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New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory

DAVID KITE ALLISON

Historian
Technical Information Division

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The history of science properly does not concern itself with the things of science: the plants, the animals, the molecules, the atoms, the ether, the quanta, or even the law or the equation. The only object of study in the history of science is Homo Sapiens.

— A. Hunter Dupree

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PREFACE

This report is a revision of my doctoral dissertation, "The Origin of Radar at the Naval Research Laboratory: A Case Study of Mission-Oriented Research and Development," which was accepted by Princeton University in January 1980. While most of the material in the text is the same, I have made some significant changes in the final chapters.

Mission-oriented research and development has comprised a substantial portion of American science since the late 19th century. This study examines one instance of it: the early development of radar at the Naval Research Laboratory (NRL). Since opening in 1923, NRL has been the principal home of advanced science and engineering in the U.S. Navy; the creation of practical radar equipment there was one of the most significant achievements of the institution prior to World War II. In the dissertation, the history of this development is told. The principal aim is to answer the question, What was the combination of technical determinants and administrative, economic, political and personal factors that caused radar to come into being and then led the project to progress as it did? That is, What characterized the institutional process involved in this case of mission-oriented research and development?

A brief introduction dramatizes the problem and explains the approach. The next three chapters then retreat from the immediate subject to examine the background. Chapter 2 summarizes those aspects of the early history of industrial research laboratories and of research and development in the U.S. Navy that pertain to the creation of NRL. Chapter 3 treats the origin of NRL in detail, moving from the initial plan for it in 1915 until it began operation 7 years later. Chapter 4 relates antecedents of the radar project. A failure to initiate such a program in 1922, when the opportunity first arose, is analyzed. Then various personal and institutional factors pertaining to the general operation of NRL are discussed.

The next three chapters trace in detail the development of the first practical radar device. Chapter 5 takes the story from the time the radar project started in 1930 through the first test of equipment in 1934. Chapter 6 continues the account until 1936, when the first satisfactory working models were demonstrated. Chapter 7 moves from there until operational equipment was being introduced into the fleet.

In Chapter 8, the story broadens. Soon after the possibility of equipment was proved, it became clear that the ideas basic to radar could lead to a host of useful devices and that a new field of technology had been opened. This chapter examines those parts of the early work in the field that were directly related to the radar program at NRL. The chapter concludes by showing how the radar project, along with other major programs and forceful administrative guidance, led NRL to assume a more important role in the Navy as the possibility of war increased during the late 1930s. Chapter 9 situates the radar work at NRL in its international context. It first compares the project to similar efforts of the U.S. Army and Great Britain and relates the effect on NRL's program when detailed knowledge of the British achievements were disclosed in 1940. It then describes how the mobilization of civilian scientists in the United States, under the National Defense Research Committee, affected NRL's institutional role. Finally, it traces progress in the NRL radar project until the beginning of World War II. The chapter closes with a description of what equipment was actually operational in the fleet at that time. A concluding chapter presents the authors assessment of the study and its significance.

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FOREWORD

The title of this volume, "New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory," inadequately describes its contents. It is, in fact, a remarkable case study of mission-oriented research and development during the critical period from World War I through World War II. Dr. Allison has completed a scholarly review of the development of radar at NRL together with the personalities and objectives of the people who were involved in it.

In a broader sense, this volume answers a group of questions which have major impact in the context of the current complex world of research-and-development administration. How and why did the Naval Research Laboratory develop as an institution? How did it evolve from the original concept of some of its early supporters? Originally, NRL was conceived as being an extrapolation of the arsenal concept of the nineteenth century. Within five to ten years of its founding, however, it evolved into a modern laboratory which integrated basic research with system developments. When was the idea of radar conceived, and when did the laboratory develop it? Why was the early equipment designed as it was and in what sense did it reflect institutional capabilities and biases? This book also discusses the response of the leaders of operational forces to the development of radar and examines the relationship of the NRL development to other independent developments both in the United States and abroad. The transition of research from an in-house government laboratory to private industry has always been a difficult, controversial problem. Hence, the case study of how private industry became involved in radar is extremely illuminating. The question of what brought about the transformation of the primitive laboratory radars of the 1930s into a mature technology that resulted in a massive production effort during World War II is examined with remarkable insight and clarity. These important issues and the responses to them record and explain one important aspect of how the Department of the Navy met its responsibility to maintain national defense in the years between World War I and World War II.

The study has gone beyond these topics to touch on much broader issues. It captures many of the essential qualities of how men react to the challenge and opportunities of scientific research and how institutions develop their positions, shape their thoughts, generate plans, and respond to the constraints of the time. Any person with major responsibilities for the research-and-development program of a mission-oriented agency or marketplace-oriented industry will find this volume remarkably interesting and provocative.

While this volume was originally developed by Dr. Allison as part of his PhD dissertation, sponsorship of its publication in book form by the Naval Research Laboratory was deemed appropriate because of the broader aspects treated. Dr. Allison has provided an invaluable historic record of the development of a particularly important technology which revolutionized warfare, transportation, weather prediction, and national defense generally. He has treated the general question of the evolution of institutions that are set up for mission-oriented research, and he has explored the very difficult process of how new ideas and technology are introduced into society. All readers should find the reading of this book an extremely rewarding experience.

/s/ Alan Berman
NRL Director of Research

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Many people assisted me during my research. They are too numerous and my memory is too poor for me to give all of them the recognition they are due, but I will acknowledge at least some of them. Dr. Charles C. Gillispie of Princeton University was my principal advisor while I worked on my thesis and gave me much helpful criticism on my writing as I progressed. Since 1977, I have been the Historian of the Naval Research Laboratory, and Mr. Earle Kirkbride, my principal supervisor there, both encouraged my work and helped me remain free enough from other activities to finish this account. He also has been a prime force in moving it toward publication.

A number of people who were involved in some way with radar at NRL consented to be interviewed by me and taught me a great deal. They are, in alphabetical order, Dr. Edward G. Bowen, Vice Adm. Harold G. Bowen, Jr. (USN, retired), Dr. Claud Cleeton, Dr. Louis Gebhard, Mr. Robert C. Guthrie, Dr. Edward O. Hulburt, Dr. Robert M. Page, Dr. Merrill I. Skolnik, and Dr. Irving Wolff. Of these individuals, I am especially indebted to Dr. Page. Not only did he answer my questions for almost two solid days, but after I had finished my dissertation, he read it carefully and made many helpful criticisms. The study would have been far poorer without the benefit of his cooperation.

In research at the Naval History Center, I was assisted by Dr. Dean C. Allard, "Cal" Cavalcante, Nina Statum, and Jerri Judkins; in the Navy Department Library, I was assisted by Barbara Lynch. At the National Archives, I was helped particularly by Dr. Gibson Smith, Tim Nenninger, and Fred Parnell. Clement Butler of the NRL Records and Correspondence Management Office guided me to the pertinent archival materials at the Washington National Records Center, and once there, I was helped greatly by Ruby Beckett and Ann Parker. Many of my professional colleagues gave me thoughts or suggestions of value, most notably Dr. Daniel Kevles, Dr. Thomas Hughes, Dr. Harvey Sapolsky, Dr. John Servos, Dr. Alex Roland, Dr. Richard Hewlett, Mr. Albert Christman, and Mr. Bernard D. Bruins.

This study places much emphasis on the importance of financial support, and I am well aware of the significance of my own. I gratefully acknowledge the aid of the National Science Foundation while a graduate student and of the Naval Research Laboratory while an employee there.

I owe special thanks to my typist at NRL, Ellie Gladmon, who not only slogged diligently through pages of messy manuscript with cheerfulness rather than complaint but also stuck with the project from the hesitant first draft of the introduction through the preparation of the final pages of the work in its present form. Dave Triantos and Stanley Weintraub contributed much by closely editing the manuscript, and Dora Wilbanks and her staff in NRL's Computerized Technical Composition section were responsible for typesetting and layout. Finally, I am deeply indebted to my wife, Ymelda Martinez-Allison, who helped me in all aspects of my project and gave me the support that can come only out of love.

UNCLASSIFIED

CONTENTS

PREFACE iii

FOREWORD v

ACKNOWLEDGMENTS vii

1. INTRODUCTION 1

2. HISTORICAL BACKGROUND 5

The Inception of Industrial Research 5
Edison and His Laboratories 7
The Flowering of Industrial Research 9
The Technological Revolution in the Navy 10
Naval Research and Development Prior to World War I 11
Facilities of the Bureau of Ordnance 12
Facilities of the Bureau of Construction and Repair 13
Facilities of the Bureau of Steam Engineering 13
Limits of the System 14
A Merging of Traditions 15

3. CREATION OF THE NAVAL RESEARCH LABORATORY 17

An Interview with Edison 17
Establishment of the Naval Consulting Board 19
Organization and Activities 22
The Laboratory Project: Success 25
The Laboratory Project: Failure 28
Naval Officers Take Command 33
The Scientific Program 35
Legacy of the Board 37

4. ANTECEDENTS OF THE RADAR PROJECT (1922 to 1930) 39

The Discovery of 1922 39
Taylor and Young 41
Guiding Policies 46
The Technical Context: High Frequency Radio 53
The Technical Context: Ionospheric Research 56

5. FROM THE BEGINNING OF THE PROJECT TO THE FIRST TEST (1930 to 1934) 61

Origin of the Radar Project 61
Research on Continuous Waves 64
The Bureau of Engineering Takes Control 68
A New Start: The Switch to Pulses 78

Progress Through the First Test 82

6. FROM TEST TO WORKING MODEL (1934 to 1936) 85

Robert Morris Page 85
Designing a Receiver 88
A Congressional Vote of Confidence 90
A New Eye for the Navy 93
Seeing is Believing 95

7. FROM MODEL TO OPERATIONAL EQUIPMENT (1936 to 1940) 99

The Path to Practicality 99
A Shipboard Test 101
Producing a Prototype 104
Radar Goes to Sea 107
Equipment for the Fleet 110

8. A DEVICE BECOMES A FIELD 113

Early Microwave Research at NRL 113
Shaping Relations with Private Industry 120
The Navy Radar Program to Mid-1940 124
A New Role for the Laboratory 129

9. THE BROADENING CONTEXT 137

Early Radar in the U.S. Army and Great Britain 137
The Tizard Mission 148
The National Defense Research Committee 155

10. RADAR RESEARCH AND DEVELOPMENT UNTIL THE WAR 161

Sorting Out Institutional Roles 161
The NRL Program Until Pearl Harbor 174
Radar when War Began 180

11. CONCLUSION 183

Approach 183
Major Themes 183
Historical Context 183
The Beginning 185
Invention and Development 186
Effect of Management 189
Evaluating the Story 190

APPENDIX A — General Order 84, March 25, 1922 193

APPENDIX B — General Order 41, May 13, 1935 195

APPENDIX C — General Order 124, September 14, 1939 197

UNCLASSIFIED

APPENDIX D — General Order 130, December 8, 1939 199

APPENDIX E — General Order 150, July 12, 1941 201

APPENDIX F — NRL Letter Reporting on Radar, June 11, 1936 203

APPENDIX G — NRL Letter Comparing British and U.S. Radars, October 4, 1940 211

SOURCES 215

Archival Materials 215
Secondary Sources 216

INDEX 225

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NEW EYE FOR THE NAVY: THE ORIGIN OF RADAR AT THE NAVAL RESEARCH LABORATORY

1. INTRODUCTION

It was mid-December 1934. Robert M. Page, a research engineer at the Naval Research Laboratory, sat alone before the glowing screen of an oscilloscope. He searched for bumps along the outer circumference of a circular pattern that was constantly being swept out by a moving dot.[1] A bump might indicate either one of the short strong pulses of energy being emitted by a radio transmitter and synchronously displayed on the oscilloscope by means of a direct electrical connection or a weak echo of a pulse coming from an airplane that was flying up and down the Potomac River. Display of the transmitted pulses was automatic, but if the screen was also showing signs of echoes, it meant that the invention he was testing—a system using radio pulses to detect and determine the range of airplanes—worked.[2]

The system was to operate in this way: The dot sweeping out the circle measured the flow of time. It would jump away from the center and form a bump whenever a transmitted pulse or an echo picked up by the receiver was fed to it. The distance between bumps caused by a pulse and its echo could be used to calculate the range of the airplane. The idea had been studied at the Laboratory for some time; this was the first practical test of equipment.

Years later, Page described the test as follows:

> Having built a radio transmitter for illuminating targets with short pulses of radio frequency energy, we desired to find out whether echoes from aircraft could be detected with those short pulses. For eight months we had dreamed and thought and planned and worked on a fantastic idea, knowing it could be doomed, but fired with a burning hope that it was destined to succeed. Many problems remained to be solved in receiving and indicating echoes from targets, if there were any echoes to receive. It was very important to find out as early as possible whether there was any need even to try to solve these other problems. All we needed was to determine whether pulse echoes would occur in sufficient energy to be detected at all. So a test was set up in which a laboratory model of a very high gain, high frequency experimental receiver with a cathode ray indicator and a separate receiving antenna was used to test for the presence of radar echoes. The pulse transmitter and keyer were in one building with a directive antenna on the roof. The receiver and indicator were in an adjacent building with a

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[1] This type of indicator was soon replaced by other forms. The familiar plan position indicator, which puts dots on the screen at such points that their polar coordinates indicate distance and direction of objects from the receiver antenna, was a much later development.

[2] The word radar, an acronym of *ra*dio *d*etection *a*nd *r*anging, was coined in 1940 by two U.S. naval officers and was soon approved as an official name in America for this type of device. By 1943 it was in general use throughout the Allied forces. After World War II it was adopted throughout the world.

> similar directive antenna on its roof. The keyer in one building and the indicator in the other were connected by a cable for synchronizing the indicator with the transmitter pulses. The two antennas were pointed out across the Potomac River, which flowed past the Laboratory, and a small airplane was flown up and down the river through the radar beam at low altitude.
>
> Echo signals from this airplane were observed while the transmitter was off in the intervals between pulses. This was proof that short pulse echo energy was sufficient to justify going ahead with solution of the receiver and indicator problems. Thus did pulse radar pass its first test with an airplane target in December 1934.[3]

Although the test was a success, there were, as Page said, many problems with the equipment. Instead of two distinct bumps, for a transmitted pulse and the received pulse, the oscilloscope showed only a confused mix, obvious in the wavering or "beating" of the bump that indicated the transmission. And, to cause any effect at all, the plane had to be so close that detection by sight or sound was much more effective than detection with the new device.[4] Indeed, Page's immediate reaction was not elation but frustration and disappointment. He later remembered,

> I was just emotionally completely thrown by [the results]. I should have known better. I shouldn't have been expecting so much. It took me, I think, a couple of days to recover.[5]

Soon, however, he realized that the receiver had indeed indicated both pulse and echo, and that thus some form of radio detection was possible. Equally important, the results convinced his supervisors that the work was promising enough to go on.

This incident is but one small part of a long process that constitutes the development of radar. But in many ways it exemplifies the whole. Talented men conceived new technical ideas based on their advanced knowledge of radio principles. They transformed them into a configuration of antennas, wires, and tubes. The equipment was tested under operating conditions, and the results led to new ideas for modifications and improvements. Overlooking the entire research process, guiding and shaping it, were administrators of the Naval Research Laboratory and the Navy as a whole. These men set the goals of the project and determined what resources would be allocated to it. Because they did not follow the details of the work, practical demonstrations often provided them with an important, objective measure of progress.

This study narrates the origin of radar at the Naval Research Laboratory. Although many of the details have been recounted before,[6] I write here from a new point of view with a definite purpose. I

[3] Robert M. Page, *The Origin of Radar* (Garden City, N.Y.: Doubleday, 1962), pp. 64-66.

[4] Robert M. Page, laboratory notebook 171, vol. III, pp. 98-99, in records of the Naval Research Laboratory, Records and Correspondence Management Office, NRL, Washington, D.C.

[5] Transcript of a tape-recorded interview with Dr. Robert M. Page, Oct. 26 and 27, 1978, in the Historian's office, NRL, Washington, D.C., p. 62.

[6] Most previous accounts concentrate on the history of technical developments. The most important discussions of NRL's work appear in: Louis A. Gebhard, *The Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979); Henry Guerlac, *Radar in World War II* (unpublished history of Division 14 of the National Defense Research Committee, 1947); John M. Hightower, "Story of Radar," U.S. Congress, 78:1, Senate Document 89 (Washington: GPO, 1943); Captain L.S. Howeth, *History of Communications-Electronics in the United States Navy* (Washington: GPO, 1963); Joint Board on Scientific Information Policy, "Radar: A Report on Science at War" (Washington: GPO, 1945); John B. McKinney, "Radar: A Case History of an Invention" (unpublished term paper for the Harvard Business School, 1961); Robert M. Page, *op. cit.* (note 3); and Charles Susskind, *History of Radar: Birth of the Golden Cockerel* (book in preparation).

UNCLASSIFIED

write from an institutional perspective. I argue that radar should be seen as the product not simply of one man or even a group of men but rather as the result of individuals working within the structure of a mission-oriented research-and-development facility. To comprehend how radar was developed, when it was developed, and why, one must follow not just the evolution of technical progress but also the administrative and political decisions that shaped it. One must understand how the talents and motivations of the people who created this new device were related to the particular institutional situation and historical context in which they labored.

The account is the story of a modern research-and-development laboratory in action. It discusses one major accomplishment of one institution. But it is also written to contribute to a broader understanding of the history of research and development laboratories in general and of the influence they have had on the course of modern American history. The work of the Naval Research Laboratory on radar is a significant episode in that story.

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2. HISTORICAL BACKGROUND

A study of institutionalized research and development should begin with the institution itself. Accordingly, the formation of the Naval Research Laboratory is the subject of these next two chapters. Knowledge of the initial organization, program, and policies is directly pertinent to understanding the development of radar, for it began during the years when they were in effect.

The Naval Research Laboratory was a product of World War I. It resulted when a group of civilians called to advise the Navy on scientific matters attempted to create within it a research laboratory modeled on those that had already been set up in American industry. Details of the origin are discussed in the following chapter. This one examines the historical context. There are two parts: a discussion of the early history of industrial research, with special attention to the laboratories of Thomas Edison, and a general consideration of Navy research-and-development facilities until World War I.

THE INCEPTION OF INDUSTRIAL RESEARCH

Industrial research laboratories were first formed in America in the late 19th and early 20th centuries.[1] Their creation was related to several major historical trends. First, the evolution of knowledge in the physical sciences led to a host of new inventions. Chemistry, optics, thermodynamics, electricity, and magnetism, for example, provided the understanding necessary for such innovations as artificial dyes, high-grade optical glass, improved gunpowder, vulcanized rubber, the telegraph, the electric light, and the telephone. The path from scientific knowledge to practical product was by no means simple or direct. Indeed, throughout this period of history, the best inventors often had little or no scientific training. Nonetheless, inventions increasingly depended on the results of scientific investigation—indirectly if not directly. The art of inventing had begun to require the methods of systematic research.

Second, business and industry were becoming more deeply involved with technological innovation. The use of machines that had transformed textile manufacture was now spreading throughout the economy to such areas as transportation, communication, and chemical production. In many cases, inventions gave birth to new companies such as those in the telephone, telegraph, photographic, and electric lighting industries.

The rapidly growing use of new technology did not come easily in business. It was inexorably tied to the adoption of new methods of management, institutional organization, distribution, and finance.[2] The creation and success of industrial research laboratories depended as much on the ability of American firms to use technical changes profitably as it did on the ability of technically minded people to produce them. Nonetheless, as has often been stated, increased reliance on technology tended to foster

[1]There is no comprehensive history of industrial research laboratories. The studies I used for general information are the following: Howard R. Bartlett, "The Development of Industrial Research in America," in National Resources Planning Board, *Research—A National Resource*, three vols. (Washington: GPO, 1938-1941), vol. II, pp. 19-77; W. David Lewis, "Industrial Research and Development," in Melvin Kranzberg and Carroll Pursell (editors), *Technology in Western Civilization*, two vols. (New York: Oxford University Press, 1967), vol. II, pp. 615-634; Harold Vagtborg, *Research and American Industrial Development* (New York: Pergamon, 1976); Kendall Birr, *Pioneering in Industrial Research* (Washington: Public Affairs Press, 1957); Leonard S. Reich, "Radio Electronics and the Development of Industrial Research in the Bell System," (PhD Dissertation, Johns Hopkins University, 1977), especially ch. 1; and David Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Knopf, 1977), especially ch. 7.

[2]See, among other accounts, Alfred D. Chandler, Jr., *The Visible Hand: The Managerial Revolution in American Business* (Boston: Harvard University Press, 1977), and Harold Passer, *The Electrical Manufacturers, 1875-1900* (New York: Arno, 1972; reprint of the original 1953 edition).

even more dependence on it. This was especially true of many electrical and chemical firms that owed their existence to innovations. Being successful increasingly meant acquiring and maintaining a lead in technology.

One particular stimulus for companies to develop their own research facilities was the American patent system. As David Noble and Lenny Reich[3] have emphasized when studying this aspect of industrial research, if a company had its own laboratory, it had a better chance to come up with significant new ideas first and then, with patents, to control the use of the ideas by other firms. Thus might whole fields of enterprise be dominated. The desire to acquire patent protection was a major reason for the creation of laboratories by such companies as General Electric, Kodak, and American Telephone and Telegraph.

Finally, industrial research was linked to the growth of formal scientific and engineering education in American institutes of higher learning. Technical education had developed slowly in America. Although several specialized technical schools had appeared before the Civil War and science had by then begun to be taught in the standard curriculum at many colleges,[4] major changes did not come until after 1860. The Morrill Land Grant Act, passed by Congress in 1862, helped bring systematic training in agricultural sciences to all states. The Massachusetts Institute of Technology (opened in 1865), Cornell University (founded in 1869), and many state universities produced college-trained engineers who began to supplant those trained in the shop.[5] Johns Hopkins University (opened in 1876) followed by Harvard, Yale, the University of Chicago, and other leading schools brought the development of research-oriented graduate education in science.[6] The elective system, started by President Charles Eliot of Harvard in the 1870s, helped open specialized training in science and engineering to undergraduates.

As such changes spread throughout America, they allowed colleges and universities to meet the technical needs of the nation in two ways: Providing scientific knowledge on which technical innovations depended and providing a growing supply of individuals with special skills in science and engineering. Although not all the men who were important in the early history of industrial research laboratories were formally trained, continued success of this type of institution depended largely on graduates from colleges and universities.

The employment of scientists, engineers, and inventors in industry came gradually.[7] In some isolated cases during the early and mid-19th century, professionals were called on as consultants to solve specific problems or test, analyze, and improve products. In other cases, inventors, particularly those, who had sold their ideas to a firm, were taken on as employees. Still others set up their own companies. In the entire period, however, the application of science and invention to industry was sporadic and unorganized, and the work of industrial scientists was usually routine application of existing knowledge.

It was not until the last quarter of the 19th century that American companies began to create organized research facilities. Those established by Thomas Edison were among the first. Since Edison

[3] *Op. cit.* (note 1).

[4] See Frederick Rudolph, *The American College and University: A History* (New York: Vintage, 1962), and Stanley M. Guralnick, *Science and the Ante-Bellum American College* (Philadelphia: American Philosophical Society, 1975).

[5] Monte A. Calvert, *The Mechanical Engineer in America, 1830-1910* (Baltimore: Johns Hopkins University Press, 1967); Edwin Layton, *The Revolt of the Engineer* (Cleveland: Case Western Reserve University Press, 1971), especially ch. 4, "The Politics of Status."

[6] See Daniel J. Kevles, "The Study of Physics in America, 1865-1916" (unpublished PhD dissertation, Princeton University, 1964), and *idem, The Physicists: The History of a Scientific Community in Modern America* (New York: Knopf, 1977), especially ch. 5, "Research and Reform."

[7] Howard R. Bartlett, *op. cit.* (note 1), p. 25.

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became deeply involved in the creation of the Naval Research Laboratory, both he and his establishments will be discussed.

EDISON AND HIS LABORATORIES[8]

Thomas Edison has best been described not simply as a brilliant inventor but as an inventor-entrepreneur.[9] He devoted his life not only to creating, designing, and improving products but also to making those products marketable and profitable. More than his remarkable ability to solve technical problems, it was this dual orientation that made his career so noteworthy and influential.

Edison's self-education and years of youthful wandering as an itinerant telegraph operator are well known. By age 21, he had decided to become an inventor. He managed to sell several of his first ideas and devices to Western Union, and, in June 1869, the company settled with him all at once for a number they had purchased, giving him a lump sum of $40,000. With it he established a manufacturing firm in Newark, New Jersey, to produce stock tickers based on his ideas. Soon he established two more small companies to develop and manufacture other inventions.[10]

These shops allowed Edison to do much experimental investigation, but they were primarily manufacturing firms. The resources and time that he could devote to developing new products were limited; the troubles inherent in making small companies profitable were great. All of his experience taught him that successful invention was closely linked to manufacture, but he began to hope more and more that he could start devoting all his time to invention. One aspect of his situation in Newark, however, he did not want to give up: an organized and talented staff of assistants. He realized that if he were to be successful as a professional inventor, he could not be successful alone. Finally, in 1876, he decided to risk making systematic and organized research profitable. He gave up his manufacturing interests in Newark and moved to an isolated country town, Menlo Park, New Jersey, to establish an "invention business."

The Laboratory he built at Menlo Park was a startling new departure, yet it was also a continuation along lines of development and aspiration that he had followed for years. Several men who worked with him and wrote his authorized biography describe the change in this way:

> ...it had been a master passion with Edison from boyhood up to possess a laboratory, in which with free use of his own time and powers, and with command of abundant material resources he could wrestle with Nature and probe her closest secrets. Thus, from the little cellar at Port Huron, from the scant shelves in a baggage car, from the nooks and corners of dingy telegraph offices, and the grimy little shops in New York and Newark, he had now come to the proud ownership of an establishment to which his favorite word, 'laboratory,' might justly be applied. Here he could experiment to his heart's content and invent on a larger, bolder scale than ever—and he did![11]

[8]The principal source I used for this section was Matthew Josephson, *Edison: A Biography* (New York: McGraw-Hill, 1959). To a lesser extent I relied on Frank L. Dyer, *et al.*, *Edison, His Life and Inventions*, two vols. (New York: Harper, 1929); Francis Jehl, *Menlo Park Reminiscences*, 3 vols. (Dearborn, Mich.: Edison Institute, 1937-1941); Thomas P. Hughes, *Thomas Edison, Professional Inventor* (London: Her Majesty's Stationary Office, 1976); *idem*, "Edison's Method," *American Patent Law Association Bulletin*, July-Aug. 1977, pp. 433-450; Harold Passer, *op. cit.* (note 2); and Robert Conot, *A Streak of Luck: The Life and Legend of Thomas Alva Edison* (New York: Seaview, 1979).

[9] This characterization has become fairly common in recent writing about Edison but probably is best elaborated in Passer, *op. cit.* (note 2), pp. 176-191, and Hughes, *Thomas Edison...* (note 8), p. 20.

[10] Josephson, *op. cit.* (note 8), pp. 84-104.

[11] Dyer, et al., *op. cit.* (note 8), vol. I, p. 269.

The laboratory building itself was rather humble. Measuring 100 feet long and 30 feet wide, (30 by 9 meters), it was covered with white clapboard and even had a front porch. But the apparatus inside in no way matched the appearance: it included a wide range of chemicals and modern electrical experimental equipment. Some of the latter was so sensitive that it was put on specially constructed "vibrationless" tables, with their own solid foundations extending deep into the ground. In addition, there was an excellent library containing thousands of volumes of scientific and engineering periodicals and reference works. Here Edison located his office and regularly perused the collection when working on new ideas.[12]

The staff was initially composed of about 12 men he had brought from Newark—mostly self-taught inventors like himself. But soon he began to add individuals with professional training. One important example was Francis Upton, who had studied mathematical physics both at Princeton and under Hermann Helmholtz in Berlin. Edison relied on Upton to make difficult mathematical calculations or solve theoretical problems. The art of invention was changing, and Edison was wise enough to change with it.

The years at Menlo Park were his most productive. There he made many of his most important inventions: the phonograph, the carbon telephone, the chalk telephone, and, greatest of all, the incandescent lighting system. These, especially the last, soon led him back into manufacturing. By the mid 1880s, the research facility at Menlo Park had fallen into disuse as he and his staff had become involved with producing and marketing his products.[13]

When he decided again to focus on inventing, he chose to make a new start on a grander scale. In 1887, he built a new laboratory and manufacturing plant in West Orange, New Jersey. The laboratory buildings were 10 times the size of the one at Menlo Park and constituted at that time the largest and most complete private research establishment in the world. The main building had 60,000 square feet (5600 square meters) of floor space and contained large machine shops, chemical and photographic departments, and rooms for electrical testing. The library housed 10,000 volumes, and the scientific staff numbered between 45 and 60.[14] Here Edison would spend the rest of his career developing and marketing earlier inventions, such as the phonograph, and making new ones, such as motion pictures. Here also he would face his greatest challenges when trying to develop storage batteries, develop a method for extracting iron ore by magnetic means, and create artificial rubber from goldenrod.

The well-publicized success of the laboratories at Menlo Park and West Orange and the companies that developed from them helped encourage other firms to establish research facilities. In many ways Edison's establishments served as prototypes for later organizations. Both were similar in their dependence on teams of workers attacking problems in a systematic fashion, for the stories of Edison devising his inventions with "strokes of genius" are sheer myth. Both had as aims the invention and design or improvement of useful, marketable products. Both relied on published technical information and on the skills of professionally trained scientists and engineers.

But Edison's laboratories also had many special characteristics that later laboratories did not share.[15] He himself and his ideas were always the center of activity. His establishments were largely an extension of his own powers to work out his plans and ideas. He was not so much the manager of his laboratories as the single focus of their activity. Later institutions would encourage more freedom

[12] Hughes, in *Thomas Edison...* (note 8), pp. 17 and 18, rightfully puts great emphasis on the sophisticated library and equipment Edison used.

[13] Josephson, *op. cit.* (note 8), p. 290, also argues that the death of Edison's first wife had much to do with why he abandoned the laboratory at Menlo Park.

[14] *Ibid.*, pp. 314 and 315.

[15] Hughes, in "Edison's Method" (note 8), argues that there are no essential differences between Edison's laboratories and later industrial research facilities. I agree with his emphasis on the sophistication of Edison's facilities, but I still think some distinctions must be made between them and those that came later.

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among investigators and adopt management techniques more suited to diversified investigation. Furthermore, although Edison saw the distinction between research and production, he never made a strong institutional separation. His own interest in both aspects of his work kept his laboratories tied closely to manufacture. Later facilities would eschew so close a link. Finally, whereas Edison drew on the findings of modern scientific research, his method was still primarily empirical. Later laboratories, which relied more heavily on professionally trained scientists and engineers than had he, would concentrate on giving scientitic explanations of phenomena. And, at least in some cases, they would encourage employees to publish and thus contribute to the increase of scientific knowledge as well as the development of practical products.

Thomas Edison bridged the change in American technological history from domination by the lone inventor to the rise of organized research laboratories. His establishments mirrored his transition. In their organization and mode of operation they mixed the methods of cut-and-try empiricism and those of systematic scientific research. They never fully changed to match the structure of the most advanced research-and-development laboratories that were established after the turn of the century. Still, Edison, as his involvement with the formation of the Naval Research Laboratory would show, never gave up advocating the type of research policy his establishments embodied.

THE FLOWERING OF INDUSTRIAL RESEARCH

Research laboratories were created by many companies besides Edison's prior to World War I. Among them were American Telephone and Telegraph, Eastman Kodak, DuPont, Parke-Davis Pharmaceuticals, the Corning Glass Works, Westinghouse, and several oil firms.[16] General Electric, in 1900, took a bold step and established the first laboratory dedicated primarily to basic research.[17] Headed by Dr. Willis R. Whitney, a chemist who had received his PhD in Germany for work under Wilhelm Ostwald, it was staffed by top PhD scientists and was based on the idea that fundamental scientific investigation in the right fields would yield practical results for GE. In this institution, a new role evolved for professional scientists in industry. Researchers were allowed to investigate scientific problems with the primary purpose of seeking new knowledge. That is, although limited in their subject selection, they shared many of the freedoms of their academic colleagues.[18]

Whitney, like Edison, served as a scientific advisor to the Navy in World War I and helped plan the Naval Research Laboratory. He brought with him the experience of having organized the first basic research laboratory in American industry, an institution that differed significantly from Edison's establishments. The differences would become apparent in the determination of the policy for the new Navy facility.

Laboratories set up by other companies varied in size and type. Some focused on purely practical problem-solving or on test and analysis of products. Others concentrated on inventing new products. A few were modeled on GE's laboratory and concentrated on basic scientific research. Probably most fulfilled each of these functions to some extent. The diversity that appeared showed that different companies hoped for different gains from organized science and engineering. But it had become clear that industry had accepted the efficacy of systematic research. The first national survey of industrial laboratories, conducted soon after World War I, counted over 300 institutions.[19] Indeed, even before the war started, industrial research was well established.

[16] See Bartlett, *op. cit.* (note 1) for a more complete listing.

[17] See Kendall Birr, *op. cit.* (note 1); George Wise, "A New Role for Professional Scientists in Industry: Industrial Research at General Electric, 1900-1916," *Technology and Culture* **21** (July 1980): 408-429; and John T. Broderick, *Willis Roderick Whitney, Pioneer of Industrial Research* (Albany, N.Y.: Fort Orange Press, 1945).

[18] Wise, *op. cit.* (note 17) explains in greater detail how the scientist at GE was similar to and distinct from his academic counterpart.

[19] Bartlett, *op. cit.* (note 1), p. 37.

THE TECHNOLOGICAL REVOLUTION IN THE NAVY[20]

In the 1880s and 1890s, the United States began to aspire toward a Navy comparable to those of Europe in order to project American power and defend American economic interests overseas. Modernizing the American fleet, which had sunk to 12th among world navies, meant extensive mechanization and the incorporation of much new technology. The same historical forces that had changed the technical base of business and industry were transforming the requirements of sea power. Wooden ships propelled by sail and armed with smoothbore, muzzle-loading cannon yielded to steel vessels with steam engines and breech-loading, rifled guns. New forms of gunpowder, slow burning and smokeless, made weapons more effective, as did stronger materials in shells. Optical range finders and other fire-control devices increased accuracy. In an attempt to match improvements in weapons, steelmakers developed stronger forms of armor. Other advances in shipbuilding allowed the creation of new types of vessels or complete redesign of old forms. New doctrines of seapower, especially those of Alfred T. Mahan, set forth new roles for warships. In short, all major aspects of naval warfare were changing. One good summary describes the situation at the turn of the century:

> ...by the end of the nineteenth century the evolution of naval technology had produced the essential elements of the combat fleet of the pre-air age: the battleship, for carrying the brunt of offensive action, the cruiser, for support of the battleship and scouting and convoy duty, and the destroyer, for screening action, scouting defense against torpedo attacks, and convoy duty: in addition, the submarine, with its promise for both offensive and defensive operations, was soon to become a practical reality. Furthermore, these fleet units had been brought far toward the ideal of well-balanced and efficient fighting mechanisms, each protected by excellent armor to the extent required by the tactical doctrine governing its use, provided with armament that was constantly being increased in power, range, and accuracy, and powered by reliable engines that gave it the speed needed for performance of its mission.[21]

Along with the major changes in construction, propulsion, and ordnance came more limited but still highly significant developments. Chief among these was the introduction of numerous electrical devices on board ship. Telegraphs, electric lights, electric firing mechanisms, telephones, turret control

[20] Despite the importance of new technology to the development of the "new Navy," the way the U.S. Navy acquired or developed technical improvements in the period from the Civil War to World War I has received little study. The best general source I found was: Pittsburgh University Historical Staff, "Naval Research and Development in World War II" (unpublished manuscript written in 1950, available from the Navy Department library). Also very useful were numerous unpublished histories in the series "United States Naval Administrative Histories of World War II," cataloged and filed in the Navy Department library. There are a few published works that related directly to how the Navy acquired its technology in this period. These include: Albert Christman, *Sailors, Scientists, and Rockets* (Washington: Navy History Division, 1971); Capt. L.S. Howeth, *History of Communications-Electronics in the U.S. Navy* (Washington: GPO, 1963); and Taylor Peck, *Round-Shot to Rockets: A History of the Washington Navy Yard and U.S. Naval Gun Factory* (Annapolis: United States Naval Institute, 1949). Also helpful were *History of the Bureau of Engineering, Navy Department, During the World War* (Office of Naval Records and Library, Historical Section, Publication 5, GPO, 1922), and U.S. Bureau of Ordnance, *Ordnance Activities, World War, 1917-1918)* (Washington: GPO, 1920), though the information they provide on the prewar period is scant.

Numerous books describe broad aspects of the technological revolution and how it affected the Navy. Those I found to be most helpful were: John D. Alden, *The American Steel Navy* (Annapolis,: U.S. Naval Institute, 1972); Frank M. Bennett, *The Steam Navy of the United States* ... (Pitsburgh: Warren, 1896); Bernard Brodie, *Sea Power in the Machine Age* (Princeton, N.J.: Princeton University Press, 1941); Walter R. Herrick, *The American Naval Revolution* (Baton Rouge: Louisiana State University Press, 1966); Dudley W. Knox, *A History of the United States Navy* (New York: Putnam's, 1936; rev. ed., 1948); John D. Long, *The New American Navy*, two vols. (New York: Outlook, 1903); Donald W. Mitchell, *History of the Modern American Navy from 1883 through Pearl Harbor* (New York: Knopf, 1946); and Harold and Margaret Sprout, *The Rise of American Sea Power* (Princeton, N.J.: Princeton University Press, 1939).

[21] Pittsburgh University Historical Staff, *op. cit.* (note 20), pp. 16 and 17.

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motors, and ultimately radio changed many facets of shipboard operations—especially communication. Summarizing the overall effect of modernization, Secretary of the Navy William Whitney wrote in his annual report of 1885,

> A naval vessel at the present moment is a product of science. Taking the world over, it will be found that each part of her—her armor, her armament, her power, and the distribution of her parts or characteristics—each of these features of the completed vessel is absorbing from year to year the exclusive study of a class of scientific men. And as men of science throughout the world are continually stimulated to new discoveries and inventions, no vessel that can be built can be considered a finality in any particular.
>
> The problem of keeping pace with the march of improvement in these lines of industry is one of incalculable difficulty; and yet unless the Government is prepared to avail itself promptly of all the improvements that are made in the construction and equipment of its ships its expenditures are largely useless.[22]

NAVAL RESEARCH AND DEVELOPMENT PRIOR TO WORLD WAR I

The technical changes adopted by the Navy in the late 19th and early 20th centuries came largely from outside the service. Many, especially in the early stages of modernization, were copied from more advanced European nations. Others came from private American companies or independent inventors. For example, the Navy put pressure on the Bethlehem and Carnegie Steel Companies to develop new high-grade materials for shipbuilding.[23] Improvements in engineering equipment were also due largely to contractors, although Benjamin Isherwood and some of his followers in the Bureau of Steam Engineering made significant contributions in this field.[24] The inventor Elmer Sperry developed gyroscope stabilizers and gyrocompasses for the Navy.[25] Thomas Edison sold it storage batteries and other inventions.[26] The Bell Telephone Company produced telephones for ships. Marconi and other radio pioneers provided the first radio equipment.[27] Inventors in the Navy also made significant contributions. Bradley A. Fiske, for instance, developed a range finder, a telescopic gun sight, and a flying torpedo.[28]

Yet, like industry, the Navy realized that new technology was becoming too important to be left solely to the initiative of others. Slowly, it began to create its own organizations. Along with the general trend of modernization, the naval build up for the Spanish-American War helped foster them. None of the early establishments was set up explicitly as a research laboratory of the type that Edison, Inc., the General Electric Company, or other leading American firms had formed. The Naval Research Laboratory was the first institution of that sort built within the Navy. Rather they were specialized, limited facilities devoted primarily to development. Also, none of them was established to serve the Navy as a whole; each was under the cognizance of one of the several material bureaus. But we shall see that taken together, they provided a surprisingly broad internal capability for improving the Navy's technical base.

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[22] As quoted in *ibid.*, p. 59.
[23] John D. Long, *op. cit.* (note 20), vol. I, pp. 47ff.
[24] Edward William Sloan, III, *Benjamin Franklin Isherwood, Naval Engineer: The Years as Engineer in Chief, 1861-1869* (Annapolis,: United States Naval Institute, 1965).
[25] Thomas P. Hughes, *Elmer Sperry: Inventor and Engineer* (Baltimore: Johns Hopkins University Press, 1971), especially ch. VIII, "Brainmill for the Military."
[26] Josephson, *op. cit.* (note 8).
[27] Howeth, *op. cit.* (note 20), Part I.
[28] Bradley A. Fiske, *From Midshipman to Rear Admiral* (New York: Century, 1919).

The bureau system of organization, under which these facilities were subsumed, had been instituted in the Navy in 1842. Changes were made in 1862, but from then until after World War I, the administrative structure was relatively constant. The bureaus were: Steam Engineering (the name was changed to Engineering in 1920), Ordnance, Construction and Repair, Navigation, Yards and Docks, Medicine and Surgery, and Supplies and Accounts. The first three were the material bureaus and controlled the acquisition, use, and maintenance of naval equipment. Although the Bureau of Navigation and the Bureau of Medicine and Surgery did some scientific research, the material bureaus were principally responsible for the development of new technology.

Facilities of the Bureau of Ordnance

Prior to World War I, the Bureau of Ordnance had the largest number of facilities conducting experimental work. The bureau was responsible for developing and manufacturing most of the Navy's guns, powder, and torpedoes, and this required much experimentation. In all cases, the work was closely tied to manufacture.

The oldest establishment in the Bureau that conducted research was the Washington Navy Yard.[29] Set up in 1799 primarily for shipbuilding, the Yard went through numerous changes of function over the years. Experimental work in ordnance began in 1847, after the arrival of John A. Dahlgren. Under his guidance a new series of guns was designed that dramatically increased the firepower of naval ships. A period of stagnation followed the Civil War, but then, once the Navy became committed to modernization, research at the Yard flourished again. After 1886, the establishment was devoted almost solely to manufacturing naval guns and, indeed, came to be called the Gun Factory. By the end of the Spanish-American War in December 1898, it had become the most modern ordnance plant in the world. Experimental development was regarded as an important part of its operation.

The Naval Torpedo Station at Newport, Rhode Island, was established in 1869.[30] John Dahlgren, by then a Rear Admiral and head of the Bureau of Ordnance, was crucial in getting it started. At the station the Navy experimented with the four major types of torpedoes of the day (spar, automobile, towed, and controllable) in order to find the best type for the service. Until the early 20th century, work focused on experiment, development, and testing of devices supplied by private companies. Manufacture was limited to components or auxiliary apparatus. Then, in 1907, a Government torpedo plant was set up at the station to produce automobile torpedoes.

Newport had both chemical and electrical laboratories, and, in addition to studying torpedoes, the staff experimented with a variety of electrical equipment and explosives—most notably smokeless gunpowder. In 1888, chemists there started with results of previous European attempts to develop smokeless powder and began work on a safe and practical product for the U.S. Navy.[31] Although it took 10 years, success was achieved.

[29] Taylor Peck, *op. cit.* (note 20), is the principal source of information used here. Also employed was: U.S. Bureau of Ordnance, "U.S. Naval Gun Factory" (unpublished history in the series "U.S. Naval Administrative Histories of World War II," deposited in the Navy Department library, 1946).

[30] Richard D. Glasow, "Naval Response to an Innovation in Weaponry: The Establishment of the Newport Torpedo Station and the United States Navy's First Ten Years with Movable Torpedoes" (unpublished paper delivered at the Society for the History of Technology 20th annual meeting in Washington, D.C., Oct. 1977); W.J. Coggeshall and J.E. McCarthy, "The Naval Torpedo Station, Newport, Rhode Island" (manuscript article originally printed by the Torpedo Station Press, 1920; reprinted in 1944 by Remington Wood Co., Newport, Rhode Island); U.S. Bureau of Ordnance, "Naval Torpedo Station, Newport, Rhode Island" (unpublished history in the series "U.S. Naval Administrative Histories of World War II," deposited in the Navy Department library, 1946).

[31] Robert Henderson, "The Evolution of Smokeless Powder," U.S. Naval Institute *Proceedings* **30** (1904): 352-372.

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At Annapolis, the Bureau set up an experimental battery and proving ground for guns and powder in 1872.[32] In 1898, this was moved to Indian Head, Maryland. In both locations were tested powder, projectiles, cartridge cases, and armor plate. Some development work was also done. In addition, a factory was established at Indian Head in 1898 to manufacture the smokeless powder that had been developed by the chemists at Newport. Eventually the Newport staff was transferred to the new site so that experiment and production would be together.

Facilities of the Bureau of Construction and Repair

The Bureau of Construction and Repair had always worked closely with private contractors on developing ship designs. Not until the late 19th century, however, did it have an experimental facility for this work.[33] In 1899, David W. Taylor, then assistant to the Chief Naval Constructor, established a model basin at the Washington Navy Yard. Based on principles enunciated by the English engineer William Froude in the 1850s, this facility provided for the use of scale models in research and development. Taylor directed the work personally until 1910, when he became chief constructor of the Navy. The basin proved of enormous value and helped change the building of ships from an art to a science.

In 1913, the Bureau, under Taylor's direction, set up a wind tunnel for aeronautical research at the Washington Navy Yard.[34] Only the third built in the United States, it had a large 8 foot by 8 foot (2.4 by 2.4 meter) test section. Similar in conception to the model basin, the tunnel allowed the use of scale models of airplanes for studies of aircraft design. With its associated facilities, it was the center of the Navy's aerodynamical experimentation until the National Advisory Committee for Aeronautics was established during World War I.

Facilities of the Bureau of Steam Engineering

The main experimental facility for the Bureau of Steam Engineering was the Engineering Experiment Station in Annapolis.[35] This institution was something of an oddity in the Navy Department at the time. It was neither part of a manufacturing operation, as were the Bureau of Ordnance facilities, nor based on special experimental apparatus, as were the wind tunnel and the model basin. Rather it was a multipurpose institution. It was the establishment within the Navy most like a general research laboratory until NRL came into being.

The station was set up only after repeated requests by Engineer-in-Chief George Melville. In 1903, during his last year in that position, Melville finally persuaded Congress to authorize $400,000 for a laboratory building and equipment. When it came into operation in 1908, the principal duty of the institution was testing, not research. As one description says, "[Before] World War I...the Experiment Station functioned primarily as a 'go' or 'no go' gauge for the Bureau of Engineering to determine whether or not American-built machinery was or could be made suitable for our Navy."[36] Quality tests were made on numerous types of mechanical and electrical equipment that the Bureau had to authorize for naval service. Limited experiment and development also went on.

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[32] U.S. Naval Propellant Plant, Indian Head, Md., *Naval Proving Ground, Naval Powder Factory, and Naval Propellant Plant: People and Events from the Past* (Indian Head, Md.: U.S. Navy, 1961); "U.S. Naval Powder Factory," in U.S. Bureau of Ordnance, "Miscellaneous Activities," two vols. (unpublished history in the series "U.S. Naval Administrative Histories of World War II," deposited in the Navy Department library, 1945), vol. II, pp. 1-73.

[33] U.S. Navy, *David Taylor Model Basin: Information Booklet*, 7th ed. (Washington: GPO, 1957); U.S. Bureau of Ordnance, "U.S. Naval Gun Factory," *op. cit.* (note 29), pp. 332-342.

[34] J. Norman Fresh, "The Aerodynamics Laboratory (The First 50 Years)" (Washington: Department of the Navy, Aero Report 1070, Jan. 1964).

[35] Allen Phillip Calvert, "The U.S. Naval Engineering Experiment Station, Annapolis," *United States Naval Institute Proceedings* **66** (1940): 49-51; Wilson D. Leggett, "The U.S. Naval Engineering Experiment Station," *United States Naval Institute Proceedings* 77 (1951): 517-529.

[36] Leggett, *op. cit.* (note 35), p. 526.

Providing radio for the fleet was another duty of the Bureau of Steam Engineering. Radio equipment generally came from private companies, but the Bureau had several small laboratories to do its own experimenting and testing.[37] One of these, the U.S. Naval Radio Telegraphic Laboratory, was under the direction of Dr. L. W. Austin and located at the U.S. Bureau of Standards. Established in 1908 as the Navy's first radio laboratory, it came under the Bureau of Engineering in 1910. In 1915, it was supplemented by the Radio Test Shop, which the Bureau established at the Washington Navy Yard to help begin developing radio receivers and wavemeters. Some experimentation with radio was also done at several Navy yards.

Like other navies of the world, that of the United States was interested in developing oil as a fuel for naval vessels. To work on making it a suitable replacement for coal, the Bureau of Engineering established a fuel-oil test plant at the Philadelphia Navy Yard in 1909.[38]

LIMITS OF THE SYSTEM

As is clear from the preceding discussion, the Navy realized that modernization of the fleet required increased use of applied science and technology. The understanding was also apparent in the beginning of postgraduate study for selected Naval Academy graduates in 1897, first at the Academy and later at the Massachusetts Institute of Technology.[39] It is found in the formation of special boards to study the adequacy of naval technology, such as the Naval Liquid Fuel Board of 1902 and the Special Board on Naval Ordnance established in 1904. Naval budgeting procedures were even altered to allow funds to be directed specifically toward research and experiment.[40] Thus, in general, it was widely understood and accepted that the quality of the Navy depended fundamentally on new technology, and a variety of changes were made to help the service develop and use it properly.

In the early 20th century, however, many criticisms were still being raised about the Navy's openness to new ideas and its methods of technological advancement. Civilian inventors, for example, complained continually about the reception their ideas received. Speaking for them in 1911, Scientific American stated,

> It is a notorious fact that the inventor who approaches certain of the government departments in the hope that his invention will be investigated without prejudice and, if found meritorious, bought up at a price which will guarantee the inventor in selling his invention with the right to exclusive use, will find he has a hard road to travel. More often than not he will experience unnecessary delays, to say nothing of considerable financial loss and ultimate disappointment.[41]

Inside the Navy, Bradley Fiske and others made similar statements.

The Navy's in-house research facilities also received negative critiques. They were small, limited, and largely devoted to development and test. There was almost no advanced research being done by well-trained professionals. Critics argued that the service was too tied to the problems of the present and was backward in its use of advances in science and engineering. It was not keeping up with leading American industries. When World War I erupted in Europe, there were many who believed reforms were essential in the way the Navy made its technical improvements.

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[37] L.S. Howeth, *op. cit.* (note 20); L.A. Gebhard, *The Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory,* (Washington: NRL Report 8300, 1979), pp. 1-25.
[38] Pittsburgh University Historical Staff, *op. cit.* (note 20), pp. 72-77.
[39] *Ibid.*, pp. 70-71.
[40] *Ibid.*, p. 78.
[41] *Scientific American*, Nov. 18, 1911, p. 444.

UNCLASSIFIED

A MERGING OF TRADITIONS

The men who brought the Naval Research Laboratory into being thought in terms of the experiences and ideologies inherent in the dual traditions of industrial research and the Navy's existing research-and-development program. These traditions provided the conceptual framework for the establishment of the institution. The Naval Research Laboratory was planned as an industrial research laboratory within the naval establishment.

Far more was involved in the creation of the Laboratory, however, than the rational merging of the two traditions. Politics, personalities, the pressures of war, the intricacies of Congressional funding, and the differences between naval officers and civilian engineers all had an important influence in how the Laboratory progressed from plan to fact. This story is the subject of the next chapter.

UNCLASSIFIED

3. CREATION OF THE NAVAL RESEARCH LABORATORY

AN INTERVIEW WITH EDISON

In May 1915, the war that had raged in Europe for almost a year was troubling many Americans. On May 7, the British liner *Lusitania*, with 128 U.S. citizens among the 1200 noncombatant passengers aboard, was sunk by a German submarine. Most passengers lost their lives. President Wilson issued stern warnings to Germany to halt its campaign against unarmed ships but received no satisfactory response. The possibility of American entry into the war was rapidly increasing.[1]

Late in the month, Thomas Edison expressed his views on how America should respond to the dangerous situation in an interview with Edwin Marshall, a *New York Times* reporter. His thoughts were published in the Sunday magazine on May 30. One of his recommendations was as follows:

> I believe that...the Government should maintain a great research laboratory jointly under military and naval and civilian control. In this could be developed the continually increasing possibilities of great guns, the minutiae of new explosives, all the technique of military and naval progression without any vast expense.[2]

Thus did he express the idea that eventually would be embodied in the Naval Research Laboratory.

At the time, however, such a laboratory was a subsidiary thought. Edison's main point in the interview was that war was not yet imminent and that America should not mobilize a large standing Army. Better preparation, he insisted, would be the mobilization of material:

> We should not take our men from industry and overtrain them, but should have 2,000,000 rifles ready, in perfect order, even greased, with armories equipped with the very best machinery to begin upon short notice in case the work should require the manufacture of a hundred thousand new firearms every day.[3]

The men to use the machines, he believed, like the minutemen of the American Revolution, could be assembled and trained quickly.

Mobilization of science and invention, especially in the research laboratory, was the key to developing good weapons. "When the time came, if it ever did, we could take advantage of the knowledge gained through [the] research work and quickly manufacture in large quantities the very latest and most effective instruments of warfare."[4]

Edison's position also had political implications that would make it palatable. Marshall alluded to them at the beginning of his article:

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[1] Lloyd Scott, *Naval Consulting Board of the United States* (Washington: GPO, 1920), pp. 7-9.
[2] *The New York Times*, May 30, 1915, V, pp. 6 and 7.
[3] *Ibid.*
[4] *Ibid.*

> [Edison] believes that we should be invincible. In the following interview, he, for the first time, tells the world how he thinks we may accomplish this without so burdening ourselves with taxation as to reduce our living standards and morale to the European level.[5]

Technology was both cheaper and more expendable than men, and mobilizing it was less controversial than calling citizens to arms.

Fig. 1 — Thomas Edison believed the Government needed a research laboratory similar to his own.

Edison was 68 years old in 1915. His greatest inventions—the phonograph, the electric light, and motion pictures—were being mass produced and were affecting the life of almost every American in some way. The inventor had become a well-known public figure. Newspapermen would interview him on almost any interesting issue, for Edison rarely failed to give them good copy. One biographer characterizes this part of his life as one of "canonization" and describes his appeal to the public in this way:

> He was a man of science, yet had the "common touch".... His legendary success story, like his expressive physiognomy—reproduced millions of times on his cylindrical records—was familiar to all men. In short, he was almost universally regarded as one of the real makers of America, one whose career millions would have liked to emulate, and

[5] *Ibid.*

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> so, well suited to serve as a folk hero. His very appearance, and his widely reported sayings, racy, humorous, and original in flavor, but strengthened the will of the multitude to idolize him.[6]

Edison's views on the relation of science and invention to the war were sure to carry weight with the public.

ESTABLISHMENT OF THE NAVAL CONSULTING BOARD

Secretary of the Navy Josephus Daniels read the interview and liked Edison's point of view. Having spent most of his career as editor of the Raleigh, N.C., *News and Observer*, Daniels always followed newspaper reporting carefully and understood well the power of publicity. In this article he perceived the possibility both of enlisting Edison's expertise as an inventor for the Navy and of gaining his direct support for the cautious political stance on preparedness then advocated by the Wilson Administration.[7]

Daniels had been named Secretary of the Navy in 1913 as a reward for early endorsement and strong support of Woodrow Wilson during the 1912 Presidential campaign.[8] The Secretary had never before held a major national office and was inexperienced in naval affairs. Nonetheless, he was determined to act independently and decisively. A populist and basically a pacifist, he made many reforms that embittered top naval officers. He stressed the importance of enlisted men and established schools aboard ship to educate them. He abolished the officers' wine mess in accord with his prohibitionist tendencies. He emphasized the need for civilian control of the Navy and refused to create a general staff of naval officers to centralize their power. Most importantly, he refused to accede to demands to build up the Navy quickly.[9] When Edison's interview appeared, Daniels' leadership was under sharp attack. Edison's assistance, he realized, might help him blunt it.

On May 31, the day after the article appeared, Daniels drafted a note to Edison, but then he put it aside for further consideration.[10] On June 7, he sent a revised, more vaguely worded letter asking the inventor for assistance. He said:

> I [want] to take up with you [a] matter to which I have given a great deal of thought—a matter in which I think your ideas and mine coincide, if an interview with you recently published in the New York Times was correct. There is a very great service that you can render the Navy and the country at large and which I am encouraged to believe from a paragraph in Mr. Marshall's interview, you will consent to undertake as it seems to be in line with your own thoughts.

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[6] Matthew Josephson, *Edison: A Biography* (New York: McGraw-Hill, 1959), p. 434.

[7] My interpretation of Daniels' political motivations is shared by others. See Joseph L. Morrison, *Josephus Daniels; the Small-d Democrat* (Chapel Hill: University of North Carolina Press, 1966), pp. 71-72. See also "Mr. Robins on the Relation of the Naval Consulting Board to Various Bureaus and Bureau Chiefs, 4/7/19" in the file "Thomas Robins," box 37(?), Naval Consulting Board Records, record group 80, National Archives Building. (Unfortunately, the records of the Naval Consulting Board have been reboxed by the National Archives since I did my research. Therefore, my references in this chapter to these records do not correspond exactly to the present storage arrangement.)

[8] Morrison, *op. cit.* (note 7), pp. 45-49; Arthur S. Link, *Woodrow Wilson and the Progressive Era* (New York: Harper and Row, 1954), p. 28.

[9] Morrison, *op. cit.* (note 7), *passim.*; Donald W. Mitchell, *History of the Modern American Navy, from 1883 through Pearl Harbor* (New York: Knopf, 1947), pp. 158-167. See also Bradley Fiske's bitter criticisms of Daniels in *From Midshipman to Rear Admiral* (New York: Century, 1919), pp. 555-560.

[10] Draft of the letter by Josephus Daniels to Thomas Edison, May 31, 1915, box 76, Josephus Daniels papers, Library of Congress Manuscript Division.

Fig. 2 — Secretary of the Navy Josephus Daniels (above) called Thomas Edison to serve the Navy as a technical advisor.

> One of the imperative needs of the Navy, in my judgment, is machinery and facilities for utilizing the natural inventive genius of Americans to meet the new conditions of warfare as shown abroad, and it is my intention, if a practical way can be worked out...to establish, at the earliest possible moment, a department of invention and development, to which all ideas and suggestions, either from the service or from civilian inventors, can be referred for determination as to whether they contain practical suggestions for us to take up and perfect.[11]

The Secretary also addressed the need of the Navy for a research laboratory:

> The Department is...unprovided with the best facilities for work of pure experimentation and investigation, with the exception of our testing station at Annapolis, which is, as yet, a small affair. Most of all, as I have said, there is no particular place or particular body of men, relieved of other work, charged solely with the duty of either devising new things themselves or perfecting the crude ideas that are submitted to the Department, by our naturally inventive people.[12]

Leaving aside the question of whether the Navy should build a new research facility, the Secretary asked Edison to assist the service in getting better use from its existing establishments and also, if he consented, to do experimental work for the Navy in his own laboratory at West Orange.

