more to the early warning by the coastal defense chain and the tenacity of the British pilots than to technical excellence of their airborne radar. The RAF finally abandoned further development of short-pulse radar, because of the lack of sufficient transmitter power. It was left to the United States to develop and produce 60-centimeter airborne radar based on the NRL work, and later, microwave radar at 10 and 3 centimeters based on a microwave version of the ring circuit oscillator, the multicavity magnetron, one contribution to short-pulse radar for which England may justly claim credit. To compare the monumental technical achievements of NRL in the development of short-pulse radar in 1934 to 1938 with the relatively simple adaptation of ionosphere probing and static direction finding radios to the coastal defense system of England in 1935 to 1939 is like comparing grapes and grapefruit. The name has a similar sound but the fruit is different. But when the sensationally successful 3-microsecond XAF radar of NRL is compared with the contemporary 20-microsecond system of the RAF, the prewar short-pulse radar efforts of England must be called a dismal failure!

Reference is also made to the radar developments by the Army Signal Corps Laboratories at Fort Monmouth, New Jersey. The first attempts at radar there ran aground on receiver blocking, for which they could find no solution. Their radar successes were based on early disclosures to them of the original developments at NRL, with one exception: antenna lobe-switching for tracking in angle. This technique was used for fire control radar by both the Army and the Navy until it was replaced by conical scan, and later by monopulse radar for antiaircraft fire control, and by microwave sector scan for naval antisurface target fire control.

Having satisfied the basic requirements of early warning and fighter direction radar for ships and aircraft, part of the NRL group turned its attention to fire control radar. Early disclosures to and frequent consultation with the Bell Telephone Laboratories resulted in lobe-switched fire control radar that was supplied to the Fleet in large numbers. The NRL group therefore concentrated on simultaneous lobe comparison. This work culminated in the invention and development of monopulse radar, which remains the all-time standard for precision tracking radar.

After the War we attempted to break the horizon barrier by using ionosphere reflection to extend radar range to targets below the horizon. This work resulted in the MADRE (magnetic drum recording equipment) over-the-horizon radar, which was developed to a high degree of perfection at the Chesapeake Bay Station during the 1950's and 1960's. It is capable of tracking aircraft flying at any altitude, even down to treetop level, at distances of 1000 to 2000 miles, depending on ionospheric conditions.

In 1941 the Radiation Laboratory of MIT came into being. Drawing on the experience and the original radar developments of NRL, the multicavity magnetron of Britain and the Plan Position Indicator developed independently by NRL and Britain, the Radiation Laboratory combined the efforts of U.S. industry with its own research to develop and produce microwave radar with great effectiveness.

This account gives what the author believes to be a true perspective of the several principal separate efforts in the original development of radar. Its purpose is to reaffirm and to clarify the solid basis of NRL's claim to fame as THE BIRTHPLACE OF RADAR.

Hindsight

R. C. Guthrie

Dr. Robert C. Guthrie joined NRL in 1929 as a physicist and radio engineer, and he became involved in the early development of radar. He was Superintendent of the Radar Division from 1954 to 1964 and has since served as a consultant in radar and electronics. Dr. Guthrie has been the recipient of the Navy Distinguished Civilian Service Award and the Presidential Certificate of Merit for his pioneering work in radar.

A look at someone in a worse position than one's own can make one feel better—it may even make one look better. Since winners usually look better than losers—at least to the winners—let us compare some features of German World War radar activities with our own (including those of the British).

Around the middle of the War the Luftwaffe got possession of a British H_2S 10-centimeter aircraft radar. A British plane had been shot down without serious damage to the radar. Although this was one of the earliest airborne microwave systems, it was, even so, a fine radar embodying techniques basic to the success of radar at such high frequencies. It was indeed a fine prize for the Germans.

At that time the German U-boat campaign was at a particularly critical phase. Information about this British airborne 10-centimeter search radar was just what the U-boat service needed urgently.

The early success of the U-boat against British shipping had not been blunted until the use of radar-equipped patrol aircraft. The courage, persistence, and competence of corvette or frigate crews could not provide adequate protection from U-boats, even within the immediate vicinity of a convoy. But a radar-equipped patrol aircraft could search up to 100 times as much area as a ship, and such rates allowed the area searched to be chosen where the U-boat was most vulnerable to detection—during transit from bases to operational areas. Medium bombers equipped with the British ASV radar (200 Mc) were particularly effective in night sorties when U-boats would surface to charge batteries or to gain full cruising speed.

Then the Germans had reduced the effectiveness of the 200 Mc ASV by equipping the U-boats with intercept receivers. When this measure in turn waned in effectiveness, owing to steps taken by the Allies, the Germans were perplexed as to what type of detection system their intercept devices were failing to apprehend. The U-boats were again being caught in undetected approaches of patrol aircraft. Discounting the possible use of microwave radar, for reasons to be discussed later, they concluded that we must be using infrared-detection systems, becoming more than ever blinded to the possibility of a new much higher frequency radar.

The U-boat's capability was at a low point at the time of the capture of the very radar they needed to know about. By one of those fantastic events of warfare, the existence of the captured H_2S system was not revealed to the German Navy for months. Critical time was lost to the U-boat service. Was it ultrasecrecy, interservice jealousy, ignorance of the importance to the Navy, or perhaps a combination of these? Many

Allied sailors' and soldiers' lives and thousands of tons of precious cargo were the bonus to the Allies because of that German blunder.

The delay was particularly important because the number of aircraft in the Allied ASV patrol was increasing rapidly, and the search would reach a density which could deny the U-boats an important part of the time they needed on the surface whether they escaped attack or not. The later German use of centimetric intercept receivers and radar decoys was therefore only partially successful. Also a 3-centimeter ASV was to follow.

A very effective radar countermeasure was to be introduced by the Germans late in the War—too late to have any significance except perhaps to scare the radar people. This was the Schnorkel, that intake/exhaust pipe device which enabled submerged diesel operation with the exposure of only a few feet of the structure. As a radar target it was orders of magnitude smaller than a surfaced submarine. The Schnorkels were coated with a material which absorbed microwave radiation, and their target size was no greater than that of the waves in any but a fairly calm sea.

Perhaps a brief description of some of the German radar would be interesting and might offer a clue as to mishandling of the captured H_2S system.

In the early part of the War the Germans were well-equipped with radar equipment equivalent to that of the Allies, possibly even superior in some respects. Their land-based equipment consisted primarily of two types: an early warning 100 Mc system called Freya for detection and monitoring of aircraft and an antiaircraft gun-control system called Wurzburg in a frequency band near 600 Mc.

The Freya radars used large array-type antennas, and their performance was probably equal to our 270/271 army radars. The Wurzburg used a paraboloidal antenna about 3 meters in diameter fed by an offset dipole, rotation of which about the antenna axis produced the lobing required for accurate angle information. The frequency band near 600 Mc was about as high as triodes of that day would produce (barely) the power needed for the transmitter.

The mechanical design of the Wurzburg was more interesting than the electrical. It was very compact and was mounted on a four-wheel dolly about the size of a Volkswagen chassis, with outriggers for leveling and stability when the system was set up for operation.

The manually operated antenna slewing and elevating cranks were beautifully smooth to operate. Metal shrouded the legs of the three seated operators from the weather. Certainly mechanical engineers enjoyed a relatively stronger hand in the design than did such people in either the United States or the United Kingdom (the early U.K. radars, assembled with utmost urgency, were quite sloppy mechanically—almost makeshift).

Another mechanical feature different from ours was the use of cast metal for many of the small shielding boxes and compartments of the radio-frequency parts of the radar. These were excellently made, but we would have used fabricated sheet metal. The design of the system obviously was tailored for large production—thousands of these radars were made.

Not as much is known by us about German naval radar, because captured equipment was not available. Intelligence photographs of German warships show no parabolic antennas such as the Wurzburg, but a dipole-array type of antenna, which scaled to a frequency band about that of the Wurzburg. The superior gunfire of the BISMARCK strongly indicated an accuracy of range finding available only with radar. The first salvo (at over 12 miles range) straddled the HOOD, which was soon sunk. The relentless pursuit by British ships was to continue at a more respectful distance until the BISMARCK was overwhelmed with a multitude of warships, submarines, and aircraft. The PRINZ EUGEN, which had accompanied the BISMARCK until after the HOOD sinking, actually escaped and returned to a German base.

No really new radar systems of operational significance were to be added to the German land-based complement for the remainder of the War, and not many modifications. There was to appear a giant Wurzburg—really a fixed-site Wurzburg in which the improvement was a large paraboloidal antenna about 20 feet in diameter, which used the same type of feed as the small Wurzburg. The improvement such a size should have made was partially lost by the antenna's poor electrical design. Here again mechanical engineering prevailed in a quick-fix-job—very impressive on the landscape. At its time it was one of the *big* reflector-type antennas in any country. Another quick-fix modification of the Wurzburg to better cope with unwanted echo signals will be mentioned later.

The main weakness of the Wurzburg radars, giant or small, was the limited frequency range imposed by the early freezing of design. It was scarcely more than needed to allow spaced frequencies to avoid mutual interference in a complex of radars deployed about a city. The Allies would take full advantage of this weakness during the heavy bombing raids over Germany.

Two types of radar countermeasures were especially effective against the Wurzburg: Pretuned airborne jamming transmitters and "Window." A few noise-modulated jamming transmitters preset to blanket the Wurzburg band and installed one each on as many bombers needed only to be switched on when the flac would be encountered. But the latter, Window, was the more effective, available earlier, and more widely used. Window was the code name of the aluminum foil strips cut to be resonant in the Wurzburg frequency band. Dispensed by releasing small packages (containing 1500 to 2000 foil strips approximately 1/4 × 10 inches) from an aircraft, these reflectors gave a large radar signal masking that of the aircraft and, soon in a false position, could cause the radar to break track on the proper target.

The work done in developing Window of good design and methods of its use make an interesting story. It is sufficient to say now that a package of 1 × 2 × 10 inches weighing a few ounces gave a signal on the Wurzburg much larger than that of a bomber. Once released into the slipstream the batch of foil strips almost exploded into a small cloud of reflecting dipoles falling slowly (500 to 1000 feet per minute).

Hundred of tons of Window were used by the American and British bombers in the last 2 years of the war. It was reported that many German dairy cows were made sick by consuming the foil strips along with the grass in their pastures.

Window being so effective, one can imagine the vigor with which an airman would dispense these packages through the ejection chute of his plane when the flac was heavy. He had to be subject to a restraining regimen, of course.

The choice of the time to start operational use of a countermeasure such as Window is a delicate one. The use reveals to the enemy a weapon he may not yet have devised. It must be decided if its use against you would be to your net disadvantage. England's defense against renewed German bombing was highly dependent on radar and therefore vulnerable. The first Allied use of Window was delayed until mid-1943, long after the start of the heavy bombing of German-held areas. The reasoning supporting such decisions is often influenced by vanity, I think.

According to Churchill (The Hinge of Fate), the Window idea was proposed also in Germany, and Goering, at last realizing his own vulnerability, had all papers impounded and the highest secrecy imposed on the proposal. He would have been much better advised had he assumed an equal ingenuity for his enemies and had had the method developed to a practical stage for use. The training of their own operators thus permitted and the likelihood of their scientists providing a counter-countermeasure were more important than the secrecy. Late in the war they did devise one countermeasure to Window, with the cute name of Wurzlaus because of the appearance it gave the signal displayed on the Wurzburg indicators. It may also have helped against low-altitude attackers, but its effectiveness was only marginal.

I have cited two examples of the Germans imposing secrecy to an extreme and to their own detriment. Perhaps these are unusual and perhaps we have our own cases which just did not happen to affect my own field of work significantly. I have also stated that their policy apparently was to freeze the early radar design and to close off any substantial continuing program of research and development in radar. In the early part of the War they possessed radar systems as good as and in some respects even superior to ours, but this soon was not the case, as the state of the art progressed so rapidly in the United States and the United Kingdom.

As the tide of the War changed in our favor, we were able to capture German and Japanese radars. We did not, to my knowledge, learn one thing from these equipments which was worth applying to our own radars. This is not to say the captured gear did not have great value for our countermeasure activities.

It was reported, after the War, that the German scientists in the early part of the War concluded that centimetric wavelengths were not worth the effort to exploit—because of assumed limitations in power capability and sensitivity, I suppose. I cannot believe that this was the opinion of their main researchers. More likely, it was an administrative decision based on imposed priorities and the assumption of a short war.

I have often wondered how those German scientists felt who ultimately examined that first captured 10-centimeter radar. I imagined them thoroughly disheartened. One of the earlier 10-cm radars, the H_2S, still embodied features so far advanced over German technology, that they could not even make a Chinese copy. For example, the material used in the magnet and the cathode of the magnetron transmitter tube or that in the first detector of the receiver would require months of experimentation for them to duplicate. Yet could it be that, believing the policy concerning microwaves stated above, they looked on the radar as something of a toy and were unconvinced of its practical utility. Admittedly it was a rather insensitive radar, but it could do its job because a surfaced submarine is a big target or when used for airborne-interception by fighter aircraft it need only pick up the bomber target at short ranges. Actually the unit captured was probably being used as a mapping system to enhance nighttime bombing accuracy.

This article started with the promise to include some favorable comparisons between German radar work and our own—including that of NRL. Take the example of the early freezing of German radar design and their abandonment of microwave development. In the prewar NRL group under Dr. Page, there never was any thought that the higher frequencies—much higher than at the start—might not have features and advantages which should be discovered and used. Our early concentration at the meter-wave frequencies was to provide excellent radars used throughout the War. But it was also expedient to use available components and techniques for the earliest possible demonstrations of the capabilities of radar. After Dr. Page's first trials in 1934 at about 60 Mc, we made one downward move to 28 Mc! This was an expedient choice to take advantage of an available transmitting tube and in particular an existing high-gain antenna together with a fine new receiver Dr. Page had designed and made during the year following his first trial. Within a few months this same transmitter/receiver was successfully adapted to first 50 Mc and then 80 Mc. In the meantime, others who had joined his group were working at 200 and 400 Mc, and higher. The trend, intentionally, was continually upward.

When, before our entry into the War, the National Defense Research Council was setting up its Division 14 (Radiation Laboratory), the NRL recommendation, or at least concurrence, was that they concentrate on the microwave frequencies. Dr. Page participated in that action, reluctantly I know, for it was an area we would have loved to have had to ourselves to exploit. The British had earlier provided a real breakthrough for microwave development by their invention of the multicavity magnetron—an efficient and rugged generator of microwave energy. But we at NRL had our hands full, and only a relatively small effort was continued in the microwave field—at least compared to the effort at the Radiation Laboratory, which was to become larger than all of NRL.

The freezing of a design in preparation for production is always somewhat painful. It had to be done—and was, expeditiously—but always there is just one more change that would have improved the product. Never, though, were we closed off from the research towards better or different systems.

With respect to secrecy—as well as interservice rivalry—the first NRL observations of the effects of aircraft and ships on transmitted radio signals were recognized as the basis of a possible detection system and as having an important potential for military and naval use. The secrecy of such information was taken for granted, even though it was realized that other experimenters with shortwave communications links were bound to make similar observations. This was especially true in the 1930's when more and more flying aircraft became common.

Yet we regretted the publication (about 1933) of an article by Bell Telephone Laboratories' experimenters citing their difficulties (as well as the possible use of the phenomenon) with flying aircraft in shortwave propagation tests they were conducting. It has been reported that this article triggered both the French and German work on radio detection (radar). This could be so, but again, wide interest in shortwave communication would have led them to the same discovery ultimately.

The point is that despite the inevitability of the discovery, the importance of it to our own defense was such that we never considered reducing the secrecy, nor were we fretted by it. Nor did it restrict in any significant way the exchange of information. From the early 1930's Army representatives were continually apprised of NRL discoveries and work. In 1936 demonstrations were made to a wide range of Army and Navy people and to representatives of major electronic corporations including Bell Telephone Laboratories, General Electric Company, and Radio Corporation of America. Actual work at these companies was started under Navy sponsorship not long afterwards. Although secrecy status continued throughout the War, such exchanges of information were continual and grew as the scope of the work expanded at NRL and elsewhere. The overnight Federal to Boston came to be known as the "milk-run" as visits to and from Radiation Laboratory became more and more frequent with the growth of their work. Liaison offices, normally staffed with Army and Navy officers with engineering background, were set up at various laboratories, including NRL.

Actually the great extent and rapid pace of the U.S. radar work and the continual selection of systems for manufacture and field distribution precluded any other policy. There had to be frequent and thorough information exchanges among the many people involved.

One rather trivial inconvenience is recalled. Some time after the radar work became so promising that its military application was certain, gates were put up in the NRL hallways to restrict entry to radar work areas. No guards were stationed but even so this cut off those nice visits by friends from other parts of the Laboratory, some of whom might even have been mildly resentful. I think it did little good, and I was glad when the gates went to more secret work such as proximity fuses and *identification friend or foe.*

Vanguard—The Laboratory Ventures into Space

J. P. Hagen

Dr. John P. Hagen's career at NRL began in 1935, and from 1950 to 1958 he was Superintendent of the Atmosphere and Astrophysics Division. During the years 1955 to 1968 he was director of Project Vanguard, and he was chief of the Vanguard Division of NASA from 1958 to 1962. Dr. Hagen is presently Professor and Head of the Department of Astronomy of Pennsylvania State University.

On March 17, 1973, 200 people gathered to celebrate the 15th anniversary of the launching of the first Vanguard satellite. Eighteen years ago, in the fall of 1955, a team of about 150 men and women, coming mainly from within the divisions of the Laboratory, was organized to attempt this country's first venture into space. The group, bound together in stress and adversity and sharing the success which culminated the effort, now periodically assembles to celebrate the anniversary.

Following World War II the Laboratory emerged from its wartime status with a well-rounded group of capable scientists in most of the scientific disciplines. Among these were people working on such things as upper atmosphere research, rocket development, and radio astronomy. Many of these people were brought together to form the Atmosphere and Astrophysics Division. The Division pursued an active program in probing the earth's atmosphere with scientific rockets and developed new and better rockets for this purpose. This phase of the work is discussed in Milton Rosen's paper in this issue.

Because of the background acquired in the design, construction, and flight of scientific instruments in rockets and in the design and construction of rockets, the Division and the Laboratory were in a strong competitive position in 1955 to offer to take part in the Earth Satellite Project proposed as a part of the U.S. participation in the International Geophysical Year (IGY). Talk of possible earth satellites was not new. Designers at the Laboratory and elsewhere had for some time realized that rocket capabilities had reached the point where serious consideration should be given to undertake the launching of a satellite. All that was lacking was approval and money. Those were the days before "big" science, and while several groups were bold enough to plan and propose, none were bold enough to approve and fund.

The coming of the IGY changed all this. The launching of a satellite was proposed in an international forum and was backed by international scientific unions. Under the prodding of the National Academy of Sciences and the newly formed National Science Foundation, the government agreed to include such a program in the IGY and set out immediately to find a group to carry out the project. NRL won out in the competition which took place during the summer of 1955. The Laboratory was notified on September 9, 1955 that it had been chosen and received funds to proceed on October 6, 1955.

Recognizing that the project was of a magnitude greater than that for which a division was geared to handle and that the successful development of the project would call for talents from many divisions in the

Laboratory, the Director, Capt. Tucker, and the Director of Research, Dr. Hulburt, decided to form a group outside the division structure to carry out the project. As Superintendent of the Atmosphere and Astrophysics Division, out of which the proposal emanated and in which resided the majority of the competence in this area, I was asked to lead the new group. It was called Project Vanguard.

The Vanguard team's first pressing problems were to properly organize for the difficult task and to immediately get work started on the design and construction of the vehicle. Experience in the Department of Defense (DOD) at that time showed that in missile programs it took more than 5 years from the start of a program to arrive at the date of the first successful launching. Vanguard had 3 years to launch the satellite in the time frame of the IGY. The team achieved the objective in 2 years, 6 months and 8 days. To make this possible, groups were organized to develop a suitable launching vehicle; to develop a scientific program which would design the satellite; to miniaturize the instruments to be placed in space and arrange to telemeter the observational results back to earth; to develop a worldwide system of tracking which would be capable of monitoring the progress of a multistage vehicle during launch and then to precisely track the satellite while in orbit; to find and instrument a suitable location for launching the vehicle; to provide liaison with and to coordinate the work in other parts of DOD in support of the Vanguard effort and to help in administering the project through the development of a fiscal plan and managerial plans to ensure adherences to schedules; and to prepare the many presentations that were to be made to DOD committees and congressional committees concerned with the funding of the project and its progress.

In October the team went into full operation. Difficult negotiations were conducted with the vehicle manufacturers, and an agreement was obtained which established the Laboratory in full control of the vehicle design and schedule. After a canvass of all other possibilities, Cape Canaveral was chosen as the launch location. At that time the Cape was instrumented solely as a missile test facility, so instrumentation suitable for launching a multistage vehicle, where critical functions had to be performed many hundreds of miles from the launch pad, had to be designed and procured. These down range facilities were installed as far east as Antigua. In the end, because the Air Force and Army claimed full occupation of launch facilities and since Vanguard had no military priority, we were forced to build our own hangar, blockhouse, and launch stand. Radio tracking was chosen for the orbital tracking of the satellite. A system of angle tracking developed at NRL for use at White Sands was further developed and became the Minitrack system.

To obtain certain coverage of the satellite at least once per orbit a fence of Minitrack stations extending from Santiago, Chile to Blossom Point, Maryland was installed; and crews, some of the members being natives of the different countries, were trained for the operation of the system. In this phase of the work we had excellent cooperation from the Army. The tracking stations also collected telemetered data from the satellite. For this purpose a communication network was established to link all stations, the Vanguard computer, Cape Canaveral, and a central control at NRL.

All the while the Scientific Program group worked with the committee for the IGY of the National Academy of Sciences to select a series of experiments from scientists around the country and then to work with those scientists in preparing the experiment so that it would stand the rigors of launching and flight.

Vanguard was never properly funded. A small initial appropriation was made by Congress to fund the satellite effort in the IGY through the National Science Foundation. As the program evolved and larger demands were placed upon it, funding became a serious problem. Frequent budget justifications had to be made, with the funds in the end coming out of the DOD emergency fund or through mandated reprogramming by DOD or the Navy.

Thirty months of tireless effort by the Vanguard team and its contractors culminated in a successful launch on March 17, 1958, well ahead of the closing date for the IGY, but several months after a spectacular accident at the launching of the first complete test vehicle and several months after the USSR launched its first satellite into space.

In retrospect, Vanguard was a huge success. It started this nation off on its space program and established as the objective scientific investigation and the advancement of knowledge. It provided the facilities for space launching and for tracking. It pioneered the use of large digital computers for orbit calculation. It achieved many "firsts" in space,[1] but the greatest achievement was perhaps the development of the group of scientists and engineers who solved this difficult problem in such a short time and who were above all honest in their relations with others and in their claims for the program. A part of this team later became one of the three building blocks out of which NASA was formed and has gone on in NASA to further successes.

The Naval Research Laboratory was born out of World War I. It grew to maturity in preparing for World War II, excelling in its achievement during the War. It blossomed into a full-fledged scientific institution in applying the talents of its people after the War. NRL was the creation of such men as Dr. Taylor, Dr. Hulburt, and Dr. Hayes, to name but a few. Each of us who spent our developing years at the Laboratory is indebted to them for their vision and courage in building a scientific program which was good for science and also good for the Navy, and in developing a method of operation for the Laboratory under which scientists and engineers could work with freedom and integrity.

[1] J. P. Hagen, "The Viking and the Vanguard," *Technology and Culture* 4 (1963).

From Sea to Shining Sea

J. A. Sanderson

Dr. John A. Sanderson came to NRL in 1935 from Johns Hopkins University, where he was an instructor in physics and astronomy. He became Head of the Radiometry Branch of the Optics Division in 1939 and Superintendent of the Division in 1949. He retired from the Laboratory in 1965 and became President of the Optical Society of America. His more detailed history of Optics at NRL appeared in the December 1967 issue of *Applied Optics*.

When I joined the old Physical Optics Division in 1935 the Naval Research Laboratory was only 12 years old. But Optics had already become something of a sea-going Division, along with several others whose work related closely to the operations of the Navy on and under the sea and in the atmosphere of the earth.

As a part of my welcome as a member of the NRL, E. O. Hulburt told me about the policy he had set for himself when he became the first Superintendent of the Optics Division on June 4, 1924. (It was then called the Heat and Light Division; I will call it "The Optics Division" with the certain knowledge that this title will be understood.) He said that he thought a naval optics program should include subjects of general scientific interest, not under thorough cultivation elsewhere, which would lead to results of interest and potential usefulness to the U.S. Navy.

These introductory conversations also left no doubt in my mind that I was to consider myself a member of the NRL and the Navy, and not merely an employee or hired hand. That was the doctrine throughout NRL, and certainly it has had a profound influence on the long-term strength and stability of the Laboratory.

My first assignment was the photoelastic determination of stresses in destroyer deck plates, under the tutelage of H. B. Maris. This certainly fitted into the concept of finding Navy needs for optical research and it led me of necessity, to read much about ship design, which is a romantic subject in its own right. Men have worked hard at it at least since the time of Noah, and those who have been involved in it surely see a little more than a visual image when they watch a destroyer cutting through a rough sea with a bone in her teeth or a sail boat making serene passage.

The present Optical Sciences Division still owns, I trust, a small Hilger E-31 spectrograph whose life story well illustrates the policy of continuing outreach toward adventuresome projects of concern to the Navy:

LTCDR Malcom P. Hanson carried it to Little America on the Byrd Antarctic Expedition in 1929, to photograph solar and lunar spectra in the hope of learning something about the amount of ozone in the upper atmosphere in those latitudes.

Harry B. Maris carried it to Alaska and up Mount McKinley in the early 1930's in what was called the international Polar Year—the forerunner of the International Geophysical year. He used it and another spectrograph of his own construction to photograph spectra of the night airglow from the altitudes he could reach by climbing the mountain. As was often the case with him, Maris was ahead of his time; he needed balloons and rockets.

Then the little spectrograph was loaned to Dr. Thompson at the Naval Weapons Laboratory, Dahlgren, Virginia, to investigate the flash of excited atmospheric gases seen when a projectile strikes armor plate. The spectrograph got struck by a shell fragment, which made a mess of the collimator. But the machinists fixed that and it came home to NRL in perfect repair.

Its next major adventure was Hulburt's use of it to photograph the spectrum of a nuclear detonation at Bikini in 1946, where we measured the total optical radiant energy and its spectrum from a ship, the USS KENNETH WHITING, and also from an airplane. Perhaps never has a ship been so carefully navigated by dead reckoning, out of sight of navigational aids, as was done by Captain A. R. Truslow to assure that our little exploding star, when it appeared, would be directly abeam and squarely illuminate the really tiny optical receiving areas of our several instruments.

The little instrument which suggested these tales went on numerous additional expeditions: to the Dry Tortugas, Eniwetok, and, of course, the Chesapeake Bay Division, a remarkably advantageous component of NRL. The examples cited are sufficient to illustrate a twofold principle involved in such program planning: awareness of the potential interest and usefulness to the Navy of quite unusual experiments; and appreciation of the advantages available to the experimental scientist through the extraordinary resources and capabilities of the U.S. Navy. To one looking back, as I am doing, there come immediately to mind many examples of the positive approach to our problems by all parts of the Support Services Department and the Naval Staff. In many of our larger programs we constructed and shipped tons of equipment to remote areas of the world. We used methods recommended by our colleagues in various Support Service Divisions. We needed and asked for officer assistance in specific projects from time to time, and we always got it, either from a DoD Joint Task Force or from the NRL Naval Staff.

There is surely nothing new either to old-timers or to present members of NRL in this brief article. But the ability of NRL to develop along such lines of responsible self-direction for 50 years surely warrants anticipatory congratulations for its future service to the United States of America.

Fracture Mechanics

G. R. Irwin

Dr. George R. Irwin was awarded his Ph.D. in physics by the University of Illinois in 1937. He joined NRL that year and pursued research in armor and other high-strength materials, projectile penetration, and deformation and fracturing in solids. Dr. Irwin was Superintendent of the Mechanics Division from 1950 until his retirement in 1967. He is presently associated with Lehigh and Maryland Universities.

The invention and development of fracture mechanics at NRL was an outgrowth of NRL research on armor. During the 1937-1942 time period, the Ballistics Branch of NRL developed a group of laboratory penetration ballistics installations by using silencers and a minimal distance from gun muzzle to target. Because of the convenience, variety, and speed of testing thus permitted, NRL soon gained a leadership position in the development of aircraft and personnel armor, which continued through the Korean War period. There was, however, a secondary objective of our ballistics work directed toward embrittlement problems of heavy armor for battleships. We could not model this problem with our laboratory-size ballistics facilities, because of the fracture size effect. A degree of brittleness which was regarded as tolerable in a 1/2-inch-thick plate was not tolerable in a plate with a thickness of 3 inches or more. Given adequate control of cooling rates, however, the steel-corporation metallurgists knew generally what should be done to prevent heavy-plate ballistic failures in the tests at Dahlgren.

There remained the nagging questions of how to explain fracture size effect, how to measure fracture toughness, and how to relate this answer to the section thickness of the metallic component of interest. It was clear that quick answers to these questions were unlikely. As a result our effort on this topic during World War II was limited to exploratory studies of fracture size effect conducted mainly by contract at the University of North Carolina. In some respects the results were repetitious of previous work with low-strength structural steels. However, the literature survey and technical discussions provided by this investigation clarified the nature of the problem (1).

Within a few months after the end of World War II, we revised the university contract work and initiated fracture studies in the Ballistics Branch; NRL research on what was later termed "fracture mechanics" became a program with a basic plan. It was evident that, under brittle conditions, there remained a substantial resistance to crack extension in the form of plastic deformation near the leading edge of the crack. We proposed to measure this resistance, calculate the elastic stress field driving force (roughly in the manner suggested by the 1920 Griffith theory of fracture strength), and show that an energy rate match existed between the driving force and the plastic deformation resistance at the point of onset of rapid crack extension. It was possible to obtain enough information from the first year of this program so that the writer could demonstrate plausibility of the basic approach in a 1947 ASM Symposium paper entitled "Fracture Dynamics" (2). During following years, the in-house "mechanics of fracture" studies grew in size and the contract work at the University of North Carolina was gradually phased out. The growth at NRL was mainly due to our irrepressible T. W. George, Head of the Armor Materials Section, Ballistics Branch, and our equally enthusiastic J. A. Kies, Head of the Fracture Studies Section, Ballistics Branch. T. W. George decided to try commercial applied research and went to Celanese Corporation in 1955. However, from the experimental verifications of the basic plan which were by that time available, all three of us understood that we had started the development of a new area of strength of materials research, a field much too large for the isolated program at NRL (3,4). Furthermore, from critical discussions of our viewpoint at technical meetings, we could see no hope of significant development assistance.

The above problem was overcome during the 1952-1960 period by a series of successful applications in practical problems, problems large enough to attract attention. In chronological order these were the development of stretch-toughened Plexiglas, the pressurized fuselage fractures of the DeHavilland Comet commercial jet airplanes, the sudden fractures of heavy rotating components of large steam turbine electric generators, and the hydrotest fractures of ultrahigh strength steel rocket chambers for Polaris. We assisted in the solution of each of these problems. In return each of these applications was a learning experience and added influential converts to fracture mechanics.

The term "fracture mechanics" was coined and used locally at NRL in the early 1950's. By 1957 we had sufficient confidence in our strain-energy-release rate concept to give it a symbol, $\mathcal{G}$ (for A. A. Griffith), and a new name, crack-extension force (5). Encouraged by possession of a sound "force" concept, we began at that time to use the term "fracture mechanics" in open technical discussions and in technical papers (6,7). Currently, technical papers on fracture mechanics rarely use the crack-extension force, $\mathcal{G}$. Instead they employ the stress-intensity factor, K. From crack stress field analysis one can show that $K^2 = E\mathcal{G}$, where E is Young's modulus. The first of the applications noted above, stretch-toughened Plexiglas, was a Joe Kies project. From Irv Wolock, then at NBS, Joe Kies learned that warm stretched Plexiglas was highly resistant to craze cracking in the presence of alcohols. He reasoned that the stretched Plexiglas must also be "toughened," persuaded Rohm and Haas to verify this idea, and soon there was a large Air Force program on stretch-toughened glazing material for combat airplanes. Initially the toughness was verified by use of an NRL-designed fracture toughness evaluation test. Joe Kies explained that determinations of the fracture toughness, $\mathcal{G}_c$, which required knowledge of E, were unnecessary. For practical applications, one only needed $\sqrt{E\mathcal{G}_c}$, a factor which was independent of the elastic modulus of the material. The response of those concerned with testing stretch-toughened glazing materials was to report their results in terms of critical values of K (for J. A. Kies).