[11] The original is in box 1 of the papers on the Naval Consulting Board in the Thomas Edison papers, Edison National Historical Site, West Orange, New Jersey. On it is written, in Edison's hand, "Hutch [Miller Reese Hutchinson]—Note and return with comments, E." A copy of the letter also appears in Scott, *op. cit.* (note 1), pp. 286-288.
[12] *Ibid.*

UNCLASSIFIED

Detailed plans for the "department of invention and development" or the "board," as Daniels also called it in the letter, were left to be worked out later, after Edison had responded. Daniels was pointedly clear, however, on the importance of Edison's participation:

> Such a department will, of course, have to be eventually supported by Congress, with sufficient appropriations made for its proper development....To get this support, Congress must be made to feel that the idea is supported by the people, and I feel that our chances of getting the public interested and back of this project will be enormously increased if we can have, at the start, some man whose inventive genius is recognized by the whole world to assist us in consultation from time to time on matters of sufficient importance to bring to his attention. You are recognized by all of us as the one man above all others who can turn dreams into realities and who has at his command, in addition to his own wonderful mind, the finest facilities in the world for such work.[13]

Edison agreed to help. Soon after receiving Secretary Daniels' letter, he sent his chief assistant, Miller Reese Hutchinson, to Washington to say that Edison would assist in organizing a board to advise the Navy on technology and invention.[14] Daniels happily announced the decision to reporters and shared with them the letter he had sent Edison.

The event was front-page news. The *New York Times*, proud of the part it had played in the story, gave especially detailed coverage. "Edison Will Head Navy Test Board," ran the headline, "...Best Engineering Genius of the Nation to Act with Naval Officers in Strengthening Sea Power."[15] Journalists speculated on whether the new body would be organized as a bureau (and thus be at the highest level in the Navy Department) or not, who would be included in it, and what its functions would be.

Publicity was particularly extensive because the Naval Consulting Board, as the new body eventually was called, was the first attempt during the wartime period (1914-1918) to mobilize science and invention along a broad front at the national level. Later, other organizations would be created for the same general purpose, most notably the National Research Council and the War Committee of Technical Societies, but in 1915, the Naval Consulting Board was unique.[16]

While the process of forming the Board and choosing its members ran its course, it continued to be both a hot news item and a subject for editorial comment. Daniels was achieving just the publicity

[13] *Ibid.* Daniels elaborated further on his idea of the Board and its purpose in an interview with Edwin Marshall of *The New York Times.* See the issue of Aug. 8, 1915, IV, p. 14.

[14] In later years, Hutchinson said he had been behind the whole affair. He wrote Daniels, "I conceived [the Naval Consulting Board] shortly before the photographically recorded visit with which Mrs. Daniels and you honored me. I drummed it into Mr. Edison's head until he took cognizance of the need and allowed me to use him as its sponsor. Then I got Ed Marshall to interview Mr. Edison on the subject and, when the article appeared in the *Times*, I paid Marshall's expenses to Washington, to see you about it. You wrote Mr. Edison you would form such a Board. He wrote, on the margin of the letter, 'Hutch: What do you think?' and sent the letter to my office upstairs in the Laboratory. I hopped the Congressional for Washington, called on you at your home, and said Mr. Edison would be glad to head such a Board." Letter from Hutchinson to Daniels, Sept. 12, 1935, in the file "Hutchinson, Miller R, 1932-35, -36 and undated" in box 84, Josephus Daniels papers, Library of Congress Manuscript Division. Other than Hutchinson's own word, I have found no evidence that this story is true.

[15] *The New York Times*, July 13, 1915, p. 1.

[16] Scott's book (note 1) was the official history of the Naval Consulting Board and is still the most complete published source on its activities. Unfortunately, it was written before the Naval Research Laboratory came into existence. For more recent, if more limited, appraisals of the Consulting Board and its activities, see Thomas P. Hughes, *Elmer Sperry: Inventor and Engineer* (Baltimore: Johns Hopkins University Press, 1971), ch. IX, and Daniel J. Kevles, *The Physicists* (New York: Knopf, 1977), chs. VIII and IX. There is no general history of science and engineering in World War I. Kevles' book is the best general work situating the Naval Consulting Board's activities among the work of other wartime organizations.

he desired for his limited plan for preparedness.[17] Civilian inventors, for example, were delighted to hear that the Navy was going to pay greater attention to their ideas for technical improvements. Their long-standing criticisms seemed finally to be having an effect.[18] Indeed, most remarks on Daniels' action expressed hopeful approval. There was little analysis of his motivation. One sober commentator in *Colliers* on October 2, 1915, however, offered only qualified praise of the new plan. He pointed out that there was as much politics as desire for technical improvement in what Daniels had done:

> · · · we have long suspected that Mr. Daniels, a newspaper editor by profession, possesses not only certain traits of the sensational journalist, but of the sensational journalist's half brother, the press agent. Therefore, while we have a reasonable belief in his civilian board, we wish to put ourselves on record as expecting no miraculous devices to develop, as hoping that the new board will take an early opportunity to declare frankly and firmly in favor of extensive, normal naval preparation, and that the somewhat sensational quality of the Secretary's new device will not for a moment divert the public mind from the less picturesque need for a very considerable enlargement of the United States Navy. Such an enlargement is not to be brought about by hand waving and incantations, but by the customary process of appropriating funds, designing ships and causing them to be built by the sweat of men's brows in ship yards—a lamentably slow and laborious process.[19]

ORGANIZATION AND ACTIVITIES

On July 15, Secretary Daniels visited Edison at his home in Lewellyn Park, New Jersey, to discuss plans for the Naval Consulting Board.[20] The ideas of other interested parties were also solicited, and by the end of the month, the Secretary had decided to make the body represent major national engineering societies.[21] The hope was that this plan would encourage continual interaction between the Board and the societies. Edison was responsible for choosing which ones would be represented, and he asked 11 societies to name two members each to the new body. Later there was extensive debate on whether he had made the best selection.[22] Two significant omissions were the American Physical Society and the National Academy of Sciences—the organization created in 1863 as the official scientific advisor to the Government. Their exclusion perhaps made clear that Edison was interested in invention and engineering, not theoretical science, but it definitely impaired the effectiveness of the Board.

Eleven societies each named 2 members apiece. Edison was named as chairman, and his assistant, Miller Reese Hutchinson, was designated as a special delegate, so the Board had 24 members. Table 1 shows who the members were, shows what organizations they represented, and lists the officers.[23] The First Vice-Chairman, William Saunders, and the Secretary, Thomas Robins, were the most active leaders. Edison concerned himself only with matters that happened to interest him; he devoted almost no effort to making the organization effective as a whole.

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[17]The link of the Naval Consulting Board to the Wilson Administration's stand on preparedness was made clear at the first meeting of the Board. President Wilson then addressed the organization on national defense and said that the nation should be prepared "not for war but for defense, and very adequately prepared." It was the President's first public declaration in favor of adequate national defense. See *The New York Times*, Oct. 7, 1915, p. 1.

[18]*Scientific American*, Sept. 25, 1915, p. 266. Bradley Fiske was happy about the organization and hoped, vainly, that he would be put in charge of it. Fiske, *op. cit.* (note 9), pp. 580-591.

[19]*Colliers*, Oct. 2, 1915. See also note 7.

[20]*The New York Times*, July 16, 1915, p. 1.

[21]The suggestion to make the Board representative of engineering societies seems to have come from Frank Sprague. See the letter from Sprague to Daniels, July 19, 1915, in the file "Secretary Daniels," box 29(?) (see note 7), Naval Consulting Board Records, record group 80, National Archives Building.

[22]See the letter from Miller Reese Hutchinson to Josephus Daniels, Nov. 6, 1915, box 84, Josephus Daniels papers, Library of Congress Manuscript Division.

[23]This information comes from Scott, *op. cit.* (note 1), pp. 11-15. For further biographical information on members, see *Scientific American*, Oct. 2, 1915, pp. 301ff, and Oct. 9, 1915, pp. 326ff.

UNCLASSIFIED

Table 1 — Original Officers and Members of the Naval Consulting Board

OFFICERS:	
Chairman:	Thomas A. Edison
First Vice-Chairman:	William L. Saunders
Second Vice-Chairman:	Peter Cooper Hewitt
Secretary:	Thomas Robins

(In January, 1917, the titles of the officers were altered to be respectively: President, Chairman, Vice-President, and Secretary.)

MEMBERS AND THE ORGANIZATIONS THEY REPRESENTED:	
Secretary of the Navy:	Thomas Edison and Miller Reese Hutchinson
American Aeronautical Society:	Matthew B. Sellers and Hudson Maxim
American Chemical Society:	Leo H. Baekeland and Willis R. Whitney
American Electrochemical Society:	Lawrence Addicks and Joseph W. Richards
American Institute of Electrical Engineers:	Frank J. Sprague and Benjamin G. Lamme
American Institute of Mining Engineers:	William L. Saunders and Benjamin B. Thayer
American Mathematical Society:	Robert S. Woodward and Arthur G. Webster
American Society of Aeronautical Engineers:	Elmer A. Sperry and Henry A. Wise Wood
American Society of Automotive Engineers:	Howard E. Coffin and Andrew L. Riker
American Society of Civil Engineers:	Andrew M. Hunt and Alfred Craven
American Society of Mechanical Engineers:	William L. Emmet and Spencer Miller
Inventor's Guild:	Thomas Robins and Peter Cooper Hewitt

(The officers and membership of the Board altered only slightly during the time the organization was active: October 1915-November 1918. The most important change was the appointment of Capt. William S. Smith as Navy liaison officer to the Board on December 7, 1915.)

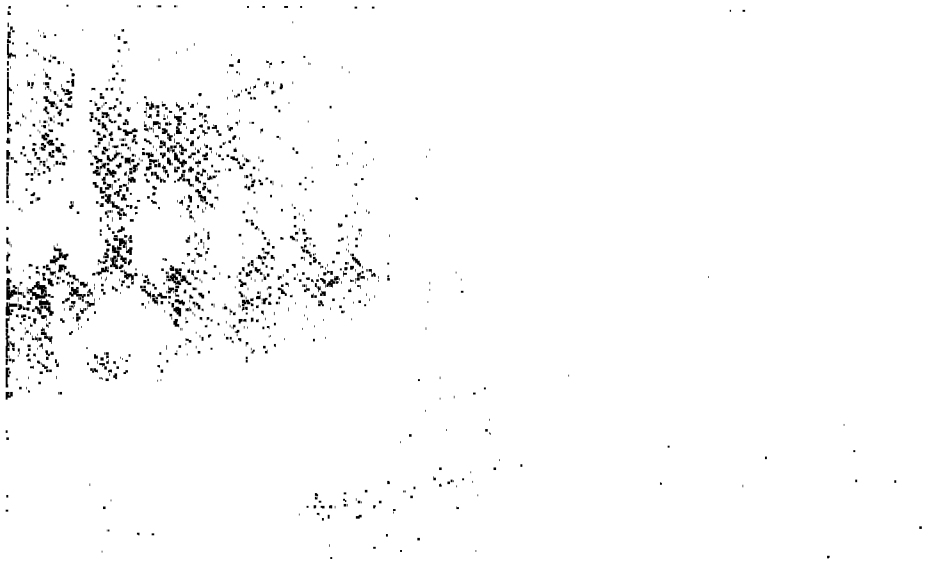

Fig. 3 — The Naval Consulting Board (shown above with other Navy Department officials) strongly supported the idea of a research laboratory but could not agree on all details of the plans for it.

Although some of America's best known inventors—such as Orville Wright and Simon Lake—were not on the Board, its members were well-respected and accomplished engineers. They were talented individuals determined to help improve the technology of the Navy. They saw membership on the Board as a great honor and served without compensation throughout the war. But for the Board to succeed, it had to gain support of the Navy's material bureaus: they would have to agree to its suggestions before the ideas would be developed and used. From the beginning, relations between the Board and the bureaus were strained. The new organization always had a dubious status, both officially and unofficially. The political motivations for which Daniels had created it were a major handicap.

The final choice of a name is one example. In preliminary discussion, the Board was usually called "The Naval Advisory Board," but top naval officers, piqued by Secretary Daniels' independent action in forming the body, argued that its purpose was to act as a consultant—speaking only when asked, rather than making suggestions on its own initiative. To placate the officers, the name became "The Naval Consulting Board."[24]

Official liaison with the material bureaus was slow in coming. On December 7, 1915, several months after the Board was organized, an "Office of Inventions" was established under the Secretary of the Navy, and Captain William S. Smith was put in charge. Smith became the "Technical Aide to the Secretary of the Navy" and also served as liaison officer to the Naval Consulting Board. He was to refer its suggestions to the appropriate men in the bureaus. Smith kept in close contact with the organization and attended most of its meetings, but he never found the suggestions to be of much use. Not surprisingly, he did not get along very well with many members, including Edison.[25]

Congress eventually recognized the Board but only tacitly and in a backhanded way: in August 1916, expenditures of $25,000 were authorized for its operations, but the legislation said nothing about its place within the Navy hierarchy or its purpose. And despite repeated requests from some of the officers, Secretary Daniels refused to press for more definite action.[26]

The general history of the Naval Consulting Board, which has been written elsewhere,[27] falls outside the present subject. In sum, although the body remained in operation until after the armistice ending World War I, and despite the dedication of its members, most of its activities had little effect on the Navy. There were some important achievements. The Board did make a significant contribution in organizing an industrial preparedness campaign.[28] It also assisted in organizing and conducting an effort within the Bureau of Engineering to combat the submarine, the most pressing technical problem of the war.[29] Individually, many members worked closely with naval officers on technical problems.[30] The

[24]See pp. 19, 48 and 49 in the document "Opinions of Members as to Future of Naval Consulting Board," box 31(?) (see note 7), records of the Naval Consulting Board, record group 80, National Archives Building.

[25]See the letter from Thomas Robins to William Saunders, Feb. 11, 1919, in the file "W.L. Saunders," box 38(?) (see note 7), records of the Naval Consulting Board, record group 80, National Archives Building, and also the letter from William Saunders to Josephus Daniels, Aug. 7, 1917, in the file "Civilian Naval Consulting Board," box 504, Josephus Daniels papers, Library of Congress Manuscript Division.

[26]See the section on "Organization" in the "Digest of Minutes" of the Board, *passim.*, but especially p. 12. The "Digest of Minutes" is in box 30(?) (see note 7), Records of the Naval Consulting Board, record group 80, National Archives Building. One reason Daniels did not want to give the Board a stronger legal position was because some of its members, most notably Edison and Elmer Sperry, had business dealings with the Government and might be accused of conflict of interest. See Josephus Daniels, *The Cabinet Diaries of Josephus Daniels, 1913-1921*, E. David Cronon, ed. (Lincoln, Neb.: University of Nebraska Press, 1963), p. 138.

[27]See notes 1 and 16. Archival records of the Naval Consulting Board are in record group 80 in the National Archives. The "Digest of Minutes" in box 30(?) (see note 7) gives the most general information on the Board and its activities.

[28]See Scott, *op. cit.* (note 1), ch. II, and Robert D. Cuff, *The War Industries Board: Business-Government Relations During World War I* (Baltimore: Johns Hopkins University Press, 1973).

[29]See Scott, *op. cit.* (note 1), ch. IV; *History of the Bureau of Engineering, Navy Department, During the World War* (Office of Naval Records and Library, Historical Section, Publication 5; GPO, 1922), pp. 47-73, and Harvey C. Hayes, "Detection of Submarines," *Proceedings of the American Philosophical Society* **59** (1920): 1-47.

[30]See Scott, *op. cit.* (note 1), ch. XI, and Hughes, *op. cit.* (note 1), ch. IX.

UNCLASSIFIED

review of ideas from the public, however, the function stressed by Secretary Daniels, failed to yield much. Of 110,000 inventions considered, only 110 were judged to be of any value at all. Only one was put into production.[31] In general, the Board simply was not an effective type of organization for focusing the power of civilian inventors and engineers on Navy problems, and it never worked well with the Navy bureau system.

The material bureaus believed that they were well equipped to solve their own problems and needed no help from the well-publicized group of civilian experts. Any contributions made by the Board, they feared, would be seen as an indication of previous incompetency by them. As Thomas Robins, secretary of the organization, said in 1918 when discussing why the Board was accomplishing so little:

> Our present trouble is not due to the Navy nor to the Board. It is due to a plan which does not take into consideration some of the most fundamental qualities of human nature. It cannot work. The Naval Consulting Board, if it be continued, must not work *for* the Navy; it must work *as* the Navy.[32]

The problem was the difficulty of applying civilian expertise to solving Navy technical problems in the proper way, in a way that was effective administratively. As we shall see, the same problem was apparent in the Board's most ambitious single effort: creating a research laboratory.

THE LABORATORY PROJECT: SUCCESS

Secretary Daniels said nothing definite about a new Navy research laboratory in his initial letter to Edison. But, in accepting the invitation to help form the Naval Consulting Board, the inventor did not give up his desire for building such an institution for the Government—or now, more particularly, for the Navy. He firmly believed the ideas he had expressed in *The New York Times.* At a meeting on July 15, 1915, he spoke to the Secretary about the facility and convinced him it was a good plan. Afterward, Daniels told reporters that he hoped to have a "great naval laboratory in Washington."[33] In September, the Secretary said further, "The Navy has...been seriously handicapped by the lack of an adequate central establishment where the ideas of its own officers as well as those suggested by civilians could be taken up and patiently developed in the same way that such ideas are handled in great manufacturing establishments."[34]

Edison came to the first meeting of the Naval Consulting Board, which was held on October 7, 1915, with a plan for the laboratory in hand. A comittee of five members was quickly composed to consider his ideas. On it were Edison; Willis Whitney, head of the General Electric Research Laboratory; Howard Coffin, Vice-President and Consultant Engineer of Hudson Motor Company; Leo Baekeland, inventor of the first practical plastic—Baekelite; and Robert S. Woodward, a well-known academic physicist and President of the Carnegie Institution of Washington. Together the men represented broad experience in both academic and industrial research; all were strong advocates of research institutions.

Edison's plan had several notable provisions. The laboratory was to be large and well-equipped for research, development, testing, and limited production. The cost was estimated to be about 5 million dollars, and the annual operating budget about 2.5 million dollars. The plan called for a naval officer to direct the operation, a significant provision, because Edison would later insist that a civilian be in charge. The staff, which was to work closely with the Naval Consulting Board, would be primarily civilian scientists and engineers. In general, the proposed institution was modeled quite closely on

[31] Scott, *op. cit.* (note 1), p. 125.
[32] "Opinions of Members as to Future of Naval Consulting Board," *op. cit.* (note 24), p. 50. The italics are not in the original.
[33] *The New York Times*, July 16, 1915, p. 2.
[34] *The New York Times*, Sept. 20, 1915, p. 9.

Edison's own laboratory at West Orange. The proposal even mentioned a "motion picture developing and printing department."[35]

There was some debate on the plan by the Committee. Baekeland, Whitney, and Woodward initially argued that research was the most important function of the laboratory and that its facilities should be much more limited than Edison believed.[36] Their thoughts were based on their own experience in chemical and physical research. Baekeland later described the situation in this way:

> Dr. Whitney, Dr. Woodward, as well as myself, took into consideration primarily the chemical and physical departments of the projected laboratory, and it was very interesting to see that although we had prepared the recommendations independently, we very closely agreed as to cost and operation and as to the amount of money which would be required in maintaining that laboratory and operating it. I must say, however, that when Mr. Edison got up, and Mr. Coffin, and the mechanical engineers began to show what ought to be done in those laboratories, we all three felt like small fry, like "pikers" to use a current expression. We realized that the needs for such a laboratory are much vaster than anything which we chemists or physicists could accomplish.[37]

Edison got his way. His plan was accepted by the committee and then the entire Board without substantial revision.[38] Like Edison, other Board members thought the Navy needed a sophisticated new research facility to make its technology equal to the best in the world. The issues of the size and function of the institution, however, were destined to arise again.

Immediately after the meeting, the plan was announced to the public, and again the Naval Consulting Board was front page news. Reaction was generally favorable—even the Navy bureau chiefs, despite their mixed feelings about the Board, expressed support of the proposal. But the cost was quickly attacked. *Scientific American*, for example, said,

> The plan as outlined...calls for the creation not of a laboratory but of a navy yard, with docks capable of accommodating a modern dreadnought and with a modern railway large enough to build experimental submarines of 1,500 tons. Now it seems to us...that in an experiment of this kind...the work should be allowed to proceed by a natural process of growth.[39]

The Navy, a magazine devoted to presenting the Navy point of view, stated,

> The service has pointed out that experimental work of a laboratory character, as well as under service conditions, is already being carried on extensively in many branches of the naval establishment. The service seems to doubt that a large central laboratory, operated perhaps to the exclusion of other experiments, would have advantages compensating for the increased cost.[40]

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[35] *The New York Times*, Oct. 8, 1915, p. 1.
[36] L.H. Baekeland, "The Naval Consulting Board of the United States," *Metallurgical and Chemical Engineering*, Dec. 15, 1915.
[37] U.S. Congress, 64th, 1st session, House, *Hearings Before the Committee on Naval Affairs* (Washington: GPO, 1916), p. 3392.
[38] See "Laboratory" section of the "Digest of Minutes," *op. cit.* (note 26).
[39] *Scientific American*, Oct. 23, 1915, p. 354.
[40] *The Navy* **9** (Nov. 1915): 239-240.

UNCLASSIFIED

As this editorial emphasized, the relation of the new laboratory to the rest of the naval engineering establishment had not been made clear. So far, Edison and his colleagues had acted almost as if the Navy had no experimental facilities whatsoever.

In reaction to the public outcry over the cost of the laboratory, the Board had decided by its third meeting, in December 1915, to ask—at least initially—for only 1.5 million dollars. Additional monies could be acquired later, once the laboratory proved itself.

The first major step in implementing Edison's plan was gaining Congressional support. For this, Edison himself was indispensable. Rear Admiral Robert Griffin, Chief of the Bureau of Engineering, put the situation this way:

> Congress will never appropriate the amount of money necessary for such a laboratory for the Navy itself; that is, for any department of the Navy, [or] the bureaus of the Navy; [but] I feel sure that if Mr. Edison will appear before the Naval Committee with all the plans and all the data he has, or a little more complete, it will make a profound impression on the Naval Committee and I am sure it will result in their giving us what we want.[41]

On March 15, 1916, five Board members—Edison, Baekeland, Coffin, Hunt, and Saunders—accompanied by Secretary Daniels, went before the House Naval Affairs Committee to argue for the new institution. Edison did make a strong impression. He spoke with complete confidence, even an air of bravado, as he outlined the great things it would do. Again he put as much emphasis on development as research. If need be, he claimed, the laboratory could build a new submarine in as little as 15 days.[42]

The Congressmen may have wondered at Edison's exaggerated claims, but they, like Secretary Daniels, clearly understood his popularity. With the war raging in Europe, they knew better than to question his advice on the needs of the Navy for research and development. And, lest they forget, William Saunders reminded them after Edison left,

> You heard this morning the testimony of the most distinguished scientist in the world, Mr. Edison. Nobody questions that today. Some think he is the greatest man in the world; he is certainly the greatest scientist in the world, and when we measure greatness, we must measure it by achievement.[43]

The Board got what it wanted. The Committee accepted the plan for the laboratory and approved expenditures of 1 million dollars. Later, after slight debate, the Senate went along. The Naval Appropriations Act for fiscal year 1917 thus included the following section:

> EXPERIMENTAL AND RESEARCH LABORATORY: For laboratory and research work on the subject of gun erosion, torpedo motive power, the gyroscope, submarine guns, protection against submarine, torpedo, and mine attack, improvement and development in submarine

[41] "Laboratory" section of the "Digest of Minutes," *op. cit.* (Note 26).
[42] The discussion is recorded in the printed hearings, *op. cit.* (note 37), pp. 3343-3403.
[43] *Ibid.*, p. 3378.

> engines, storage batteries and propulsion, aeroplanes and aircraft, improvement in radio installations and such other necessary work for the benefit of the Government service, including the construction, equipment, and operation of a laboratory, the employment of scientific civilian assistants as may become necessary, to be expended under the direction of the Secretary of the Navy (limit of cost not to exceed $1,500,000), $1,000,000.[44]

A supplemental appropriation of $500,000 was formally made on March 4, 1917. Thus the Naval Consulting Board had the 1.5 million dollars it wanted to build the laboratory.

The quick and successful action that led to the granting of these funds marked the high point of the attempt by the Naval Consulting Board to build a research laboratory for the Navy. What followed was disagreement and delay.

THE LABORATORY PROJECT: FAILURE

After obtaining funds for the laboratory, the Board had to determine where to build it and what, exactly, its organization and function should be. The appropriation contained only general instructions. So a committee was established and charged to make recommendations. Its members were Edison, as chairman, Lawrence Addicks, Leo Baekeland, Thomas Robins, Frank Sprague, and Willis Whitney.[45]

Choosing a location was important to many people besides the members of the Naval Consulting Board. As far back as the first announcement of the project, in October 1915, local boosters had petitioned the Board to consider their cities. Congressmen, always on the lookout for such morsels to skewer for their districts, also had been interested. The Board, to be fair, initially considered 61 locations but then quickly narrowed the list to three: Annapolis, Maryland, Sandy Hook, New Jersey (on the northern tip of the New Jersey coastline, across the bay from New York City), and Washington, D.C.

After several months of deliberation, two opposing views on which site was best emerged from the committee. There was also disagreement on the function of the laboratory and who would head it. On December 9, 1916, a pair of conflicting reports was presented to the entire Consulting Board for consideration, one endorsed by Edison, and the other endorsed by everyone else.[46]

The majority believed that the laboratory should be built in Annapolis. There were four principal reasons:

- The Engineering Experiment Station already existed there, and since the cost of the new institution and its equipment was now set at 1.5 million dollars instead of 5 million, it seemed prudent to expand the experiment station, changing it into a general Navy laboratory. Also, the majority was now well aware of the existing Navy research program. "It is vital," they wrote, "that in order to make the most of the available funds there should be no unnecessary duplication of equipment and facilities which already exist in other Government plants, and no avoidable expenditures outside of buildings and equipment."[47]

- Because the Naval Academy was in Annapolis, the laboratory might attract many visitors and win their sympathy and support.

[44] United States, *Statutes at Large, Public Laws*, vol. 39, (64th Congress, 1st session) ch. 417, 1916, p. 570.
[45] "Laboratory" section of the "Digest of Minutes," *op. cit.* (note 26).
[46] Copies appear in Smith, *op. cit.* (note 1), pp. 225-232, but the majority report was rewritten after the meeting to take Edison's point of view into account.
[47] *Ibid.*, p. 225.

UNCLASSIFIED

- Annapolis was close enough to Washington to allow easy access to all Government resources and information there but distant enough to disallow complete control by Navy top brass. Thus an atmosphere conducive to long-range research might be possible.

- Annapolis had a good harbor for seagoing ships but at the same time was well protected by the wide Eastern Shore of Maryland.

The majority now disagreed with Edison on the purpose of the new institution. They believed that its main function should be research, not development. They argued,

> [The laboratory] is not intended to rival in investment, equipment, or output great industrial factories or machine shops or do the work legitimately belonging to the navy yards or gun shops. It is primarily intended for a research laboratory.[48]

The dispute that had arisen at the first meeting of the Consulting Board between Edison and research-minded men such as Baekeland and Whitney now reemerged. The opposing viewpoints reflected the difference between Edison's laboratory at West Orange and others in America, such as the one Whitney had established at General Electric.

Finally, the disagreement concerned who should administer the laboratory. At first, all Board members had hoped that the facility would essentially be under their control. As Hudson Maxim later said, "We certainly believed at [first] that the Laboratory was to be for the use of the Naval Consulting Board, and that although it would be essentially under the auspices of the Navy it was to serve the special purposes of the Naval Consulting Board."[49] However, the continuing difficulties the Board was experiencing in trying to work together with the Navy bureaus had altered the view of most members. The report of the majority of the committee stated,

> As to the manner in which [the laboratory] should be operated, the idea that the work should be more or less under the direction of bureau chiefs, individually or collectively, or the members of the Naval Consulting Board, should be discarded, for such would lead to many-headed and inefficient organization. Instead the laboratory should be under a responsible officer of high rank, to whom the various bureau chiefs should turn over their problems, accompanied by all available information. And so too, with regard to problems which may be submitted to the Naval Consulting Board.[50]

Making the laboratory an integral part of the naval establishment had become an important concern. If the institution was to be effective, it had to work *as* the Navy, not just for it.

Edison believed that the laboratory was basically his project and that he had the right to decide where it would be and how it would function. Like his own establishment, he thought it would depend primarily on his inventive talent. As he later told Secretary Daniels,

> It is fixed in my mind, whether right or wrong, that the public would look to me to make the Laboratory a success, and that I would have to do 90% of the work. Therefore, if I cannot obtain proper conditions to

[48] *Ibid.*, p. 230.
[49] "Opinions of Members as to Future of Naval Consulting Board," *op. cit.* (note 24), p. 19.
[50] Scott, *op. cit.* (note 1), p. 226.

> make it a success, I would not undertake it nor be connected with it in the remotest degree, or be held responsible for its success.[51]

As a location for the laboratory, Edison favored Sandy Hook. That way the facility would be located near his own establishment at West Orange and would also be near New York City, where labor, all types of supplies, and well-trained technical men were readily available. Being near New York had helped make his laboratory a success, and he felt it would be equally important for the new Navy facility. He continued to insist that the new institution should concentrate on development. Of research he now said:

> I do not think that scientific research work to any great extent will be necessary. Research work in every branch of science and industry, costing countless millions of dollars and the labor of multitudes of men of the highest minds, has been carried on for many years. All of this has been recorded, and yet only a ridiculously small percentage has as yet been applied and utilized. It is therefore useless to go on piling up more data, at great expense and delay while we are free to use this ocean of facts.[52]

Throughout his career, Edison had been able to rely heavily on existing scientific knowledge when making his highly successful inventions. Once again, his experience strongly influenced what he thought about the Navy laboratory.

The most important difference between the inventor and the rest of the committee, however, concerned who should administer the facility. Edison now insisted that a civilian be in charge. He expressed this point at a meeting of the committee in November 1916:

> [The] laboratory always in my mind has been for only one purpose, to work under civilian conditions away from naval and government conditions. A purely civilian...laboratory. Not to have anything to do with the Navy except that if any naval officer has an idea he can go there and have it made.[53]

This view, like his predilection for Sandy Hook, merely hardened in the future.

Edison's statement reflected an antagonism toward the Navy that he had developed since he agreed to form the Naval Consulting Board and devote most of his energies to work on Navy problems. Edison had come to believe that no naval officer really understood scientific research, and that none could properly administer a successful research-and-development laboratory. All the reasons for this feeling are not clear, but two can be discerned. First, most of the suggestions he was sending to the Navy through Secretary Daniels were not being followed, despite Daniels' personal efforts to assure they got attention.[54] Edison later complained,

> I made about forty-five inventions during the war, all perfectly good ones, and they pigeon-holed every one of them. The Naval officer

[51] Letter from Thomas Edison to Josephus Daniels, Dec. 22, 1916, in box 76, Josephus Daniels papers, Library of Congress Manuscript Division.

[52] Scott, *op. cit.* (note 1), p. 23.

[53] Notes on the meeting of the Laboratory Site Committee, Nov. 26, 1916, p. 8, in box 25 (?) (see note 7), Records of the Naval Consulting Board, record group 80, National Archives Building.

[54] See Josephus Daniels, *op. cit.* (note 26), pp. 193 and 222 for examples.

UNCLASSIFIED

> resents any interference by civilians. Those fellows are a close corporation.[55]

Second, the Navy was testing for use in submarines a new type of storage battery being developed by Thomas Edison, Inc. The inventor and his company had been working on batteries for this purpose since 1910, when four naval officers visited him and discussed problems with those being used.[56] Partly due to pressure from Daniels, the Bureau of Engineering had agreed to test the batteries on the submarine E-2.[57] On January 15, 1916, an explosion occurred in the vessel while it was moored in New York harbor, and four men were killed. Subsequently, a well-publicized inquiry put the blame squarely on Edison's battery, despite his argument that operating procedures on the submarine had caused the disaster.[58] Miller Reese Hutchinson, Edison's assistant on the Naval Consulting Board, and his man in charge of storage battery development, wrote an angry letter to an official in the Bureau of Engineering, with a copy going to Secretary Daniels, asking for vindication:

> We are now basking in the light of having sold something to the Navy Department that is a gold brick and being a pair of crooks not worthy to be trusted with the confidential relation that members of the Naval Consulting Board should and must bear to the Navy Department if any results are to be achieved by that Board. I realize that it is a difficult situation to handle, but it can never be handled by sitting tight and doing nothing. If the Navy Department does not want to avail itself of our services, we want to know it.[59]

Despite this pressure, the Bureau of Engineering stood by the results of its investigation.

At the meeting of the Consulting Board in December 1916, both reports of the laboratory-site committee were discussed fully, but the majority report prevailed. The Board recommended to Secretary Daniels that the laboratory be built in Annapolis.[60] Daniels refused to act. He wanted unanimity on the decision. He obviously could not accept Edison's view and use Navy funds to build a laboratory that would be wholly civilian in operation, and he simply would not accept the majority view unless Edison concurred. He wrote to Edison of his decision:

> In view of these conflicting opinions, it would seem to me I should approve the majority report. I have not yet acted solely because of my deference to you and my great confidence in your judgement.[61]

A new committee of the Naval Consulting Board was formed to convince Edison to change his mind, but it failed. The same stubborness that had characterized the inventor's search for a practical

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[55] As quoted by Matthew Josephson, *op. cit.* (note 6), p. 454.

[56] Ronald W. Clark, *Edison: The Man Who Made the Future* (New York: Putnam's, 1977), p. 219.

[57] See the letters from Miller R. Hutchinson to Josephus Daniels in box 84, Josephus Daniels papers, Library of Congress Manuscript Division.

[58] The affair may be followed in reports of *The New York Times* beginning with that on January 16, 1916, II, p. 1. The notebook entitled "Explosion of H_2 from Edison storage batteries installed on USS E-2" in the NRL historical file, Historian's office, NRL, Washington D.C., contains several items of interest on the E-2 incident, including a report made for Thomas Edison by Lamar Lyndon, a New York consultant, that was favorable to Edison, and a letter of rebuttal from R.S. Griffin, Chief Engineer of the Navy, to the Secretary of the Navy.

[59] Letter from Miller R. Hutchinson to Louis Howe, Dec. 23, 1916, in the file "Hutchinson, M.R., 1914-1915," box 84, Josephus Daniels papers, Library of Congress Manuscript Division.

[60] "Laboratory" section of the "Digest of Minutes," *op. cit.* (note 26).

[61] Letter from Josephus Daniels to Thomas Edison, Dec. 20, 1916, box 76, Josephus Daniels papers, Library of Congress Manuscript Division.

incandescent light bulb, which the world of science had called impossible, now determined his stand on the research laboratory.

The disagreement halted progress on the project. Meanwhile, the war went on. Work that might have been done at a new research institution was done elsewhere. The Bureau of Engineering expanded radio research at several locations and established two groups to study antisubmarine warfare, the first at Nahant, Massachusetts, in cooperation with the Naval Consulting Board, and the second at New London, Connecticut, with the assistance of the National Research Council. Other bureaus made similar increases in their research work. With the need so obvious, the failure to build the laboratory seemed to many on the Board an egregious mistake.

In February 1918, Frank Sprague and several other Board members took the initiative to get the laboratory project moving again. To gain support, they decided to recommend that the facility be built on the location always favored by top Navy officers: the grounds of a Navy magazine in Bellevue, an area in the southeast tip of the District of Columbia. The Board as a whole went along and passed a resolution stating, in part,

> WHEREAS, On account of the change of conditions wrought by the war in which we have now been engaged for nearly a year, a second choice of the Site Committee in favor of Washington may now be given more favorable consideration, therefore be it
>
> RESOLVED, That the Naval Consulting Board recommends for immediate consideration of the Bellview [sic] Magazine site in Washington, and the prompt construction of the proposed laboratory on plans approved by the Navy Department.[62]

Following this meeting, two Board members were instructed to draw up plans for a laboratory designed for construction on the Washington site. They were made and approved.[63] Significantly, although the plans gave a detailed description of the physical plant of the institution, they said nothing about its administration or scientific program. These disputed issues were left to be settled after the facility was constructed. Copies of the plans were given to Secretary Daniels and the chiefs of the material bureaus in mid-June. Again, however, the Secretary refused to act, because Edison, who had taken no part in the Board's new initiative, would not support it.

On November 11, 1918, World War I ended. In December, the Naval Consulting Board met to decide the Board's future. Edison, as usual, was not present. Most members were willing to continue meeting if the Secretary of the Navy desired them to do so, but all freely admitted they could accomplish little unless they developed a better means of cooperating with the bureaus. On the subject of the laboratory, all were agreed: the facility should be built. But all felt that they had no power to do anything further about it.[64]

At the request of Secretary Daniels, the Naval Consulting Board did continue to exist after this meeting, but it ceased to be active. It was up to the regular naval establishment to decide whether and how the laboratory was to be constructed.

[62] "Laboratory" section of the "Digest of Minutes," *op. cit.* (note 24).
[63] *Ibid.*
[64] "Opinion of Members as to Future of Naval Consulting Board," *op. cit.*, (note 24).

UNCLASSIFIED

NAVAL OFFICERS TAKE COMMAND

In late 1919, William S. Smith, the officer who had served as the Navy liaison to the Naval Consulting Board throughout the war and now had attained the rank of Rear Admiral, took the initiative. He convinced the chiefs of the material bureaus to advise Secretary Daniels to go ahead and build the laboratory. They sent a memorandum to the Secretary on October 1, 1919, that read in part,

> It is recommended that the Bureau of Yards and Docks proceed with the construction of the Naval Experimental and Research Laboratory as approved by the preliminary committee representing the Bureaus of Steam Engineering, Construction and Repair, Ordnance and Yards and Docks, of which Rear Admiral W.S. Smith was the senior member, and that the construction of the buildings and the equipment contained should follow after the general lines of the report of the Naval Consulting Board.[65]

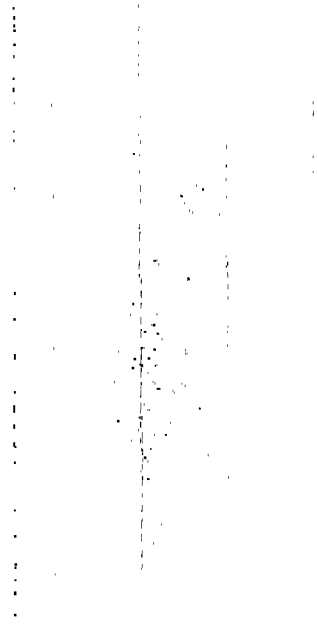

Fig. 4 — Rear Admiral William Strother Smith led a campaign by naval officers to build the research laboratory in Washington and later became NRL's first director.

This time, Secretary Daniels, perhaps sensing that if he did not act now the facility would never be built, went along. On October 20, he authorized construction on the Bellevue site. After having made the decision, he wrote Edison asking his consent. The inventor was as adamant as ever. He replied,

[65] Memorandum from the Engineer-in-Chief, Chief Constructor, and Chief of the Bureau of Ordnance to the Secretary of the Navy, Oct. 1, 1919, file "A1" in box 1, job order 7184, record group 181, records of NRL, Washington National Records Center, Suitland, Md.

> I have not changed my mind in the least about the location of the Laboratory. Nor have I changed my opinion that such a Laboratory should not be under the control of Naval officers, either directly or indirectly. I still think that the Secretary of the Navy only should have control through civilians. If Naval officers are to control it the results will be zero. This is my experience due to association with them for two years and noting the effects of the system of education at Annapolis.
>
> When you are no longer Secretary and have returned to business, I want to tell you a lot of things about the Navy that you are unaware of.[66]

Despite Edison's opposition, the project continued, albeit slowly. A contract for construction was finally granted in November 1920. Work began on December 6 but progressed slowly. The five buildings composing the laboratory would not be ready for use until mid-1923.

Daniels' action insured that the institution would exist but did not settle the thorny question of how it would be administered. Admiral Smith and the Bureau chiefs had not addressed that issue when they prodded the Secretary to act, and he had made no decision on it since. They, of course, expected that a naval officer would be placed in charge and that the laboratory would operate like the other Navy test-and-development facilities. Edison, whom Daniels still hoped would cooperate, continued to want a civilian. The rest of the Board members preferred a civilian but were willing to accept the other alternative if necessary.

The choice rested with Secretary Daniels, at least while he remained in office. In a final attempt to get Edison's cooperation, the Secretary decided to support his position. He wrote him,

> I...am in entire harmony with your view that there must be, in order to fulfill the purpose for which the appropriation was made, perfect cooperation between civilians and naval officers, and as to the plan of doing it, in my annual report I am saying there must be civilian direction and I hope this civilian direction will be undertaken under such plans and policies as you will outline....I do not think we will have the least trouble about arranging this, and I would like you to work out a plan for such organization and management.[67]

Still the stubborn inventor, now 73 years old, refused to go along. Instead of cooperating, he resigned from the Naval Consulting Board altogether.[68] Thus he forfeited his final chance to help determine the policy of the institution he had conceived and for which he had obtained Congressional funding. With no support from either Edison or the bureau chiefs, Daniels' recommendation had little force. Besides, the pace of construction meant that the ultimate decision would be left up to his successor, Edwin Denby.

[66] Letter from Thomas Edison to Josephus Daniels, Nov. 7, 1919, box 76, Josephus Daniels papers, Library of Congress Manuscript Division.

[67] Letter from Josephus Daniels to Thomas Edison, Nov. 19, 1920, box 76, Josephus Daniels papers, Library of Congress Manuscript Division.

[68] Letter from Thomas Edison to Josephus Daniels, box 76, Josephus Daniels papers, Library of Congress Manuscript Division.

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Soon after the new Secretary took office, William Saunders, chairman of the Naval Consulting Board, wrote to him to express the opinion of the membership:

> A large majority have a very earnest interest in the future direction of this laboratory....They hold the officers of the Navy in high respect as executives to carry out the policy of the Department, but they believe that by education, training and experience, those officers are not in a position to develop new things through experimental work; that this belongs essentially to those who are free and uninfluenced by traditions....It is the hope of a large majority of this Board that you will decide to place a civilian director in charge of this laboratory.[69]

Nonetheless, Denby decided to support the officers. On September 13, 1921, although the laboratory was still under construction, he named Rear Admiral William S. Smith its first director. This meant in addition that Smith and the bureau chiefs would decide the policy of the institution. Indeed, they already had a general order for this purpose in draft form. It circulated in the Navy Department and underwent slight revision, then it was issued as General Order 84 on March 25, 1922. It was the basic statement of Laboratory policy. Neither Edison nor any other member of the Consulting Board had anything to do with writing it. The order read in part,

> 1. As provided in the Naval Appropriations Act approved 19 August 1916, the Experiment and Research Laboratory is hereby established and placed under the Assistant Secretary of the Navy. The Laboratory shall be under the direction of a naval officer, not below the rank of captain, who will be designated "The Director of the Experiment and Research Laboratory" and be attached to the Office of the Assistant Secretary of the Navy....
>
> 2. The Laboratory staff shall consist of such officers as may be detailed from time to time or assigned to work on special problems, civilian scientific assistants as provided for by law, and such technical assistants as may be employed.[70]

THE SCIENTIFIC PROGRAM

As important as who controlled the laboratory was what it would do. The wording of the appropriation for the institution, which was based on Edison's plans, actually had almost no influence. Few of the problems which Congress had supposedly created the laboratory to study would ever be investigated there. Instead, like the administrative structure of the institution, the scientific program ultimately was formulated by naval officers.

Admiral Smith wanted the laboratory to be a facility serving all the material bureaus, and initially they all expressed interest in having it do work for them. Moreover, they all agreed that its purpose should be research. As Admiral Charles B. McVay, Chief of the Bureau of Ordnance, wrote in July 1922,

[69] Letter from William Saunders to Edwin Denby, Mar. 17, 1921, in file "W. Saunders," box 38 (?) (see note 7), records of the Naval Consulting Board, record group 80, National Archives Building.
[70] The complete order appears in Appendix A to this dissertation.

> This Bureau has a number of field experimental stations. It is believed that other bureaus are similarly equipped. The Bureau therefore considers that the operation of the Laboratory as an experimental station would be but additional to the already existing facilities of the several bureaus.
>
> On the other hand, the Navy Department has not any center of pure scientific research. It is believed that the Laboratory can and should build up a research organization of able scientists and skilled naval officers, in order to conduct purely scientific researches into such branches of science as affect naval material and the use thereof, and as are not adequately covered by existing naval organizations.[71]

The bureaus, therefore, generally agreed with the purpose of the laboratory as described by the majority report of the Naval Consulting Board laboratory-site committee. Their enthusiasm for having work done there, however, evaporated when they learned that they would have to pay for it.

Financing the laboratory had become difficult. Since the supplemental appropriation of 1917, no additional monies had been granted by Congress. To get operating expenses for fiscal year 1923, Captain E. L. Bennett, who had succeeded Admiral Smith as Director on December 21, 1921, went before the Naval Appropriations Committee on March 23, 1922, to ask for $100,000. He had originally hoped for $300,000, but that sum had already been cut by 2/3 during internal Navy review. And Bennett could not convince Congress even to give him the smaller amount. Without the war and without a popular advocate like Edison, arguments for the special needs for scientific research had little influence on the legislators. No money at all was appropriated for fiscal year 1923, and there was even some discussion of changing the purpose of the facility.[72] Only numerous pleas a year later shook loose $100,000 for fiscal year 1924. In fact, only nominal yearly funds were granted to the institution until the mid-1930s.

The small direct appropriation could cover not all costs, as had originally been hoped, but only overhead and salaries for a fraction of the employees. Thus, most personnel and research expenditures would have to be charged to the bureaus for the work they ordered. Upon learning this condition, all bureaus but one decided against using the laboratory, for they realized that doing so would mean curtailing existing programs elsewhere.

The exception was the Bureau of Engineering. Several high-ranking officers, led by the head of the Radio Division, Commander Stanford C. Hooper, thought the facility was an excellent location for regrouping and centralizing the Bureau's sound research, which was primarily concerned with the detection of submarines, and its radio research. Both activities had expanded during World War I, but they had become scattered among numerous Navy yards and stations. The Bureau Chief, Admiral J. K. Robison, was hesitant to commit funds to the new institution but finally agreed to the plan of his subordinates. On February 12, 1923, it was authorized in a Bureau memorandum entitled "Centralization of Radio and Sound Research at the U.S. Naval Research Laboratory at Bellevue."[73] Hooper later remembered his role in shaping the function of the Naval Research Laboratory in this way:

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[71] Memorandum from the Chief of the Bureau of Ordance to the Secretary of the Navy, July 26, 1922, NRL historical file, Historian's office, NRL, Washington, D.C.

[72] See the documents labeled "Estimate of the Situation" and "Naval Research and Experimental Laboratory" in the papers of E.G. Oberlin, Naval Historical Foundation, Washington Navy Yard, Washington, D.C., and also the *Annual Report of the Navy Department* (Washington: GPO) for 1922 and 1923.

[73] File "A1" in box 1, job order 7184, record group 181, records of NRL, Washington National Records Center, Suitland, Md.

UNCLASSIFIED

> Rear Admiral W. S. Smith sent a circular letter around to the bureaus to ask them to report how much space each bureau required for each activity and whether they desired any special arrangements of the space and so on. I was quick to seize on that as our great opportunity to finally have a radio research laboratory for the Bureau rather than having it scattered all around from New London to Pensacola and Annapolis and the different Navy Yards and the Bureau of Standards and Anacostia.
>
> So I immediately applied and told him we would like very much to have the top floor, the third floor of the new building....I was very hopeful that I could get at least part of that floor. I asked for the top floor primarily because I thought I would be lucky even to get [that]. But after receiving my letter Admiral Smith came in to see me one day and was very much pleased that I had made the request. He told me that not a single desk of any Bureau had requested any space or help there at the Naval Research Laboratory except my division. So he said, "You can have the whole place. You just tell me what you want to do down there and send down your men and the money and I will have it done just the way you say and your men will be directly under your division." Well, that was wonderful news and that made it possible for us to move in and start the first real Naval Research Laboratory such as we have now.[74]

Thus, soon after the institution opened on July 2, 1923, the 24 men of the research staff were organized into two divisions: Radio and Sound. The Radio Division was composed of personnel who had come from the Naval Radio Research Laboratory at the Bureau of Standards and the Aircraft Radio Laboratory at the Naval Air Station, Anacostia, D.C. The Sound Division comprised men transferred from the Annapolis Experiment Station, where they had worked since being previously transferred from New London, Connecticut. When operations began, the men simply continued the work they had already been doing for the Bureau of Engineering. For the Sound Division, this meant experimentation on devices to detect submarines. For the Radio Division, it meant a broad research effort including work on radio propagation, radio communication, radio direction finding, radio control, and radio standards and instrumentation.[75] Work in high frequencies soon became the hallmark of the Radio Division in most of these areas.

LEGACY OF THE BOARD

Thomas Edison and the Naval Consulting Board had little to do with the final stages of the creation of the Naval Research Laboratory. Their inability to come to a unanimous decision on a plan for the institution and their failure to make the Board itself an effective, permanent body meant that the facility they had formulated was actually built by others in the Navy Department. Nonetheless, a strong effect was exerted on the institution by the Board's thoughts and intentions—especially because the early administrators of the facility agreed with many of them.[76]

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[74] Transcript of tape recordings on "Radio-Radar-Sonar" p. 67R160 and 67R161, box 38, Stanford C. Hooper papers, Library of Congress Manuscript Division.

[75] Louis A. Gebhard, *Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979), pp. 31-39.

[76] It is reported that even Edison himself later changed his mind about the Laboratory and wrote a gracious letter to the Assistant Director saying his objections to the Laboratory as it had been established were apparently without foundation. A. Hoyt Taylor, *The First 25 Years of the Naval Research Laboratory* (Washington: Navy Department, 1949), p. 4.

The Board had wanted a laboratory independent from control by the bureaus and designed to serve the entire Navy by investigating all areas of science pertinent to the Service. They had wanted it to have a predominantly civilian atmosphere and to concentrate on research rather than routine test or development work typical of other Navy experimental facilities. To a large extent, as we shall see in the following chapters, NRL became that kind of institution.

Not everything developed as the Board had hoped, of course. The Laboratory was much smaller than they had desired. It had no direct ties to civilian engineering or scientific societies. Its policy of serving as a general Navy laboratory was more of a hope than a reality, due to the lack of support by the material bureaus. Its ability to retain a civilian atmosphere and devote resources to research was restricted somewhat by its position within the naval service. Many officers in the bureaus did fail to understand the institution and its importance. But given these limitations, the dream of the Board to create a central scientific research laboratory in the Navy was realized. And unlike the Board itself, the institution was able to operate successfully within the naval establishment. Finally, the policies that were followed at the Laboratory allowed it to increase scientific knowledge and develop much new technology for the Navy, just as the Board had expected. Radar is a good example.

In conclusion, it is interesting to note that although the Naval Consulting Board became inactive after World War I, it did not go out of existence until the 1940s. Most of its members, who remained well-known and respected figures among American engineers, were happy with the Laboratory for which they had been partly responsible. The early directors of NRL wisely kept in contact with them and often spoke at the annual dinner meeting they continued to hold over the years. On several occasions when the Laboratory desperately needed outside pressure put on Congress or on officials in the Navy Department, the alumni of the Naval Consulting Board were called upon, and they gladly lent their support.

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4. ANTECEDENTS OF THE RADAR PROJECT (1922 to 1930)

THE DISCOVERY OF 1922

Creating a device that used radio waves to detect objects was an idea conceived independently by a number of early radio scientists; it first occurred to two Navy engineers in September 1922, about 8 months before the Naval Research Laboratory opened. Later both men would be transferred to NRL, and the early experimentation they had done would be closely related to the radar project there, once it was established. Now, however, they were stationed at the Naval Aircraft Radio Laboratory of the Naval Air Station in Anacostia, a section of the District of Columbia.

The men, Dr. Albert Hoyt Taylor and Leo C. Young, were studying experimental equipment using high-frequency waves as part of a general search for new communication channels for the Navy.[1] In their transmitter, they used a 50-watt tube designed to oscillate at low frequencies but wired to produce vibrations at around 60 megahertz. Their "superheterodyne" receiver was a device whose basic principles had only recently been published by Dr. Edwin Armstrong.[2] The set was crude but satisfactory for pioneering research.

In one experiment, the men turned on the transmitter, placed the receiver in an automobile, and, like tourists in a new town, drove around the station to see what they could find. Quickly they learned that steel buildings were reflecting the signals and setting up standing waves. These were particularly noticeable in doppler effects when the receiver was moving. Other objects, when situated between the transmitter and the receiver, would blank out reception completely.

In hopes that the wave pattern would be less complicated over the water, where there were fewer obstructions, Taylor and Young drove the receiver to Haines Point, a location across the Potomac from the Naval Air Station. Young later described what occurred:

> As we got farther up towards the city end of Haines Point [closer to downtown Washington, D.C.], we began to lose our signal as we went behind the big willow trees there. So we decided to put the equipment out of the car, on the seawall, and see what happened. While we began making observations, we began to get quite a characteristic fading in and out—a slow fading in and out of the signal. It didn't take long to determine that that was due to a ship coming up around Alexandria.[3]

With their set Young and Taylor had detected the presence of a wooden steamer, the *Dorchester.* The vessel's passage in and out of the path between the radio transmitter and receiver had created the variable interference patterns. Quickly they realized that this phenomenon might be extremely useful.

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[1]There are several sources on the 1922 discovery. The only contemporary one is the letter Taylor wrote to the Bureau of Engineering: letter of Sept. 27, 1922, from the Commanding Officer, U.S. Naval Air Station, Anacostia, D.C., to the Bureau of Engineering, in the NRL historical file, Historian's office, NRL, Washington, D.C. Taylor discussed the work retrospectively in *Radio Reminiscences* (Washington: NRL, 2nd printing, 1960), pp. 90 and 91. Young commented on it in a tape-recorded reminiscence he made in 1953 on his role in the development of radar. It is on reels 150 and 151 in the collection. "History of Radio-Radar-Sonar" in the papers of Stanford C. Hooper, Library of Congress Manuscript Division (duplicates of the tapes for auditing are in the Library of Congress Sound Division). Henry Guerlac wrote about it in *Radar in World War II* (unpublished history of Division 14 of the National Defense Research Committee, 1947) pp. 58-60. Other accounts are either based on these or are less informative than they.

[2]Edwin Armstrong, "A New System of Short Wave Amplification," *Proceedings of the Institute of Radio Engineers* **9** (1921): 3-11.

[3]Young's taped reminiscence (note 1).

Within a few days, Taylor dispatched a letter to the Bureau of Engineering to describe the work he and Young had done on high frequencies and ask for further support. To justify his request, he stated,

> If it is possible to detect, with stations one half mile apart, the passage of a wooden vessel, it is believed that with suitable parabolic reflectors at transmitter and receiver, using a concentrated instead of a diffused beam, the passage of vessels, particularly of steel vessels (warships) could be noted at much greater distances. Possibly an arrangement could be worked out whereby destroyers located on a line a number of miles apart could be immediately aware of the passage of an enemy vessel between any two destroyers in the line, irrespective of fog, darkness or smoke screen. It is impossible to say whether this idea is a practical one at the present stage of the work, but it seems worthy of investigation.[4]

To this letter, the Bureau of Engineering made no response.[5] No one there seemed excited about the possibilities of radio detection, at least based on the results Taylor and Young had attained so far. Unfortunately, there is not even a record of the discussion of the idea by its judges. However, Stanford C. Hooper, then head of the Bureau's Radio Division, commented when thinking back on the early detection work,

> We were long convinced...that because of the lack of a proper generator or tube, which could generate short waves...such a system would be impractical for application to ships or planes....Consequently the Bureau did not actually put priority on this work, as compared with more recent and urgent projects, and the small funds available.[6]

Without further support and with many other problems demanding attention, Taylor and Young abandoned the idea of a radio detection project. They did nothing further to initiate one until a second important discovery was made in 1930. Thus died the first possibility of a program to build radio detection equipment for the U.S. Navy.

Several aspects of this episode should be noted. First, it developed from research on high-frequency radio. Indeed, the same subject would lead Taylor and Young back to the idea of radio detection in 1930. The existence of an extensive high-frequency radio program at NRL was a necessary requirement for its development of radar.

Second, the importance of institutional support is clear. Because Taylor and Young were involved in organized, administered research, they had to gain approval from their superiors to undertake a major research effort. Unless and until the Bureau of Engineering agreed to support the work, little could be done. Obviously it was not technical difficulties but rather the Bureau's decision that put a stop to the investigation at this time. It should be pointed out, however, that Taylor and Young did not now push hard for a project. If they had firmly believed that a radio detection program should be given high priority, they would have appealed more strongly, as they did later.

[4]Taylor's letter (note 1).
[5]Taylor, *Radio Reminiscences* (note 1) p. 91; Young's taped reminiscence (note 1). Extant Bureau of Engineering records contain neither the letter nor any information related to it.
[6]Statement of Stanford C. Hooper prefacing the reminiscence by Young (note 1).

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On the surface, it appears that both the Bureau and the two scientists were shortsighted in not realizing the importance of radio detection at this time. Taylor himself drew this conclusion in retrospect.[7] But it may also be true that the decision to do nothing further was both justified and better for the development of radar in the long run. Although beginning a project in 1922 might have produced some useful equipment earlier, it might also have led to frustration and failure. The entire field of high-frequency radio, on which radar depended, was as yet immature. Moreover, even if useful equipment had been produced, it might easily have been crude and cumbersome, and its existence might well have inclined the Navy against supporting development of the more sophisticated equipment that was possible later.[8] All that is certain is that a chance to develop radio detection equipment was passed up. One cannot know what form of equipment, if any, might have resulted had it been taken.

Finally, although Taylor and Young temporarily put aside their interest in radio detection, they continued to work for the Navy and to work with high-frequency radio. It was they who would lead radar development at NRL once that development did get started. When the idea surfaced again, they recalled and referred back to their brief study in 1922 and its results. In that sense, the earlier work served as a prelude to the development of radar.