Skipping over nonmilitary applications, during the fall of 1957 a serious risk to the time schedule of the Polaris program arose because too many of the welded ultrahigh-strength steel rocket chambers broke during hydrotesting. The completed chambers which passed this test were, in fact, inadequate for firing trials. The main responsibility for solving this problem was given to the fracture mechanics team at NRL. It seemed desirable to increase the size of the NRL team (J. A. Kies, Herschel Smith, Villette Sullivan, Joe Krafft, Steve Hart, and the writer) in order to ensure adequate effort. By means of funding transfers, we enlisted Harold Bernstein at the Naval Gun Factory and Dr. H. Romine, at the Naval Proving Ground. Subsequently we added a small group at Frankford Arsenal and several university contracts on special topics. Meanwhile, beginning in early 1958, we were examining and writing brief trip reports on every Polaris rocket chamber hydrotest fracture failure. Most of the uncertainties disappeared when we established two facts: (a) the fracture failures were due to fabrication cracks having final dimensions which corresponded to the failure stress in terms of the fracture toughness of the steel; (b) these flaws were invisible to available techniques of nondestructive inspection and would remain in that category unless the welds were ground so as to produce smooth surfaces. This much was accomplished by September 1958 and permitted timely corrective actions. The remedies were not inexpensive to the Navy. Sophisticated welding equipment was procured by Aero-Jet in order to achieve adequate fabrication process control. Inspection improvements allowed by smoothing the welds were also quite helpful. The Minuteman program, phased about 1 year behind Polaris, also encountered fracture problems and received some help from NRL. As with other aspects of Minuteman, guidance from the Polaris learning experience was very helpful to that program.

The toughness evaluation methods and the criterion for adequate toughness introduced by NRL for use in the Polaris program are still widely employed. Meanwhile, usage of fracture mechanics in areas other than toughness evaluation such as fatigue, stress corrosion cracking, and adhesion continues to grow. Although acceptance and use of fracture mechanics is not worldwide, the objectives of the NRL team were definitely nationalistic. We wanted to assist solution of U.S. Navy problems, and we wanted our country to lead in knowledge and applications of fracture mechanics. The record indicates achievement of both goals.

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A Laboratory Is People

P. King

Dr. Peter King joined NRL as a scientist in 1939 and received his Ph.D. from Catholic University in 1942. He was promoted to Branch Head in 1944 and to Superintendent of the Chemistry Division in 1954. His research interests included organic chemistry and radiochemistry. Dr. King served as Associate Director of Research for Materials from 1956 until he became chief scientist of ONR in 1966. He retired in 1972.

"If you hear any noise around here, shoot first and then go see what it was."—One evening very early in the War (World War II, that is), when several of us at the Lab were staying a bit late because we wanted to, I heard these directions being given by a marine sergeant to a member of that night's guard force. There was a break in the fence next to the building due to some construction (so what's new?) and they were examining the break just below me, unaware of my presence. It obviously was time to leave before it got too dark, seeing that we would undoubtedly make some noise close to that break when we left and we wanted safe passage! Despite the humor of the situation there was something else more significant. Someone had told the "Sarge" that the work going on was important to the Navy and the nation, and he believed it, thus joining a small club. He was giving physical protection and other kinds were also needed—financial, continuity and freedom of investigation, and these too were being protected by the then also small group of civilians and officers who were in a position to do it and believed in the "Lab."

"If I hadn't had the FTS (False Target Shell), I would have lost the boat and crew."—How does one react to a statement like this in a war patrol report by a submarine skipper? I have seen the reaction and what it is is great satisfaction and the encouragement to try something else new. The feelings of the individual really cannot be described but at least he can now go on even if what he is working on at that moment is equally foolish in the minds of some uninitiated.

"Without the rebreather we would have lost the ship."—This statement was made in reference to a "rebreather" used in a very bad fire aboard one of our carriers. When reading this one can forget all the extra effort and time that went into setting up a small pilot plant to produce in quantity a superoxide that was just a laboratory curiosity a short time before.

How do you convey the feeling of the place and the people in it, to the unbelievers who have not been a part of it? How do you make them understand that this history, this desire to do something is not just of the past but is now going on with different people and different problems and that "protection" is still needed?

What was it that made people drop into the "Lab" on Saturdays during mid-1940 and up through the first eleven months of 1941? It was not money, for they did not get any for doing it. It was not for glory, because there was none of that either. We were not at war, so it was not that either. It was done because people saw things they knew needed to be done, and they wanted to do them. There was an atmosphere of urgency surrounding most of the NRL people, which made them do what they thought was needed. As time passed more and more scientific types appeared on Saturdays until finally one would believe from the number of cars that it was just another normal day. This was the spirit of the place then and still is.

It is not the intent of this very brief paper to recite the glamour items and scientific accomplishments of the Laboratory. These are listed in many places. More important to me is to try to understand or find what it is that makes the place productive and a good place to be. As expected it is not something simple or singular. It is a combination of so many things—some funny things that happened—the presence and availability of so many people expert in diverse fields—facilities, machine shops for example not found any place else, and with this, mechanics who were friends and willing to do anything reasonable or unreasonable (*e.g.* who else would accept without question a request to get a crew to scrub the roof of one of the buildings and to save the water used for the job!) in making apparatus. These are all physical items which one can easily identify. In addition others, intangible, were even more important. One was mutual respect between civilian and military found in few other places to the degree which was existed at NRL. This had to be real respect to survive after seeing some of the military people appear at work on December 8, 1941, in uniforms that obviously had not been tried on for several years. It was very clear that some people had expanded and were caught tight rather than short. But besides that the opinion of the military was really sought for both ideas and help in evaluation—things beyond research and in the development stage. That the respect was mutual was demonstrated in the scientific freedom enjoyed by the staff. Where else would you have found a chemical stockroom with small amounts of as pure rare earth elements as were then available, not just a few but many? Or where else was gathered early a better collection of very pure surface active chemicals? Such things were possible only because of the respect the Navy people had for the judgment of the scientists in their own business.

What finally defines the "Lab" and makes it possible to make an FTS, to make a superoxide in large quantities, to find radioactivity in rain (and on roofs), to produce quantities of diborane, and to paint anything (from small parts of radar to determine operating temperature, to a half million barrel oil storage tank), is the people. If we just follow the example of the "old timers" (*i.e.*, anyone before my time), be selective in hiring staff, and support it when hired, then the present NRL will be no different from the past, and 50 years from now the same things can be written. Also we will have many more members for that Sarge's exclusive club.

An Example of Why Research Cannot Be Planned

W. A. Zisman

Dr. William A. Zisman joined the Chemistry Division in 1939 and became its Superintendent in 1956, a position that he held until 1968. Since then he has been Chief Scientist of the Laboratory for Chemical Physics. He was honored for his contributions to pure science and his applications of surface chemistry to practical problems by being named to a newly created Chair of Science in 1969.

In 1941 D. L. Pickett and I were preparing a 0.1-percent solution of n-heptadecylamine in purified white mineral oil for use in spreading experiments. While gently shaking the solution in a Pyrex flask, we were very much surprised to observe that the oil rolled down or "peeled off" the interior surface of the flask and left a clear and apparently unwetted area behind it. This kind of behavior had been observed by Langmuir years before with oil drops rolling down the surface of an inclined glass plate coated with a Langmuir-Blodgett stearate multilayer, and he had described this as oleophobic behavior. I could not believe that we were observing the formation of a Langmuir-Blodgett multilayer on our flask wall; we thought the flask was not clean or the oil contained water. The experiment was repeated many times with increasing care, but the same oil retraction of the amine solution resulted. When the heptadecylamine was dissolved in various pure nonpolar solvents such as n-hexadecane and dicyclohexyl, essentially the same results were encountered. Any one of a number of paraffinic polar compounds such as palmitic and stearic acids and eicosyl alcohol behaved in the same way.

These observations led me to infer that we were observing the formation on the glass of an oriented film adsorbed from its original state in solution in the organic solvent, the orientation of the film being such that the polar end of each amine molecule was adsorbed on the glass wall, whereas the hydrocarbon chain with its terminal methyl ($-CH_3$) group was extended so that its principal axis was perpendicular to the wall. I proposed that the reason the oil was rolling off the wall was that it could not wet the surface of the adsorbed film because the outer surface of the film was composed of methyl groups.

Similar films were adsorbed more readily on clean glass microscope slides and also on polished, clean, flat strips of platinum, stainless steel, chromium, and nickel. It was sufficient simply to immerse each slide in the oil solution and slowly remove or retract it from the liquid at right angles through the oil-air interface. After removing each slide from the oil solution, we could readily measure with a simple goniometer the contact angle exhibited by an oil drop. Contact angles around 30° were common at room temperature. Experimentation with the effect of tilting the plane of the "retracted" oil-free specimen revealed that a slight tilt from the horizontal caused the drop to roll off the surface readily. The surface was apparently unchanged by contact with the oil, because the oil-drop experiment could be repeated again and again. Hence, it was correct to describe the adsorbed film on the solid as an "oleophobic film."

Further experimentation with retracted oleophobic heptadecylamine films on such solids was done with the use of a flame-cleaned, polished, platinum foil. Similar results were obtained with other long-chain amine, acid, alcohol, and amide compounds. Approximate measurements of the contact potential difference occurring between two pieces of platinum foil, one of which was coated with an oleophobic monolayer of stearic acid, showed that the presence of a single adsorbed film caused a potential difference of 0.2 volt. We concluded that each such retracted adsorbed film on platinum was a monolayer of the polar compound oriented at right angles to the metal foil and arranged in close-packing.

As urgent research problems arising from World War II swarmed out of the Navy Department into the Laboratory, further investigations of these interesting films became impossible. However, later in the War, I persuaded Professor L. O. Brockway and Dr. Jerome Karle of the University of Michigan to examine our oleophobic monolayers of stearic acid and octadecylamine on platinum by using electron diffraction techniques. Subsequently, they reported that each of the two polar compounds had formed a monolayer on the platinum and that the polar molecules were oriented along the normal to the solid surface with a random tilt of several degrees of the principal axis of the molecule.

In an early postwar paper Bigelow, Glass, and I reported a method by which the energy of adsorption per long-chain polar molecule adsorbed on platinum from nonpolar solvents was estimated at from 10 to 14 kcal. per mole. These values obviously corresponded to a physical adsorption process. Since the energy of adsorption per mole increased linearly with the number of methylene ($-CH_2-$) groups per polar molecule, the energy increment per aliphatic carbon atom was in accord with available literature estimates of the energy of molecular cohesion for adlineated (or crystalline) paraffinic compounds.

Further experimentation revealed that a wide variety of polar paraffinic compounds could be adsorbed on smooth clean Pyrex glass, silica, and sapphire and on numerous clean polished metals by retraction of the solid from the molten compound. This was a welcome find since it led us to a simple method for preparing a variety of adsorbed condensed monolayers of many pure compounds without any possibility that solvent molecules could be present in the adsorbed film.

By retracting a monolayer from a dilute solution of octadecylamine in dicylohexyl on a polished platinum dipper and then removing it by heating the foil red hot so as to destroy the film, and by repeating the same procedure again and again, we could count the number of retracted films possible until the supply of amine solute in a given small volume of solution had been exhausted. By dividing the number of retractions possible into the total number of solute molecules initially present in the sample of solution, we could estimate the approximate cross-sectional area per adsorbed molecule of the retracted compound. The resulting value of somewhat less than 30 square angstroms per molecule of amine left no doubt that the film was a condensed monolayer. Many solutes besides aliphatic polar-nonpolar compounds could adsorb as oleophobic films; included were a great variety of other less simple compounds whose adsorptive polar groups existed at one extremity of the organic molecule and whose methyl groups existed at the other extremity.

Systematic measurements were made of the wetting properties of various smooth, clean, and polished surfaces, each coated by a retracted monolayer of a variety of polar-nonpolar compounds. Small drops of various well-defined pure liquids exhibited contact angles reproducible to $\pm 1^\circ$ when placed gently and slowly advanced on the monolayer-covered surface of the solid specimens. Such contact angles exhibited by drops of each liquid were essentially independent of the nature of the solid substrate! The resulting surface properties of these monolayers and the retraction process by which they were produced attracted our increasing interest, since the retracted films had been obtained under conditions of adsorption equilibrium at the solid-liquid interface. Such films were later found to occur widely in the arts and in technology.

The large and reproducible contact angles observed with a few organic liquids on various types of retracted monolayers intrigued us and led Elaine Shafrin and me to investigate the retraction of a variety of dissolved organic polar-nonpolar molecules adsorbed from their aqueous solutions as a function of the solute structure and concentration as well as the hydrogen-ion concentration. Because water has one of the highest surface tensions among liquids (72.8 dynes/cm at 20°C), the retraction technique allowed us to adsorb and to isolate for further examination monolayers of hydrophobic adsorptive compounds of a great variety of organic structures. The competitive adsorption by hydrogen-ion and hydroxyl-ions in the water was especially of wide interest.

By 1950 our investigations of retracted monolayers began to open up for us a major field of research on the subject of the wetting of solids. Although contact angles and wetting had been described in an apparently simple way by Thomas Young in 1805, his theoretical equation was not very useful; later it was improved by others with emphasis on a more thermodynamic approach. Attempts by latter investigators to use the Young equation to explore the subject resulted in only minor advances. Available experimental data and their significance were the sources of much argument, and even by 1940 the subject was still obscured in a cloud of philosophical and theoretical arguments and experimental difficulties. By 1955 we had opened new approaches to the relationship of the contact angle to solid surface and liquid composition and properties through the retraction method. The success in obtaining reproducible contact angles for sessile drops led us to broaden greatly the variety of liquids studied and the kinds of films retracted.

In 1957, following a different pathway, Levine and I used retracted monolayers on hard solids to investigate the boundary friction of monolayer-coated solid surfaces; this investigation advanced the understanding of that subject. In 1963 another path was followed with Karl Bewig to succeed in explaining the effect of an homologous series of adsorbed organic monolayers upon the contact potential differences among a variety of metal substrates. Incidentally, both friction and surface potential investigations could readily distinguish under what special conditions a solvent could be included between the molecules of a retracted monolayer.

When, in 1950, H. W. Fox and I examined the contact angles of various pure liquids on many clean, polished, hard, metallic or nonmetallic "high energy" surfaces, we tried to maintain them free of adsorbed organic films. Later we found that a major difficulty arose from the fact that such surfaces readily adsorbed any contaminating films of water, gases, or organic material which often originated from pollution of the atmosphere or in the storage chamber. We reasoned that, because such solids have very high surface energies, each would be expected to have great susceptibility to the adsorption of gases and liquids and so lower the surface energy of the solid. Therefore, we focused attention first on the spreading of pure liquids on such low-energy solid surfaces as those of linear polymeric solids like perfluorocarbons and polyethylene which were made smooth, glossy, and clean. Because the majority of liquids were nonspreading on polytetrafluoroethylene (Teflon), we presumed that Teflon had an extremely low surface energy and therefore we investigated how the contact angles of the homologous n-alkanes varied on these surfaces. Subsequently, we also examined the contact angles of many other chemically well-defined linear polymers by using the homologous series of n-alkane liquids and obtained new data which enabled us to show that rather simple relationships existed between the cosines of the contact angles and the liquid surface tensions.

Further investigations of polymeric surfaces revealed by 1953 that when the wettability data concerning a polymer or of a monolayer-coated high-energy solid were compared, the results were surprisingly similar so long as the surface composition of the solid was essentially the same. This led us to the concept of the critical surface tension (γ_c) of the solid surface. By 1960 Mrs. Shafrin and I had found how the value of γ_c could be predicted from the solid surface composition. Soon it became possible for us to bring much order into the hitherto obscure subject of the contact angle and wettability. The results from this continuing series of investigations on wettability *versus* solid and liquid constitution were summarized in 1963 in the ACS Kendall Award Symposium. As a by-product of the evolution of these laws relating γ_c to wetting, we finally were able to explain the mechanism of the retraction process from which we had started in 1941!

In this 50th Anniversary Symposium commemorating the founding of NRL, I would like to express my appreciation for the support ONR has given me through so many years of my postwar scientific career—the support that made possible the kind of research I have described in this paper.

Combustion Suppression at NRL

R. L. Tuve

Dr. Richard L. Tuve came to NRL in 1938 after holding positions with the Department of Agriculture and in private industry. Here he studied some of the critical factors involved in controlling and extinguishing flames from hydrocarbon fuels and discovered the fire-fighting capacities of fluorocarbon surface active agents. Following several years as Head of the Engineering Research Branch of the Chemistry Division, Dr. Tuve became Head of the Combustion Suppression Research Center and, subsequently, a consultant to the Associate Director of Research for Materials. He is currently associated with the Applied Physics Laboratory, Johns Hopkins University.

The year of 1938 was a notable one at NRL, primarily because of the first successful shipboard test of "bed-spring" antennae for the yet unnamed radar system. In that year too, a unique and new chemical and engineering research effort began which was destined to put NRL and the U.S. Navy in a position of world leadership in the field of protection of men and material from the ravages of accidental fire.

The origins of this new research project in fire protection sprang from Germany, our enemy to be, where a U.S. Naval Attache came upon a new kind of liquid material for fighting fires in gasoline and other flammable fuels. At the time, our aircraft carriers and fuel storage stations used a bulky and sensitive chemical powder, which only *sometimes* worked to put out a fire in an emergency—provided that conditions of operation were fully favorable.

The tests and fire studies of the NRL scientists on this project in the 1938 to 1941 period were conclusive in showing the merit of these new type, easily and quickly employed liquids for producing light, frothy foam from air and proteinaceous materials, without their undergoing a chemical reaction: The foam quickly and efficiently "smothered" the fire in fuels which water could not cool and extinguish. Working closely with U.S. industry and officials in the Bureau of Ships and Bureau of Yards and Docks, NRL chemists and engineers guided the production of the first successful U.S. formulations of a protein liquid concentrate for generating the new "mechanical" or "air" foam for fuel fire fighting of all types.

It took only a short time in those early World War II days for massive production of the new liquid, sufficient in volume to equip all the ships of the Fleet and our Naval Advance Bases with superior fuel fire fighting tools; materials which had evolved from NRL tests and research and which became nationally employed after the conflict.

Wartime activities of this team of chemical investigators were widely varied. They were named the "Special Research Section" of the Chemistry Division and turned their attention to the use of dyestuffs in warfare. From this work came the green fluorescent "Dye Marker" for sea rescue of personnel, responsible for the documented rescue of 157 lives. Ichthyological research was also undertaken and today's "Shark Chaser" packet, worn by all the world's military aviation personnel, came from their work in the period 1943 to 1945. Naval ordnance used the results of this team's dye chemistry research before the development of radar ranging of projectiles.

The fundamental character of the fire research conducted by this group led to the overthrow of several theories concerning the basic action of foams on hydrocarbon fuel fires. In 1948 these scientists were invited to present their data to a technical committee of the National Fire Protection Association. It was not long after this that the section of the National Fire Codes pertaining to the use of foams for fire protection of fuels was completely rewritten to embrace the discoveries from the NRL fire research. NRL continues to be internationally represented in these Fire Code deliberations.

It is not surprising that the NRL research in fuel fire protection was used from its early stages onward in the continuing development of special vehicles for the extinguishment of fires at aircraft operations areas. The first special foam-making trucks for aircraft accident fire fighting developed by the then Bureau of Aeronautics (1945) employed giant "snowstorm" foam nozzles which followed NRL design recommendations. Many of today's modern airport fire protection vehicles all over the world have continued these precepts to a wide extent.

Fire research at NRL did not stop with protein foam developments, even as they became internationally used, and in 1959 a series of original investigations by this group in the area of chemical flame extinction by the highly complicated free radical quenching mechanism gave birth to the discovery of a new, dry, chemical agent, potassium bicarbonate powder.

Powdered bicarbonate of soda as a flame halting agent had been employed for many years, but its action had never been satisfactorily explained. Working with other investigators, the NRL conducted fire test after fire test with many different powdered substances, helping to clarify and elucidate the chemical actions involved. This work came to the final conclusion that the simple substitution of the potassium ion for sodium extended the flame quenching efficiencies of chemical powders by a factor of *two*. Again, U.S. industry was called in and the new highly effective product, called "Purple-K-Powder" by its NRL discoverers, became used throughout the Navy and in U.S. municipal and industrial fire protection, and then in the world.

It is not without precedent that the fertile minds of technical investigators such as those represented by the NRL fire research team continue to dwell upon the shortcomings of some much earlier research results and try to devise ways to improve on them. Such deliberations led to a completely new and different attack in 1959 on the problem of making water into a more efficient fire fighting agent for fires in flammable fuels which were lighter than water. Even in the form of highly effective protein foam, the flame-cooling water component sinks ineffectively to the bottom of burning fuels without exerting useful effects on a fire.

Over a span of about 3 years, the NRL team conducted fire tests, laboratory experiments, and detailed studies, utilizing all that could be learned concerning surfactants, foaming agents, interfacial tensions of liquid-gas regimes, and the physical chemistry of liquid surfaces. New and promising chemical surface modifying agents were carefully studied at length with the hope that in some way, the characteristics of water on a burning hydrocarbon surface could be changed so that the water in foams would be more effectively used.

One new chemical compound showed distinct promise during this investigation even though it had a price of $80 per pound. However, when this synthetic chemical was properly formulated to make a new foam material, its price was lowered and its efficiency showed an astounding three- to fourfold increase in fire-killing capacity over the old protein foam-making materials! A dilute water solution of the new liquid spread quickly over the burning surface of gasoline, putting out the flames and keeping them from reignition, *without* sinking as ordinary water would have done.

The new foam-forming liquid was named "Light Water" by its NRL discoverers, a name that has become world-known, since its announcement in 1962.

Not content with this great stride in fire-fighting capability, the NRL fire research group linked up this new nonsinking, highly efficient "Light Water" material with the potassium bicarbonate flame quenching powder in a unique dual-nozzle apparatus to be used by a single fire fighter. It was an instant success. In repeated tests a *twelvefold* advance in extinguishment capability was consistently attained by this "Twinned Agent" fire fighting system.

The "Twinned Agent Unit" was quickly put into use by the then Bureau of Naval Weapons (1965), and industry in the U.S. and in foreign countries began its commercialization. In 1967, at the time of the FORRESTAL aircraft carrier disaster, these units were manufactured in quantity sufficient to equip all our carriers with the new device.

The merits of the NRL-developed "Light Water" material have reached distant audiences. It has supplanted protein foam in every U.S. military service installation. All large U.S. airports and some foreign aerodromes depend on "Light Water" for aircraft fire protection. Many petroleum refineries use the material in preference to the old protein materials with their much poorer performance. The National Fire Codes have been further amended to assure the benefits of the NRL research developments to all U.S. fire-protection needs.

The Genesis of Rocketborne Space Research

E. H. Krause

Dr. Ernst H. Krause was a research physicist at NRL from 1938 to 1951 and Associate Director from 1951 to 1954. Since then, he has held a number of executive positions with firms doing aerospace and systems research and is currently senior Vice President for Development of the Aerospace Corporation. His work has ranged over several fields of pure and applied research, notably missile systems, rocket development, radio astronomy, and guidance systems.

During its half-century of existence, the Naval Research Laboratory has provided leadership in many fields and has made innumerable salient contributions to the advancement of science and technology. The most dramatic and far-reaching of these contributions in my judgment is the pioneering of high-altitude and space research which led this country into space. Not only did NRL provide much of the groundwork for space science and engineering, but many of today's leaders in space exploration were among the group who started the programs at NRL more than a quarter of a century ago.

The genesis took place in early 1946 (1). A large number of captured unassembled V-2 rockets were being shipped to the White Sands Missile Range for assembly, test, and launching. The concept of using some of the rockets for high-altitude research occurred to some of us. It was clear that a platform capable of lofting 2000 pounds of payload up to altitudes of about 100 miles would make possible the clarification of many unknowns in the fields of solar radiation, cosmic radiation, atmospheric-density-temperature profiles, and the characteristics of the ionosphere. We made a proposal to the management of NRL, to the Office of Naval Research, and finally to the Army (which had the responsibility for the assembly and launching of the V-2's). All agencies responded enthusiastically, and an Upper Atmosphere Research Program was created. A triservice (Army-Air Force-Navy) committee, the V-2 Upper Atmosphere Research Panel, was formed to coordinate the use of the limited number of costly boosters. The committee membership was drawn from the agencies involved, including the Air Materiel Command, the Applied Physics Laboratory, the Army Signal Corps Engineering Laboratories, Michigan University, Princeton University, the California Institute of Technology, and the Naval Research Laboratory.

The first of the V-2's was successfully launched on May 10, 1946. Sixty-six V-2's were assembled and launched in the period from 1946 to 1951, carrying aloft a total payload in excess of 100,000 pounds to altitudes between 50 and 120 miles. During this period, research was also carried on by use of the Aerobee rocket, which carried small payloads to about 50-mile altitudes.

Because the number of V-2's was limited, it soon became apparent that new boosters would be required. The Viking, a research-oriented high-altitude sounding rocket, was designed and subsequently built by the Glenn L. Martin Company, under the technical direction of NRL.* The Viking introduced new

*See M. W. Rosen, this issue, p. 49.

features that have been incorporated in later designs. It used all-aluminum construction and propellant tanks integral with the skin. The resultant weight saving produced a mass ratio of 0.80, which was phenomenal for that time. Another important innovation was the use of a gimbal-mounted motor, so that corrections for deviations in the flight path were made by altering the line of thrust.

Eleven Vikings were built and fired in the program, carrying an additional 10,000 pounds of payload into space and achieving the highest altitude of any research rocket (136 miles) of its time. The design features of the Viking were used in the Vanguard booster, which launched one of the first U.S. payloads into orbit.

The program fulfilled its promise of new information on the atmosphere and extraterrestrial radiation. Prior to the V-2 program, the physical properties of the atmosphere had been well explored up to 30 kilometers by means of balloons. Above that altitude the model of the thermosphere was based upon ground-level observations. By means of measurements using the V-2's and Aerobee rockets the atmospheric density, temperature, and pressure data were extended to an altitude of about 200 kilometers (2). The major share of the data was obtained by NRL investigators (3,4), and significant differences were found with respect to the previous atmospheric model.

Composition of the upper atmosphere was explored by means of sample collection techniques (5,6) and rocketborne mass spectrometers (7). As could be expected, the early experiments proved to be difficult due to sample bottle sealing problems and contamination by rocket outgassing. Nevertheless, evidence was obtained showing that diffusive separation did not set in below 60 kilometers. Above that altitude the data obtained through the first phase of rocket exploration were uncertain. An NRL group (8,9) measured ozone by means of solar spectrographs carried aloft by the rockets. They found a vertical distribution of ozone in good agreement with the result of photochemical theory and provided the first direct experimental check of the formation of a "Chapman" layer (10). The same group measured the ultraviolet spectrum of the sun by using a 40-centimeter concave grating spectrograph (11). In their first experiments highly resolved spectra were obtained to 2100 angstroms, which indicated no absorption by rare atmospheric constituents other than ozone. An interesting sidelight of this work was the recovery of the spectrographs by separation of the warhead and breakup of the missile. Parts of the missile then had sufficiently high drag that they landed at fairly low speeds. One spectrograph was so slightly damaged in two such impacts that only moderate repairs were needed for another flight. Such good fortune was not universal, and on the third flight the spectrograph was damaged beyond repair.

Prior to the rocket program, knowledge of the ionized layers of the upper atmosphere was by indirect measurements from the ground. The data were limited in that radio probing techniques could not obtain values of electron densities beyond the region of maximum ionization of a layer. Several techniques with continuous-wave (11) and pulsed (12) radio transmissions and Langmuir probes (13,14) were used in V-2's and Aerobees. Both the instrumentation and data reduction were difficult, but valuable data were obtained that helped in developing an understanding of the processes which lead to the formation of ionized layers.

The primary cosmic radiation reaching the neighborhood of the earth from outer space interacts with the atmosphere to produce secondary showers. Balloon and mountain-top experiments had been performed to eliminate as much of the atmospheric side effects as possible, but the flux and composition of the primary radiation had to be inferred from these measurements. The rocket program offered a unique opportunity to obtain direct measurements at high altitudes before the character of the radiation was altered by the atmosphere. As with other experiments, the task proved to be more difficult than had been anticipated. Early experiments (15) showed that the advantage of high altitudes was compromised by the short time of flight, and the rocket mass to some extent replaced the atmosphere in causing secondaries. These difficulties were overcome by designing experiments to measure the energy and mass of each particle, and to provide shielding and electronic discrimination against the secondaries. The intensity of the primary radiation and the proton-alpha particle ratio were established with good agreement by two groups (16,17).

On September 29, 1949, in V-2 No. 49, Herbert Friedman began his brilliant experiments on solar x rays (18).* In that flight, he and his coworkers first detected x rays at 87 kilometers, with the intensity increasing up to 145 kilometers. In 1952 he made measurements up to 200 kilometers in a Viking rocket (19), which showed that the solar x-ray spectrum supplies sufficient energy to account for all of the E-layer ionization. His continued work in this field has gained worldwide recognition and is an outstanding example of the accomplishments of NRL.

Some secondary experiments had unusual fallout. As early as March 1947, cameras were carried in V-2's and Vikings to aid in defining the orientation of the rocket from photographs of the earth. The photographs proved useful for this purpose, but also provided information on large-scale cloud formations which could not be obtained from ground observations. In a real sense they were an unintentional small beginning of today's weather satellites.

In addition to the experiments themselves, very valuable information was gained with respect to operations in rockets and in the space environments. NRL developed the telemetry system used in all of the V-2 launches and pioneered the recovery of data from space by electronic means. The problems of providing continuous communication with a gyrating vehicle were worked out; as were power supply, energy management, and preflight checkout techniques. All of the lessons somewhat painfully learned by the space pioneers were of great value to the later space programs.

This necessarily limited discussion of a few years of NRL's work in one field illustrates the impact that NRL has had on science and technology. In this, as in other fields in the forefront of science, NRL can be proud of its leadership. Today, almost 20 years since I left NRL, I find that NRL continues its scientific leadership; its reputation is nationwide in such diverse fields as plasma research, high velocity impact, laser technology, satellite navigation systems, solar x-radiation, etc.

NRL's performance record in science and technology over the past half-century is matched by very few institutions in this country. It is certainly well on its way toward a century of scientific leadership!

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*See H. Friedman, this issue, p. 52.

The Pre-Vanguard Era at NRL

M. W. Rosen

Mr. Milton W. Rosen came to NRL in 1940 as an engineer in the Radio Division and very early became involved in radio guidance systems for pilotless aircraft. After a brief period of teaching at Cal Tech (1946-47) he returned to NRL to head the Rocket Section of the Rocket Sonde Branch. He became Head of the Rocket Development Branch in 1953 and Technical Director of Project Vanguard in 1955. After the Vanguard group became the nucleus of the newly formed NASA, Mr. Rosen served for two years as Chief of Rocket Vehicles Development. In 1960 he was named Assistant Director of Vehicles, Office of Launch Vehicles Program. He is currently Deputy Associate Administrator for Space Science (Engineering), NASA.

My recollection of coming to NRL is still vivid, because I was interviewed for the job (junior engineer) by Dr. A. Hoyt Taylor, then Head of the Radio Division. Dr. Taylor was a kindly man, who put me at ease immediately, and assigned me to work in the Communications Security Section under Dr. Claud E. Cleeton. There were then only a few hundred engineers and scientists at NRL, most of whom were in the Radio Division.

At the Laboratory expanded and organized to meet the increased workload arising from World War II, a Guided Missile Subdivision was created under Dr. E. H. Krause[1] and I was assigned in it to work on guidance systems for what were then called Pilotless Aircraft. Later in the war, the U.S. built copies of the German V-1, the buzz-bomb, and I was in charge of a project to design a radio-control system for it. My report, R-2616,* "The Development of Remote Control for the VB-2 Flying Bomb," is dated October 1, 1945. I was fascinated by the great progress and variety of German missile developments and kept a card file in which I recorded the characteristics of the V-1, V-2, Wasserfall, and others.

After the War's end, Krause called together his section heads to plan our future. We would meet each day in his office and construct greater and grander guided-missile projects. But I was dissatisfied and suggested that we strike out on a new course, one that used our experience, but was on the forefront of research. My suggestion was that we use rockets, not missiles, but as high-altitude probes to explore the upper atmosphere. At first I had only one convert, Gilbert Perlow, but gradually, he and I won over all of the others. It was Krause, however, who sold the project to the Navy.

The Rocket Sonde Research Branch started business officially on January 1, 1946. Some of those in the initial group under Krause were Homer Newell, Jack Mengel, Conrad Hoeppner, C. H. Smith, Dan Mazur, Gil Perlow, Ralph Havens, Jack Clark, Bob Burnight, Bob Bourdeau, Art Ruhlig, Carl Seddon, Joseph Siry, and Thor Bergstralh. (Please forgive my inability to list everyone.)

Whereas we were confident of our ability to build instruments and transmit data by radio, one question worried us—where would we get the rockets? At this point we learned about the captured German V-2's the Army would assemble and launch in New Mexico and our problem was solved, temporarily at least. The

[1]See E. H. Krause, this issue, p. 46.

*Co-authors were M. L. Kuder and E. N. Pettit.

Army invited Government laboratories and universities to instrument the V-2's for high-altitude scientific investigations. But Krause decided we should develop a rocket of our own and he assigned me to devote myself to it. The Viking rocket that resulted was my conception and was not based on the V-2 as some have claimed, but it was a new design incorporating some innovations, such as the gimballed motor.

The first Viking was launched on May 3, 1949, at White Sands Missile Range in New Mexico; it ascended 51 miles. The fourth Viking was launched from the deck of the USS NORTON SOUND (see Figure 1), a Navy seaplane tender outfitted as a guided-missile test ship. The launching took place at the intersection of the geomagnetic and geographic equators in the Pacific Ocean near Christmas Island (see illustration). It was the first fully successful Viking flight, reaching an altitude of 105 miles. The payload was a cosmic-ray telescope, consisting of alternate layers of geiger counters and lead blocks, built by Perlow, C. A. Schroeder, and C. Y. Johnson.

In all, 12 Vikings were launched; seven reached altitudes over 100 miles, the highest over 150. We measured temperature, pressure, and winds in the upper atmosphere; electron density in the ionosphere; and we recorded ultraviolet spectra of the sun and took high-altitude pictures of the earth.

About 1954 we began to think of other uses for the Viking, for example, as a test vehicle for guided missiles. We generated designs called M-10 and M-15 Vikings, so called because they attained reentry speeds

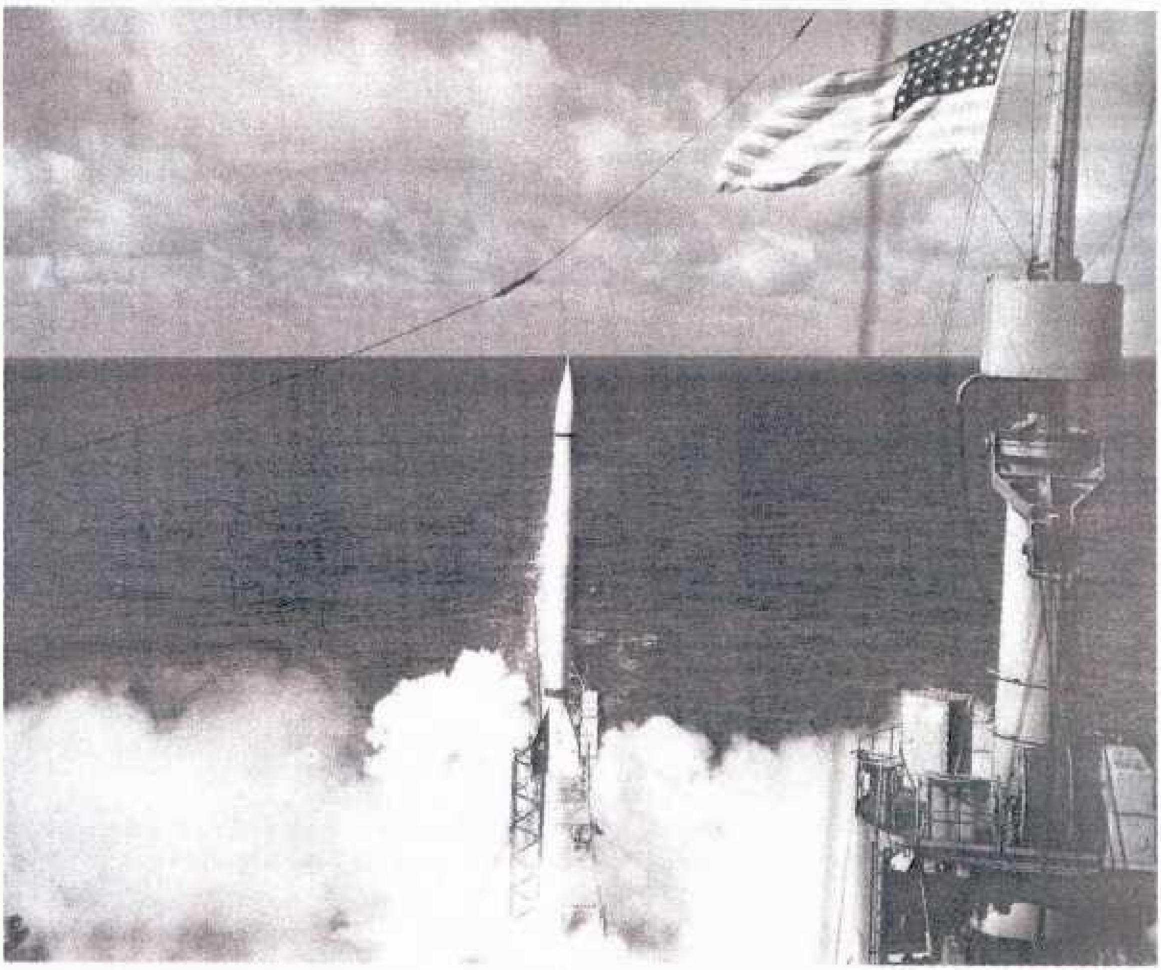


Figure 1 – Viking 4 aboard the USS NORTON SOUND

of Mach Number 10 and 15. We designed a radio-guidance system based on phase comparison. Out of these evolved the concepts for Vanguard.

In 1954 I had assembled a proposal for the American Rocket Society entitled "On the Utility of an Artificial Unmanned Earth Satellite." Homer Newell had contributed a chapter on "Physics of the Earth's Upper Atmosphere."

When, in the spring of 1955, interest was rising in the use of a satellite as part of the International Geophysical Year (IGY), NRL was in a good position to make a proposal for sponsorship to the Department of Defense. The Vanguard launch vehicle was based on the M-10 and M-15 Vikings, a radio-tracking system called Minitrack evolved from our work on radio phase-comparison techniques and our depth of experience in upper-air research verified our ability to instrument a satellite. Such a "Scientific Satellite" proposal was prepared by NRL in April 1955. In succeeding months the concept, later named Vanguard, was presented to the Stewart Committee,* which recommended to DoD that it support the NRL-Vanguard proposal as a United States contribution to the IGY.

Dr. John Hagen was named Director of the Vanguard Project and, as Technical Director, I was responsible to him for the design and production of the Launch vehicle. The Vanguard vehicle has been the subject of much controversy. It was a novel design and was built on a crash schedule. Hence, it is not surprising that the first full-scale attempt to launch ended in failure. Rather, it is remarkable that Vanguard succeeded on its third launch attempt on March 17, 1958, and eventually placed into orbit three spacecraft, all of which are still revolving around the earth. Vanguard was the prototype of a three-stage satellite launcher of which the present-day version is called Delta. Actually, the second and third stages of Delta are Vanguard designs. Delta has had a remarkable record—85 successes out of 91 attempts—and has placed into orbit the majority of NASA's communication, meteorological, and scientific satellites.

*DoD Advisory group on IGY with H. J. Stewart of JPL as Chairman, responsible for selecting the most suitable of satellite plans.

Undirected Research

H. Friedman

Dr. Herbert Friedman came to NRL after obtaining his Ph.D. in physics from Johns Hopkins University in 1940 and is presently Superintendent of the Space Science Division and Chief Scientist of the E. O. Hulburt Center for Space Research. Dr. Friedman is an internationally recognized researcher in the fields of x-ray spectroscopy, electron diffraction and microscopy, upper atmosphere research, nucleonics, astrophysics, and radio astrostronomy and has served on numerous governmental and scientific committees, including the President's Scientific Advisory Committee. He is a member of the National Academy of Sciences.

In these days of directed research and emphasis on instant relevance, it is interesting to reminisce about World War II years at NRL and the pattern of random walk from research to application that was then so common. The total professional staff was considerably smaller than that of today; we communicated constantly, freely, and with almost no security inhibitions, across divisional boundaries. The "need-to-know" barrier had not yet become ingrained.

I entered the Metallurgy Division in 1940, fresh from a Ph.D. in solid state physics, and with the thought of pursuing fundamental studies of ferromagnetism. Instead, I discovered a world of problems that had never penetrated my academic consciousness. My predecessors in metallurgy included Robert Mehl and Charles Barrett, who had left a legacy of x-ray diffraction research techniques and equipment to Herman Kaiser, with whom I set about designing a variety of applications to nondestructive testing, stress analysis, studies of high-temperature phase transformations, *etc*. Nondestructive testing was typically a primitive and routine operation; for example, gamma-ray radiography of valve castings. The need for electronic diagnostics was obvious, and I was led into research on all forms of radiation sensors from diamond crystals to scintillators to gas discharge devices. These studies were pursued as basic research but with a constant eye to invention and application.

A gratifying opportunity to apply such techniques to an urgent military problem came immediately with our entry into the War. B-17 bombers were grounded in Africa for lack of quartz crystal oscillator plates to fit their communication sets. It seems that the Japanese had stripped South America of its best-quality quartz just prior to Pearl Harbor and had left us with only poorly faced material which could not be oriented for cutting by optical means. Because the Laboratory, through the work of Elias Klein in the Sound Division and Leo Dawson in Optics, had become a center of expertise on piezo-electric crystals and their applications, it was only natural that the problem should be brought to our attention. Within a couple of days I was able to assemble a proportional counter x-ray diffractometer suitable for orienting unfaced crystals so that they could be cut to the various prescriptions for oscillator plates. The method went immediately into industrial use with commercial x-ray units, but there were no suppliers for proportional counters. An essential feature of the counter tubes was a very thin reentrant glass bubble window to transmit soft x-rays. These windows were a special skill of Leland Clark, our master glassblower. We were obliged to set up manufacturing to supply hundreds of these tubes to industry on a rush basis. (Each batch included a few spares to satisfy the irresistible urge of the inspectors to poke a finger at the delicate glass window.) I believe this x-ray orientation device may have been the first introduction of radiation-counter techniques into an industrial-type production and control process. A citation for this invention made the generous estimate that it saved 50-million man-hours in the war production effort.

In the process of further developing instrumentation connected with the production of crystal oscillators, interesting properties of detectors and x-ray systems were recognized which stimulated new research and invention. The counters were ultraviolet sensitive as well as x-ray sensitive, which offered the possibility of developing solar blind and narrow band ultraviolet detectors of very high sensitivity; the quartz orientation spectrometer suggested obvious extensions to diffractometry in its broadest aspects. About that time I was adopted by Dr. Hulburt, who established the Electron Optics Branch. To justify this new organization, Mrs. Mae Pope, the Chief Clerk of the Laboratory, wrote a one-paragraph job description to the effect that I would devote my efforts to developing applications of electrons to naval problems. In those days Mrs. Pope could handle 90 percent of the administrative operations of the Laboratory. Then, and for many years subsequently, I had no need to work up detailed budgets; money seemed to appear whenever required in response to any reasonable request to follow up a promising new avenue of research.

Contact with Dr. Hulburt opened up an awareness of natural optical phenomena. We talked of ozone and the blue color of the sky, of the exosphere and particles in satellite orbits, of camouflage and haze and visibility, and solar control of the ionosphere. We produced an ultraviolet backscatter haze meter employing a solar blind photon counter. An ultraviolet smoke detection scatterometer and a flame detector for use on aircraft and in hangars and ships' holds were also demonstrated. Invisible ultraviolet corona around high-voltage equipment and corona around missiles launched from aircraft were shown to be readily detectable. My chemist friends, particularly Peter King, became intrigued with the possibilities of ultraviolet detection of chemical warfare agents like mustard and phosgene and industrial toxic contaminants such as mercury and tetraethyl lead. All of these were found to be detectable in extremely low concentrations with simple, nondispersive ultraviolet absorption systems.

Peter King and Alan Alexander were also attracted to the use of the x-ray diffractometer in studies of pigments. Soon I was involved in problems of bottom paints to prevent barnacle growth. The diffractometer showed very simply that the major difference in effectiveness of paints then in use depended on whether they contained cuprous or cupric oxides. From the diffractometer to an x-ray fluorescence elemental analyzer was a short step, and our metallurgists were enthralled with the new capability for rapid analysis of alloys which ordinarily required hours of wet chemistry. These early diffractometer and fluorescence analyzers have since grown to enormous sophistication and are basic to any modern analytical laboratory. At the time of their development, however, most physicists were still using slow thyratron counting circuits. Claud Cleeton and his electronics group at NRL had developed hard vacuum tube circuits for fast counting, which were immediately adapted to our x-ray systems. Here again, the first visibility of such hard vacuum tube fast counting instruments in industrial applications came from these NRL inventions.

We never lost touch with a host of nondestructive testing applications of x rays and gamma rays coupled with specially devised detectors. The requirements of uniformity in production of detectors, of long life and ruggedness, were solved with the knowledge gained from fundamental studies of electronegative quenching gases in electrical discharges. First, halogenated hydrocarbons and then halogens themselves came to be used as quenching agents in relatively stable and permanent gas counters. These were applied to the recovery of radium-tagged test torpedoes at Piney Point; to a backscatter gamma-ray device for checking the corrosion of tanker bulkheads at the Norfolk Navy Yard; to measuring the lead coating thickness on condenser tubing aboard the U.S.S. NEW JERSEY in the Boston Navy Yard. A publication describing many such potential uses came to the attention of Commander Lester Wolfe at the Navy Bureau of Aeronautics and brought him quickly to talk with us about applications to aircraft instruments. Fuel quantity gauges on aircraft were a particularly sore problem. On the SBD dive bomber, the tendency was for the float gauges in the wing tanks to bounce during takeoff against the tacky self-sealing liner and then stick to that surface for the rest of the flight, leaving the pilot to guess about his fuel reserves. Lester Wolfe wanted a radiation gauge. We placed radium luminous buttons on four corners to the SBD wing tank and a Geiger counter on the underside. The system worked well and was contracted to Bendix for further development.

With the end of the war in Japan, the Navy Medical Research Center was assigned the responsibility for assessing radiation damage in Hiroshima and Nagasaki. Shields Warren, then a Navy Captain, came to NRL to discuss the problem of measuring residual radioactivity. No simple portable radiac devices were available. The Manhattan Project had done a magnificent job in building the bomb, but had not developed any of the now common types of portable radiation monitoring devices. Within about a week we prepared a set of portable radiacs for the Navy team, which permitted them to contour map the entire radiation patterns.

This contact with medical people led to many new ideas for research and instrumentation. Captain R. H. Draeger, then Executive Officer at the Naval Medical Research Institute, became a regular visitor to NRL. We worked on evaluation of anoxia by flicker fusion tests and developed an instrument intended for use aboard military aircraft. Studies of blood hemoglobin were accomplished by x-ray fluorescence of the iron constituent. Detectors were developed for use with radioactive iodine tracers in studies of thyroid malignancy and other such biomedical applications. The possibilities for physical instrumentation in medicine seemed endless.

In the immediate aftermath of the war we became concerned with atomic weapons effects. Ed Hulburt, John Sanderson, and Ernie Krause organized a program of tests for Bikini. Simple rugged high-dose dosimeters were required. From experience in the frequency adjustment of quartz crystal oscillator plates by x-ray irradiation we had become aware of radiation-induced color centers in crystals and glasses. Silica glass dosimeters were placed near bomb zero and gave direct measures of dose and spectrum. Next came the challenge of how to guarantee detection of a Soviet bomb test if and when they succeeded in producing a nuclear device. The Laboratory's expertise included filter technology for chemical warfare under Gene Ramskill in Chemistry. With his help Irving Blifford and Joe Nemecek disigned a variety of collectors ranging from small portable units to huge, thousands of cubic feet per minute, installations. As we began to observe the collection of atmospheric radon decay products with such equipment, we became aware of the scrubbing action of rainfall. Back to Dr. King we went with the question of how could we concentrate particulate radioactivity from rainfall. He proposed the standard flocculation treatment of our municipal reservoirs. From Dalecarlia reservoir we scooped some flocced sludge and at NRL Sverre Gulbrandsen and Luther Lockhart teamed to extract cerium and yttrium. Our beta-ray counter went wild when their concentrate was placed under it. We were detecting fission products from Bikini. Lockhart then made similar analyses of rainwater from catchments in the Virgin Islands and Kodiak with positive results. We then equipped Navy air weather centrals with catchments made of Sears, Roebuck and Co. plastic roofing and arranged to floc the collected rainwater routinely and ship the residues back to NRL. At each site we placed large banks of Geiger counters surrounded by water trays to indicate directly and immediately any large precipitation of radioactivity. The first Soviet test triggered the Rain Barrel system with a bang. Lockhart analyzed the floc from Kodiak for plutonium, and he separated it successfully without access to the classified plutonium procedures developed in the Manhattan Project. Maurice Shapiro in the Nucleonics Division confirmed the analysis by measurements of the alpha tracks.

In 1949 a huge solar flare erupted. Our detector banks at the Air Weather Stations recorded a large ground-level signal along a wide range of latitudes from Arctic to Antarctic. It was the clearest definition of such an event obtained to that date; by then everything related to Rain Barrel was classified, however, and we had to keep this spectacular secret to ourselves. But it opened our eyes to a new aspect of solar-terrestrial relationships that was perfectly matched to the opportunities of space research then being exploited with captured V-2 rockets at White Sands. The stage was set for a quarter century of commitment to a new frontier of science.

I was recently asked by a management analyst whether my Division communicated with other divisions of the Laboratory. The questioner implied that he did not expect a very positive response. I explained that the greatest attraction of NRL over my 33 years of experience had been its multi-disciplinary character and the diversity of expertise that could be consulted on almost any conceivable research problem. Communication across all divisions of the Laboratory is an invaluable asset. Inhibitions created by security and the "need-to-know" philosophy can throw a large monkey wrench into the combined creative capability of the Laboratory.

I often long for the good old days—administration with a good measure of benign neglect, minimal security barriers, and the "can do" response we could always expect for a good proposal.

NRL's Contribution to Countermeasures

H. O. Lorenzen

Mr. Howard O. Lorenzen joined NRL in 1940, as an engineer in the Radio Division, where he pioneered in the development of ultra-high frequency receivers for the Navy. He was promoted to Head, Countermeasures Branch in 1950, and to Superintendent of the newly formed Electronic Warfare Division in 1967. In 1971 he was made Superintendent of the Space Systems Division, from which position he retired in 1973. He has been widely honored for his contributions to electronic warfare, intelligence, and space technology.

ANTISUBMARINE DIRECTION FINDING

In successful submarine operations the submariner needs to tell his operational commander where he is located and receive the benefit of any changes in orders or guidance or intelligence on the enemy's disposition. The conventional submarine's range is limited by the amount of fuel it can carry, and periodically it must be refueled and replenished. To effect a rendezvous with a tanker or support ship he must be able to tell them where and when to meet him. Recognizing the dependence of the submarine on communications, the Naval Research Laboratory from its inception concentrated on utilizing and improving radio direction finding (DF) as a tool for antisubmarine warfare. From its beginning the DF research assumed an important role in the tasks of the Laboratory. In World War I shipboard and shore stations detected and located the submarines operating in the Atlantic.

Soon after the establishment of NRL, Warren Burgess with Harris F. Hastings set out to modernize the Navy's shipboard, airborne, and shore DF equipment. Advances in the accuracy of high-frequency (HF) shore DF stations were achieved by the use of rotating H Adcock antennas and spaced-loop systems. On shipboard, the Burgess correcting loop made low- and medium-frequency DF possible. When WW II began the improved technology was ready, but the Navy had to establish hurriedly its shore station network throughout the Atlantic Ocean areas. Rupert Haskins of NRL did yeoman service in supervising the site selection based on careful measurement of soil conductivity for the new DAJ*-equipped stations. After the stations were built he checked out and calibrated these same stations. Dr. Maxwell Goldstein led the Laboratory effort to adapt the first crossed loop DF's to Coast Guard and Navy ships. Sterling Thrift was responsible for a massive effort to transfer Navy DF knowledge to industry and a ship fitting effort to place the new DAR* equipments in service.

Once completed, the new stations along the coast line on both sides of the Atlantic and on the mid-ocean islands began continuously searching the air waves for enemy submarine broadcasts, noted their bearings, and fixed their position by plotting their location from several widely-spaced shore stations. These

*Equipment model designators.

fixes were correlated and radioed daily to the ships in the Atlantic where the information was supplemented by additional bearings taken by Navy ships in the immediate vicinity of the broadcasting submarines. These fixes allowed the Navy to reroute convoys to avoid the individual submarines or the "Wolf Packs" lying in wait for them across the shipping lanes. More important, it enabled the carriers and destroyers in the anti-submarine fleets of the United States, Britain, and Canada to locate and sink submarines.

In the Pacific the Japanese were using airborne UHF and VHF radars to search for our submarines operating against them. Again NRL was called in to fill the breach. A team under Frederick Harris with Mack Sheets and Godfrey Loveberg developed and installed intercept and DF equipments on our submarines to alert them to the Japanese airborne antisubmarine patrols.

Recognizing the limitations of the shore station intercept and DF equipment, after WW II, under Howard Lorenzen's leadership, NRL embarked on a research program to improve the U.S. Navy's capability in this area. James Trexler, Mack Sheets, Raymond Gleason, Robert Green, and Jack Hortman worked to develop a new system based on a circularly disposed antenna array (CDAA). This new antenna system allowed all the functions of intercept and direction finding to be done by the same basic antenna system. By this new concept of a multielement array, beams could be formed for any number of bearings simultaneously, while utilizing the same array for direction finding. This technique soon showed marked improvement in the number and accuracy of the bearings it could take, thus greatly improving the Navy's DF capability. In 1955, in nearby Virginia, an experimental equipment was constructed by NRL on the U.S. Coast Guard Washington Radio Station.

To complement the improvements resulting from adoption of the CDAA's, the basic concepts and hardware for the automation of the naval HF-DF net operations were developed under the direction of Robert Misner. One major component of the system was a computer, designed by Bruce Wald, for combining bearings from each DF station to derive a fix (emitter location information). A completely new concept of high-speed operation was evolved for the operating Navy based on early Naval Research Laboratory research investigations and systems plans. Today this system is one of the mainstays in meeting one of the Navy's important operational requirements.

EARLY ELECTRONIC COUNTERMEASURES (ECM)

Electronic countermeasures had its major capabilities recognized in World War II when the British "bent the beams" of the German Radio Transmitters which guided the Luftwaffe to the strategic targets in England, so that their bombs dropped on the unoccupied countryside. Later noise jamming by the British denied the German radar operators information on Allied aircraft raids. When the United States entered the War major emphasis was on weapons of attack, and not until the Germans unleased their radio-controlled glide bombs at our Fleet in 1943 did countermeasures become a matter of major importance to the Navy and NRL. Only then were the Destroyer Escorts DAVIS and JONES fitted with NRL-developed intercept receivers, signal analysis equipment, and recorders to try to determine the radio frequencies used and the types of guidance modulation employed by the enemy.

This effort was successful, and during the Allied invasion NRL-constructed equipment successfully protected the ships by giving repeated false commands to the bombs from the deception transmitters. Also NRL designed towed-radar decoys, consisting of large chicken wire reflectors, which were used to lead the Germans to believe the landing was directed at Pas de Calais instead of Normandy.

After the War a small group of engineers in Mr. Louis Gebhard's Radio Division were convinced that, as the navies of the world increased their use of electronics in radar, communication, guided missiles, *etc.*, there would be an ever-increasing demand for better and better electronic countermeasures to defeat these weapons. Early in this effort a group under Mr. Howard Lorenzen began research programs and to update the countermeasures technology. One early result was a series of intercept equipments that covered the entire frequency spectrum in use at that time. Finished designs were taken by the Navy, put into production and installed in ships as well as aircraft, becoming the Navy's shipboard and airborne intercept systems.

Later, under Henry Weidemann, these equipments were converted so they scanned the various frequency bands in seconds, and a wide-band panoramic display of the spectrum activity was presented to the operator. Again the improved designs were rushed into production for installation in ships and aircraft, providing the intercept systems with a display capability. About this same time James Gall developed a cathode ray tube with a number of separate gun structures in the same glass envelope. He developed this basic device into a multi-gun cathode ray tube signal analyzer; thus permitting complete analysis of all the signal parameters derived from a single RF illumination of the intercept receiver. This same basic technology is still the backbone of the instantaneous signal analyzers used today.

Under Don Lowe, Robert Misner, and Don Christman extensive research was done on the then budding art of signal recording. From the early wire recordings they progressed to the paper-backed magnetic tape and later to tape with polyester backing. This group continued to push the recording bandwidths from a few kilohertz to the bandwidths of hundreds of kilohertz and later to a range of a few megahertz. No longer is it necessary to have the recording device limit the bandwidth of the video signal being recorded, any practical bandwidth required can be provided. The early problems of magnetic voids in the recording media and inadequate coating of such materials were licked through cooperative efforts of this group and chemical scientists at NRL.

Eventually all the technology was available to fit the first airborne and shipborne reconnaissance platforms. The early PB-4Y2 aircraft were hand-fitted by the Lorenzen group at NRL to reconnoiter the electronic emissions of the various foreign navies around the world to make the first assessments of electronic progress of the various nations of the world. New long-range aircraft types called for the latest fittings from NRL, and as time went on the PB-4M's, P-2V's, *etc.*, each required NRL's latest and best equipment. U.S. knowledge of the electronic fittings and technology of the world progressed measurably.

On the offensive side of the countermeasures activity, John Miles headed an NRL group that included Frank Hollweck, John Williams, and George Page; they developed high-power tunable magnetrons and transmitters to cover the entire frequency spectrum matching the intercept receiver ranges. These components were periodically tested in a contest involving the best fire control radar and missile guidance systems. The results were always the same: electronic countermeasures quickly found and identified the Achilles Heel of these new offensive radar systems.

Eventually these radar systems began to employ the same principles of broad-banding used in the passive countermeasures systems. Frequency hopping and rapid frequency sweeping triggered the recognition of a new era of countermeasures. Lynwood Cosby and a small group of engineers had anticipated this requirement and had been working on broad-banding traveling-wave tubes which could be employed in cascade. Thus the signal could be detected and amplified in a chain of traveling-wave tubes and retransmitted back at the radar with appropriate deceptive modulations added; in this way the radar always provided the carrier for the jammer, and frequency hopping or sweeping could be countered easily in the jamming system. Cosby's developments included higher and higher power traveling-wave tubes covering greater and greater portions of the frequency spectrum. This was the Genesis of the repeater-jammer system. To cope with the wide bandwidths and rapid rise times required he developed, in conjunction with S. T. Smith and George Graham, an electron multiplier secondary emission tube to operate as the high-power modulator for the system. Today this is the backbone of the most sophisticated jamming-deception systems for shipboard and airborne service.

John Link and Victor Kutsch pioneered in the improvements in the deceptive chaff decoys. Utilizing explosive charges to propel and disperse the chaff packages, they could provide effective deceptive decoys for aircraft and ships and could put them at an effective placement relative to the craft under attack.

Throughout the Korean War and the war in Viet Nam NRL has been called upon continually to develop electronic countermeasures to neutralize each new weapon introduced by the opposition. The basic new technology developed by NRL has kept the Navy's electronic warfare suites updated and ready to deal with each new threat as it materialized.

Sound and the Sea
A Short History of the Sound Division

H. L. Saxton

Dr. Harold L. Saxton received his Ph.D. in 1934 from Pennsylvania State University, where he remained as an instructor and later became an assistant professor of physics. For more than 25 years after he came to NRL in 1940, Dr. Saxton was concerned with investigations of all phases of underwater sound and its applications to the Navy's undersea defensive and offensive weapon systems. After serving as Head of the Electronics and Propagation Branch, Dr. Saxton succeeded the late Dr. Harvey C. Hayes as Superintendent of the Sound (now Acoustics) Division. He retired from NRL in 1967.

The Sound Division came into being at the opening of NRL when Dr. Harvey Hayes moved over from the U.S. Navy Experimental Station to become its Superintendent. He soon acquired the assistance of Dr. Edward Stephenson and a little later that of Dr. Elias Klein. Prescott Arnold joined the group in the mid-30's. There were but few others aboard.

Submarine detection was the chief objective. To this end, ultrasonic frequency was chosen so that transducer dimensions could be kept within Navy restrictions. The QB transducer was developed with the use of Rochelle salt crystals for broad-based operations, then passive sonar for listening to submarine noise, then active sonar for echo ranging. These systems were to find use in World War II.

In the period prior to WW II, the singing propeller was studied and the singing quenched by sharpening the trailing edges of the propeller blades. Sound propagation losses to shoot ranges were correlated with water temperature gradients, which in turn were related to atmospheric conditions.

In 1940, the vanguard of expansion for possible war included Drs. John Ide, Raymond Steinberger, and Harold Saxton who joined Arnold as section heads; Dr. Horace Trent, Thomas Jones, George Vernon, and Melvin Wilson.

Developments during WW II included the Sector Scan Indicator for visual presentation of the target, in aspect, positioned within the sound beam; the "Gump" equipment for communication between ship and frogmen; the first homing-torpedo decoy (FXP), an operator training device for use aboard ships to insert echoes into fleet equipment simulating planned target maneuvers; two acoustic actuating devices for mines; further development of the British-devised sonar dome; and an equipment for locating exploding depth charges to facilitate avoidance maneuvers.

In mid-WW II, the Crystal Section under Paul Egli was formed as a pilot plant for ADP crystals. Their activities were greatly expanded particularly in another division to which they transferred later. The Shock and Vibration Section was formed and also transferred later to another division.

Systems development was quite extensive from 1944 to 1960. The Type A Sonar for surface ships was developed and mounted in the USS FOSS. It had three-dimensional stabilization of the trainable and tiltable transducer, as well as both horizontal and vertical SSI's, the former tied in with fire control equipment. Performance far exceeded that of fleet equipment. This system was used for research such as studying multiple acoustic paths from the target.

The first experimental helicopter dipped sonar was developed and operated successfully. Manufactured components were then integrated by the Laboratory into the first fleet helicopter sonar. Blimp sonar was also developed and used experimentally.

An advanced sonar for submarines was combined with a new torpedo data computer in the USS SEACAT. This system introduced the encapsulated transducer, trainable and tiltable, with three-dimensional stabilization. By means of two such capsules, near the ends of the submarine and linked to SSI's, an accurate means of passive ranging was achieved. This system carried out early studies of coherency of noise as a function of difference in time of arrival at two separated regions.

At the close of 1947, Dr. Hayes was succeeded by Dr. Saxton as Superintendent. In a reorganized division, Dr. Steinberger became Associate Superintendent and the Branch Heads were Prescott Arnold, Melvin Wilson, William Finney, and James Fitzgerald. Shortly thereafter a new branch under Arthur McClinton was transferred from another division. Others who succeeded to Branch Head positions were Robert Urick, Homer Baker, Chester Buchanan, Robert Mathes, Robert Faires and Sam Hanish.