There would be no radar project until 1930. During the intervening period, however, NRL became a good location for the investigation because of the way the research staff and research policy were established and the way the early work of the Radio Division progressed. The remainder of this chapter will be devoted to these subjects.

TAYLOR AND YOUNG

Hoyt Taylor and Leo Young were both transfered to NRL when it opened in 1923. Taylor became chief radio scientist, and Young became one of his top assistants. Because of their involvement in the radar project, it is important to know more about the two men.

Taylor was born in Chicago in 1879 to an advertiser who had little interest in technical matters. Almost nothing is known of his boyhood except what Taylor himself wrote. "Long before I ever had any higher schooling," he said,

> I was constructing simple voltaic cells with zinc and copper plates in acidulated water, stringing a number of them up in series, trying to make a carbon arc and an induction coil. At this time I was living in a small village named Wilmette, a few miles north of [the] Chicago city limits. Wilmette was then a town of only a few hundred people and was a very rural community indeed. I attended high school in the neighboring city of Evanston where I sopped up all the mathematics, physics, and chemistry I could get hold of.[9]

Throughout his high school years, he continued to experiment with electrical devices, especially the telegraph.

He aspired to college, but family finances made his choice of schools limited; he ended up studying engineering at Northwestern University's Evanston campus, beginning in 1896. As he said frankly

[7]Taylor, *Radio Reminiscences* (note 1) p. 91.

[8]It has been argued that just this sort of complacency did hinder the Navy later in making decisions about replacing some of its long-wave radar with microwave equipment.

[9]Taylor, *Radio Reminiscences* (note 1) p. 1. Almost all the biographical material related in this account derives from this source.

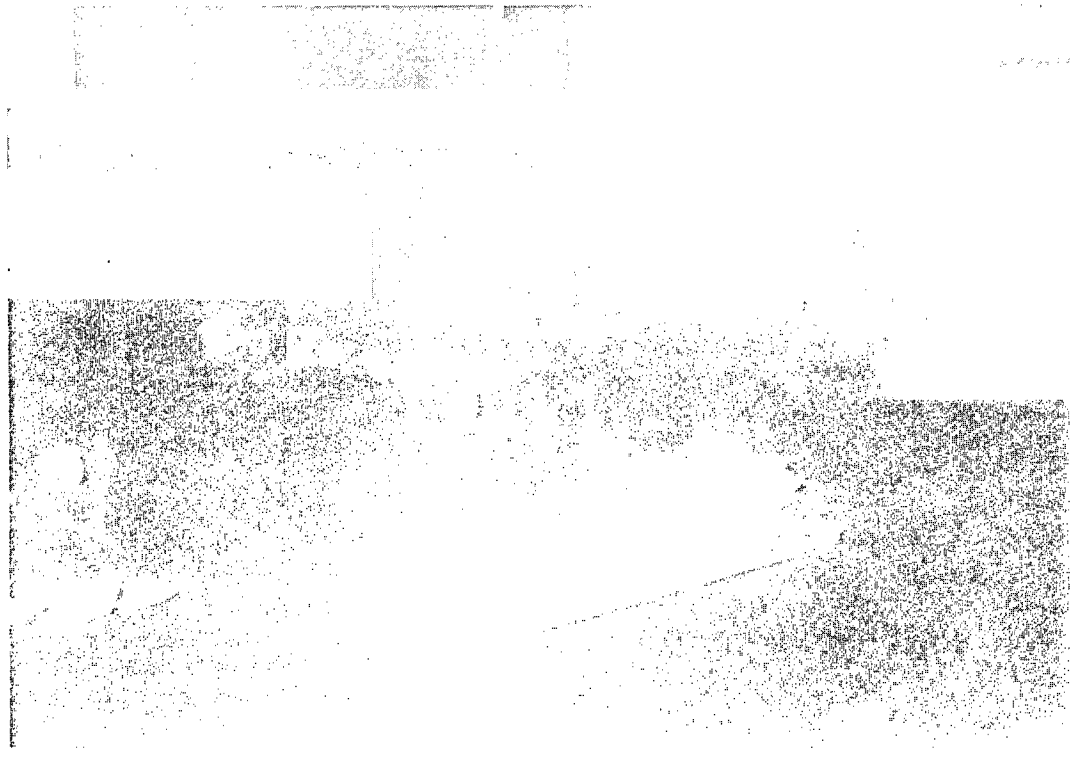

Fig. 5 — Albert Hoyt Taylor, NRL 's leading radio scientist until the end of World War II, was involved in the early discovery of the radar principle and later helped gain crucial financial support for the NRL radar project.

in 1948, "[It] did not [then] have the splendid school of engineering it now posesses."[10] In 3 years, he exhausted his savings and had to begin interspersing school time with work before earning his BS in 1902.

From 1903 until 1908, he taught physics at the University of Wisconsin and showed enough promise to be granted a year's leave for study in Germany to prepare himself to teach graduate courses. Electing to go to the University of Goettingen, he worked primarily in the Institute of Applied Electricity, where he conducted a special research project on electron tubes. In addition, he attended courses under Max Abraham on electron theory, Herman Voigt on optics, and David Hilbert on complex variables. In short, he studied under some of the best scholars in Europe.

Although he had not expected to earn a degree, his progress was so rapid that he was able to pass the doctoral examination in the spring of 1909. He then hoped for yet another year in Germany, but he quickly changed his mind when offered a position as Head of the Physics Department at the University of North Dakota. His perseverance in getting a good education had paid off. He taught at North Dakota from 1909 until 1917. His greatest interest was in radio research, and he was a "dyed in the wool experimentalist."[11] He established an experimental radio station at the University in 1910 and worked closely with radio amateurs as well as colleagues.

Taylor first learned of the Navy radio program through the Institute of Radio Engineers, which he joined when it was established in 1912. At a meeting in New York in 1916, he met Admiral W. H. G. Bullard, then Director of Naval Communications, and Lieutenant Stanford C. Hooper, head of the

[10] *Ibid*, p. 2.

[11] He so characterizes himself in *ibid*, p. 28.

UNCLASSIFIED

Radio Division of the Bureau of Engineering. Bullard offered him use of the radio equipment at the Great Lakes Naval Station near Lake Bluff, Illinois, for his experiments. Taylor accepted the invitation and soon began working there occasionally. After World War I broke out, the Director of the station was able to persuade him to join the Naval Reserve. He was called to active duty at the Station on March 28, 1917.

"At the time I was commissioned," he wrote later, "I had practically no knowledge of the Naval Service except that I knew the Navy was progressive and doing excellent work in the field of radio."[12] But as he had shown before, he learned quickly. He was first made District Communications Superintendent and put in charge of radio operations in the Naval District covering the states around Illinois. Then, in October 1917, he became Trans-Atlantic Communications Officer and head of the Navy Radio Station at Belmar, New Jersey, which handled much of the overseas traffic. At Belmar, he supervised both radio operating and radio experimental work. The latter centered on study of buried antenna wires, which were being used in an attempt to improve signal-to-noise ratios. In July 1918, he was transferred again, this time to the Naval Air Station at Hampton Roads, Virginia, where he directed the experimental program on aircraft radio.

In the fall of 1918, the Bureau of Engineering decided to move the aircraft radio group closer to Washington, and he moved with it. At first it was located in quarters at the Bureau of Standards, but by August 1919, it had been situated at the Naval Air Station in Anacostia. There, over the next 4 years, Taylor supervised a wide variety of research projects, many that were specifically related to aircraft radio but others that were more general, including the study of high frequencies described at the beginning of this chapter.

Taylor returned to civilian status in 1922 but remained in Navy employ. When he was detailed to NRL in 1923, he had risen to become the leading radio scientist working for the Navy. It is likely that he decided to stay with the service instead of returning to university teaching because in the Navy he had found a strong need for his expertise and strong support for his passionate interest in radio.

Taylor's background made him well suited for what he would be doing at NRL. His education gave him knowledge of physics and radio principles on which he could build a research program. His work as a naval officer taught him to understand the Navy mind and the Navy mode of operation. His experience with practical radio problems under the pressure of war made him understand the balance that had to be maintained in a Navy laboratory between research and more routine problem solving. All these qualities helped him lead the radio research program at NRL, which he did from the time it began until 1948.

Leo Young summarized his career and his work in radar in a tape-recorded reminiscence in 1953. His ending was almost an epitome; he said as he signed off, "This is Leo C. Young, old W3 William Victor, W3WV. I started out as a ham back in 1905 and I am still a ham."[13] His interest in amateur radio had, in fact, shaped his whole career and always was intertwined with it. After doing radio research for the Navy during the day, he would spend nights beside his short-wave set at home. Unfortunately for the historian, his notebooks on extracurricular experimentation are often more detailed than his records of his employed labor!

Young was born on January 12, 1891, near Danville, Illinois, but spent most of his youth in a rural region near Van Wert, Ohio.[14] He started building radio sets at around age 14, without any professional assistance or training. Before long, he was able to fashion a spark coil with a coherer-decoherer,

[12] *Ibid*, p. 45. The Navy had a monopoly on radio operations in America from April 1917 until the end of World War I.
[13] Young's taped reminiscence (note 1).
[14] Biographical file on L.C. Young, Historian's office, NRL, Washington, D.C.

Fig. 6 — Leo C. Young originated a number of the basic ideas leading to radar and supervised the radar project for many years.

tuners, and earphones. He even made a sensitive crystal set. Eventually, his family moved into Van Wert, which gave him access to the commercial power he needed to build more powerful equipment. In 1910 he graduated from high school, and by 1912, was working as a telegraph operator for the Pennsylvania Railroad, a position he held until 1917. Then came World War I.

Looking back at his situation at that time, he stated,

> Being of draft age, we finally came to the conclusion that we'd better join the naval reserve and get into radio or something we liked rather than the possibility of being drafted or getting into some other type of work that we were not fitted for.[15]

[15]Young's taped reminiscence (note 1). Young uses the plural "we" to refer to himself throughout the early part of his monologue.

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Like Taylor, Young was aware of the Navy's radio program and hoped work in it would fulfill his military obligation. His wish was granted: after enlisting in the Naval Reserve, he was sent to the Great Lakes Naval Radio Station to work as a radio operator. On the first day, he met Dr. Taylor.

Within a short time, Young was sent to an outlying receiving station at Calumet, Michigan. Taylor wrote about his performance there, "...the Calumet Station took a lot of fixing up. When I first visited it on an inspection trip, I made up my mind it wouldn't have worked at all except for the ingenuity of a young first class radioman named L. C. Young. I kept my eye on this young man and wherever I went in the Navy, he went with me."[16] Taylor and Young eventually went to NRL together. Until then, Young did a combination of radio operating and experimentation for his boss.

In the early years, Taylor always worked closely with Young and his other associates. Louis A. Gebhard, an early NRL employee whose career was similar to Young's and who also began working with Taylor at the Great Lakes Station, later described the situation in this way:

> At that time, [Taylor] had, you might say, the brains and the ideas. What we did was to follow through with them, rather than to generate the ideas. Except we had the ideas of how to do the things that he may not have had. I don't think that he had any great capability of winding a coil or anything like that. But he may have; he did it in his own station probably. But now, he didn't have to do it; he could let other people go ahead and do it.[17]

After NRL was established, Taylor increasingly had to devote his time to administration and to selling Laboratory programs to the Bureau of Engineering. Nonetheless, he always kept a hand in experimental work. As Gebhard said,

> He would come right over and work with you and make the adjustments on the equipment and so forth. He would play with it himself and see how it would work. In other words, he was really interested in what you were doing.[18]

On the other hand, Taylor could also be hard on his men and was well known for his authoritarian manner. Robert Guthrie, who joined the NRL Radio Division in 1929, remarked in looking back,

> He liked to be tough, you know—[he was] highly disciplined. In fact, anyone who went to one of those German schools for his doctorate went through that period.... If you got in with him, nothing he could do was too good for you, but if you didn't size up, he could dress you down in the most embarassing circumstances—I mean beyond overkill. So in a way, when he walked around the Lab with any young people, he practically scared them to death, [or if] he walked up behind you when you were experimenting....He was a disciplinarian.[19]

The combination of Taylor's dominating, military attitude and his love for and faith in radio research made him an excellent liaison between his research staff and the uniformed sponsors of the Laboratory in the Navy bureaus.

[16]Taylor, *Radio Reminiscences* (note 1) p. 46.
[17] Transcript of a tape-recorded interview with Dr. Louis A. Gebhard, Sept. 12 and 19 and Oct. 3, 1977, in the Historian's office, NRL, Washington, D.C., p. 63.
[18]*Ibid*, p. 63.
[19]Tape recorded interview with Mr. Robert C. Guthrie, Apr. 13, 1978, in the Historian's office, NRL, Washington, D.C., side 4.

Taylor and his top assistants, Gebhard and Young, remained the leaders of the Radio Division for the entire period under consideration in this study. Recently Robert Page, the man principally responsible for the technical advances in the development of radar, discussed the situation:

> *Questioner*: One of the things that's evident to anybody looking at the history of the Radio Division is that the men who came in and led it were a small circle of Taylor, Young, Gebhard, and a few others who had really been together during World War I and cohered and remained together.
>
> *Dr. Page*: That's correct.
>
> *Questioner*: Did you sense that as people coming in, that there was this "old guard" that was on top?
>
> *Dr. Page*: Yes, I think we did, but to me it was a natural situation and didn't disturb me at all. It was—the governing body—that was the elite group—they called the shots, they made the decisions. They gave me a pasture to play in, but gave me a lot of freedom as to how I played.
>
> *Questioner*: Did other people at the Laboratory feel the same way?
>
> *Dr. Page*: I'm sure they did.[20]

GUIDING POLICIES

The official commissioning of NRL on July 2, 1923, was a relatively small event.[21] Photographs show a few high-ranking officials from the Navy Department, Laboratory employees, guests, and several members of the Naval Consulting Board gathered in the bright summer sun in front of Building 1 to witness the ceremony. Thomas Edison had been invited, but, not surprisingly, he refused to attend. Unlike the beginning of the Naval Consulting Board, the opening of the Laboratory received almost no notice in the press. Not even the Washington papers deemed it important enough to cover.[22]

The principal speaker at the occasion was Assistant Secretary of the Navy Theodore Roosevelt, Jr. Following Navy tradition, he read the general order that set forth the official policy of the institution and then gave an address on "the aims of the Laboratory."[23] His remarks were not recorded, but earlier he had told the House Subcommittee on Appropriations,

> I feel very strongly that the Navy must not be allowed to petrify. We will petrify unless we are constantly reaching out for new and better things. The research laboratory is in direct line with this thought.[24]

Roosevelt's view was important, because he was formally in charge of the new facility. As the Naval Consulting Board had wished, it was placed administratively in the Secretary's Office, under the Assistant Secretary. This was done to prevent it from being controlled by any of the material bureaus and thus to allow it to become a research establishment for the whole Navy.

[20]Transcript of a tape-recorded interview with Dr. Robert M. Page, Oct. 26 and 27, 1978, in the Historian's office, NRL, Washington, D.C., p. 68.
[21]A. Hoyt Taylor, *The First 25 Years of the Naval Research Laboratory* (Washington: NRL, 1948), pp. 2-4.
[22]Herbert J. Gimpel, *History of NRL* (unpublished manuscript available at the NRL library, deposited in 1975), pp. 29 and 30.
[23]NRL Station Log, Historian's office, NRL, Washington, D.C. vol. I, p. 3.
[24]U.S. Congress, 67:4, House, *Hearings Before the Subcommittee of the House Committee on Appropriations in Charge of the Navy Department Appropriation for 1923* (Washington: GPO, 1922), p. 728.

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Fig. 7 — The Naval Research Laboratory, which was located in a sparsely populated region of the District of Columbia, opened in 1923 with 5 buildings.

The plan succeeded to some extent. Slowly the Laboratory was able to create divisions other than Radio and Sound: a Ballistics Division in 1923, a Heat and Light Division (later Optics) in 1924, a Physical Metallurgy Division and a Chemistry Division in 1927, and a Mechanics and Electricity (later Mechanics) Division in 1931.[25] Certainly the ability to work in all these areas would not have been possible had the institution originally been placed under one bureau. Funding, however, kept it from becoming a truly general research facility. There was never enough money from the direct appropriation from Congress or from the bureaus other than the Bureau of Engineering to do more than a modicum of work in subjects besides radio and sound.

Over the years, the Laboratory's administrative position in the Navy would be altered several times. It was transferred from the Assistant Secretary's Office to the Bureau of Engineering in 1931. It remained there until 1939, when it once again was placed under the Secretary. In 1941 it was moved to the Bureau of Ships. Then in 1945 it was subsumed under the new Office of Research and Inventions. When that was reorganized and became the Office of Naval Research in 1946, NRL was put under it and has remained there ever since. The reasons for these changes are significant, and several of them will be discussed more fully later, in their appropriate contexts. The official documents effecting the moves are reproduced in Appendices B, C, D, and E.[26]

The Director of the Laboratory, despite the wishes of the Naval Consulting Board, has always been a naval officer. When the facility opened, Captain E. L. Bennett was in charge. Because he also

[25]Taylor, *The First 25 Years*...(note 21), pp. 25-28.
[26]Alfred T. Drury also discusses the changes, in *War History of the Naval Research Laboratory* (unpublished history in the series, "U.S. Naval Administrative Histories of World War II," deposited in the Navy Department library, 1946.

served as Technical Aide to the Secretary of the Navy, his principal office was in the Navy Department in downtown Washington. Commander Edgar G. Oberlin, the Assistant Director, was in residence and supervised routine operations. The Naval Consulting Board had feared that if naval officers ran the institution, both the chances of maintaining a civilian atmosphere and the freedom required for productive scientific labor would be precluded. However, due to the wisdom of Captain Bennett, and perhaps even more of Commander Oberlin, the fears proved largely unfounded. These officers realized that directing a laboratory was quite different from commanding a ship; they understood the needs of professional scientists and engineers and also their general dislike of Navy discipline. On the other hand, the scientific staff at the Laboratory realized, as the Board surprisingly had not, the benefits of having a naval officer in charge. Hoyt Taylor later summarized the early governing policy and its advantages in this way:

> To a considerable extent the future policy of the Laboratory was laid down in the very early years by Captain Bennett, Commander Oberlin, and [the original Division] Superintendents. This group insisted that the Division Superintendents have full authority, within their own divisions, to organize and carry on the work, and full responsibility for the direction of the division activities and all reports thereon. These division superintendents reported only to the Director of the Laboratory and had free access to him at all times. This compromise between military and civilian direction has, throughout the years, worked out remarkably well....
>
> There are many advantages in this arrangement. With a competent officer of sufficient rank at the head of the Laboratory it is easier for the Laboratory to maintain close contacts with the various offices of the Navy Department and with the Navy as a whole. It was difficult enough in the early days to "sell" the work of the Laboratory to the Naval Service and it would have been practically impossible if the organization had been a civilian organization from top to bottom.[27]

The principal function of the institution was to conduct research. This was emphasized by the early directors and scientific staff alike. Captain Bennett had expressed his understanding of it in March 1922 to a session of one of the appropriations subcommittees of the House of Representatives. He was then arguing for initial operating funds for the second year in a row, and the Chairman of the Subcommittee, Rep. Patrick H. Kelley of Michigan, questioned him pointedly:

> *Mr. Kelley*: Why should we open that place?
>
> *Capt. Bennett*: I think there is a very distinct need for research work under the direct supervision of the Navy Department; something we have never had except in scattered items.
>
> *Mr. Kelley*: Would the opening of this experimental and research laboratory at Bellevue lessen the amount of experimental work being done at other places by the Navy Department?
>
> *Capt. Bennett*: I do not think there is a great deal of purely research work being done, as distingushed from experimental work and test

[27]A. Hoyt Taylor, "The Relations Between Naval Scientists and Naval Officers" (unpublished article written in 1946) in *Monographs of the Naval Research Laboratory Personnel*, vol. I, 1941-1948, in the NRL library, Washington, D.C.

UNCLASSIFIED

> work. There is a certain amount being done and a part of that will be taken over by the research laboratory, if we get it going.
>
> *Mr. Kelley*: Tell me the difference between a research laboratory and an experimental laboratory.
>
> *Capt. Bennett*: Research and experimentation overlap to such an extent that it is difficult to define and contrast them. Broadly speaking, a research laboratory is where you start in with an idea and work it out. An experimental laboratory is where you take an apparatus and find out what it will do. The first deals more with scientific principles and the other with mechanics. We expect to combine research and experimentation. There is very little research work being done by the Navy anywhere.[28]

Indeed, to emphasize the importance of research at the Laboratory, its official name was altered in 1925 from "Naval Experimental and Research Laboratory" to simply "Naval Research Laboratory."

The institution actually did more than just fundamental research, however. In an undated document written around 1927 and entitled "Functions of the Radio Division of the Naval Research Laboratory," Hoyt Taylor outlined the work of his division in particular.[29] He said,

> Functions of the Radio Division...may be roughly divided under the following headings:
>
> (a) Fundamental Research.
> (b) Engineering Research.
> (c) Engineering Development.
> (d) Advisory work for Government Bureaus, particularly the Bureau of Engineering, Navy Department.
>
> In general, fundamental research which is successful in discovering new ideas which appear to be of special benefit to the Naval Service is followed up by engineering research whose object is to reduce the idea to a practical form....
>
> If this in turn is successful, further work, which may be called engineering development, is done to round out a concrete piece of apparatus which shall establish a new or improved type for the service and which can serve as a basis upon which the specifications can be drawn up for quantitative production by commercial concerns. The Laboratory aims not only to so direct its work that there will be no unnecessary overlapping with other government departments, but it also aims not to undertake problems which appear to be progressing towards satisfactory solutions in other Laboratories whose work is available to the Naval Service.

[28]U.S. Congress, *op. cit.* (note 24), p. 719.

[29]In file A1, box 1, job order 7184, record group 181, records of NRL, Washington National Records Center, Suitland, Md.

Thus, while avoiding competition with industry, the Radio Division attempted to cover the full range of scientific and engineering research and development from studying basic laws and principles to developing prototypes of naval equipment.

Like Bennett, however, Taylor always argued that research was the most important activity. Although he never expected the division to engage only in research, he always realized that it was the activity most in danger of being curtailed. He knew that because the main responsibility of the Bureau of Engineering in the radio field was maintenance and operation of equipment, it naturally felt that the Laboratory should concentrate on practical problems. He feared that Bureau officials did not understand that sponsoring research was in their own best interest in the long run. He had good reason to worry.

Captain Stanford C. Hooper, who had been so important in getting the Laboratory established,[30] was one of several important individuals in the Bureau who were particularly upset about the devotion of NRL to research. He said later,

> Unfortunately...I was transferred to sea [in 1923] and did not have [an] opportunity to assist in guiding the policy of the Laboratory with the radio group in the very beginning, for when I returned from sea in 1926 and again had charge of the Radio-Sound Division of the Bureau, I was surprised to find that the Bellevue Laboratory personnel had assumed a role quite different from what I originally had in mind. Instead of finding a thriving group of engineers engaged in assisting the Radio Division in the preparation of specifications and testing soap-box models and go-betweens between the Bureau and our great commercial laboratories, I found, principally, a combination research and manufacturing staff, and one in competition with commercial companies.[31]

Hooper wanted NRL to leave both fundamental and engineering research to industry or the universities and concentrate on design of naval equipment. He believed he himself had established the model for how the Navy and industry should cooperate when he led the Navy effort to help establish the Radio Corporation of America after World War I.[32] By emphasizing in-house research, NRL did not follow this model. To change the policy of the Laboratory, Hooper and those who thought like him ultimately were able to have it transferred to the cognizance of the Bureau of Engineering, as will be discussed further in the next chapter.

The concern over the role of research in the Laboratory, and more particularly in the Radio Division, had direct relevance to the radar project. Radar began as a combination of scientific and engineering research. It never could have become an acceptable project for the Laboratory had not Taylor, Oberlin, and others labored continuously to establish a policy based on the central importance of research. If Hooper had had his way, radar would have been developed first—if at all—by commercial companies. Moreover, the continual difficulties of NRL leaders in obtaining strong support for research meant that once the radar project was started, it had to be a small effort until concrete results were achieved.

Like the administrative policies, the way NRL was financed had a major effect on the work it performed. Directors and division superintendents had to temper their conceptions of what NRL ought to

[30]See note 29, p. 68.

[31]Statement of S.C. Hooper, Jan. 4, 1932, in the file "Jan-Feb, 1932," box 14, papers of S.C. Hooper, Library of Congress Manuscript Division.

[32]Capt. L.S. Howeth, *History of Communication Electronics in the United States Navy* (Washington: GPO, 1963), ch. 30. Many of Hooper's papers in the Library of Congress (note 31) also relate to this matter.

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do to fit the reality of its funding. Getting enough money to remain in operation was a continual difficulty in the years between World War I and the buildup prior to World War II. Naval appropriations in general were kept low during much of this period, and funds for new ships always took precedence over funds for research. The staff at NRL simply learned to think small.

As mentioned earlier, money for NRL came from several sources: the general yearly appropriation from Congress, funds from various Navy bureaus, and allocations from other Government agencies. Table 2 shows the totals of NRL appropriations for fiscal years 1924 through 1946, as well as can be determined from extant records.[33] The importance of Bureau of Engineering funds throughout this period of the history of NRL is clear from the totals. They also reflect the definite effect of the depression (see the years 1933, 1934, and 1935) and the rearmament prior to and during World War II.

Table 2 — NRL Funding

Fiscal Year	Funding (thousands of dollars)					
	Congress	BuEng or BuShips	Other Navy	Total Navy (col. 3 + col. 4)	Other Govt. Agencies	Total
1924	100,000	153,447	42,855	196,302	13	296,315
1925	125,000	194,401	54,802	249,203	25	374,228
1926	150,000	197,010	49,187	246,197	2,879	399,076
1927	175,000	249,409	51,461	300,870	17,402	493,272
1928	175,000	276,748	79,140	355,888	39,311	569,199
1929	200,400	261,061	82,485	343,546	12,577	556,123
1930	220,350	236,515	97,582	334,097	3,208	559,655
1931	230,000	280,651	87,887	368,538	23,204	621,742
1932	229,675	—	—	406,620	—	(636,295)*
1933	213,000	—	—	478,463	—	(691,463)*
1934	199,381	—	—	259,526	—	(458,907)*
1935	204,916	—	—	307,293	—	(512,209)*
1936	310,000	238,461	96,140	334,601	—	(644,601)*
1937	300,000	—	—	330,257	—	(630,257)*
1938	310,000	—	—	392,028	—	(702,028)*
1939	335,000	354,381	122,012	476,391	—	(811,391)*
1940	370,000	—	—	552,612	—	(922,612)*
1941	653,350	—	—	1,085,520	—	(1,738,870)*
1942	1,479,500	—	—	2,077,631	—	(3,557,131)*
1943	2,327,923	—	—	3,967,826	—	(6,295,749)*
1944	3,075,000	—	—	7,649,749	—	(10,724,748)*
1945	3,075,000	—	—	10,000,000	—	(13,075,000)*
1946	4,239,508	6,666,058	4,735,058	11,401,116	—	(15,640,624)*

*Not a complete total, because information is not available on funding from other Government agencies.

[33]The principal extant financial records of NRL for this period are in the National Archives, in files L1-1(3) of boxes 32 through 35 of the records of NRL, Unclassified series, and box 24 of the Confidential series (now Unclassified), record group 19, National Archives building. I also used data from Drury, *op. cit.* (note 26), and U.S. Congress, 78:2, House Select Committee on Post-War Military Policy, *Hearings, Surplus Material—Research and Development* (Washington: GPO, 1945), pp. 228 and 229.

The money from Congress was spent first for general overhead expenses (maintenance and operation of the plant) and then for research. The small "basic science" divisions (Optics, Physical Metallurgy, Chemistry, and Mechanics and Electricity) were supported almost entirely with it.[34] Hence, Congressional generosity determined their birth and prosperity.

The Bureau of Ordnance paid for most of the work in the Ballistics Division, until this division was broken up and merged with the Metallurgy and Chemistry Divisions. The Bureau of Engineering paid for virtually everything done by the Radio and Sound Divisions, which remained the largest divisions until the end of World War II. Projects sponsored by other bureaus were few and were conducted by various sections of the Laboratory. Although the bureaus were not charged for overhead expenses, they paid for both materials and labor. The salaries of almost all the personnel of the Radio Division, for example, came from allocations by the Bureau of Engineering.

The basic unit of work in all the scientific divisions was the "problem"[35]—a specific assignment of work to be done. Money was allocated in terms of problems; personnel received assignments on the basis of them; they were the subject of the regular reports. Some were specific; others were broad and general. They could last a short period of time or run over many years.

Problems could originate either at NRL or in the Navy bureaus. Since Congress left administration of the funds it allocated almost entirely up to NRL, most of the problems dependent on Congressional money originated with suggestions of Laboratory personnel and were regulated by internal decisions. The quantity of money obtained every year directly from Congress had more to do with arguments based on general principles or major achievements than on details.

Money from the bureaus was another matter. A large portion of the problems they paid for were based on work they asked the Laboratory to do. Many projects resulted from operating difficulties in the fleet and thus were test or development efforts. If NRL wanted to originate a problem that depended on bureau appropriations, it had to "sell" it to the bureau. And throughout the 1920s and 1930s it was difficult to sell the bureaus on research. As Assistant Director E. D. Almy said of relations with the Bureau of Engineering in 1931,

> ...the Bureau's urgent and immediate needs are engineering and not research. In fact... I have been impressed and depressed by the almost total absence of appreciation of the value of research I have encountered in my contacts in the Department [of the Navy]. Not one officer in a hundred that I contact seems to value research on naval problems. Probably less than one percent of the officers of the service have any knowledge of this Laboratory, its functions, its organization, its problems, or their relation to the Naval Establishment.[36]

In such a climate of opinion, it was hard to get support for what the leadership of the Laboratory thought it ought to be doing.

After initial discussion of a problem, the Director of the Laboratory had the right to accept or reject it. Many test problems were in fact rejected as inappropriate for the institution. If a problem was accepted, it was given to one of the divisions for review and for initial cost estimates. Once this was

[34]Taylor, *The First 25 Years...* (note 21), p. 25.

[35]This discussion is based in part on general reading in the administrative records of NRL and in part on Drury, *op. cit.* (note 26), pp. 54 and 55.

[36]Memorandum from E.D. Almy to the NRL director, Oct. 20, 1931, in file A1, box 1, job order 7184, record group 181, records of NRL, Washington National Records Center, Suitland, Md.

UNCLASSIFIED

approved by the Director and the sponsor, the money was allocated. Administration of problems was then the job of the division superintendents. To some extent, the Director was free to juggle funds among the divisions to keep the work progressing.

In cooperation with NRL, the bureaus assigned priorities to problems they supported to help guide allocation of resources. Reports were made as required or as deemed necessary. Problems were closed when the objectives were met, when it was learned they could not be met, or when the funds were exhausted and results seemed not to warrant further expenditures. The outcome of the work might be a report or a series of reports, publications, or equipment. The last outcome was usually a prototype that could be turned over to a manufacturer for quantity production.

The radar problem was initially funded with money from the Bureau of Engineering. Later some funds from the direct Congressional appropriation were applied to it, and, after practical equipment had been developed, other bureaus began funding work on radar sets for their use. Thus, the successful development of radar would require a mixture of support. As we shall see, the way the investigation progressed depended on how and when that support was obtained and on the status given to the investigation both at NRL and by the bureaus.

Overall, the policies of NRL defined the reasons for which the radar investigation was made, the way it was supported, and its size and limits. Radar did not, of course, result *merely* from an application of the policies in the area of radio detection, but the administration of the project did in fact govern its development.

THE TECHNICAL CONTEXT: HIGH-FREQUENCY RADIO[37]

Radar depends on the reflection of radio waves by distant objects. Presently, radar frequencies are considered to stretch between 3 megahertz and 40,000 megahertz (wavelengths between 100 meters and 7.5 millimeters), as shown in Table 3, although early British radars used lower frequencies. All radio waves are returned by conducting objects, but the phenomenon is easily observable only with high-frequency radiations. Moreover, unlike long waves, short waves can be focused into narrow beams and thus hit distant targets with concentrated power.

The knowledge that radio waves undergo reflection is as old as the knowledge of radio waves themselves. They were first predicted as a logical conclusion of James Clerk Maxwell's seminal *Treatise on Electricity and Magnetism*, which was published in 1873. In 1887 and 1888, Heinrich Hertz demonstrated them experimentally and showed their similarity to light waves by proving, among other things, that they could be reflected.

Hertz, and other experimenters who shared his interest in verifying Maxwell's theory, worked primarily with high frequencies. By the late 1890s, however, Guglielmo Marconi and other men who had become interested in using the waves for communication had determined that low-frequency radiation was more practical. Their success in developing communication equipment soon attracted almost all scientific attention to the same area of the electromagnetic spectrum. Low-frequency reflections being so slight, little thought was given to making use of wave echoes.

[37]References used for general information contained in the next two sections were Louis A. Gebhard, *The Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979), and Henry Guerlac, "The Radio Background of Radar," *Journal of the Franklin Institute* **250** (1950): 285-308.

In 1903 and 1904, however, a German engineer, Christian Hulsmeyer, learned that by using a spark-gap transmitter and coherer-equipped receiver, he could detect echoes from barges passing along the Rhine River. Convinced that such a phenomenon could be employed to detect ships in fog or at night, he developed and patented the idea in both Germany (patent 165,546, issued April 30, 1904) and England (patent 25,608, issued November 1904). The equipment he built was too crude to interest private industry or the German Navy, and it played no role in stimulating later developments, but his work does indicate the general awareness that radio waves could be reflected and that those reflections could perhaps be put to use.[38]

A general revival of interest in higher frequencies occurred among radio researchers in World War I, due to the potential of using them for secret point-to-point communications.[39] This interest, fueled by the enthusiasm of radio amateurs, continued to grow after the War. Wave reflection soon became a subject of discussion once again. Marconi himself, in an address to a joint meeting of the American Institute of Electrical Engineers and the Institute of Radio Engineers in 1922, stressed the importance of short-wave research and, almost incidentally, pointed to one possible use of the reflective property:

> As was first shown by Hertz, electric waves can be completely reflected by conducting bodies. In some of my tests I have noticed the effects of reflection and deflection of these waves by metallic objects miles away.
>
> It seems to me that it should be possible to design apparatus by means of which a ship could radiate or project a divergent beam of these rays in any desired direction, which rays, if coming across a metallic object, such as another steamer or ship, would be reflected back to a receiver screened from the local transmitter on the sending ship, and thereby immediately reveal the presence and bearing of the other ship in fog or thick weather.
>
> One further great advantage of such an arrangement would be that it would be able to give warning of the presence and bearing of ships, even should these ships be unprovided with any kind of radio.[40]

These remarks were published in August 1922—a month before the experiments of Taylor and Young. Whether they were aware of Marconi's suggestions, however, is unclear.

Once NRL opened in 1923, Taylor focused the efforts of the Radio Division on the study of high frequencies. In retrospect he explained, "Although...we did not realize the tremendous possibilities for the use of high frequencies in the field of naval communications, we did see that they would certainly be extremely valuable, provided we could sufficiently stabilize transmitters and receivers to make use of such frequencies practical under naval conditions."[41] Elsewhere he noted, "Probably the most important service of the Radio Division in the early days was the selling of the high-frequency program to the Navy, and indirectly, to the radio communications industry."[42]

[38]Charles Susskind, *History of Radar: Birth of the Golden Cockerel* (manuscript of a book in preparation), pp. 3 and 4.
[39]Guerlac, *op. cit* (note 37), p. 290.
[40]Guglielmo Marconi, "Radio Telegraphy," *Proceedings of the Institute of Radio Engineers* **10** (1922):237. Marconi later became involved in the development of Italian radar equipment.
[41]Taylor, *Radio Reminiscences* (note 1), p. 105.
[42]Taylor, *The First 25 Years...* (note 21), p. 17.

Table 3 — Radio Spectrum

	FREQUENCY (megahertz)	WAVELENGTH (meters)		
		0.00001 (10 micrometers)	(infrared)	
	10,000,000			
	3,000,000	0.0001		
	1,000,000 (1 terahertz)		submillimeter waves	
	300,000	0.001 (1 millimeter)		(experimental radars)
	100,000			
	30,000	0.01 (10 millimeters, or 1 centimeter)		
	10,000		microwaves	
	3,000	0.1 (10 centimeters)		(existing radars)
ultrahigh frequencies (UHF TV)	1,000 (1 gigahertz)	0.3		
	300	1		
very high frequencies (VHF TV, FM)	100			
	30	10	short waves	
high frequencies	10	30		(over-the-horizon radars)
	3	100	long waves	
medium frequencies	1		(AM radio)	
	0.3	1,000 (1 kilometer)		
low frequencies	0.1			
	0.03	10,000	(maritime radio)	
very low frequencies	0.01		(systems to transmit to submarines)	
	0.003	100,000		

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The Bureau of Engineering was initially skeptical of these frequencies, because transmission and reception with them was known to be very erratic. However, the Bureau had little choice but to move to higher bands. In the early 1920s, the Navy was forced to relinquish the frequencies from 0.550 to 1.500 megahertz to the radio broadcast industry.[43] Because of this, and because of Taylor's enthusiasm at NRL, the Bureau sponsored a research program there that would provide the knowledge and expertise needed for making the change. Subsequently, the Radio Division pioneered work in high-frequency radio propagation theory, quartz-crystal frequency control, power generation, reception techniques, and general equipment development.[44]

By 1925, enough progress had been made for the Bureau to begin incorporating high-frequency equipment into the fleet; NRL was given the principal responsibility for its development and design. For years, this was the main effort of the Radio Division.[45] As one historian of Navy radio and communications has said,

> Between 1925 and 1929, with the radio boom in full swing, the Navy was almost entirely dependent upon its own research facilities for the development of radio equipment suited to its needs. The radio industry as a whole was far too occupied providing millions of receivers for American homes and in the development of improvements which might increase sales in this highly competitive market. Apparatus designed by Naval Research Laboratory personnel was manufactured for the Navy by the Radio Corporation of America, the Westinghouse Electric & Manufacturing Co., the Western Electric Co., the National Electric Supply Co., and other smaller companies. Practically no research or development of Navy equipment was performed by any of these companies during this period.[46]

As a consequence, employees of the NRL Radio Division became extremely knowledgable and experienced with short-wave propagation. Moreover, they kept up with all new technical developments in the field and developed a reservoir of components and equipment. This institutional situation would make possible the investigation of many new ideas in the short-wave field, one of which would be radar. Although most of the research of the Radio Division prior to 1930 related to the later work on radar only indirectly, several investigations on the propagation of high-frequency radio waves were closely tied to it. Once again they were part of a general interest in the subject shared by a number of investigators outside NRL.

THE TECHNICAL CONTEXT: IONOSPHERIC RESEARCH

In 1901, Marconi had succeeded in transmitting radio signals across the Atlantic. This quickly led to speculation by physicists on how it was possible for the waves he used to bend around the curvature of the earth. Almost simultaneously in 1902, Arthur E. Kennelly in America and Oliver Heaviside in England theorized that it must be due to ionization of the upper atmosphere, which yielded a charged layer that could reflect the radiation.[47] In 1910, Dr. W. H. Eccles set forth a detailed hypothesis for this conducting layer.[48] This was, in turn, superseded by a well-reasoned theory based on free electrons

[43] Gebhard, *op. cit.* (note 37), pp. 43 and 44.
[44] *Ibid.*
[45] *Ibid.*, pp. 43-169.
[46] Howeth, *op. cit.*, (note 32), p. 403.
[47] A similar account appears in Guerlac, *op. cit.* (note 37), pp. 296-304. See also Gebhard, *op. cit.* (note 37), pp. 44 and 45, and A. Hoyt Taylor and E.O. Hulburt, "Propagation of Radio Waves Over the Earth," *Physical Review* 27 (1926): 189-215.
[48] W.H. Eccles, "On the Diurnal Variations of the Electric Waves Round the Bend of the Earth," *Proceedings of the Royal Society of London* **87A** (1912): 77-99.

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published by Sir Joseph Larmor in December 1924.[49] It provided the first sound mathematical explanation of the atmospheric reflection of radio waves.

Earlier in that same year, however, A. Hoyt Taylor and his colleagues at NRL, in cooperation with Mr. John L. Reinartz and other radio amateurs, had discovered that high-frequency radio waves could jump from a transmitter to a distant receiver while being imperceptible at many points in between.[50] Taylor labeled the gaps "skip distances" and conducted an in-depth investigation to determine their characteristics.[51] He began publishing detailed experimental measurements of skip-distances in early 1925, including with them rough estimates of the height of the conducting layer.[52]

The discovery of skip distances could not be understood simply in terms of Larmor's theory—it worked only for long waves. Thus Taylor enlisted the aid of the new superintendent of the NRL Heat and Light Division, E. O. Hulburt, to come up with a new explanation. Starting with Larmor's reasoning, Hulburt developed a new mathematical account that applied to short as well as long waves and published it jointly with Taylor in 1926.[53] It was a brilliant paper, perhaps the best of Hulburt's career. As he said later,

> [This study] put the Laboratory on the map. Because it was not only of theoretical interest to theoretical people, but it was useful to the Navy....[It] was a lucky piece of work that was of first class theoretical standard.[54]

Subsequently, NRL supported a continuing investigation of the properties of the upper atmosphere and their relation to radio-wave propagation.

While Taylor and Hulburt were involved in this study, similar investigations were being undertaken independently by the English scientists E. V. Appleton and M. A. F. Barnett and by the Americans Gregory Breit and Merle A. Tuve. In 1924, the latter team conceived the idea of measuring the height of the conducting layer, later termed the "ionosphere," by the use of radio pulses. They planned to compare the time it took for a signal to go directly from transmitter to receiver with the time it took for a signal to go from the transmitter to the conducting layer, where it was reflected, and thence to the receiver. The results could be displayed visually at the receiver by means of an oscillograph, and then photographed for precise measurements. Because transmission was pulsed rather than continuous, the direct signal and the reflected signal would show up clearly as two distinct bumps.[55]

In a meeting in Washington in November 1924, Breit and Tuve discussed their plan with leading radio experts. Soon arrangements were made for a test with equipment of the Westinghouse Electric and Manufacturing Company (Station KDKA), the Radio Corporation of America (Station WSC), the National Bureau of Standards (Station WWV), and NRL (Station NKF). After a period of experimentation, Breit and Tuve reported that

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[49] Sir Joseph Larmor, "Why Wireless Electric Rays Can Bend Round the Earth," *The London, Edinburgh, and Dublin Philosphical Magazine and Journal of Science* **48** (1924): 1025-1036.

[50] John L. Reinartz, "A Year's Work Below Forty Meters," *Radio News* 6^2 (Apr. 1925): 1394ff.

[51] Transcript of tape-recorded interview with Dr. Edward O. Hulburt, Aug. 22 and Sept. 8, 1977, in the Historian's office, NRL, Washington, D.C., p. 15.

[52] A. Hoyt Taylor, "An Investigation of Transmission of the Higher Radio Frequencies," *Proceedings of the Institute of Radio Engineers* **13** (1925): 677-683, and A. Hoyt Taylor and E. O. Hulburt, "Wave Propagation Phenomena at High Frequencies," *Bureau of Engineering Monthly Radio and Sound Report*, Sept. 1, 1925, pp. 14-59.

[53] Taylor and Hulburt, *op. cit.* (note 47). See also the Hulburt interview, *op. cit.* (note 51), pp.14-23.

[54] Hulburt interview, *op cit.* (note 51), pp. 17 and 18.

[55] Guerlac, *op. cit.* (note 37), pp. 302-304; M.A. Tuve and G. Breit, "Note on a Radio Method of Estimating the Height of the Conducting Layer," *Terrestrial Magnetism and Atmospheric Electricity* **30** (1925): 15 and 16.

> The most definite results have been obtained from the Naval Research Laboratory owing to the fortunate relative location of the [equipment] and to the high constancy of the frequency emitted by the NKF transmitter. This is achieved by the use of crystal control and makes it superior to any of the other stations we tried for the purpose in question.[56]

The apparatus had been constructed by Leo Young and Louis Gebhard. With it, Breit and Tuve determined that the heights of the ionosphere tended to vary with both the time of day and the time of year and that it ranged between 90 and 210 kilometers (55 and 130 miles).[57]

The pulse technique was simpler and more precise than any other that had previously been employed for ionospheric measurements. Soon it was adopted by investigators throughout the world. Significant improvements were made in the instruments used, such as the addition of a multivibrator to generate sharp pulses and the substitution of a cathode-ray tube for the mechanical oscillograph.[58] Consequently, the pulse technique for sounding the ionosphere became both widely known and well developed.

The equipment that evolved for this purpose and the principles on which it was based are similar to those of pulse radar. One historian, Henry Guerlac, went so far as to state that the latter followed directly from the former:

> Radar was developed by men who were familiar with the ionospheric work. It was a relatively straightforward adaptation for military purposes of a widely-known scientific technique, which explains why this adaptation—the development of radar—took place simultaneously in several different countries.[59]

The statement is true of radar development in England. There, as will be discussed in a later chapter, Robert Watson-Watt did develop his first radar directly from existing ionospheric measuring devices.[60] And it seems that it ought to be true of the development of radar at NRL, for, after all, NRL had been deeply involved in the first pulse measurements of the ionosphere in America. Yet it is not true. Leo Young later remarked on this point,

> A good many publications and information out indicates that Heaviside layer [ionospheric] reflections were the beginning of radar. Well, this was not the beginning of radar insofar as my viewpoint is concerned. It was a very good background—I was working on it, others at the Laboratory were working on it. Yet, there was no one who came up with the idea of using pulses of very much shorter time and getting echoes from very much smaller objects [than the ionosphere], which was necessary for military use....While it was a very good background, I don't believe it was radar.[61]

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[56] G. Breit and M.A. Tuve, "A Test of the Existence of the Conducting Layer," *Physical Review* **28** (1926): 555. Tuve gives an interesting (although somewhat flawed) retrospective view of the experiments and their relation to the development of radar in "Early Days of Pulse Radio at the Carnegie Institution," *Journal of Atmospheric and Terrestrial Physics* **36** (Dec. 1974): 2079-2084.

[57] Breit and Tuve, *op. cit.* (note 56), p. 575.

[58] Guerlac, *op. cit.* (note 37), pp. 302 and 303.

[59] *Ibid.*, p. 304.

[60] Sir Robert Watson-Watt, *The Pulse of Radar* (New York: Dial, 1959), pp.55-59, 427-434, and especially 492.

[61] Young's taped reminiscence (note 1).

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The development of radar at NRL did not evolve directly from the work on ionosphere measurements. In fact, the first attempts to build radar equipment did not rely on pulsed radiations at all but on continuous waves. Like NRL's general research in high-frequency communications, the ionospheric measurements were, as Young said, only a good background. The remainder of the story is much more complex than Guerlac's conclusion indicates.

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5. FROM THE BEGINNING OF THE PROJECT TO THE FIRST TEST (1930 to 1934)

ORIGIN OF THE RADAR PROJECT

June 24, 1930, was a standard summer day in the District of Columbia: hot, muggy, and miserable. But at NRL, Leo Young and Lawrence Hyland ignored the conditions and went outside to test a high-frequency direction finder that had been designed and built by the Radio Division.[1] In the course of their investigation, Hyland made a fateful discovery, one that would lead to the establishment of the radar project. Young later described the event as follows:

> We were conducting experiments relative to guiding planes into a field by using high-frequency beams. We had built quite a number of beams for communications purposes, various frequencies, and various numbers of elements, so we had a pretty good idea of what beams were all about....We had built a [fixed] beam that was directed vertically...[and] had [both] a horizontal and a vertical beam working around 30-some megacycles. We were flying a plane determining just what effects were in the air when the plane was trying to follow these beams and what-not.
>
> In making some field measurements on this set-up, Mr. L.A. Hyland...had field strength equipment out at what is now the lower end of Bolling field, just north of the Laboratory. And of course, as soon as planes began flying around, he noticed the meter bobbing all up and down. Since we were using quite a bit of power, the field strength direct to the equipment was rather low, but up to the planes was rather high, so we got a good reflection off planes. This gave a good interference pattern, or doppler effect.
>
> [Hyland] determined that he was getting some sort of effect from planes that flew through those beams. When he came in he immediately brought it to our attention, and of course, we immediately realized that we were getting the same effect from planes that we had from a ship back in 1922. So this warmed the subject up again.[2]

It was no surprise that airplanes reflected high-frequency radiation: this conclusion followed directly from the basic principles of electromagnetic-wave propagation. What was startling was that the reflections were powerful enough to cause a discernible interference pattern in a distant receiver.

[1] There are several good sources on the discovery of 1930. The only contemporary source is a letter Taylor wrote several months after the event to the Bureau of Engineering, from NRL to the Chief of the Bureau of Engineering, Nov. 5, 1930, which may be found in the file "Nov-Dec 1930," box 12, papers of S.C. Hooper, Library of Congress Manuscript Division. Surprisingly, the official copies of this correspondence are missing from both NRL records and Bureau of Engineering files. The discovery is also discussed by L.C. Young in a taped reminiscence he made in 1953, which is on reels 150 and 151 in the collection "History of Radio-Radar-Sonar" that is part of the Hooper papers. Taylor wrote about the event retrospectively in *Radio Reminiscences* (Washington: NRL, 2nd printing, 1960), pp. 155 and 156. Henry Guerlac discussed it in *Radar in World War II* (unpublished history of Division 14 of the National Defense Research Committee, 1947), pp. 86-91.

[2] Young's taped reminiscence (note 1).

Hyland, Young, and Taylor, who was quickly informed of the discovery, now realized that radio detection equipment might be capable of detecting aircraft as well as ships. And since the airplane was rapidly developing into an important instrument of war, this was a significant addition.

Instead of reporting their findings immediately to the Bureau of Engineering, the men conducted further experiments on their own over the next several months, whenever they had a bit of free time. They always used the same receiver, a "super-regenerative type comprising a strongly oscillating detector, a super-audible variation oscillator, and one or two stages of audio-frequency amplification,"[3] but they modified the transmitter in different tests. They tried different antenna shapes, which gave waves of various polarizations. They used different frequencies: the earliest work was at 32.8 megahertz, and later they went as high as 65 megahertz. In some instances, they moved the equipment to locations away from the Laboratory; in one, they drove the receiver around in an automobile to simulate conditions on a naval vessel. Even then they were able to note the interference.

By late fall, Taylor was ready to inform the Bureau of Engineering about their investigation. On November 5, he sent a detailed, 11-page letter complete with diagrams and full descriptions of the experiments they had done. He argued,

> It should be clear from what has been said that the echo signal from a moving object would, if it alone affected the receiver, be a more or less constant signal, but varying slightly and very slowly in intensity as the position of the plane shifts. Such an effect would be of no great use. Unquestionably such an effect occurs, but such variations in signal as are due to it are too vague and too slow to be of any practical use. The body of this report shows clearly, however, that what we have observed is a combination at the receiver of two wave fronts, one of which is the direct wave with a second wave which is reflected or reradiated (if you will) from the moving object. This produces an interference effect, the pattern of which is rapidly changing as the relation of the two waves varies while the moving object proceeds on its path.[4]

Taylor hoped that this varying interference effect could be exploited to determine the velocity of the moving object and outlined briefly his thoughts on the possibility. Then he concluded the letter,

> The Laboratory has at present two definite objectives in this work: the first is to detect the presence of moving objects in the air or on water, possibly later even on the ground, at such distances that their detection by other well-known methods is difficult or impossible. It may be remarked that the personnel piloting any moving object would probably not know that any observations were being taken upon them. Second, to develop as a byproduct of the principal investigation as a check on the validity of the general theory of the same, a method of measuring the velocity of moving objects at great heights or at considerable distances, or on the surface of the water....
>
> Much more work remains to be done with transmitter and receivers very close together. It is hoped that the next report will have something of interest on this particular point. It is not desired in this report to give the Bureau the impression that the work is anything like in a

[3]Taylor letter (note 1).
[4]*Ibid.*

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> finished state but it does appear to this Laboratory to be far enough advanced to warrant much further and intensive investigation over a considerable period of time.[5]

In sum, Taylor's lengthy report officially told the Bureau of Engineering that NRL might be able to build equipment that could detect and determine the velocity of aircraft and ships at significant distances. Additionally, the letter showed that the Radio Division intended to continue its investigation, in some form, with or without encouragement. Taylor obviously hoped for Bureau support, but NRL's relatively independent position and the availability of general research funds from the direct Congressional appropriation meant that such support was not absolutely imperative, as, for example, it had been when he and Young made their earlier discovery in 1922 while working at the Naval Air Station. Yet, at the same time, Taylor knew that unless he obtained approval, any project undertaken would have to be very limited.

As before, response to the discovery at the Bureau of Engineering was unenthusiastic, despite Taylor's detailed experimental results and glowing predictions.[6] To help strengthen his petition, the Acting Director of NRL, E. D. Almy, wrote to the Bureau on January 16, 1931,

> The Director considers [this] subject matter of the utmost importance and of great promise in the detection of surface ships and aircraft. No estimate of its limitations and practical value can be made until it has been developed. However, it appears to have great promise and its use [appears to be] applicable and valuable in air defense, in defense areas for both surface and aircraft and for the fleet or the scouting line.[7]

In answer to these letters, the Bureau of Engineering finally established two new problems at NRL. The first was assigned on November 25, 1930. Labeled problem B1-1, it authorized the Laboratory to make "experimental investigations of high and super frequency directional transmitting antenna systems of the types possibly applicable for Naval use."[8] ("Super frequencies" being in the region now commonly known as "very high frequencies" (Table 3, Chapter 4). Although the primary purpose of the problem was not to study radio detection, it did include some support for it. The specification said, "The Bureau is particularly interested in the possibilities [of such antennas] for handling secret Fleet communications, and in connection with problem W5-2 (use of super-frequencies to detect presence of enemy vessels or aircraft)."[9] The second problem established was W5-2 itself. Formal authorization for it came on January 19, 1931—soon after Almy's letter had been sent. The specification ordered the Laboratory to "investigate [the] use of radio to detect the presence of enemy vessels and aircraft," and went on to say "especial emphasis is placed upon the confidential nature of this problem."[10]

By making these two authorizations, particularly the second, which became the official sanction for almost all the early radar work, the Bureau had finally agreed to sponsor a project on radio detection. This action, however, did not mean that a significant amount of money and manpower would immediately be invested in it. No funds were allocated to hire new employees; no man on the staff was even

[5] *Ibid.*

[6] This conclusion is based largely on circumstantial evidence in the sources cited in note 1, since there is extant no definite record about the Bureau's response. Confirmation by someone in the Bureau at the time, however, may be found on pp. 6-8 in the notebook "Electronics History, Volume II" by S.C. Hooper in box 40 of the Hooper papers (note 1). Hooper there admits that he and his colleagues did not see the importance of radar in its early stages of development and did not support it strongly.

[7] Letter from NRL to the Bureau of Engineering, Jan. 16, 1931, in file C-S67-5 #1, box 31, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building.

[8] Statement of problem B1-1, Nov. 25, 1930, in file C-S67-5 #1 (note 7).

[9] *Ibid.*

[10] Statement of problem W5-2, Jan. 21, 1931 (active date Jan. 19, 1931) in file C-S67-5 #1 (note 7).

assigned to the effort full time. Radio detection simply had to compete with other work for the limited capabilities of the existing staff of the Radio Division. For several years, the project would be overshadowed by other investigations that had higher priority.

Before any form of equipment would become a reality, many technical problems had to be solved. The nature of the work was inherent in the letter Taylor had written to the Bureau on November 5: it would be "engineering research."[11] That is, using the principles he had outlined, NRL would immediately begin trying to design a practical device. Taylor and his associates believed that this was feasible with existing knowledge and available radio components.[12] Their investigation was to focus on the interference patterns that objects caused in *continuous-wave* radiations, not on the echoes of radio *pulses*. Thus even though the men had the experience of using pulses in sounding the ionosphere some 5 years earlier, they were following a different method in their initial efforts to build radio detection equipment. In essence, they planned to exploit their discoveries of 1922 and 1930, which had given experimental proof that the continuous-wave method would work.

RESEARCH ON CONTINUOUS WAVES

For the first 3 years of the project, from 1931 until early 1934, all experimentation focused on the continuous-wave, or doppler, method. Extant records about research in this period are few and hazy, but they do disclose both the principal progress that was made and the continuing difficulties encountered.

In December 1931, the Navy dirigible *Akron* was sent to the Laboratory for use in calibrating high-frequency direction finders. Taylor and his associates used the opportunity to test one of the principles involved in radio detection. Through experimentation, they learned that the large dirigible would reflect radio signals of frequencies as low as 1.4 megahertz but that a much smaller Curtiss Condor transport plane would not. As they reported to the Bureau of Engineering, this confirmed their view that the wavelength of the radiation employed had to be the same order of magnitude as the objects to be detected.[13] Indirectly, the letter also showed that not too much effort had been devoted to the project. Indeed, when writing later in December, Taylor admitted, "...the pressure of other problems has somewhat prevented the active exploitation of the [problems on radio detection, B1-1 and W5-2]."[14]

Soon it became clear that the greatest difficulty in building equipment would be designing something that could be used on board ship, that is, with the transmitter and the receiver close together. The experimental sets that were built during the first 1-1/2 years would work effectively only when the transmitter and the receiver were widely separated. A device of this type was of little use to the Navy. Unless NRL could learn how to design shipboard equipment, the whole project would soon have to be dropped. Such were the constraints on a Navy laboratory, especially in these lean years.

Nonetheless, Taylor knew that the development, if not appropriate for the Navy, might be valuable to others, in particular to the Army. In accord with this thought, he drafted a letter for the Secretary of the Navy that was sent to the Secretary of War on January 9, 1932. It officially informed the Army of NRL's investigation and Taylor's conclusions about it. The letter read in part,

> For the past eighteen months there has been under investigation at the Naval Research Laboratory, Bellevue, Anacostia, D.C., a system for detecting moving objects, especially aircraft, by use of echo signals from radio transmissions....

[11] Taylor elaborates on this term in the quote in the preceding chapter where note 29 applies.
[12] Taylor, *Radio Reminiscences* (note 1), p. 190.
[13] Letter from NRL to the Bureau of Engineering, Dec. 14, 1931, in file C-S67-5 #1 (note 7).
[14] Second endorsement, NRL to the Bureau of Engineering, of letter C-F42-1/67 (4574) in file C-S67-5 #1 (note 7).

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Nov. 27, 1934. A. H. TAYLOR ET AL 1,981,884

SYSTEM FOR DETECTING OBJECTS BY RADIO

Filed June 13, 1933 3 Sheets-Sheet 3

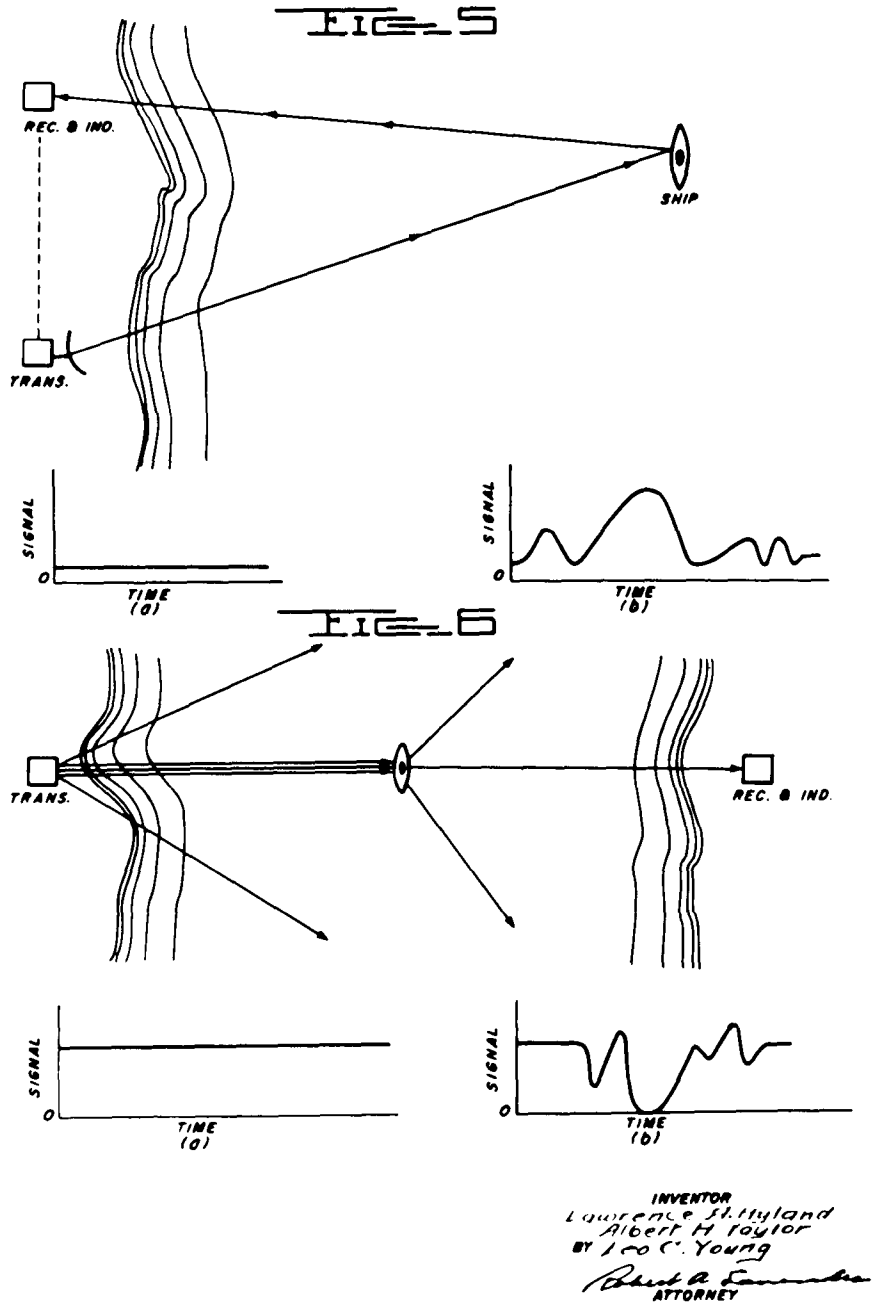

Fig. 8 — The principles of the continuous wave type of radar are evident in this patent drawing.

> Certain phases of the problem appear to be of more concern to the Army than to the Navy. For example, a system of transmitters and associated receivers might be set up about a defense area to test its effectiveness in letting the passage of hostile aircraft into the area.[15]

Actually, as will be discussed further in Chapter 9, the Army's Signal Corps had known about NRL's work since at least December 1930 but had taken no steps to initiate its own radio detection project. The main purpose of the letter, therefore, seems to have been to stimulate action at the highest level. Within a matter of months, the Army did initiate its own radar project, partly, but not solely, due to the Navy suggestion.

After making this communication, NRL continued to work on radio detection, but only sporadically. A report written in July 1932 stated,

> Some scattered observations of great interest have been made between ship and ship and between ship and shore on superfrequencies and at moderate distances not in excess of one mile which show that the passage of an intervening ship, in this case a tug, between transmitter and receiver is very distinctly observable...Under certain special conditions, airplanes in motion have been detected when they were nearly 50 miles distant from the transmitter. So far the effects from moving objects in the air are much more pronounced than those on the surface of the ground or on the sea, but comparatively little work has been done on the latter end of the problem.[16]

In short, more test equipment had been built and experimentation continued with some significant successes—like the detection of aircraft at distances up to 80 kilometers (50 miles)—but no practical, shipboard equipment was being designed.

A report of a year later showed that not much more had been accomplished. It also displayed some of the difficulties involved in assigning men to work on the problem. Leo Young wrote to the Bureau of Engineering,

> Up to the time of the furloughing of the engineer in charge of this work, satisfactory progress had been made, particularly with reference to different types of receivers, recorder systems, etc. The special receiver mentioned under investigation of super-frequencies for limited range communication [in another part of the report] is being tested and adopted for work on the location of moving objects in the air and on the ground. Only one man is now available to carry on both of these projects.[17]

In the margin of the report is penciled "B" to indicate that the radio detection problem had priority B, or, in other words, that if was classified as only "active" rather than "urgent." Following this report, extant records show little further progress until early 1934.

One important event did occur in the meantime, however. In March 1933, Carl L. Englund, Arthur B. Crawford, and William W. Mumford of the Bell Telephone Laboratories published a long,

[15]Letter from the Secretary of the Navy to the Secretary of War, Jan. 9, 1932, in file C-S67-5 #1 (note 7).
[16]Report on problem W5-2, 1 July 1932, in file C-S67-5 #1 (note 7).
[17]Report from NRL to the Bureau of Engineering covering the quarter ending June 30, 1933, dated July 15, 1933, in file C-A9-4/EN8, box 3, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building.

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detailed article entitled "Some Results of a Study of Ultra-Short-Wave Transmission Phenomena," in the *Proceedings of the Institute of Radio Engineers.*[18] There, for the world to read, were the same discoveries that stood at the basis of NRL's classified work on radio detection. In their account, the Bell engineers explained that by using very-high-frequency radio waves (Table 3, in Chapter 4), they had obtained reflections from trees, buildings, and mountains; and that these reflections caused characteristic interference patterns in their receiver. They even noted reflections from airplanes:

> It is well known that the motion of conducting bodies, such as human beings, in the neighborhood of ultra-short-wave receivers produces readily observable variations in the radio field. This phenomenon extends to unsuspected distances at times. Thus, while surveying the field pattern in the field described above, we observed that an airplane flying around 1500 feet (458 meters) overhead and roughly along the line joining us with the transmitter, produced a very noticeable flutter, of about four cycles per second in the low-frequency detector meter.[19]

After reading this, Taylor, Hyland, and Young realized that the confidentiality of their work was compromised, and they quickly began thinking about patent protection. They submitted an application on June 9, 1933. On November 7, 1934, they would receive patent 1,981,884 on a "System for Detecting Objects by Radio." It covered the idea of using interference patterns in radio receivers as a means of detecting and locating objects both in the air and on the surface of the earth: the plan they worked on from 1931 to 1934. The patent made no claims, however, about the ability of the system to determine velocity, which implies that NRL had not yet been able to develop this capability.[20]

The published article and the patent made openly available the basic principles of continuous-wave radar. Fundamental knowledge and working equipment, however, are quite different, as was clear from NRL's continuing difficulties in designing practical sets for naval use. Consequently, it was decided that the radio detection project should be kept confidential.

As we have seen, NRL's initial efforts were hampered not only by the technical difficulties involved in designing equipment suitable for ships, but also by problems of finance and priority. To some extent, the reluctance of the Bureau of Engineering to give the project strong support reflected the pressure being placed on the Bureau itself. Money for all Navy expenditures was extremely limited. The early 1930s were, of course, the depths of a great depression. Moreover the international treaties limiting naval expenditures that had been signed in the 1920s were still in effect.[21] Paying for the building and equipping of new ships was difficult enough without the drain of other expenses.

Yet, beyond these general causes, the low level of Bureau support also displayed a definite lack of interest in long-range research. Naturally, this deeply concerned the staff at NRL. In late 1931, Captain Edgar G. Oberlin, then Director of the institution, wrote a letter to the Secretary of the Navy criticizing this attitude. In one section, he related it to the development of radar as follows:

> In the detection of airplanes and probably ships by radio, although this was found feasible over a year ago, it has been impossible to secure bureau support for the further development of this vitally important problem by reason of the fact that its military value will find more

[18] Volume **21** (1933): 464-492.

[19] *Ibid.*, p. 475.

[20] File on patent 1,981,884, box 167, job order 60A-702, record group 241, records of the U.S. Patent Office, Washington National Records Center, Suitland, Md.

[21] See Donald W. Mitchell, *History of the Modern American Navy, From 1883 Through Pearl Harbor* (New York: Knopf, 1947), and Stephen Roskill, *Naval Policy Between the Wars,* vol. I (London: Collins, 1969).

> ready understanding and appreciation from higher command afloat or from a broad conception of national defense than in a crowded bureau schedule where available funds for development and equipment are already over obligated and primary bureau emphasis is placed on radio as a means of communication. On the other hand, recent discoveries which affect radio transmission were immediately taken up by the bureau as they showed a means of meeting a long recognized need and perhaps of effecting considerable economies. The last example further supports the contention that the bureaus' immediate financial interests are the controlling factor in their use of funds available.[22]

This passage shows that Oberlin was deeply concerned about why the pace of progress on radio detection was so slow. His letter as a whole, however, addressed an even larger problem. He was expressing strong opposition to an effort then being made by the Bureau of Engineering to gain complete control of NRL and its operating policies. He believed that if this maneuver was successful, it would mean the death of advanced scientific and engineering research in the Navy—the death of all long-range projects of which radar was but one example.

Fig. 9 — Captain Edgar G. Oberlin, who served as NRL's first Assistant Director and later its sixth Director, put up a determined fight to prevent a takeover of the institution by the Bureau of Engineering.

THE BUREAU OF ENGINEERING TAKES CONTROL

Oberlin learned that the Laboratory was being transferred to the Bureau of Engineering in a somewhat shocking way. One Saturday morning, a low-ranking official simply walked into his office with instructions to start taking over. Soon Oberlin found out that the transfer had been engineered by Captain Stanford C. Hooper, the Director of Naval Communications, and his superior, Admiral Samuel M.

[22] Letter from NRL to the Secretary of the Navy, Nov. 2, 1931, in file A1, box 1, job order 7184, record group 181, records of NRL, Washington National Records Center, Suitland, Md.

UNCLASSIFIED

Robinson, Chief of the Bureau of Engineering.[23] He later explained the situation to a friend in this way,

> One morning a clerk from Engineering dropped in and told me that the Laboratory had been placed under the Bureau of Engineering. Further investigation showed this had been accomplished within the space of one day several days previously. I have since learned that the matter had been discussed secretly for some time and then that Hooper had given Robinson a memorandum recommending the Laboratory be converted into a glorified test shop and turned over to the Bureau of Engineering; that Robinson on 14 October [1931] had prepared a memorandum for C.N.O. [the Chief of Naval Operations], to which he had secured the approval of C&R [the Chief of the Bureau of Construction and Repair], Ordnance [the Chief of the Bureau of Ordnance], and Aeronautics [the Chief of the Bureau of Aeronautics], and on the morning of 15 October the Secretary of the Navy approved this memorandum which placed the Laboratory under the Bureau. Naturally, I felt quite hurt that all such underhanded action had been taken without my being consulted or advised officially, and I was tempted to hang both Robinson and Hooper. But after cooling down a bit, I decided to do the sensible thing and that was to ignore any personal slight I may have been given and fight the question on its own merits. I must admit, though, that Robinson's action was quite a disappointment to me.[24]

Oberlin's description of what happened is no exaggeration. The decision to transfer the Laboratory was indeed made very quickly and was based largely on the reasoning Hooper presented in his memorandum to Robinson. The document read in part,

> In confirmation of our discussion yesterday on the subject of Bellevue, my feeling is that if the Laboratory is to be retained by the Navy it must be administered directly under a Bureau, otherwise the cost of the Laboratory will continue to mount out of all bounds, and the Laboratory become so headstrong that little good for the Navy will come out of it. The part research plays in assisting our Navy to a place superior to other navies must be attained through the use of our great commercial laboratories (in which this nation surpasses) and the Navy can never hope to own a laboratory commensurate with these. My experience has been that having Bellevue as a research laboratory actually hinders making full use of the commercial laboratories and that there is a spirit of competition between the two which results in feeling against the Navy.
>
> Frankly, I have never been able to get the results desired from Bellevue, and we never will get these results because we cannot possibly spend enough money there, so, insofar as research is concerned, I would favor abolishing the Laboratory, except that I would keep a few high-class research technicists [sic] (perhaps six) there to act as liaison

[23] Draft of a letter from E.G. Oberlin to the Secretary of the Navy, Oct. 19, 1931, E.G. Oberlin papers, Naval History Foundation, Washington Navy Yard, Washington, D.C.

[24] Letter from E.G. Oberlin to Capt. A.T. Church, Dec. 16, 1931, Oberlin papers (note 23). The Navy order placing NRL under the Bureau of Engineering was issued on Nov. 3. It is here reproduced in Appendix B.

> between the Bureau and the commercial laboratories, for the sole purpose of keeping in touch with specific research problems which the commercial laboratories are working on.[25]

In his own memorandum to the Chief of Naval Operations requesting that the Laboratory be transferred, Robinson added only that NRL's relatively independent administrative position was both poor financial management and an affront to the Navy's bureau system of organization.[26] The approval of his suggestion by other Navy officials followed just as Oberlin described.

Thus did the basic purpose of NRL and its proper place in the Navy Department once again become important issues. During World War I, debate about them had split Edison from the other members of the Naval Consulting Board and had delayed construction of the Laboratory. In 1923, the matter had been resolved by naval officers, but their primary consideration had only been putting NRL into operation, not setting forth a well-conceived, long-range policy. Now the entire subject had to be reconsidered. This would involve far more than the quick action taken by Hooper and his supporters, for Oberlin was determined not to give up the Laboratory's independence without a fight.

Although he had been surprised by the takeover, Oberlin knew well that difficulties between NRL and the Bureau of Engineering had arisen previously. As was noted in the previous chapter, Hooper, who had been deeply involved in getting NRL into operation, had been sent to sea from 1923 to 1926, the first three years of the Laboratory's activity. Upon his return to the Radio Division at the Bureau, he was unhappy with the way the leaders of the Laboratory, including Oberlin, who was then Assistant Director, were running it. In March 1927, a major disagreement had broken out. Just as he would contend later, Hooper argued then that the Laboratory was too interested in using the Bureau's money to do speculative research rather than to solve the Bureau's problems and that it thus was doing work that was better left to private companies. To get his point across, he called top Laboratory officials to a meeting at the Bureau, dressed them down, and explained that new policies would be put into effect to make NRL more responsive to the Bureau's needs. The Laboratory replied to this meeting with a lengthy, self-justifying memorandum to the Chief of the Bureau of Engineering; Hooper countered with a memorandum explaining his own point of view. The Chief, Rear Admiral John Halligan, then cooled the situation off and left matters as they were.[27] In 1931, with a new man in charge of the Bureau, Hooper had once again pressed to change the relation between it and the Laboratory. This time he had succeeded.

Hooper's views on the relation between NRL and commercial laboratories were based on more than just his own feelings. The Bureau had, in fact, been receiving some complaints that NRL was competing unfairly with private industry. On a particular level, the complaints usually related to patents. NRL employees were given commercial rights to all patents they received, and most of the members of the Radio Division were trying to use this privilege to make extra money. For a while, one small outside firm even had a standing offer to buy commercial rights on patents held by NRL radio engineers, rights it would then try to resell for profit.[28] Radio companies argued that they should not have to pay for using patents resulting from tax-supported research. Moreover, the situation made

[25]Memorandum from Capt. S.C. Hooper to Adm. S.M. Robinson, Oct. 10, 1931, in the file "Sept-Oct 1931," box 13, Hooper papers (note 1).

[26]Memorandum from Adm. S.M. Robinson to the Chief of Naval Operations, Oct. 14, 1930, Oberlin papers (note 23).

[27]See the memorandum from A.H. Taylor, Harvey C. Hayes, and Lynde P. Wheeler to the Chief of the Bureau of Engineering, Mar. 19, 1927, and the memorandum from the Chief of the Bureau of Engineering to NRL Apr. 4, 1927, both in the Oberlin papers (note 24), and also see the memorandum from S.C. Hooper to the Chief of the Bureau of Engineering, Mar. 26, 1927, in the file "March 1931," box 13, Hooper papers (note 1).

[28]Transcript of a tape-recorded interview with Dr. Louis A. Gebhard, Sept. 12 and 19 and Oct. 3, 1977, in the Historian's office, NRL, Washington, D.C., p. 21; transcript of a tape-recorded interview with Dr. Robert M. Page, Oct. 26 and 27, 1978, in the Historian's office, NRL, Washington, D.C., pp. 158 and 159; memorandum from Capt. Theelen (?) to the file, Aug. 15, 1927, Oberlin papers (note 23).

UNCLASSIFIED

them somewhat leery of cooperating fully with NRL or disclosing new ideas to the Laboratory, for fear that rights to them would have to be bought back later.

Beyond this specific competition, there was a more general rivalry. Private radio companies had always sought to sell their products to the Navy, but the importance of its business fluctuated. Just after World War I, the companies concentrated on the booming public market and gladly left most Navy radio research and development to NRL. As the firms grew and prospered, however, Navy contracts began to look increasingly attractive, and NRL began to seem a threat. The depression aggravated the situation by making Government business even more desirable.[29]

Thus some tension did exist between the Laboratory and private industry. Extant records, however, indicate that it was never very great. NRL never manufactured more than a small amount of radio equipment and never attempted to replace private laboratories. It had no desire to foster a strong spirit of competition with the companies on which it relied to get its new equipment produced. The stated Laboratory policy was to restrict research and development to subjects of special interest to the Navy that were not being explored adequately by industry, and the evidence seems to show that generally this policy was followed. Hooper's argument, then, appears to have been based more on possible conflicts rather than on actual ones.

After learning of the takeover of NRL, Oberlin was quick to unsheathe his sword. On October 22, 1931, he wrote in a memorandum to the Chief of Naval Operations: "In my opinion it would be far preferable to close down the Laboratory entirely as a research activity than transfer it as such to any Bureau." He then recommended that the entire matter be studied further by some disinterested authority.[30]

On November 2, at the request of the Secretary of the Navy, Oberlin wrote another memorandum amplifying his position. In it, he argued that NRL should be left as it was. First, he said that moving the Laboratory would eventually mean the end of research. Its function and that of the Bureau, although interrelated, were very different. Inevitably, the Bureau would replace research with engineering projects. Second, he said that the Navy needed an institution like NRL to serve as a basis for any wartime expansion that might be required. If the Laboratory were under a single Bureau, it would not be able to meet fully the research needs of the entire Navy Department, and the result would be confusion similar to what had existed in World War I. Third, he disputed the charge that NRL did not give a good return for the money it spent. This accusation, he declared, had been made without documentary evidence and could not be substantiated. Finally, he rebutted Hooper's claim that private industry could do all the Navy's research. He wrote,

> I would point out that in times of depression, commercial companies are eager to get naval work, but that in times of prosperity, an entirely different condition exists. This is true as regards bidding on naval proposals, but it is even more true as regards research. Unless the Navy can be assured that it can obtain satisfactory and necessary research work at all times, under all conditions, and at a reasonable cost, it is not warranted in taking the hazard involved, which alone warrants expenditures for a research organization.[31]

[29] See, for example, L.S. Howeth, *History of Communications-Electronics in the United States Navy* (Washington: GPO, 1963), ch. 34.
[30] Memorandum from E.G. Oberlin to the Chief of Naval Operations, Oct. 22, 1931, in file A1 (note 22).
[31] Memorandum from E.G. Oberlin to the Secretary of the Navy, Nov. 2, 1931, *ibid.*

After receiving the letter, the Sectretary referred the matter to the General Board of the Navy "for study and recommendation as to the policy which should be pursued with respect to the Naval Research Laboratory, its proper functions and its proper position in the naval establishment."[32]

The General Board was the highest and most important advisory body in the Navy Department. Formed in 1900 by Secretary of the Navy John D. Long, it had, over the years, made studies of a wide variety of subjects, many of them related to administrative problems. It consisted of top-ranking officers who were nearing retirement, and who thus were unlikely to be influenced by thoughts of their own careers as they advised what was best for the Navy.[33]

When studying NRL, the men made a thorough investigation. They visited the institution to get a first-hand understanding of its operation. They ordered accumulation of information that explained its policy, activities, and history. Finally, they held two days of hearings in January 1932, during which representatives of the Laboratory, the Bureau of Engineering, and all the other Bureaus had an opportunity to express their opinions.

Most of the arguments the Board heard had been made before.[34] Admiral Robinson contended that NRL's position under the Secretary of the Navy was contrary to the Navy Bureau system. The transfer was basically an administrative matter, he said, and should not be seen as a threat to the Laboratory's research activities. Oberlin repeated his worries about the change and claimed that having a qualified naval officer in charge was as good insurance that NRL would be properly managed as was administration by a Bureau. Hooper again stressed that NRL was doing tasks that would be better performed by industry. Under pressure, he stated quite clearly his views about research work at the Laboratory:

> *Admiral Bristol*: I want to get down to whether you believe in a research laboratory or not.
>
> *Captain Hooper*: Not a research laboratory for the Navy. I don't believe that the men who originally recommended this had the slightest idea of how our work was organized.[35]

Later Hooper recommended that after NRL had been put under the Bureau of Engineering, the Radio and Sound Divisions should be restructured so that most projects would be "design and model work." Research would be very limited: "four or five of [the employees in the Radio Division could do] research and go around and keep in touch with the commercial laboratories and report directly to the Bureau on that. Then you may need a few men to keep similar control for such things as heat, light, and sound."[36]

On February 9, 1932, the board issued its opinion. Although it incorporated points made by representatives of both NRL and the Bureau of Engineering, it was most favorable to the arguments Oberlin had presented. The ruling stated in part,

> The Board believes that the present questions concerning the Naval Research Laboratory have arisen by reason of a departure from the ori-

[32]Second endorsement to Oberlin's letter, *ibid.*

[33]Rear Admiral Julius A. Furer, *Administration of the Navy Department in World War II* (Washington: GPO, 1959), pp. 107 and 108.

[34]"Hearings of the General Board of the Navy, 18-19 January 1932," in the bound volume for 1932 in the Operational Archives, Naval History Division, Washington Navy Yard, Washington, D.C.

[35]*Ibid.*, p. 111.

[36]*Ibid.*, p. 135.

UNCLASSIFIED

> ginal purpose for which it was established. Instead of being engaged solely in research and attendent experimental work, its activities have been extended to include service test work and even production. This expansion, to the detriment of research, had been brought about by undertaking production in an attempt to supplement maintenance and reduce overhead. This condition now exists to such an extent that test and production work for the Bureau of Engineering constitutes a large portion of the Laboratory's activities....
>
> The Navy...requires a research organization capable of maintaining an active liaison with the research activities of the nation and of prosecuting research along certain lines not paralleled in American industry. Both these requirements can be met by a naval research organization based upon the Naval Research Laboratory. The board believes that the sole purpose of the establishment of the Laboratory was to conduct such activities. A research laboratory under naval control will also more surely preserve the secrecy of certain developments the publication of which would be prejudicial to national defense.[37]

As an administrative location for the institution, the Board recommended the Office of the Chief of Naval Operations. Since the CNO had general cognizance of all the material needs of the Navy, he had appropriate governing authority. At the same time, his office stood above all the bureaus, so the Laboratory would be able to function as a general research organization.

When this recommendation was routed to the Chief of Naval Operations, however, he disagreed, saying that his office lacked the administrative machinery to oversee the Laboratory and that it would be preferable to keep it under the Bureau of Engineering.[38] On February 24, 1932, the Secretary of the Navy endorsed that proposal.[39]

The investigation by the General Board, therefore, did not change the decision to put the Laboratory under the Bureau. Yet it did have an important effect. By supporting so strongly the dedication of the Laboratory to research, the Board helped insure that, in the future, research would remain its principal function. To this part of the recommendation the Secretary had agreed. Indeed, the Board's opinion became the basis for the official mission statement of the Laboratory that would be used until the 1950's: "to increase the safety, reliability, and efficiency of the Fleet by the application of scientific research and laboratory experimentation to Naval problems."

After the investigation was over, Oberlin continued his fight. Realizing that the Subcommittee on Naval Appropriations of the House of Representatives Appropriations Committee would discuss the matter to some degree, he sought to make their deliberations a full-scale inquiry. For assistance, he called on members of the inactive, but still existing, Naval Consulting Board.

Miller Reese Hutchinson, the man who had been Edison's chief assistant and served with him on the Board, had a particular interest in the matter and was most willing to help. In addition, he was a friend of Congressman William Oliver, a member of the Appropriations Subcommittee. On February 12, 1932, Oberlin wrote Hutchinson outlining the strategy to be followed. He said in part,

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[37]Memorandum from the General Board to the Secretary of the Navy, Feb. 9, 1932, in the folder on issue 410, records of the General Board of the Navy, Operational Archives Branch, Naval History Division, Washington Navy Yard, Washington, D.C.

[38]Memorandum from the Chief of Naval Operations to the Secretary of the Navy, Feb. 24, 1932, Oberlin papers (note 23).

[39]Memorandum from the Secretary of the Navy to the General Board, Feb. 24, 1932, *ibid.*

> I just returned from seeing Congressman Oliver. I reminded him of your desire to see him, and he suggested that you come down week after next. He then said he had noticed the change of the Laboratory in the appropriation bill and had intended to contact me to find out what it was all about. I told him I was not in [a] position to discuss the matter, but that my personal opinion was [that] the change was a deadly blow to scientific advancement in the Navy and would practically destroy the purpose for which the Laboratory was originally established. I added that this was a matter you wished to discuss with him, that the Consulting Board took a keen interest in the Laboratory....Mr. Oliver said he wanted to see you and would be glad to arrange for you to present your views to the whole committee. I stated that probably you and Mr. Sprague as well would like to appear before the committee.[40]

On February 25, Hutchinson wrote a letter to Representative Oliver mentioning several points he would like to bring out in the hearing. He stated that the present move by the Bureau of Engineering was contrary to everything the Naval Consulting Board had wanted and contrary to the research practice followed in industry. He summarized, in the name of the whole Consulting Board,

> We contend, Mr. Oliver, that Congress made the original appropriation with the distinct understanding that the Naval Research Laboratory would be maintained as a separate entity. In fact, if I mistake not, it was specifically understood that the Commanding Officer would rank as Rear Admiral, just to prevent that which Steam Engineering now is trying to effect.[41]

Before the hearings took place, another significant event occurred. On March 1, Captain Oberlin was discharged from his position as head of NRL. He was to continue as "Technical Aide to the Secretary of the Navy," but this job had little importance when separated from the directorship of the Laboratory. The action was almost inevitable, given Oberlin's strong stand against the transfer, because he could not now be expected to serve faithfully under the Bureau of Engineering. Yet when Hutchinson heard the news, he was furious and dashed off a letter Congressman Oliver:

> Of all the——things to do! Here is *one* Naval officer who is preeminently fitted by temperament, genius, broad-mindedness, technical training, long experience and proven executive ability to carry on the work of this Laboratory. They will probably substitute some gold laced, decorated, numb-skull, who will drift in at 10 AM and leave at 4 PM, with an hour out for lunch, who knows nothing of the problems in hand and who will disrupt the whole civilian personnel, thereby literally wasting thousands of dollars thus far expended on this work in hand.
>
> Mr. Edison was afraid of some such fool performance, when first he conceived this Laboratory idea: and he was greatly gratified up to the time of his death, to see that his fears had not been realized. He had great faith in Oberlin, "The first *technical* Naval officer I have ever met," he characterized him.[42]

[40]Letter from E.G. Oberlin to M.R. Hutchinson, Feb. 12, 1932, *ibid.*
[41]Letter from M.R. Hutchinson to William Oliver, Feb. 25, 1932, *ibid.*
[42]Letter from M.R. Hutchinson to William Oliver, Feb. 29, 1932, *ibid.*

UNCLASSIFIED

But there was nothing to be done. Oberlin had wagered his career on the fight before the General Board, and he had to pay the price for losing.

The Subcommittee on Naval Appropriations began its inquiry into the transfer on March 4, by questioning Admiral Robinson. Naturally he defended the change, saying it was best for the Navy and for the Laboratory. He spoke highly of research: "...it is, of course, necessary that the bureaus do research work. Their whole future depends on it." But he went on to comment, "The bureaus naturally know what problems are to be solved. They are looking way ahead all the time, and they are the part of the Navy Department which is in the most intimate contact with the fleet."[43]

Members of the Committee questioned his point of view. The sharpest rebuttal came from Congressman Oliver. He said,

> It is significant that, so far as I am informed, no successful industrial plant believes that what you are undertaking to do here is right. They do not practice that method themselves, and when you have great business organizations, deeply interested in the success of their own business, and who adopt an entirely different method from the one you are adopting here, then it is well to stop, look, and listen before going too far. The very fact that the General Board was opposed to this plan, and the very fact that this committee at this session has heard rumblings of possible discontent, or, at least, expressions of opinion that this is not the best way to handle it, tend to show that perhaps, you are not pursuing the right course. I have a letter, from perhaps one of the closest living friends of Mr. Edison, who says that Mr. Edison had expressed to him on several occasions his fear and apprehension lest there might some day happen just exactly what has happened so recently after his death. His idea was that the overemphasizing of testing there by your bureau such as you have outlined might ultimately lead to the action being taken that has been taken.
>
> So long as you lend undue emphasis to the testing side, or, as you call it, the experimental side of the work there, you will soon lose sight of that which is equally, yes, far more important, perhaps, the scientific and research study of great underlying problems, that not only will cause you to advance, but will invite others from the outside to come in and willingly lend their aid and assistance to you in advancing. So far as I can understand, there seems to be a unanimity of sentiment on the part of the real school of research study that this is a mistake. It is not at all aimed at your bureau, but it is simply a recognition of the practical truth that an engineering bureau is not the bureau to head and direct a laboratory for research.[44]

On March 9, Hutchinson testified before the Committee. He discussed with its members the origin of the Laboratory, Edison's intentions, the position of research laboratories in large American companies, and other matters. To some extent, his testimony became an opportunity for creating myths about Edison and his wisdom. Congressman Oliver even went so far as to remark, "Mr. Edison was not opposed, in the slightest, to a naval officer being named as the head of the laboratory; on the other

[43]U.S. Congress, 72:1, House, *Hearing Before the Subcommittee of House Committee on Appropriations...in Charge of Navy Department Appropriation Bill for 1933* (Washington: GPO, 1932), p. 510.

[44]*Ibid.*, p. 514.

hand, I understood that he felt very kindly to that idea. But he felt that the Commanding Officer should have the same rank as the Chiefs of Bureaus."[45] To this, Hutchinson simply replied, "Oh, yes."[46] Indeed, some of the misconceptions about Edison's role in the creation of NRL that were expressed that day are still believed and published as fact.

Overall, Hutchinson's testimony invoked the name of Edison and the Naval Consulting Board to support the view that the Laboratory should not be transferred. Statements of several Board members and of several leading industrialists were read into the record; all argued against the move. Admiral Robinson and Secretary of the Navy Adams were both present and challenged Hutchinson's testimony at several points, but the prevailing sentiment of the Committee was with Hutchinson, and their objections were given little attention.

Like the opinion of the General Board, that of the Naval Appropriations Subcommittee did not persuade Secretary Adams to reverse his decision. The Laboratory would remain under the Bureau of Engineering. The hearings did, however, have the effect of showing that the intent of Congress was that NRL primarily do research, not routine engineering. And one significant change did result. Before the debate, the Bureau of the Budget had proposed that the appropriation to NRL that Congress had made each year be eliminated and that the money be allocated directly to the Bureau of Engineering. The Subcommittee now decided not to accept that alteration. Congress would continue to appropriate funds specifically for use by NRL in its performance of scientific research. As we shall see, this decision would have a significant impact on the radar investigation.

Even after having gotten Congress to look into the transfer of the Laboratory, Oberlin did not cease his attempt to get NRL out from under the Bureau. Although his dismissal as Director of the institution greatly reduced his power, his plans became even more grandiose. He now called for a complete restructuring of the Navy Department, one which would eliminate the Bureau system altogether. "It is suggested," he wrote the Secretary of the Navy, "that...the Navy Department be reorganized along functional lines; that all present bureaus be abolished, and military functions distributed between 4 main subdivisions—Operations, Personnel, Material, and Inspection."[47]

Instead of abolishing the bureau system, however, the Navy decided to wipe out the Office of the Technical Aide to the Secretary, as soon as Oberlin left the Navy.[48] He was ready to go, and he retired on July 15, 1932. In writing about the matter to Thomas Robins, who had been Secretary of the Naval Consulting Board and involved in the Congressional hearings, he said,

> It may only be a coincidence, but it amused me to receive word from the Bureau of Engineering, the day after I put in my request for retirement, asking me whether I would prefer duty in Honolulu or the China Station. I told them that either place would be satisfactory as long as they wanted to get me further away from Washington.[49]

Thus the transfer of NRL to the Bureau brought Oberlin's Navy career, which had been tied to the Laboratory for almost a decade, to a pathetic end.

Oberlin had sacrificed his future for the benefit of the Navy's. He had believed that NRL was absolutely essential to keeping the Navy prepared, and he had realized that in the matter of the transfer, the institution was fighting for its scientific life. His efforts to save it were not in vain; they insured that the effects of the change on the Laboratory were not as great as he initially feared—not as

[45] *Ibid.*, p. 850.
[46] *Ibid.*
[47] Memorandum from E.G. Oberlin to the Secretary of the Navy, Mar. 9, 1932, Oberlin papers (note 23).
[48] Memorandum from the Secretary of the Navy to all bureaus and offices, June 23, 1932, Oberlin papers (note 23).
[49] Letter from E.G. Oberlin to Thomas Robins, July 1, 1932, Oberlin papers (note 23).

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great as Hooper had wanted them to be. Research work was reduced in the Radio and Sound Divisions, but it did not come to an end. And the other, basic science divisions were hardly affected. Any drastic changes that might have occurred had been moderated by the two sets of hearings. Moreover, the facilities at the Laboratory were designed primarily for research, and the staff was devoted to it. Without replacing them—and there was never any serious discussion of doing so—any alterations had to be limited.

As things turned out, being under the Bureau even had a few advantages. Following the detachment of Captain Oberlin, NRL had a succession of five directors within 3 years. The strong link to the Bureau at least provided some stability in operation. Furthermore, the situation may have helped provide financial security. Hoyt Taylor later concluded, "...it is doubtful whether our research would have been so plentifully supplied with operating funds had we remained under the Secretary during the financial depression."[50]

Still, the new administrative situation brought significant modifications in the Laboratory's operations. At the request of the Bureau, the Radio Division was split into two parts: the Radio Research Division and the Radio Engineering Division. Dr. Taylor headed Radio Research and was given a staff of but nine men. Mr. Louis A. Gebhard was put in charge of Radio Engineering and a group of 20 engineers.[51] The Bureau was leaving no question about where it placed its emphasis. Its intent was further confirmed by the growth in test problems in the Radio and Sound Divisions. In 1930, there were but ten; in 1931, 14; in 1932, 19; in 1933, 50; in 1934, 68. At the same time, the size of the staff remained relatively constant. Research and development work suffered in consequence.[52]

Fortunately, the situation did not last. As the depression began to lift, the pressure on the Radio Division to do test work instead of research eased. The isolation of the one from the other in two separate Divisions proved a failure. As Hoyt Taylor had claimed all along, they were inextricably linked.[53] On December 29, 1933, the Bureau of Engineering ordered the two Divisions merged once again.[54] A brief experiment in the creation of a separate Aircraft Radio Division also failed, and in March 1935, all radio research, development, and tests were united once again into a single Radio Division under Taylor. Concurrently, as Franklin Roosevelt assumed the Presidency of the United States and as the economy picked up, naval expenditures were increased, and money for long-range research became easier to get.

Administrative changes that occurred in the Navy Department also affected the status of the Laboratory. In 1935, Captain Hooper was relieved as Director of Naval Communications and became Director of the Technical Division under the Chief of Naval Operations. Thus he lost direct touch with NRL.[55] On May 29 of the same year, Rear Admiral S.M. Robinson was replaced as head of the Bureau of Engineering by Rear Admiral Harold G. Bowen. Bowen was a strong believer in the need for NRL and acted quickly to upgrade its position. As he noted in his memoirs,

> When I became Chief of the Bureau of Engineering, I transferred the Naval Research Laboratory, which had been under the control of the Radio Division of the Bureau of Engineering, to my own office and made it directly responsible to me. While I believe I was the first Chief

[50] A. Hoyt Taylor, *The First 25 Years of the Naval Research Laboratory* (Washington: NRL, 1948), pp. 31-32.
[51] NRL laboratory order 141, June 3, 1932 in L.A. Gebhard (compiler), *Establishment and Organizational Documents of the Naval Research Laboratory* (unpublished bound collection in the NRL library).
[52] *History of the U.S. Naval Research Laboratory, 1916-1935* (anonymous manuscript available in the NRL library, written in 1936), pp. 41-45 and Table I.
[53] Taylor, *The First 25 Years* ... (note 50), p. 32.
[54] Memorandum from the Bureau of Engineering to NRL, Dec. 29, 1933, in Gebhard, *op. cit.* (note 51).
[55] Biographical sketch of S.C. Hooper, box 44, Hooper papers (note 1).

> who even attempted to follow the developments in radio, at the same time I used every effort to prevent the Research Laboratory from becoming just another communications laboratory.[56]

Within a month after Bowen took over Engineering, NRL had been given a new Director, Captain H. M. Cooley, who would serve in that position for 4 years. Hoyt Taylor later wrote of his tenure,

> Captain Cooley did more towards selling the Laboratory to the Naval Service than any director who had preceeded him, or for that matter, almost as much as all the former directors put together....Captain Cooley himself doesn't claim to be a distinguished engineer, but he has another characteristic which was of the highest possible value to the Laboratory: namely, the ability to make friends in all quarters and to persuade every high ranking officer in the Navy who happened to be in Washington for a few days, to come down to the Laboratory and see what was going on.[57]

In sum, research at NRL, especially that in the field of radio, slacked some because of the shift to the Bureau of Engineering. By early 1934, however, the low point had passed and support began to flow once more. It increased steadily, then rapidly, in the years up until World War II.

As we shall see in detail later, the radar project was directly affected by the administrative changes discussed above. Had the dire fate Oberlin predicted when the Laboratory was transferred actually befallen it, NRL would never have developed radar. After a period of inattention, however, the project gained new support in early 1934. Soon dramatic successes were obtained. Once this happened, radar became, in turn, the Laboratory's most visible argument for the importance of all its activities.[58] Here was something commercial laboratories had not developed—and might never have. Without NRL, Laboratory officials argued, there would be no Navy radar at all. In the future, then, the growing support for NRL and its development of radar would go hand in hand.

A NEW START: THE SWITCH TO PULSES

In early 1934, activity on the radar project began to increase again. In February, it was assigned priority "A," or "urgent," the highest level of importance of any program at the Laboratory. Other problems on this level in the Radio and Sound Division at the time included developing aircraft detectors for submarines, developing the model QB sonar equipment, and developing receivers that could filter out water noise for ships traveling at high speeds.[59] Probably the new priority for the radar project related to another event. Members of the Naval Appropriations Subcommittee were coming to visit the institution and view its work; one of the things they were to see was the radio detection equipment.

The project until this time had been mostly a part-time effort by Leo Young, Raymond A. Gordon and W. F. Curtis. To help ready the equipment for the demonstration, a new man was now assigned, Robert M. Page. In retrospect, this action was the most important administrative decision of the project. More than anyone else, Page would be responsible for changing the general possibilities of radar into technological fact.

The demonstration took place in mid-February. The apparatus used a transmitter with a 60-megahertz wave frequency and a 500-hertz modulation. No record was made of the reaction of the

[56]Harold G. Bowen, *Ships, Machinery, and Mossbacks* (Princeton: Princeton University Press, 1954), p. 46.
[57]Taylor, *Radio Reminiscences* (note 1), p. 185.
[58]*Ibid.*
[59]Memorandum from NRL to the Bureau of Engineering, Feb. 7, 1934, in file C-A9-4/EN8 (note 17).

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Congressmen who saw it in operation, but later developments indicate that they were favorably impressed by it and by what they saw at the Laboratory as a whole. A year later, when Hoyt Taylor petitioned them to increase the direct Congressional appropriation to the Laboratory, they raised it substantially, as will be discussed further in the next chapter.

Nonetheless, the equipment suffered from serious problems. Page wrote in his laboratory notebook:

> This system had two major short-comings: (1) the dipole had to be pointed at the transmitter to kill the direct wave, so the direction of arrival of echoes could not be determined, and (2) the nonlinearity of response of the super-regenerative receiver made adjustments very critical for the production of beats between direct and reflected waves.[60]

He then described his thoughts on the improved characteristics needed in a practical system:

> 1. The direct wave from transmitter to receiver must be blocked down to such a level as will not prevent the detection of reflected waves.
>
> 2. It must be possible to determine the direction from which echoes come.
>
> 3. It should be possible to locate transmitter and receiver relatively close together (e.g. at opposite ends of a ship).
>
> 4. The apparatus should be capable of detecting airplanes at distances up to 50 or 100 miles.
>
> 5. It should be possible to determine whether the reflecting object is approaching or receding from the station or ship, and the rate of approaching or receding.[61]

These comments show that most of the fundamental problems of developing equipment that would be suitable for ships still remained to be solved.

After the demonstration, work slacked off, as the men returned to other problems. Page, for example, devoted most of his time during March to continuing study of a "decade frequency analyzer," a device designed to help make precise measurements of radio frequencies. When the Bureau of Engineering cancelled that program at the end of the month, however, he went back to radio detection.[62] At that point he was told to try a new idea. Instead of building a system based on continuous waves, he was to attempt one using short pulses of radio energy.

The decision to try this approach was the most important technical choice of the project. Leo Young had investigated the idea briefly in late 1933, and was encouraged by his results. Shortly after the demonstration of equipment to Congress, he convinced Hoyt Taylor, not without some difficulty it seems,[63] that it might work. Page was instructed to find out.

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[60] Robert M. Page, laboratory notebook 171, vol. III, p. 71, in the records of NRL, Records and Correspondence Management Office, NRL, Washington, D.C.

[61] *Ibid.*

[62] *Ibid.*, p. 79.

[63] Guerlac, *op. cit.* (note 1), p. 93; John M. Hightower, "Story of Radar," U.S. Congress, 78:1, Senate Document 89 (Washington: GPO, 1943), p. 8; Page interview (note 28), p. 43.

Conflicting historical arguments have been advanced about exactly what led the Laboratory to the pulse method. The prevailing thesis, which appears, for example, in the passage written by Henry Guerlac that was cited in the preceding chapter,[64] holds that NRL merely applied the principles used in sounding the ionosphere to detecting ships and aircraft.[65] Detailed research, however, shows that this argument misrepresents what actually happened.[66]

Leo Young was, without question, the man who conceived the plan to use pulses. He first thought of them in 1930, and, at that time, the reason was indeed NRL's previous work on ionospheric measurement.[67] Yet, because of the technical characteristics of the equipment that had been employed in those experiments, he quickly came to the conclusion that they would not work. As he explained later,

> While the Heaviside layer pulse techniques were developed and had been in use for a long time, [the pulses] were quite long as compared to pulses necessary for radar, or radio detection as we called it then, and the surface they were reflected from was quite large.[68]

That is, detecting airplanes and ships seemed to Young to require equipment of quite different capabilities. Thus, as we have seen, NRL chose in 1930 to work only with continuous-wave radiations, with a method that had been proven experimentally.

Young came back to the idea of using pulses late in 1933. The Laboratory had, by then, spent 3 years on the continuous-wave method and had failed to develop equipment suitable for shipboard use. A new approach was necessary. Young's reason for thinking of pulses this time, however, was not the same as before, which is why the idea could now appear promising to him. This time he derived it from an investigation he and several associates had been making on suppressing key clicks in radio transmitters and from considerations related to the work of the NRL Sound Division on sound ranging equipment.

Key clicks are radio signals that are produced unintentionally in the process of sending code signals with high-frequency transmitters; they can cause bothersome interference patterns in radio receivers located nearby, especially because they spread over a wide range of frequencies and cannot be eliminated simply by tuning the transmitter and receiver to different channels. They create great difficulties when operators on board ship are trying to carry on reception and transmission simultaneously with proximate sets.

To study the key-click problem, Young and his associates devised a means of displaying the signals visually on cathode-ray tubes. Robert Page later said about the experiments,

> We would tune a receiver far off of the transmitter, then operate the transmitter, and look at the key clicks on the cathode ray tube. It was amazing to us how narrow and how strong those key clicks were.[69]

[64]Note 59 in the preceding chapter.

[65]Essentially the same argument is made in Joint Board on Scientific Information Policy, "Radar: A Report on Science at War," (Washington: GPO, 1945), p. 5; Hightower, *op. cit.* (note 63), p. 8; Howeth, *op. cit.* (note 29), p. 446; and James P. Baxter, *Scientists Against Time* (Boston: Little, Brown, and Co., 1946), pp. 139 and 140. It is interesting that Guerlac, in his chapter "Early Radar Research in the U.S. Navy" in *Radar In World War II* (note 1), does *not* explicitly make this argument, as he does elsewhere.

[66]An account similar to what follows appears in L.A. Gebhard, *The Evolution of Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979), p. 172.

[67]Guerlac, *op. cit.* (note 1), p. 92; Page interview (note 29), p. 43.

[68]Young taped reminiscence (note 1). To be reflected from the ionosphere, radio waves must be about 10 meters long. Radar waves are generally much shorter and not reflected from that surface.

[69]Page interview (note 28), p. 44.

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Young explained the relation of the studies to his ideas about radio detection,

> I had been doing quite a bit of work on key-click measurements and suppression and had noticed that we did get some pretty short signals with key clicks. And from the powers involved and the ranges we had obtained with the doppler detection [continuous wave radar], I felt pretty sure we could get a system using the pulse method.[70]

Short, sharp pulses. This is what Young found in his key-click experiment that he had not had in the equipment for sounding the ionosphere and what made him think pulses might be suitable for radio detection of objects.

While the key-click experiments gave Young ideas about the transmitter circuits, work that was being done by the NRL Sound Division on sound ranging stimulated his thoughts about how the whole system, and especially the receiver, might work. Page noted in his notebook when he was assigned to the project,

> It was decided to attack this problem in a manner similar to that by which super-sonic depth finding is accomplished. The time axis [of the receiver] can be provided in the form of a circle on a cathode ray oscilograph [sic], and the signal [of the transmitter] can be a sharp pulse synchronized with the circle.[71]

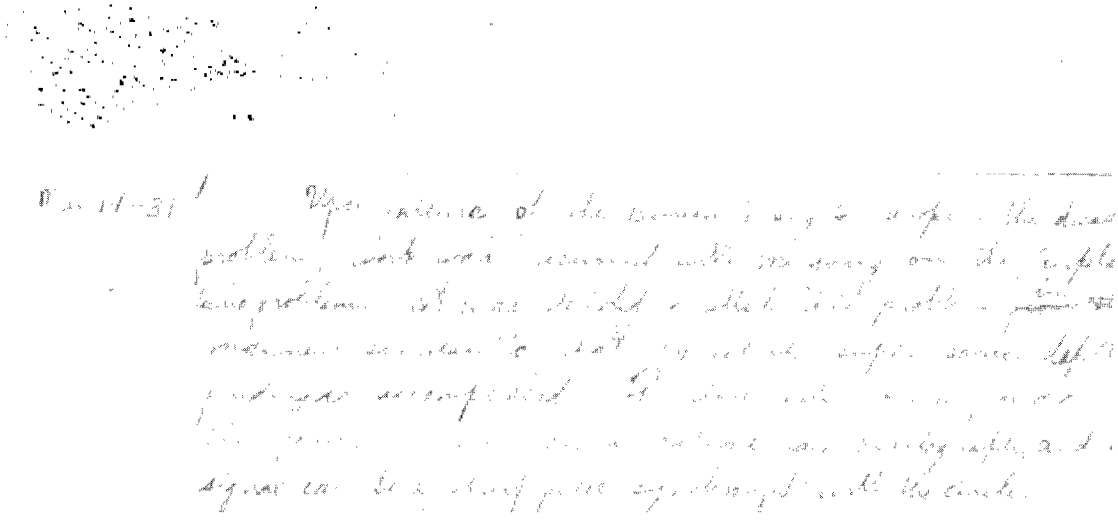

Fig. 10 — This passage from the notebook of Robert M. Page marks his switch from the continuous wave to the pulse method of radio detection.

[70]Young taped reminiscence (note 1).
[71]Page notebook 171, vol. III (note 60), p. 79.

In a recent interview, he recalled the influence of the work in the Sound Division on the new idea for radio detection in this way,

> We were doing a lot of work in the Sound Division of the Laboratory on sonar, and that had a circular sweep. I had seen it—we had all seen it—and we were familiar with it. Harvey Hayes was the head of that Division. So that was an established technique. You measure range by the position on [a] circle.[72]

Consequently the first radar receivers were designed to display pulse and echo as radial deflections on a circular sweep. The equipment was, of course, built differently from the sonar receiver, but the basic concept was the same.

In sum, the ionospheric measurements provided NRL researchers with significant experience in the use of pulses but not the impetus to build pulse ranging equipment. That came from studying key clicks and from the sound ranging equipment that had been developed by the NRL Sound Division.

PROGRESS THROUGH THE FIRST TEST

Once given the task of investigating pulses, Page knew what he had to do. He had to assemble a transmitter that could emit short, strong bursts of energy; a receiver that could withstand the transmitted signals, recover quickly, and then pick up and amplify their weak echo; and, finally, an indicator that would show both signals as outward radial deflections on a circular time scale. From March until December 1934, he worked on putting such a system together. He labored alone. Young, having made several initial suggestions, left the detailed problems up to Page to solve. Page recalled when looking back,

> In my...working with Young, I found him making suggestions for me to carry out. Young would say to me many times: "This is what we ought to do, but I don't know how to do it...you're the smart guy—you can figure it out."[73]

After the investigation of pulses had begun, work continued on the doppler method.[74] Taylor and Young were not yet ready to stake everything on the new idea. The results of this continuing effort will be discussed further in Chapter 8.

In his investigation, Page started with the indicator. It took only a few days to wire an oscilloscope so that it would produce the circular pattern desired and make radial deflections in response to input signals. The difficult task was next: building a signal generator and a transmitter. He had to make a set of instruments that would send out signals lasting only around 10 microseconds (10 millionths of a second), remain silent for 90 microseconds, and then emit another signal.[75] To accomplish this, he began by constructing a multivibrator, an instrument that produced pairs of square wave signals by switching electronically between one output tube and another. He then adjusted its circuits carefully so the signals would have the high inequality and repetition rate he needed. The shorter signal produced by the device was fed to a high-power, high-frequency radio transmitter. Page radically modified one that was already in use at NRL so that it would accept this signal, amplify, and broadcast it. Finding tubes was a difficult problem. Few on the market would give him the power he needed. He knew

[72]Page interview (note 28), p. 45.
[73]*Ibid.*, p. 48.
[74]See monthly reports on problem W5-2 in file C-A9-4/EN8 (note 17).
[75]Robert M. Page, *The Origin of Radar* (Garden City, N.Y.: Doubleday, 1962), p. 100; Page notebook 171, vol. III (note 60), pp. 78-80.

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from wave propagation theory that the strength of any echo that returned to the set was proportional to the inverse fourth power of the distance to the object; atmospheric conditions reduced the strength of the returned signal to even lower levels. Thus, to get a discernible echo from airplanes at any useful distance, the transmitted signal had to be very strong. Because of this and other difficulties, he had to spend most of the time he devoted to the project between March and December on making the transmitter function properly and making it compatible with the indicator.

For the receiver in the initial tests, Page simply used an existing piece of equipment with slight modifications. As was true so often throughout the development of radar, being at NRL where there was a wealth of knowledge and high-frequency equipment available was a distinct advantage. As Page remembered,

> I had no receiver of any kind, so [I inquired] around the Laboratory as to what I could get my hands on without having to start from scratch, which would have taken months and months. The receiver section of the Laboratory—Tommy Davis was the head of that section and he and I were good friends—Tommy Davis said he had a wide band communications receiver that I could try, if I didn't modify it too badly. I couldn't tear it to pieces. So this was the widest band thing that we had, and it was fairly high gain. In order to shorten the time constant, as I knew I would have to do, I loaded the circuits with resistance. I got it down to a time constant of probably 100 or so microseconds—something on that order of magnitude, which, of course, was much too long for the operation. But I had to get something, and that was all I could get.[76]

The short time constant was necessary so that the receiver could recover quickly from the strong transmitter pulse and then be ready for the echo.

The first test was made in mid-December, as has been described in the Introduction. It proved that a pulse-echo ranging system was possible and that the basic design of the transmitter was satisfactory. The receiver was not. It functioned—it picked up both pulse and echo—but it did not recover fast enough from the transmitted pulse to display both signals separately. Page now realized that a modified communications receiver would not suffice for this purpose. He would have to design something specifically to meet the requirements of a radar system. In the future, this would be his most difficult problem.

[76]Page interview (note 28), p. 57.

UNCLASSIFIED

6. FROM TEST TO WORKING MODEL (1934 to 1936)

ROBERT MORRIS PAGE

Robert Morris Page first reported to NRL on June 21, 1927, a scant 2 weeks after receiving his Bachelor of Science in physics.[1] His life and career are interesting not only because he was a central figure in the development of radar but also because he was, in many ways, an example of the type of person the Laboratory liked to hire during this period of its history: young, bright, and inexperienced but with signs of great promise; a man who could be brought in at a low level and then trained specifically in subjects of interest to NRL and the Navy.[2] As Louis Gebhard, who often handled personnel matters for the Radio Division, said when discussing the sort of employee he would look for,

> Not doing theoretical work, we went mainly to the engineer. The electrical engineer with radio courses and so forth...we got a few PhD's, not many. The PhD would probably be the individual we would have selected had we been doing theoretical work. But we couldn't get support for starting theoretical work. You had to do practical work. And the only reason we survived as long as we did was because of the practical results that came out of the organization.[3]

Page, seventh among nine children, was born on June 2, 1903, in St. Paul, Minnesota, and was raised in a rural area outside the Twin Cities. For the first 6 years of his life, his father, who earlier had been a school teacher, worked as a painter and paperhanger. Then, in 1909, his father took up farming as a full-time career and moved the family to the small town of Eden Prairie, where Robert spent the rest of his youth.

His first 8 years of formal education were in a one-room schoolhouse in Eden Prairie. For high school, he went to an institution in St. Paul and found himself among 2000 students. As he said in retrospect, "For the first year, I was just completely lost."[4] Yet he maintained his determination to learn.

The Page family, although not destitute, was poor. For financial reasons, Robert had to change secondary schools several times, and he even had to drop out one year. He helped pay for his education by working in the afternoons, during vacations, and in the summer. Throughout his first year, he labored with one of his brothers, an electrician, wiring houses. Besides being a source of income, this gave him his first practical experience with electrical components. Despite his financial burdens, Robert did well in school and graduated at the top of his class.

As he turned his thoughts toward college, he foresaw a specific career. He later recalled how he had conceived his plan:

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[1]Information used in this section comes from the biographical file on Dr. Robert M. Page, Historian's office, NRL, Washington, D.C., and from the transcript of a tape-recorded interview with Dr. Robert M. Page in the Historian's office, NRL, Washington, D.C.

[2]The Laboratory actually hired very few new people until the buildup prior to World War II. There was not even a formal personnel office until during the war.

[3]Transcript of a tape-recorded interview with Dr. Louis A. Gebhard, Sept. 12 and 19 and Oct. 3, 1977, in the Historian's office, NRL, Washington, D.C., p. 53.

[4]Page interview (note 1), p.3.

Fig. 11 — Robert M. Page solved most of the technical problems inherent in developing the first pulse radar for the Navy.

> I had a long talk with my Dad one time on the way to the market. We used to haul farm products to market and we'd start at 4 o'clock in the morning with horse-drawn vehicles, to go to Minneapolis with a load of produce to sell at the market. And I went with him one morning and on the way, I remember this part distinctly, we had a long talk about what I was going to do with my life. In answer to his question, I said I would like to go into some profession where I could be of useful service to my fellow man, one that had some influence, some usefulness. And Dad said, "Well, that could be in the ministry, it could be in journalism, in writing—these are the main areas where you would fulfill that kind of an ambition." And, I guess, from that, my ambition was to go into the ministry. With that in mind, when I graduated from high school I went to Hamline University to prepare for the ministry.[5]

The family influence on Robert's choice was strong. Religion was always important in his home; his father often preached as a lay minister on Sundays. Hamline University was a church-supported institution in St. Paul that one of his brothers had also attended. Robert never gave serious consideration to any other school.

During his initial years of college, he changed his plan to become a clergyman. There are several reasons. First, and perhaps most importantly, he found that he was not effective as a public speaker. He recalled,

> I went out occasionally with teams of young people who spoke at religious meetings, supposedly witnessing on religious subjects. And I

[5] *Ibid.*, pp. 6 and 7.

UNCLASSIFIED

> found myself trying to do something which led me into one embarassment after another. I was a complete flop at it.[6]

Secondly, his faith was shaken because of his exposure to the questioning of fellow students and professors. He remained deeply religious, but his growing awareness of the intellectual difficulties and demands of a thorough understanding of Christian doctrine made him wonder seriously if he should devote his life to preaching.

Finally, he became increasingly interested in and involved with science. His physics professor, Jens M. Rysgaard, exerted a strong, formative influence on him. Page later remembered the intellectual aspect of it in this way:

> I had not yet had advanced algebra, and [Prof. Rysgaard] combined physics with advanced algebra and differential equations so that we learned our mathematics with our physics and by means of our physics. The result was that I had a physical understanding of what the mathematics meant. Differential equations and integral equations became living things to me because they represented physical phenomena....Throughout my career...my understanding of nature—my understanding of physics—was a conceptual understanding....I could see the significance, qualitatively, well enough to invent and predict, to research in my own mind without physical manipulation and come to answers—come to correct results.[7]

On the practical side, Rysgaard persuaded Page to pursue a scientific career.