After a comprehensive study on extension of maximum sonar detection range, the descent to low frequency was started with an experimental 10-kilocycle sonar mounted in the submarine USS GERAVINA. The transducer used ADP crystals in a "unit design." Power was increased tenfold over fleet searchlight sonars, and pulse length was increased up to thirtyfold. Advanced signal processors included the sequential transmission of eight pulses in frequency steps with multichannel receiver and multipen recorder, and the frequency scanner which plotted Doppler shift against range. A tenfold increase in maximum range was achieved with this system, and salient features were adopted in fleet sonar.

Prior to 1950, a range-rate indicator was developed which proved useful in attack, avoidance maneuvers, and navigation.

Further descent in frequency in experimental systems was carried out by installations in a landing craft from which a towed body was launched through a large center well. A 5-kilocycle system performed satisfactorily but served mainly during the solution of towing problems. The transition to a 1-kilocycle system was carried out as soon as possible. Multiple transducer elements were mounted in the towed body and on its tail. Beam steering was electronic, and the signal processing was by means of contiguous narrow-band mechanical filters. The first convergence zone echoes from a surface ship were obtained. Multiple paths to convergence zones were studied with the aid of a blimp-dunked receiver.

Other projects carried out in the 1950's included research on computers with their application to visual displays and perception studies, analysis of submarine noise signatures, the two-helicopter attack system, and statistical processing techniques.

In the late 1950's and early 1960's a combined attack system for ship and helicopter was developed. The helicopter was directed into the neighborhood of a target, where it dropped an echo repeating buoy for reference. The ship then gave the helicopter a vector from buoy to target.

In the 1960's, the interlaboratory project, Artemis, attacked the problem of widespread bottomed arrays with shore-based processing for echo reception. The transmitting transducer at very low frequency and very high power was lowered to a depth of 1000 ft from a ship, the MISSION CAPISTRANO. NRL engineered and supervised this component of the project. The transducer weighed about 35 tons and gave years of service.

In this period of the early 1950's, an east-coast deep-water research facility received high priority. NRL acquired a railroad barge which it had converted to a mobile research platform. This barge was anchored in Seneca Lake in water over 500 feet deep where it became the heart of the required facility, operated by NRL for several years before operations were turned over to another laboratory. This facility permits operation at low frequency and high power and has proved invaluable to Navy bureaus as well as laboratories.

A project at NRL, which made use of the Seneca Lake facility, was the development of a near-field array for obtaining far-field transducer beam patterns. In addition, a line hydrophone array with improved shading was devised as a space saving alternative. This project has application to *in situ* measurements of fleet transducers.

In the 1960's, NRL concentrated some of its continuing sound propagation research on measurement of loss into shadow zones and over the return path under various water conditions identified by bathythermograms. These data correlated well with computations.

A program in ocean ecology was undertaken together with the development of biological sensors and the study of turbidity by means of echo-ranging with light.

A broad program of ocean engineering was instigated with orientation toward inspection of the ocean floor. When the USS THRESHER was lost, the division was able to mount a search operation within 48 hours. NRL was able to locate THRESHER at its first passage over the target. Tens of thousands of photographs were taken. Thus started a detection inspection capability which has served in several emergencies.

Other projects in the 1960's were bottom-scattering studies of sound originating on a moving ship, echo formation studies in which echoes from small simply shaped objects at ultrasonic frequency were accurately delineated and explained, and ultra-precise measurements of sound speed and attenuation as functions of water temperature, pressure, and salinity.

Atmospheric Physics at NRL

J. E. Dinger

Dr. Jacob E. Dinger joined the NRL family in December 1941 as an assistant physicist in the Mechanics and Electricity Division, working on nondestructive detection of flaws and inhomogeneities in critical wartime materials. He was named Head of the Aerology Branch in 1950, with major emphasis on meteorological instrumentation, atmospheric electricity, and a wide range of atmospheric physics studies. He retired from NRL in 1971, but is serving again as a Consultant in the Atmospheric Physics Branch, Ocean Sciences Division.

The research in atmospheric science at NRL began with Dr. Ross Gunn, a scientist at NRL during the Laboratory's formative years. Dr. Gunn's absorbing interest in the physical phenomena of the atmosphere, particularly the area relating to the troposphere, and his leadership during World War II were the motivation for a major project to develop ways for reducing "precipitation static" that can "black out" radio communication with aircraft operating in certain weather conditions. Radio static results from electrical discharges from the aircraft to the atmosphere giving rise to noise signals to the aircraft radio receiver. Electrical discharging occurs under two conditions: when an aircraft is flying in precipitation, particularly in snow or graupel, wherein the frictional contact between the precipitation and the aircraft surface charges the aircraft to a very high electrical potential, and when the aircraft is operating in a strong electric field occurring in or around an electrically active cloud, particularly a cloud producing lightning.

One result of this project was the development of dischargers which permit the electrical discharge to occur in a manner that does not generate radio noise. Such dischargers are still in use today on military and commercial aircraft. Aircraft used in this project were among the first to be instrumented to make in-cloud measurements and as such were some of the first to engage in what has become known as cloud physics research. Some of the research carried out in the project sought an understanding of the physics of charge production on raindrops, snow and ice particles in a cloud, the separation of these charges leading to the strong electrical fields that produce lightning. NRL experiments were carried out which for the first time showed that air bubbles entrapped in cloud ice, such as hail, snow, or graupel, are electrically charged and upon melting of the ice the air bubbles escape, carry with them a negative charge, and leave a positive charge on the water drop. In this manner precipitation in a cloud undergoing freezing and melting can produce pockets of positive charge. It is now known that there are several mechanisms in clouds which can produce a separation of charge, but even today there is not a consensus on which ones are the most important.

During World War II the importance of weather on naval operations was brought to the fore, and the Naval Aerological Service became an important component of practically every operating unit of the Navy. During and following the War there was an effort to improve and develop new instrumentation to assist the Navy in this requirement. The then Bureau of Aeronautics was responsible for providing materiel to the Naval Aerological Service. Personnel of this Bureau sought out NRL to assist in this effort, and the Aerology Branch was established. Since that time this Bureau and its successor organizational units have continuously supported a research and development effort at NRL. The initial effort was largely directed toward new instrumentation for atmospheric measurements, particularly instruments for use on aircraft, and in this work R. E. Ruskin assumed the leadership role. One important example of his work was the development of the axial-flow vortex thermometer. This instrument makes possible the measurement of true air temperature from an aircraft. Because of the aircraft speed, an exposed temperature sensing element is heated above the air temperature as a result of the dynamic heating effect of the air motion relative to the sensor. This effect at its maximum is proportional to the square of the aircraft speed. This thermometer became an operational instrument for use on naval aircraft. Other instrumentation developed under this program included equipment for measuring humidity, raindrop sizes, and the total water content per unit volume in a cloud. W. A. Von Wald assumed leadership in developing unmanned weather stations which have the capability of measuring weather variables at a remote location and telemetering by radio link these measurements to a receiving station. Such instrumentation is adapted for use on land and buoys anchored at sea. For a number of years such stations were used by the Naval Support Group in Antarctica.

As a part of the instrument development program, H. J. Mastenbrook and A. D. Anderson proposed a new method for obtaining upper-air data over remote ocean areas. In this method a balloon is constrained to float at a predetermined altitude but is free to move with the air current; tracking it by radio techniques gives the balloon trajectory which maps out existing air mass systems and their movements. Mastenbrook and H. D. Cubbage developed the balloon system and necessary electronic equipment which made possible the implementation of what became known as the Transosonde System. The development led to an operational phase of the Transosonde, wherein balloons were launched from Japan and tracked across the Pacific. The altitude for the floating balloons was approximately 30,000 feet. With the development of commercial jet aircraft the operational phase was discontinued because of the potential collision hazard. However, the concept is being pursued by another agency where equipment and methods compatible with jet aircraft are being developed.

With the growth within the Branch of a basic research effort complementary to the instrument development, the Branch name was changed to Atmospheric Physics to reflect the more comprehensive nature of the work. One of these research projects under the direction of Mastenbrook is concerned with the measurement of water vapor in the stratosphere. The instrumentation and technique developed for this work utilize an automatic frost-point instrument carried aloft by a balloon. Upon reaching a preset altitude (approximately 100,000 feet), the balloon is programmed to descend, with the meaningful data being taken during the descent. The measurement of frost point together with temperature, pressure, and in some cases, ozone content is telemetered to a ground reciever *via* a radio link. By use of this method NRL has acquired the only body of data on stratospheric water vapor content. These data give seasonal and some latitudinal variations and thus provide information on stratospheric circulations, interchange between stratosphere and troposphere, and the only background data for determining if and when technological activity in the stratosphere alters the water content of the stratosphere.

Another fruitful area of NRL research is that dealing with atmospheric electrical phenomena including that associated with fair and disturbed weather. This work has been carried forward by J. H. Kraakevik, R. V. Anderson, W. A. Hoppel, and S. G. Gathman. Especially instrumented NRL aircraft have made possible studies of the potential of the ionosphere, and of electrical effects associated with snow producing clouds, measurements of the variation of electrical conductivity and electrical potential with altitude, and diurnal variation of electrical field well above the exchange layer. Theoretical studies and field experiments have been carried out to measure the electric field profile near the surface (the electrode effect) of both land and water. The measurements taken over water are among the first such experiments.

Cloud physics studies have made use of the NRL aircraft to measure the time and space variables in and around cumulus clouds. These variables include total water content, cloud droplet and precipitation drop size and concentration, interdroplet humidity, temperature, and vertical air velocities. All such studies were directed toward an understanding of the dynamics of convective clouds. Joint field experiments have been performed with the National Oceanographic and Atmospheric Administration on a number of occasions to further the knowledge of cumulus dynamics. R. E. Ruskin and R. M. Schecter were in charge of the NRL participation.

Under the initial direction of Dr. S. Twomey an extensive effort was undertaken to study the nature and source of nuclei for cloud and fog droplets. Each cloud and fog droplet requires a particle, referred to as a cloud nucleus, upon which condensation can be initiated at a very low supersaturation (from slightly above 0% to 3%). With the use of an NRL aircraft the first survey of the number concentration of cloud nuclei was made on a global basis. This survey confirmed that over continental areas there is a greater abundance of cloud nuclei, thus accounting for the continental clouds, in general, being considerably more stable (having more and smaller droplets) than maritime clouds, the latter being more likely to produce precipitation. Such measurements have shown that the ocean is not a principal source of cloud nuclei. Further experiments showed that the atmosphere itself provides the principal medium within which the nuclei are formed, and this suggests that these cloud nuclei are composed of sulphates or sulfuric acid formed by chemical or photochemical processes involving gaseous SO_2 or H_2S.

Good Fortune and Research

R. Tousey

Dr. Richard Tousey came to NRL in 1941 following his Ph.D. from Harvard and a teaching tour at Tufts. He has been Head of the Rocket Spectroscopy Branch of the Space Science Division since 1958 and is currently principal investigator for four Skylab experiments. He is a member of the National Academy of Sciences and is the recipient of many major scientific honors and awards.

Upon asking myself the question, what brought me to NRL 32 years ago, and what forces led me into projects that turned out to be of supreme interest, I am able to find no single simple explanation except good fortune. The story is not short.

Before the century turned, my paternal grandfather, Professor of Logic and Ethics at Tufts, sailed east without charts, to purchase land on the coast of Maine, where as a boy I came to love the sea, the forest, and the sky. Often we made summer quarters on board our old Lawley yawl, at moorings just off our shore in Buck's Harbor. One evening, in 1930 as I recall, a small sailing craft dropped anchor alongside. A Dr. and Mrs. Hulburt and two small children, soon hailed us. Then a graduate student in Physics at Harvard, I found that Dr. Hulburt was a physicist at NRL, a place I had in imagination idolized when, as a 13-year old possessor of an amateur radio license, I listened in vain for NKF, hoping I could manage to work it. Asked what he did, Dr. Hulburt replied, "Infrared," and irreversibly changed the subject! Naively, I was puzzled; in research I had not yet come to recognize the existence of the word, secret.

The next year, and others to follow, the Hulburts returned to Buck's Harbor, but in the Vega, their Casey ketch, which is still live and healthy along with themselves on the Tred Avon River.

The second thread I wish to follow leads from Tufts, where following family tradition I received my AB degree, to Harvard. After I had spent a year in graduate school, Professor John C. Slater said to me, "Would you not like to become a Theoretiker?" And Professor Theodore Lyman said to me, "Would you not like to become an Experimentiker?" Tossing a mental coin, I chose "TL." Not long after, he assigned me to one of his vacuum spectrographs, and I spent the first month chasing leaks. After a couple of weeks of this, Lyman himself came in, put on a laboratory coat and offered to help. Soon came his advice; "Well, Tousey, the longest way 'round is the shortest way home. Take it all apart and start from the beginning!"

My Thesis was, "An apparatus for the measurement of reflecting power in the extreme ultraviolet with application to the optical constants of fluorite." Certainly it was not relevant to anything at all, so it then seemed, except that Ph.D.

Following the depression, jobs were scarce. But Tufts came to the rescue, with a position embellished as "Research" Instructor in Physics. This really meant three bosses; research, teaching, and students, a frustrating impossibility to separate in priority, and to serve each as it deserved.

And so in 1941 the stage was set. When World War II research came to the fore, I looked for a spot in which to fit. My first try was a well-known project near to home, perhaps best unmentioned by name. I was turned down flat! The second was halfway to Washington, a fire-control project. Thank goodness an offer never came. My third attempt met with good fortune, and Dr. Hulburt invited me to come to NRL. Thus began the most rewarding and evermore interesting era of my life.

In 1941, NRL was small, hot, and backwoods by standards of today. Believe it or not, everyone left exactly at 4:30 p.m. at the sound of the siren; even to enter after-hours took the personal approval of the Director. Long-distance telephone calls and travel by commercial air were unknown, at least to me. But on the other hand, we were exceptionally free to follow our own inclinations in research; almost no time was lost in searching for support, or justifying one's work. Proposal writing was an unknown thing; our advice was constantly sought. The administrative staff acted quickly and efficiently, seemingly unhampered by regulations.

But above all else, the pleasure of working with Dr. Hulburt, the utter absence of feuds and jealousies, and the realization that I was part of the defense effort combined to form the greatest positive discontinuity of my life.

The 5 years of World War II research were of extreme stimulation. My first project came as a request from the Bureau of Aeronautics to study the visibility of stars in the daytime sky in order to develop an instrument for celestial avigation at any time of day or night. Even then the sky above Washington was too murky. So with Dr. Hulburt I flew to 10,000 feet in a JRF, to look at the sky and to try to see stars. Most vivid is my recollection of Dr. Hulburt holding a Speed Graphic camera we had on board, fixed up as a crude coronagraph to photograph the sky around the sun. Suddenly smoke started to issue forth from all the cracks and joints. We novice photographers realized not that the sun, at that altitude very bright, if allowed to come to focus for a monent on the focal plane shutter, would set the cloth ablaze.

Our final attempt was to try at the highest altitude possible, flying in a PBY together with our colleague-competitors from the Smithsonian Astrophysical Observatory. After some hours we reached 21,000 feet, breathing oxygen through a sort of mask contraption. We could see the earth below, clear as if it were a map, for even then the atmosphere was not as polluted as it is today. We opened the "Mae Wests" and looked through our instruments. It was very cold. I saw nothing, but my Smithsonian colleague was seeing star after star. Unable to understand this, he generously suggested swapping positions on the chance that air turbulence was destroying my seeing. Strapping on his frosty mask, at once I found his oxygen line frozen solid, decided that I would soon perish, passed the word forward to the Commander, and down we went in a fraction of the time it took to ascend. Why did he see stars? Of course nothing was proved, but anoxia was my explanation.

The effect on me of this first project was to make me aware of the great beauty of an unpolluted, dark blue, "Rayleigh" λ^{-4} sky, that we often saw from the JRF at 10,000 feet, but which was rarely encountered from the ground, even then. On travel for NRL I have seen a truly Rayleigh sky several times when high in the Alps, once in Greenland, and on a few memorable occasions in the Rockies. But year by year this becomes more rare. Were I not not tied to space projects, carried out where the sky is black, I would find myself hard put to choose a field, and might very well select research on air pollution.

For the rest of the War there followed research project after project related to night vision; they were lots of fun, because discoveries were easy, though rarely did they turn out to be new. One project asked, what could a hostile watcher see of the infrared sources, then being developed for night vision use. Another concerned dark adaptation, in the study of which Dr. Hulburt was a pioneer. Well do I remember his daring, when investigating the effect of flash blindness, in igniting directly before his eyes the largest flash lamp he could find, after spending an hour in complete darkness.

My favorite vision project was "night myopia." With Martin Koomen and others we showed that many people could indeed see much better at night when wearing spectacles with concave lenses of about 1-meter focal length. Try it yourself if you have doubts. You may be one of many who happen to have eyes that are not well-corrected for spherical aberration. I used to try out my night-vision spectacles on astronomers, and always found a goodly number who were astonished at the new myriads of stars they were then able to see by unaided eye. But the Navy was not to be convinced; binoculars, yes—but spectacles, never! About that time, however, they came to rely more on the radar screen than on the lookout on the bridge.

I am afraid I have never been one to make inventions, but in those early days, I did manage to get one patent. Everyone should go through this experience once. It was on a gunsight, invented at about the same time by Eastman Kodak, and dubbed by them the "Fly's Eye Gunsight." Aided by NRL's patent officer, I somehow established prior claim, and also succeeded in convincing the patent office that all those other patents with which they bombarded me were not relevant.

When the War ended in 1945, we were engaged in finishing projects and cleaning up loose ends. At NRL's request I went recruiting to the laboratory which had refused me a job 4 years before. This time I had the satisfaction of interviewing them, and I was reminded of the good fortune that had led me to NRL.

One afternoon early in 1946, Dr. Hulburt strolled into my office to tell me that NRL had secured space in captured V-2 rockets with which we might conduct research from altitudes above the sky. How about flying that little old Hilger quartz spectrograph that had gone to the Arctic during the Second Polar Year, 1932-33, with Dr. Maris? We could have a try at recording the then unknown ultraviolet spectrum of the sun.

Good fortune once again! Taking up the challenge, it soon became apparent that a special instrument could be designed that indeed might record the Lyman-alpha line of hydrogen, way down in the extreme ultraviolet, and discovered some 40 years before by dear old Professor Lyman. Thus, all of a sudden, did I return to my original field that in the early 1930's seemed of interest only to get that Ph.D., but from 1946 on was to become a field of ever-increasing activity with expenditures of many millions, now culminating in Skylab. In retrospect it seems strange indeed, but somehow we then believed that never again would there be such a chance to discover the sun's hidden ultraviolet, once the V-2's were gone.

Our crash program's first attempt ended in a real crash, for the V-2 disappeared far underneath its great impact crater. Despite all attempts at excavation it was never found. But on October 10, 1946, well under a year after Dr. Hulburt's suggestion, we were successful, winning out over several eminent competitors, and secured the first solar spectrum in the far-ultraviolet.

It is now 27 years later. Many of our colleagues left NRL in 1958 to form the nucleus of The National Aeronautics and Space Administration (NASA). Our cherished alumni form an impressive group, including Homer Newell, Associate Director of NASA, and many other leaders. Thanks to confidence in NRL, year by year we have received inestimable support. And we in turn have worked hard indeed to justify it all, producing many first discoveries and assisting the space program in every way we could.

The climax has come in Skylab, of origin dating back nearly a decade. Starting as NASA's Apollo Applications Program it offered the possibility of making observations of the sun for prolonged periods with an astronaut actually operating the instruments, and because of recovery, photographic film could be used for recording—offering the equivalent in observing time of thousands of rockets. Proposals were accepted from Dewitt Purcell and me for several solar instruments. Little did we or NASA know what lay ahead. The project burgeoned into Skylab, its orbiting workshop, and to us most important of all, the Apollo Telescope Mount (ATM) which had provided the start. We became involved in high finance, science politics, great responsibility, a tripled staff, and often felt entangled by the machinations of a great agency. We found it close to impossible to publish the papers that are expected from scientists, we covered millions of miles in travel, and many times we came close to complete frustration. All this for solar research in the once-obscure extreme ultraviolet.

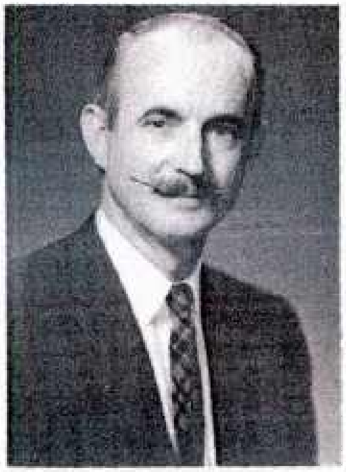
But on May 14, 1973, Skylab went into orbit, and many of us now find ourselves living in Houston. To oversee operation of our experiments requires a continuous night and day watch by NRL scientists and engineers, no doubt a routine duty in the life of naval personnel on shipboard, but not so for scientists.

Now, I can say that ATM was worth all this effort. It is a pleasure to report that we have obtained really excellent results from the first visitation to Skylab, which splashed down on June 22, 1973. When first taken from the darkroom into light, the XUV solar spectroheliograms were breathtaking. The second visitation is now in progress—successful too, we believe.

To be associated with Skylab is the pinnacle of my career and is the result of T. Lyman, E. O. Hulburt, and not the least, good fortune.

Submarine Atmospheric Habitability

H. W. Carhart



Dr. Homer W. Carhart accepted a position at NRL as a Research Chemist in 1942, following a teaching position in chemistry with Gallaudet College. His early work related to chemical warfare agents and later expanded into a team effort in the preparation and properties of the hydrides of the lighter elements, chlorocarbons, and hydrocarbons. The success of this work led to the establishment of the Fuels Branch (now the Chemical Dynamics Branch) in 1942 with Dr. Carhart as its Head. He is a recognized authority on submarine habitability as well as fuels research and serves the Navy, other government agencies, and industry as a consultant on problems concerned with the use, handling, and control of liquid fuels.

One way of building a submarine is to start with about a 100,000 cubic-foot bubble of air, wrap it up with a steel hull, fill it with all sorts of gear, launch it, put in about 100 men, and proceed to submerge. But trouble begins as soon as the hatch is closed, because the bubble of air becomes increasingly vitiated or contaminated. Work on this atmosphere problem started at NRL in 1929 following two submarine disasters, and a special Submarine Board appointed by Secretary of the Navy recommended that NRL study hydrogen detection and removal, elimination of carbon dioxide, ventilation of batteries, and development of fire extinguishers for submarines. From this early work stemmed the gradual development of lithium hydroxide as a carbon dioxide adsorbent, the chlorate candle as a source of oxygen, superior storage batteries, and improved analytical techniques.

With nuclear reactor-powered submarines, however, it became evident to NRL chemists that the period of submergence would no longer be limited by the capacity of the batteries and that additional far-reaching measures to control the bubble of air would soon be needed. Sufficient chemicals for regeneration of the air could no longer be carried to make possible the desired long periods of submergence. Accordingly, work at NRL on submarine atmosphere habitability was greatly accelerated and at one time encompassed a team of over 20 chemists.

It was obvious that means for the *continuous* replenishment of oxygen, removal of carbon dioxide, hyrodgen, carbon monoxide, and other contaminants, and monitoring would be a must; and research and development proceeded along those lines. It also became apparent early in the game that the overall nature of the problem, especially subtleties, could be defined much better if NRL chemists could participate in an actual cruise of a nuclear submarine. This was accomplished in what later was to be termed the "Habitability Cruise" of the USS NAUTILUS in 1956. Not fully knowing what to expect, these chemists took on board many types of analytical instruments and sampling devices, including a mass spectrometer (disjointed so it could go through the hatch), an infrared spectrometer, and one of the first commercial American gas chromatographs. The NAUTILUS remained submerged for a record-smashing total of 11 days—to everyone's great exhilaration. It had been hoped to detect and identify different contaminants that might be present, but the one real lesson learned was that the submarine atmosphere was much more heavily loaded with more types of pollutants than had been expected and which by the very complexity of the mixture defied detailed identification on board.

The pollutants were shown to be mostly hydrocarbons, suspected to be from the solvents used in oil-base paints. Since the first few nuclear submarines were the object of almost constant visits by important and interested people, surfaces that needed it were touched up, in the good old Navy tradition, even while submerged. The volatiles from the paint so used had no place to escape, and the organic vapor loading of the atmosphere kept going up. It is true that nuclear submarines had been equipped with activated charcoal beds to remove "odors," but analyses showed that the charcoal beds became "saturated" with organic solvents in as little as 2 days, after which they were useless. That none of them caught fire is something for which we are still grateful.

It was difficult to break the "tradition" of painting at the slightest provocation but in time it was done, with NRL's encouragement. In the meantime, NRL chemists developed special water-base paints for touching up—special in the sense that they also had to be fire-retardant in keeping with Navy requirements.

It is recognized that the demands on submarine crews during extended patrols are rather considerable, so it is desirable to impose a minimum of constraints on these crews. Hence, the smoking of tobacco has never been banned for any length of time, although it contributes its own share of pollution to the atmosphere. One such pollutant, carbon monoxide, is particularly obnoxious on board, so, along with the hydrogen from the batteries, a means for its removal had to be found. NRL chemists turned to a World War I developed catalyst called Hopcalite which oxidizes carbon monoxide to carbon dioxide at moderate temperature. Above 100°C it will oxidize hydrogen also. However, Hopcalite is a good adsorbent for organic matter, and, as was found the hard way, in an atmosphere containing organics the catalyst can load up and then react explosively. By heating it even hotter, however, it is also an excellent catalyst for burning most organic matter. Having learned all these things, in cooperation with industry, a catalytic burner containing Hopcalite was developed which is now used in nuclear submarines. It turned out to be a mixed blessing because at the high temperatures Hopcalite also decomposes the fluorochlorocarbons used as refrigerants* in submarines to yield corrosive halogen acids. The thin line of compromise in temperature to optimize burning and minimize halogen acid production had to be found, and was. Extensive studies at NRL on the relative stabilities of the refrigerants themselves also led to the choice and use of more stable ones in current submarines, thus minimizing the problem considerably.

As the hydrocarbon content of submarine atmospheres was gradually reduced, their great masking effect for other potentially much more toxic contaminants was also reduced, and some of these reared their ugly heads, sometimes suddenly. A good example is a request which came to NRL to track down the source of extensive corrosion on a new boat. Good detective work on the part of NRL chemists traced it to the use of methyl chloroform as the solvent for the adhesive used for the hull insulation sheets, a new use. It is undesirable enough to have methyl chloroform in the atmosphere, but more importantly, since the catalytic burner can also act as a chemical plant, it yielded hydrogen chloride which is corrosive, and a new chemical, vinylidene chloride, which is even more toxic than methyl chloroform. Again, the real solution was to eliminate any further use of methyl chloroform. In similar vein, NRL has had to play detective repeatedly and by finding the sources of lachrymators, irritants, poisons, *etc.*, was able to eliminate many causes of discomfort and danger. The souces of some of these have not always been readily apparent. For example, who would have thought that by allowing the men to bring aboard their own propellant-type shaving creams and deodorants, something on the order of 30 pounds of fluorinated propellants of undetermined composition would get "dumped" into the atmosphere on a single patrol?

Unlimited smoking on board also created the need to remove the "smoke." Early analyses indicated that the aerosols in submarines were about 75 percent tobacco smoke and that the total levels were high. Here again NRL chemists accepted the challenge and today nuclear submarines carry highly efficient electrostatic precipitators for aerosol removal at a capacity of about 12,000 cubic feet per minute, making the atmosphere much more habitable than it had been.

Some of NRL's greatest contributions have been in the analysis, identification, monitoring, and control of pollutants. At the beginning, it was apparent that the submarines must have their own analytical devices,

*Experience has taught us that these refrigerants seem to have a great tendency to leak into the submarine atmosphere.

and so, after intensive research, the Mark I Atmosphere Analyzer came into being, almost literally held together with baling wire. But it performed well, and evolved into later and better models which are still in use. These devices monitor gases such as oxygen, carbon dioxide, carbon monoxide, hydrogen, and the simple refrigerants but do not monitor the higher molecular weight organics. Again, NRL chemists came to the breach and developed what is called the Total Hydrocarbon Analyzer, a specialized gas chromatograph for this purpose, which is now being installed in each nuclear submarine. A much improved overall analytical scheme based on mass spectrometry is now being developed which not only will monitor specific contaminants but, based on their concentrations, will eventually turn control equipment on and off as needed.

We take pride in the fact that this has indeed been a "success story," and that NRL has played a major role in it. That it has been successful can be seen by the fact that the atmosphere no longer determines the length of a submergence, that it does not impair performance, and that the men stay remarkably healthy. Although we have largely "tamed" our "bubble of air," advances are still called for and NRL continues to accept the challenge to improve the quality of nuclear submarine atmospheres.

Radio Astronomy at NRL

C. H. Mayer

Mr. Cornell H. Mayer joined NRL in 1943. Initially, he worked in the field of radar and radar components at centimeter wavelengths. In 1949, he began using radar electronics techniques for radio astronomy purposes. In 1968, he became Head of the Radio Astronomy Branch which is now a part of the Space Science Division.

The Navy's historical interest in astronomy was extended to radio astronomy 27 years ago at the Naval Research Laboratory. The intriguing possibilities of day and night and all-weather celestial navigation by using "radio stars" and direct radar sensing of the moon gave an incentive, along with the exciting prospect of unfolding a whole new universe as seen with radio waves. It is gratifying that NRL has been able to play an important part in the development of this area of astronomy.

Radio Astronomy at NRL evolved from part-time efforts of J.P. Hagen, F.T. Haddock, and a few others in developing and applying radio techniques at centimeter and millimeter wavelengths to observations of the sun and moon by the use of surplus radar antennas and radiotelescopes. These pioneering efforts not only provided a testing ground for techniques, but resulted in new information on the radio emission properties of the sun and moon pertinent to navigation applications and to the understanding of the physical process involved; for example, the first observations at short wavelengths of solar radio noise bursts accompanying flares, and investigations at millimeter wavelengths of the subsurface temperature of the moon (1). Significant contributions to accurate microwave radiometry were made in the areas of absolute calibration of radio noise power referenced to thermal noise sources (2), the application of nonreciprocal ferrite isolators and ferrite switches to virtually eliminate measurement errors due to impedance mismatch and allow rapid switching between arbitrary radio-frequency sources or antennas (3), and extension of microwave radiometry to millimeter wavelengths (4).

The early period included a series of observations of total solar eclipses (see Figure 1), with small radiotelescopes. These used the high "strip" angular resolution of the occulting disk of the moon to investigate the small-diameter regions of enhanced radio emission associated with sunspots, and to determine the size of the radio sun and the distributions of temperature and electron density in the solar atmosphere (5).