At first, he thought he might become a physics teacher, and to this end he took a minor in education. But Rysgaard was a friend of Hoyt Taylor, who happened to come to Hamline on a recruiting trip while Page was finishing his degree. Page did not meet Taylor at this time, but Rysgaard, after the trip, advised his student to take the civil service examination in physics. He did so and received top marks in every category. Coupled with his standing at the head of his college class, this made him very attractive to prospective Government employers.

Page's first job prospect came not from NRL but from the Department of Terrestrial Magnetism of the Carnegie Institution of Washington which offered to send him to a station in Huancayo, Peru. Excited by the exotic appeal of this position—he had never been outside of Minnesota—and fearing he would get no other choice, he almost accepted. Rysgaard, however, convinced him to wait. His next offer was from NRL.

When he came to the Laboratory, he had little experience in radio engineering, so at first he was assigned to help others on projects already underway. He quickly discovered that he had a knack for making inventions—for finding answers that were unapparent to his new colleagues. Soon his supervisor was saying that he "had more ideas than a dog had fleas,"[8] a remark Page recalls as the first comment he heard about his performance.

In the light of his new-found ability, he began to appreciate the patent policy of NRL, which allowed him full commercial rights on any patent he produced. He remembered,

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[6] *Ibid.*, p. 12.
[7] *Ibid.*, p. 18.
[8] *Ibid.*, p. 32.

> Within two weeks after I went to the Laboratory, I became imbued with the idea that I was going to be an inventor, that I was going to invent things and that I was going to have patents on them and that the commercial rights were going to be worth something. It was all just a hunch. But on the basis of that hunch, I turned down every offer of employment everywhere, because this was the one place that gave me commercial rights on my inventions. I had no reason to believe, no way of knowing that it would ever amount to anything, but...I had great faith that it would happen. It was a hunch, that's all. And as it happened, it panned out.[9]

Until beginning on the radar project, Page concentrated on studying precision measurement of frequency. His last effort in this field was the design of a decade frequency analyzer, a device that would allow a direct reading from a set of nine dials of the frequency being measured. He never carried the project to completion, but later a similar idea would lead to the decade frequency synthesizer, which became a common component in ham radio. His project was cancelled by the Bureau of Engineering in 1934, and it was at that time that he was reassigned to radio detection.

For the remainder of his scientific career, he would work on this subject. As we shall see, he made most of the basic inventions leading to the first practical shipboard equipment. In subsequent years, he would originate many other ideas in the radar field. The 65 patents he eventually would receive are one good indication of his productiveness.[10] He would also rise in rank at NRL, mixing administrative with technical work for a while and, for the last 15 years of his career, doing only administration at high levels. The pinnacle of this period was his tenure, from 1957 until he retired in 1966, as the Laboratory's third Director of Research, the highest job open to a civilian scientist. Yet, he was never as effective in administration as he had been in engineering, and in retrospect he remembered his earliest achievements most fondly. A few years before retiring, he said,

> [My] greatest satisfaction [while] working [at NRL] was back in the days when I was most productive at the bench, when I could look at a radar circuit diagram or an electronic circuit diagram that covered two pages and in one glance I'd see the whole thing, and I could go back and tell you the value of the constants and why. Those were the glorious days. After having left and thinking back over it, the satisfaction of knowing what I gave the country is one of the greatest satisfactions I could have.[11]

DESIGNING A RECEIVER

In the time he spent on the radio detection project between December 1934 and November 1935, Page concentrated on designing a new receiver.[12] His initial problem was theoretical. He understood that the device would have to have a rapid response time and thus have resonant circuits with a low selectivity, or, in technical language, a low Q value. It also would have to have many amplifier stages, each of which would affect the total Q for the composite. At first he was unable to derive the mathematical equations that showed the relationship of parts to the whole, given the condition that the receiver was to be excited by an extremely short pulse. He recalled,

[9] *Ibid.*, pp. 159 and 160.

[10] A list of them is on file in the papers of Robert M. Page, Historian's office, NRL, Washington, D.C.

[11] Tape-recorded interview of Dr. Robert M. Page by Mr. Ernie Smith and Mr. James Sullivan, filed in the Historian's office, NRL, Washington, D.C.

[12] Principal sources for this section are Robert M. Page, *The Origin of Radar* (Garden City, N.Y.: Anchor, 1962), chs. 4 and 5; *idem*, "The Early History of Radar," *Proceedings of the Institute of Radio Engineers* **50** (May 1961): 1232-1236; *idem*, laboratory notebook 171, vol. III, and laboratory notebook 346, vol. IV, both in the Records and Correspondence Management office, NRL, Washington, D.C.; and the Page interview (note 1).

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> I worked a long time on the solution of the receiver problem—the receiver design. My first attempts in solving the thing mathematically were not successful. And looking back on it, the reason they weren't successful was that I didn't go back to the original derivation of the equation for [circuit] decrement. I took the completed solution for the equation for the decrement of a single circuit and then tried to solve for a series of circuits, all with the same decrement, and it didn't work out that way.[13]

Fortunately, he soon benefited from a stroke of luck. He discovered in the French periodical *L'Onde Électrique* an article on "Time Constants, Buildup Time, and Decrements"[14] that had been published in June 1934. It taught him how to determine the relationship between individual Q values, the time constants, and the total gain of a multistage amplifier. He wrote what he had learned in his notebook and thence derived the basic design characteristics the receiver had to possess.[15] The date, although not recorded, was sometime in late January or February 1935.

The basic theoretical problem was now solved, but numerous practical difficulties remained. Page had to find the right components and the correct way to wire them. He had to determine how to get the receiver to recover in several microseconds, so that it could record echoes from nearby objects immediately after the transmitter pulse had been emitted. Feedback would have to be eliminated entirely to prevent unwanted oscillation of the amplifier tubes and, more importantly, to prevent any increase in the time constant of the circuit. This would require extreme precautions in shielding, filtering, and grounding.[16] Determining the correct design took much experimentation and testing. One example will serve to indicate what was involved. As part of his task, Page had to find very-high-frequency tubes that he could wire in circuits having low capacitance and low Q. By another stroke of luck, which demonstrates how closely he was working to the edge of advancing technology, the RCA Corporation had recently developed an "acorn" tube that could meet his basic requirements. But he still had to learn experimentally how to make it function in his special circuits, as is evident in this passage from his notebook:

> Some...work was done with acorn pentode tubes to find what order of Q was possible. A Hammerlund midget 20 $\mu\mu$f condensor was altered to reduce minimum capacity. Original minimum was 5.7 $\mu\mu$f, maximum, 18.2 $\mu\mu$f. The rear support and bearing were removed, one stator plate removed, the other stator plate cut down in size, the rotor plate cut back to increase edge distance at minimum capacitance, and one of the other stator supports removed. Each change brought a reduction in minimum capacitance. The final form left the stator supported at one point, and the rotor supported in one bearing. The final capacity range was 1.7 $\mu\mu$f to 8 to 10 $\mu\mu$f depending for maximum capacitance on plate spacing.[17]

Such radical alterations, which were necessary to obtain custom parts with the required performance, characterized the extensive experimentation Page made.

Technical problems were not the only ones he faced. Support for the project from the Bureau of Engineering remained low. At one point, it virtually disappeared. Page wrote in his notebook, "At Mr. Young's direction, the receiver was designed so as to cover a large frequency range if necessary, for

[13] Page interview (note 1), pp. 62 and 63.
[14] René Mesny, "Constantes de temps, durées d'établissement, décréments," *L'Onde Électrique* **13** (June 1934): 237-243.
[15] Page notebook, vol. III (note 12), pp. 106-111.
[16] Page, *The Origin...* (note 12), pp. 71-79; *idem*, "The Early History..." (note 12), pp. 1234 and 1235.
[17] Page notebook, vol. III (note 12), p. 111.

general high-frequency use, and the work charged to the new high-frequency communications problem 62-V-146."[18] That is, the equipment was disguised as something for communications so that funds could be siphoned from another project! Clearly there was a limit on how long this could continue.[19]

The low priority also affected the fate of Page's design when he submitted it to the shop, sometime in early March. Action was slow. He noted at the end of April, "As the shop has been held up on work on the receiver, [it] stood half built with no progress for two months—consequently is unfinished at this date."[20] The situation remained bad. Not until late November would construction be complete.[21]

Finally Page was spending much of his time on other projects. This was due partly to the status of radio detection and partly to his own broad interests. He remebered,

> I was under no pressure to get anything done in a hurry and that's one reason I so easily went to other projects as they presented themselves. I was interested in them; I would pick them up and do them. All of the psychological pressures to move and get some results came from Taylor, and they came from him largely because financing required demonstration of results and he was always having difficulty getting enough finances to do basic work.[22]

Indeed, during the same months Page had labored sporadically on the receiver, his boss had been striving to improve the financial situation of the Radio Division.

A CONGRESSIONAL VOTE OF CONFIDENCE

Taylor had concluded, sometime in early 1935, that the time was ripe to request new research money from Congress. With the approval of the Director of the Laboratory and the chief of the Bureau of Engineering, he and Harvey Hayes, the Head of NRL's Sound Division, went up to Capitol Hill. They visited James Scrugham, the most influential member of the Naval Appropriations Subcommittee at the time and an engineer by training. Taylor recalled the meeting in this way:

> We put up a strong plea for a substantial addition to the small direct appropriation which the Naval Research Laboratory usually received from Congress, this increment to be earmarked for long time investigations, particularly in the field of microwaves and supersonics. Mr Scrugham listened in silence, asked a few questions, but promised us nothing. We left his office feeling very much discouraged, but on the following Monday morning, he telephoned to state that the Committee had agreed to give us an extra $100,000.00 to be spent on this work. This looks like a small amount in these days but it looked like ten million dollars to us then.[23]

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[18] *Ibid.*

[19] Page remembers that the project was actually canceled (interview (note 1), p. 53). However, this recollection appears to be in error, as the Laboratory kept making monthly reports on the effort (files C-A9-4/EN8 in boxes 3 and 4, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building). There is no doubt, however, that support dropped.

[20] Page notebook, vol. III (note 12), p. 120.

[21] *Ibid.*, p. 145.

[22] Page interview (note 1), p. 69.

[23] A. Hoyt Taylor, *Radio Reminiscences* (Washington: NRL, 2nd printing, 1966), p. 173.

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The additional money boosted the fiscal 1936 appropriation from Congress by 51% over that of fiscal 1935,[24] and NRL was assured that the higher level of funding would be continued in future years.

In early May, an extended discussion began between the Laboratory and the Bureau of Engineering over how to spend the money.[25] It was decided that the bulk of it, about $56,000, would go to expanding scientific personnel. The rest would go for shop labor ($20,000), clerical help, and research materials. The crucial part of the discussion centered on the status of various projects at the Laboratory and how the new people and supplies would be allocated among them. In a letter of June 25, NRL proposed that new men be assigned to 15 studies in the following order of priority: investigation of microrays, propulsion of submerged submarines, study of radio superfrequencies, study of aircraft homing devices, photoelastic investigation of ship structures, study of dehumidifying agents for submarines, underwater sound research, *study of the detection of ships and aircraft by radio* (emphasis added), study of underwater radio reception, study of recognition signals, study of the fouling of paints for ship bottoms, study of dazzle camouflage, study of the direct conversion of heat energy into electrical energy, and study of the measurement of radio-frequency power.[26]

There are several notable aspects of this list. First, it shows the range of important investigations underway. Second, it displays that Page's pulse radar project was not considered by the Laboratory to be the most significant of them at this time. Finally, it demonstrates quite clearly the emphasis being given to studies of microwaves. In the past, NRL had pioneered many uses of high frequencies, and now Taylor wanted it to lead the way into the use of even shorter radiations. More will be said about the relation of this investigation to the radar story in Chapter 8.

When responding to these recommendations, the Bureau of Engineering made several changes. Most importantly, it moved the radio detection problem from ninth in priority to third. Only the investigation of microwaves and of propulsion for submerged submarines were put above it. This seems inexplicable, given that the Bureau had let money for the effort become almost exhausted several months earlier, but perhaps the change was due to the influence of Harold G. Bowen, who had become Chief of the Bureau on May 29 and who had signed this letter to the Laboratory. In addition to raising the status of radio detection, the letter added two other related projects: the study of detection of aircraft by acoustic methods and the development of either radio or acoustic detection equipment for submarines.[27] Clearly the Bureau now had a renewed interest in technology to warn of enemy attack.

After receiving this response, Captain Cooley, the Director of NRL, wrote an internal memorandum explaining what the Laboratory would do. He accepted the new priority for radio detection, but about acoustic methods, he told his staff,

> We did not recommend this idea inasmuch as we do not believe a solution is feasible and the Army and Navy have already spent about $500,000 on the subject. We strongly recommend against further expenditure on this problem, believing that the solution will be found by other methods.

He added that development of any equipment for warning submarines of air attack hinged on successful solution of the radio detection problem.[28]

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[24] See Table 2, in Chapter 4.
[25] See File LI-1(3), 1936, in Box 34, records of NRL, Unclassified Series, record group 19, National Archives Building.
[26] Letter from NRL to the Bureau of Engineering, June 25, 1935, in file LI-1(3) (note 25).
[27] Letter from the Bureau of Engineering to NRL, July 12, 1935, in file LI-1(3) (note 25).
[28] "Memo for Information and Guidance of the Naval Research Laboratory," from H. M. Cooley, Director, July 24, 1935, in file LI-1(3) (note 25).

Because of its new priority, the radar investigation was allocated a second man. Funds became available in July, but not until November was it decided that Robert C. Guthrie, an engineer already on the staff at the Laboratory, would be ordered to work on the project full time. He reported to Page on November 22.[29]

Fig. 12 — Robert C. Guthrie was the second man to become heavily involved in developing pulse radar and proved to be a highly capable assistant to Robert Page.

Guthrie's background was similar to Page's. Son of a sheep rancher in Montana and raised in a rural area, he had graduated from the University of that state in June 1929 with majors in physics and mathematics. He took the civil service examination during his senior year, and this led to his receiving the offer of a job at NRL. Since it paid more than another position he was tendered by Bell Laboratories, he took it. He reported to work in July 1929 and spent his entire scientific career in the institution. Once he got involved with radar in 1935, he, like Page, stayed with it. Ultimately he would rise to become the head of NRL's Radar Division, a post he held from 1954 until he retired in 1964.[30] Guthrie did not have Page's inventive ability, but he proved to be a highly competent engineer, and together the two men made an effective and harmonious team.

The new status of the radar project also led to more pressure being placed on Page to get results. He recalls that Taylor came to him and said, "Well, this problem looks like it isn't getting anywhere; I'll give you 6 months to produce some results and I'll give you a helper....If you can't show some results [on] this with a helper in 6 months, we'll cancel it out."[31]

[29] Robert C. Guthrie, laboratory notebook 170, in box 11, job order 67A-6317, records of NRL, record group 181, Washington National Records Center, Suitland, Md., p. 20.
[30] Biographical file on Robert C. Guthrie and tape-recorded interview with Robert C. Guthrie, April 13, 1978, both in the Historian's office, NRL, Washington, D.C.
[31] Page interview (note 1), p. 53.

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One final impetus to move more rapidly came from knowledge about developments abroad. In the summer of 1935, a radio device to detect icebergs, ships, and other obstacles—actually a crude form of continuous-wave radar—was installed on the French liner *Normandie*.[32] As it was not a secret development, there were numerous reports about the equipment in the press, and word soon reached NRL. Leo Young wrote in his monthly report to the Bureau of Engineering for October,

> Several recent news items report the French liner S.S. *Normandie* as being equipped with a system for detecting objects in her path. The system is said to use a 16 centimeter wave and to detect ships at 4 kilometers by use of reflected waves. It is requested that the Office of Naval Intelligence be requested to obtain, if possible, information or details on this installation and its performance.[33]

Although Young and his colleagues knew from their own experience that the pulse method would probably produce a better shipboard device than what the *Normandie* had, the awareness that other nations were following similar lines of research made it obvious that there was no time to lose in developing their own equipment.

The confluence of these various changes brought an end to hesitation. Laboratory records indicate that once Guthrie began working with Page, both men spent almost all their time on the radar project.

A NEW EYE FOR THE NAVY

After Page finally got his receiver from the shop in November 1935, he had to make tests and modifications. Basically, the equipment worked as planned, but months of troubleshooting lay ahead. His logbook for this period reports a series of tests, adjustments, replacement of parts, and so on. For example, he noted in late November,

> One fundamental weakness appeared in this receiver. The input circuits were not sufficiently isolated from the second oscillator-amplifier, so that the receiver was subject to a paralyzing signal on every harmonic of this oscillator.[34]

The answer, of course, was better shielding, but it took time to figure out how best to obtain it.

During the same period, Guthrie labored to improve the transmitter. Soon Page suggested that he use a "self-quenching" or "squegging" circuit. Guthrie recorded the new idea on December 6, and wrote, "It was decided to try a different type of impulse generator....This should have the advantage of giving enough power to require no additional amplification. It can be made the actual transmitter."[35] That is, instead of using a multivibrator to key a separate transmitter, as Page had done in December 1934, the transmitter circuit was wired to key itself. The economy of design helped provide the high-power rapid pulse that was needed.

The idea had originated in a conversation Page had with La Verne Philpott, another member of the Radio Division and a close friend. Once again the importance of working in a community of radio engineers is clear. Page recalled the conversation in this way:

[32] Charles Susskind, *History of Radar: Birth of the Golden Cockerel* (manuscript in preparation), pp. 19-23; E. Giboin, "L'évolution de la détection électromagnétique dans la marine nationale," *l'Onde Électrique* **29** (Feb. 1951): 53-64.

[33] Report from NRL to the Bureau of Engineering, Nov. 1, 1935, in file C-A9-4/EN8 (note 19).

[34] Page notebook, vol. III (note 12), p. 146.

[35] Guthrie notebook 170 (note 29), p. 21.

> I had been using a squegging oscillator as the test oscillator in the Laboratory for some time. [I used it as] my pulse signal generator....Philpott suggested, "Why don't you use that circuit in your transmitter?" That was his input right there. It was unofficial, oral, completely informal between us, and of course that was the solution. That was the flash. Sure, that's what we'll do.[36]

Subsequently, Guthrie transformed the idea into wires and components, but that task also took months to reach completion.

By December 17, Page had decided to replace the circular time sweep, that he had been using in his cathode-ray indicator since March 1934, with a linear sweep and logarithmic scale.[37] Although this pattern did not have the virtue of returning on itself and thus being limitless, it was simpler to use and easier to interpret. Thus began what was later called the "type A" presentation.

By the end of March, the men had their equipment developed well enough to begin thinking about a practical test. Although the trial of December 1934 had been with a frequency of 60 megahertz, they now chose to drop to 28.3 megahertz. At the lower level, they could use a large directive antenna, a "curtain array," already in existence at NRL. This would allow them to experiment with a large amount of power without bearing the expense of a new antenna. As Page recalled,

> The construction of an antenna was a big undertaking. I hadn't yet come to the point in my career where I took seriously the responsibility for building something big. I was building little circuits on bread boards. A great big antenna—that was clear out of my class. And here was one ready made, so we matched to that antenna.[38]

Within another month, the transmitter, receiver, and antenna were all wired together, all debugged, ready for the first test. The transmitter was located in the field house and connected to the huge curtain array stretched, like a huge web, between 60-meter (200-foot) towers nearby. The receiver was positioned in the penthouse of another building, connected with an 80-ohm cable (appropriately brand-named "giant killer") to a much smaller antenna tacked between wooden posts on the roof. Both the transmitter and the receiver were extremely fragile, with wires and dials jutting out in all directions and tubes glowing unprotected.

The initial test was run on April 28.[39] Success came immediately. Planes flying about randomly were picked up at distances of 4 kilometers (2-1/2 miles). The echoes were clear and distinct. There was no smearing out or fuzziness, and the received pulses were as sharp as those transmitted. The ringing of the receiver that had marred the test of 18 months earlier was completely gone.[40] The next day, the plate voltage in the transmitter was jumped to 5000 volts and an aircraft was followed out 8 kilometers (5 miles) and back.

By now it was clear that Guthrie and Page had attained the goal Taylor had set for them. To be sure, much effort remained in developing reliable equipment for ships, but there could no longer be any doubt that it would become a reality. Radar had been invented. A new eye had been opened for the Navy.

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[36] Page interview (note 1), p. 75.
[37] Page notebook, vol. III (note 12), p. 148.
[38] Page interview (note 1), p. 76.
[39] The date may actually have been April 29. Page's and Guthrie's notebooks disagree on this part, see Page notebook, vol. IV (note 12), p. 8; Guthrie notebook 170 (note 29), p. 32.
[40] Page, *The Origin. . .* (note 12), p. 85. Guthrie notebook 170 (note 29), p. 32. The receiver used in these tests was given by NRL to the Smithsonian Institution in 1970.

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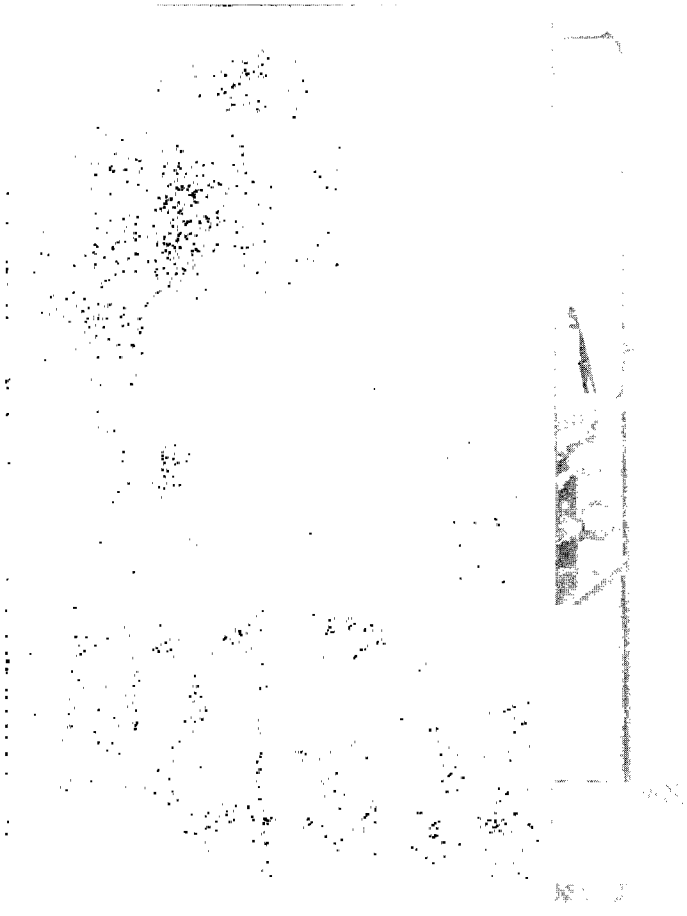

Fig. 13 — This transmitter, which was used in tests during the spring of 1936, was capable of generating 28-mHz signals of 3.5-kilowatts peak power and 7-microseconds duration.

SEEING IS BELIEVING

The time had come for demonstrations. On May 6, the equipment was displayed to Taylor, Harvey Hayes, Captain Cooley, and other leaders of the Laboratory,[41] and a distinct reflection was obtained from an airplane 27 kilometers (17 miles) away. When, years later, Robert Guthrie was asked at what point it became obvious that radar would be far more than a routine project, he pointed to this demonstration saying, "It was realized right when they saw those airplanes at 17 miles on that unit."[42] His memory of the event was so strong that he recalled, without hesitation, the exact distance.

A demonstration of perhaps even greater importance, however, came on June 10. Commander Wilbur J. Ruble and Lieutenant J. B. Dow, both of the Radio and Sound Division of the Bureau of Engineering, came to the Laboratory to see the equipment in operation. Again, it functioned perfectly. Within two days, a letter signed by Admiral Bowen, head of the Bureau, was dispatched to change the status of the project. It read in part,

[41] Page notebook, vol. IV (note 12), p.9.
[42] Guthrie interview (note 30), side 1.

Fig. 14 — This receiver was used in the spring of 1936 in conjunction with the transmitter shown in the previous illustration. In both figures, the experimental nature of the equipment is plainly evident.

> The demonstration indicate[d] that material progress toward the solution of this problem has been made by the Laboratory, and in view of the importance of the subject and the fact that a practical solution now appears feasible, the Bureau requests that the remaining work be given the highest possible priority.
>
> It is requested that work in the immediate future be carried out with a view to providing for shipboard use one equipment based upon the use of a manual and motor driven beam operating at the highest frequency consistent with obtaining the required power with a view to providing in a single equipment the means for both detection and ranging. The Bureau will discuss with the Laboratory the detailed requirements as to size and location of such equipment aboard ship as soon as progress has been made to an extent warranting discussion of such details.
>
> It is requested, upon receipt of this letter, that the subject problem be placed in a secret status, that all personnel now cognizant of the problem be cautioned against disclosing it to others, and that the number of persons to be informed of further developments in connection therewith be limited to an irreducible minimum.[43]

[43] Letter from the Bureau of Engineering to NRL, June 12, 1936, in file C-567-5 #1, box 31, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building.

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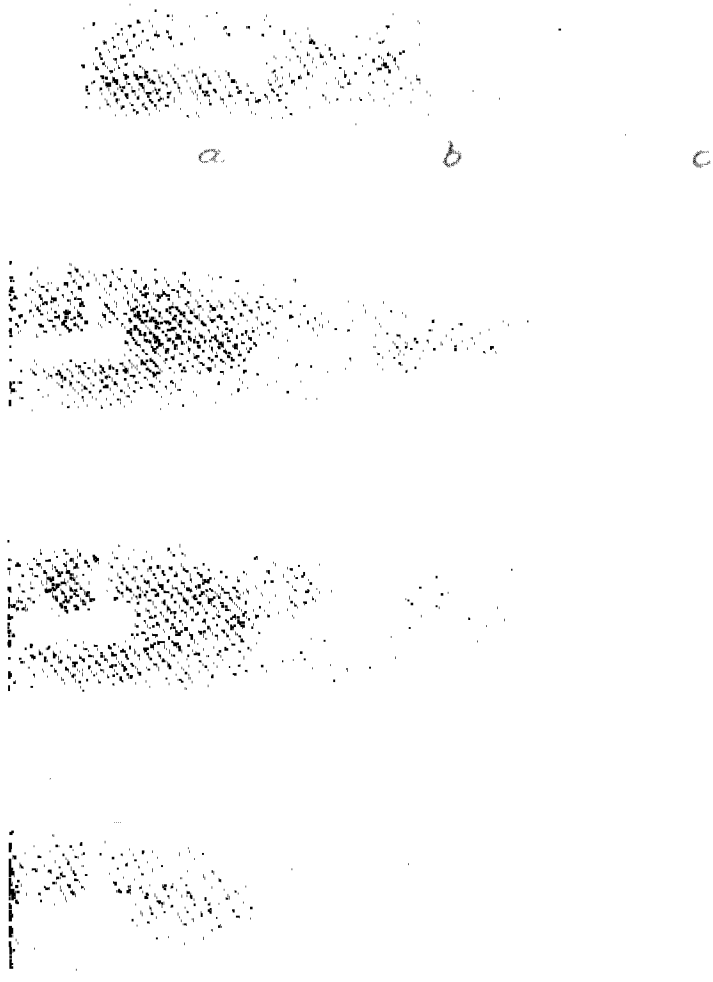

Fig. 15 — Echoes from the radar equipment used in 1936 appeared as above. In (a) only the sweep current is shown; in (b) the sweep current and the transmitted pulse; in (c) the transmitted pulse and nearby ground clutter; and in the remainder of views, the echoes from airplanes as well as what appears in (c).

The initial phase of the radar project, the invention phase,[44] was now complete. The letter from the Bureau marked the transition administratively. On the technical side, it was indicated by a letter Robert Page wrote to the Bureau at almost exactly the same time. This letter related in detail the technical developments up to that point and recommended that the Navy submit secret patent applications on the basic ideas of pulse radar.[45]

In the months ahead, Captain Cooley had the equipment demonstrated to many top-ranking naval officials, including the Commander in Chief of the U.S. Fleet, the Chief of Naval Operations, and the Assistant Secretary of the Navy. It was the most effective way he had to impress on them the importance of NRL and to strengthen their support for the institution. Radar and the Laboratory were both entering a new period of development.

[44] For an interesting treatment of the various phases in technical development, see John Jewkes, David Sawers, and Richard Stillerman, *The Sources of Invention*, 2nd ed. (New York: Norton, 1969).
[45] Due to its importance, this letter is reproduced in Appendix F.

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7. FROM MODEL TO OPERATIONAL EQUIPMENT (1936 to 1940)

THE PATH TO PRACTICALITY

In mid-1936, radar was still very much an experimental device. Pieces of equipment spreading over shelves of laboratory work cases, wires dangling in air, a transmitting antenna measuring 60 meters square (200 feet square) and stretched between rigid towers 75 meters (250 feet) apart, and a separate receiver antenna mounted on a wooden frame were hardly components ready for shipboard use! Furthermore, as reflected by the state of the apparatus, the knowledge Page and Guthrie had acquired was as yet quite limited. They had concentrated only on putting a test set into operation. To make radar practical and effective, they now would have to expand their understanding. What frequency would be best for naval use? How could it be obtained with sufficient transmitter power? What type of antennas should be employed? To what extent were radar pulses affected by atmospheric conditions and by land and sea clutter? What was the best means of display? How could the delicate laboratory equipment be modified to withstand the shock and vibration experienced by naval vessels? These were some of the difficult questions that now had to be addressed.

The success to date, however, had won the project time and increased support. On May 8, 1936, another engineer, Arthur A. Varela, was assigned to it.[1] In June, two more were added, and soon there would be others, including Page's nephew, Irving Page. By September 1940, the number would climb to around 12.[2] Although not overwhelming, this expansion was without parallel in the history of the Laboratory.

Once Varela reported to work, Page assigned him the task of developing a set on 200 megahertz. There were several reasons for this decision. Higher frequencies were necessary to reduce the size of the antennas for transmission and reception. Moreover, they could be focused more easily and would give better target resolution. Only the difficulty of obtaining sufficient power restricted how high one ought to go. Page believed that 200 megahertz was about the limit with tubes currently on the market. Also, the National Bureau of Standards had developed a 200-megahertz radio receiver that would make building the new radar receiver relatively simple. With slight modification, this equipment could be used to detect the echoes; they could then be converted to a lower frequency; and, finally, they could be amplified without distortion by the receiver Page had already employed successfully on 28 megahertz.[3] All things considered, 200 megahertz seemed the best choice for quick practical development. Still, there was no guarantee that this frequency would work, so Page had equipment built on 50 and 80 megahertz as well.[4] In addition to being insurance, this equipment helped clarify the relationships between frequency and other characteristics of performance.

While the new sets were being developed, much attention was devoted to components, circuits, and especially antennas. The curtain array used previously had consisted of stacked dipoles in a vertical plane. Page and his associates now began to wonder if other forms might prove more practical.

[1] Robert M. Page, laboratory notebook 346, vol. IV, p. 12, in records of NRL, Records and Correspondence Management office, NRL, Washington, D.C.

[2] Bureau of Ships memorandum for Admiral Van Keuren, Sept. 30, 1940, in the NRL historical file, Historian's office, NRL, Washington, D.C.

[3] Page notebook, vol. IV (note 1), pp. 12 and 31; transcript of a tape-recorded interview with Dr. Robert M. Page, Oct. 26 and 27, 1978, in the Historian's office, NRL, Washington, D.C., pp. 91 and 92.

[4] Page notebook vol. IV (note 1), pp. 27 and 28; Robert C. Guthrie, laboratory notebook 170, in box 11, job order 67A-6317, records of NRL, record group 181, Washington National Records Center, Suitland, Md., pp. 41-45.

They investigated several types, including the Yagi array, which was far less cumbersome and could be rotated and pointed easily.[5]

Out of this study came a remarkable new invention. Initially NRL radars had required two antennas: one for reception, one for transmission. Hoyt Taylor, however, believed it should be possible to perform both functions with just one. Because of the general importance of restricting additions to the superstructure of naval vessels, he insisted that the idea be investigated. Young relayed the order to Page, who remembered his reaction in this way:

> Well, when Young broke the news to me, I said, "Why that's utterly impossible. There is just no way! The receiver just couldn't possibly take that power from the transmitter." Young said, "Well, think about it. There ought to be a way." And I did. [Soon], a fresh idea dawned on me; it was an inspiration type of thing so that the basic duplexer circuit was my invention.[6]

That is, despite his initial skepticism, Page was able to design a new instrument, the radar duplexer, that did allow both transmitter and receiver to use a common antenna. The duplexer was an electronic switch that short-circuited the receiver during the time the transmitter was active, thus directing the pulse to the antenna. Then when the transmitter was off, it relayed weak echoes from the antenna to the receiver circuits.[7] In retrospect, Page would write about the invention,

> I had no intellectual idea whether it would work or not, for I did not understand how it worked, even after it was successful. I did have a subjective conviction that it would work. This conviction, or faith as some would call it, was so strong that when it proved successful I was more elated than surprised. It was not until many years afterward, when several other people were claiming invention of the radar duplexer and everyone had a different explanation of its operation, that I was forced to give a rigorous explanation of how it did work. Then for the first time, I think I began really to understand it. Then it appeared that the original form in which I first tried it was the most simple, most direct, and, for the frequencies used, most efficient design I could have made.[8]

The duplexer was essential in making radar suitable for ships. Not only did it bring the economy of a single antenna, but it also eliminated differences in position and angular direction that had existed when using two of them. Like so many other aspects of the development of radar, the creation of this device resulted from personal ability shaped by institutional forces. Page's inventive talent allowed him to conceive the duplexer, but only Taylor's guidance, which was a manifestation of the institutional situation, made Page reject his initial conclusions about the problem and exercise his talent.

The duplexer and the 200-megahertz radar were given their initial tests at the same time. Varela, with the assistance of Page and others, had completed the new set in the astonishingly short time of about 10 weeks. It first went into operation on July 22, 1936.[9] Although echoes from airplanes were

[5] Page notebook, vol. IV (note 1), p. 19.
[6] Page interview (note 3), pp. 93 and 94.
[7] The technical principles of this invention are well described in Robert M. Page, *The Origin of Radar* (Garden City, N.Y.: Doubleday, 1962), pp. 106-125.
[8] *Ibid.*, p. 124.
[9] Page notebook, vol IV (note 1), p. 33.

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poor, reflections were good from buildings and towers. The duplexer worked excellently. Page wrote in his logbook,

> The reflection signals were stronger, as should be expected with the more concentrated radiation pattern, and the apparent directivity was increased by combining the same beam pattern between transmitter and receiver, and the operation was as if the two systems were wholly independent.[10]

From this time on, the device would be an integral part of naval radar.

In general, the first results with Varela's equipment indicated that practical radar could be built on 200 megahertz. The principal problem continued to be generating sufficient power. Already the radar group was using a pair of transmitter tubes in a "push-pull" combination to get greater pulse strength, but the increase was not sufficient. In mid-December, Page, searching for ever more power, began experimenting with an extension of this principle by wiring four tubes in a ring. The configuration worked, and by mid-1937 a four-tube oscillator was in use on 200 megahertz.[11] The problem solving was proceeding apace. At about this time, Hoyt Taylor intervened with a new demand. Radar was to be tried at sea.

A SHIPBOARD TEST

In his memoirs, Taylor explained why he asked for such an experiment at this time:

> In the latter part of 1936, when Vice Admiral A. J. Hepburn...was Commander-in-Chief of the United States Fleet, he advised Admiral Bowen, then Chief of the Bureau of Engineering, to arrange for an early demonstration and practical test of radar with the Fleet. The Laboratory was not yet ready to send a search radar to the Fleet, but I felt that we should make some tests on board a ship with gear we had even if it was only what we called soap-box equipment. We obtained an opportunity early in April of 1937 to put such equipment on the USS LEARY, a destroyer which had docked at the Washington Navy Yard, a very convenient place to make the installation.[12]

Page was somewhat displeased with this idea, for he thought it premature.[13] Although he understood well the power of successful demonstrations, he also knew the disastrous effect a poor one might have. But as Guthrie later remarked, "[Taylor] always liked to see [new equipment] go in the field and be tried out,"[14] and, after all, he was the boss.

The 200-megahertz set was reworked so it could withstand conditions on board the *Leary*, and it was placed in makeshift shelters on the deck above the galley. The Laboratory was given use of the destroyer for a month and spaced out the tests in two periods: a week at sea and, after a week in port, a second week at sea.

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[10] *Ibid.*, p. 34.
[11] *Ibid.*, p. 48.
[12] A. Hoyt Taylor, *Radio Reminiscences* (Washington: NRL, 2nd printing, 1960), pp. 175 and 176.
[13] Tape-recorded interview with Mr. Robert C. Guthrie, Apr. 13, 1978, in the Historian's office, NRL, Washington, D.C., side 2.
[14] *Ibid.*

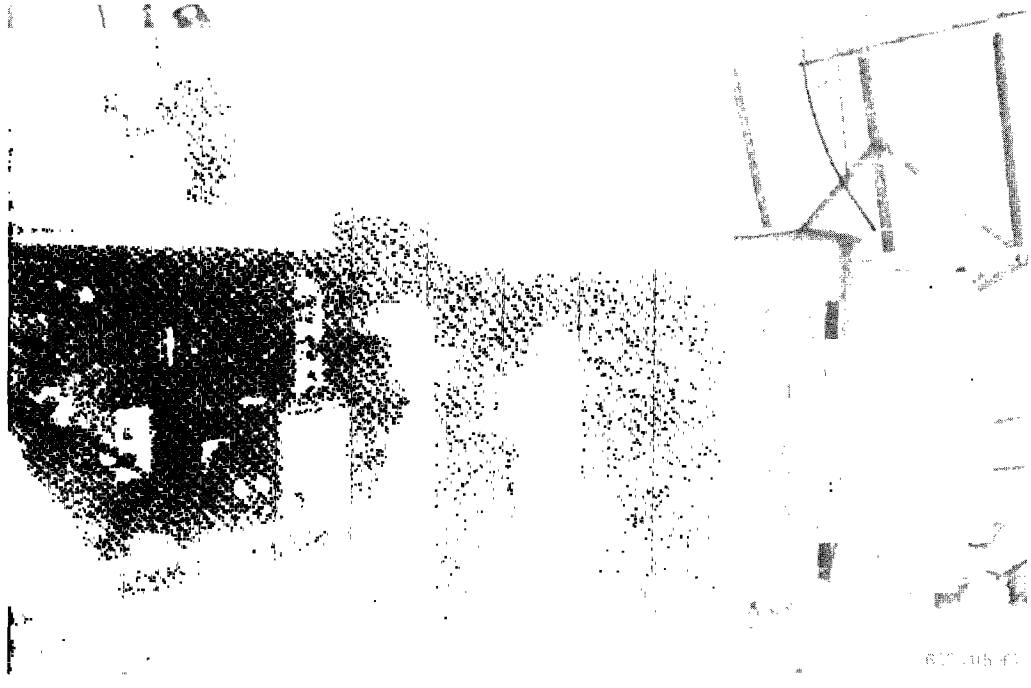

Fig. 16 — The first tests of radar at sea were made with the equipment shown here on board the USS *Leary*. To the far right, mounted on a gun, is a Yagi antenna for the radar.

Numerous experiments were made to investigate transmitter and receiver performance.[15] In addition, several Yagi and planar antennas were designed to be mounted on the guns of the ship so they could be rotated and pointed. The radar functioned fairly well, but results were disappointing. Ranges of only around 25 kilometers (15 miles) were obtained on aircraft, a dismal showing compared to the 65 kilometers (40 miles) that had been reached back at NRL. Distances with the planar antenna were somewhat better than with the Yagi, but not much. Mainly, the test reconfirmed what the men had already known: pulses with the existing transmitter were too weak.[16]

After the expedition, a detailed search was begun of all tubes available on the commercial market to find something more suited for generating strong pulses.[17] Before long, attention was fixed on a new product of the Eitel-McCullough Company, the Eimac 100TH.[18] This tube had been developed principally for amateurs, who were known to be hard on their equipment—it was said they checked the quality of a tube by seeing if it would glow hot enough to light pages of *Radio News!* The company had decided to design tubes that could stand the abuse; the Eimac 100TH was one result.

Experimentation at NRL showed that it was well suited for the radar transmitter. For a few microseconds at a time, it could take up to 10 to 15 kilovolts on the anode, although it was rated for

[15] Louis A. Gebhard, *The Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979), pp. 176-178.

[16] Page interview (note 3), pp. 97 and 98.

[17] *Ibid.*, p. 102

[18] Gebhard, *op. cit.* (note 15), p. 178; Henry Guerlac, *Radar in World War II* (unpublished history of Division 14 of the National Defense Research Committee, 1947), p. 108.

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continuous use at far lower levels.[19] Its internal dimensions were somewhat large for generating 200-megahertz oscillations, but this problem was solved by using a high positive-plate voltage, thus decreasing the transit time of electrons from cathode to anode so that output frequency was boosted. In short, it could give high power at high frequencies and yet was also rugged enough for naval use.

Following the *Leary* test, Page had also turned his thoughts back to the ring circuit design for the transmitter, to determine how it could be modified to increase power. He soon concluded that "This circuit can readily be extended to include any even number of tubes,"[20] and drew a diagram of a circuit calling for six. On January 26, 1938, he began using Eimac tubes in this configuration. Excellent results were obtained within a few days. The key to a transmitter of sufficient power had now been found.

While Page and several associates concentrated on the transmitter, others in the group modified the receiver. Basically, the earlier designs were followed, but the circuits of the Bureau of Standards detector and Page's 28-megahertz set were now restructured to incorporate lessons that had been learned during the first year and a half of radar research. Other components—the sweep circuit, the pulse modulator, and the display—also were rebuilt. And a rotating antenna was added. Page recalled,

> The antenna mount was developed by a man by the name of Shuler in the drafting room. He got an old truck axle out of the junk yard and used that for the horizontal member and where the differential came he put the vertical shaft down through the roof to the platform below. He used a piece of sewer pipe for the vertical post that it was mounted on. [He] built bearings to hold it, and then where the truck wheels would be, he had the mounts for the antenna so that you could rotate on those to elevate it. Then he ran a rod down the side of the sewer pipe with a crank at the bottom so he could crank it there and that would rotate the antenna on the ends of that axle. That was the antenna mount.[21]

By mid-February, the new 200-megahertz set and its rotating antenna were finished. After noting one final modification, Page penned in his log,

> This completes the entire 200 mc development of radio echo equipment. This development was started on the 8th day of May, 1936, and completed on the 17th day of Feb. 1938. The completed system is to be kept in operation for demonstration purposes, at least until equal or better operation is obtained at higher frequency.[22]

The natural inclination of the radar group was to continue experimentation, pushing up to higher frequencies and exploring new designs. But the Bureau of Engineering had other ideas. Commander Wilbur J. Ruble, head of the Radio Division, was already pushing for rapid introduction of equipment into the fleet. Pressure from him had been one reason the 200-megahertz set was put in finished form. Indeed, he had called for production specifications as early as December 1937.[23] The Laboratory had provided them but warned, "the present state of development of radio ranging equipment does not

[19] Page interview (note 3), p. 103.
[20] Page notebook, vol. IV (note 1), p. 62.
[21] Page interview (note 3), p. 104.
[22] Page notebook, vol. IV (note 1), p. 73.
[23] *Ibid.*, p. 65.

permit the writing of performance specifications that have much significance, save perhaps for fixed stations on land."[24] On February 3, however, Page could report to the Bureau that "The small equipment now under development should be practical for shipboard use, at least as a preliminary model for service tests."[25] Its completion later in the month led to a crucial administrative decision.

PRODUCING A PROTOTYPE

On February 24, a meeting was held at the Bureau of Engineering. Its purpose was "to discuss the status, prospective application, and further action in connection with Problem W5-2 at the Naval Research Laboratory."[26] Present were representatives of NRL and of the Bureau of Engineering, the Bureau of Ordnance, the Bureau of Aeronautics, the Bureau of Construction and Repair, and the Office of the Chief of Naval Operations—all the Departments of the Navy responsible for technical development.

It is interesting that the NRL representatives were Captain Cooley, the Director, Lt. Cdr. M. E. Curts, Officer Assistant for the Radio and Sound Divisions, and Hoyt Taylor, head of the Radio Division. Neither Page nor any other person who worked on the technical details of radar was included. This was typical. As Page recalled, at two points in an interview, "Everything that went on in [Taylor's] Division was his. He made the decisions; he made the inventions," and "he was the boss and I was the little boy in the back room. That relationship always existed fundamentally."[27]

The meeting focused on the need for rapid practical development of radar. Thus Captain McFall of the Office of the Chief of Naval Operations,

> outlined the great importance and value to the Naval Service [of radio detection equipment] and the utmost importance of expediting this development as much as possible; also the necessity for various interested Bureaus to make such provisions as might be necessary for the installation of this equipment on new construction vessels.[28]

The result was predictable: NRL was ordered to build a set "for the earliest practical date of completion for experimental installation and operation in the U.S. Fleet."[29] The Bureau of Engineering set the target as September 1 and promised to supply three additional men for the effort. The estimated cost was $25,000 including the salaries of the new employees.[30]

A new project was established at NRL to complete the task. Louis Gebhard, who had been a close associate of Taylor since World War I, was chosen to head it. It was hoped he could rely primarily on new personnel, so that present members of the radar group might continue research. This, however, was not to be. The Bureau of Engineering, as frugal as ever with its research money, reneged on its promise to bring in more engineers at this time. A letter of March 28, 1938, said,

> In view of the number of problems at the Laboratory recently completed and nearing completion and the unlikelihood of any additional preliminary model test work during the next six months, the Bureau

[24] Letter from NRL to the Bureau of Engineering, Jan. 7, 1938, in file S-S67-5 #1, box 4, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building.
[25] Letter from NRL to the Bureau of Engineering, Feb. 3, 1938, in file S-S67-5 #1 (note 24).
[26] Bureau of Engineering memorandum for files, Feb. 28, 1938, copy in NRL file S-S67-5 #1 (note 24).
[27] Page interview (note 3), pp. 48 and 71.
[28] Bureau of Engineering memorandum for files, Feb. 28, 1938 (note 26).
[29] *Ibid.*
[30] Report of consultative service by M.E. Curts pertaining to the meeting of Feb. 24, 1938, in file S-S67-5 #1 (note 24).

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> desires to accomplish the work with the existing Laboratory personnel. If desired by the Director, it will be feasible to cancel or reduce the priority of several existing problems in order to make personnel available in connection with the construction of this equipment.[31]

Whatever the intent of the Bureau, this had the effect of tying the original radar group to the design effort and slowing further research.

NRL was accustomed to transforming experimental sets into service equipment, and doing so with radar, although by no means easy, was routine. The main problem was reducing size and weight and increasing ruggedness without drastically changing performance. The antenna caused the greatest difficulty. NRL felt its planar shape could not be made smaller than 5 meters (17 feet) square or reduced in weight below 385 kilograms (850 pounds).[32] The resulting structure was so cumbersome that it could be mounted with sufficient height only on a battleship or aircraft carrier. And not every captain of even these large vessels was willing to be a guinea pig for the new gadget. After some debate, it was finally decided that it would be placed on the USS *New York*. According to Hoyt Taylor, this was largely "because Admiral A.W. Johnson, commanding the Atlantic Squadron, had seen our radar equipment and was very eager to give it an opportunity to be tested at sea."[33] The *New York* was Johnson's flagship.

On December 8, the finished set, now labeled the XAF, was shipped from the Laboratory. It had been designed and built in 8 months by seven engineers, three draftsmen, and the NRL shop force, except for the antenna, which was constructed by the Brewster Aeronautical Corporation according to NRL's plans. Total cost was just over $25,000, very near what had been initially predicted. The radar was installed quickly and was ready, as had been hoped, for the fleet exercises that were to be held in the Caribbean in January through March 1939.[34] The sailors on the *New York* were told as little as possible about the secret device, but they could not help noticing the bizarre new antenna overlooking the deck. In appearance it was a hollow rectangle with strands of dipoles crisscrossing the open area at right angles. They dubbed it the "flying bedspring."[35]

Surprisingly, the XAF was not the only radar to be tested on this historic expedition. The Bureau of Engineering had concluded that it was unwise to depend only on its in-house research laboratory and had given a contract to the Radio Corporation of America to build another model. This contract was the beginning of the Navy's relations with industry in the radar field; hence the background to it is significant.

Around 1932, RCA had begun a general investigation of radio microwaves.[36] Dr. Irving Wolff, who headed the effort, later recalled why it was initiated. "I decided it might be sensible to start some work on microwaves....There was work going on in Germany and Japan, but nothing in the United States."[37] The motivation, that is, was simply to make a broad, fundamental study of microwaves in hopes that ultimately it would pay off for the company. The structure of RCA's research division was

[31] Letter from the Bureau of Engineering to NRL, Mar. 28, 1938, in file S-S67-5 #1 (note 24).
[32] Letter from NRL to the Bureau of Engineering, Dec. 1, 1938, in file S-S67-5 #1 (note 24).
[33] Taylor, *Radio Reminiscences* (note 12), p. 191.
[34] Gebhard, *The Evolution...* (note 15), p. 189; memorandum from L.A. Gebhard to A.H. Taylor, Feb. 6, 1939, in file S-S67-5 #1 (note 24).
[35] Taylor, *Radio Reminiscences* (note 12), p. 191.
[36] Information about RCA's work was derived mainly from three sources: Irving Wolff, "The Story of Radar," *Radio Age* **5** (Oct. 1945): 10-13; *idem*, "Radio Vision—The Early Days of Radar at RCA," *RCA Engineer* **23** (Feb.-Mar. 1978): 11-13; and "RCA's Contribution to the War Effort Through Radar, 1932-1946" (unpublished manuscript obtained from RCA), section I.
[37] Tape-recorded interview with Dr. Irving Wolff, circa 1976, in the Historian's office, NRL, Washington, D.C., part I, side 2.

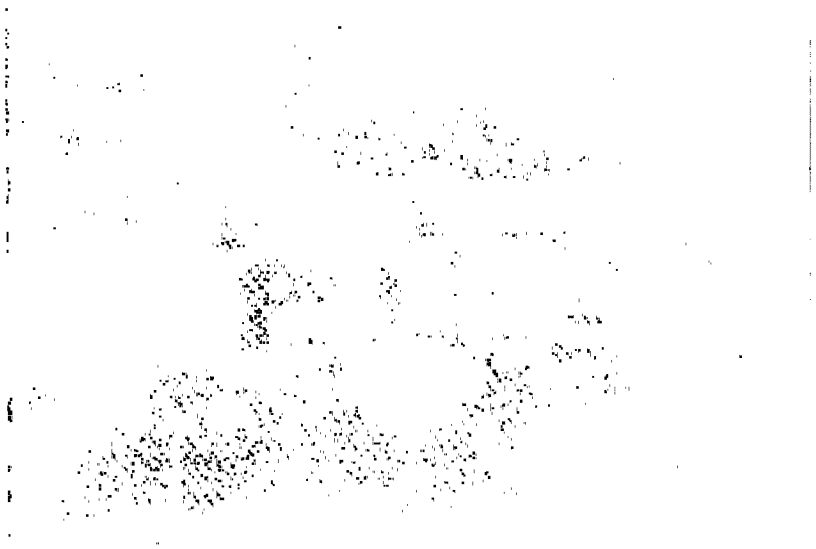

Fig. 17 — The antenna (open rectangular structure on the center of the picture) for the first production model radar, the XAF, as it appeared on the USS *New York*. Its shape earned it the epithet "flying bedspring."

such that Wolff needed only to get approval from his boss to commence his study—he and his colleagues had far greater freedom for undirected research than researchers at NRL.

Like everyone else who studied microwaves in the early days, the men at RCA had difficulty generating much power. But, by 1934, they had test equipment operating well enough for demonstrations, and they displayed it at the annual meeting of the Institute of Radio Engineers. One particular phenomenon they pointed out was the reflection of microwaves by metal objects and ionized gases. Later in the year, the Signal Corps invited them to bring their equipment to Sandy Hook, New Jersey, and test it for communications purposes. During the experimentation, they found that it might prove useful for detection of objects. Wolff remembered,

> [The Signal Corps representatives] said, "Do you think it would be possible to detect a boat? Would you be able to reflect these microwaves off a boat?" We said, "Well we might just as well try." And so there

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> was a boat coming in the harbor, and we tried it, and sure enough, we pointed it towards this boat and we could get a signal off the boat which combined with a signal coming directly from the transmitter to give us a beat. And I guess that was our first radar experiment.[38]

Soon, Wolff and his associates learned that work was being done in France on the use of microwave equipment for navigation—for detection of icebergs and other obstacles. This, along with their own experimental results, led them to begin pursuing that possibility themselves. RCA's research director, who was encouraging them to find applications for the research, also believed it was a good idea.[39]

Once committed to practical equipment for radio detection and ranging, the men, like their counterparts at NRL, soon realized that pulses were better than continuous-wave transmissions because they made distance determinations so easy and direct. Looking back, Wolff, who previously had worked in acoustics at RCA, believed that the idea of pulses came to him either through his experience with sound ranging equipment, which relied on pulses, or his general knowledge of radio ranging of the ionosphere with pulse transmissions.[40] Whatever the source of the thought, he and his colleagues pursued it and, by 1937, had an experimental microwave pulse radar operating on the roof of an RCA building in Camden, New Jersey. With it they could get echoes from tall buildings in Philadelphia, about 3 kilometers (2 miles) away.

The Bureau of Engineering kept in close contact with RCA and was well aware of its studies in the radar field. Sometime around mid-1937, it disclosed to the company the results of the work NRL had done[41] and negotiated an agreement for the construction of a 400-megahertz pulse radar set. There was some further exchange of information between NRL and RCA, but not much.[42] The equipment was designed and built, not by Wolff and his staff, but by the RCA Production Department. As it had done with the XAF, the Bureau of Engineering pushed to have this set ready by early 1939, so it could be tested in the fleet exercises.[43] The radar, called the CXZ, was finished and was installed on board the battleship USS *Texas* in January 1939.

The engineers at NRL had not protested the RCA contract, but they were unhappy about it. They too had worked on higher frequency radar, and for the Bureau to allocate part of its limited budget for similar research by a private company seemed to them a disappointing lack of confidence. The situation was particularly irksome because RCA was given $60,000 for the CXZ as compared to the $25,000 NRL got for the XAF. The fleet exercises, therefore, assumed an air of competition. Here, it appeared, was the verbal debate of 1932 over the value of in-house versus industrial research made manifest in hardware!

RADAR GOES TO SEA

A journey to the Caribbean in January was welcome relief from winter in the Northeast, but the two teams of radar engineers had little chance to enjoy the weather. Tests of their equipment began

[38] *Ibid.* See also "Range Tests with 75-cm Radio-Optical Equipment," Signal Corps Laboratories, Fort Monmouth, N.J., Engineering Report 288, in file C-S67/35, box 22, job order 11101, records of NRL, record group 181, Washington National Records Center, Suitland, Md.
[39] Wolff interview (note 37), part II, side 2.
[40] *Ibid.* Wolff's remarks on this point, however, were not recorded.
[41] Hoyt Taylor, *Radio Reminiscences* (note 12), p. 192. NRL had explicitly agreed to outside contracts by the Bureau. See the memorandum from NRL to the Bureau of Engineering, June 30, 1937, in file S-S67-5 #1 (note 24).
[42] Memorandum from E.H. Pierce to the Director, NRL, Oct. 6, in file S-S67-5 #1 (note 24).
[43] Letter from the Bureau of Engineering to the Chief of Naval Operations, Dec. 19, 1938, in file S-S67-5 #1 (note 24).

immediately after it was installed and they continued throughout the cruise.[44] First came the mundane but crucial task of determining whether there would be any interference between radar and existing electronic equipment on board. Fortunately there was none. Next followed a series of experiments to measure the ability of the new devices to detect fixed objects, such as land targets and buoys, and moving objects, particularly ships, shells, and aircraft. How well could radar warn of the presence of enemy vessels? How effectively could it signal impending attack, especially at night or in fog? Could it track shells and thus be of use for fire control? How would it perform during the confusion of battle? All these questions had to be answered.

Throughout the tests, the XAF worked exceedingly well. It operated continuously for 16 to 24 hours a day, in high winds, rain storms, and gunfire as well as in fair weather. Only two tubes and three other components had to be replaced during the entire expedition. Ships were ranged at distances over 16 kilometers (10 miles), aircraft were ranged at up to 77 kilometers (48 miles). Buoys were detected in excess of 6 kilometers (4 miles). The set could follow 14-inch shells in flight and could see splashes of both 5-inch and 14-inch shells at distances up to 13 kilometers (8 miles). Even large birds would cause noticeable pips on the screen![45]

The highlights of the tests came in mock attacks, the first of which occurred on January 16. Page recalled it this way:

> They told me, "tonight we're going to have a simulated destroyer test." They said, "we're going to send a destroyer out over the horizon and then he's going to come in with lights out and approach us to make a simulated torpedo attack. We want to see if you can pick him up." We weren't told when they were going to come, but the skipper knew when they were going to come, [and the] Admiral knew when they were going to come. The Admiral of the Fleet [A.W. Johnson] was on our ship. So we went up to turn on the equipment and watch it while he went to the movies.
>
> When the movies were over, he came up. He started watching the scope—he watched it and watched it—and he knew it was time for the attack, and he didn't see anything. Finally, he gave up and said [that] he had gotten tired of watching the scope. He turned to leave. [When] he got to the doorway, he stopped for some reason and turned around and came back to look once more before he went down. Within a couple of rotations of the antenna, as we swept past, he saw the destroyer. He saw it, and like a kid he jumped, "There she is!" And ... I think it was 9000 yards where we picked up the destroyer. "All right," he said—he just stayed right there, watched [the screen], and we tracked [the destroyer] in. We gave him the bearing. He said, "All right, turn on the searchlights on that bearing."
>
> So we turned on the searchlights. We didn't see a thing. It was slightly hazy and we got the reflection off the haze and the lights and couldn't see very far. [But] the next day, the officers from the destroyer [and] the destroyer skipper came aboard. [The skipper] was just

[44] Robert Page kept a notebook during the voyage on tests with the XAF: Robert M. Page, notebook 152, in records of NRL, Records and Correspondence Management office, NRL, Washington, D.C. Another source is a letter from R.M. Page to the Commander, Atlantic Squadron (Engineer's Report on Service Tests of Model XAF Radio Ranging Equipment), Apr. 8, 1939, in file S-S67-5 #1 (note 24). See also the Page interview (note 3), p. 108.

[45] Page interview (note 3), p. 114.

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> a little bit shaken. He said, "what'd you have on that ship last night? When you turned on your searchlights, you illuminated my lead destroyer!"[46]

In a second drill run on February 21, the destroyer captains were apprised of the radar in advance and were told to try to avoid detection. Yet again they were picked up outside of effective torpedo range. Page remembered the general reaction after the tests:

> These performances were at night, with no possibility of seeing the destroyers. Their lights were out. That really impressed the officers. From then on they were sold on the stuff and they would give us anything we wanted.[47]

Later in the month, a simulated battle was fought. During it, the XAF proved it could easily spot both "friendly" and "enemy" aircraft. This information was of limited value, however, since there was as yet no way of telling the difference between them.

Compared to this outstanding performance, results from the CXZ were disappointing. The set had been rushed into development and showed it. Exposed parts deteriorated rapidly in moisture, and the equipment would not withstand the shock of heavy gunfire. Ranges on objects were far less than those with the XAF—only 8 to 12 kilometers (5 to 7.5 miles) on large ships, and merely 5 to 8 kilometers (3 to 5 miles) on aircraft. Warning was successfully given in several mock attacks but again at ranges shorter than those afforded by the competing device.[48] The Commanding Officer of the *Texas* concluded, "The apparatus, as actually installed on board TEXAS would be of very little value in war. It might be described as in a 'highly experimental state.'" Then he added hopefully,

> The Commanding Officer discussed it on a number of occasions with the senior R.C.A. Engineer on board, and agrees with that gentleman that the R.C.A. personnel learned enough this winter to insure that the next model of this apparatus will be much more practical and valuable. The apparatus displayed potentialities which, when developed, would be invaluable in war.[49]

Page and the other NRL personnel on the expedition could not help but be pleased with the turn of events, but they empathized with the failures experienced by RCA. As Page wrote to Leo Young,

> The performance of [the RCA] equipment has been very disappointing...and the officers of [the TEXAS] make no secret of the fact that they do not think much of it....While [this] may enhance our own success, it is not the kind of thing I like to see happen, even to our rivals.[50]

Page knew that practical radar was possible at 400 megahertz and even higher frequencies and hoped that the poor showing by the RCA set would not dampen enthusiasm or support for efforts to move up the spectrum.

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[46] *Ibid.*, pp. 116 and 117.
[47] *Ibid.*, p. 118.
[48] Letter from the Commanding Officer, USS *Texas*, to the Commander, Atlantic Squadron, Mar. 19, 1939, in file S-S67-5 #1 (note 24).
[49] Letter from the Commanding Officer, USS *Texas*, to the Commander, Atlantic Squadron, Mar. 24, 1939, in file S-S67-5 #1 (note 24).
[50] Letter from R.M. Page to Leo Young, Mar. 3, 1938, in file S-S67-5 #1 (note 24).

The official judgment on the XAF was exactly the opposite of what it had been for the CXZ. The Captain of the *New York* recommended

> That [radar] be installed at once on all [aircraft carriers] and as soon as possible on other vessels. I would make no reduction in size at the expense of range for the present, particularly for [carriers]. The device looks big, but really caused very little inconvenience. After all we can't expect to get something for nothing. It is well worth the space it occupies.[51]

Admiral Johnson echoed this view in his report to the Bureau of Engineering:

> Commander, Atlantic Squadron considers that the equipment is one of the most important military developments since the advent of radio itself. Its value as a defensive instrument of war and as an instrument for avoidance of collisions at sea justifies the Navy's unlimited development of the equipment....
>
> Commander, Atlantic Squadron considers that the present state of development of the equipment is such as to NOW warrant making it a permanent installation in cruisers and carriers.

In conclusion, he noted that he was "especially impressed with the efficiency and capabilities of Mr. Page. His services are, it seems, most valuable to the government, and it is recommended that the Bureau assure his retention in the government service."[52] The Bureau agreed with the Admiral's appraisal and followed his advice.

EQUIPMENT FOR THE FLEET

The status of radar now zoomed upward. Soon it was redesignated special project 1 of the Office of the Chief of Naval Operations, and on May 8, 1939, representatives of NRL, all the material bureaus, and the CNO met to determine the next step. As might have been expected, the result was a call for immediate action:

> On a motion...concurred with by all...representatives, it was agreed to recommend that procurement of from 10 to 20 of the [radar] devices in their present form, with only minor and readily accomplished changes, be undertaken at once, for installation and Service trial on vessels of the Fleet; this procurement not to interfere with concurrent development. Immediate procurement was considered imperative because (a) the device is of great military value in its present form; (b) experience in Service will permit exploration of its capabilities and limitations, will provide training in its use and will point the way to further development; (c) the international situation requires that immediate advantage be taken of every device leading to greater military effectiveness; (d) there is no positive guarantee that development of the improved device will be successful.[53]

[51]Letter from the Commanding Officer, USS *New York*, to the Commander, Atlantic Squadron, Mar. 24, 1939, in file S-S67-5 #1 (note 24).

[52]Letter from the Commander, Atlantic Squadron, to the Bureau of Engineering, Apr. 4, 1939, in file S-S67-5 #1 (note 24).

[53]Memorandum for file of the Office of Chief of Naval Operations on the Conference on Special Project 1, May 8, 1939, in file S-S67-5 #1 (note 24).

UNCLASSIFIED

This recommendation was accepted. Next came the decision of who would build the equipment. Realistically, there were only two possibilities: Western Electric Company or RCA. Both had been told about NRL's highly secret developments, and both were already working in the radar field. Representatives of RCA came to the Laboratory on May 19 to learn more about the XAF, which the Navy wanted to be copied exactly; Western Electric engineers visited on May 26.[54] Later, to help expedite the bidding process, NRL drew up complete intructions for production, although even this, as Louis Gebhard moaned in one report, would not help much. He wrote,

> Manufacturers...indicated they would require 100-180 days for first model and 120 days after release to complete contract. *It will take a year to get this equipment*! AND MFGR HAS EVERYTHING WORKED OUT FOR HIM! [Emphasis is in the original.][55]

On October 17, 1939, it was announced that RCA had won the bidding.[56] Meanwhile, the Bureau of Engineering had decided to limit initial production, so only six sets were ordered. Construction went smoothly, although RCA was not thrilled about having to copy NRL's design so closely. For one thing, this meant having to use tubes produced by Eitel-McCullough, a competitor.[57] Furthermore, at least as Page recalled,

> They rebelled at having to copy the Laboratory equipment. This was a government Laboratory; they didn't know how to do anything. [RCA thought], "if we are forced to put out a piece of equipment that they developed and we have to copy it and can't design it right, it's going to react against us in public relations. It's going to cost us something in our reputation."... [Moreover] I insisted that the nameplate include the words, "Developed by the Naval Research Laboratory." And I think I had more trouble over that than any other thing about it. They absolutely refused to do it! And the Bureau would not make them do it. I made such a fuss that finally, the Bureau made them do it. Then, instead of putting it on their nameplate, they made another nameplate, a little tiny one they put down on the bottom corner—"Developed by the Naval Research Laboratory."[58]

Nonetheless, the work got done. The first preliminary model was delivered in November 1939; the first finished, or production, model in May 1940. The other sets followed soon afterward.

The equipment went aboard the heavy cruisers *Chicago, Chester, Pensacola,* and *Northhampton*, the carrier *Yorktown,* and the battleship *California.*[59] Once tests had shown the value of these sets and some slight modifications had been made, 14 more, now called the CXAM-1, were produced. These were placed on the heavy cruiser *Augusta*, the light cruisers *Albermarle* and *Cincinnati*, the carriers *Lexington, Saratoga, Ranger, Enterprise,* and *Wasp*, the battleships *Texas, Pennsylvania, West Virginia, North Carolina,* and *Washington,* and the seaplane tender *Curtis.* Almost all were installed by the beginning of American involvement in World War II.[60] During the conflict, CXAM sets would make a good showing

[54] Records of consultative services for May 19, 1939, and May 26, 1939, both in file S-S67-5 #1 (note 24).
[55] Record of consultative services for June 21, in file S-S67-5 #1 (note 24).
[56] Record of consultative services for Oct. 17, 1939, in file S-S67-5 #2, box 4, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building.
[57] Page interview (note 3), p. 113.
[58] *Ibid.*, pp. 112 and 113.
[59] Taylor, *Radio Reminiscences* (note 12), p. 197.
[60] Gebhard, *The Evolution...* (note 15), p. 183; Cdr. Charles W. Harrison, Jr., USN, and James E. Blower, "Electronics—Your Future," *Journal of the American Society of Naval Engineers* **62** (1950): 116.

for themselves and contribute substantially to naval operations. Fleet Admiral King, for example, noted in his final report on the war, "Radar of this type contributed to the victories of the Coral Sea, Midway, and Guadalcanal."[61]

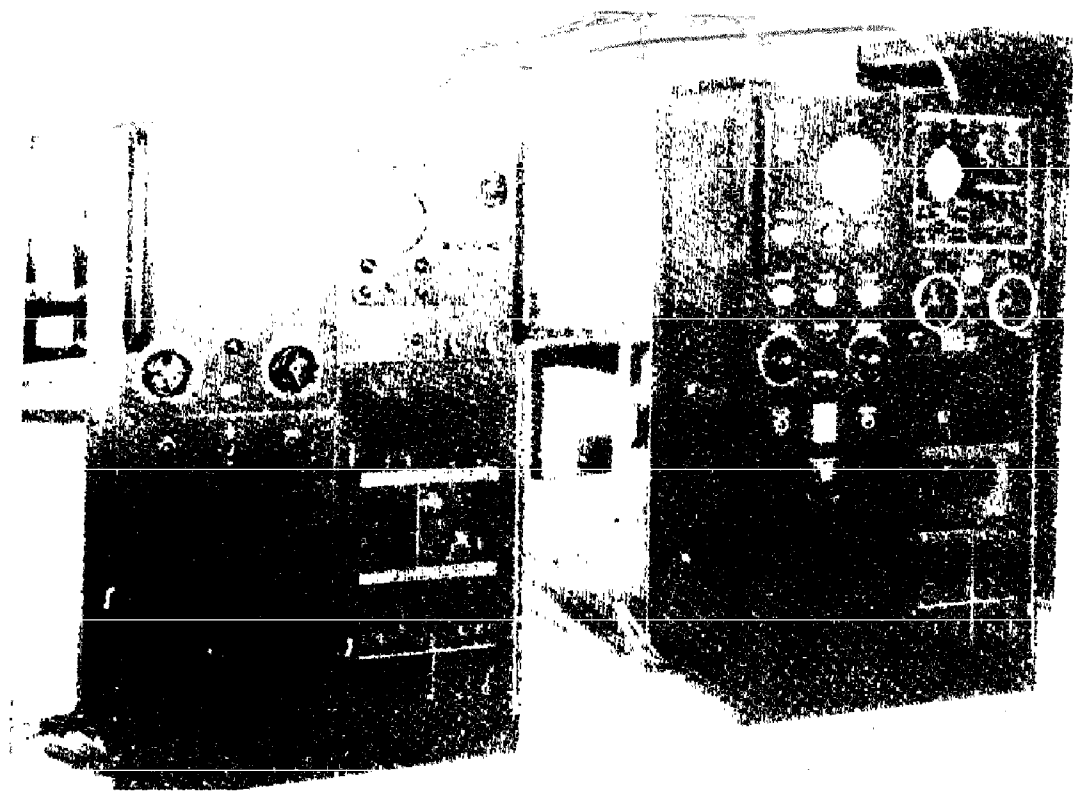

Fig. 18 — To the left is NRL's XAF radar and to the right, the production model based on it, the CXAM, twenty of which were built for the Navy by RCA.

A handful of engineers at NRL had worked for 17 years—at first sporadically, then hurriedly—on the research and development that underlay these first naval radar sets. They were a great accomplishment. But they were only a slight indication of the power of radar. The men had not just designed a single device, they had helped open a whole new field of electronics that would yield a huge assortment of equipment that could perform a host of functions, a field whose limits of productivity are, even today, undetermined. Looking back it seems that perhaps RCA was right in talking not of "radar," but of "radio-vision."[62]

[61] Adm. E.J. King, USN, "United States Navy at War: Final Official Report to the Secretary of the Navy," *US Naval Institute Proceedings* 72 (1946): 171.

[62] The RCA name, later dropped in favor of the widely accepted Navy terminology, is discussed in Irving Wolff, "The Story of Radar" (note 36).

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8. A DEVICE BECOMES A FIELD

Although no one in the 1930s could have foreseen the vast potential of radar, researchers clearly understood that the basic ideas inherent in it might give rise to a wide variety of different devices. Thus, it is hardly surprising that even before the work described in the previous three chapters was complete, other, related development programs were underway both at NRL and elsewhere. Neglected in these previous chapters in the interest of concentration, some of these must now be considered as we examine the transformation of radar into a broad field of technology.

EARLY MICROWAVE RESEARCH AT NRL

It is now well known that the microwave region of the electromagnetic spectrum, a variously defined region in which wavelengths are measured in centimeters and that here will mean frequencies between 300 megahertz and 30,000 megahertz, is the most useful for radar (Table 3, in Chapter 4). Basically this is because microwaves can be focused more easily than waves at frequencies below 300 megahertz, require smaller antennas, and give better target discrimination.[1] Although similar to light in physical characteristics, they have the power to penetrate fog and other atmospheric conditions that would reflect or absorb visible rays. All these advantages were generally understood even in the early days of radar development. In fact, both RCA and the Army began their investigations of radio detection by studying microwaves.[2]

The Naval Research Laboratory had made its technical reputation by pushing the edge of radio research higher and higher up the electromagnetic spectrum, and it too became interested in using microwaves for detection at a relatively early date. Indeed, for several years in the mid-1930s, the Laboratory put as much emphasis on microwave research as it did on short-wave radar research at lower frequencies. Only when the latter seemed clearly to be the quickest path to practical equipment was work concentrated on these lower frequencies. The story of NRL's initial research on microwaves, although it did not lead to practical equipment, is worth recounting as counterpoint to the main themes discussed in previous chapters and as a means of illuminating the broad institutional character of NRL's development of radar.

Although many civilian scientists at NRL foresaw the general importance of microwaves, the plan to study them for the purpose of detecting and ranging objects originated with a naval officer, William S. "Deke" Parsons, who was stationed at the Laboratory as a liaison officer for the Bureau of Ordnance. A 1922 graduate of the Naval Academy, Parsons had 8 years of experience in the fleet and 3 years of postgraduate instruction in ordnance before coming to NRL. Already, he was proving himself to be one of the most capable technical officers of his generation. This ability would become even clearer in World War II, when he would become involved in the Manhattan Project and win the respect of America's top physicists. Indeed, he would be the man chosen to make the final preparations on the first atomic bomb ever used in combat.[3]

Parsons reported to the Laboratory in July 1933. Soon he was apprised of the radio detection project, and he immediately envisioned application in the area of gunfire control, a major responsibility

[1] Louis N. Ridenour, *Radar System Engineering* (New York: McGraw-Hill, 1947), pp. 10 and 11.
[2] Irving Wolff, "The Story of Radar," *Radio Age* 5 (Oct. 1945): 10-13; Dulaney Terrett, *The Signal Corps: The Emergency* (Washington: GPO, 1956), pp. 40ff.
[3] Biographical sketch of William S. Parsons, records of the Officer Biographies Branch, Office of Naval Information, in the Operational Archives Branch, Naval History Divison, Washington, D.C.

of the Bureau of Ordnance. He was astonished that no one else had seriously considered the idea. Years later he recalled the situation in this way:

> At that time there was good liaison with the Navy Department (Radio Division, BuEng [Bureau of Engineering]) in only two fields; testing and development of radio equipment, and development of underwater sound equipment. In other fields, including BuOrd [the Bureau of Ordnance], BuAer [the Bureau of Aeronautics], BuC&R [the Bureau of Construction and Repair], and OpNav [the Office of the Chief of Naval Operations], the liaison was very sketchy it seemed because neither side would take the initiative.
>
> To show how inadequate was the knowledge of fire control essentials at NRL at that time, one of the senior physicists was surprised at my enthusiasm over the radio echo possibilities and remarked that he understood that the Navy had such fine optical range finders that radio echo [devices] could improve matters very little. I was tremendously surprised to find that BuOrd had not been informed of these possibilities, although the War Department had been informed eighteen months earlier.[4]

Was this not the result of putting NRL under the control of the Bureau of Engineering instead of guarding and emphasizing its position as a general research institution for the Navy? Was it not confirmation of Captain Oberlin's warnings?[5]

Resolved to initiate a research project, Parsons conveyed to his superiors at the Bureau of Ordnance his vision of the importance of radio ranging in a letter sent by the Director of NRL to the Bureau on August 2, 1933. It stated,

> It is desired to inform the Bureau of Ordnance regarding a development in radio research which has possible applications in fire control work and airplane detection.
>
> In tests of super-high frequency radio transmission to airplanes, certain beat notes were heard in receivers located on the ground at a considerable distance from the transmitting station. These beat notes were found to be caused by a combination of the transmitted and *reflected* waves which were out of phase by an amount proportional to the rate of change of distance between the reflecting object and the transmitting and receiving stations. Beats were obtained at ranges above 20 miles and at altitudes above 8000 feet....
>
> When the distance between transmitter and receiver is negligibly small compared to the distance between them and the reflecting object, one beat occurs whenever the distance (range) changes by one half wave length. Wave lengths used to date have been about four meters. Using micro rays (wave length less than 1 meter) reflection should be improved.

[4]Letter from W.S. Parsons to E.B. Taylor, Nov. 6, 1945, in file P16-1/00, box 3, account 38-76-81, records of the Chief of Naval Operations, record group 38, Washington National Records Center, Suitland, Md. I am endebted to Mr. Derk Bruins for bringing this letter to my attention.

[5]Quotations cited in Chapter 5 by notes 22, 30, 31, and 40.

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> Possible applications of this development to fire control and aircraft detection work appear to be:
>
> (a) Continuous measurement and indication of the exact rate of change of range to a surface target.
>
> (b) Aircraft detection, possibly followed by measurement of the rate of change to the target when located.
>
> To date, work along these lines, due to the large number of urgent Engineering problems, has been going on slowly toward the development of a device for airplane detection by means of beats. In view of the fact that the airplane detection and fire control features apply to the Army as well as the Navy, it is possible that the Army has done work along these lines or would cooperate in further research and development. Should the Bureau of Ordnance feel that this work merits greater effort, it would be necessary under the present set-up to support the work financially. An outlay of $5,000 a year would permit hiring additional personnel whose services would be devoted exclusively to this project.[6]

Comments on Parsons' suggestion by Bureau of Ordnance officials have survived.[7] One said, "Recommend that this be followed up," but the other readers of the proposal were not impressed. One officer wrote,

> Can this be developed for measurement of range? If not, then it would seem that immediate efforts should be concentrated on development for airplane detection, as the detail of most immediate military value onboard ship, where "listeners" are not used.

Another answered,

> Use of "beat" frequency will *not* measure anything but *ratio* and to do this for surface or aerial targets it must be known definitely that the *reflected* signal from target is the ship or plane on which range rate (along line of sight) is desired to be established. Until beam transmissions can be reduced to much smaller values this method has no possible use.

In technical detail these analyses are correct, but they show no appreciation of the promise of continued research. Indeed, it is the attitude they reveal that is most important. Like the Bureau of Engineering, the Bureau of Ordnance was closely tied to practical development in these years and was hesitant to support anything that would not produce quick results.

Undaunted by the initial reactions, Parsons followed up his first request with a second on September 15. He now acknowledged the problems of ranging objects when using the continuous-wave method, but he indicated that the problems could be solved by modulating the transmitted waves in

[6]Letter from NRL to the Bureau of Ordnance, Aug. 2, 1933, in file S-67, box 208, entry 25, records of the Bureau of Ordnance, record group 74, Washington National Records Center, Suitland, Md. Parsons' initials on the upper margin of the letter prove his authorship.

[7]These are penciled either on the back of the letter (note 6) or on attached papers.

such a manner that the particular interference patterns in the receiver would indicate the distance to the target. Again he stressed the ultimate payoff,

> New developments in radio at this Laboratory have suggested the idea of building a single "beam" transmitter and receiver with additional gear which, if completely successful, would be able to do the following:
>
> (a) Take range on any object from which a continuous echo can be received.
>
> (b) Take bearings on a ship or airplane by means of the radio echo.
>
> (c) Give an indication of the rate of change of range to any object from which a continuous echo can be received.
>
> (d) By means of (a), (b), and (c), to detect and track unseen ship or airplane.[8]

Once again of course, Parsons requested Bureau of Ordnance support. This time, the letter went to the Bureau of Ordnance via the Bureau of Engineering. Officials there, when forwarding it, added pessimistic comments about Parson's recommendations:

> In view of the results from a large amount of somewhat similar work done in the past or now underway by highly competent companies, the time involved to obtain either negative or positive results might well exceed two or three years. In the case of negative results it would be difficult to predict that positive results were impractical of accomplishment.
>
> Progress in micro ray work is so closely associated with vacuum tube development that the major progress in this field is now being made by organizations conducting vacuum tube research....The Bureau of Engineering considers that progress along the lines suggested by the Laboratory can best be accomplished by a development contract with a suitable commercial company.[9]

Once again we see the tendency to leave long-range research to industry, rather than to develop NRL's capability to perform it.

Based on this recommendation and on a conference held with representatives of NRL after receiving the letter, the Bureau of Ordnance told the Laboratory,

> After due consideration...the Bureau of Ordnance is of the opinion that, in view of the present stringency of funds, it is unable to recommend any participation by this Bureau at the present time in the development of this project by the Naval Research Laboratory.[10]

[8]Letter from NRL to the Bureau of Engineering, September 15, 1933, in file C-S67/35, box 22, job order 11101, records of NRL, record group 181, Washington Natonal Records Center, Suitland, Md.
[9]First endorsement, Bureau of Engineering to the Bureau of Ordnance, to the letter cited in note 8, Oct. 20, 1933, in the same file.
[10]Letter from the Bureau of Ordnance to NRL, Oct. 28, 1933, in file C-S67/35 (note 8).

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Thus ended the possibility of an early entry by the Bureau of Ordnance into fire-control radar or of a close partnership with the Bureau of Engineering in bringing the first practical naval radar into existence. The Bureau of Ordnance would not play a major role in radar development and production until around 1940. When it did enter the field, it had to do so with great haste and confusion. The story might have been quite different had Parson's suggestions been followed.[11]

Despite lack of support from the Bureau of Ordnance, study of microwaves at NRL was not dropped entirely. In a roundabout fashion, money was obtained from the Bureau of Aeronautics, from a problem it was sponsoring for development of an alti-drift meter, a device for accurately determining altitude and drift of an airplane.[12] Between 1932 and 1934, engineers working on the project had used sound waves. Their plan was to send out a continuous, concentrated beam from the airplane toward the ground below and in front of it. Reflections of these waves would cause interference in a receiver in the airplane as they mixed with others that came directly from the transmitter. The characteristics of the pattern could be used to determine distance to the ground and the velocity of the aircraft. Unfortunately, atmospheric disturbance and air turbulence combined to prevent clear and continuous reflection of the sound.

In March 1934, with the approval of the Bureau of Aeronautics, a switch was made to radio microwaves. The same principles of wave reflection and interference were to be employed. Since these principles were exactly those of continuous-wave radio detection, it was anticipated that the research might conceivably lead to practical microwave radar as well as to an alti-drift meter. In later years, Parsons looked back and commented,

> ...BuAer support for microwave investigation was sought and obtained. This was put over in spite of BuEng objection that the instrument would certainly be too heavy and clumsy to carry in an airplane....
>
> Note 1: The foresight and energy which won this argument were characteristics of BuAer under (then) Rear Admiral E. J. King, USN.[13]

Any project based on microwaves in these years, when very little was known about them, soon became of necessity a basic research effort. Consequently, the first report after the switch to microwaves, a report that covers the period from April to December 1934, discussed experiments on the basic means for transmission and reflection of the radiation and experiments for discovering its fundamental characteristics.[14] The Bureau of Aeronautics, progressive though it may have been, shared with the other Bureaus the belief that its funds should be directed primarily to applied rather than basic research. Responding to the report, the Bureau stated,

> ...to date there has been allocated a total of $21,000 during a period of four years for the development of an alti-drift meter. Experimental funds available for the development of aircraft instruments do not justify continuing this rate of expenditure for the development of an alti-drift meter unless there are good prospects for the early development of a micro-ray alti-drift meter model suitable for flight tests in an airship or in an airplane, preferably in an airplane....It appears that the construction of such a model depends to a large extent upon the availability of vacuum tubes of considerably better characteristics than at present obtainable and that consequently it may be advisable to restrict

[11]Buford Rowland and William B. Boyd, *U.S. Navy Bureau of Ordnance in World War II* (Washington: GPO, 1947), pp. 414-417.
[12]NRL Report R-1111, Jan. 15, 1935, "Development of Micro-Ray Radio Apparatus for Use in Aircraft Alti-Drift Meter Equipment," in file C-S67/35 (note 8).
[13]Letter from W.S. Parsons to E.B. Taylor (note 4).
[14]See note 12.

> the funds expended on the alti-drift meter until such time as there is greater likelihood of obtaining more satisfactory tubes.[15]

Clearly, unless money could be obtained from some other sources, there was great danger that the project would be eliminated.[16]

Ross Gunn, the Technical Assistant to the Director of NRL, the man generally responsible for acting as liaison between the Director and the research staff, became quite distressed about the situation. A PhD in physics from Yale, Gunn had come to the Laboratory in 1927 and had proved himself an extremely able scientist. Within a year after his arrival, he had been named Assistant Superintendent of the Heat and Light Division, and in 1934, he became Technical Advisor to the Director. Later in his career, he would achieve distinction by recognizing, as early as 1939—the same year in which nuclear fission was discovered—the potentials of nuclear power for the Navy. He would quickly lead NRL into this field, thus making it the first government agency to conduct research in atomic energy.[17] In 1935 he, like Parsons, saw clearly the future importance of microwaves and endeavored to change the opinion of the material bureaus. On March 7, several weeks after the Bureau of Aeronautics had indicated its waning support, Gunn set forth his ideas in a lengthy memorandum to the NRL Director, a memorandum that obviously was aimed at a higher target:

> I conceive that the main purpose of this Laboratory is, by the aid of science, to so change the methods of warfare that the enemy will always be at a tremendous disadvantage....
>
> Our fleet must be provided with better and more useful eyes, ears, and voices if it is to survive a major engagement and if its power is to be used to the greatest advantage.
>
> One field of scientific investigation that offers great possibilities for increasing the efficiency and effectiveness of the Navy is not now receiving the attention it richly deserves. This field of investigation relates to electromagnetic waves situated in the spectrum between light waves and radio waves. These waves have remarkable and extremely valuable properties. They may be produced with moderate ease and completely controlled by electric means. The waves have all the desirable properties of light; they may be reflected or refracted into a beam like a search light or they may be broadcast in all directions. The waves have the remarkable property of penetrating fog and hence any operation that can be performed by ordinary light under normal conditions may be reproduced in fog by means of these waves....
>
> Perhaps the most important applications of these waves to the Naval Service are not so obvious or easy of accomplishment. Their reduction to useful form will entail much study, effort and consideration and could only be accomplished by continued research over a period of years, yet so valuable will be successful results that the expenditure of almost any amount of money could be justified....
>
> Research problems are seldom encountered which offer so much promise of ultimate Naval usefulness in so many different directions. Many of the applications are extremely important and useful in war....It is

[15]Letter from the Bureau of Aeronautics to NRL, Feb. 26, 1935, file C-S67/35 (note 8).
[16]Records indicate that despite this warning the Bureau continued to support the problem.
[17]Biographical file on Ross Gunn in the NRL historical file, Historian's office, NRL, Washington, D.C.

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recommended, therefore, that a general continuing problem be authorized to investigate the quasi-optical region of electromagnetic waves.[18]

To bolster Gunn's claims, the Laboratory produced a formal report on "Present Day Technique for Radio Transmission and Reception in the Micro-Ray Frequency Band (300-3000 Megacycles) With Suggested Applications for Naval Purposes."[19] While lacking the rhetorical punch of the letter, the report also stressed the utility of microwaves: for secret point-to-point communications, for detection and ranging of enemy ships and aircraft, and for secret aircraft-to-ground communication. The document was sent to both the Bureaus of Engineering and Aeronautics to stimulate increased support.

Response from the Bureaus was not encouraging. This was a major reason Taylor made his special appeal to the House Naval Appropriations Subcommittee in the spring of 1935 for additional research funds.[20] Indeed, when he was successful in obtaining an extra $100,000 for NRL in fiscal 1936, the highest priority for using the additional money was given to the investigation of microwaves. A new, basic-research problem on the subject was established in July 1935[21]: the Laboratory then had the authorization needed to begin a broad, fundamental investigation of microwaves. However, like all basic-research problems, this one had to remain limited in extent. Only three to five men worked full time on the subject for the remainder of the 1930s.

The principal goal of the investigation was fundamental understanding of transmission and reception of microwave radiation. Tube testing, circuit building, and experiments to determine fundamental properties constituted the bulk of the project. Nonetheless, hopes remained high that practical equipment—including radio detection devices—could soon be developed, and much effort was directed toward this end. The continuous-wave method was always used, because it was believed that sufficient power could not be generated for the pulse method to be effective.

In the material bureaus, interest in the problem grew after public disclosure, in late summer 1935, of the existence of microwave radio detection equipment on board the French liner *Normandie*.[22] It seemed that practical equipment might not be so far in the future as previously thought. Still, there was hesitancy to provide increased support. Answering a request for more money in January 1936, the Bureau of Engineering stated, "Such progress as has been indicated in the [Laboratory's] reports either as having been made by the Laboratory or as coming to the attention of the Laboratory, has been quite limited."[23] A month later, it pointed out that the General Electric Company and Bell Telephone Laboratories were both doing basic work on microwaves. Might it not be more profitable, was the implication, for the Navy simply to wait and reap the benefits of their work, to let them take the risks?[24]

Strongly supported by NRL leaders, the microwave investigation continued to trudge along. Whenever new experimental equipment was developed, its potential for radio detection was tested. In May 1936—soon after the pulsed, 28-megahertz radar developed by Page and Guthrie had achieved its dramatic results[25]—a microwave set using the continuous waves was tried on the Potomac River. A report stated,

[18]Memorandum from Ross Gunn to the Director, NRL, Mar. 7, 1935, in file S-S67/35, box 19, job order 11029, records of NRL, record group 181, Washington National Records Center, Suitland, Md.
[19]NRL Report 1149, Apr. 23, 1935, in the Documents Section, NRL library, Washington, D.C.
[20]Taylor's description of this appeal was cited in Chapter 6 by note 23.
[21]Problem 0-2S. See the letter from the Bureau of Engineering to NRL, July 12, 1935, in file L1-1(3), box 34, records of NRL, Unclassified series, record group 19, National Archives Building, and the letter from NRL to the Bureau of Engineering, Oct. 3, 1935, in file C-S67/35 (note 8).
[22]Letter from the Bureau of Ordnance to NRL, Nov. 13, 1935, in file C-S67/35 (note 8). The *Normandie* equipment was discussed in Chapter 6, where notes 32 and 33 apply.
[23]Letter from the Bureau of Engineering to NRL, Jan. 4, 1936, in file C-S67/35 (note 8).
[24]Letter from the Bureau of Engineering to NRL, Feb. 11, 1936, in file C-S67/35 (note 8).
[25]Described in Chapter 6, where notes 41 and 42 apply.

> The transmitter was set up on the Naval Research Laboratory roof at Bellevue, D.C., while the receiver was on a motor launch plying the river....Beats were obtained from passing barges, and when direct vision was obscured the signal was occulted. Reflections were reliably obtained from a heterogeneous shore line at 1-1/4 mile distance and from a metallic sphere 40 feet in diameter and a brick chimney at 1/2 mile. This again was not the limit of distance. More information will be available soon.[26]

These results were significant, but they tended to be overshadowed by those being obtained with the longer wave equipment, which was already detecting aircraft at ranges greater than 40 kilometers (25 miles).

The peak of the early efforts to develop microwave radar at NRL came in tests aboard the USS *Leary* in April 1937. As reported earlier,[27] 200-megahertz pulse equipment was tried at the same time. Experiments were made with two microwave sets: one using 1200 megahertz and the other using 500 megahertz. Results were dismal—especially when compared to ranges obtained with the 200-megahertz equipment. The 1200-megahertz set got echoes from objects at a maximum distance of but 2 kilometers (1-1/4 miles). The 500-megahertz device got no results, due to rough weather.[28]

The general study of microwaves at NRL continued after these tests, but the attempts to build microwave radar using the continuous-wave method were dropped. The Laboratory did not, by any means, abandon the idea of using microwaves for radio detection. But in the future, it approached this possibility by moving up from lower frequencies, where success had already been attained. And in the future, it worked almost entirely with the pulse technique. In 1937, given the results to date, it finally seemed imperative to accept the policy advocated by the Bureau of Engineering and await further developments by industry on microwave tubes.

The history of NRL's early microwave radar project is notable for several reasons. First it shows that there was no obvious path to practical detection equipment. The choice of method, the choice of frequency, and the choice of components all depended on the results of research and experimental development. Second, the project provides another view of the forces that shaped the origin of radar at the institution. By comparing it to the pulse radar effort, one gains a far deeper understanding of the interplay of administrative decisions and technical developments that characterized NRL's operation in these years.

SHAPING RELATIONS WITH PRIVATE INDUSTRY

The development of naval radar would not have been possible without cooperation between NRL and American electronics companies. Not only were they called on to produce anything the Laboratory designed, but they also created many new forms of equipment for naval use. In those cases, NRL frequently passed judgment on their models prior to large-scale procurement. Hence, interactions with industry were an extremely important part of the Laboratory's activities and an important part of the evolution of the radar field.

RCA, whose early work was discussed in the preceding chapter, was the first firm to be drawn into the Navy radar program and produced the first shipborne equipment. This would be only the beginning of an involvement that would grow to massive proportions during World War II and would make radar a major portion of RCA's business. The company would eventually design and build various series of radars for a wide variety of ships and aircraft and for numerous naval uses—including general search, fire control, and navigation.[29]

[26]Letter from NRL to the Bureau of Engineering, May 5, 1936, in file C-S67/35 (note 8).
[27]In the preceding chapter, where notes 12 through 16 apply.
[28]Letter from NRL to the Bureau of Engineering, May 8, 1937, in file C-S67/35 (note 8).
[29]"RCA's Contribution to the War Effort Through Radar, 1932-1946" (unpublished manuscript available from RCA).

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The American Telephone and Telegraph Company was the second major industrial firm to become involved in the development of naval radar. The story of its efforts would be one of even greater magnitude. In 1946, Mervin J. Kelly, Director of Bell Telephone Laboratories, looked back and summarized,

> The Bell System played a larger part in the [American] radar program [until the end of World War II] from research through production, than any other industrial organization. Through its manufacturing company—the Western Electric—it produced about half of all the radar made in the United States; and through its research organization—the Bell Telephone Laboratories—it carried out a comparable portion of the research and development programs.
>
> Throughout the preparedness and war years, the Laboratories, with a continuously expanding program, gave almost half of its effort to radar science and technology. It expended approximately $85,000,000 in its research, development, design, and early model production effort.
>
> The Western Electric Company produced equipments resulting from this effort in a volume of approximately $900,000,000, and there was reproduction of radar facilities by others, from designs completed in Bell Laboratories, in volume of at least $100,000,000.[30]

Note that these figures include radar produced for the Army and American allies as well as for the Navy.

AT&T's huge radar program originated from interaction with NRL. Captain W. J. Ruble, head of the Radio Division of the Bureau of Engineering, was well aware of AT&T's expertise in the radio field, and, in mid-1937, he reasoned it would be profitable to the Navy to disclose its progress in radar to the company. On July 13, 1937, three engineers from Bell Telephone Laboratories, accompanied by Lt. Cdr. J.B. Dow of the Bureau of Engineering, visited NRL to hear about radio detection. A report on the conference states,

> [The men] were given a brief review of the Laboratory's earlier experience with the beat method of detecting echoes, followed by a statement of the reasons why the Laboratory believes the pulse echo methods to be far superior. The nature of the pulses was discussed and a pulse transmitter was examined. One of the pulse receivers was examined and discussed. The experimental rotatable 200 megacycle beam used in connection with pulse work on that frequency was examined and discussed....The difficulties inherent in carrying the work to still higher frequencies more suitable for shipboard installation were considered.[31]

The report also recorded the reactions of the group to what NRL had accomplished:

> The principal comments were made by Mr. [E.L.] Nelson of the Bell group. He stated that it was almost unbelievable that echo signals of this character could be received from such distances but he was forced

[30] Reprint of Mervin J. Kelly, "Radar and Bell Laboratories," *Bell Telephone Magazine* **24** (winter 1945), pp. 5 and 6.

[31] All quotes are from a memorandum from the Superintendent, Radio Division, to the Director, NRL, rough draft, undated, but circa July 16, 1937. This draft is in the papers of Robert M. Page, Historian's office, NRL, Washington, D.C. The final draft is the letter from NRL to the Bureau of Engineering, July 16, 1937, in file C-S67-5 #1, box 31, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building. It is essentially the same as the rough draft but is shorter.

> to believe the evidence of his own eyes. (We refer here to the photographic records.) He further stated that it was unlikely that such a problem would ever have been undertaken by the Bell Laboratories and that we were fortunate in not having a board of directors to approve such a problem, as it might have been very difficult in the beginning to guarantee success and show an important outcome, especially in financial returns, from their point of view.

Obviously the NRL representatives had not felt it necessary to discuss their own administrative problems! On the role for AT&T in the radar field, everyone agreed:

> [Mr. Nelson] stated that while the Naval Research Laboratory had very beautifully laid the research foundation for this work and demonstrated the ultimate feasibility, yet there was evidently an enormous amount of practical development work to be done in the future to adapt it satisfactorily to Naval conditions. In this opinion he was confirmed by the Superintendent of the Radio Division and assured that it was the intention of the Laboratory to urgently prosecute this problem but that we would welcome any assistance which the Bell Laboratories might be prepared to give and which might be arranged between them and the Bureau of Engineering.

Back at company headquarters, there was interest, yet hesitance and hence delay. Then in November, representatives from the Bell Telephone Laboratories came to NRL for a second time to learn about further progress in pulse radar. After this, the directors of AT&T decided that the company should definitely enter the radar field. Initially, however, they determined that it had to be at their own, not Navy, expense. Mervin Kelly later explained,

> Under the then established rules of Navy development contracts, it was not practicable to contract for such a highly speculative research program [as we wanted to initiate]. However, all concerned recognized the importance of such an investigation.
>
> Because of its potentialities for results of importance to our country's preparedness program, and because of the intimate relation of Bell Laboratories' centimeter-wave radio research to the centimeter-wave radar problems, the American Telephone and Telegraph Company authorized the Laboratories to proceed with the investigation. This made it possible for the Laboratories to carry the radar excursion into the shorter wavelength or to a place where, if successful, contracts could be entered into with the Navy for a development to specified requirements.[32]

The official history of the Bell Telephone Laboratories in these years elaborates further,

> During the war years most Bell Labs military work was done under contract with Western Electric (on a cost-plus-fixed-fee basis), with Bell Labs being a Western Electric subcontractor. Under this arrangement, Bell Lab's costs on a project were a part of the Western Electric contract cost. This was a fair way to handle a project for a military weapon, but AT&T was reluctant to have Western Electric accept a contract for a research project which might ultimately benefit the Bell System more

[32] Kelly, *op. cit.* (note 30), p. 9.

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> than the military. For this reason, AT&T bore the cost of a number of exploratory jobs until such time as it became apparent that a contract would be appropriate because the specific work covered could be aimed at developing a useful and producible military device or system. Some of this reasoning was responsible for the AT&T payment for early radar exploration.[33]

A microwave radar research program was established at the Bell Telephone Laboratories in New York late in 1937. In May 1938, it was moved to facilities at Whippany, New Jersey, 50 kilometers (30 miles) away, to facilitate secret experimentation. Throughout 1938 and early 1939, all work was done at the company's own expense.[34] And, despite what was said when the engineers from the Bell Telephone Laboratories visited NRL, there is some indication that the project was initially regarded with at least some indifference by the military. In a report about a conference held on April 29, 1938, Robert Page noted,

> According to Mr. [F.K.] Lack [of Western Electric], European manufactures are busy supplying heavy demands of their respective governments for high-frequency apparatus in the decimeter wave-length range for military application. They are using methods and machinery developed in this country, based on theoretical work originating in this country. They expressed surprise at the lack of interest in such developments on the part of the U.S. Government, particularly since said developments are available in this country and their military value both to the Navy and the Army is so obvious.
>
> Mr. Lack said that Gen. Mauborgne, Chief of the Signal Corps, recently indicated definitely that the Army was not interested in decimeter waves, an attitude which Mr. Lack could not understand. He (Mr. Lack) said further that about the only interest shown by the Navy was an occasional visit by Commander Ruble [of the Bureau of Engineering]. He wondered why radio engineers did not visit their laboratory to learn of the contributions they had to offer, pointing out that it has been over ten years since the Bureau of Engineering has sent Radio Engineers from NRL to visit Bell Telephone Labs. In referring to a one-time difficulty over patent matters, Mr. Lack said that when it came to a matter of national defense such things should be forgotten rather than to let foreign powers use us as a spring board to get the jump on our government in the developments of high military value.[35]

In subsequent months, perhaps partly because of this report, communication did improve. NRL engineers visited the Bell Telephone Laboratories on several occasions, and representatives from there were kept up to date on NRL's 200-megahertz radar.

In their development program, Bell engineers worked on designing pulse radar that operated at around 500 to 700 megahertz. Like Deke Parsons, they saw the tremendous potential of microwaves for gunfire control and other tasks that 200-megahertz radar was not precise enough to perform well. But, like the men in the microwave project at NRL, they were beset with the difficulty of generating

[33] M.D. Fagen, ed., *A History of Engineering and Science in the Bell System*, vol. II (Murray Hill, N.J.: Bell Telephone Laboratories, 1978), pp. 12 and 13.
[34] Kelly, *op. cit.* (note 30), p. 10.
[35] Report on the conference of Apr. 29, 1938, in file S67/35, box 77, job order 11704, records of NRL, record group 181, Washington National Records Center, Suitland, Md.

radiation of sufficient power at such a high frequency. Indeed, at the Bell Telephone Laboratories, as elsewhere in America, the real potential for sophisticated microwave equipment had to await the disclosure by the British, late in 1940, of a radically new transmitting tube called the multicavity magnetron. However, with the resources available within AT&T, including a large, experienced tube department, the researchers were able, by mid-1939, to devise a microwave transmitter strong enough and receiver sensitive enough to detect targets at ranges up to around 25 kilometers (15 miles).[36]

Army and Navy representatives first witnessed demonstrations of this equipment in July at Atlantic Highlands, New Jersey, overlooking New York Harbor. The results prompted the Navy to give the company a contract to begin producing shipboard equipment.[37] After a year and a half of independent investigation, the radar venture was finally paying off. But, as at NRL, it took months to move from test equipment to a finished set. Work on the Navy contract started in December 1939; the first equipment, dubbed the CXAS, would not be installed until June 1941. After some improvements, including the addition of a magnetron transmitting tube, nine more, renamed in production form the FA, would be manufactured and placed on other vessels. They would be the Navy's first standard fire-control radars. From then on, AT&T would remain the leader in this line of equipment, producing most of the fire-control radars used in the war.

The relations the Navy established with RCA and AT&T are good examples of the kind of interactions it would come to have with numerous industrial firms. Soon General Electric, the Submarine Signal Corporation, Raytheon, Federal Telegraph and Radio, and thousands of other, smaller companies would be involved in equipping the fleet. The diversity of the products they would produce has led one student of radar history to comment, "In 1945, the U.S. Navy operated a vast, even bewildering, variety of radars, a variety due more to the variety of manufacturers than to a spectrum of requirements."[38] But such would be the price of relying on a wide range of American firms. The extent of this interaction was not yet apparent at the end of the 1930s, but the foundation, as we have seen, was firmly in place.

THE NAVY RADAR PROGRAM TO MID-1940

As success followed success in the pulse radar project at NRL and as new men were added, the effort began diversifying. This tendency, which had started before the XAF was tested at sea,[39] became even more pronounced afterward. NRL engineers recognized the need to exploit the new field as fully and as rapidly as possible and were eager to do so.

On February 26, 1940, the Director of the Laboratory sent a letter to the Chief of the Bureau of Engineering reporting in detail on the radar program. Written by Robert Page,[40] it was the first comprehensive summary in over a year. Reviewing it will give a clear picture of the extent to which the project had expanded and changed by that time. Page discussed six areas of activity: improvements to the 200-megahertz equipment, development of 400-megahertz equipment, investigations at higher frequencies, development of auxiliary apparatus, recognition with radar sets, and fire-control applications.

Improvements of the 200-megahertz equipment had been made after the test of the XAF and before production of the CXAM. These involved design modifications that gave the set greater range, a slightly smaller antenna, and clearer indication of echoes. Although NRL was completely satisfied with the result, Page explained that not much more was being done at this frequency:

[36]Kelly, *op. cit.* (note 30), p. 10.
[37]Fagen, *op. cit.* (note 33), p. 25.
[38]Norman Friedman, "U.S. Naval Radars: An Introduction" (unpublished Hudson Institute discussion paper, H 1-2570-DP, 1977), p. 61.
[39]See, for example, the paragraph where notes 20 and 21 apply.
[40]Letter from NRL to the Bureau of Engineering, Feb. 26, 1940, in file S-S67-5 #2, box 4, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building. The initials in the upper-right corner prove Page's authorship.

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> ...it would be possible to develop a far superior 200 m.c. system. However, since the XAF (now designated CXAM) equipment is adequate for indoctrination of the system in the service, and furthermore, since the undesirably large antenna structure can only be appreciably reduced by going to a higher frequency, further 200 m.c. development is no longer being continued.[41]

Instead, effort was being concentrated on developing practical equipment operating at around 400 megahertz. This idea was not new. As early as August 1937, Page had assigned several of his assistants, including Robert Guthrie and Irving Page, to work on pulse radar equipment in this region of the spectrum.[42] For several months, they experimented at around 500 megahertz and higher, but by February 1938, they had decided, on Dr. Taylor's suggestion, "to complete [a] system on 440 m.c. rather than to make too big a jump to 700 or 800 m.c."[43] By the fall of 1939, 400-megahertz equipment was in operation and achieving ranges on aircraft up to 70 kilometers (80,000 yards). However, it had not yet matched the performance of the XAF. It was hoped that, with alterations, the set would be able to do so by the summer of 1940. As for RCA's competitive 400-megahertz radar, which had been sent to NRL after testing in the fleet, Page commented, "It is apparent that performance of this equipment is far below that obtained from 400 m.c. equipment developed at this Laboratory."[44]

At the time of the report, the chances of practical radar at frequencies much higher than 400-megahertz still looked rather bleak, because of the lack of transmitter tubes that could produce enough power. The contract with the Bell Telephone Laboratories, Page notes gloomily, had as yet "failed to produce results."[45] But he pointed out that progress in the radio industry might soon change the situation. In any case, NRL, as the Bureau of Engineering had mandated, was now leaving this area of investigation up to private companies.

The Laboratory was better suited for improving auxiliary control and indicating equipment for radar. One significant project of this sort it had undertaken was devising a means for using a double beam of radiation from a single transmitter to get two signals—right and left deviations—from each target. This technique, called lobe switching, allowed far greater precision in tracking. Once perfected, it would become a standard technique.

Also under investigation was a new form of indicator that would, for each object detected, make a dot on the cathode-ray screen corresponding to its position in space. As Page explained, "such a system would amount to a polar chart on the cathode-ray tube of the area surrounding the ranging equipment."[46] The need for something of this sort had become obvious to him during the mock battles in which the XAF was tested, for there were then so many aircraft in the area that it was impossible to keep track of them all one by one. Eventually, his plan for a solution would lead to what became called the plan-position indicator, or PPI. This screen, with rotating radial sweep, has since become perhaps the most widely recognized part of all radar technology. Page would acquire several patents on circuits employed in his design, but, like so many other aspects of radar, the PPI was also invented independently elsewhere.

Finally, in the area of design modifications, NRL engineers were studying improvements to keying circuits, receiver circuits, antennas, duplexers, and cathode-ray indicating tubes. They were even beginning to consider standardizing parts, in anticipation of large-scale mass production of various forms of radar.

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[41] *Ibid.*
[42] Robert M. Page, laboratory notebook 346, vol. IV, p. 53, in records of NRL, Records and Correspondence Management office, NRL, Washington, D.C.
[43] *Ibid.*, p. 70.
[44] Letter from NRL to the Bureau of Engineering (note 40).
[45] *Ibid.*
[46] *Ibid.* Page also discusses his work on indicators in *The Origin of Radar* (New York: Doubleday, 1962), pp. 135-167.

In addition to radar equipment itself, it was necessary to develop related devices. One of the most pressing requirements was equipment that could distinguish friendly from enemy objects at distances comparable to those at which they could be detected. Without it, the utility of radar in wartime would be greatly diminished. Actually, the need for better means of long-range identification had long existed in the Navy; radar had only made it more critical. Researchers at NRL had decided that the answer might come from some form of radio equipment[47] and had begun working, in the microwave project, on developing such a system. The first practical model, the XAE, was initially tested in 1937.[48] A different, strictly radar-related set went on the fleet exercises along with the XAF in 1939. In his letter, Page now reported that the device was not yet ready for production but that research was continuing, particularly on making the system secure from enemy interference or deception.[49]

All of the early work on pulse radar equipment—that on the 28-megahertz, 200-megahertz, and 400-megahertz sets—had been directed toward building general-purpose detection and ranging devices. These were not well suited to perform specialized functions like fire control. As described earlier in this chapter, an attempt had been made at NRL to design continuous-wave fire-control radar that used microwaves, but to no avail. The Bell Telephone Laboratories now had its Navy contract to design fire-control radar, but Page reported that some work was also being done on the subject under his direction. He said,

> The program of development has been laid out for this problem and there is a certain amount of preliminary work already done. However, neither this problem nor the one on recognition is being actively prosecuted at this time due to the urgency of [other facets of the radar project].[50]

Before closing the report, Page outlined work being done on radar by other institutions in America. After he described the situation at the Signal Corps Laboratories, RCA, Bell Laboratories, and the General Electric Laboratory, he concluded proudly,

> While there is no intention to discredit the fine engineering done on this very difficult problem at other laboratories, the fact remains that rather complete and comprehensive disclosures have been made by this Laboratory to representatives of each of the above four groups before any consequential progress was made in that group, and no other group has as yet remotely approached the state of development of the problem that has been reached by the Naval Research Laboratory.[51]

This statement was true when Page wrote it, but it would not long remain so. The gap between NRL and other American institutions was rapidly closing and would soon disappear. And, as will be explained in the next chapter, had he known what was going on across the Atlantic in Britain, he might have been less boastful even now.

Looking to the future of radar at NRL, Page recommended that highest priority be placed on finishing the 400-megahertz equipment and getting production models into the fleet. The work on auxiliary equipment should continue, and industry should be urged to develop, as rapidly as possible, better tubes for transmitting and receiving microwaves. Finally, he recommended that more personnel and

[47] A. Hoyt Taylor, *Radio Reminiscences* (Washington: NRL, 2nd printing, 1960), pp. 180 and 181.
[48] Louis A. Gebhard, *The Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979), p. 251.
[49] Letter from NRL to Bureau of Engineering (note 40).
[50] *Ibid.*
[51] *Ibid.*

UNCLASSIFIED

facilities be allocated to him for work on recognition equipment and fire-control radar. At this time, there were still less than a dozen people prosecuting all parts of the radar project.[52]

Response to Page's report from the Bureau of Engineering was rapid and unambiguous:

> The Bureau considers that the inherent possibilities of radio echo methods are such as to offer compelling reasons for pressing the development of all phases of this problem insofar as is consistent with reasonable economy.[53]

And soon a few more men were added to the project.

The general policy of the Bureau, which was then heading the Navy radar program, is best seen in a letter it sent to the Chief of Naval Operations later in March.[54] The letter began by putting the new field of technology in perspective:

> Radio echo equipment at present is in that critical stage of development that occurs between a successful demonstration and the widespread service application of fully developed practical equipment in any major development. That this can be a long and trying period is amply borne out by the histories of television and underwater sound development. Serious work on underwater sound equipment was commenced by the Navy in 1922, and it was not until very recently that this instrument could be considered to have approached its full effectiveness in the fleet. Television is a more difficult achievement and much more time and effort have been expended in bringing its development to the present stage....The Bureau is pressing the development of radio echo equipment but it is probable that considerable time and effort will be required to bring it to fruition.[55]

Considerations of time were critical. World War II had been embroiling Europe for over six months, and it was having a decided impact on Navy planning. The letter continued,

> It is unfortunate, from the viewpoint of economy, that a developmental program involving a moderate annual expenditure over a period of ten or fifteen years cannot be allowed prior to the necessity of producing a shipboard equipment. It is recognized, however, that the present situation requires that the best equipment practicable (for the time being) be available for installation in the Fleet at any time. This is a quite different problem from that of a long range development alone. While it is impossible to foretell the progress of a development of this nature, it appears at present that the degree of performance of any equipment available in the near future will bear a close relation to the amount of funds provided annually for development. The several fold increase in funds (over that required by a long time development program) necessitated by the present situation should be regarded as insurance. It is the cost of having the best possible equipment available for the Fleet in case of an emergency.[56]

[52]This figure comes not from Page's report but from Bureau of Ships memorandum for Admiral Van Keuren, Sept. 30, 1940, in file S-S67-5 #2 (note 40).
[53]Letter from the Bureau of Engineering to NRL, Mar. 7, 1940, in file S-S67-5 #2 (note 40).
[54]Letter from the Bureau of Engineering to the Chief of Naval Operations, March 23, 1940, in file S-S67-5 #2 (note 40).
[55] *Ibid.*
[56] *Ibid.*

The appropriate policy to follow, the Bureau had decided, was one of diversified effort. Consequently the letter proposed that,

> ...the three major electronic laboratories of this country, namely the Bell Laboratories, the General Electric Company and the RCA Manufacturing Company, as well as the Naval Research Laboratory be kept actively engaged in this project. There will not necessarily be any serious duplication of effort among these companies as the Bureau exercises some guidance in this respect. It is believed that the disadvantage of any unavoidable duplication will be more than offset by the advantage of commercial competition and rivalry in this new field.[57]

The letter concluded with a warning about undue haste, recommending that,

> ...in spite of anticipated pressure from the Fleet, no more [radio echo] equipment be procured in quantity until a much superior equipment to that now in sight is produced. With the current pace of development, considerable improvement is to be expected annually, and unless caution is exercised, equipment may be ordered for the Fleet which will be obsolescent in a short time.[58]

These then were the guidelines for controlling the radar program as it entered its period of maturity: move with haste on development but with caution on procurement; begin equipping naval vessels with radar but beware of installing large numbers of a model that would soon be superseded.

Straightforward as such a policy may seem, following it was not easy, especially under the pressure of impending war. And there were other problems. Radar had been kept as secret as possible.[59] Only top naval officers and the engineers and scientists directly involved with its development had been kept informed. Once introduction into the fleet began, however, electricians in shipyards and on ships had to be taught the intricacies of the new devices. Radar operators also had to be trained. Unless they understood fully the use of the equipment, it was of no practical value. All this had to be done hurriedly, yet secretively. And it had to be done while the radar field was beset with difficulties inherent in any new system, such as frequent changes and improvements.

Moreover, there were administrative problems connected with the growing importance of the radar field. As has been explained, early Navy radar development was almost entirely the responsibility of the Bureau of Engineering. Once procurement of specialized fire-control radar began however, the Bureau of Ordnance became involved. The advent of airborne radar would draw the Bureau of Aeronautics into the program. Each of the bureaus would have the responsibility for radar that was to be used in connection with the other equipment under its control. Thus each had its own special interests in development projects, and each had its own sense of priorities, a sense not wholly shared by the other bureaus. Who was to control what in radar development and production? The threat of war, of course, fostered cooperation and helped eliminate minor disagreements, but the pressures of the situation tended to aggravate serious problems. Workable solutions would be found, but disagreements

[57] *Ibid.*
[58] *Ibid.*
[59] Even Deke Parsons was not immune to the effects of tight classification. In 1939, several years after leaving NRL, he happened to be in the Caribbean when the first radar was being tested on the *New York*. Seeing the antenna and guessing what it was for, he ventured aboard and began asking how the tests were going. He was quickly threatened with a court martial! (Letter from W.S. Parsons to E.B. Taylor, Nov. 6, 1945 (note 4)).

UNCLASSIFIED

over cognizance would nag the Navy radar effort until the end of World War II.[60] Such difficulties had already begun to appear by mid-1940.

Confusion in the implementation of the radar program was mirrored in confusion about the changing role of NRL itself. German military operations were making obvious the overwhelmingly technical nature of modern warfare and the significance of technical advancements. The products of organized research and development were crucial. Was not radar itself evidence of that fact? The same might be said of NRL's work on sonar, communication equipment, chemical products, optical devices, and other projects during its first 17 years of operation. Yet, it was no secret that the Laboratory had been strapped by low funding and low priority throughout this period. Much had been accomplished with limited means. How much more might be possible with greater support? In the face of this emergency how should the Navy administer its leading research institution?

The impending war led to a reconsideration and change of NRL's place in the Navy establishment. As it happened, this was related to an even broader administrative reorganization: the combination of the Bureaus of Engineering and Construction and Repair into a single new organization, the Bureau of Ships.

A NEW ROLE FOR THE LABORATORY

For many years, a basic administrative difficulty had been troubling the Department of the Navy.[61] Responsibility for constructing and equipping naval vessels was divided. The Bureau of Construction and Repair was charged with the design, fabrication, and maintenance of ships; the Bureau of Engineering was charged with installing and overseeing the machinery used for propulsion. As shipbuilding became more complex, these duties had become increasingly intertwined, but effective cooperation between the bureaus was not always realized. In the late 1930s, two particular occurrences made the problem embarrassingly evident.

The first was the introduction of high-pressure, high-temperature steam machinery into destroyers. The Bureau of Engineering had decided early in the decade that this step was essential in modernizing propulsion. However, there had been hesitancy in other parts of the Navy, and as the program evolved, protracted disagreements erupted between Engineering and the Bureau of Construction and Repair, the Board of Inspection and Survey, and the General Board of the Navy.[62] At issue was not only the wisdom of the decision but who should be making it.