The most important achievement of this early period, though, must be attributed to the inspiration and accomplishment of the first large radiotelescope for use at centimeter wavelengths; the 50-foot diameter, solid-surface, paraboloidal reflector mounted on the roof of NRL's Building 43 (see Figure 2) (6). This large radiotelescope, which was made operational in the early 1950's, has a solid aluminum surface which was

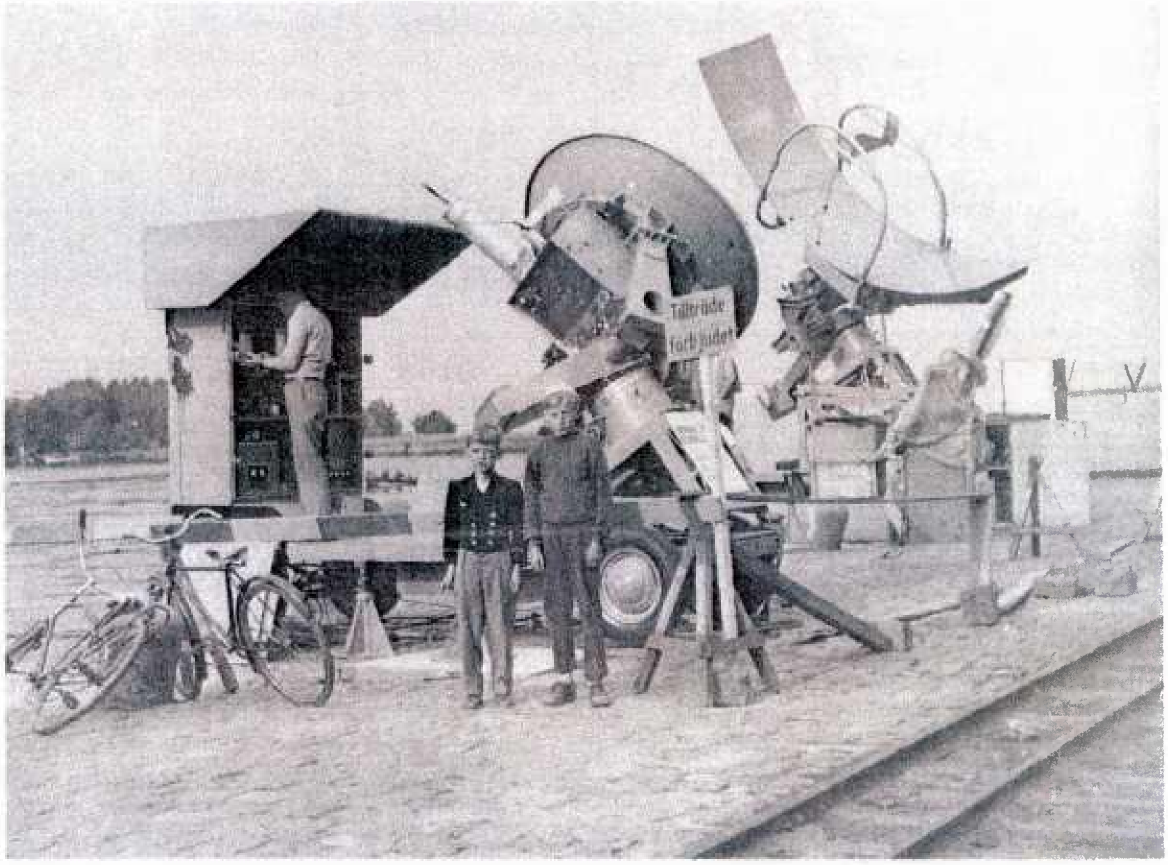


Figure 1 – NRL eclipse setup. Oskarshamn, Sweden, 1954.

accurately machined to a paraboloid of revolution. It was used successfully for astronomical observations to wavelengths as short as 2 centimeters (15 GHz), and less successfully at 8.6 millimeters. The main short-wavelength limitations, not surprisingly, are the restricted pointing accuracy of the surplus gunmount support system compared with the narrow antenna beam (0.1 deg at 2-cm wavelength), and gravity deformations of the cast-aluminum surface. The reflector was mounted on altitude-azimuth axes but could be pointed and guided in celestial coordinates through an ingenious electromechanical axis-converter with a nominal root-mean-square precision of 0.01 degree.

The combination of the 50-foot reflector and the microwave technology developed during the preceding years gave NRL a unique capability for microwave radio astronomy throughout the 1950's and resulted in many outstanding contributions to radio and radar astronomy. The first determination of the microwave spectra of the non-thermal celestial radio sources–radio galaxies and supernova remnants–was made, and a new class of discrete radio sources was identified in the thermal radiation of compact ionized hydrogen clouds, the H II regions (7). A new area of research was opened up by the discovery of absorption of the continuum radiation of discrete radio sources by the 21-centimeter line transition of neutral hydrogen atoms in the line of sight (8), making possible determination of the optical depth of the intervening hydrogen and providing a means of estimating the minimum distance to the source and of gaining angular resolution greater than the antenna resolution for studying the neutral hydrogen in the galaxy. The discovery of linearly polarized radiation from supernovae remnants and radio galaxies (9) confirmed the hypothesis that synchrotron radiation from relativistic electrons orbiting in a magnetic field is the responsi-

Figure 2 – 50-foot diameter, solid-surface, paraboloidal reflector

ble mechanism, and opened another new research area in investigations of magnetic field structure in the sources and magnetic fields in the intervening space. Still another new research area was provided by the pioneering investigations of thermal radio radiation from Venus, Mars, and Jupiter(10), which discovered the high surface temperature of Venus and led to the identification of synchrotron radiation from Van Allen belts around Jupiter by researchers at the California Institute of Technology by use of their large interferometer. The first accurate radar measurements of the distance to the moon were made (11), which gave information on the radius of the earth and lunar topography over the range of subearth points covered by lunar libration. In addition to these and a number of other contributions to astronomy, the 50-foot reflector served as a test antenna for the first microwave maser amplifier (developed by C. H. Townes' group (12)), and for other new electronic techniques.

Concurrently, pioneering millimeter-wavelength radio astronomy was carried out with 10-foot diameter, precision reflectors machined from solid aluminum castings. The machining tolerance was a few thousandths of an inch, and the reflectors have been used to wavelengths as short as 4 millimeters. The millimeter wavelength brightness distributions of the sun and moon and absorption of millimeter waves by the earth's atmosphere were measured (13), and the microwave spectrum of Venus was extended to 8 millimeters (35 GHz) and 4.3 millimeters (70 GHz) wavelengths (14).

By 1960, larger, 85-foot diameter reflectors with solid surfaces capable of operation to 3-centimeter wavelength had been set up for radio astronomy by the National Radio Astronomy Observatory at Green Bank and by the Office of Naval Research at the University of Michigan and at the University of California, so that NRL no longer enjoyed its superior capability. E. F. McClain, then head of NRL's radio astronomy group, wisely decided that remounting the smaller 50-foot reflector to make it more competitive would not be effective. Instead, a new, 85-foot reflector (see Figure 3) (15) was constructed at NRL's Maryland Point Observatory site, 45 miles south of the Laboratory. This antenna was designed to be a substantial improvement over the other 85-foot reflectors, and since its completion in 1965 it has been shown to meet or exceed design goals. As a result, NRL now has the most precise existing radio-telescope of this large size for short-centimeter and long-millimeter wavelengths with excellent performance, verified by measurements of astronomical sources to 40 GHz (7.5 mm wavelength).

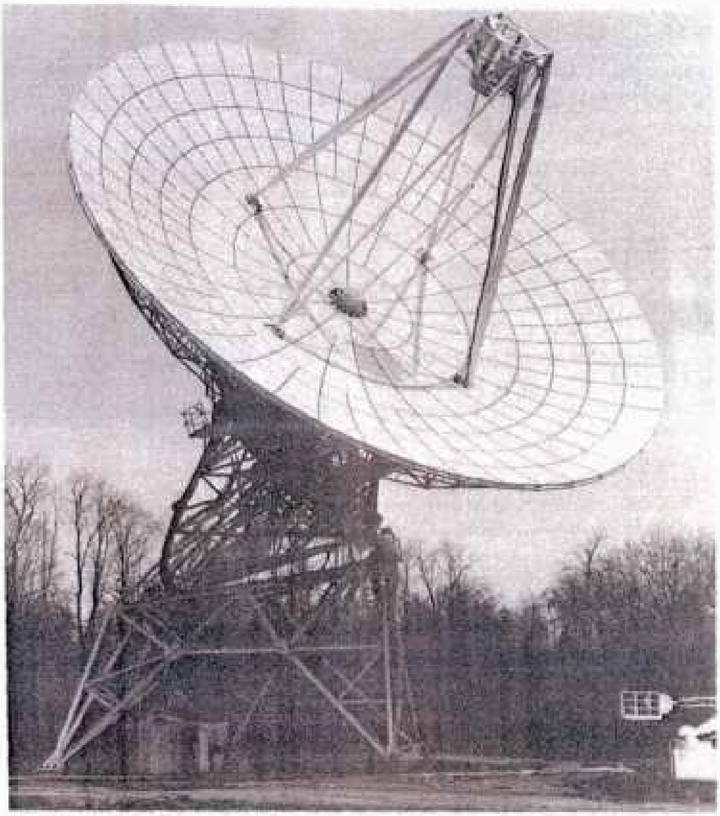


Figure 3 – 85-foot reflector at the Maryland Point Observatory site

The 85-foot radiotelescope has opened up a new short-wavelength region of the radio spectrum for investigation with high angular resolution and sensitivity and has made possible measurements of the temperatures in the atmospheres of the planets, including the distant planets Uranus and Neptune, of the short-wavelength intensity and polarization characteristics of radio sources and their variations with time, and of the polarized radiation properties of small-diameter active emission regions in the large magnetic fields associated with sunspots. Moreover, the superior short-wavelength performance of this antenna has made it possible for NRL to take an active part in new areas of radio astronomy, in investigations of molecules in space, through their microwave radiation in spectral lines and by very long baseline interferometry. A majority of the water-molecule sources have been discovered by NRL, and it was here primarily that their significant characteristics of extremely high spectral intensity, narrow line-width, polarization, short-time variability, and small angular size were determined, confirming properties expected of maser amplification by water molecules. We are investigating the properties of other molecules in space with less spectacular but physically important radio line radiation including ammonia, formaldehyde, OH, isocyanic acid, methyl alcohol, hydrogen cyanide, and carbon monoxide, and we are looking for new sources and molecules. In cases where the line transitions are at short-millimeter wavelengths, the 36-foot millimeter-wavelength reflector of the National Radio Astronomy Observatory is used. One of the most interesting and important recent results was the detection, in collaboration with Aerospace Corp. scientists, of radiation from carbon monoxide molecules in a giant ionized hydrogen region in another spiral galaxy, M 33, showing that the instellar molecular processes observed in our galaxy are not unique and are shared by other galaxies.

An important new area of investigation is the very long baseline interferometer technique in which the 85-foot antenna is used in conjunction with distant antennas, as far away as California and the USSR, to form Michelson interferometers having extremely high angular resolution, as high as 0.0003 arc second. It seems a paradox that the highest angular resolution in astronomy is achieved at the long-wavelength end of the electromagnetic wave spectrum. This technique is being used to determine the sizes, structures, positions, and possible motions of the very small diameter H_2O and OH molecule sources as well as small-diameter sources in quasars, galaxies, and pulsars.

Many of the significant developments and accomplishments in the history of radio astronomy at NRL have not been mentioned in this brief article, but the general course of development is indicated by the highlights covered. The current program is strongly coupled to the superiority of the 85-foot reflector at short wavelengths. NRL is indeed fortunate, in this period of hard times for science, to be able to benefit from its previous foresight and investments which have provided first-line facilities for current research in radio astronomy.

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NRL and the Fleet Ballistic Missile
Tapping the Corporate Laboratory

P. Waterman

Dr. Peter Waterman came to NRL in 1943 from the University of Vermont to head the Equipment Research Branch of Radio III (later Radar) Division. He was made Head of the Naval Analysis Staff in 1966 and continued in that position until his appointment in 1968 as a Special Assistant (Systems) on the staff of the Assistant Secretary of the Navy for Research and Development. Early in 1973 he was appointed Acting Assistant Secretary of the Navy (R&D) and served in that position until a successor to Dr. Robert A. Frosch was selected.

The Polaris Fleet Ballistic Missile (FBM) Program has many remarkable qualities. For one, it got there in a hurry without giving up excellence of performance. The development was seriously begun with establishment of the Special Projects Office on November 17, 1955, and culminated in 5 years with the deployment of the first of its operational 16-missile submarines, the SSBN 598 GEORGE WASHINGTON on November 15, 1960. The fact that this complex development job was accomplished with credit is a matter of record. These thoughts reminisce on the role that NRL played as a member of the Polaris team.

The NRL role began in the fall of 1955 when Admiral Raborn and Captain Merrill called in representatives of NRL to assist in structuring a contractor/laboratory team to implement the concept of a sea-based ballistic missile system. Soon other laboratories were involved, and principal representatives were made members of the Special Projects Office staff. This important step paved the way for the laboratories to provide their expertise at the appropriate working level and simultaneously at the management level of the program.

Representatives of Chrysler Corporation, working for the Army, headed the first industrial team to implement the development for the Special Projects Office. The basic Army-Navy agreement to the Jupiter version of an FBM was negotiated into existence at NRL (on a Saturday)! NRL for a time was directly involved, with Jet Propulsion Laboratory of Pasadena, California, in a radio-inertial guidance system for the missile, capitalizing on the experience from the Vanguard Program. However, in the fall of 1956 the Navy stopped development of the liquid-fueled Jupiter in favor of the solid-fueled, submarine-launched Polaris. The Jupiter missile, however, enjoyed its own success when it was used to launch the first U.S. orbital satellite, the Explorer.

During the hectic years between program initiation and the first deployment, there were technical problems directed to the Laboratory in such variety and quantity to engage and challenge the skills of every Division of the Laboratory. Yet, the problems were undertaken and solved without directly reorienting the ongoing research program of the Laboratory.

Thus one of the reasons for the timeliness of the first deployment was the effective melding of in-house Laboratory skill resources with Polaris technical needs. A survey of the NRL scientific program, in the spring of 1956, established that in nearly all major Laboratory areas of work there were active research program efforts under way having direct correlation with FBM research and development requirements. As a result, a continuing project was established at NRL, having the following objectives:

- To call out those currently active Laboratory efforts having direct correlation with FBM research and development requirements.
- To orient work on these so as to maximize their contribution to the FBM program with the least compromise to other existing objectives.
- To determine and define research and development areas wherein FBM requirements are inadequately covered at NRL or elsewhere, and to recommend to NRL and the Special Projects Office (now PM-1) that consideration be given to the initiating of appropriate R&D projects.

The Laboratory, with its wide range of scientific and technical capability, was in many ways prepared to join the Navy's emerging FBM team. That this was so is related to the wide range of technical demand called for by the concept and how it was matched by a Laboratory organizational structure which made it possible to bring the creative talent to bear on the problem when needed without the cumbersome trappings of what often is called "modern management."

The ability to reach across all department organizational and administrative lines made it latently possible to bring the creative talent to the job. The informality of dealing directly with the problem without the prior need to prove technical competence through some ritualistic proposal process stimulated the participation of our better people. The fact that they were able to make timely contributions to the running problem attests to their inherent skill. Solutions were often available when they were needed, in a form in which they could quickly be used.

The effort was people and problem oriented. Skill availability, however, was balanced with the demand. The range of involvement is so extensive that I remark on some of them with great risk that my memory may miss others of equal importance.

Much of the success of the Polaris development must be credited to the Submarine Habitability Program of the Chemistry Division under Dr. William A. Zisman. Surely the long submergence required could not have been possible without it. But the continuing participation, uncovering sources of contamination and toxicity, recommending alternatives and methods for policing the materials which go aboard our submarines, has led directly to the effectiveness of the total system.

Similarly, the Laboratory's Shock and Vibration program, under the late Dr. Irvin H. Vigness, Dr. Robert O. Belsheim, and Mr. Harold E. Forkois resulted in equipment and components which were designed and validated to operate in the environment and to survive severe conditions and still permit carrying out the mission. As an example, specifications for the shock mitigation system for the Polaris missile were prepared by Bob Belsheim. These involvements were both directly applied to the Polaris program and indirectly applied through the general contributions to submarine technology.

The development of the theory of fracture mechanics and its application to the program was spearheaded by Joe Kies, Dr. Henri Marcus, and Dr. George Irwin. They applied a newly emerging technology to the hydrotesting of Polaris steel motor cases and effectively made possible the first successful solid fuel, large rocket motor cases. They led industry by the hand into new methods of analyzing, fabricating, and proof testing of the cases. Their contributions stimulated evolution of the glass filament wound motor cases as well as the metal motor cases. The nondestructive testing of high-pressure vessels, the stress-corrosion cracking, and the fatigue characteristics of materials were advanced by Dr. B. Floyd Brown of Metallurgy. The fabrication and use of high-strength steels into the submarine was advanced by Mr. William S. Pellini, Dr. Peter P. Puzak, and their associates. The forming and welding of high-strength steels is still a very difficult technology.

There was great challenge in the test program of the developing Polaris missile. For instance, the sensing, telemetering, data readout, and analysis of missile system performance was of critical importance. Paul Stine made a large impact on the instrumentation of the missile system as well as on the ground instrumentation systems. He was concerned with the radar tracking and its accuracy; with the continuous reception of telemetry through the exhaust flame; with the ability of test instrumentation to perform reliably during the launch phase, as well as during each succeeding phase. William W. Balwanz fired many miniature rocket motors in our high-altitude chamber to measure the attenuation through the flame of telemetry signals. There were many other components of the system which were in some way worked on by NRL: the ability of a vibrating periscope to get a sufficiently good navigation fix; the transfer of a guidance command to the guidance package by optical techniques (Dr. Louis Drummeter and James Tucker); the isolating of freon leaks from the submarine airconditioners; and the nondestructive testing of missile components (Steve Hart and Lou Cardinal). The large and continuing effort in communications was applied directly to the Polaris program through Dr. Louis Gebhard, Charles Young, Loren Bearce, Bob Zeek, Bill Garner, and many others. There was a large involvement of the Laboratory in this area. Contributions of Bud Leppert, Dave Cubbage, Martin Musselman, and others all contributed to the effectiveness of the system. There was, as well, a steady stream of systems studies, hardware design studies, and materials investigations coming out of the Laboratory to assist the program in the optimization of design and the understanding of tactics. Notable among those who spearheaded these efforts were Charlie Dodge, Bill Hodgson, Forrest Titcomb, and Bill Shaddix.

The Laboratory participated directly on the FBM Steering Task Group as well as on many of its working groups. When special consultants were needed by the program, NRL reacted quickly to render assistance. By NRL's being a part of the program, many problems were recognized and solutions found before the problems became "program stoppers."

Shortly after the first deployment, the Polaris program, and particularly the laboratories participating, began to look at systems beyond the Polaris. Under the title Advanced Sea Based Deterrence Program, the NRL staff undertook many more basic research tasks, particularly in the areas of strategic communications, antisubmarine warfare, materials, upper atmosphere research, and others, some of which were forerunners to, or part of, some very sophisticated communication scheme now in use, or some to other classified application.

It would not be proper to say NRL was responsible for the success of the Polaris program. But, by having the expertise, by making it readily available, and by direct involvement in the day-to-day decisions of the Special Projects Office, the Laboratory did contribute materially to the success of the system, and particularly to the reduced time between concept and deployment. This could not have been done without the involvement of the existing skills of individual NRL people, an involvement which made use of what they were doing anyway rather than through a reorganization to achieve the value stated to be available by using more modern textbook management techniques. Though the job was done and supported by *all* the people at NRL, I have only remarked on a few of the specific efforts to show some of the extent of involvement. The notion was again demonstrated that a research laboratory can be responsive to the needs of the present.

Some Thoughts on the Years 1946-1957

O. T. Marzke

Dr. Oscar T. Marzke came to NRL as Superintendent of the Metallurgy Division in 1946. He became Associate Director for Materials in 1954 and the second Director of Research, 1956-1957, succeeding Dr. E. O. Hulburt. He was Vice-President, Fundamental Research, with U.S. Steel, 1957-1972, and is now retired. He has conducted and administered research in metallurgy, notably that of iron and steel, and is a member of numerous professional societies and committees.

The decade 1946 to 1957 was a wonderful time to be at NRL, because during this period the power of research became recognized by the general public, the military, and the Congress with the result that funds and personnel authorizations were most adequate. The major problem was building up the staff for there was too much competition for the limited number of good people available.

This seemed especially true for the Metallurgy Division, where I spent my first few years at NRL. But a few top-notch people were enticed, Bill Pellini being one of them. I remember especially discussions with Bill about the necessity for a test that would measure susceptibility to fracture at small deformation under fast loading rates. Before long he came up with the concept of the Drop-Weight Test, which today, with its several modifications, is used worldwide for evaluating materials where there is concern over the possibility of a catastrophic failure.

The largest research effort of the Division when I came to the Laboratory was on foundry technology. NRL was very well known for this work; it had contributed significantly since the late twenties. I asked Bill Pellini to head this activity. He did his usual fine job, but after a few years recommended that the work be phased out. He argued that the "cream had been skimmed" and that there was now an adequate base for handling the major current problems and those likely to arise in the future. I concurred, and the decision was soon known to industry for very shortly there was objection from industry and I was the object of some rather unflattering comments. Complaints were even registered with the Navy Department. The work was stopped, however, and after a few months the furor died down and relations between industry and NRL again became cordial.

Very early in the 1950's I was asked to take on the additional task of administering the Metallurgy Program of ONR. This dual assignment lasted for 2 or 3 years. The ONR work enabled me to become well acquainted with academic personnel and research and with the ONR organization and its administration policies. One amusing aspect of running both jobs was the necessity of writing letters to myself for the sake of proper records. I particularly enjoyed writing from one office requesting me to do something in the other and then replying that I couldn't. The ONR experience was very valuable and led to additional assignments there, one of which was to implement some of the research directed toward the Navy's submarine ballistic missile system.

Also in the early fifties one of the Directors of the Laboratory, Captain Beltz, felt it was desirable to review critically the organization and administration of the Research Department. Bob Page, Ernie Krause, and I were assigned this task and I was given the chairmanship presumably to prevent domination by either the well-entrenched electronics or the newly emerging, aggressive nuclear group. The report recommended a line organization with three Associate Directors of Research—one for Electronics, one for Materials, and one for Nucleonics—with the existing Divisions reporting to one of these Associate Directors. I like to think that bringing the Chemistry, Mechanics, Metallurgy, and Solid State Divisions into a single administrative unit was the forerunner of the Interdisciplinary Laboratories for Materials which the Advanced Research Projects Agency set up a few years later in several academic institutions.

The big event of the midfifties was, of course, Project Vanguard. NRL, working with ONR, submitted a proposal to launch an earth satellite. NRL's proposal won out over those of competing laboratories largely, I think, because of NRL's experience and capabilities in launching missiles for exploring the upper atmosphere. The NRL proposal was based on using an aerodynamically shaped body for the satellite and proven launching components wherever possible. But then, to minimize the potential military applications of a satellite, a committee of the National Academy of Sciences was appointed to select experiments that would emphasize the scientific potential. This resulted in the necessity for extensive modification of the system. I like to think that if NRL had been permitted to go ahead with its original scheme, the United States would have beaten the Soviet Union in launching the first satellite. Maybe being second was a good thing, for it probably contributed to the outlay and effort for such work which occurred shortly thereafter. An upper limit of $20,000,000 was placed on Vanguard, and the White House doled the money out rather miserly. What a change in but a few years!

I left the Laboratory just a few months before the first Vanguard launch but only a few weeks before Sputnik. Some of my "friends" accused me of leaving because of my knowledge that the Russians would beat us and that I did not want to be around "to face the music." But the opportunity to do something that I had long felt needed doing—building a fine research organization in the steel industry—was too great to turn down.

Since leaving I have often thought of my days at NRL, not only of the research work but also of the people. They were not only technically competent but had a strong loyalty and desire not only to help the Navy but the country as a whole. Several members of the staff turned down very remunerative industrial offers. Of course, NRL had much to offer—excellent facilities, adequate funds, and an excellent research philosophy with a most sympathetic military administration.

A few years ago I served on a committee made up largely of industrial and academic people charged with looking into the role and operations of all the government laboratories. It was always a source of much pride and pleasure to have NRL cited frequently as an example of an outstanding government laboratory. It was considered to be in the same class as the best industrial and academic ones. I am sure NRL will continue to be held in such high regard throughout its next 50 years.

A Love Affair

C. L. Buchanan

Mr. Chester L. Buchanan joined NRL in 1946 as an electronics engineer in the Sound Division. He has headed in succession the Sonar Systems Branch and the Deep Sea Research Branch of the Sound Division and the Ocean Engineering Branch of the Ocean Technology Division. Currently he holds the position of Associate Superintendent of the Ocean Technology Division. He is widely recognized for his accomplishments, and those of his group, in the THRESHER, H bomb, SCORPION, and ALVIN incidents.

I had to laugh—there she was, the funniest ship I had ever seen. With those big holds forward empty, her huge ice-strengthened bow pointed up toward the sky. The rat guards on the mooring hawsers looked like two big red eyes, and the mooring hawsers sloping sharply down to the dock looked for all the world like whiskers. At first impression, here was a huge gray rodent with red eyes and rust-colored whiskers staring curiously at the two intruders who strode down the dock.

On board the mood was different. She was a real lady. Her paintwork was clean—the furniture was of honey maple, freshly varnished. After we entered the strangely silent engine room, a huge lump raised in my throat. I was falling in love, but I was not the first. Taped to the engine was an ordinary sheet of paper torn from a writing tablet. Written across it were the words "Goodby Old Girl."

Jerry (J. J. Gennari) had recommended MIZAR to me, but neither of us had seen her before. Her 260-foot length and 50-foot beam, with a displacement of 3700 tons, made her big for an oceanographic research ship. Her ice-strengthened hull, twin screws with single rudder, and her deisel electric propulsion system promised excellent maneuverability at slow speeds. These were the characteristics we were looking for, the characteristics we must have to probe the 8000-foot depth of the North Atlantic and to locate and photograph the hulk of the submarine THRESHER, tragically lost on April 10th of that year, 1963.

It was November now, and our frustrations from the failures of that summer—due to inadequate ships and equipment—were giving way to a dogged determination to do it right, in 1964. The first requirement was a suitable ship and we quickly set about our task—to determine whether this ship could do the job.

We climbed and searched throughout every frame of this lonesome ship. At every turn it was the same story—her big forward holds would permit speedy modifications. Nothing would have to be torn out. The main operatioh center could be built in the after end of the second deck of the number-two hold, and on either side of the yawning hatch was room for staterooms for the scientists required to man the search gear. Towing would be no problem—a saddle could be built on the gunnel, one of the huge cargo booms could be used to lift the sensor vehicles over the side, and the boom secured in the saddle would provide a perfect towing arrangement.

It was with a sense of excitement that we flew back to Washington—our heads filled with plans and ideas—eager to commence the transformation of this freighter into an oceanographic research ship, specially equipped for deep-ocean search and inspection.

We were not the only group planning for the next summer's operations. At the Navy Electronics Laboratory in San Diego, an equally dedicated group was hard at work modifying the submersible TRIESTE in preparation for the search.

The winter months passed quickly. We planned, estimated costs, built equipment, and experimented, tested, and checked our gear. We even went to sea on ROCKVILLE (PCER 851) to test our equipment at sea. On the 10th of March, MIZAR sailed from Seattle for the long voyage to the east coast. The Military Sea Transport System (now Military Sealift Command) operated the ship for us. After a 3-week shipyard modification in Savannah, Georgia, we installed our equipment and sailed for Boston. After a few minor modifications, we sailed on our first short shakedown cruise.

Our faith in MIZAR was high, but we had not yet learned how to use her. First we practiced launching our "fish" without any instruments on it. The ship's crew soon learned how to launch the "fish" without crashing it into the side of the ship. Soon we started installing the sensors and testing them. At the same time we were trying out our Underwater Tracking Equipment (UTE). This equipment was designed to determine the location of transponders on the ocean floor and also to determine the location of our "fish." It also was intended for use in tracking the submersible TRIESTE.

After a week or so we were confident of our ability to "work" the ship. We headed back into Boston to make our final preparations.

Meanwhile the TRIESTE had been brought to Boston and was also being prepared. Since her preparation involved the safety of men, the work was more exacting and she was not ready for sea.

Admiral Coates was the Task Force Commander (CRF 168), and Captain Frank Andrew was Task Group Commander (CTG 168.1). Their little fleet consisted of the MIZAR (now classed as an oceanographic ship–T-AGOR-11), the USS HOIST (ARS 40), and the submersible TRIESTE. MIZAR sailed on June 25, 1964, to start the THRESHER search. In the incredibly short time of 2 days we located and photographed the hulk. By July 22 we had photographed almost the entire hulk which was broken into five major pieces.

Proudly, yet sadly, we sailed into Boston on July 23 with a broom displayed at the signal halyard.

By this time the TRIESTE was nearly ready, and we all turned to in preparation for the task of guiding TRIESTE to the wreckage to make a close-up inspection.

In early August the entire task force arrived in the area, and preparations were made for TRIESTE to dive. Her first dive on August 14 was unsuccessful, due to a fault in her gyrocompass. Her second dive also was thwarted by the same problem. During these dives we on MIZAR were gaining experience in tracking her and were very sure that once her gyrocompass was operational, we could direct her with ease.

The third dive proved to be one of the most amazing operations we could have imagined. Captain Andrew elected to make the dive as observer. The officer in charge and pilot were Lt. Commander J. B. Mooney and Lt. John Howard, respectively. As the dive progressed we all breathed a sigh of relief as it became evident that the gyrocompass was working. For a short time the TRIESTE rested on the ocean floor at 8400 feet, while we meticulously determined her position by use of the UTE. Lt. Denny Curtis was the dive director aboard MIZAR, and he was frequently in telephone contact with those aboard TRIESTE. Unfortunately, the telephone transmissions interfered with the UTE operations so that he had to restrain these communications.

We informed Captain Andrew of the course to steer and the distance to the THRESHER hulk, and wonder of wonders, the TRIESTE position moved slowly in the correct direction.

This had never been attempted before, and we really had no way to determine the accuracy of the system. Finally, the little pencil dots on our plotting board reached the point where we had previously judged the THRESHER hulk to lie. Lt. Curtis informed Captain Andrew that they were now so close that they were within our circle of confusion. Now Captain Andrew is an excellent mathematician and a great believer in statistics. He therefore immediately asked the pilot to set TRIESTE down on the ocean floor. He then informed Lt. Curtis that they saw nothing, and requested that we compile a lot of data and average it so we could vector them closer to the hulk. We did so but to no avail. The statistics always converged on the spot we had selected (and which by now we had begun to question).

TRIESTE is a pretty tight fit for three men. The third man, who is looking out of the viewport, is sometimes almost forgotten down on the bottom of the dark capsule as he peers out into the murky water.

As they sat still waiting, the observer said, "Say, how high are we above the bottom?" "Man, we're resting on the bottom," shot back the pilot, "we're just waiting for those clowns up there to tell us which way to go." Observer: "Well, then! Why can't I see the bottom?" Pilot: "You can't see the bottom?" Observer: "Heck no! I can't see anything!" Pilot: "Wait a minute, let me turn this thing around!" Observer: "Hold it! Holy cow, there's a piece of metal—and it's made of metal, it's—it's the hulk!"

Yes, stranger than fiction, all the while TRIESTE had been neatly balanced directly on top of one section of THRESHER, with the viewport looking over the end and too high above the bottom to see it.

Nine years ago, yet it seems like yesterday. Since then there have been many other memorable events. With MIZAR we assisted in the location and recovery of an H bomb, located and photographed the lost submarines SCORPION and EURYDICE, and located and actually recovered the research submersible ALVIN from a depth of 5100 feet (with an assist from the research submersible ALUMINANT). In all these years she has never let us down. MIZAR, I love you!

Reminiscences of a Research Director

J. H. Schulman

Dr. James H. Schulman, currently Associate Director of Research for Materials and General Sciences, came to NRL in 1946, following teaching positions at Suffolk University and MIT, and research positions in industry. His research accomplishments in solid state physics, particularly luminescence in solids, led to his appointment to the Chair of Materials Science in 1964. He has also served as Deputy Scientific Director of the London Branch of the Office of Naval Research (1960-61) and is the recipient of many distinguished honors. He was appointed to his present position in 1967. During the past year Dr. Schulman has carried the additional responsibility of Deputy Director of Research.

I was rather vague about the early history of NRL when I arrived here, in the spring of 1946, but a quick inspection made it clear that L'Enfant had had nothing to do with its layout nor Christopher Wren with its architecture. I had been asked to join the Crystal Branch of the Sound Division to initiate a research program on luminescence materials—the "phosphors" that were so important for cathode-ray-tube screens, phosphorescent paints and coatings, and light sources—and which presumably had potential for many other wondrous applications.

The conditions of employment were good: I was told that I could recruit my own group of scientists, and that I could have a spacious laboratory once the cafeteria was moved out of Building 12. So I took up residence in Building 1, that model of Navy-Yard neoclassic design which gives the older part of our campus its charm, and tried to get to work. This was rather slow business because the cafeteria took its time about moving, and the system took even more time to reconstitute the cooking premises into a furnace room for high-temperature firing of phosphors. But the wait was made tolerable by the wonderful library, the ferment of new people and new ideas that were permeating the Laboratory, and the weekly (or was it biweekly?) visits of the paymaster, who set up his table in the hall, rang a bell, and with the help of a yeoman or two paid us off in cash. Nuclear energy had been tapped, but the Navy had not yet discovered payment by check! This was a delightful anachronism, but fortunately such a backward attitude did not carry over into the research program.

The Laboratory, and the Navy through the Office of Naval Research, had realized that there were few scientific disciplines that did not have a strong bearing upon naval and defense operations. This realization had produced a broadening of the Laboratory's research horizons. The peacetime (albeit cold-war) environment gave the Laboratory a chance to enhance its basic research program and to push it into new areas not previously explored. For example, besides the Laboratory's new venture into luminescent materials, research was burgeoning on crystal growth, cryogenics, semiconductors, photodetectors, very high magnetic fields, and radiation effects in solids—to name but a few of the topics in my general fields of interest, solid-state physics and chemistry.

After my experience with industrial research, which was limited by short-term economic goals, it was extremely gratifying to be able to pick basic research problems of longer range and to feel that one was working for the national community rather than for a narrow private profit goal. I was not only gratified but grateful, and I felt a strong obligation to repay the investment being made in my basic research program by seeking out applications of this research.

These were not long in forthcoming, and they lay largely in the field of integrating detectors (dosimeters) for ionizing radiations. A Secretary of Defense of that era had indicated his contempt for basic research by stating that he did not care to know why the grass turned green. In the face of that know-nothing attitude, I derived great pleasure from pointing out that the Fleet hardware we were producing had originated from the academic question, "Why does rock salt turn yellow when it is exposed to x-rays?"

The leap from academic research into production was rather precipitous; within little more than a year from our first demonstration of radiation measurement by the radiophotoluminescence of silver-activated phosphate glass, production of personnel dosimeters for Navy, Marine Corps, and Air Force use was under way under Bureau of Ships' auspices. Research had to take a back seat to the consultation, monitoring, design, and production problem-solving that occupied the next year or so, which involved three of the country's major glass companies and a number of leading optical and electronics firms.

The success of this development laid the groundwork for NRL's reputation as the world's center for research and development in solid-state dosimetry. A highlight of this work, which produced an indelible memory, was my work at the Nevada Test Site to put the dosimeters to their final critical tests. The military personnel dosimeter was followed in succession by a miniature version useful for insertion in body cavities or implantation in tissue for use in clinical radiology; by inorganic and organic solid-state dosimeters for measurement of very intense radiation doses, such as are useful for pasteurization and sterilization; and finally by a dosimeter based on thermoluminescence, which sparked an outburst of worldwide research on this type of system which has given rise to three international conferences. NRL-invented solid-state dosimeters of several types have been produced and used not only in the United States but in most of the NATO countries, Japan, and in the Soviet bloc. Although these inventions were originally motivated to produce a diagnostic tool for military personnel in the event of nuclear attack, they have fortunately seen wide use in applications of a more peaceful nature, such as clinical radiology.

In this stream-of-consciousness reminiscence, I have omitted any reference to my colleagues, simply because one can pursue only one train of thought at a time. The growth of the group was slow, but one could hardly have been more fortunate in the individuals who joined it. The initial additions, led by Robert Ginther, had a strong flavor of chemistry. The arrival of Clifford Klick signaled the wave of solid state physicists that was to come and outnumber the chemists. Such people as Drs. Klick, Howard Etzel, John Lambe, Dale Compton, Herbert Rabin, and David Patterson—and consultants such as David Dexter—made the group one of the world's leading research groups in the field of color centers in solids. The accession of Frank Attix added an acknowledged expert in radiation physics to lead the dosimetry efforts of the group. It is significant and gratifying that most of these leading researchers are either still at NRL occupying key supervisory or research positions or have similar high responsibilities in other Government agencies, universities, or private industry. The successors to these outstanding scientists have carried on in the old tradition and have made their own reputations in the fields of kinetic spectroscopy of solids, solid-state optical memory systems, fundamental studies of glass, fast neutron dosimetry, nonlinear optics, and many other fields of endeavor.

The national attitude toward scientific research has changed markedly since the two golden decades following the close of World War II. Although the concern with both military and civilian relevance is an understandable attitude on the part of a tax-paying public, it has tended to shorten and sharpen our focus and to preclude some of the free-wheeling excursions into new fields that we were permitted years ago to undertake on our own. This, coupled with the trend toward detailed managing of research programs from on high, have made the individual researcher somewhat less the master of his own fate than used to be the case. The vitality of the laboratory's scientists and engineers seems unquenchable, however, and as the Laboratory embarks upon its second 50 years, the work that we are engaged in seems to be at least the match of our earlier output in terms of intellectual excitement, quality, and importance to science, the Navy, and the Nation.

Structures That Matter

J. Karle

Dr Jerome Karle obtained his Ph.D. in physical chemistry from the University of Michigan in 1942. After working on the Manhattan Project, he joined NRL as Head of the Electron Diffraction Section in 1946. He then became Head of the Diffraction Branch in 1958. Dr. Karle has developed theoretical methods for determining the structure of crystalline materials while pursuing experimental measurements on free molecules, and ordered and disordered solids, by electron, x-ray, and neutron scattering. He presently heads the Laboratory for the Structure of Matter at NRL.

Almost my entire professional life to the present has been spent in pleasant association with the Naval Research Laboratory, a period which extends over somewhat more than half the years which form the golden anniversary. The outstanding impressions of this experience concern not only the challenging subject matter of my research program but also the circumstances under which this program was carried out.[1] The Laboratory afforded an atmosphere in which it was possible to pursue free inquiry on behalf of long-range scientific goals. This was very much in the tradition of Navy-sponsored research. To its everlasting credit, the Office of Naval Research supported after World War II a broad spectrum of basic and applied research throughout the national and international community, both because it could and because it was recognized to be important to do so. In a most enlightened fashion it fulfilled its mission and simultaneously enhanced scientific, technological, and cultural well-being on a national and international level. If this enlightment comes as a surprise to the reader, as it has over the years to many of my professional colleagues—so be it!—enlightened and very fruitful it certainly was.

The subject matter of my research program has concerned the structure of matter on the atomic and molecular level. Structural research occupies an almost unique position in science because of the fact that the information it affords is utilized continuously in so many other fields of research. A host of phenomena in the physical, chemical, metallurgical, geological, and biological sciences are interpretable in terms of the arrangements of atoms in the substances under study. Structural information not only facilitates understanding, it also motivates the pursuit of more penetrating research investigations.

It has always been one of our research objectives to improve the theoretical and practical aspects of structure research in order to extend the range of materials which could be investigated and to facilitate the analyses. Our initial research at NRL in the late 1940's concerned the study of gaseous molecules by the

[1]In my early years at NRL, Dr. H. Friedman was the head of the Branch and Dr. E. O. Hulburt was the head of the Division in which I worked. As time went on Dr. Hulburt became the first Director of Research and Dr. John Sanderson took his place as head of the Optics Division. Dr. Wayne Hall later became an Associate Director of Research with my research program under his purview. In recent years, Dr. James Schulman, as an Associate Director of Research, assumed administration of my research program. There have been several Directors of Research since the retirement of Dr. Hulburt and the present administration of Dr. Alan Berman. I wish to express my deep appreciation to all these people for their sympathy and support. (See accompanying articles by Drs. Friedman, Hulburt, Sanderson, Schulman, and Berman.)

technique of electron diffraction.[2] In pursuing our studies in this field, we had the good fortune to develop some insights and principles concerning structural research which have been applicable to various other techniques of analysis, *e.g.* x-ray and neutron diffraction and the other states of matter such as solid and liquid. These developments in fact permitted us to readily extend our program into these additional fields of structure research and successfully attack a problem which had been written off as inaccessible.

We learned from the electron diffraction investigation of gases the value of introducing mathematical and physical constraints into the analyses. These constraints consist of such matters as the range in which interatomic distances can be expected to occur, the shapes and areas associated with the probability distributions for interatomic distances, and the shape of the intensity of atomic background scattering. A very important constraint is the required nonnegativity of the probability distribution representing the distribution of values for the interatomic distances. The usefulness of this constraint led to the search for a similar one in the other fields of structure research. A moment's reflection would suggest that the required nonnegativity of the electron distributions in the unit cells of crystals may afford an important constraint for facilitating crystal structure analyses. This proved to be true and led to the solution of the key problem in crystal structure analysis which had been regarded as unsolvable.

The problem concerns a certain portion of the information which is needed in order to compute directly the locations of the atoms in the unit cells of crystals. This computation is made by means of Fourier series whose coefficients are in general complex numbers. A complex number can be expressed in terms of a magnitude and an angle called a phase. Ordinarily, in an x-ray diffraction experiment, only the magnitudes of the coefficients for the Fourier series may be measured, but not the phases, giving rise to the so-called phase problem in x-ray crystallography. As a consequence of the absence of phase information in a diffraction experiment, it was generally believed into the 1950's that the phase problem was insoluble in principle. The *in principle* aspect of this belief is rather curious, because it was known since 1935 that, at least in simple cases, a special Fourier series discovered by Patterson could obviate the need to know the phases. In any case, those who would agree that a solution may in principle somehow exist vehemently maintained at that time that no practical solution could be found. The fact is that the phase information is contained in the observed magnitudes of the Fourier coefficients in a subtle way, and the practical aspects of phase determination involve the mathematical manipulation of these magnitudes.

There is an important type of information available which was ignored in the arguments presented to show that a solution did not in principle exist. The arguments involved the fact that the computed Fourier series represented electron distributions which were treated as if they were completely unknown. The desired structure would be interpretable in terms of atoms whose centers are located essentially at the maxima of the electron distributions. The point of interest here is that the electron distributions around free atoms are known very accurately and they change only slightly when atoms make bonds and form crystals. The structure problem then is initially one of locating atomic centers, rather than electron distributions. An algebraic analysis of this circumstance shows that with the usual amount of x-ray diffraction data which are collected, centrosymmetric structures are overdetermined by a factor of about 40 to 50 and noncentrosymmetric ones by a factor of about 20 to 25. We indeed have a solution in principle, and the great overdeterminacy implies that a fairly simple solution exists. The problem then was to find it.

The realization of the great mathematical overdeterminacy of the phase problem was a strong motivation in the search for a practical solution. The main mathematical developments on which the current procedures are based occurred at the end of the 1940's. We were interested in learning the mathematical consequences of the fact that the electron distribution functions representing crystal structures must be nonnegative. This physical constraint led to an infinite set of inequality formulas, one of which is the main formula presently used. There was a paper on inequality formulas published 2 years previous to ours, by D. Harker and J. S. Kasper. It represented a significant breakthrough in the phase problem, since it showed

[2]The early research on electron diffraction was performed in collaboration with Dr. Isabella L. Karle. The early theoretical work on crystal structure analysis and some of the practical aspects were performed in collaboration with Dr. H. A. Hauptman. The currently employed method for crystal structure analysis, the symbolic addition procedure, was developed in collaboration with Dr. Isabella L. Karle.

promise in application to some simple structures. The mathematical approach employed by Harker and Kasper was quite different from ours, in that some specialized techniques in inequality theory were used and it was only after we derived the complete set of formulas based on the nonnegativity of the electron distribution that we realized that there was a connection between their work and ours. It was possible to derive the Harker-Kasper inequalities as special case of our results. The main inequality formula now employed appeared explicitly in our results.

Our initial interest in the crystal structure area was simply motivated by the feeling that it could be useful to explore the mathematical implications of the fact that the electron density distribution is a nonnegative function. I wonder how convincing I would have been had I been asked at the time to make a statement of relevance regarding this mathematical pursuit. What may be more to the point, I shudder to think what may have happened to this research project had the administrators of my program felt impelled to act on the judgment of the crystallographic community regarding the value of this program, in view of the fact that, with possibly rare exception, the phase problem was regarded as insoluble even in principle. The administration was aware of the opinions of my colleagues, but they decided that I was not given to foolish pursuits. They offered me the encouragement that a young man requires and a working atmosphere which was free from unrealistic time restrictions.

It took some years to develop the practical application of the mathematical results. This was due as much to the lack of sophistication in the computers that were available and the tedium involved in collecting a full set of three-dimensional x-ray data during the 1950's, in the absence of automatic diffractometers, as it was to the difficulties which almost always arise in bridging the gap between pure mathematics and its application to practical problems. At the present time, most of the crystalline substances composed of atoms of almost equal atomic number are analyzed by means of the direct method of phase determination developed at NRL. Many more types of substances are now accessible to study than were heretofore. An x-ray structure analysis can be used to identify chemically new materials which are not amenable to other methods of analysis because of their small quantity or because of their chemical novelty or complexity. Examples are natural products and rearrangements induced by the effects of radiation. The x-ray analysis can determine which elements are present, their stoichiometric formula, and their arrangement in three-dimensional space. Such information can be used to clarify geophysical and geochemical phenomena, chemical reaction mechanisms, mechanisms of radiation damage, and a variety of biomedical phenomena such as cardioactivity, ion transport through membranes and its associated neurological implications, antibiotic activity, the action of toxins and venoms, and the effects of chemical and physical agents on genetic material. Often a knowledge of such processes on the molecular level can facilitate the development of new substances which are modified to improve their behavior. Examples of such matters can be readily drawn from the experience of the past decade.

One rather interesting illustration concerns a structure investigation carried out in our laboratory on batrachotoxin, the most potent nonprotein venom known. The venom is used as an arrow poison by Colombian Indians and is extracted from the frog *Phyllobates aurotaenia.* Heroic extraction and chemical purification efforts by a group of scientists at the National Institutes of Health produced one tiny crystal of a derivative having a diameter of about 0.03 millimeter. There was too little material available for identification and characterization by chemical methods. Even if there were enough material, chemical analysis would have been a long and costly process. It was possible to perform an x-ray structure analysis on the tiny crystal of the batrachotoxin derivative within about 4 to 6 weeks. As a consequence, it was possible to determine the chemical composition, the molecular formula, and the stereoconfiguration of the molecule. It was immediately recognized that a part of the molecule is a steroid with a type of junction which causes the molecule to curl up, similarly to cardioactive steroids such as digitoxigenin and strophanthidin which have also been investigated in our laboratory. The curled shape can be correlated with the strong effect that batrochotoxin has on the heart. This venom also acts on the nervous system, having a profound effect on the transport of sodium ions. Batrochotoxin reverses the effect of tetrodotoxin, which occurs in the Japanese blowfish, on the transport of these ions. A consequence of the x-ray structure analysis of the batrochotoxin derivative was the complete characterization of the chemistry and configuration of the batrachotoxin molecule. This has facilitated its laboratory synthesis and also its modification to make it more potent. Its cardioactivity makes it a material of interest in the search for improved cardioactive drugs. Because batrachotoxin and tetrodotoxin can act as reversible antagonists in the transport of sodium ions, they have become important laboratory reagents in neurophysiological studies.

Over the past decade our laboratory has had the opportunity to apply the method for x-ray structure analysis based on direct phase determination to a variety of interesting materials such as products of the radiation damage of genetic material, photorearrangement products of pharmacodynamic amines, clathrates, insecticides, an antinarcotic drug, a high-blood pressure repressor, the chemical involved in the primary visual response in the retina, and many others. In these studies, chemical identification, configuration, and bonding were clarified, and a deeper understanding of reaction mechanisms and chemical and physical processes has resulted.

I have illustrated briefly how the formulation of physical and mathematical constraints in one field, in this case, the field of electron diffraction of gases, could be extended and developed into a productive activity in another field of research, x-ray structure analysis. In a parallel fashion, such constraints have also been applied to the analysis of the glassy state. It has recently been determined in our laboratory, by the use of appropriate constraints in the structure analysis, that some basic glasses possess a great deal more crystalline-like ordering than had been formerly reported.

The potential for new developments in mathematical and analytical skill, motivated by the field of structure analysis, is still very great, and the practical realization of significant advances appears to be within reach. There is still considerable potential for progress in, for example, the field of x-ray structure analysis, because there is still a great deal of information contained in the observed data which currently may be extracted in only a relatively inefficient fashion. In attempting to increase this efficiency we have been led recently to mathematical developments which have implications that extend far beyond the field of structure research. This is the mark of a good problem, one which originates from a basic precept or concept (nonnegativity in this case), leads to significant progress in its fields of application, and ultimately has broad implications which facilitate advances in many other scientific disciplines. The provision of the time and the opportunity to pursue such a program of research is the mark of a good administration.

Questions of policy concerning the support of science which profoundly affect the course and extent of scientific research arise anew from time to time. Currently the trend is to favor reduced support, with emphasis on short-range goals. The implication is that our society cannot afford to do otherwise. If one pauses for a moment to consider the consequences of such a policy on scientific and technological progress both in terms of the creativeness and the productivity of the established professionals and the encouragement of young, talented people to make the sacrifices of time and expense which are necessary for the development of their talents, one must conclude that a policy which offers reduced support and favors short-range goals, disproportionately, is most certainly the one thing that our society cannot afford—not even remotely.

Congratulations for 50 Successful Years

B. F. Bennett

Captain Bradley F. Bennett, USN, graduated from the U.S. Naval Academy in 1935. He has master's degrees from MIT in physics (1953) and in naval construction (1940). Captain Bennett was the Engineering Services Officer at NRL from 1947 to 1950, and he was the Director of Administration at NRL from 1960 to 1963. He served as the Laboratory's 20th Director, from May 1963 to January 1965.

After retirement from the military service, Captain Bennett joined forces with Universities Research Association, Inc., as Vice President for Administration, a position which he still holds.

Organizations rise and fall. It is a remarkable achievement for a research laboratory to show the vitality which NRL shows after 50 years. This must be due to the great people carrying on the programs of the Laboratory, their excellence, their dedication, and their pride. The chief credit belongs to them.

But environment is important to success in research, and I would like to pay tribute at this time to some of those not so fortunate as to have directly participated in the research program of the Laboratory but who nevertheless made important contributions to creating a favorable environment. First there was Edison and his committee, who gave NRL its broad charter. Then I think of scrappy Commander Oberlin, who was, in fact, the first administrative head of the Laboratory. His eagerness to implement the full breadth of the charter played a key role in bringing in Hulburt in Optics, Bichowsky in Chemistry, and Mehl in Metallurgy.

Another critical time in NRL's history was at the end of World War II. We owe a great debt to the entrepreneurs, Admirals Harold Bowen, Sr., and Louis de Flores, and the architects, Robert Conrad, James Wakelin, Bruce Old, and others who created in ONR a suitable agency to represent this great Laboratory and to protect its integrity and independence. We also owe a great deal to Commodore Schade, who undoubtedly negotiated that independence, who structured a coherent Laboratory out of the monstrosity which had grown to satisfy wartime needs, and who set the policies which ever since have kept a balance between fundamental and applied work and have emphasized personal excellence of staff.

NRL has had friends in high places, their friendship based on respect, not on charity. I remember well the consideration of James Wakelin as ASN(R&D) and of Harold Brown as DDR&E, just to mention two whom I observed first hand. But sometimes there may not be any such friends, and NRL must stand firm on its principles and its reputation. I pray that its proud heritage never be sold for any promise of temporary comfort or affluence.

Underwater Acoustic Calibration Technology

R. J. Bobber

Mr. Robert J. Bobber came to NRL after 4 years with the Marine Corps. Since 1947, he has been with the Underwater Sound Reference Laboratory (now Division) in Orlando, of which he was Chief Scientist, 1967 to 1968, and Superintendent, 1968 to the present. His work has been in the fields of undersea warfare, underwater sound measurements, and electroacoustic transducers.

In 1941, on the eve of the United States' participation in World War II, the U.S. Navy had only begun to exploit underwater acoustics as a means of detection. There was no capability for calibrating, testing, or evaluating what was to become known as sonar transducers, except by simple "cut-and-try" techniques.

The Office of Scientific Research and Development, recognizing the paucity of research in this area, entered into a contract with the Bell Telephone Laboratories in July 1941 and with Columbia University in March 1942 for the establishment of the Underwater Sound Reference Laboratories—the organization that was to evolve eventually into the Underwater Sound Reference Division of NRL. It was a modest organization of several dozen people with an office in New York and field stations in Mountain Lakes, New Jersey, and Orlando, Florida. Bell Telephone Laboratories was to supply measurement instrumentation and systems. Columbia University was to operate the laboratories, do research on methods, and perform calibration measurements. Thus was born a bureau of standards of underwater sound. Why was it not a part of the National Bureau of Standards to begin with, or even later? The answer is available nowhere in the archives, but an educated guess points to the essentially exclusive military application of the subject.

As in many other World War II crash programs, much was accomplished in a short time. Theory and practice drew heavily on established technology in the telephone and audio engineering areas. The electroacoustic reciprocity principle was firmly established as a means of obtaining a primary calibration. Natural piezoelectric crystals, magnetrostrictive metals, and moving-coil loudspeakers were adapted to underwater sound transducer applications. Hundreds of experiments were conducted to test new theories, techniques, and instruments under the impetus of a rapidly expanding technological and military need.

Although the Underwater Sound Reference Laboratories was the principal organization charged with developing the science and technology of underwater electroacoustic calibration measurements, other organizations contributed to this effort. There were groups at Harvard, the Massachusetts Institute of Technology, the University of California, and, of course, the Naval Research Laboratory. In private industry, there was the General Electric Company, the Submarine Signal Company, Radio Corporation of America, Sangamo Electric, and the Brush Development Company.

By the end of World War II, the state of the art had made great strides forward. But viewed from the perspective of 1973, it was still very crude. We had learned that the ocean is not the simple, stable, homogeneous, quiet, and harmless medium that the layman imagines, and that the effects of temperature, hydrostatic pressure, dissolved salts and gases, marine life, pollutants, bubbles, weather, and intractable boundary conditions are the bane of all who attempt scientific acoustic measurements under water. The technology nurtured in the benign environment of a studio or home could not be applied to the ocean. The ranges of frequency, hydrostatic pressure, temperature, power level, and physical size in sonar transducers far exceeded those in air acoustics, and much remained to be done to accomodate these ranges.

After World War II, the sharp drop in military-connected research and development affected underwater acoustics and sonar calibration work. The Bell Telephone Laboratories and Columbia University both withdrew from the Underwater Sound Reference Laboratories, and the management responsibility was transferred to the Navy. The organization was consolidated at the Orlando site. A massive postwar turnover in personnel, together with the lesser emphasis and support, had a depressing effect on research and development.

It was not until the middle 1950's that the state of the art began to move forward again. Standard hydrophones needed better stability with time, temperature, hydrostatic pressure, and frequency. Wide-band sound sources had none of the "high-fidelity" characteristics important in research and development. The problem of simulating deep ocean depths was particularly troublesome because a thick-walled pressure vessel is so incompatible with the unbounded medium needed for free-field measurements. Some sonar transducers were becoming so gigantic that even small lakes were not big enough for conventional measurement techniques. Little systematic study had been made of the active and passive materials used in underwater acoustics and electroacoustic measurements. Some of the theoretical aspects of calibration methods remained unclear and unstudied. The limitations imposed by cavitation were poorly understood.

These challenges comprised a research and development program that lasted from 1955 through the late 1960's, and to some extent to the present. This program has brought forth new generations of highly stable standard hydrophones and wide-band sound sources. Acoustic transmission line techniques have been developed to circumvent some of the problems with bounded media in high-pressure vessels. Material measurement theory and practice has been added and exploited. The reciprocity calibration theory has been studied and used for all manner of wave shapes and boundary conditions. A whole new concept of near-field measurements has been developed. The conditions and thresholds of cavitation have become better defined.

Research and development in more recent years has turned toward new signal configurations. Acoustic signals now may be noise and impulses as well as the conventional sinusoids. Electrical signals are more often digital than analog. These changes bring with them whole new concepts as to how to deal with signal measurement and analysis.

Some of the older problems remain. There still is no way that the Navy can calibrate an underwater sound transducer at ultrasonic frequencies and at simulated or real depths greater than 1000 meters. A particle-velocity probe sensor eludes us in both theory and design, as does a direct measurement of acoustical power. Thin wide-band anechoic coatings seem beyond reach. A high-powered, wide-band, towable, sound source of reasonable size is a much-needed transducer for research and development measurements at sea—but a practical design concept is not yet at hand.

Future work is expected to focus on the use of modern signal processing and computer technology to obtain calibration data quickly, accurately, reliably, and economically.

In parallel with these many years of research and development has been a steady growth in the Division's calibration and standards services to the Navy. Many of the research and development accomplishments have matured into methods, instruments, systems, and facilities in everyday use. The range of the facilities of the Division and the depth of its expertise in calibration methods and instruments is unmatched. Nowhere else can underwater electroacoustic measurements be made to simulated depths of 7000 meters and in the frequency range 1 to 4000 Hz. Nowhere else can acoustic torpedo transducers be tested close to their design limits of frequency, power, temperature, and hydrostatic pressure. No one else has developed a hydrophone with a sensitivity independent of hydrostatic pressure and temperature and uniform over five decades of frequency.

The Division currently has four major calibration facilities that produce about 6000 pages of calibration, test, and evaluation data and information per year. About 7000 electroacoustic standards are calibrated and loaned to other users every year. These services typically go to about 40 naval activities, 40 naval industrial contractors, and 20 universities and research institutions.

As long as underwater sound is a major tool for undersea detection, navigation, and communication, the Navy will need a focal point for the science and technology of underwater acoustic measurements, and the Underwater Sound Reference Dvision expects to continue to fill this role.

Waves of Transition

W. S. Pellini

Mr. William S. Pellini came to NRL from the Oak Ridge National Laboratory in 1949 to head the Metal Processing Branch of the Metallurgy Division. He has been Superintendent of the Division since 1954, with additional special assignments with the National Academy of Sciences and the Office of Naval Research, London. He is recognized as an international authority in advanced applications of metals for submarines, ships, aircraft, missile systems, and general engineering structures.

My first vivid impression of NRL (1948) was that it had undergone a dramatic transition from a Bureau of Ships establishment to "free agent" of the new Office of Naval Research. We are today involved in transitions of major proportions—the scope and significance of which can only be assessed by looking back, say 10 years from now. Between 1948 and 1973 there have been other notable transitions in the management structure and in external relationships. NRL has never been and, hopefully, will never be involved in a static situation. The extent to which the NRL membership seizes and exploits opportunities for dynamic change will decide the future, as it has decided the past.

The invitation to provide a personal documentary perspective is welcomed—not for reasons of citing fond remembrances of things past (I leave this to historians) but for reasons that bear on the present and the future. I find that changes that are agonizing to specific groups were very much part of the picture in the late 1940's as they are in the early 1970's.

Today there is concern that changes to emphasize directed research may lead to wiping out our research and technology base. In the late 1940's there was severe concern that the new role of NRL would result in cutting life-lines with the Bureaus, and the result would be loss of true value to the Navy. In brief, the concern was that NRL was headed for a research-oriented disaster, loss of contact with the real Navy, benign neglect, isolation, deterioration, and eventual disappearance as a useful entity. This was not the case, as events proved.

These arguments (pro and con) were most evident to me, as I proceeded to reorganize and direct the operations of two Branches (Welding and Casting) that were joined to form the new Metal Processing Branch. The membership of these two groups had known only the life of *directly assigned research*, *i.e.*, tasks by the parent Bureau of Ships engineering Codes.

Dr. O. T. Marzke, the Superintendent, gave me explicit instructions—wipe out the task-oriented problem structure and establish a completely new research-oriented operation. He hoped that this job could be done within a year. Moreover, he stated that 100-percent ONR funding was available and that I should not be concerned with problems of external funding.

The assignment criteria were much to my liking with certain exceptions:

1. Since the objectives were to demonstrate that the best of research was essential for ensuring advances in engineering practices, a strong connection with the "users" had to be established. In brief,

missionary effort for educating backward engineering groups required direct contact; publications were not enough, there must be participation. The best route to participation was to obtain partial financial support so that "they" would be using the product of their supported (endorsed) efforts. Accordingly, I proceeded to "sell stock" in the new work while retaining full control of the operation–100-percent ONR funding was rejected.

2. The time of transition to a new operating mode of 1 year was too long–the job was done in 3 months. The reasons were partially personal, I had come to NRL with the intention to stay 5 years and then move on. At the time I viewed NRL primarily as a good place to launch my career, and time was of essence.

Mr. H. Bishop, who had been the nominal Head of the Casting Group, warned me that "many came to NRL with a 5-year plan but ended up staying permanently." A modern management consultant would "flip" on this one, because the implications could be (erroneously) that it was a retirement haven. Conversely, the implications were that NRL provided unmatched opportunities and that any other place would be unappealing by comparison. Bishop was right, and I found myself turning down enticing offers for other employment, because of the excitement of NRL work and association with critically important military projects.

Almost exactly 5 years after I joined NRL as a GS-12, Dr. Marzke called a Branch Head meeting and announced that a major reorganization of NRL had been placed in effect. He was to become the first Associate Director of Research for Materials, and Dr. Hulburt was to become the first Director of Research. Prior to this, the Superintendents reported directly to the Military Director of the Laboratory. Other aspects of major transition involved the establishment of the Solid State Division, largely by reduction of the Metallurgy Division to skeletal force. Other, new divisions were created at the same time.

In the course of the meeting, I decided that the time had come to leave–my 5 years were up. Moreover, my own Branch was the only large group (26) left in the Division. Two other smaller Branches in rather poor state were left–plus, fortunately, the superb Analytical Chemistry Branch, headed by Mr. Dean Walter.

At the end of the meeting Marzke said he had one more announcement to make–he turned to me and said "as of this moment you are now Superintendent; start working to rebuild the Division." Civil Service Commission Rules and Selection Board procedures were much simpler in those days!

My immediate response was "you did not ask me" and "please describe the guidelines–what is expected?" The guidelines were simple–all of the ceiling points required were available, and no funding restrictions were to be applied. Thus, in exactly 5 minutes it was decided, and the instructions were accepted along with commission. In retrospect, much of this is unbelievable in the context of present-day formalities–for all factors involved; however, the year was 1954.

My first formal action was to inform Dean Walter that he should no longer consider the Analytical Chemistry Branch as purely a "service organization" but as a research group as well. NRL research problems were immediately established to fund research and as a base for publication activity. I had developed a strong respect for the Branch, because of their effective support provided to the Metal Processing Branch, which contributed directly to the metals research effort.

The wave of transition that swept over the Laboratory in 1954, in turn led to waves of transition within the Metallurgy Division. It was my good fortune to recruit Dr. B. F. Brown to Head the new Physical Metallurgy Branch; Dr. E. Salkovitz to Head the new Metal Physics Branch; Mr. L. E. Steele to Head the new Reactor Materials Branch; and Mr. R. J. Goode to Head the Metal Processing Branch under the new name of the Strength of Metals Branch. Later, Dr. A. I. Schindler (who was a charter member of the Metal Physics Branch) assumed charge, as Dr. E. Salkovitz resigned to join the ONR staff. Another member of Dr. Salkovitz's group, Dr. M. E. Glicksman, emerged as the Branch Head of the Transformations and Kinetics Branch–thus completing the present Branch structure.

It is impossible to cite all of the other superb investigators that were recruited by the above-named Branch Heads. The level of accomplishment and international reputation of the cited Branch Heads–speaks volumes for the quality of their associates.

The most difficult problem during these early formative years (1954 to roughly 1964) was that of competing in recruitment with industry and universities on a salary basis. The new talent was attracted—not by matching offers but by overmatching in terms of opportunities to evolve scientific and/or technological careers. In these respects the enticement was "you cannot do better than at NRL."

I have fond recollections of Dr. B. F. Brown's terms for joining—"two SP assistants, a small Laboratory, no management duties, no Section or Branch responsibilities." I agreed, but he did not know that I had any fingers crossed at the time!

Documentary perspectives could go on and on—it is best to close on a theme that the cited aspects imply—

> NRL was, is, and will be shaped by people who have highly personal reasons for joining the Establishment. When these reasons are examined and found to fit the needs of the Laboratory (scientific, technical and management), then the selections are proper. The selection mix that served in years past changes with waves of transition. Thus, it is realistic to expect that change was and change will be the order of the day, if a static posture is to be avoided and challenges accepted.

Strange Particles and Exploding Stars
A Chronicle of High-Energy Physics*

M. M. Shapiro

After a wide experience in teaching and research, Dr. Maurice M. Shapiro came to NRL in 1949 from Oak Ridge National Laboratory and served 12 years as Superintendent of the Nuclear Physics Division. In recognition of his prestige in the world community of scientific leaders he was appointed to a Chair of Cosmic Ray Physics in 1966. He is currently Chief Scientist of the Laboratory for Cosmic Ray Physics.