The second incident was a controversy over what became known as the "top heavy" destroyers. On April 2, 1939, the USS *Anderson*, first in a new class, failed its inclination test. The vessel was overweight and improperly balanced. An inquiry showed that responsibility for this design flaw was divided almost equally between the Bureau of Engineering and the Bureau of Construction and Repair; lack of coordination seemed to have been the principal cause of the error. It had resulted partly from the administrative fact of bureau independence and partly from personalities—Rear Admiral W. G. Dubose, Chief Constructor, and Rear Admiral Harold Bowen, Chief Engineer, did not get along well,

[60] Rowland and Boyd, *op. cit.* (note 11), pp. 415-416; U.S. Bureau of Ships, "An Administrative History of the Bureau of Ships During World War II" (unpublished history in the series "U.S. Naval Administrative Histories of World War II," deposited in the Navy Department library, 1952), p. 288.

[61] The principal sources consulted for information on the formation of the Bureau of Ships were Capt. Robert M. Madden, "The Bureau of Ships and its E.D. Officer," *Journal of the American Society of Naval Engineers* **66** (1954): 9-41; Rear Adm. Julius A. Furer, *Administration of the Navy Department in World War II* (Washington: GPO, 1959), pp. 210-222.

[62] Madden, *op. cit.* (note 61), pp. 16 and 17; Rear Adm. Harold G. Bowen, *Ships, Machinery, and Mossbacks* (Princeton: Princeton University Press, 1954), pp. 47-126.

personally or professionally.[63] The press, not surprisingly, jumped on the destroyer story with enthusiasm and publicized it extensively, thus creating widespread demands for action. Additional pressure came from the Senate Naval Affairs Committee, which, under the leadership of Chairman Carl Vinson, had been seeking to make basic administrative changes in the Navy Department since 1933.

On August 11, 1939, Charles Edison, Acting Secretary of the Navy, initiated a reorganization that eventually led to the creation of the Bureau of Ships. Initially, the change was not to be so far-reaching. He called only for the consolidation of the design divisions of the two shipbuilding bureaus and ordered the two bureau chiefs to draw up plans for effecting it. Predictably, the two men could not agree, and on August 29, two plans were submitted. The Secretary then established a board under Rear Admiral Samuel Robinson to reconcile them. Instead of doing so, the Board recommended a complete unification of the bureaus, saying that as long as there were two separate chiefs, the danger of divided responsibility would continue.

Secretary Edison, although surprised, concurred in this judgment and began taking all steps legally open to him to bring about the consolidation. He appointed new chiefs of the two bureaus: Rear Admiral A. H. Van Keuren for the Bureau of Construction and Repair and Rear Admiral Samuel M. Robinson for the Bureau of Engineering. Additionally, Admiral Robinson was assigned as Coordinator of Shipbuilding, and Admiral Van Keuren was assigned as Assistant Coordinator. Finally, the Secretary ordered the two men to rearrange their organizations in preparation for a merger. The final steps had to be left to the legislators. A bill establishing the Bureau of Ships and making other changes was introduced into Congress, debated, amended, and ultimately passed. It was signed into law on June 20, 1940. Rear Admiral Robinson was then named the first chief of the Bureau of Ships.

The former Chief Engineer, Rear Admiral Bowen, who had coveted the new position for himself, was unhappy about what had transpired. He wrote in retrospect,

> The amalgamation...was the work of opportunists. On September 12, 1939, the [Robinson] Board, not even having been in session two weeks, submitted its report recommending the merger of the two Bureaus and the merger of the engineers and the naval constructors, subjects never even mentioned in the precept of the Board. The Board also recommended that Rear Admiral Robinson be the first Chief of the new Bureau of Ships. Naturally I felt that my representatives on the Board [one of whom was Robinson] had run out on me. Men have been hung for less than that. Their defection put Mr. Edison, who had supported me all along, into an isolated and peculiar position. And he said: "Even your own people went against you." The reason given was that "the constructors would revolt" if I were the combined Chief. How I wish they could have had an opportunity![64]

Secretary Edison, who respected Bowen and his abilities, wanted to give him a new position of importance[65] and named him Director of NRL. On the surface, this seemed a demotion. Since Rear Admiral W. S. Smith presided over the Laboratory's creation, NRL directors had all been only captains. Furthermore, Bowen had previously had administrative control of NRL while head of the Bureau

[63] Madden, *op. cit.* (note 61), p. 16; tape-recorded interview with Rear Adm. Harold G. Bowen, Jr., Apr. 23, 1979, in the Historian's office, NRL, Washington, D.C.

[64] Bowen, *op. cit.* (note 62), pp. 119 and 120. The view about the Board's "opportunism" is corroborated in Madden (note 61).

[65] Letter from the Secretary of the Navy Charles Edison to Hon. James G. Scrugham, Feb. 13, 1940, in Bowen, *op. cit.* (note 62), pp. 375 and 376.

UNCLASSIFIED

of Engineering. Now it seemed he was being "banished" to rule over a small part of his former fief.[66] The admiral would later, only half jokingly, describe the status of his new position in this way,

> The job of Director was not regarded in the Navy as much of a job. The laboratory was located in an unfinished part of the District of Columbia, between St. Elizabeth's (a mental hospital) and the Sewage Disposal Plant. Obviously, I was in bad odor.[67]

But, despite appearances, the assignment was not an exile. Edison was planning to use Bowen to upgrade the place of NRL in the Navy. A son of Thomas Edison, the Secretary had worked in the family business for many years before entering politics in 1932. His time there included the period of World War I, when his father was President of the Naval Consulting Board. Thus he was intimately familiar with why NRL had been created. And, like his father, he came to believe that the Navy did not have enough respect for the power of research and was not giving it adequate support. Indeed, to his mind, the history of NRL showed that this was as true in 1939 as it had been in 1916. Yet the technical prowess of Germany made NRL far more important than ever before. Edison decided that the best way to remedy the situation was to build a new, centralized organization for naval research based on the Laboratory. Bowen, he believed, was the right man to head it. The Secretary explained his views in a letter to Lyman Chalkey on December 8, 1939,

> The whole subject of research in the Navy has been constantly under my attention for a long time....
>
> After a great deal of investigation and consideration, recently I have inaugurated the following steps to centralize the control of research in the Navy and to emphasize its importance.
>
> Naval Research is conducted by the Naval Research Laboratory, by other Naval laboratories, by other Government laboratories, by commercial research laboratories, and by the laboratories of certain universities.
>
> Early in October I assigned Rear Admiral H.G. Bowen, USN, formerly Chief of the Bureau of Engineering, as Director of the Naval Research Laboratory, with additional duties as Technical Aide to the Secretary of the Navy. I also transferred the Laboratory from the jurisdiction of the Bureau of Engineering to the jurisdiction of the Office of the Secretary of the Navy. Furthermore, where it had not already been done, I have established in each of the Bureaus of the Navy Department a full-time research investigation section with an officer of suitable rank in charge who is a liaison officer between that Bureau and the Director of the Naval Research Laboratory. These liaison officers, together with the Director of the Naval Research Laboratory, form the Research Council of the Navy Department which is directly responsible to the Secretary of the Navy. In order to still further amalgamate all activities which have anything to do with research, I have transferred from the Chief of Naval Operations to the Office of the Director of the Naval Research Laboratory the Office of Inventions.

[66] *Ibid.*, p. 137.
[67] Harold G. Bowen, "Reminiscences, "*Journal of the American Society of Naval Engineers* **69** (1957): 293.

> You will therefore see that for the first time in the history of the Navy we have a centralized and coordinated control of all research work.[68]

Edison's plan was formalized in two Navy Department general orders. General Order 124, dated September 14, 1939, transferred NRL to the Secretary's Office; General Order 130, dated December 8, 1939, established the Navy Department Council for Research and made the Director of NRL its senior member. It also resurrected the position of Technical Aide to the Secretary of the Navy, which had been abolished in 1932,[69] and bestowed it once again on the Director of NRL.[70]

Bowen described his reaction to his new situation in a letter to a friend,

> Of course, I would have liked very much to have been Chief of the combined Bureaus of Engineering and Construction & Repair if for no other reason than the Bureau of Construction & Repair was not, in my opinion, as far advanced or as well organized as the Bureau of Engineering, and I was the logical candidate to make some much needed and extensive changes in that Bureau as well as in its policies.
>
> However, all's well that ends well, and I am very glad to be here in charge of the Laboratory and Technical Aide to the Secretary for two reasons. First, because I like the work and the location of the Laboratory, and because Mr. Edison has some advanced ideas in regard to centralizing and emphasizing research in the Navy which I shall be very glad to direct.[71]

The idea of using NRL as a center for Navy research was not new: it had been part of the original plan of the Naval Consulting Board. The realities of Navy funding after World War I and the reluctance of most of the material bureaus to use the institution temporarily submerged it, but it soon resurfaced. Various Laboratory administrators continued to suggest and promote it over the years. For example, when submitting data to the Secretary of the Navy for the annual report of 1928, then Assistant Director E. G. Oberlin wrote that NRL was engaged in the "building up of an organization with the Naval Research Laboratory as a nucleus, to use and coordinate the scientific brains of the country in national defense in time of war."[72] Similar arguments became standard in the repertoire of those who argued for support over the years. Until Secretary Edison, however, no one had ever taken them very seriously. Until Bowen was put in charge, no one had the authority to put them into effect.

In his seminal *The Politics of Innovation: Patterns in Navy Cases*, Vincent Davis chooses Harold Bowen as one of a class of men he characterizes as "innovation advocates" in the Navy.[73] Such men, Davis argues, have frequently been responsible for winning adoption of new technical programs, often in the face of entrenched opposition. Although seldom inventors themselves, they usually have the technical background to understand advances better than their colleagues. Once convinced of the importance of an innovation, they become passionate zealots in promoting it. Davis states,

[68]Letter from Charles Edison to Lyman Chalkey, Dec. 8, 1939, in box 1, papers of Harold G. Bowen, Mudd Manuscript Library, Princeton University, Princeton, N.J. Surprisingly no records relating to this action have been preserved in the official archival records of the Office of the Secretary of the Navy.

[69]See note 48 in Chapter 5.

[70]Copies of the general orders are included in Appendixes C and D.

[71]Letter from Harold G. Bowen to R.W. Bates, Nov. 15, 1939, in box 1, Bowen papers (note 68).

[72]Memorandum for the NRL Director with data for the Annual Report of the Secretary of the Navy, Nov. 26, 1928, in file A9-1, box 7, records of NRL, Unclassified series, National Archives Building.

[73]Vincent Davis, *The Politics of Innovation: Patterns in Navy Cases* (Denver: University of Denver, 1967).

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> The innovation advocate is a man of strong attachments and convictions, given to expressing himself in enthusiastic and sometimes exaggerated and even emotional terms, but his real love is the organization itself (i.e., the Navy) and the nation that he tends to identify with the organization. He is a dedicated patriot, but he does not hesitate to criticize those things he loves (although he is not very charitable towards outsiders who may offer similar criticisms) because he is also a perfectionist. The characteristics of an organization or a procedure that he tolerates least well are inefficiences of any kind, obsolete practices that can be justified only by tradition, and dull conventional thinking. If there is a better way to do it, he is determined to see it done the better way, and he is greatly annoyed by opponents who give him routine unthoughtful replies....He tolerates and indeed appreciates well-reasoned rebuttal, but he does not suffer fools gladly. He has a certain intellectual arrogance, because he is impatient with what he regards as dull minds. He attracts followers in part by the sheer charm of his driving dedication and his superior intellect, but he also attracts opponents from among equally intelligent people who prefer a calmer and more cautious approach as well as from among those more conventional individuals who fear change in contrast to the comfort of established routines.[74]

This is an apt characterization of the man whom Secretary Edison chose to build the Navy's central organization for research.

Born on November 6, 1883, Bowen entered the Naval Academy in 1901.[75] Upon graduation, he served in a variety of positions at sea and ashore before entering the Naval Postgraduate School in 1912. This led, in 1914, to a master's degree in mechanical engineering from Columbia University. Because of his advanced training, he was able to become, in 1917, one of the first naval officers to be designated for engineering duty only. From that time on, his career was linked to technical improvement of the Navy.

For the next 14 years, he worked as a technical administrator in a number of minor posts. Then, in 1931, he was named Assistant Chief of the Bureau of Engineering. In 1935, he became Chief, a position he held until 1939. His most important work in this period was the fight to win adoption of high-pressure, high-temperature steam for propulsion equipment, but he also helped bring about such changes as the introduction of high-speed turbines and double reduction gears, alternating current on ships, auxiliary diesel generators, and flameproof cables. In his crusades, he made many friends and he made many enemies. Some of the enemies have already been mentioned. One of the most important of the friends was Congressman James Scrugham, the leading member and ultimately chairman of the Naval Appropriations Subcommittee. Scrugham, who had also been responsible for increasing NRL's appropriation at a critical time,[76] gave Bowen the financial and political support he needed to attain many of his goals.

Bowen assumed the directorship of NRL on October 9, 1939, and began work with enthusiasm and determination. He was greatly aided by an energetic Assistant Director, R.P. Birscoe, and a small staff of naval officers. These associates would frequently take care of details at the Laboratory while

[74] *Ibid.*, pp. 52 and 53.

[75] Information here comes from Bowen *op. cit.* (note 62) and from the biographical sketch of Harold G. Bowen, Records of Officer Biographies Branch, Office of Naval Information, in the Operational Archives Branch, Naval History Division, Washington, D.C.

[76] See Chapter 6 where notes 23 through 26 apply and the present chapter where notes 18 through 21 apply.

Fig. 19 — Harold G. Bowen became head of NRL after having served as Chief of the Bureau of Engineering from 1935-1939. At the same time that he was presiding over the greatest period of growth in the history of the laboratory, he led an unsuccessful attempt to make it the center of all Navy research and development.

Bowen lobbied downtown. The first order of business was increasing the budget and staff. When making his initial appearance as Director before the House Naval Appropriations Subcommittee, Bowen explained the situation he faced and plans to deal with it in this way:

> *Admiral Bowen*: The Laboratory is hampered at the present time by a lack of space for scientific work and by a lack of an adequate number of personnel. On account of the wide application of radio for other purposes than communication [e.g., radar] it is proposed, when funds and space become available, to establish an electronics section, to cover the ever-widening and greatly increasing importance of this field of engineering. The same remark applies to research work in sound and supersonics....
>
> In general the expansion will consist of doubling up of personnel working on problems already underway in projects insofar as space limitation will permit in order to speed up completion of many urgent phases of work in connection with the naval expansion program.

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Mr. Scrugham: I think you covered that quite adequately.[77]

Bowen's appeal, along with the threat of war, had a decided effect. In the next budget, for fiscal year 1941, Congressional appropriations for NRL jumped from $370,000 to $653,350. In fiscal year 1942, the last year of his tenure, this total would more than double again, to $1,479,500. Funding from the bureaus increased in like proportions, from $552,612 in 1940 to $1,085,520 in 1941 and $2,077,631 in 1942. The number of buildings housing Laboratory activities increased in the same period from 17 to 42; the plant area increased from 28 acres to 58 acres; the number of personnel increased from 322 to 2,116.[78] NRL even gained an outpost. In 1941, land was purchased at Randle Cliffs on the western shore of the Chesapeake Bay, and facilities were constructed there for the testing of radio equipment, especially radar. This quickly became, and remained throughout World War II, one of the best sites in the United States for this purpose. In short, under Admiral Bowen, NRL would be transformed from a small institution employing less than 500 men into a multimillion dollar facility staffed by scientists, technicians, and skilled artisans by the thousands.[79] Bowen presided over the greatest period of growth in the Laboratory's history.

The most important technical project underway at NRL when Bowen arrived was radar, and he knew it. From his position in the Bureau of Engineering, he had watched it grow from a mere idea into the most important electronic device since radio. As Chief of the Bureau, he had helped increase funding for the project. Now he was in a position to affect the development of the new technology and its installation into the fleet even more directly. He soon began involving himself personally in the radar program. Around April 1940, he persuaded Hoyt Taylor to start an investigation of the idea of radar for submarines.[80] This eventually led to the SD series of submarine equipment designed and produced jointly by NRL and RCA. In August 1940, he met personally with Mr. Charles Wilson, the President of the General Electric Company, to draw the firm more deeply into work on Navy radar. A visit of top GE engineers to NRL was arranged, and soon Wilson was promising to devote to the subject "every facility at our disposal, promptly and vigorously."[81]

More generally, the Admiral gave high priority to development of all aspects of the field in order to get equipment into operation. Undoubtedly, he saw progress in radar as a test case of his new authority to centralize, upgrade, and strengthen Navy research and development. And he meant to exercise his power effectively.

[77] U.S. Congress, House, 76:3, *Hearings Before the Subcommittee of the Committee on Appropriations...on the Navy Department Appropriation Bill for 1941* (Washington: GPO, 1940), pp. 694 and 695.

[78] Alfred T. Drury, *War History of the Naval Research Laboratory* (unpublished history in the series, "U.S. Naval Administrative Histories of World War II," deposited in the Navy Department library, 1946), p. 46.

[79] *Ibid.*, p. 34.

[80] Bowen, *op. cit.* (note 62), p. 149; letter from NRL to the Bureau of Engineering and Bureau of Construction and Repair, Apr. 30, 1941, in file S-S67-5 #2 (note 40).

[81] Bowen, *op. cit.* (note 62), pp. 150 and 151. The quote is from a letter from C.E. Wilson to Admiral H.R. Stark, Chief of Naval Operations, Nov. 1, 1940, in file S-S67-5 #2 (note 40).

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9. THE BROADENING CONTEXT

The appointment of Admiral Bowen as head of NRL and the reorganization of research in the Navy brought the radar story full circle. The program and policies of the institution had led to the creation of a new technology. Now it, in turn, was a leading factor in basic administrative changes. Thus the focus of this narrative, having moved initially from administrative to technical events, will move back to administration, to the organization of Navy radar research and development for war. First, however, the broadening context will be sketched more fully. Admiral Bowen's actions would be strongly shaped by other developments in the radar field, both nationally and internationally. These must be reviewed.

EARLY RADAR IN THE U.S. ARMY AND GREAT BRITAIN

Radar is a classic case of simultaneous invention. As the historian Henry Guerlac has explained,

> Radio detection devices using the pulse-echo principle were developed independently and almost simultaneously during the 1930's by a number of the great powers. In 1939 closely guarded secret programs were in various stages of advancement in Great Britain, France, Germany, Canada and the United States. Russia, China, Japan and Italy were at that time without the equipment and seem to have acquired it after the outbreak of war, by capture and by disclosures from their allies....
>
> Such a duplication of effort will surprise only those who cling to a Hero Theory of scientific progress and demand for each discovery a single putative inventor; or those who are unaware of the frequency—one is tempted to write, the regularity—with which such parallelisms are encountered in scientific work.[1]

Early developments of radar that directly affected NRL were those made by private corporations in the United States, discussed in the preceding chapter, by the U.S. Army, and by Great Britain. The Army's program was linked fairly closely to that of NRL and, in many respects, followed a parallel path. This was true not only in technical progress but also in administrative matters. As in the Navy, funds for a long-range research project like radar were difficult to get and maintain in the 1930s. Summarizing the Army's development provides an interesting comparison to that of NRL.

As early as 1926, Major William Blair, who was then the Chief of Research and Engineering at the Office of the Chief Signal Officer in Washington, suggested to his boss, Major General Charles

[1] Henry Guerlac, "The Radio Background of Radar," *Journal of the Franklin Institute* **250** (1950): 285.

Saltzman, that a project be established to investigate the use of radio to detect aircraft.[2] Blair was well acquainted with radio propagation, especially of microwaves, because he had studied the subject while conducting his doctoral research under A. A. Michelson at the University of Chicago in 1905 and 1906. Saltzman saw merit in Blair's idea, but no support for testing it could be found. Both the Army Coast Artillery and the Ordnance Corps believed that the existing sound detection equipment was adequate for their needs.[3] Thus Blair had to shelve his thought temporarily.

Four years later, in June 1930, he was appointed Director of the Signal Corps Laboratories at Fort Monmouth, New Jersey, where detection of aircraft was a problem under investigation. The method being studied, however, was not reflection of radio waves but reflection of much shorter infrared light rays (Table 3, in Chapter 4). The idea was similar to that of continuous-wave radar: send out strong beams of radiation and locate objects by detecting reflections. Blair supported this project. Indeed, in December, when he was invited to NRL to witness a demonstration of the continuous-wave radio detection equipment then under investigation, he was unimpressed. He believed that NRL's device offered too little precision: it showed that there were aircraft in the vicinity, but not where each aircraft was.[4] Infrared detection seemed to him a better investment. Perhaps his negative view was partly shaped by emotion. He and Hoyt Taylor had an argument after the demonstration over whether any new scientific principles were involved—he saying there were none, and Taylor saying quite the reverse. After the incident, Blair, according to his own testimony, was never asked back to NRL.[5] In any case, this test did not lead him to initiate a radio detection project in the Signal Corps at this time. Work continued solely on infrared methods.

In February 1931, the Office of the Chief of Ordnance transferred to the Signal Corps Laboratories "Project 88," which was entitled "Position Finding by Means of Light." Soon this would provide the initial administration authorization for the study of radio detection. At the time of the transfer, "light" was already being construed to include infrared and heat rays—thus the project fit well with detection studies in progress. For the first year, all work under the project centered on locating aircraft and ships by the heat emitted from their engines. But in 1932 it became known that infrared radiations would not penetrate fog or clouds; hence the attractiveness of infrared detection diminished sharply. At about the same time, the letter from the Secretary of the Navy to the Secretary of War formally disclosing NRL's work in radio detection was forwarded to Ft. Monmouth with the remark, "this subject is of extreme interest and warrants further thought."[6]

The combination of these two developments with previous thinking by Blair and others at the Laboratories about the potentials of radio led to an amendment in the detection project to include radio microwaves. Microwaves were chosen instead of longer radiations because Blair was convinced of their theoretical advantages. NRL had one more input to the Army project at this stage. One of the principal researchers assigned to the Signal Corps project was William D. Hershberger, who recently had come from NRL's Sound Division and was acquainted with the Laboratory's work on radar.[7]

[2]Information for this section has been taken from several sources: Dulany Terrett, *The Signal Corps: The Emergency* (Washington: GPO, 1956); Henry Guerlac, *Radar in the World War II* (unpublished history of Division 14 of the National Defense Research Committee, 1947), ch. IV; John B. McKinney, "Radar: A Case History of an Invention" (unpublished term paper for the Harvard Business School, 1961); Harry M. Davis, "History of the Signal Corps Development of U.S. Army Radar Equipment," Part I (unpublished manuscript available at the U.S. Army Center of Military History, Washington, D.C.); Roger B. Colton, "Radar in the United States Army," *Proceedings of the Institute of Radio Engineers* **33** (1945): 740-753; and Arthur L. Vieweger, "Radar in the Signal Corps," *Transactions of the Institute of Radio Engineers* **1-MIL** (1960): 555-561.
[3]McKinney , *op. cit.* (note 2), pp. 75-79.
[4]Terrett, *op. cit.* (note 2), p. 40.
[5]McKinney, *op. cit.* (note 2), p. 102.
[6]Note 15 in Chapter 5. The quote is given in Davis, *op. cit.* (note 2), p. 22.
[7]Guerlac, *op. cit.* (note 2), p. 122.

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From 1932 to 1936, almost all the work the Signal Corps did on radio detection was concentrated on designing microwave equipment that used the continuous-wave method. Institutional support for the effort was minimal, but around five men were able to participate in it part time. Like researchers at NRL and in industry, they found that their main problem was generating microwaves with sufficient power. In 1934, as was mentioned,[8] joint experiments were made at Atlantic Highlands with Dr. Irving Wolff and his group from RCA, but, as always, low power limited the reflections to short distances.[9] The experiments pointed only to more research, not rapid practical development.

New ideas had to be sought and investigated. The thought of using pulses instead of continuous waves occurred to Hershberger as early as 1933,[10] but initially he conceived of making pulse lengths and intervals of silence in between them of equal magnitude—an idea that is not well suited to radio detection. Indeed, his initial attempts to employ this method failed. Major Blair, writing in the Annual Report of the Laboratories in July 1934, also proposed the pulse idea:

> It appears that a new approach to the [radio detection] problem is essential. Consideration is now being given to the scheme for projecting an interrupted sequence of trains of oscillations against the target and attempting to detect the echoes during the interstices between the projections. No apparatus for this purpose has yet been built.[11]

Despite this official announcement, no such equipment would be tried for almost 2 more years.

In early 1936, Hershberger visited both the National Bureau of Standards and NRL to gather information that might be useful in radio detection. At NRL he witnessed a test of continuous-wave detection equipment and also learned of the investigation Page was making on the pulse method. He made detailed reports on the work. Soon, in an unrelated development, his project was given a new boost of support. The Chief of Coast Artillery wrote to the Signal Corps Laboratories, "It is desired that the development of [both heat and radio] detectors be given the highest priority practicable, with particular emphasis on the detection of aircraft."[12] This vote of confidence brought no additional financial support, but pressure was obviously building to push the project along. It forced consideration of the new technical ideas. Sometime later in the spring of 1936, Hershberger and Robert H. Noyes began systematic work on their first pulse radar device.[13]

Throughout this early period, information about NRL's radar work had been routed to the Signal Corps in the Confidential monthly reports the Laboratory made to the Bureau of Engineering. They provided at least one means of cooperation. But in June 1936, the NRL project was reclassified Secret due to the success Page and Guthrie had achieved with their 28-megahertz pulse radar set.[14] The reports abruptly discontinued any further mention of radio detection. Page later said of the effect,

> Hershberger at the Signal Corps Laboratories, of course, had been following those reports avidly. He said for months we had some statement about progress of the work. All of a sudden it disappeared—never showed up in the report again. He said to him that indicated that

[8] In Chapter 7 between where notes 36 and 38 apply.
[9] Colton, *op. cit.* (note 2), p. 742.
[10] Guerlac, *op. cit.* (note 2), p. 124.
[11] As quoted in Colton, *op. cit.* (note 2), p. 742.
[12] Davis, *op. cit.* (note 2), p. 32.
[13] Guerlac, *op. cit.* (note 2), p. 128.
[14] Note 43 in Chapter 6.

> we hit pay dirt. It had come through and we had quit talking about it—slapped secrecy on it. He was right.[15]

From this time until around late 1940, it appears that there was little further communication between the Army and Navy laboratories on radar.[16] With projects veiled in secrecy, each service had chosen to go its own way.

In July 1936, the Signal Corps detection project finally was given strong financial support, with a direct allocation of $80,000. This came not from new funding but from redistribution of the regular annual appropriation. Petitions for increased expenditures had failed. Although a directive from the Office of the Secretary of War had admitted that "the development of an efficient means of detecting the approach of aircraft is considered of such vital importance to all branches of the Army that it is considered essential to place it in the highest priority,"[17] it had gone on to order that money come from cutting back in other projects.

By the fall of 1936, Hershberger and his colleagues had assembled their first pulse detection equipment. They had also, by this time, made a crucial technical decision: to abandon the use of microwaves and concentrate instead on short waves—around 100 to 200 megahertz in frequency (Table 3). Only in this way, they had learned, could enough power be generated for a practical device. In October, Hershberger left the Laboratories to complete his doctorate in physics, but the project continued. By December, test equipment had been constructed that could track aircraft to distances of up to 11 kilometers (7 miles). Effort was then directed toward building a practical prototype. By May 1937, pulse equipment had been developed to a sufficient state to warrant major demonstration. On May 18 and 19, it was shown to the Chief Signal Officer, Major General James B. Allison; the Chief of Coast Artillery, Major General Archibald H. Sunderland; and the Assistant Chief of the Air Corps, Brigadier General Henry H. Arnold. On May 26, the Secretary of War, Mr. Harry Woodring, saw it in action. In the tests, both a 100-megahertz and a 240-megahertz transmitter were used. Aircraft were detected to distances of up to 18 kilometers (11 miles).[18]

These demonstrations brought further administrative support and action. Authorization was now given to establish a Radio Position Finding Section, with the Laboratories being encouraged to develop production equipment as rapidly as possible. Over the next 3-1/2 years, engineers were able to develop sets to meet several different needs: the SCR-268, for searchlight and gun direction, and the SCR-270 and SCR-271 for mobile and fixed long-range detection. The 268, operating on a frequency of 205 megahertz, was effective to ranges up to around 37 kilometers (23 miles). The 270 and 271, relying on 110-megahertz signals, were reliable at distances up to 190 kilometers (120 miles) on large aircraft but somewhat less for smaller aircraft. Contracts for procurement of all three types were in effect by August 1940. The final design specifications had been worked out jointly by the Signal Corps Laboratories and private electronic companies, most notably Western Electric and Westinghouse. The designs were the major accomplishment of the Signal Corps in the radar field until the end of 1940, and sets based on them would be used extensively throughout World War II.[19]

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[15] Transcript of an interview with Dr. Robert M. Page, Historian's office, NRL, Washington, D.C., pp. 80 and 81.

[16] Robert Page, when reviewing this manuscript, disagreed. He wrote, "The statement that there was little communication between Army and Navy laboratories on radar between 1936 and 1940 is misleading. The records do not show the extent to which Signal Corps success in radar development resulted from knowledge of what NRL had accomplished and how NRL had done it. For example, the use of 28 and 200 megacycles instead of microwaves, the elimination of receiver blocking and ringing, the low *average* power transmitter power supply, the all-metal curtain array antenna, the duplexer, and the ring circuit oscillator all were transferred directly or indirectly from NRL to the Signal Corps." Letter from Robert Page to David Allison, Mar. 20, 1980, in the Historian's files, NRL, Washington, D.C. Unfortunately, official records neither confirm nor disprove this interaction.

[17] As quoted in Davis, *op. cit.* (note 2), p. 33.

[18] Colton, *op. cit.* (note 2), pp. 743 and 744.

[19] *Ibid.*, pp. 744-753.

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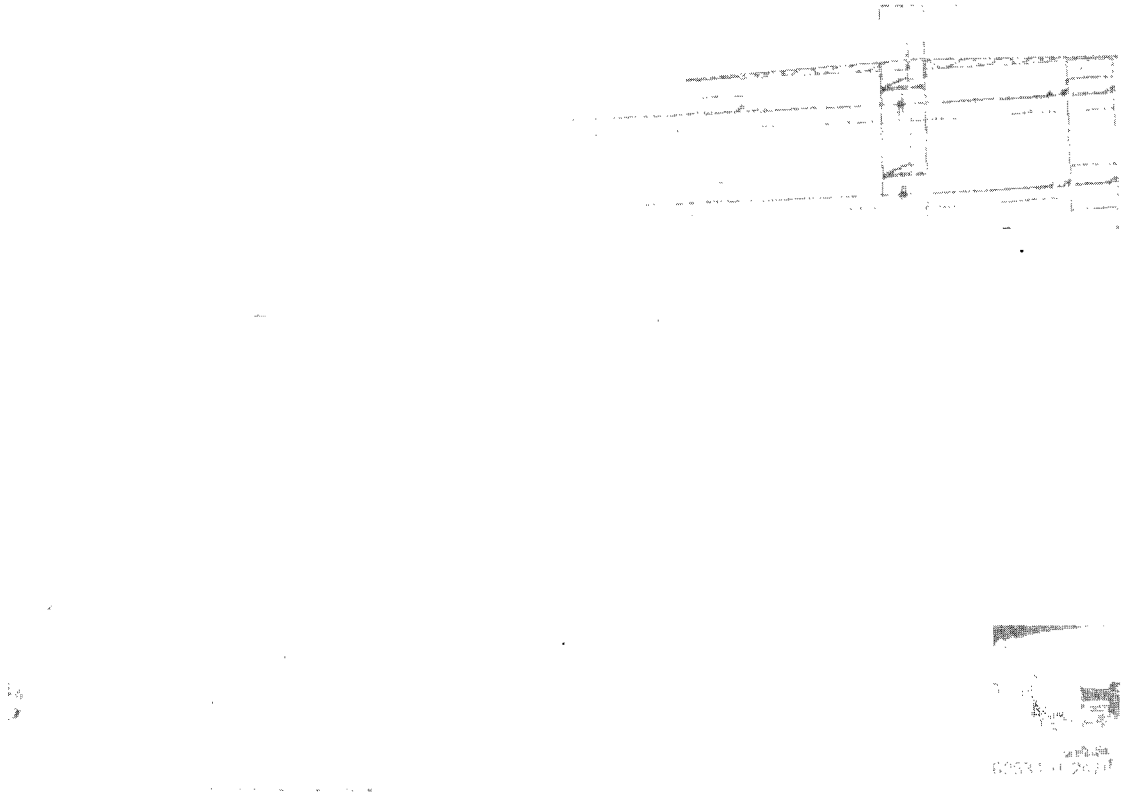

Fig. 20 — The SCR-270 radar, which operated at a frequency of 110 MHz, was the Army's first long-range detection set.

In summary, the Army and Navy radar programs had sprung from similar needs and similar sources. Yet, as development progressed, they had become largely separate. Each service knew something about the other's work, but there had been no effort to integrate development or production. As the nation approached involvement in World War II, each service planned to manage its own radar program independently.

The way radar was developed in Great Britain was strikingly different from the way it evolved in the United States.[20] Radar began with administrative action at the top of the military rather than technical discoveries at the bottom. It began as a definite solution to a pressing problem—Adolph Hitler's bombers across the English Channel—rather than as a vague answer to uncertain threats. It was guided by definite commitment and strong financial support rather than by the politics of limited means. It was created in a special organization established solely for developing it rather than being one of many competing projects in an existing government laboratory. Research on radar started later than in the United States, but it progressed more rapidly and equipment was put into widespread operational use much sooner. Ready when needed, it played a key role in defeating the German Air Force during the Battle of Britain.

English radar development began in June 1934 when A. P. Rowe, a member of the staff of the Director of Scientific Research in the Air Ministry, started thinking seriously about how well British air defense would respond to a German air attack. In his words,

[20]Principal sources for this section were: Robert Watson-Watt, *The Pulse of Radar* (New York: Dial, 1959); A.P. Rowe, *One Story of Radar* (Cambridge, England: University Press, 1948); Henry Guerlac, *op. cit.* (note 2), ch. V; and Ronald Clark, *Tizard* (Cambridge, Mass.: MIT Press, 1965).

> I undertook an informal survey of the problem of air defence and to this end collected every available file on the subject; there were fifty-three of them. It was clear that the Air Staff had given conscientious thought and effort to the design of fighter aircraft, to methods of using them without early warning and to balloon defences. It was also clear, however, that little or no effort had been made to call on science to find a way out. I therefore wrote a memorandum summarizing the unhappy position and proposing that the Director of Scientific Research should tell the Secretary of State for Air of the dangers ahead. The memorandum said that unless science evolved some new method of aiding air defence, we were likely to lose the next war if it started within ten years. Unfortunately, I was not clever enough to think of a new method.[21]

The Director of Scientific Research at the time, Dr. H. E. Wimperis, agreed fully with Rowe's evaluation of the situation. Throughout the summer, an answer was sought in the Air Ministry, but none was found. In November, Wimperis looked outside. He established a Committee for the Scientific Survey of Air Defence under the chairmanship of Henry T. Tizard. Other members were P. M. S. Blackett, A. V. Hill, Wimperis, and Rowe, as secretary. Only the last two were from the Air Ministry.[22] Tizard took firm control, and the committee soon became known simply as the "Tizard Committee." Throughout the years of preparation for war, the drive and inspiration of this man would motivate radar researchers and the military men who would use their products. His abilities are even more astonishing in view of the fact that he always acted only as an advisor—never as an official with line authority.[23]

While final preparations for organizing the Tizard Committee were being made, Wimperis spoke to Robert Watson-Watt, Superintendent of the Radio Department of the National Physical Laboratory, asking him to evaluate the possibilities of using a "death ray"—some form of beamed electromagnetic energy—either to destroy aircraft in flight or, that being impossible, to wreak severe physiological damage on the pilot. Indeed, Wimperis's desire to have this idea considered seriously had been one of the main reasons for creating the Tizard Committee.[24] Watson-Watt, while not at all sanguine about the prospects, promised to analyze the idea quantitatively. With the technical assistance of one of his aides, A. F. Wilkins, he soon composed and sent to Wimperis a memorandum on the subject. He later summarized,

> My memorandum showed that we could not hope that an aircraft would linger so long, in the most intense beam of radio energy we could produce, as to raise the pilot's body temperature to an artificial fever level, to interfere effectively with the working of the engine, or to weaken the aircraft structure itself.[25]

Thus the death ray was out. But the concluding paragraph of the memorandum suggested another plan:

> Meanwhile attention is being turned to the still difficult but less unpromising problem of radio-detection as opposed to radio-destruction and numerical considerations on the method of detection by reflected radio waves will be submitted when required.[26]

[21] Rowe, *op. cit.* (note 20), p. 5.
[22] *Ibid.*
[23] Clark, *op. cit.* (note 20).
[24] *Ibid.*, pp. 110 and 111.
[25] Watson-Watt, *op. cit.* (note 20), p. 52.
[26] *Ibid.*, p. 53.

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When this document was read at the first meeting of the Tizard Committee, which was held on January 28, 1935, the members became enthusiastic about radio detection. Wimperis related that he had already asked Watson-Watt to draw up a more complete treatment of the idea. On February 14, this second memorandum, entitled "Detection and Location of Aircraft by Radio Means," was discussed over lunch at the Athenaeum Club by Tizard, Wimperis, Sir Christopher Bullock, and Watson-Watt.[27]

The paper began by considering whether planes could be detected by measuring the energy they radiated in flight: sound, heat, light, and radio communications. These were of no use, concluded Watson-Watt, again depending largely on Wilkins' technical expertise. They could be shielded or muffled, and they were not propagated readily or rapidly in all atmospheric conditions. For reliability, the aircraft would have to be "illuminated" with radio energy under one's own control and detected on the basis of reflections. The transmitted wave employed could, according to the calculations, be made strong enough to obtain echoes from targets at significant distances. For a method of transmission, radio pulses were suggested. Watson-Watt and Wilkins were intimately familiar with ionospheric ranging equipment, and they believed it might be adapted to serve the function. The memorandum stated,

> If now the sender emits its energy in very brief pulses, equally spaced in time as in the present technique of echo-sounding of the ionosphere, the distance between craft and sender may be measured directly by observation on a cathode-ray oscillograph directly calibrated with a linear distance scale, the whole technique already being worked out for ionospheric work at the Radio Research Station.[28]

This idea was supported with numerical examples. Thought had been given to the appropriate frequencies and pulse length, although it was realized that final determinations would have to await further research. If, on the other hand, the pulse method did not work, it was suggested that a frequency-modulated continuous-wave technique might be employed instead. Finally, means were proposed for distinguishing friendly from enemy aircraft using an electronic device triggered by the detection equipment. Thus, without the benefit of a single experiment, Watson-Watt and Wilkins provided in this one amazing document both qualitative and quantitative arguments for the development of pulse radar, continuous-wave radar, and associated airplane identification equipment. Their colleagues on the Tizard Committee were suitably impressed and strongly convinced that the ideas should be put to test. On the following day, Wimperis began soliciting funds from the Air Ministry. The radar project in Great Britain was underway.[29]

In this context, it is interesting that in the United States, only a few months later, Congressman James Scrugham of the House Appropriations Subcommittee tried to push the U.S. Navy into investigating the "death ray" idea. During hearings on the Navy Department budget for fiscal 1936, he questioned Admiral S. M. Robinson, Chief of the Bureau of Engineering:

> Admiral...I am informed that experiments have been made, notably in Germany, whereby an automobile has been stopped by radio or waves of somewhat similar character; that is, energy waves. Now, if it is possible to stop an automobile it is possible to stop an airplane, and theoretically it is possible to stop a battleship; is it not?[30]

Robinson responded that he did not think this was possible. Shielding of a battleship engine was too effective. Scrugham, however, pressed the matter. Was the Navy doing anything on this subject?

[27] Clark, *op. cit.* (note 20), pp. 116-118.
[28] Watson-Watt, *op. cit.* (note 20), pp. 429 and 430.
[29] Clark, *op. cit.* (note 20), p. 118.
[30] U.S. Congress, 74:1, House, *Hearings Before the Subcommittee of the House on Appropriations in Charge of the Navy Department Appropriation Bill for 1936* (Washington: GPO, 1935), p. 490.

Robinson hedged—yes, at least in the same general subject area. But on this particular matter? The Admiral had to say it was not. Scrugham continued:

> *Mr. Scrugham*: I may give it an undue importance; but I am of the opinion that a considerable sum of money...could well be put on the development of these fields of activity; and I would like to know just how far you are going into this subject; and if it is regarded as a thing that is sufficiently important to put money into for experimental work.[31]

Robinson responded by saying that he thought it best to see if any research already underway gave indications that this idea might be practical. He did not think it wise to initiate a new study. Consequently, consideration of the death-ray matter was cut short and, in the U.S. Navy, played no part whatever in the development of radar. Thus does the force of the same technical ideas differ markedly in different historical situations.

Once the Tizard Committee had accepted the idea for radar, an experiment was arranged at Daventry on February 26 to examine its feasibility. A. P. Rowe has given probably the most honest description of the event:

> Graphic accounts have been written of this demonstration, of how senior officers from the fighting Services went to Daventry on that great day; how for the first time the position of an aircraft was obtained by radar and how success was hailed with congratulations from the distinguished on-lookers. In fact, none of these things happened. Though there was not a demonstration of the location of an aircraft, what happened was significant enough. Overnight one of Watson-Watt's assistants, A. F. Wilkins, had erected equipment in a van near Daventry. All that was hoped of this equipment was that it would show that an aircraft, when in the Daventry beam, would reflect enough of the beam's energy for its presence somewhere in the vicinity to be inferred from observations in the van. This is just what happened on 26 February 1935. So far from the demonstration being witnessed by distinguished officers from the Services, the sole representative from the Service departments was one humble civilian scientific worker—myself. Watson-Watt and I were pleased with the demonstration, since reflections from the aircraft were obtained when it was estimated to be about eight miles away, but we knew that we had not seen the location of an aircraft by radio.[32]

The test at Daventry was similar in intent and in outcome to Robert Page's test at NRL several months earlier, in December 1934,[33] but the response was altogether different. To the Tizard Committee, the trial was proof enough that the method was worthy of strong backing. As Rowe later remarked, "From then on there was no hesitation on the part of the Committee."[34] Perhaps more astonishing is that, over the coming months, approval of ever-increasing sums of money would be obtained from the Government without much difficulty. Rowe commented,

> Those were the great days. There was not always to be unanimity between the scientist, the Service user and the men with the money,

[31] Ibid., p. 491.
[32] Rowe, *op. cit.* (note 20), p. 8.
[33] See the Introduction.
[34] Rowe, *op. cit.* (note 20), p. 8.

UNCLASSIFIED

> but in 1935 the almost traditional obstructiveness which scientific men are supposed to meet was absent.[35]

One particular reason the project received such favorable treatment was that, in the spring of 1935, the Government had established a new organization on the cabinet level: the Committee of Imperial Defence. This body, charged with strengthening Britain's military, soon subsumed the Tizard Committee and became a major source for political and financial support of its radar program.[36]

To develop radar from an idea into reality, the Tizard Committee recommended establishment of a new research station instead of proposing that work be done in an existing institution. The decision to accept this recommendation would prove to be extremely wise, but it was made largely by chance. Watson-Watt had recommended that the research be conducted at the National Physical Laboratory—the facility he headed. His colleagues there, however, objected: radio detection was applied research—they were devoted to pure science.[37] Thus the experimentation was, by default, set up at Orford Ness, a Government-owned island just off the East Coast of England. Described variously as "one of the loveliest places in the world,"[38] and "dismal, windswept, and uninhabited...except for a few dilapidated hangars from the last war, a deserted waste,"[39] this was to be the location of the exciting early phases of development. A small group of about three or four men, handpicked by Watson-Watt and Rowe, went there to begin work in mid-May 1935.

Effort immediately focused on producing practical equipment. The principal aim was the design and fabrication of early-warning systems to be positioned along the English coast. In seeking this goal, the men were unconstrained by the rigid requirements concerning weight, size, and ruggedness of equipment that faced the engineers at NRL. Thus they were able to move toward practicality more rapidly than had Page and Guthrie. Besides, Watson-Watt, who was in charge of the project, was by nature a compromiser. He later described his approach to research and development as being "the Cult of the Imperfect," and epitomized it in a slogan: "Give them the third best to go on with; the second best comes too late, the best never comes."[40]

By June 17, 1935, equipment had been built capable of tracking a plane to 47 kilometers (29 miles); in July, 55 kilometers (34 miles); in September, 90 kilometers (56 miles). Work had begun on 6 megahertz—the standard frequency for ionospheric measurements—but by midsummer was on frequencies up to twice that. In the fall, detailed planning had begun for a chain of early-warning stations.

It now became clear that the group would soon outgrow its quarters, so a move was made to an isolated, 1-square-kilometer (250-acre) estate about 32 kilometers (20 miles) to the south: Bawdsey Manor. The Government purchased the land and the large ornate home standing on it. If the radar researchers had previously held any doubt about the commitment to them, there was none now. Watson-Watt, who became the full-time director, ran the institution in the style of a university rather than a Government facility. Hours were long but irregular; many of the employees—mostly young bachelors—had quarters in the manor house, which also was being used as the main laboratory facility. Meals were in common, and discussions of work intertwined with leisure activities. By late 1936, the work force had grown to around 50; within several years it would number 10 times as many.[41]

The first radar warning station was erected at Bawdsey Manor. It was in operation in experimental form by March 1936, sending signals on a frequency of 11.5 megahertz and using separate transmitting

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[35] *Ibid.*, p. 19.
[36] *Ibid.*, p. 18; Clark, *op. cit.* (note 20), pp. 121-129.
[37] Tape-recorded interview with Dr. Edward G. Bowen, Historian's office, NRL, Washington, D.C., side 1.
[38] Rowe, *op. cit.* (note 20), p. 13.
[39] Guerlac, *op. cit.* (note 20), p. 175.
[40] Watson-Watt, *op. cit.* (note 20), p. 46.
[41] Guerlac, *op. cit.* (note 20), pp. 185-191.

and receiving antennas—both mounted on towers around 12 meters (40 feet) high.[42] Within a year, the frequency had been jumped to 23 megahertz, and the height of the towers had been jumped to 70 meters (240 feet) for the receiving antenna and 110 meters (360 feet) for the transmitting antenna. Both antennas were stationery. Obviously, this was equipment designed to fill a specific, land-based function. The British, like the Americans, would have difficulty trying to push to higher frequencies and to reduce antenna size and equipment weight.

In May 1937, the Bawdsey station was complete and was handed over to the Royal Air Force for operation. Within another year, similar installations were erected at four other locations, and continual watch was being kept on air traffic to and from the country. By the spring of 1939, the number had grown to 20 stations, and coverage extended to the entire east coast.

This chain was the principal achievement of the British in radar by the time war began in 1939. But the network and the improvements that were made to it, such as the addition of 200-megahertz equipment to detect low-flying airplanes, were not the only British successes in the radar field and, from the point of view of the Americans, would not be the most revolutionary. Although amazed by the extent and effectiveness of the early-warning system, U.S. engineers would acquire more technical knowledge from other advances, such as the development of airborne radar.

Like so many innovations, the idea of airborne radar came from the fertile mind of Henry Tizard. Dr. E. G. Bowen, the man principally responsible for realizing it, remembered Tizard's role in this way:

> Tizard was the first to see that [in a war with Germany] there would be a day [air] battle, that [the Germans] would be beaten back, and that [they] would turn to night attack. And he posed the question, "What do we do about night attack?" And in [a] typical Tizard line of reasoning, he said, "From ground radar you can't put the fighter close enough to the bomber to see him; you've got to put a radar in the aircraft." And that was the logic that led him very early—and this is as early as 1935—to point out the need for airborne radar.[43]

Watson-Watt had also been mulling over the concept, although from a technical standpoint rather than an operational one. It was first discussed in the Tizard Committee in February 1936.[44] Soon, Bowen, who had been with the radar project since its inception at Orford Ness, was assigned to begin working out the technical requirements for the equipment. He recalled,

> I was the airborne *group* with a mandate...to try and reduce the size and weight of a complete transmitting and receiving equipment to a point where it could go into an aircraft. [This] in many ways was ridiculous, because the typical receiver of that day filled a room 20 feet by 10 feet; the antennas were then 200 feet high on the top of masts and 40 feet long, and the transmitter weighed about 10 tons with God knows how many kilowatts to run it. So when you think about it now in the cold, silver light of day, it was a ridiculous concept....But we were cocky.[45]

Other problems besides size and weight faced Bowen and the small staff he soon began to accumulate: The frequency to be used in any equipment had to be higher than that of the ground based stations so that antennas could fit on the planes. (Eventually 200 megahertz was chosen, the same frequency as was being used at NRL in the XAF development.) Power on airplanes was quite limited and

[42] *Ibid.*, p. 188.
[43] Bowen interview (note 37), side 1.
[44] Clark, *op. cit.* (note 20), p. 158.
[45] Bowen interview (note 37), side 1.

UNCLASSIFIED

had to be shared with other equipment. Vibration was both extreme and constant. Readings from the set had to be simple enough so that either a pilot or a copilot, each of whom had many other things to do, could understand them readily. Hence, progress was slow.

The work focused on building two forms of equipment: an air interception (AI) radar for locating enemy aircraft in flight, and an air-to-surface-vessel (ASV) radar for locating enemy ships. The technical requirements of both were closely related; thus research was done on them simultaneously. Test ASV equipment was in operation as early as the spring of 1937.[46] In a dramatic event about a year and a half later, a modification of this equipment successfully located the British fleet in an exercise when all other planes not equipped with radar failed to do so.[47] But, despite these successes, production equipment was not immediately forthcoming. No ASV sets were in use when the war started.[48] Some were installed during the early months, but satisfactory operational equipment did not begin to be employed until September 1940.[49] AI radar fared little better. Six sets of a production run of 30 were in aircraft when the war began; all 30 were in aircraft by the end of September 1939.[50] But they, and those that followed soon after, generally proved ineffectual. Not until the AI Mark IV made its appearance in the autumn of 1940 was there a good airborne radar for night fighting.[51] Because of the few sets in operation and the lack of training in their use, airborne radar played a much smaller role in turning back the initial airborne assault against England than did ground-based equipment. Only in the summer of 1941 did airborne radar begin to prove significantly useful. Nonetheless the overall airborne-radar program was a striking technical achievement; nothing like it was being done in the United States. If success had not been rapid, it was profound. Airborne equipment of British design would ultimately be produced by the thousands in America as well as in the United Kingdom.

Radar development in the other two services, the army and the navy, lagged behind that of the Air Ministry but still was significant. Stimulated by disclosures from the Air Ministry concerning Watson-Watt's work, a naval radar program began in October 1935, at the Admiralty Signal School in Portsmouth. The first development models, using a frequency of 43 megahertz, were ready by 1938 and were installed in August and September on HMS *Sheffield* and HMS *Rodney* for testing. Results were good, and researchers pushed ahead. By October 1940, type 281 radars, using a frequency of 100 megahertz, had been designed and ordered in quantity. Delivery began in February 1941. Although these early production equipments used lower frequencies than those that had been designed at NRL during the same years, developments had also been started at 600 megahertz as early as 1938, and sets were built at this and much higher frequencies during the war, to satisfy the need for fire control and precision search.[52] The army, unlike the navy, had funded a research program at Bawdsey, and concentrated effort there on fire-control equipment for antiaircraft guns. In short, both of the other services were following the lead of the Air Ministry and were developing radar equipment to meet their own special needs.

All of the initial work on radar in Great Britain, and all of the first operational equipment, depended on high frequencies—not microwaves (Table 3, in Chapter 4). But, as indicated by the navy's program, the British were strongly aware of the advantage of moving up in frequency. This was particularly necessary for progress in airborne radar. As long as 200 megahertz was used, ground echoes limited the maximum range an aircraft interception radar could obtain: to get a range greater than one mile, for example, a plane had to be higher than one mile or the reflection from the earth would block out the target signal. The only solution was to go to microwaves, for only they could be

[46]Guerlac, *op. cit.* (note 20), pp. 194 and 195.
[47]Bowen interview (note 37), side 2.
[48]Rowe, *op. cit.* (note 20), p. 46.
[49]Watson-Watt, *op. cit.* (note 20), p. 127.
[50]Guerlac, *op. cit.* (note 20), pp. 200 and 201.
[51]*Ibid.*, p.214.
[52]Bowen interview (note 37), side 4. J.D.S. Rawlinson, "Development of Radar for the Royal Navy," *Naval Electrical Review* (July 1975): pp. 51-57.

focused well enough to prevent the ground reflection. Microwaves would also provide for better target discrimination. Calculations showed that a wavelength of about 10 centimeters would be ideal.[53]

Consequently, numerous studies of microwaves were commissioned in the hope that they would yield means for generating radiations with sufficient power for radar. One study, supported by the Admiralty, was established under the direction of M. L. Oliphant at the University of Birmingham in September 1939. Initially, work focused on improving the best existing transmission tubes. But this did not appear promising, and, besides, it was being tried in many other places. Thus two researchers in Oliphant's group, J. T. Randall and H. A. H. Boot, started over. In a single afternoon in November 1939, they came up with the answer: a copper resonator of a radical new design. Both men were new to the field and were largely ignorant of the wealth of existing literature on designs for microwave transmission tubes. Thus they had returned to first principles in their reasoning, and it appears that this, more than any other factor, allowed them to succeed where others had failed.[54]

The device they sketched that day was, in its final form, a cylinder of copper with a large central hole drilled out. Parallel to this hole was a ring of much smaller ones, all symmetrically spaced and all with centers on a circle concentric with the center hole. Connecting each of the smaller holes to the central cavity was a narrow slit. The entire configuration served as the anode of the resonator. The cathode was a large, oxide-coated spiral of wires running down the middle of the central cavity. The name chosen was descriptive of the appearance: "multicavity magnetron." The first model was tried on February 21, 1940. Within a day, Randall and Boot were able to generate 400 watts of power at a 9.8-centimeter wavelength,[55] a remarkable performance—far better than any other available tube. Test and development continued rapidly, and, by September 1940, the first production models were becoming available. They would prove to be the key to making microwave radar possible. One of them would soon be displayed to radar experts in the United States as a prime exhibit in a general exchange of technical information between the two nations, an exchange that was the beginning of extensive Anglo-American cooperation in technical development during the war.

Thus the main points of comparison and contrast of early British radar work with the United States programs are clear. Both nations possessed the technical expertise to develop radar, and both did so independently. Yet the histories of their efforts were quite distinctive because of the different circumstances in which they progressed. The differences would first become apparent during the exchange of technical information, and the knowledge of them would radically affect the policy and organization of American research and development in radar.

THE TIZARD MISSION

Soon after Britain entered the second World War, in September 1939, Henry Tizard became convinced that, in the area of technical production at least, British needed the help of the United States. As early as November, he began suggesting official collaboration between the technical communities in the two countries. As a first step, he proposed that A. V. Hill, a Nobel laureate, a joint secretary of the Royal Society, and a member of his committee on air defense, be sent to America as a special advisor charged with learning the views about the idea among top scientific leaders. In England, this plan encountered strong opposition. There was fear that America would leak secrets to Germany, fear that Britain would have to give up much to gain little, and fear that the United States would not cooperate wholeheartedly after reaping the benefit of British knowledge. Also militating against the scheme was a strong sense of pride and self-sufficiency. As Dr. E. G. Bowen, a close friend of Tizard, remembered,

[53] *Ibid.*, side 5; Rowe, *op. cit.* (note 37), pp. 76 and 77.
[54] Guerlac, *op. cit.* (note 20), pp. 291-295.
[55] *Ibid.*, p.297.

UNCLASSIFIED

> The first reactions to this [idea] were not good. Churchill, who was certainly being groomed for the position of Prime Minister at that time and was, of course, Chief of Naval Staff, was doubtful....Watson-Watt was definitely hostile.[56]

Tizard himself noted of his discussions with Britain's leading radar man,

> [Watson-Watt] maintained that the Americans could not teach us anything, and that we should get much the worst of the bargain, that there was nothing in the production argument, and that by the end of the year our facilities for production would be greater than theirs. I said that if so it would be the first time in history that this had happened.[57]

Despite these reactions, Tizard kept pushing and, in the spring of 1940, got approval for the exploratory mission. A.V. Hill became a scientific attaché to the British Embassy, visited numerous American universities and industrial firms, and got a warm reception everywhere. In April, he recommended to the British Ambassador, Lord Lothian, that a broad exchange of technical information, especially information on radar, be undertaken.[58] Resistance in England was still strong, but events were developing to change it: in May, the German army began its rapid conquest through the Low Countries and France. On May 18, the British military chiefs gave their support to the plan. Hesitance remained elsewhere, but pressure in favor of the exchange increased over the next few weeks. On June 28, Lord Lothian informed Churchill, now Prime Minister, that President Roosevelt had been told of the idea and was favorably inclined toward it. Lothian suggested that the British act immediately. Finally, on July 6, he was ordered to go ahead. Thus, on July 8, he sent a secret *aide-memoire* to the President for consideration. The key concept it advanced, based on a suggestion by Hill,[59] was that Britain would not attempt to trade secret for secret, but rather would be entirely open in hopes that this would lead to the maximum benefit for both nations. The document read in part,

> The British Government have informed me that they would greatly appreciate an immediate and general interchange of secret technical information with the United States, particularly in the ultra short wave radio field.
>
> It is not the wish of His Majesty's Government to make this proposal the subject of a bargain of any description. Rather do they wish, in order to show their readiness for the fullest cooperation, to be perfectly open with you and to give you full details of any equipment or devices in which you are interested without in any way pressing you beforehand to give specific undertakings on our side, although of course they would hope you would reciprocate by discussing certain secret information of a technical nature which they are anxious to have urgently....
>
> As to subsequent procedure, should you approve the exchange of information, it has been suggested by my Government that, in order to avoid any risk of the exchange of information, in order to avoid any risk of the information reaching our enemy, a small secret British mission consisting of two or three service officers and civilian scientists should be dispatched immediately to this country to enter into discussions with Army and Navy experts. This mission should, I suggest,

[56]Bowen interview (note 37), side 4.
[57]As quoted in Clark, *op. cit.* (note 20), p. 252.
[58]*Ibid.*, p. 250.
[59]*Ibid.*, p. 254.

> bring with them full details of all new technical developments, especially in the radio field, which have been successfully used or experimented with during the last nine months. These might include our method of detecting the approach of enemy aircraft at considerable distances, which has proved so successful; the use of short waves to enable our own aircraft to identify enemy aircraft, and the application of such short waves to anti-aircraft gunnery for firing at aircraft which are concealed by clouds or darkness. We, for our part, are probably more anxious to be permitted to employ the full resources of the radio industry in this country with a view to obtaining the greatest power possible for the emission of ultra short waves than anything else.[60]

The *aide-memoire* was discussed at the Cabinet meeting on July 11 and endorsed by President Roosevelt and the Secretaries of Navy and of War.[61] Final approval in Britain came on July 25.[62] Thus was laid the cornerstone of scientific cooperation.