In 1947 pi mesons (pions)—the heavy quanta postulated by Yukawa to serve as a "nuclear glue"—were discovered among the secondary cosmic rays (1) and were soon produced at the Berkeley cyclotron. These events marked a turning point in physics. Within a decade, the number of so-called elementary particles was to grow from seven to thirty.

The discovery of the long-sought pions rekindled my interest in cosmic rays. At the time, I was working in Oak Ridge measuring the megabarn cross section of xenon-135, the notorious pile poison, as a function of energy (2). It was essential to map the neutron-capture resonance of this prodigious fission product in order to design nuclear-powered reactors for submarines. Captain Rickover† was just initiating this effort in Oak Ridge, and some of his young naval officers were attending my class in nuclear and reactor physics. What I learned about power reactors while teaching this course was soon to bear unexpected fruit at NRL.

A year later, heavy nuclei—from helium to iron—were found among the "primary" cosmic rays near the top of the atmosphere (3). This exciting discovery, coming soon after the evidence for pions, fired my imagination. Apparently samples of matter—in the form of relativistic atomic nuclei—were reaching us from beyond the solar system. These heavy nuclei, as well as the pions, had been discovered by using photographic emulsion detectors. At the University of Chicago between 1938 and 1941 I had been the only U.S. physicist applying this technique to cosmic-ray studies. Despite some skepticism, Arthur Compton had allowed me to employ this method for investigation of cosmic-ray "stars," *i.e.*, high-energy disintegrations of nuclei. As a graduate student, I had even begun to try "stripped" emulsions, which were to culminate in effective detectors a decade later at NRL.

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*And other nuclear research, mostly in the 1950's at NRL.

†Now Vice Admiral. Rickover appreciated our research on xenon-135, and offered me—well before the reactor was even designed—a ride on the first voyage of a nuclear-powered submarine. I answered "Thank you very much; I'll take a ride on the *second*."

When Franz Kurie invited me to join the recently established Nucleonics Division, the opportunity to start a new program in high-energy physics and cosmic rays proved irrestible, and I arrived at NRL early in 1949.* Kurie understood the growing importance of high-energy physics and elementary particle research. He believed that the Navy should acquire real expertise in these disciplines by actively exploring the promising terrain.

Kurie's dream of building a billion-volt machine did not materialize, but nature's accelerator, the cosmic radiation, was available. Moreover, a simple and inexpensive detector, the electron-sensitive photographic emulsion, was revealing new, strange particles, and opening up broad avenues of research. Bertram Stiller, Hank O'Dell, and I developed apparatus for processing thick emulsions and began to equip a laboratory for track microscopy. We also launched into the stratosphere our first detectors suspended from the large ONR Skyhook balloons (those flown today are much larger; see Figure 1).

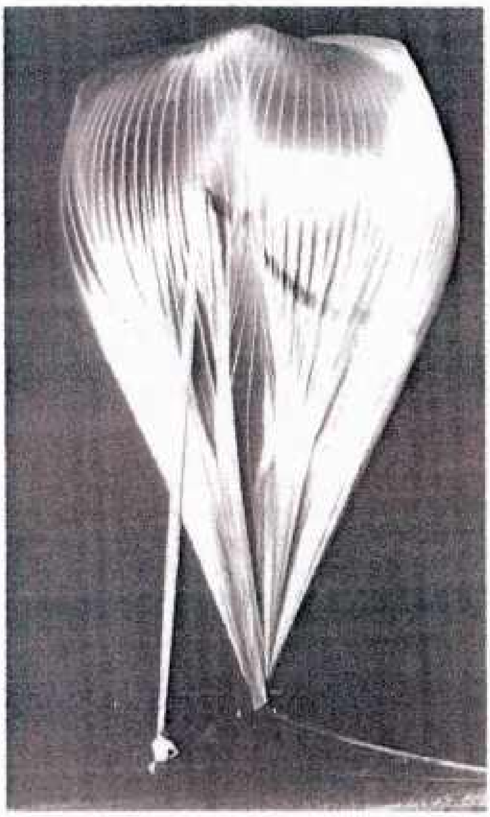

Figure 1 – A ten-million cubic foot plastic balloon ready for launching. Even since the advent of satellites, most of the new information on the primary cosmic rays has come from experiments carried out "near the top of the atmosphere" with balloon-borne equipment. Altitudes commonly achieved today are close to 140,000 feet; the residual atmospheric pressure is less than 0.3 percent of that at sea level. (Photograph by N. Seeman.)

This work was interrupted by a chance meeting with Edward Teller, who had conceived the idea of a low-enrichment fission reactor for submarines. Teller showed me his rough calculations as to feasibility and urged me to undertake the nuclear design of such a reactor; I completed the task in 10 weeks. Karl Cohen and a group of engineers at Kellex checked my calculations and produced an engineering design. Though

*Though I founded the Laboratory for Cosmic-Ray Physics (or rather its precursor, the Cosmic-Ray Branch) in that year, prior work in this field at NRL had been done by Gilbert Perlow, Ernst Krause, Leo Davis, and others. Constraints of space, and the nature of this Anniversary publication, have made the present narrative a rather personal account of research about which I have first-hand knowledge.

this type of reactor turned out to be inconveniently large for submarines, it is my understanding that similar reactors were later installed in larger ships, and in the power station at Shippingport as well.

Returning to our cosmic-ray research, we encountered two obstacles that hampered the exploitation of photographic detectors. One impediment, a gap in our knowledge of the behavior of relativistic particles while penetrating a solid medium, limited the *quantitative* use of track microscopy. The other constraint cramped the style of the nuclear emulsion as an *exploratory* device.

For particles whose total energy greatly exceeds their rest mass, the rate of ionization loss in a solid medium should at first increase with energy, but then level off to a "plateau," according to a theory of Fermi that went beyond the Bethe-Bloch formulation. Several experimenters who sought to verify this theory failed to detect the "relativistic rise" in ionization. Others found evidence for a rise, but its magnitude seemed to conflict with theory. This uncertainty had to be resolved if the rate of energy loss by ionization was to serve as a diagnostic tool for particles moving nearly as fast as light. Stiller and I measured the magnitude of the rise in a novel way and then extended and confirmed the result (4), showing that a plateau existed at the theoretically predicted level. (It was necessary to take into account that the measured density of silver grains in emulsion reflected a "restricted" rate of ionization loss rather than the total rate of loss.) The Fermi-Halpern-Hall theory was thus verified; at the same time, the rate of ionization loss in solid detectors was established as a discriminating tool for identification and energy estimation of ultra-high-energy particles.

The second problem, more prosaic, was nevertheless serious. The sensitive volume of an emulsion layer, normally coated on a glass plate, was severely limited by the working distance of microscope objectives at high magnification, as well as by problems that arose in chemically processing thick emulsions. One could increase the effective volume by using a stack of such plates, through which some energetic particles could penetrate, enabling the microscopist ultimately to follow the successive segments of their tracks despite interruptions in glass. This configuration suffered from large invisible gaps in the record which offset certain advantages of the photographic method.

A major attraction of a visual detector (*e.g.*, cloud chamber, emulsion, or the then nascent bubble chamber) in particle research is its suitability for *exploration* which often depends on phenomenological observations. It is desirable first to *see* an "event" as completely as possible; in the ideal detector, it should then be thoroughly measurable as well. Wanted was a *stack of pellicles* ("stripped" emulsion) forming a large volume sensitive throughout, in which an event could be fully visible, with all its secondaries, tertiaries, *etc.*, traceable through contiguous layers. However, when such pellicles were processed "free," their degree of swelling and final dimensions were difficult to control. The problem was to find a scheme for darkroom processing of thick pellicles without inducing the irreproducible swelling and distortion that were discouraging their use. Hank O'Dell, Bert Stiller, and I devised a method for mounting the pellicle layers on glass *after* exposure, but before chemical processing (5). Once this technique worked successfully, pellicle stacks became standard and powerful tools in many cosmic-ray experiments and at all the multi-GeV accelerators, enriching high-energy physics.

When the USSR detonated its first fission bomb in 1950, Peter King, Herbert Friedman, and Luther Lockhart employed radiochemical techniques to identify the nuclear explosive. Their analysis of a precipitate from rain showed the presence of plutonium. Kurie told me that other air samples had been collected after the Russian test; he thought it would be useful to have an independent check. Impregnating some spare Ilford emulsions with bits of a sample, we measured tracks of emitted alpha particles, and confirmed the identification of the bomb material.

In the early 1950's the NRL group became the first U.S. laboratory to conduct systematic investigations with nuclear emulsions of the newly discovered "strange particles." We reported on the masses of K mesons and certain of their modes of decay to the Second International Conference on High Energy Physics in Rochester in 1952. In those days it was still necessary to find these esoteric particles laboriously among the cosmic rays. Further investigation of the new mesons was carried out in a stack exposed high above the Galapagos Islands.

In the same detector stack, we made the first reliable measurements of the intensity of primary cosmic-ray helium near the geomagnetic equator. This was important in establishing the latitude-sensitive energy spectrum of the primary helium component. Additionally, it was possible to pin down the abundance of cosmic-ray helium relative to that of primary hydrogen at the top of the atmosphere (6).

In 1953 we helped establish the existence of the charged sigma hyperon—an unstable particle intermediate in mass between proton and deuteron. NRL was one of the first laboratories in the world to find evidence for this new form of nuclear matter—a charged, highly "excited nucleon" (7).

A year later the Berkeley bevatron came into full-scale operation, and a new era dawned in the field of "strange particle" physics. Controlled beams of K mesons became available, and detailed studies of the properties of these particles and their secondaries were begun. The NRL group investigated the interactions of K^- mesons with free protons in nuclear emulsion; they studied the cross sections for these fundamental interactions, and they observed some of the earliest examples of $\overline{K^0}$ mesons. They also measured the lifetimes and other properties of charged sigma hyperons (8).

One of the most celebrated controversies in the field of cosmic radiation, ever since the discovery of "heavy primaries," was the question as to whether the relative abundances of the light and heavy elements in the cosmic rays reflect the general abundances of these elements in the universe. In particular, the general abundance in the universe of lithium, beryllium, and boron is known to be exceedingly low—a few parts in a billion of hydrogen. If these three light elements were indeed present in the "primary" radiation arriving at the top of the atmosphere, this would be highly significant. It would indicate either the existence of very anomalous cosmic-ray sources or, more probably, that in traveling through galactic space, the heavier nuclei had collided with interstellar matter and yielded Li, Be, and B among their fragmentation products.

After a decade of conflicting measurements by several laboratories, we performed a definitive experiment which established the relative abundance of these light elements in the primary cosmic radiation (9). The ratio of the three light elements to the prominent C-N-O group was found to be ~25 percent, and the cosmic-ray abundance of these elements relative to their general abundance was shown to be about 10^4 times higher than the corresponding ratio for carbon. This result yielded the first good estimates of the average path length of cosmic rays, *i.e.*, the amount of material they traverse while they are trapped by the magnetic fields of the galaxy.

In the summer of 1953, when I returned from an international cosmic-ray meeting, I learned that Dr. Kurie had accepted the post of Technical Director of the Navy Electronics Laboratory in San Diego; he recommended me as his successor. I had lively misgivings, and when the Laboratory carried out its customary search, I helped assiduously. A couple of months later, Dr. Hulburt asked me to become Superintendent of the Nucleonics Division. Reluctantly, I told him I would soon be (scientifically) moribund unless I devoted half of my time to research. Hulburt settled the matter by saying, "I'd be disappointed if you acted otherwise."*

We were fortunate in attracting able and dedicated scientists to the Laboratory, and I found satisfaction in helping to support and encourage them. Many of these come to mind—Bob Jastrow, George Snow, Bob Glasser, John McElhinney, Freddie Herz, Martin Block, Paul Kellogg, Walter Wada, Harry Holmgren. Some of these men have moved on to university professorships; others occupy prominent posts in national laboratories.

Meanwhile, with the development of multi-GeV accelerators, many cosmic-ray physicists who had been concentrating on elementary particles shifted their theater of operations to the big machines. Others, more interested in the astrophysical aspects of the cosmic radiation *per se*, focused their studies on the composition, energy spectra, and modulation of cosmic rays, and related phenomena. Some of us were particularly stimulated by the brilliant insights of Vitale Ginzburg and Joseph Shklovsky in the Soviet Union, who called attention to the striking features of supernova remnants, and brought them to the fore as plausible

*If I succeeded as Division head, it was due largely to the help of Warren Mutch, John McElhinney, and the late Clifford Armhold. In 1965, after 12 years in this post, I resigned in order to concentrate more fully on cosmic-ray astrophysics. Soon thereafter, the Laboratory established a Chair for me.

cosmic-ray sources (10). Fermi had put forward his celebrated statistical mechanism of acceleration, and Forbush and Simpson had demonstrated the importance of temporal variations.

Our NRL group, leaning toward cosmic-ray astrophysics, was loath to abandon the other major discipline in high-energy physics—elementary particles. Whereas bubble chambers were yet in their infancy, the nuclear emulsion technique had come into its own as a versatile tool in elementary-particle physics. It was being employed at all of the world's GeV accelerators.* This situation changed rapidly, as Alvarez and others put the hydrogen bubble chamber on a firm footing. Nathan Seeman, Bob Glasser, and George Snow, after their productive work with nuclear emulsions, shifted to the analysis of bubble-chamber film obtained in accelerator experiments.

However, the emulsion technique remained of great value in research with accelerators, as exemplified by the remarkable measurement of the lifetime of the neutral pion (π^0), one of the three types of pion generated in violent collisions between nucleons. Charged pions live only about 10^{-8} seconds. The lifetime of the π^0 is a million times shorter—so short that even at a speed close to that of light, the distance it traverses from birth to death is a fraction of a micron. Its direct measurement involved a stretching of the most refined microscopic techniques. The mean lifetime of the π^0 is an important physical constant because of its bearing on the nature of nuclear forces. Various attempts had been made to measure it, but these had yielded very approximate estimates, or at best upper limits. Glasser, Seeman, and Stiller (12) made the first good determination of its value, and their report was a highlight of the Rochester Conference on High Energy Physics in September, 1960.

In the decade of the sixties we were gradually to disengage from elementary particle physics† in favor of high-energy astrophysics. It had become increasingly apparent that the cosmic radiation is a ubiquitous phenomenon having universal ramifications. The riddle of cosmic-ray origin remained a central problem, stimulated by discoveries in x-ray and radio astronomy. From preoccupation with strange particles, cosmic-ray physics had shifted to fascination with strange stars. Hypothetical cosmic-ray sources, however, would still have to be tested by the observed and inferred properties of the radiation, *e.g.*, its composition, energy spectra, propagation, and transformations in space. These and related problems commanded most of our attention in the next decade.

To mention only a few highlights of our work in the sixties, we studied the isotopic composition of helium, the interactions of cosmic rays with the interstellar medium, the path length distribution of cosmic rays, their "age," their source composition, and some aspects of the theory of solar modulation. We also explored that elusive but important component—the cosmic gamma radiation—in an energy domain (15 to 150 MeV) largely by-passed by others. To describe even briefly the significance of these diverse investigations would require a separate essay.

Before concluding this chronicle of the fifties, I am tempted to leap over into the 1970's and recount how heavy cosmic rays were brought down to earth. Just as relativistic hydrogen nuclei had been generated in proton synchrotrons, so heavier ions (carbon, oxygen, *etc.*), were accelerated to characteristic cosmic-ray energies (~GeV/amu—a hundred times higher than before) starting in 1971. This was achieved at the Princeton Particle Accelerator and at the Berkeley Bevatron. As soon as these beams were available, we and other physicists embarked on a program of measuring the cross sections for various modes of fragmentation of the heavy ions (see Figure 2).

Biophysicists also began to use these beams, while cancer specialists started to explore their possibilities for tumor therapy. The extensive calculations by Rein Silberberg, Chen-Hsiang Tsao, and the author

*An interesting comparision between a "mature" technique and an incipient one can be found in two reviews appearing contiguously in the "Handbuch de Physik," one on bubble chambers, the other on nuclear emulsions (11). These reviews were prepared *ca.* 1957.

†Our swan song was accompanied by the lusty birth cries of the high-energy physics group at the University of Maryland, with whom we collaborated (*e.g.*, in discovering a new decay mode of the famed omega hyperon). George Snow became leader of this group, and Bob Glasser joined it subsequently.

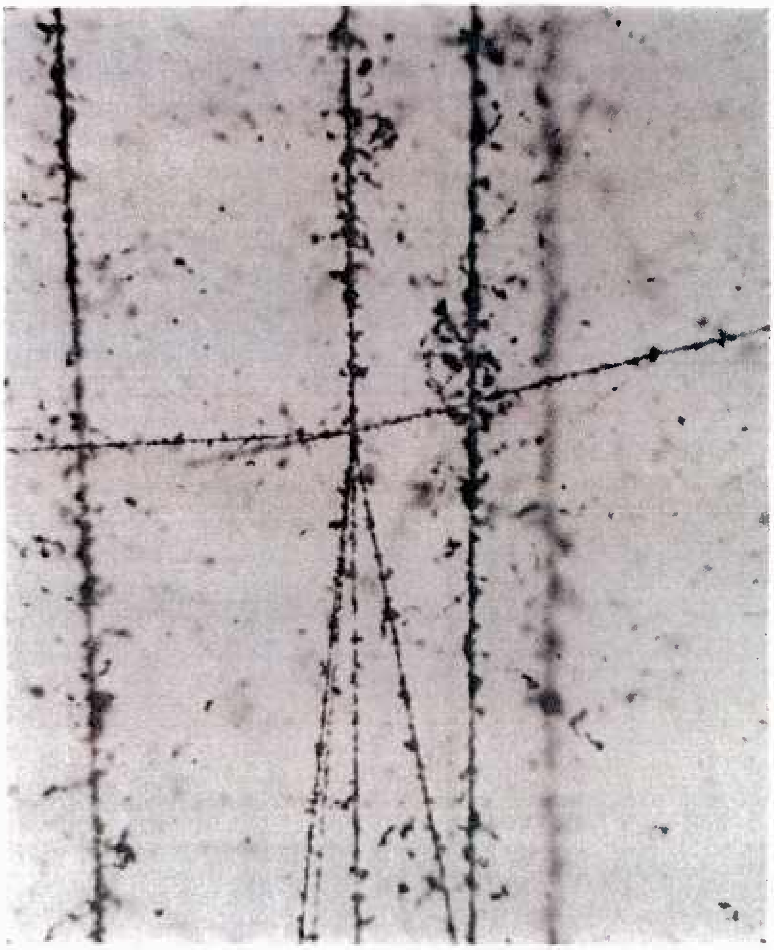

Figure 2 – A high-energy interaction of a nitrogen nucleus in photographic emulsion. The nitrogen ion from the Princeton Accelerator enters the photographic layer, leaving a dense track in its wake. It collides with a "target" nucleus, and breaks up into several lighter fragments. These go forward in the same general direction as the incident nitrogen ion. Two additional nitrogen tracks are also seen.

(13,14) on cosmic-ray transformations–particularly on the breakup of fast, heavy nuclei–proved to be of great interest and utility not only in astrophysics, lunar research, and planetary sciences, but also in the life sciences. If anyone had suggested to me, when heavy cosmic-ray nuclei were discovered, that laboratory beams of such ions would find a practical use within a couple of decades, I would have been skeptical. This confession brings me back full circle to those ephemeral particles–pions–with which I opened this narrative.

In this year of NRL's Golden Anniversary, intense beams of pions are being generated in the new Linac at Los Alamos–the so-called meson factory–and a medical research laboratory at the site is ready to probe the efficacy of pion beams in the treatment of cancer. These are the same particles discovered just 25 years ago in the cosmic-rays–a field of research which might (in today's parlance) have been dismissed as "irrelevant."

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Through a Glass Lightly

C. C. Klick

Dr. Clifford C. Klick received his Sc.D. in physics from Carnegie Institute of Technology and joined NRL in 1949. He later became Head of the Luminescent Materials Section and the Optical Materials Branch. His research is mainly concerned with luminescent and color centers, and radiation damage in solids. Since 1968 Dr. Klick has been Superintendent of the Solid State Division.

The 50 years of NRL's history can be thought of in two parts: the time from the end of World War I to the end of World War II is one part, roughly 25 years in length, and the time after World War II to the present covers the other 25 years. My own period at NRL covers this second half of the Laboratory's history almost exactly. Looking back now to those early days is like looking back to childhood scenes. The remembered days are all golden and fair. Laughter rang through the halls, it seems. And there was a bucolic simplicity of life and an ingenuous unity of commitments in both America and NRL that appear very different from our own sophisticated but uncertain times.

This view of NRL's history is tempered though by thinking about what some other senior person might have written in 1948 on looking back over the first 25 years of NRL's history. He, too, would have relished the early days, the handful of people, the simplicity of the gear and the easy way things were done. By contrast, a laboratory swollen in World War II to around 2000 people with no fixed purpose at war's end and sprawling over a dozen divisions must have seemed chaotic and cold. Just those very times in 1948 seem to me now to have been exciting, gay, and carefree. So perhaps the changes one notes are as much in the eye of the beholder as they are in honest fact.

The Branch that I joined on coming to NRL was presided over by Paul Egli. The section Heads were eager, brash, and occasionally combative. Those characteristics served them well. One, Eli Burstein, is a professor at the University of Pennsylvania. Another, Jim Schulman, is Deputy Director of Research at NRL. We worked in a big room with dividers to mark off offices. These dividers kept out light, air in the summers, but not sound. "Conferences" between Mel Lax—a summer visitor now at Bell Labs and Burstein were frequent, intense, and at full voice as each used every weapon he had to convince the other. None of your sweet reason there!

Early in the game we decided to try looking at some of our luminescent materials at very low temperatures. So we had a couple of nested glass dewars made in the glass shop and learned a little of the magical lore from the low-temperature types who seemed to be all over what is now building 60. Finally the day came and we got some liquid helium of our very own. It wasn't too easy to know if you really had the stuff, and we soon became expert at using a flashlight to illuminate the shimmering surface of the helium. I always found that a thrill and probably I still would today if everyone had not changed to metal dewars. In fact one of our treats for visitors was to help them see liquid helium for the first time!

Those early years held other visual delights too. Peering at luminescent materials excited by an ultraviolet lamp in a darkened room has a bit of magic to it. All that light and no heat! It may be that we have lost some of the sensuous satisfaction of experimentation by developing elaborate machines. We no longer see or smell or hear or feel the experiment itself—only the paper tape running out of the instrument. That's a pity. Several of my own most exhilarating moments (excepting social life) were to see a glow arise where and when predicted by a model that we had just invented the day before. Or the extended delight of an afternoon spent taking 60 points on a transmission curve that came out well while using a Beckman DU spectrophotometer—seven manual operations per point. One ended exhausted but the experiment had received a long hard period of thought.

So there is some of the picture as I recall it. It is not true to life but perhaps something like a Grandma Moses farm scene—all bright colors and seen from a great distance with no hint of flies or odors or sweat!

Variety and Challenge

H. Rabin

Dr. Herbert Rabin, Associate Director of Space Science and Technology, came to NRL in 1952 from the University of Illinois. His major scientific accomplishments have been in the fields of high-energy gamma ray and electron facilities, radiation dosimetry, solid state lattice defects, and nonlinear optics and laser physics. He has served as a visiting scientist at the Techniche Hochschule in Stuttgart, Germany and as a consultant to the school of Engineering of the University of Sao Paulo, Sao Carlos, Brazil. He was appointed to his present position in January 1972.

In pausing to look back over a career at the Naval Research Laboratory, spanning a period of 21 years, I am taken aback by the fact that this much time has elapsed. An association of 21 years provides innumerable opportunities for boredom and disinterest. Fortunately, two of the most powerful preventatives to these debilitating states of the human spirit have been ever present during my association with NRL. I refer specifically to variety and challenge. In simplest terms, there has been too much going on at NRL to get bored, and too many challenges to become disinterested. NRL has all the features of a dynamic, multifaceted society, with extensive opportunity for participation in a variety of ways. Let me briefly share a few reflections on my career experience as the framework for elucidating this point.

I appeared on the scene in 1952 with a Master's Degree in physics and had three opportunities for employment. These were in the areas of space science, mechanics, and radiation effects on materials, and I found it quite difficult to make up my mind. Wayne Hall interviewed me for the latter position in his role as Superintendent of the Electricity Division, the forerunner of our present Solid State Division. I believe Dr. Hall's kindly and convincing manner had much to do with my turning to radiation damage studies. This was the period when there was considerable interest in the behavior and survivability of materials in intense radiation fields, flowing from reactor and nuclear weapon developments.

Under Emanuel Brancato, my immediate supervisor, and Arthur McClinton, my Branch Head, I was asked to put together a radiation facility for studying insulating solids. We chose to emphasize ionizing effects, since electronic excitation and ionization appeared to be the predominant mode of damage to insulators. We proceeded to put together a rather sizeable radiation facility, consisting of a 2-MeV electron Van de Graaff and a 5000-Curie source of cobalt-60. The latter represented one of the hottest radiation sources at the time, and there was much interest in it. We exercised our imaginations fully in building in all the features that were to be needed in our subsequent studies. I recall also spending a fair amount of time considering the question of potential radiation hazards to personnel working with the facility. After about 2 years the facility was fully in operation, and our studies were under way. I recall in retrospect that I was very much impressed that the Laboratory was willing to give one of its junior members considerable latitude and responsibility in a fairly major undertaking. I felt very fortunate indeed.

It was at about this time the Laboratory underwent a major reorganization, and I was transferred into the newly formed Solid State Division. Dr. Hall was the Superintendent, and I was a member of the

Dielectrics Branch under James Schulman. I was asked to join Clifford Klick's Section and was offered the possibility of participating in a variety of new research. First, it was the area of solid state dosimetry—using fundamental property changes in solids to monitor radiation fields; later it was color center research—the study of solids on an atomic level to increase our understanding of defect structure, particularly as induced by a radiation environment.

In the dosimetry area I simply rode the crest of a vast ground swell of activity that had been initiated some years earlier. Dr. Schulman and his colleagues already were internationally known for their extensive and imaginative research work in this field, and for their important contributions to the Navy in developing simple, reliable, and inexpensive personnel dosimeters. As a junior participant I was able to learn a new field of research and contribute to several of the studies in progress at the time. This was a richly rewarding experience in many respects, but especially from the viewpoint of observing a most healthy and productive interplay of basic and applied research by individuals within the same group. Phenomena occurring in fundamental studies of solids such as radiophotoluminescence, thermoluminescence, photoconductivity, and absorption effects were always considered as potential candidates for translation into practical devices and uses.

Before long my interests were also stimulated by the associated ongoing effort I mentioned earlier in color center research, and I was also encouraged to participate in this area. In this regard, both Dr. Schulman and Dr. Klick had a most profound effect in developing these research interests, and I have been extremely grateful to both of them for their patience and perseverance in the multitudinous ways they helped an inexperienced scientist learn a new field. I should also mention their urging that I continue to pursue further graduate study under the NRL science education program was instrumental subsequently in my completion of a Ph.D. degree. In this connection I have also been most appreciative to Jules de Launay, former Superintendent of the Solid State Division, for many helpful assists as the NRL coordinator of this program with the University of Maryland. The fact that I was able to further my education exemplifies another aspect of the broad range of opportunity available at NRL and provided yet another dimension to the NRL career experience. I recall a very stimulating environment of a rather sizeable group of individuals doing research at NRL and taking University of Maryland courses during the mid-50's. Many of my colleagues similarly benefited from the forward-looking NRL educational policy.

Returning to the color center field, I was able to spend more than a decade doing basic solid state studies in perhaps the best atmosphere that one could ask for. The group was competitive with the leading groups elsewhere in the world, and we had the chance to participate broadly in this community. My associations with Dale Compton, Howard Etzel, and John Lambe during this period were especially enjoyable. They contributed much to a dynamic and intellectually stimulating environment. At the same time, there was always room for an expression of humor and a healthy laugh.

There were so many times we shared the opportunity to participate in the process of discovery that I really cannot begin to relate them all here. Each was an exciting moment in the research experience, and we all were highly motivated to the constant chase to follow the last of such experiences with new ones. Dr. Klick and I were extremely pleased that nature provided a consistent picture of the x-ray coloration process of alkali halides at low and higher temperature (helium *vs* room temperature). Dr. Schulman and I were able to open up a range of new coloration phenomena in the cesium-halide-type alkali halides which previously had not been explored in any depth. There were other highly rewarding research experiences associated with expanding the knowledge of specific defect structures. In this regard work on the M center with Dale Compton and Bruce Faraday, and the ionized aggregate centers with Irwin Schneider are especially remembered as being most stimulating.

During the early and mid-1960's I observed from the sidelines the new and exciting work ongoing in laser-related research. I was given the chance to initiate a new research effort in the field of nonlinear optics at NRL, and in retrospect I am most pleased to have participated in this highly interesting field.

Nonlinear optics is concerned with the behavior of laser radiation in propagating through matter. Briefly speaking, intense laser radiation alters the medium properties to the extent that the resultant physical phenomena are not strictly proportional to light intensity, but rather behave in a more complex way associated with the altered medium properties induced by laser radiation. These interactions are of interest in their own right, but more importantly they allow one to manipulate light in unique ways.

The research program in nonlinear optics was started in the Solid State Division, and later continued and expanded in the Optical Sciences Division. The initial program was started by Paul Bey, Jack Giuliani, and myself, and over roughly a 5-year time span many memorable research experiences were shared among us. A number of experimental and theoretical studies were conducted. We were able to study nonlinear processes in media with rotatory and anomalous dispersion, and we provided some of the initial results of this type. We also had success in optical third-harmonic studies, and the particular properties of circularly polarized light in third-order processes. During this period I also shared vicariously in the research successes of many of my other colleagues and associates, and I have a very warm feeling relating to these associations. Unfortunately, there are far too many names to mention.

Over the last several years I have had the opportunity to interact over a vast new subject area, that of space science and technology. Although my duties have been largely administrative, I have also had the chance to learn a considerable amount of new technical material as part of this assignment. Furthermore, I have valued the opportunity to interact directly with many additional people at NRL. The experience has been most rewarding. In particular working with such highly capable people as Alan Berman, the Director of Research, and my colleagues John Allen, Ralph Goodman, and James Schulman, has been most exhilarating and rewarding.

Space-related research offers many opportunities for the Navy, and NRL's unique role in the space program puts this Laboratory in a central position with respect to these challenges. Our interests cover the broad expanse from fundamental science to practical engineering in support of the Navy space mission. This includes the major areas of communications, environmental monitoring, navigation, and surveillance and brings to the fore an appreciable part of the total spectrum of modern science and technology in support of these pursuits. One could not ask for a more stimulating activity in which to participate.

In bringing this account up to date in this golden anniversary year of NRL, I really see no reason why the great variety and challenge that this Laboratory has offered in the past will be in any way diminished in the future. There is much that still needs to be done, and I have a great deal of confidence that NRL will play a major role in doing it. If experience is any guide, those who choose to participate can have a most exciting time.

The Mid-60's at NRL

T. B. Owen

Admiral Thomas B. Owen served the NRL-ONR community in three major administrative capacities, beginning as NRL's Director of Support Services in 1963. He became Director of the Laboratory upon the retirement of Captain Bradley F. Bennett in 1965 and Chief of Naval Research following the retirement of the late Admiral John K. Leydon. Admiral Owen saw combat action in the Pacific Theater during World War II. From that time until his coming to NRL he had held major assignments associated with the Navy's R&D programs. After receiving a Ph.D. in Chemistry from Cornell University, he completed a year of postdoctoral work at the University of Amsterdam, Netherlands. He retired as CNR in 1970. Dr. Owen is now with the National Science Foundation, where he is Assistant Director for National and International Programs.

Upon reporting to NRL, I found that one of my first responsibilities was to assist in preparation for and to participate in the Laboratory's 40th Anniversary. It was a hot, muggy day in July when Vice Admiral C. B. Martell spoke at the celebration and unveiled the plaque that was ultimately to be mounted in the new electronics building that had just been approved by the Congress. The principal speaker at the Anniversary was Assistant Secretary of the Navy for Research and Development, Dr. James Wakelin. During his address Dr. Wakelin emphasized that the Naval Research Laboratory was the Navy's "corporate" laboratory for research. This designation as the central research laboratory of the Navy has been and continues to be important. Later in the day, I escorted Mrs. Wakelin on a visit of the Laboratory. The highlight of our tour was the Linear Accelerator, which was in the final stages of completion.

Late 1963 and most of 1964 were what I would call learning years. As Director of Support Services it was my responsibility to become familiar with and to manage the activities of the very important support elements of the Laboratory. On bicycle and on foot I constantly toured the support activities and came to know many fine people. It was always a pleasure to roam through the shops to see the fabrication of complicated scientific equipment; to go through the library and note the wonderful collection; to watch the Public Works Division keep the plant in good shape; and to visit and talk with those involved in other support activities as well. It was obvious that all were concerned with the future of the organization and were working together in order to advance the capabilities and reputation of the Laboratory in every way possible.

In 1964, the Laboratory became heavily involved in labor-management relationships. A bargaining unit was formed by the Washington Area Metal Trades Council, and after an election as to representation the Laboratory and the Council negotiated a labor-management contract under the terms of Executive Order 10988. Also in 1964, Captain Brad Bennett arranged that I attend the Advanced Management Program at Harvard Graduate School of Business. I left the Laboratory in September and returned in mid-December. Throughout my stay at Harvard I was kept in contact with activities at NRL by weekly mail sent to me by Sam Cohen of the Management Office. Early in 1965, after my period of "training" and with the strong support of Captain Bennett, I relieved him as Director.

In view of my now broadened responsibilities, I made every effort to familiarize myself with the work going on in the research area. I talked with many Division Superintendents with regard to the work going on in their particular domain. Memories do fade, but in some elements of the Laboratory's work I found myself particularly interested. There was the SPASUR satellite tracking system that had been developed by Claud Cleeton and later managed by Roger Easton. It was during this time that the Laboratory acquired extra land in Texas to expand the capabilities of the system. The work in surface chemistry done by Bill Zisman and Elaine Shafrin, along with that related to submarine habitability, was extremely important to the Navy. I had many conversations with Herb Friedman and Talbott Chubb and their colleagues with regard to solar radiation satellites and to rocketry associated with the observation of electromagnetic radiation at several wavelengths. Bill Pellini's work in metallurgy and in materials was always fascinating. It was always fun to stop by and talk to Alan Kolb about his theta-pinch experiment which was contributing to knowledge of phenomena that would be important in nuclear fusion processes. The very low frequency measurements made by Louis Gebhard's group were important. They depended upon the capabilities of the Staff Office and the airplanes that were operated by the Laboratory in order to measure vlf strength at various points on the world's surface. Howard Lorenzen's group working on countermeasures was always fascinating. I enjoyed talking with Lynnwood Cosby and others about the latest "black boxes" they were working on. Next door, one could always find Bob Guthrie and later Merrill Skolnik for a good discussion on new radar developments. Because of my own particular interest in x rays and x-ray crystallography, I would always stop by to talk to Jerry and Isabella Karle about their work. Isabella took some x-ray data that I had left over from my graduate days at Cornell, and she and Jerry were able to determine the complicated structure of a flourinated hydrocarbon that had defied analysis for many years. Also in the x-ray field there was LaVerne Birks who did important work in analyzing x-ray energy levels for the Atomic Energy Commission. John Sanderson's people in the Optics Division were just getting into the field of lasers. He and his staff made an excellent analysis for the Director of Defense Engineering of the projected capabilities of lasers and their importance in a variety of defense applications.

There were some major tasks taken on by the Laboratory during my tour as Director. One of these was the major study of stress corrosion cracking under the sponsorhip of the Advanced Research Projects Agency (ARPA). The Laboratory had the honor of acting for the Navy and competed with the Army and the Air Force for sponsorship by ARPA in this particular area. With its fine team headed by Floyd Brown of NRL working with people from the Boeing Aircraft Company and from Lehigh and Carnegie-Mellon Universities, we won the job. It was a fine example of the coupling of a defense laboratory with an industrial company and universities, all working together as a team toward the solution of a common problem. Another capability developed at about this time was the Laboratory's development of an interest in ocean science. Wayne Hall was very effective in laying out a plan for the development of an oceanographic group.

During these years, NRL was involved in several operations of importance to the Navy. In 1963, it was of assistance in the search for the sunken submarine THRESHER. Later the Laboratory was involved, along with ONR and others, in the search for the lost nuclear bomb dropped by an Air Force plane in the region near Palomares, Spain. Chester Buchanan was instrumental in developing a capability for deep ocean search that was later to be important in the location of the second nuclear submarine that was lost, the SCORPION. NRL was responsive to the needs of the Office of the Chief of Naval Operations in a special study related to submarine communications. It was apparent that the Laboratory's interests spanned the whole spectrum from very fundamental research on through development and test, as well as addressing problems of vital interest to the Navy.

In addition to its major facility at Anacostia, the Laboratory has a variety of field stations, each one of which involves an interesting research or development activity of its own. As Director, I had the opportunity to tour each one and to appreciate the complexity of our operations. There was the 60-foot space-communication antenna at Waldorf, Maryland. Located in the same area was an observing facility of interest to Herb Friedman in his astronomy work. At LaPlata, Maryland, I appeared before a court in connection with a zoning hearing for construction of a cement plant at Waldorf. The Laboratory joined local citizenry in fighting the zoning because of interference it would cause to our activities. We were successful, and the cement plant was not built. On two occasions I visited Sugar Grove, West Virginia to see

NRL's work in the building and the utilization of radio telescopes. I followed with interest Jim Trexler's construction of the very capable 150-foot antenna located there. Speaking of antennae, I can recall my visit to Maryland Point on the completion of the new 85-foot radio telescope used by Connie Mayer. Then there was the requirement for additional pier facilities and a boat area at Seneca Lake, where the Laboratory operated a sound barge. I worked with Harold Saxton and his people to convince the residents that our requirements were valid and that we needed four additional acres of land in order to carry out our activities. I joined Howard Lorenzen and his people in a visit to Hybla Valley to see the Wullenweber antenna operated by the Laboratory on the Coast Guard site. Here again, we eventually ran into zoning problems in that the Virginia Power Company wanted to put some power lines through the area. We worked with the Company to establish the particular location of the lines. The home-owners nearby, however, had some complaints, and it was only after extended conversation that we were able to work out an appropriate solution. Claud Cleeton and I visited the Underwater Sound Reference Laboratory at Orlando, which eventually became the Underwater Sound Reference Division of NRL. Going even farther, I flew in the Laboratory's R4D to Panama to visit the exposure sites that were located at various points in the Panama Canal area.

Another important element in the Laboratory's life was its building program. In the early 1960's Captain Art Krapf, working with people in the Laboratory, developed a long-range building program for NRL. It fell to Captain Brad Bennett, to me, and to my successor, Jim Matheson, to defend these requests for new buildings as a part of the long-range program before the Congress. It was a pleasure for me to participate with others at NRL in groundbreaking ceremonies for the Electronics Building, in which the first dirt was turned over by Bob Page and by Captain Krapf. Next there was the groundbreaking for the Cyclotron Building at which Dr. Glenn Seaborg, Chairman of the Atomic Energy Commission, officiated. As I recall, this was followed by a double groundbreaking operation, in the morning for the new administration building and in the afternoon for the Space Sciences Building. Later as Chief of Naval Research I had the honor of participating with Captain Matheson and Bill Zisman in turning the ground for the new Chemistry Building. Appearing before the Congress always seemed something like the "Perils of Pauline." We would make our best case to the various committees responsible for military construction and then keep our fingers crossed to see how we would finally make out. It is a tribute to Captains Krapf and Bennett to see the major construction activity that has resulted from their early efforts.

Another important element of NRL activity was its interaction with other portions of the Federal Government. As the corporate laboratory, of course, there was very close contact with the Office of the Assistant Secretary of the Navy for Research and Development. Drs. Jim Wakelin, Robert Morse, and Robert Frosch were extremely interested in the activities of the Laboratory and visited often to familiarize themselves with the important work going on. The Laboratory was held in high esteem in the Office of Chief of Naval Operations by many elements of the operating Navy represented therein. The staff always ensured that top management, both military and civilian, at Navy Headquarters was familiar with the Laboratory's capability. In many instances NRL took on special jobs for these people and again demonstrated the viability and importance of the corporate laboratory concept.

We were honored by visits from very high ranking officers of the Navy, as well as members of the Navy Secretariat. Admiral David McDonald and Admiral Thomas Moorer came to see some of our special projects, as well as to attend the biennial Sponsors' Day activities. The Secretary of the Navy, Paul Nitze, visited the laboratory on two occasions and indicated how impressed he was with the extent of work going on. NRL at that time also was doing work for the Army, the Air Force, the Atomic Energy Commission, the Secret Service, and for other elements of government. This shows the importance of having a laboratory (located in Washington) with a broad-base capability that can be responsive to needs for scientific assistance by elements of the Executive Branch.

The Laboratory has often hosted a variety of visitors from overseas. I briefed groups from the North Atlantic Treaty Organization, from Japan, France, United Kingdom, the Soviet Union, Italy, and several countries of Latin America. It was always a pleasure to receive such distinguished guests and to be able to tell them of our many activities.

As I dwell on my 4 years at NRL, many details come to mind, but space does not permit continued discussion. It was my pleasure to work with wonderful people there at all levels of responsibilities. I recall my close relationships with the research directorate, namely Bob Page, Wayne Hall, Alan Schooley, Claud Cleeton, Pete King, Dick Dolocek, and Jim Schulman. I enjoyed working with Everett Breed in the Comptroller's shop and his relief Jack Donovan. Of course, I remember Madeline McDonough and Pat Freeman, who did a noble job in the front office during my tour. Then, too, there were Mrs. East and the staff at the dispensary. Margaret Canfield did outstanding work in developing recommendations for recognition on the part of many people in the Laboratory. I always enjoyed talking with the supervisors and the mechanics in the various shops. There are just too many people with whom it was my real pleasure to work to be named here.

My tour at the Laboratory ended in May 1967, with Captain Matheson taking over after I had been nominated to relieve Admiral Leydon as Chief of Naval Research. It was at this time, however, that we participated in the retirement ceremonies of Dr. Robert Page. I had the honor of officiating at this retirement dinner where in addition to recognition from a host of people within and without the Laboratory, he was honored by a personal letter from President Lyndon Johnson for the outstanding service that he had rendered to his country. Even now, I can say that my tour at NRL was perhaps the most rewarding job that I have ever had. The opportunity to work with over 3000 dedicated people working in the common interests of the Navy was a privilege that only a few can enjoy and one that can never be forgotten.

As Chief of Naval Research my principal attention was focused on maintaining support for an adequate research base for the Navy. It was here that the importance of the corporate laboratory was emphasized even more. I had the particular responsibility of ensuring that there was an appropriate balance of inhouse and subcontracted basic research. In those scientific disciplines in which it was involved, the Laboratory's work was outstanding and set a high mark against which university-based research could be compared and evaluated. In every instance, NRL proved itself to be outstanding. Here again in these years, the Laboratory was called upon to perform special tasks of extreme importance to the Navy. One involved a special study of fleet defense, led by Alan Berman, the new Director of Research. There was the problem of installing special equipment aboard the battleship NEW JERSEY before her deployment to the Southeast Asia area. I mentioned it previously, but the Laboratory's work in the SCORPION search was outstanding and again a feather in the cap for the capabilities that had been developed by Chester Buchanan and his colleagues. At the request of the Assistant Secretary of the Navy for Research and Development, NRL chemists performed special analyses in areas of ocean dumping off the east coast of the United States. NRL became involved in special work in lasers. New developments in that area were such that I could report them directly to the Chief of Naval Operations as items of particular interest to him and to the Navy's future capability.

As I sat down to dictate these remarks, more and more memories of some very wonderful days came back to me. On the occasion of its 50th Anniversary, the Naval Research Laboratory has every right to be proud of its past accomplishments, its current effort, and its potential for continued important fundamental research and for service to the Navy of the future. The Laboratory is a unique combination of people, who with their enthusiasm, their capability, and their dedication ensure that the next 50 years will be even more significant to our country than ever before.

The Need to Look Deeper

R. R. Goodman

Dr. Ralph R. Goodman, Associate Director of Research for Oceanology, came to NRL and his present position in 1968 from the Colorado State University, where he was a professor and Acting Chairman of the Department of Physics. In addition to his role as an educator he has held positions with the Naval Electronics Laboratory and has served as a consultant to the Applied Physics Group at the SACLANT ASW Research Center, La Spezia, Italy. Dr. Goodman is an authority in the fields of acoustic propagation, scattering, and physical acoustics and holds numerous responsible advisory positions in the scientific community.

My arrival at the Laboratory in 1968 was preceded the year before by a reorganization which placed ocean-related research groups under the title "Oceanology," one of NRL's current four major areas of research. Even as a relative stranger, however, I knew that experimentation at sea was no novelty to NRL. Our newest oceanographic vessel, USNS HAYES, is in fact named after a Navy physicist who got his feet wet before becoming part of NRL's charter staff. The May 1973 issue of Scientific American in the column, "50 and 100 Years Ago," reprinted the following article about his early work.

"Sonic sounding is rendered possible by the fact that sound vibrations, passing through water and striking a solid surface, are returned as an echo to the source from which they originated. Working on this principle Dr. H. C. Hayes has developed at the Engineering Experimental Station in Annapolis a method of determining ocean depths, which has been used by ships of our Navy in some very successful and striking demonstrations. Of these the most notable was a series of soundings taken aboard the destroyer STEWART while she was en route from Newport, R.I., to Gibraltar from June 22 to June 29 of last year. In the nine days of the trip the STEWART took 900 soundings, the speed of the ship being 15 knots. Very interesting is the resulting view of the mountains and valleys of the Atlantic. The Navy is to be congratulated on the success of this new and valuable aid to navigation."

By glancing at the globe next to my desk I can see that the track followed by the STEWART would have her pass over the same Atlantic Valley that NRL will investigate next fall. More about that investigation later; meanwhile I'll venture to say that the new view thus provided should be a fitting memorial to this 50th Anniversary year of the Laboratory.

This sounding method of Harvey C. Hayes lead to the continuous echo sounder, a device which has since revealed a wealth of detail about that 70 percent of the earth's surface hidden by the ocean. But with this new knowledge came new questions, many of them vital to the operational needs of the Navy. In the years since 1923 NRL has richly furnished the Navy with an increasingly sophisticated sonic capability—a capability that helps answer many of the questions. But Dr. Harold L. Saxton has very ably traced the early history of NRL's underwater acoustics research elsewhere in this issue. My purpose instead is to call your attention to the way in which a cluster of largely isolated groups, some of which rarely found an opportunity to experiment at sea, has grown in a few years into an organization of cooperating Divisions with sea-going programs. In the 6 years since its inception I feel that the Oceanology Area has reached the stage where it is at last fulfilling that Navy need which gave it birth—the need to look deeper.

I am particularly proud of the way separate Divisions have worked together recently to solve problems at sea which neither of them could tackle alone. Very often in fact outside groups have been called in for assistance, a fact which in no way impugns NRL's capabilities but merely emphasizes the wide span of disciplines needed to look more deeply at the ocean.

Only last summer, for example, the Ocean Science and Ocean Technology Divisions teamed up with scientists from Florida State University, the Naval Oceanographic Office, the Naval Weapons Laboratory, and Edgewood Arsenal to try and answer one of the toughest questions thrown at us. We were asked to look beneath more than a mile of water to ascertain the health of the bottom-living animals and the quality of the local environment in the vicinity of a disposal site for obsolete munitions. The mission required the capabilities which the Ocean Technology Division has developed over the years to search and photograph the sea floor at great depths for sunken objects of importance to the Navy. The equipment used was the latest generation of that which achieved acclaim for NRL in the famed THRESHER, SCORPION, and EURYDICE searches and others of less renown. Scientists from the Ocean Science Division, experts on the complex chemistry of seawater, provided the capability to accurately measure trace quantities of fluoride ion. Fluoride normally present in seawater in known and very minute quantities, is a sensitive indicator for the presence of nerve agent, one of the most toxic substances disposed at the site. Containers of the deadly agent along with other munitions had been given the "deep-six" in four surplus World War II liberty ships. Two of the four ships were found, photographed, and identified. The debris field resulting from the detonation of the third was also located, but the fourth ship was not found. Biologists from Florida State provided a trawl which was tethered beneath the towed search gear during the final phase of the mission. Remarkable photographs were taken of the trawl as it skittered along the rock-strewn seabed performing gyrations which would make prior generations of deep-sea biologists shake their heads and say, "No wonder we only got 50 percent recovery." By streaming the trawl from NRL's so-called "fish," not a single sample was lost. The system has won acclaim from the deep-sea biologists as a significant advance over previous methods. In addition to the animals caught in the trawl, water samples were taken on command from the "fish" and in none of these was a trace of contaminant ever found. Furthermore, it was shown that the animals at the deep site bore no ties with those farther up the continental slope closer to those populations harvested by commercial fishermen.

Another outstanding example of cooperative work at sea involves a recent experiment shared by NRL's Underwater Sound Reference Division (USRD—located in Orlando, Florida) and the Ocean Technology Division. Here the problem was one of testing a new idea for calibrating acoustic transducers associated with Navy work at great depths in the sea.

Because of the way existing laboratory facilities severely limit the measurements that can be performed on large transducers which operate at low frequency, the investigators proposed an *in situ* system suspended beneath a ship such as MIZAR or HAYES. Cable telemetry techniques such as those used to control the deep-sea search system would provide the means of making the needed remote measurements. An actual test of a prototype system was conducted aboard MIZAR in December of last year when the response of a side-looking sonar transducer was measured. The results were intriguing and strongly indicative that known difficulties we are now having with hydrophones at great depth may be evaluated by means of the system.

Mutually beneficial research has quite naturally sprung up between USRD and the Acoustics Division. The calibration expertise of the former has been of great value to the latter, freeing Acoustics Division programs for additional work at sea. The gains by the Orlando acousticians have been realized by numerous opportunities for research at sea during Acoustics Division experiments. A recent product of this symbiotic relationship is a shipboard hydrophone calibration system.

My final example (unfortunately space and time limits permit no more) is the productive work of the Acoustics and Ocean Sciences Divisions in the Arctic during recent years. The acoustics portion of the work involved as many as six research vessels in addition to participating aircraft. Personnel from the Ocean Sciences Division found the occasion an excellent opportunity to determine concentrations of trace elements in the atmosphere, in fog, and in the seawater in this region which was relatively remote from the activities of man. The 1971 Arctic experiments revealed an indication of aerosol washout in heavy fog—a fact having far-reaching implications in fog models. Meanwhile the equipment and expertise of the ocean chemists were needed to precisely determine the seawater sound speed structure to support the experiments of the underwater acousticians. Great emphasis had to be placed on environmental measurements, because of known but poorly understood acoustic phenomena, related in some way to a sea-ice boundary. Among the results that can be freely reported is the observation that propagation losses in the duct-like zone beneath the ice were some 10 decibels less than expected at a range of 60 miles. This experiment took advantage of MIZAR's ice-strengthened hull, a factor which permitted her to be stationed far inside the ice pack, where sound-speed probes and acoustic sources (as well as other instruments) were lowered directly into the water through her center well.

Exceedingly complex problems and rising costs have ushered in the decade of the 1970's. The collaborative efforts I mentioned above are helping to meet these crises. While advances in technology can be blamed on the one hand for greater costs, these same advances, on the other hand, allow rapid acquisition and analysis of data with more reliable gear, a development which actually reduces costs. The newly formed Shipboard Computing Group is implementing systems which will soon permit data reduction at sea, thus shortening the time required to digest and report results. These advances will permit even further collaborative use of our ships at sea, an evolution I will continue to encourage and endorse in the future.

The Ship Facility Group, established in 1968 to coordinate the scientific program with NRL's two research vessels USNS MIZAR and USNS GIBBS (since replaced by HAYES) has already gained a splendid reputation in making our sea-going research more efficient and effective. This competent group of experts in mechanics, electronics, and navigation fills needs common to each Division's ocean-oriented researchers. To these sea-going engineers and technicians and the others like them in the Area go "the most valuable player award" on nearly every trip. Much as the senior investigator might hesitate to admit it (and on my first cruise or two that was I), the advice offered by these individuals is often the best around when the going gets tough. During the 5 years I have been at NRL the members of this rare breed have grown but not kept pace with my growing respect for their suggestions.

Another rapidly growing aspect of NRL's research in oceanology has been the extensive use of aircraft both as data-gathering platforms and as speedy vehicles to enlarge the scale of experiments at sea. Several long-range, open-ocean, acoustic transmission experiments have been performed over the past 4 years, for example, in which explosive charges were dropped from aircraft speeding along precise tracks. The technique was followed during the multiship, international NEAT experiments in which, during 3-month intervals, almost half of the North Atlantic Ocean was tested for its acoustic transmission qualities.

Only last spring NRL's specially outfitted research aircraft, the four-engine "Orion" (RP-3A) was flown through the plume rising from Iceland's newest volcano, Kirkjufell, on Heimaey Island. Here the ocean scientists confirmed their view that volcanoes might be a major natural source of mercury in the atmosphere. Their hypothesis had been put forth to explain high levels of the toxic metal randomly distributed in time throughout some Greenland ice cores they had examined. The peak levels might thus be indicative of periods of high volcanic activity. Even more significant to today's ecology-minded world—NRL's measurements offer convincing evidence that nature, rather than man, is a major contributor of mercury to our environment.

These examples are only two of the many ways ocean scientists use aircraft. Some others include geomagnetic measurements, measurements of temperature, humidity, and cloud condensation nuclei (centered on cloud physics research), measurements of trace gases including carbon monoxide, methane, radon, freon, and carbon tetrachloride, and finally, remote measurements of the surface temperature of the sea. Furthermore a technique was recently developed for rapidly dispensing NRL's monomolecular piston film by aircraft.

The view ahead in the Oceanology Area is one showing an increase in cooperative research at sea. For example, this fall the Oceanology Area is merging elements of three Divisions aboard MIZAR to study the mid-Atlantic-rift valley as part of project FAMOUS (for French-American Mid-Ocean Undersea Study). The sea-floor navigation and photography capabilities of the Ocean Technology Division will amplify the geophysical knowledge of some of the members of the Acoustics Division near a fascinating region known as the triple-junction (referring to a boundary shared by three of the continent-sized plates which are thought to make up the mobile crust of the earth). The chemical expertise of the Ocean Sciences Division will be applied to the analysis of water samples taken on command as NRL's fish glides over possible "hot spots" of newly formed crust. Highly rewarding yields of this kind convince me of the worth of all the administrative spade work that must precede them. It is that kind of oceanology which prompts me to ask, "How's chances for an invitation aboard?"

View from a Vantage Point

A. Berman

Dr. Alan Berman received his Ph.D. degree in physics from Columbia University in 1952. From 1952 to 1955 he was a research scientist at the Hudson Laboratories of Columbia University. He became Assistant Director of Hudson Laboratories in 1955, Associate Director in 1957, and Director in 1963. On May 29, 1967, Dr. Berman became Director of Research for the Naval Research Laboratory. Dr. Berman's research specialities include the areas of underwater acoustics, oceanography, and signal processing. He has published numerous papers on these and related subjects. At present he is a member or chairman of a wide variety of Navy and Oceanographic advisory groups. He also provides advisory services for a number of Department of Defense and other Government agencies. Dr. Berman has on three occasions been visiting scientist to the Admiralty Research Laboratory, Teddington, England (1955, 1957, 1960), and once at the SACLANT ASW Research Center, La Spezia, Italy (1960).

It has been my privilege to serve as Director of Research at the Naval Research Laboratory for the last 6 years, a period which constitutes only 12 percent of the Laboratory's total history. Nonetheless, it has been a period during which the organization of the Laboratory has changed considerably, in response to both internal and external decisions.

In the late 1960's, many of NRL's senior personnel, who had joined the Laboratory either in the late 1930's or in the early days of World War II, were approaching retirement age. With their departure, the Laboratory was faced with the problem of recruiting new leadership and with the need to attract younger scientists and engineers. Fortunately, the history and prestige of the Laboratory were such that we were able to attract many extremely talented new people. What is more to the credit of the Laboratory, we found that within our own ranks a number of talented men were available who were prepared and able to take over responsibility for the direction of the Laboratory's programs.

From 1967 to 1973, we have witnessed the transition of the Naval Research Laboratory into a full seagoing organization that is able to take its place professionally and technically alongside of the great oceanographic institutions of the world. An Oceanology Area was formed in the late 1960's, and with the fortunate choice of Dr. Ralph Goodman as our Associate Director of Research for Oceanology, the Laboratory has achieved a remarkable status in the areas of ocean science, ocean technology, and underwater acoustics.

Dr. Goodman's efforts in this Area have been augmented by four new Division Superintendents; Dr. John Munson, Dr. Victor Linnenbom, Dr. Paul Walsh, and Mr. Robert Bobber. All of these men have assumed their duties within the last 6 or 7 years. Our growth in this Area had a rather solid base on which to develop. In addition to our internal resources, the Laboratory was assigned in the late 1960's the responsibility for the programs previously held by Hudson Laboratories. The infusion of personnel from that Laboratory, the availability of several fine oceanographic vessels (the USNS MIZAR, the USNS HAYES, and the USNS GIBBS), and our many experienced people who were previously associated with some aspect of oceanographic work, provided the Laboratory with the basis for its development into a first-rate oceanographic organization. This organization has shown itself to be capable both of carrying out its regularly assigned research programs and of responding to a number of national emergencies varying from the search for the lost submarine SCORPION to the survey of the BRIGGS nerve gas dump site.

In the last few years, the Laboratory has coordinated all of its space-related activities under the direction of a single Associate Director of Research for Space Science and Technology. Upon the retirement of Dr. Wayne C. Hall, Dr. Herbert Rabin assumed this position and has overseen a significant growth of Laboratory efforts in this Area. NRL is now the Navy's prime in-house laboratory organization for the development of space-related systems.

Our Space Science Division has continued to work under the able and inspired leadership of Dr. Herbert Friedman. Despite the severe budgetary pressures of the last 5 or 6 years, this Division has continued to demonstrate ability to perform interesting and critically important scientific feats. The scientific talent and perception of this Division are shown by its selective choice of experiments so as to maximize the scientific return. Our scientific and national payoff has far exceeded our investment. We are particularly proud of the many accomplishments of our rocket program where, considering the relatively few minutes represented by the combined time in flight of all of NRL rocket shots, the scientific payoff has been substantially greater than the payoff achieved by many satellite programs which had years in orbit.

Under Mr. Howard Lorenzen, NRL has continued to maintain a program of developing military satellites for Navy and national needs. We have played a key role in the present development and potential development of communications, navigation, environmental warning, and other military satellites. We have developed a group which can produce and deliver into orbit satellites which have had an excellent historic record of performance. We are particularly proud not only of the performance of our payloads but also of the role we have played in developing a variety of national systems based on our satellites.

One of the most unusual developments in the last 6 years has been the development and transformation of our Plasma Physics Division, led by a newly selected Superintendent, Dr. Ramy Shanny. This Division has matured into a broadly based group which has an experimental and computational competence that is not surpassed by any group in this country and possibly the world. I am particularly impressed by the many fine physicists and mathematicians who have joined this group and who have developed computational physics and numerical simulation into a new profession within the Laboratory. Not only has our computational group in the Plasma Physics Division completed many basic research projects which are of extreme importance relative to the development of future energy sources and future defense systems, but it has developed a capability to simulate and model numerically such previously intractable problems as the ionosphere and turbulance transport in the ocean. The Division has also completed numerous complex hardware projects for simulating and testing many of the concepts which are needed for the ultimate development of fusion devices.

Dr. Paul Richards has guided our Mathematics and Information Sciences Division during its formative years. We have developed a small but excellent mathematics research center and have seen the Division undertake new and imaginative research directions.

Under the able stewardship of Dr. John L. Allen, who joined us about 2 years ago on the retirement of Dr. Claud E. Cleeton, our heavily systems-oriented Divisions in radar, electronic warfare, and communications sciences have evolved into fine technical engineering organizations.

We are particularly happy about the overall success of the Tactical Electronic Warfare Division under Mr. Lynwood A. Cosby. This Division is a peculiarly NRL organization which began as a small countermeasures section, eventually grew to branch size, and has in the past 3 years evolved into a divisional size organization. Our efforts here are involved in all aspects of electronic and logical countermeasures as applied to the defense of our ships against missile attack.

Our Radar Division, under Dr. Merrill I. Skolnik, although involved in a fairly mature engineering field, has continued to show surprising technical vitality and an ability to adopt new technologies in surprisingly interesting and innovative ways.

Dr. Bruce Wald has recently assumed the superintendency of our Communications Sciences Division. This Division, which is one of the two oldest Divisions in the Laboratory, is currently evolving into a modern communications and data handling organization which can address the Navy's many problems of information transfer at all levels from hardware to software.

Finally, Mr. Albert Brodzinsky has guided the evolution of the Electronics Division into an excellent components division which is capable of producing specialized devices that are not otherwise available to our programs.

Our Materials and General Sciences Area has been expanded and strengthened under the guidance of Dr. James H. Schulman. The development and evolution of our Optical Sciences Division under the leadership of Dr. Walter Sooy has been a source of great personal satisfaction. This Division is now a competent, broadly based organization involved in activities ranging from quantum optics to applied optical systems. We have seen the development of a variety of optical systems and lasers in this Division. We believe that this organization is now in the forefront of optical science and technology.

Our Metallurgy Division under Mr. William Pellini has continued to make striking advances in understanding the structure of metals, the techniques of assembly and fabrication of metals, and the general nature of the cause of failure of metals in complex structures and environments. This Division is typical of all that I consider to be best about NRL. It works on projects that range from intricate quantum mechanical studies of the electronic structure of metal to the failure modes of heavy-section welds. Being as broadly based as it is, it can approach any presently known problem in metallurgy and has thus served as the Navy's metallurgical and materials consultant and conscience.

The Nuclear Sciences Division, the Solid State Division, and the Chemistry Division under the leadership of Dr. John McElhinney, Dr. Clifford Klick, and Dr. Ronald Kagarise, respectively, in the last 6 years have all undergone major evolutionary changes in both program and personnel. In all cases, they have attracted outstanding young people, and they have continued their high publication rate and professional accomplishment for the Navy. All three of these Divisions have benefited by an infusion of new facilities, such as our new cyclotron or our new chemistry building, new people, and new programs. In particular, these Divisions have made a serious and successful effort to bring their competence and technology to bear on joint programs. In the Nuclear Sciences Division, our Van de Graaff accelerator now is used to work on many problems of great interest to our electronics, materials preparation, and solid-state physics community. This interaction has been a source of great pride to me, and I believe that it represents an optimum use of the great resources of this Laboratory.

In addition to the accomplishments of our major research divisions, we have set up a number of small research groups under the leadership of individual, world renowned, scientists such as Dr. Jerome Karle, Dr. William Zisman, and Dr. Maurice Shapiro. These positions which we entitle "Chairs of Science" have provided a few of our most distinguished personnel an environment which allows them to pursue a research program largely unburdened by the administrative problems of our larger divisions.

Above and beyond the personnel and organizational changes witnessed during the last 6 years, the Laboratory faced and adjusted to a major change in the R&D environment. I believe that for the R&D community, the key event of the last 6 years was the Mansfield Amendment of 1970. Although this amendment was only attached to the Appropriations Act of 1970, it set a tone and a basis for research which will be acknowledged and respected throughout the Department of Defense and government-sponsored research community for many years to come. Under the Mansfield Amendment, our work was required to show direct and apparent military relevance. This we did with little difficulty. The point is that the Laboratory adjusted to an environment wherein it was able to do basic research and to demonstrate that our choices of programs were both relevant and responsive to the spirit and intent of the law. Furthermore, we have shown that good research can indeed be performed in the context of the needs of a mission-oriented agency.

I believe that the concept of a large government Laboratory such as NRL, which is dedicated both towards basic research and systems development in support of the Navy's needs, is indeed valid. It has proven itself to be surprisingly successful. It is still a young organization after the first 50 years of its existence. I have confidence that NRL will not grow intellectually old and that 50 years hence, when the pages of this journal record comments for the Centennial Issue of the Laboratory, people will still marvel at the youth, vitality, and intellectual excellence of this Laboratory.