Tizard became head of the mission and soon began organizing its membership. In addition to him, it included three military representatives and three civilians. Two of the latter, J.D. Cockcroft and E.G. Bowen, were specialists in radar. Extensive preparations were made for the interchange in both Britain and the United States, for it was to include all fields of scientific and technical activity pertinent to the war effort.

Among the American military, the idea generally received strong support, although there was less willingness than in Britain to disclose everything. The highly secret Norden bombsight, for example, along with numerous devices still under development, were withheld.[63] And, at least in some quarters, there was doubt that the British would have much of value—even in the field of radar. Admiral Bowen, for instance, wrote the Chief of Naval Operations on July 26,

> Prior to 1939 the British were making determined efforts to trade British submarine detection devices (underwater sound) for our radio airplane detectors. They were instructed by the Navy Department to take the matter up through diplomatic channels. About the same time, extravagant claims were made by British civilian engineers in regard to the efficacy of the British equipment for detecting submarines (underwater sound).
>
> If the British during the past year have succeeded in working out the details of airplane detection by radio to the extent commonly believed in our Navy Department, they have indeed effected a remarkable achievement. It seems more likely, however, that the British have radio direction finder stations so located and interconnected with central stations that they are plotting the position of aircraft by intersecting circles....
>
> The information and equipment which we expect to get from the British in regard to radio airplane detection and submarine detection may

[60] *Aide-memoire* from Lord Lothian to President Roosevelt, July 8, 1940, copy in file A8-3/EF 13, July-Aug. 1940, records of the Secretary of the Navy/Chief of Naval Operations, Confidential series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.

[61] Memorandum for the Army Chief of Staff from Brig. Gen. V. Strong, July 19, 1940, in file A8-3/EF 13, July-Aug. 1940 (note 60).

[62] Clark, *op. cit.* (note 20), p. 256.

[63] Discussed in various documents, file A8-3/EF 13 (note 60).

UNCLASSIFIED

> possibly prove to be disappointing. For a nation that has been so backward in mechanical and electrical engineering as applied to Naval vessels to have established outstanding progress in two other fields of engineering does not seem credible although it may be possible.[64]

Hauteur, obviously, was not restricted to the mother country.

Tizard came to the United States by way of Canada, where he discussed British work with Canadian scientists to enlist their aid.[65] He arrived in Washington on August 26, 1940. The other members of the mission, who were traveling by boat, would not come in until early September. They were bringing blueprints, diagrams, technical documents, films of radar equipment in operation, and samples of various components. The most important item was the multicavity magnetron. It was still so new that it had not yet been incorporated in any operational equipment. Indeed the tube that came to the United States was one of the first 12 to be manufactured.[66]

Before the rest of the mission arrived, Tizard and other representatives of the British Government laid the foundation for the upcoming detailed technical discussions by making preliminary disclosures. A conference for this purpose was held with top NRL personnel on August 30. At that time, Admiral Bowen, Briscoe, Taylor, Young, and Ross Gunn got their first general knowledge of British radar.[67] In the first official meeting of the mission with service representatives, on September 10 at the Bureau of Engineering, the main subject, once again, was radar. Tizard presided over a discussion of both technical details and operational use.[68] The latter may have been more important at this stage, for, as E. G. Bowen recalled,

> The big impact of the Tizard Mission as I saw it at [the] time and see it now, was in persuading the top brass—the admirals and the generals—that here was an operational tool that they had to have. That was lacking, and that was one of the first things the Tizard Mission did.[69]

On September 12, four members of the mission, including the radar experts Cockcroft and Bowen, went to NRL to see and learn about the U.S. Navy's radar equipment and to discuss their own in greater depth. There were demonstrations, frank and open discussions, and an exchange of thick folders of technical data.[70] Over the coming weeks, NRL engineers met frequently with the British, and both sides shared all aspects of their knowledge, particularly in radar but also in other technical fields such as underwater sound ranging and chemistry. The multicavity magnetron was discussed in detail on September 17 but was not put into operation.[71] That would first be done at the Bell Telephone Laboratories on October 6.[72] In general, the meetings resulted in a virtually complete exchange of ideas and information. All indications are that nothing in the field of radar was withheld.

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[64]Letter from H.G. Bowen to the Chief of Naval Operations, July 26, 1940, in file A8-3/EF 13, 1940, records of the SecNav/CNO, Secret series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.

[65]The importance of the Tizard mission to Canada is well described in Wilfred Eggleston, *Scientists at War* (London: Oxford University Press, 1950), esp. pp. 17-19.

[66]Bowen interview (note 37), side 4.

[67]Letter from NRL to the Chief of Naval Operations, Sept. 10, 1940, in file A2-14, 1940, records of the SecNav/CNO. Secret series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.

[68]Bureau of Ships memorandum for files, Sept. 10, 1940, in file S-A8(3) #1, box 1, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building.

[69]Bowen interview (note 37), side 5.

[70]Letter from NRL to the Chief of Naval Operations, Sept. 13, 1940, in file S-A8(3) #1 (note 68).

[71]NRL memorandum for files, Sept. 17, 1940, in file S-A8(3) #1 (note 68).

[72]Guerlac, *op. cit.* (note 20), p. 328.

The British discovered no radically new developments at NRL, but they learned much there, especially on the level of design details. Complete descriptions of most of the radar equipment the Laboratory had created were sent back to England for further study. The radar duplexer used on the CXAM was of particular interest to them, because no such device was being employed on any English equipment at the time.[73] However, the U.S. Navy profited even more. On October 4, NRL sent to the Secretary of the Navy a letter summarizing the results of the mission during its first month and giving a sober appraisal of its value. It said,

> The Navy has received a vast mount of important information from which much benefit can be gained, particularly since data furnished by the British are based, in great part, on the results of wartime experience....Up to the present it is felt that the Navy has gained more than the British by the free exchange of information.[74]

The letter stated flatly that the British were ahead in radar and explained why:

> Because of the urgent requirements of the war, the British have been forced to prosecute the development of their RDF [radio direction finder, the British name for radar] to a much greater extent than has been reached in this country, and as a result have been able actively to employ the RDF on shore, in aircraft, and in various types of ships. The results obtained by the British have been so encouraging and of such great immediate importance as to warrant the employment in Great Britain of approximately 500 development engineers, with necessary assistants and laboratory facilities; and to bring about the existence of a complete organization for manufacture, procurement, and installation of RDF equipments. Unavoidable exigencies have thus led the British to forge far ahead of this country in the applications of this device.[75]

This lead was spelled out in detail in a chart comparing British equipment to its American naval counterparts. It is included here as Appendix G. The chart shows the U.S. Navy behind in every category of radar equipment: ground-based-detection, fire-control, shipboard, airborne, and airborne-identification radars. The gap was particularly evident because the comparison was based on equipment in operational use. In research, the lead was not nearly as great. As we have seen, the U.S. Navy already had, thanks to NRL and the major electronics companies, the knowledge necessary to build all these types of equipment except airborne radar and already had small procurement programs underway. Most of what was ordered was either as good as or better than the British sets that were in use. The main difference between the two countries at this stage was that the American Navy had been responding to a different international situation, and its leaders had been, as the letter pointed out, following a different policy:

> ...the British are willing to introduce, in quantity, various equipments incorporating new ideas and principles into service prior to complete exploitation and development of their potentialities. At present, such a doctrine is mainly due to the pressure of events. On the other hand, our own Service is generally reluctant to accept developments which are

[73] Letter from NRL to the Chief of Naval Operations, Sept. 10, 1940 in file A2-14 (note 67). It has been said that the British had not previously invented any form of duplexer. However, in an interview Dr. E.G. Bowen asserted that this was not true. Although not in use, a crude form of duplexer had been designed and tried. It was not being employed because of British need for multiple antennas to perform their range-finding techniques. Bowen interview (note 37), side 7.

[74] Letter from NRL to the Secretary of the Navy, Oct. 4, 1940, in file A8-3/EF 13, 1940 (note 64).

[75] *Ibid.*

UNCLASSIFIED

> short of perfection. Such a policy on our part will eventually produce apparatus of the highest order, but [it] unnecessarily delays the adoption by the Fleet of new developments and the associated training of personnel.[76]

As might be expected, the document ended by recommending increased activity. Radar had long had high priority, but, as the Navy was now learning, there are multiple levels of high priority. Having noted the 500 British personnel on the job, the letter stated,

> It is noteworthy that the Laboratory has marked the growing importance of radar by increasing the number of personnel working thereon from five to eighteen during the period from April 1939 to September 1940; however, this small group can be little more than a nucleus in light of present requirements.[77]

Staff would have to be increased, laboratory space would have to be increased, and funding would have to be increased. The British mission had made it clear that radar was, at least at this time, "the outstanding development of the war in the technical field."[78]

Actually, the Bureau of Ships, also motivated by the Tizard mission, had already made recommendations similar to those from NRL,[79] and the Chief of Naval Operations, on the same day the NRL letter report was sent, had authorized that "The number of research engineers engaged in this development at the laboratory be increased several fold as soon as possible....Additionally, if more floor space is necessary to permit the recommended expansion in personnel, it is suggested that a temporary structure be completed without delay and in advance of any permanent buildings which may be contemplated."[80]

Outside the expected calls for just more, NRL's report proposed one action that was somewhat surprising:

> The subject [of radar] is so supremely important that it now appears desirable to place the coordination of the entire project, with all of its ramifications, under one officer, who will be attached to the Naval Research Laboratory.[81]

Here was the mark of Admiral Bowen. Now more aware than ever of the potential of radar, he was determined to see it exploited vigorously and rapidly and to have it as much as possible under his own control. In his mind, the reorganization of Navy research, the powers vested in him by General Order 130, gave him the right and responsibility to act in this manner.

On October 7, three days after the letter about the Tizard mission was dispatched, Bowen sent another to the Secretary of the Navy amplifying the suggestion:

> It is the opinion of the Technical Aide to the Secretary of the Navy that, in order to properly develop and procure for the Naval Service

[76] *Ibid.*
[77] *Ibid.*
[78] *Ibid.*
[79] Letter from the Bureau of Ships to NRL via the Bureau of Aeronautics, the Bureau of Ordnance, and the Chief of Naval Operations, Sept. 24, 1940, in file C-S67-5 #3, box 31, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building.
[80] Third endorsement to the letter cited in note 79, filed with the letter.
[81] Letter from NRL to the Secretary of the Navy, Oct. 4, 1940 (note 74).

> suitable radio range finders (pulse type), the entire development program should be placed under the coordination of one officer who should, for this purpose alone, be attached to the Naval Research Laboratory. This opinion is based upon study of the results of twelve years of experimental work at the Naval Research Laboratory, the accomplishment of the U.S. Army at Fort Monmouth, the commercial development by RCA, the Western Electric Company, and the Bell Telephone Laboratories, and upon the information derived from the recent visit of the British Technical Mission. At the present time this development is under the cognizance of the Bureau of Ships and is of utmost importance to the Bureau of Aeronautics and the Bureau of Ordnance....
>
> The officer who will be the coordinating officer for all of the development mentioned above must, of necessity, be a technically trained officer in the line of electrical or radio engineering. He must be capable of recognizing the requirements of fire control instrument design, and be able to assimilate the special requirements of ultra high frequency radio. There are at present in the Navy very few officers available with the above combined characteristics.[82]

The Bureau of Ships had readily agreed with the recommendation to place more emphasis on radar, but now that Bowen began trying to usurp some of its authority, it was quick to differ. Commenting to the Secretary of the Navy on the Admiral's advice, the Bureau stated,

> The Bureau of Ships is fully appreciative of all the work, including the inception of the idea, done in connection with radio-direction-ranging equipment [at NRL], and has gladly sponsored the project for years. The officer and civilian personnel of the two organizations have worked in close cooperation since the beginning of the project and have redoubled their efforts since rapid developments abroad have emphasized its importance. The most efficient method of advancing the project as a whole, however, is, the Bureau is convinced, to strengthen existing organizations and foster even closer cooperation than heretofore, rather than to set up a new head over efficient going organizations....
>
> The conclusion to be reached...is that the Naval Research Laboratory's function in connection with this project is not the greater part of the project as a whole. By the Navy Regulations this Bureau is responsible for development, manufacture, and installation of radio echo equipment in the Fleet. These functions, plus cooperation with the other Bureaus indicated, are a part of the daily routine of the Bureau, and its organization is set up accordingly and is believed to be performing them smoothly. On the other hand, the Bureau's functions would be new and strange to a purely research organization such as the Naval Research Laboratory.[83]

[82]Letter to the Secretary of the Navy, from the Technical Aide to the Secretary of the Navy, Oct. 7, 1940, in file S-S67-5 #3, box 4, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building.

[83]Letter from the Bureau of Ships to the Secretary of the Navy, Oct. 31, 1940, in file S-S67/A1, box 24, records of NRL, record group 181, job order 11029, Washington National Records Center, Suitland, Md.

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The letter went on to recommend simply strengthening the Bureau's administration of radar.

Bowen won this fight. As a candidate for Coordinator of Radar Development, he had suggested Commander Louis Dreller, who was attached to the Bureau of Ships. Instead, he himself got the job. On November 8, 1940, Admiral H. R. Stark, the Chief of Naval Operations, directed him to "coordinate all research in connection with the development of pulse radio." As authority for the appointment, he cited General Order 130. He also stated, "By copy of this letter, interested Bureaus are requested to afford the Naval Research Laboratory that degree of cooperation which will result in the effective prosecution of this task."[84] Bowen now had the authority he needed to build on NRL's established expertise in the field of radar and make it the focal point for the Navy radar program. His planning, however, would soon be disrupted.

THE NATIONAL DEFENSE RESEARCH COMMITTEE

The Tizard Committee, of course, imparted its knowledge and experience to others in the United States besides the Navy. As Lord Lothian had stressed, the main hope of the British was to build relations with American manufacturing firms so as to increase the productive capacity on which Britian could draw. Thus members talked with RCA, AT&T, GE, and other companies. As was mentioned, the multicavity magnetron was first demonstrated to the Americans, on October 6, at the Bell Telephone Laboratories. This choice was logical in that the Bell Telephone Laboratories were already working in microwave radar for the Army and the Navy and had the capacity to produce duplicates of the tube in quantity, which they soon began to do. The mission also shared its knowledge with the Army, strongly affecting its radar program as it had the Navy's. Finally, it established close ties with the National Defense Research Committee (NDRC), a newcomer to the organizations in America that were working to apply science to the needs of defense.

NDRC was the brainchild of Vannevar Bush, President of the Carnegie Institution of Washington and Chairman of the National Advisory Committee on Aeronautics—the body that linked civilian and military activities in aviation research and development. Bush had gotten his first taste of military research in World War I. After graduating with a PhD jointly from Harvard and MIT in 1916, he worked during 1917 and 1918 to develop antisubmarine devices for the Navy. In 1919, he returned to MIT to teach and do research in electrical engineering, and he excelled in both. He became a full professor in 1923 and became a dean of engineering and a vice president in 1932. In 1938, he became a member of NACA and also was elected to head the Carnegie Institution, which at that time was spending a 1.5-million-dollar research budget in a wide variety of scientific fields, including astronomy, archeology, geology, biology, botany, entomology, embryology, and nutrition. Administering this variety of programs would prove to be good training.[85]

In 1939 and early 1940, Bush began to worry about the state of American defense, especially with regard to the developments in Germany. He shared his concerns with other leading civilian scientists who sat with him on the Committee on Scientific Aids to Learning of the National Research Council—Frank Jewett, President of Bell Telephone Laboratories and President of the National Academy of Sciences; James Conant, President of Harvard; Richard Tolman, Dean of the California Institute of Technology Graduate School; and Karl Compton, President of MIT. Bush remembered, "Discussion of the [defense] problem cropped up whenever the group gathered for committee business and at other times as well."[86] The men resolved to take action; Bush, located permanently in Washington, was to be their agent.

With the help of Oscar Cox, one of Roosevelt's New Deal attorneys at the Treasury Department, he approached Harry Hopkins, the President's closest advisor. In the course of several conversations,

[84]Letter from the Chief of Naval Operations to H.G. Bowen, Nov. 8, 1940, serial 067320 (SC A6/A1-1) in records of the SecNav/CNO, serialized file, Operational Archives Branch, Naval History Division, Washington, D.C.

[85]"Vannevar Bush," *Current Biography* 8 (May 1947): 8-11; Daniel J. Kevles, *The Physicists* (New York: Knopf, 1978), pp. 293-297.

[86]Vannevar Bush, *Pieces of the Action* (New York: Morrow, 1970), p. 32.

he convinced Hopkins of the view that in the area of military technology, American defense was woefully inadequate, that the nation needed to call on its scientists to help out, and that Vannevar Bush should be given the necessary power to mobilize them. Setting aside the possibilities of working through the National Academy of Sciences or under a National Inventor's Council that would have been similar to the Naval Consulting Board of World War I, Bush recommended a new federal agency resembling NACA. It would be funded by the Government but, although linked to the military, would be answerable only to the President himself. Cox had worked out a way to establish it. Back in 1916, as part of the preparedness movement that preceeded America's entry into World War I, a committee of Cabinet members called the Council of National Defense had been created to "coordinate industries and resources for the National security and welfare."[87] Although long inactive, the organization had never ceased to exist. Cox advised resurrecting it long enough to parent NDRC. Hopkins accepted the scheme and, early in June 1940, made an appointment for Bush to see Roosevelt on the matter. Bush recalled the meeting:

> I had the plans for NDRC in four short paragraphs in the middle of a sheet of paper. The whole audience lasted less than ten minutes (Harry had no doubt been there before me). I came out with my "OK-FDR" and all the wheels began to turn. The Council signatures were obtained, we found how to get money, and we organized in a hurry.[88]

The formal executive order creating the new organization was issued on June 27. The members were to be Bush, as Chairman, Conant, Tolman, Compton, Jewett (as President of the National Academy of Sciences), Conway P. Coe (as Commissioner of Patents), one member representing the Army to be designated by the Secretary of the Army, and one member from the Navy as designated by the Secretary of the Navy. The Navy member chosen was Admiral Harold Bowen. The Committee was given broad powers. The order stated:

> The Committee shall correlate and support scientific research on the mechanisms and devices of warfare, except those relating to problems of flight included in the field of activities of National Advisory Committee for Aeronautics. It shall aid and supplement the experimental and research activities of the War and Navy Departments; and may conduct research for the creation and improvement of instrumentalities, methods, and materials of warfare. In carrying out its functions, the Committee may (a) utilize, to the extent that such facilities are available for such purpose, the laboratories, equipment and services of the National Bureau of Standards and other Government institutions; and (b) within the limits of appropriations allocated to it, transfer funds to such institutions, and enter into contracts and agreements with individuals, educational or scientific institutions (including the National Academy of Sciences and the National Research Council) and industrial organizations for studies, experimental investigations, and reports.[89]

This was a radical departure—a decision to put a large portion of military research under the control of civilians answerable only to the President. Bush himself later said,

> There were those who protested that the action of setting up NDRC was an end run, a grab by which a small company of scientists and

[87] A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass.: Harvard University Press, 1957), p. 305.
[88] Bush, *op. cit.* (Note 86), p. 36.
[89] James P. Baxter, III, *Scientists Against Time* (Boston: Little, Brown and Co., 1946), p. 451.

UNCLASSIFIED

> engineers, acting outside established channels, got hold of the authority and money for the program of developing new weapons. That, in fact, is exactly what it was.[90]

But, he also argued,

> ...it was the only way in which a broad program could be launched rapidly and on an adequate scale. To operate through established channels would have involved delays—and the hazard that independence might have been lost, that independence which was the central feature of the organization's success. The one thing that made launching it at all possible was the realization by the President that it was needed.[91]

James Conant was utterly astonished by the plan and by the fact that it had actually received Presidential approval. He recalled in his autobiography,

> I shall never forget my surprise at hearing about this revolutionary scheme. Scientists were to be mobilized for the defense effort in their own laboratories. A man who we of the NDRC thought could do a job was going to be asked to be the chief investigator; he would assemble a staff in his own laboratory if possible....I could see the consequences of this way of mobilizing science to assist the Army and Navy would be profoundly different from what I had known in World War I....I had imagined, as war drew near, that many of my scientific friends and, perhaps, I myself, would once again put on a uniform. It was not to be. Bush's invention insured that a great portion of the research on weapons would be carried out by men who were neither civil servants of the federal government nor soldiers; they would be employees of a contractor.[92]

Bush and his colleagues believed that the military had bungled defense research in the interwar period—that it had hardly begun to tap the immense power of science to create instruments of warfare. It was quite likely, they feared, that the Germans had done much better. They thought that the only solution was for civilian scientists to take power into their own hands and show what they, given independence and authority to work in their own institutions, could do with their knowledge.[93]

Even before the formal Presidential order establishing NDRC was issued, Bush was organizing. Soon it was decided that his Committee would have divisions covering four areas of research: armor and ordnance; bombs, fuels, gases, and chemical problems; communication and transportation; and detection, controls, and instruments.[94] Within the last area, it was agreed from the outset that attention would be given to some phase of radio detection but also that NDRC should limit itself to the field of microwaves.[95] This left high-frequency radar in the hands of the service laboratories that had developed

[90] Bush, *op. cit.* (note 86), pp. 31 and 32.
[91] *Ibid.*, p. 32.
[92] James B. Conant, *My Several Lives: Memories of a Social Inventor* (New York: Harper and Row, 1970), p. 236.
[93] For confirmation of this view see Irwin Stewart, *Organizing Scientific Research for War* (Boston: Little, Brown and Co., 1948), pp. 3 and 4.
[94] *Ibid.*, pp. 10-12.
[95] Guerlac, *op. cit.* (note 20), p. 317.

it, and was consistent with NDRC's policy of focusing on long-range research rather than on strictly development projects.[96]

A microwave committee was set up under Alfred Loomis, who in his own private laboratory had been experimenting with microwaves for some time. In the next few months, he and other members of the Committee surveyed existing developments in both the radar and microwave fields. They visited leading commercial laboratories, NRL, and the Signal Corps facilities. They inspected existing equipment and listened to plans for future development. They were impressed with the importance and the promise of radar technology, but they soon became gloomy about the hope for using microwaves: there was no sign of a tube that could generate adequate power at 10 centimeters or below, the most desirable frequencies.[97] This was late August. Fortunately, the answer was already on its way from Britain.

When the Tizard Committee began its discussions with the American military, there was some question about whether it should be allowed to disclose its information to NDRC as well. NDRC wanted contact and so did the British, but, in a meeting on September 14, General Mauborgne, head of the Signal Corps, said that he "did not think it desirable for the British to give information to the National Defense Research Committee."[98] Others also had doubts. Nonetheless, action was already being taken to bring the two together. Admiral Bowen, at Tizard's request, helped arrange acceptance by the Navy, which officially came on September 16, 1940.[99]

The first meeting of the radar experts in the Tizard mission and NDRC representatives occurred on September 19.[100] The British were told about the American microwave program but said little about their own. They waited to outline it and disclose the multicavity magnetron in a second meeting at Loomis' Laboratory in Tuxedo Park, New York, in late September.[101] When told of the new tube and of the British plans for microwave radar, Loomis and his colleagues were ecstatic; they immediately began thinking about how to exploit the possibilities of the new component. They realized that the magnetron had opened a whole new region of possibilities but, at the same time, that much research lay ahead before it could be used in practical equipment. By mid-October, NDRC had decided it would be best to conduct its microwave radar work in a new laboratory established under civilian direction and staffed as much as possible by civilian scientists. The model for this arrangement was the structure of British radar research, which had proven itself so effective.

Initially, it was proposed to place the new institution at Bolling Field, an Army installation in Washington, D.C., where a heated airplane hangar and laboratory buildings could be constructed. Ironically, this choice would have put the facility immediately adjacent to NRL. Delays were encountered in getting started at Bolling, however, and doubt soon arose about how well the Navy would accept a radar laboratory located on Army property so near its own research facility.[102] After further debate, the Committee chose to place the new institution instead at the Massachusetts Institute of Technology. Karl Compton, President of the University and a member of NDRC, had no part in making the decision but readily acceded to the wish of his colleagues. The space required to get started could easily be found in existing buildings.[103]

[96] *Ibid.*, pp. 317-319.
[97] *Ibid.*, p. 322.
[98] NRL memorandum for files, Sept. 14, 1940 in file S-A8-3(1) #1 (note 68).
[99] Letter from Henry Tizard to H.G. Bowen, Aug. 31, 1940. The acceptance is in the letter from Rear Adm. W.S. Anderson to V. Bush, Sept. 16, 1940. Both are in file C-A8-3(1) #1, box 1, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building.
[100] Guerlac, *op. cit.* (note 20), p. 326.
[101] *Ibid.*, p. 327.
[102] John Burchard, *QED: MIT in World War II* (New York: Wiley, 1948).
[103] *Ibid.*, p. 220.

UNCLASSIFIED

The organization was soon underway. It was given the name "Radiation Laboratory," because this at once seemed both to describe and at the same time conceal its true function.[104] A staff of 50 people was planned, including mechanics and secretaries, and recruitment started. This number would increase to almost 4000 by the end of the war. With the exception of the institutions that would work on the atomic bomb, the Radiation Laboratory would become the largest civilian research and development agency created during the conflict.[105] Yet, as operation got started on what still seemed a risky undertaking, no one dreamed of the growth that lay ahead.

The Microwave Committee, in close consultation with the Tizard Committee members, especially E. G. Bowen, determined that the Laboratory should begin by concentrating on three problems: building a 10-centimeter air intercept radar, developing a precision microwave fire-control radar, and designing long-range aircraft navigational equipment.[106] Operations were underway by mid-November. The first magnetrons arrived from the Bell Telephone Laboratories on November 18. By the end of December, the first experimental microwave set was already being tested. The Laboratory was off to a propitious beginning of what would be a distinguished course in the development of microwave radar. The extent to which the Navy would rely on the institution, however, was still subject to debate.

[104]Guerlac, *op. cit.* (note 20), p. 337.
[105]Burchard, *op. cit.* (note 102), pp. 219 and 220.
[106]Guerlac, *op. cit.* (note 20), p. 330; Office of Scientific Research and Development, *Radar: Summary Report and Harp Project* (Summary Technical Report of Division 14, National Defense Research Committee, vol. I) (Washington: GPO, 1946), p. 4.

UNCLASSIFIED

10. RADAR RESEARCH AND DEVELOPMENT UNTIL THE WAR

Taken together, the growth of radar into a field of technology, the establishment of radar development programs in a number of different American institutions, the Tizard mission and its opening of transatlantic interchange, and the mobilization of civilian scientists under NDRC, particularly its Radiation Laboratory, had the force of a gale on NRL. Finding a new bearing, both in terms of its organizational structure and in terms of the technical program, would be difficult, for there were differing views on what NRL's function should be. Handling the situation created by the existence of NDRC was the most pressing task.

SORTING OUT INSTITUTIONAL ROLES

The NDRC had been established to assist the Army and Navy, to supplement their research-and-development work in order to meet the common goal of preparing the United States fully for a world war. But because of the unique way the new organization was structured and because of the unprecedented power and independence it gave civilian scientists to decide for themselves what the armed forces needed, it came to be perceived as a threat by many military leaders, especially by Admiral Bowen. Like Vannevar Bush and his colleagues, Bowen had been striving to rectify what he believed had been a lack of appreciation of the potential that lay in scientific research and development. He too was trying to build an organization that would give increased emphasis and increased funding to technical development. While NDRC, and particularly its Radiation Laboratory, were getting underway, Bowen was pursuing his own strategy for rebuilding the Navy's internal structure for research and development. Almost inevitably this would lead to conflict with NDRC, to serious questioning of Navy scientific policy, and to a difficult sorting out of institutional roles.

Having been named coordinator of Navy radar development in November 1940, Bowen took his next major step on December 13. He drafted a letter to the Secretary of the Navy recommending that research be given bureau status. NRL, he argued, should become a "Navy Research Center," and should be authorized "to supervise all Naval Research."[1] In calling for this action, he held up the NDRC as both an example and a target. He wrote,

> Some idea of the status of research in the Navy on 27 June 1940 can be gained by considering the research work undertaken and the amount of money expended therefore by the National Defense Research Committee for the Army and for the Navy. By July 1, 1941, the National Defense Research Committee expects to have expended or obligated $6,500,000. The budget estimates of the National Defense Research Committee for 1942 are in excess of $10,000,000. The National Defense Research Committee has research underway on projects affecting the Navy as follows:
>
> (a) Development of Radar and its applications.
> (b) Investigation of new explosives.
> (c) Investigations of gas warfare.
> (d) Fire control.
> (e) Instruments and devices for Naval use.

[1] Letter from NRL to the Secretary of the Navy, Dec. 13, 1940, in the folder on issue 410, records of the General Board of the Navy, Operational Archives Branch, Naval History Division, Washington, D.C.

> All of this new development work could have been previously undertaken by the Naval Research Laboratory if funds had been available to the Laboratory either by direct appropriations to the Laboratory or by transfer from the bureaus. Had such a policy been followed, obviously all of this research and investigation now underway by the National Defense Research Committee would be much further along and in some instances completed. It is interesting to note in this connection that total expenditures at the Laboratory for all purposes in 1941 will be $865,000. If to this is added the value of projects set up by the bureaus, the total amount of money expended at the Laboratory during 1941 will be $2,250,000. Because there was no space available at the Naval Research Laboratory, the National Defense Research Committee has set up a section at the Massachusetts Institute of Technology for the investigation of one feature of the Radar problem for which they have alloted almost one million dollars to be expended or obligated before July 1, 1941. This sum is well in excess of all of the total amount of money expended at the Naval Research Laboratory since the Laboratory developed Radar. It is believed that the Navy Department should attack the subject of research with the same vision as outsiders.[2]

Furthermore, Bowen was already looking ahead to the postwar period. How would present policy affect the situation then? He stated,

> The termination of the present emergency will find the National Defense Research Committee with a considerable program underway which will have to be turned over to the Army and Navy for completion. If the Army and Navy are unable to handle this program on account of lack of facilities, lack of money, or both, it will be necessary to continue the National Defense Research Committee...after the present emergency is over. In the long run, the Army and the Navy are best fitted to have charge of their own research, and it is only because research has not been sufficiently emphasized in the Army and the Navy in the past that conditions warranted the establishment of the National Defense Research Committee.[3]

Bowen's letter was circulated among top officials in the Office of the Chief of Naval Operations for comments. There was little support of his position. Others failed to see NDRC threatening Navy control of its own research; instead, they saw Bowen threatening bureau prerogatives. Captain R. H. English, the Director of Fleet Maintenance Division, wrote,

> To divorce the Bureaus from any direct control over research through the transfer of research funds to the Naval Research Laboratory and by concentrating all research work in one center, would subordinate the Bureaus to the Laboratory and would be a case of the "tail wagging the dog." A parallel situation would exist if the people who fight with ships

[2] *Ibid.* Bowen's figures for spending differ somewhat from the totals computed from existent records and given in Table 2 (in Chapter 4).
[3] *Ibid.*

UNCLASSIFIED

> were deprived of a voice in the development of the types of ships with which to fight.[4]

H. F. Leary, Director of the Fleet Training Division, seconded that view and said,

> The National Defense Research Committee cannot be replaced by any organization the Navy can set up and is the only way by which the Navy can avail itself of the services of such outstanding people in their respective fields. I think we should utilize their services and not attempt to set up an inferior organization. Also, they—by their prestige—can obtain far more money than the Navy could ever obtain for research. The Bureaus should outline the problems, control the development, and supply the detailed technical information—the Naval Research Laboratory should be for particular problems that we are unable, for various reasons, to farm out to a civilian research center.[5]

Bowen got some support from Leigh Noyes, the Director of Naval Communications, but it came with distinct limitations. Noyes agreed that more emphasis should be placed on research and that there should be one man, a Director of Naval Research, supervising the whole of the Navy's program from an administrative standpoint. But he did not believe NRL should become a Bureau of Research, nor did he think that the control of all research money should be taken from the material bureaus. He asserted,

> The Naval Research Laboratory is only a part (although a very important part) of research in the Navy. Several bureaus maintain research establishments, to say nothing of the research available to the Navy through the facilities of commercial corporations and universities.[6]

After receiving copies of these comments, Bowen wrote a memorandum answering them directly and then revised his letter slightly before sending the final draft to the Secretary. Responding to the charge that he was trying to usurp bureau power, he said,

> Under a centralized scheme of research, the Director of the Naval Research Laboratory should be in a better position, on the whole, to know where research problems should be assigned than the Chiefs of Bureaus.[7]

And, since his previous warning about NDRC had made little impact on his colleagues, he now wrote,

> Whether or not the National Defense Research Committee will become permanent after the present emergency is, after all, a matter of opinion, except that I have much more to support my contention than seems

[4] Letter from the Director of Fleet Maintenance Division to the Assistant Chief of Naval Operations, Dec. 19, 1940, in the folder on issue 410 (note 1).

[5] Letter from the Director of Fleet Training Division to the Assistant Chief of Naval Operations, Dec. 20, 1940, in the folder on issue 410 (note 1).

[6] Memorandum from the Director of Naval Communications to the Assistant Chief of Naval Operations, undated, but circa Dec. 20, 1940, with an attached memorandum from the Director of Naval Communications to the Chief of Naval Operations, Nov. 27, 1939, all in the folder on issue 410 (note 1).

[7] Memorandum of H.G. Bowen, Jan. 28, 1941, in the folder on issue 410 (note 1).

> appropriate at this time to include in a brief discussion. My information has been obtained as a member of the National Defense Research Committee.[8]

Elsewhere, he charged, "Every day it becomes more apparent that the National Defense Research Committee will eventually *supplant* instead of *supplement* the research activities of the Army and Navy." [9] Upon receiving Bowen's letter and the associated documents, the Secretary referred the whole matter to the General Board for consideration and recommendation.[10] Thus, for the second time in a decade, this body was to examine the role of NRL in the Navy Department.[11] Now, however, the question was not whether to demote or phase out the institution but whether to make it the capital of the rapidly growing domain of naval research.

In preparation for its hearings, the General Board solicited much information to augment Bowen's letter to the Secretary and the comments he had received. Data were collected, for example, on research in industry and on the structure and operations of the National Defense Research Committee. The Board even went to the trouble of soliciting E. G. Oberlin's opinion. Oberlin gladly replied, saying that since retirement he had studied naval research in Europe as well as reflected on its condition in the United States. "Naval research," he stated, "had been my assignment for 10 years preceding retirement and my avocation during the past 9 years."[12] His comments to the Board contained several items worthy of note.

Oberlin generally agreed with Bowen's argument that research should be taken away from the control of the bureaus and put under a centralized head, but he believed that the best way to do this was to create a new division within the Office of the Chief of Naval Operations. And he was sharply critical of Bowen's personal role in the movement to reorganize Navy research. Making explicit what others no doubt were thinking but were not in a position to commit to paper, Oberlin stated that everyone knew of Bowen's previous position as head of the Bureau of Engineering and that his attempt to create a new bureau he once again would head was bound to "create suspicion." Reviewing the history of NRL after he himself had retired, Oberlin wrote,

> Failure to have mobilized scientific endeavor is quite evident, and no apparent steps have been taken to develop competent research administrators. Plant expansion has been haphazard and the fault cannot entirely be ascribed to the lack of funds, for the general attitude of [the] appropriation committee of Congress has been favorable. Referring to the Naval Research Laboratory letter under consideration by the General Board, any backwardness of research in the Navy on 27 June 1940 reflects directly on the present director, who has had it under his control since 1935....[The letter] indicates Bureau control of research is a failure but fails to show that Laboratory control as proposed would better conditions.[13]

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[8] *Ibid.*

[9] Emphasis is in the original. Letter from NRL to the Secretary of the Navy via the Chief of Naval Operations, Jan. 29, 1941, in the folder on issue 410 (note 1).

[10] Letter from the Acting Secretary of the Navy to the General Board, Feb. 20, 1941, in the folder on issue 410 (note 1). The General Board and its function is briefly described by the paragraph in Chapter 5 where note 33 applies.

[11] See the paragraphs in Chapter 5 where notes 32, 34, 35, and 36 apply.

[12] Memorandum from E.G. Oberlin to Adm. Greenslade, Mar. 15, 1941, in the folder on issue 410 (note 1).

[13] *Ibid.*

UNCLASSIFIED

The General Board's hearing occurred on March 11, 1941. Represented were the Bureaus of Ships, Ordnance, Aeronautics, and Yards and Docks, the Office of the Chief of Naval Operations, and NRL. The discussion contained little that was new. Bowen simply restated the position he had set forth in his letter, and the Bureaus defended the existing arrangement under which they controlled most of the Navy's research budget. Perhaps the arguments were best summarized in a statement of Captain G. L. Schuyler of the Bureau of Ordnance:

> In the paper proposing the Navy Research Center [Bowen's letter] much had been made [about] the NDRC possibly getting out of hand, and eventually supplanting rather than supplementing the Navy's research activities as represented by the NRL.
>
> To prevent this the NRL now suggests building itself up on a much larger scale, but, among other things, taking over the direction of Ordnance research.
>
> An overgrown NDRC looks very objectionable to the Naval Research Laboratory. But a Naval Research Center enlarged in this way in its scope and in its authority to direct Ordnance research looks objectionable to the Bureau of Ordnance.[14]

The principal support Bowen got came from Captain E. D. Almy, who had been Assistant Director of NRL under Oberlin, and Director from 1932 to 1933. Almy, although not endorsing Bowen's position directly, testified that, in his experience, there was little appreciation in the bureaus of the value of research. He advised,

> The research organization should be such that in times of scarcity of funds the Navy would not restrict its research unduly....The director should have enough prestige so that he could go ahead on his own responsibility with a certain percentage of naval problems, i.e., when he has no sympathetic support from practically anyone.[15]

The Board issued its opinion on March 22. It advised against Bowen's recommendations: NRL should not be made the basis of a new Navy Bureau of Research. The opinion stated,

> The General Board considers that "Naval Research" comprehends the entire field of research within the limits of the Naval profession, inclusive of material, equipment, personnel and operational developments, whereas the field of the Naval Research Laboratory cannot be considered as extending beyond the most restricted field of material research of basic nature. The present status is incompatible with a far-sighted policy concerning the broad question of naval research. Stressing the need for expanding the functions of the Laboratory, it tends to depreciate other sources of research and development, both public and private, available to the Department. Throughout the entire course of this continued discussion as to the best place in the Naval Establishment for the Laboratory, the material bureaus have stoutly and very

[14] "Hearings of the General Board of the Navy, 11 March 1941," in the bound volume for 1941 in the Operational Archives, Naval History Division, Washington, D.C., p. 160.
[15] *Ibid.*, p. 167.

> logically presented arguments which sustain their contention that, being charged with responsibility for technical and material developments, they must also have authority over research and development pertaining thereto. Once the more fundamental policies as to research are placed on a sound basis there will be no difficulty in developing the minor policies and an appropriate basis of administration and coordination of all the research agencies and activities under the Naval Establishment.[16]

Although the Board did not believe that NRL should become the center of Navy research, it did recognize that better coordination was needed among the numerous, wide-ranging Navy research efforts. It also recognized that some change in the status of NRL should be made. As it had when issuing its opinion on the Laboratory in 1932, it looked to the Office of the Chief of Naval Operations and advised,

> The Chief of Naval Operations [should] have cognizance over the prosecution of research policies; over such projects, liaisons and coordinations as do not come under individual and joint bureau cognizance; over the reception and evaluation of ideas and inventions from naval and outside sources; and over the administration of the Naval Research Laboratory.[17]

Further coordination was to be obtained through a "Navy Research Council," a body which would comprise members from the General Board, the Office of the Chief of Naval Operations, the Marine Corps Headquarters, each bureau, the Shore Establishments Division, the Board of Inspection and Survey, and NRL. This council was to be "charged with the development and evolution of research policies, appropriate liaison with external research sources and agencies, and basic determination of methods, means, and cognizance."[18] The Board saw no reason to change the present means of funding research and development either in NRL or elsewhere in the Navy establishment. And it did not even mention NDRC. Obviously, it believed that this new body should have no particular effect on the internal organization of Navy research administration.

The Secretary of the Navy did not act immediately on the recommendations of the Board. For while it was making its deliberations, another matter involving NRL had arisen that also had to be taken into account. This included not just internal administration of research but the relation of administration to technical progress in a particular subject and the relation of the Navy research efforts to the mobilization of civilian scientists. The issue might easily have been radio detection, but instead it turned out to be antisubmarine warfare.

In October 1940, the Navy had asked the Naval Research Advisory Committee of the National Academy of Sciences to survey research and development in antisubmarine warfare and make recommendations. For this purpose, the Advisory Committee established a subcommittee under Dr. E. H. Colpitts, formerly a Vice President of the Bell Telephone Laboratories. From the time NRL had been established, it had been the principal location of Navy work on the subject, so the Subcommittee naturally focused on the results of NRL activities. It also, however, examined other subjects, particularly

[16] Letter from the Chairman, General Board, to the Secretary of the Navy, Mar. 22, 1941, in the folder on issue 410 (note 1).
[17] *Ibid.*
[18] *Ibid.* This council was to replace one that had been established by General Order 130 (reproduced in Appendix D) and was already in operation.

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the way that the equipment that NRL had designed was being used. The investigation took several months; a final report was sent to Admiral Bowen, as Technical Aide to the Secretary, on January 31, 1941. In his judgment, it did not contain good news.

The document praised NRL's major accomplishment in antisubmarine warfare:

> Ships of the Navy are now using highly developed supersonic [detection] apparatus [designed by NRL]. This apparatus is used to detect the sound of the submarine's propeller or to receive the echo reflected from the hull of the submarine. From the records available and from our own observations it can be stated that under favorable water conditions, location of a submarine is attained for ranges up to several thousands of yards.[19]

But it went on to assert, "However, water conditions are frequently encountered which seriously limit or even completely preclude the operation of this method." And it stated, "This Committee believes that the present art in the detection of submarines can be improved significantly."[20] The report recommended improved training of operators, saying that the potentials of the existing equipment were clearly not being fully exploited. It recommended some changes in the present apparatus. But most of all it recommended exploring alternative methods of detection. All of NRL's equipment used supersonic pressure waves; the study said that audible sound waves, radio-acoustic methods, magnetic devices, and other means should also be considered. In conclusion it argued,

> The gravity of the [present] emergency is such that the present research facilities and personnel are wholly inadequate. We need the best talent of the country. In these days of aroused patriotism that talent is available. The effort demands a large staff of the highest competence and a properly located and equipped laboratory, with ample ship facilities....It is the considered and unanimous opinion of the committee that the importance, magnitude, and difficulty of the problem call for an effort no less than that recommended.[21]

Bowen might have viewed this appraisal from an outside point of view as confirmation of his own attitude about Navy research, especially since the report recommended that any new facilities should be "under the administration of the Director of the Naval Research Laboratory." Instead, he was completely dissatisfied with the study. Perhaps because he believed the criticism it contained would jeopardize his position on the reorganization of research, he sat on it for over a month while the General Board finished its inquiry. Not until the hearings were over and not until after Frank Jewett, the President of the National Academy, brought the subject up with the Secretary of the Navy did Bowen forward the report to his superior.[22] When he did so, on March 17, he covered it with a strongly worded letter that attacked both the conclusions and the methods of the Colpitts subcommittee. Concerning the recommendation that audible sound might be a profitable area of research, for example, he stated,

> There is nothing in the report of the Committee which would indicate that it has any reason, based on fact, to believe that better results can

[19]Letter from the Naval Research Advisory Committee of the National Academy of Sciences to the Technical Aide of the Navy, Jan. 31, 1941, in file S68, 1941, Records of the SecNav/CNO, Confidential series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.
[20]*Ibid.*
[21]*Ibid.*
[22]Julius A. Furer, *Administration of the Navy Department in World War II* (Washington: Department of the Navy, 1959), pp. 775 and 776.

> be obtained by the employment of audible sound instead of super sound. The Committee has made no investigation in regard to the possibilities of audible sound. In fact, the Committee is obviously so impressed with the acknowledged limitations of super sound that it jumps to the conclusion that audible sound should be much more useful for the purposes of the Navy....It would appear at the present time that the only reason for acceding to such a recommendation would be on account of the pressure exerted by certain well-known scientists, some of whose names appear in this correspondence.[23]

Such a response to the considered opinion of the National Academy of Sciences by the man designated as the leader of Navy research could hardly be construed as anything but outright hostility to the efforts of civilians to aid the Navy in preparing for war.

Bowen's letter deeply distressed Secretary Knox; he referred it and the Colpitts report to the General Board for comment and review. The Board held a hearing on the matter on March 28. Frank Jewett was present for the National Academy of Science. The National Defense Research Committee was represented by Vannevar Bush. Bowen was not there. During the debate, General Board members naturally related this subject to the deliberations on Navy research policy which they had so recently completed. One particular interchange epitomizes the discussion,

> *Dr. Bush:* The National Defense Research Committee was never called upon to enter the field of submarine warfare. Just on the basis of the general interest of the General Board in expediting this matter it would be well to form a better organization than we have at the present time—more complete—for entrance into this field.
>
> *Admiral Richardson:* In reading this correspondence the Navy is represented by one man who thinks it is futile to pursue this.
>
> *Admiral Sexton:* We should definitely override it.
>
> *Admiral Horne:* I think our recommendations should be very specific.
>
> *Dr. Jewett:* The scientists are unanimous in their view that this thing is not only a very difficult problem in which you can't promise anything but it is a thing which should be attacked on the broadest possible lines with all the talent we have.
>
> *Admiral Richardson:* No scientist can be as wrong as a man who shuts his mind to investigation.
>
> *Dr. Jewett:* Admiral Bowen may be right but it is an assumption at the present time and you can't laugh down the opinions that the Millikans and Ketterings have on this. You can't laugh that down. Some of them including myself were in the whole show and we have kept up a certain amount of interest in it during the last two decades.
>
> *Admiral Greenslade:* The Board in its paper on March 22nd definitely recommended the taking of such broad authority out of the hands of one man and putting it in a broader administration of research directly

[23]Letter from H.G. Bowen to the Secretary of the Navy, Mar. 17, 1941, in file S68 (note 19).

UNCLASSIFIED

> and actively under the Chief of Naval Operations. This council would have cognizance of broad policies and be advisory to the Secretary himself.[24]

In its formal report on the subject, the General Board left absolutely no doubt about where it stood. It stated,

> Without, and aside from consideration of the merits of controversial points developed in the papers [under consideration], the Board gives unqualified approval in general to the constructive work of the Committee, of the President of the National Academy of Sciences and also of the suggestion to proceed without delay with technical research throughout the field of detection and location of submarines operating beneath the surface in the open sea....
>
> The Board is definitely of the opinion that all research fields similar to the subject one should be held open and under study by the most competent and available sources in peace time as well as emergency; and to this end the Board urges early consideration and action on the report of the Board...submitted to the Secretary of the Navy on March 22, 1941.[25]

Thus, to the General Board, the way Bowen responded to the Colpitts report seemed direct confirmation of the wisdom of the opinion it had already given on the organization of Navy research.

The Admiral had one more chance to assert his own view. At the request of the Chief of Naval Operations, he wrote a formal set of comments on the recommendations of the General Board's report of March 22. Ironically, his letter was sent on the same day the Board issued its opinion on the Colpitts matter.

Bowen disagreed with most of what the General Board had advised, especially with the proposed Navy Department Council for research and the continued control of most research funds by the bureaus. About the council, he said,

> It is believed that a "comprehensive Council" consisting of 12 members, constituting a cross section of the Navy Department, representing the

[24]Transcript of testimony, in the folder on issue 420, records of the General Board of the Navy, Operational Archives Branch, Naval History Division, Washington, D.C.

[25]Letter from the General Board to the Secretary of the Navy, Mar. 28, 1941, in the folder on issue 420 (note 24). Admiral Bowen did not forget this episode. Over a decade later, after he was retired from the Navy, the Admiral wrote to NRL to inquire what had happened: "I never did learn whether or not anything profitable was developed as a result of the Colpitts Report...I would appreciate very much if you will let me know what has resulted in that direction because I remember at that time that all of us in the Navy thought that any further work in sonics and sub-sonics was futile." (Letter from Bowen to NRL Mar. 8, 1955, in problem file "S-General, July 1954-" Records and Correspondence Management Office, NRL, Washington, D.C.)

The then head of NRL's Sound Division, H.R. Saxton, answered, "The Colpitts report, per se, did not lead to any program on the use of lower frequencies. However, in 1948, I made a thorough study of the problem of obtaining long echo-ranges and, with the help of [other experts], arrived at the conclusion that substantially longer ranges were obtainable under good water conditions with improved equipment parameters and the use of lower frequency.

"Since the middle of 1948, a large research effort has been devoted to investigating possibilities. This Laboratory has [led] the way. As to the results, I can say here only that they have been gratifying and that significant improvements have resulted.

In defense of the position of my predecessor, Dr. Hayes [the head of the Sound Division when Bowen directed the Laboratory], I think it only fair to point out that the restrictions placed on size and weight of equipment before World War II made the employment of lower frequencies futile. Furthermore, even today, considerable improvement occurs only when good water conditions are found." (Letter from H.R. Saxton to H.G. Bowen, Apr. 1, 1955, same file.)

> most widely varying professions, techniques and experience, would by its very nature be unwieldy and ineffective. Its ineffectiveness would be enhanced by its advisory status.[26]

On the issue of controlling research, he said that the bureaus were wrong to see his plan an attempt to grab their laboratories. Those institutions were for test, experiment, and detailed practical development and rightfully should stay under bureau management. His concern, he claimed, was only with advanced research and development. In his best reasoned argument to date, he wrote,

> Due to the lack of leadership in Naval Research, practically all Bureaus have failed to actively prosecute advanced research in certain lines such as bottom paint, Radar, explosives, armor, strategic direction finding, use of plastics, etc. No criticism is meant, personal or otherwise, in respect to the Bureaus charged with the responsibility. It is simply impossible for the Chief of a Materiel Bureau whose main responsibilities are finance, design and production to give research the same personal attention. I know the above to be true from my own personal experiences as a Chief of Bureau....It has also been shown all too often that, in general, research problems of an advanced nature do not originate in the cognizant Bureau, but appear as a by-product of basic research for an entirely different purpose. For example, Radar originated from studies of phenomena accompanying tests of high frequency transmitters....I do not believe that it is proper to expect that the materiel Bureaus can be expected to instigate new material, its procurement and maintenance.[27]

Bowen had no objection to transferring NRL to the Office of the Chief of Naval Operations, "if Naval Research could thereby be centralized, controlled and emphasized, and removed from the influence of the Production Bureaus."[28] Otherwise, he still believed a Bureau of Research to be necessary. And he objected strongly to leaving funding of research as it was, pointing at radar as an example:

> In the instance of Radar, the slowness of the development of this material was directly caused by the failure of the interested Bureau not only to furnish adequate funds, but any funds, during a critical period of development because at that time the Bureau saw no immediate use for this development and withdrew its financial support. Had it not been for the vision and tenacity of the Chief of the Radio Division at the Laboratory and his assistants, the project would have died with incalculable damage.[29]

To conclude the letter, Bowen asserted bluntly, "The Director [of NRL] is of the opinion that the recommendations proposed by the General Board would set up an organization inferior to [the] existing organization."[30]

With all the materials from the General Board and Admiral Bowen in hand, the Secretary of the Navy decided to ask for yet another opinion. He requested Dr. Jerome C. Hunsaker to make an

[26]Letter from the Director, NRL, to the Chief of Naval Operations, Mar. 28, 1941, in file A11/A1-2, 1941, records of the SecNav/CNO, Confidential series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.
[27]*Ibid.*
[28]*Ibid.*
[29]*Ibid.*
[30]*Ibid.*

UNCLASSIFIED

independent study of what to do about research in the Navy, its relation to the mobilization of civilian scientists, and the administration of NRL. Hunsaker, a 1908 graduate of the Naval Academy, was then head of the Department of Aeronautical Engineering of MIT, Chairman of the National Advisory Committee on Aeronautics (he had succeeded Vannevar Bush in that position), and treasurer of the National Academy of Sciences.[31] He was well known and respected in Navy technical circles, having served as the chief of Navy aircraft design from 1916 to 1923 and having kept in close contact with the service leaders after his return to civilian life. He made his report on June 27. The report, which would serve as the basis of the Secretary's action, stated,

> There appears to be general agreement in the Department that, during the present emergency, special action must be taken to ensure that the pressure of current design and production problems shall not stop the longer range effort to improve naval material through research and development. Civilian scientific research resources and personnel have been organized through NDRC to supplement the overburdened facilities of the Army and Navy....
>
> The Navy Department's own research and development work can best be coordinated with that of outside agencies by the setting up of a central organization which shall be in a position to—
>
> (a) coordinate research programs among the several subdivisions of the Department, with opportunity for exchange of ideas and experiences:
>
> (b) provide information as to the status of research in the Navy, desired objectives, and important problems in need of solution:
>
> (c) arrange for cooperation with outside research and development agencies and for the allocation of work or for competition among them....
>
> There is some reason to believe that the degree of coordination and cooperation both within and without the Naval Establishment has not been good, presumably because of the anomalous position of the NRL.
>
> I recommend that the Department set up, by a new General Order (cancelling Nos. 124 and 130) a "Coordinator of Research and Development"...and at the same time transfer the NRL to the Bureau of Ships. It would be desirable to change the name of the NRL, which is very misleading. The Naval Research Laboratory is by no means what its name implies, but is only one of a number of Naval laboratories devoted to special fields of research. The NRL could more suitably be designated the Naval Physical Laboratory, Naval Radiation Laboratory, or Naval Apparatus Laboratory, to indicate its concern with special apparatus development.[32]

Hunsaker's sparse comments and recommendations embodied points from both sides of the previous debate on naval research policy. However, they were largely a defeat for Admiral Bowen,

[31]Furer, *op. cit.* (note 22), p. 776.

[32]Letter from Jerome C. Hunsaker to Mr. James Forrestal, June 27, 1941. The letter is in the file "Research and Development, J.C. Hunsaker," in box 7 of the papers of Julius A. Furer, Library of Congress Manuscript Divison. Attached to the letter are other explanatory materials.

especially for his views about the relation of NRL to NDRC. Secretary Knox accepted Hunsaker's advice and followed it closely when issuing General Order 150 on July 12, 1941.[33] The order did not change the name of NRL, but it did place it under the Bureau of Ships, where it would remain until 1945. It also established a Coordinator of Research and Development, whose main purpose would be building a strong relationship between NDRC and the Navy. Hunsaker himself was the first man given the job.[34]

The history of this whole matter of determining the appropriate position for NRL in the Navy reveals far more than a failed bureaucratic maneuver by Admiral Bowen. It reveals how the Navy formulated its science policy, how it came to terms with the new role science was to play in World War II, and how and why it chose to administer its research and development as it did. Tightly intertwined with the policy issues were technical factors. Radar, although not the only one, was certainly among the most important.

The essence of Admiral Bowen's position was that the Navy needed to expand its scientific research but, at the same time, needed to exercise control over it. To his mind, NDRC was bringing the required increase but usurping Navy power. The Navy, he thought, had to act forcefully to remedy the situation. This argument was unacceptable to other Navy leaders because they saw Bowen trying to rob them of some of their control of research in order to augment his own. His examples of previous failing by the bureaus were not sufficient to win his case—especially since, in pointing a finger at others, he was pointing a finger at his own past record. Bowen's colleagues undoubtedly shared some of his worry about the loss of cognizance to NDRC. But they understood that its research funds came directly from the President and from Congress, not from the Navy Department. And they did not believe they could get such huge additional appropriations for themselves. Most importantly, they realized that the main point was not that the highly trained scientists mobilized by NDRC work within the Navy itself but rather that they work on Navy problems at all.

Bowen never outlined in concrete terms what he would have done if, for example, NDRC's resources for radar research had gone to the Navy or, more particularly, to NRL. But, with the advantage of hindsight, it seems doubtful that NRL, even if it had been greatly expanded, could have achieved both what it did during World War II and, in addition, what was done at the Radiation Laboratory. This seems particularly true if one considers the early phases of the war, when NRL had to respond to strong pressure for immediate development of operational devices, while the Radiation Laboratory was somewhat freer to explore the microwave field. Moreover, as Robert Guthrie, one of NRL's principal radar engineers, commented in retrospect,

> I felt that you would have lost the whole identity of this Laboratory and certainly of the Radio Division, which included radar then, if you had created...from the same source—professors all over the country and others—a Radiation Laboratory and put it here. I think you would have lost more than you would have gained. I think that other factors come into this that, with hindsight, made it turn out better than to have created it here. They were a group put together, they were wartime, they had the gung-ho spirit. They had not gone through President Coolidge, President Hoover, and all the hard times. Back in President Coolidge's time, they counted the pencils you could check out of the stockroom, and you didn't dare check out too many too often....I don't think we could have ever quite expanded and done it in the grandiose way that they could do it.[35]

[33]This order is reproduced in Appendix E.

[34]A manuscript history of the Office of the Coordinator of Research and Development is available from the Operational Archives Branch, Naval History Division, Washington, D.C.

[35]Tape-recorded interview with Mr. Robert C. Guthrie, Historian's office, NRL, Washington, D.C., side 4.

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Noting that NRL did expand markedly during the War, Guthrie pointed out that this occurred in a quite different manner from what would have resulted by combining its functions with those of the Radiation Laboratory:

> We [introduced] personnel underneath supervisors mostly from our own old group. So it was more like growth than a drastic change, as it would have been had you brought in a DuBridge [Lee DuBridge, head of the Radiation Laboratory] to sit by Taylor, you know, or Ridenour [Louis Ridenour, a leading staff member of the Radiation Laboratory] to be in competition with Page....It would have been a very difficult situation. I didn't think it was being done wrong then, and now I am sure it wasn't.[36]

Even Bowen himself admitted in retrospect, albeit grudgingly, that he had been mistaken in his planning:

> The National Defense Research Committee, being well loaded with university professors, was able to mobilize all the members of the union, something which the Naval Research Laboratory never could have done in a thousand years. Also, the National Defense Research Committee had a private pipeline to the President of the United States and the U.S. Treasury.
>
> Nevertheless we must be practical. I do not think, for reasons I have cited, that science could have been mobilized for use in World War II by any other method than the one used, [but] those of us who had been working in applied science for years cannot be blamed for not always enthusiastically endorsing all the efforts of the Johnny-come-latelys who inevitably steam into Washington at the beginning of a war.[37]

Secretary Knox's action brought the policy debate to its end and would fix matters for the war years. It is interesting, however, that there would be significant repercussions from the disagreement. The initial hostility of the Navy toward NDRC would be, for example, one of the reasons that the atomic bomb project would be placed with the Army, even though the Navy had previous experience with research on atomic fission. Vannevar Bush would explain to a historian in 1960,

> When the [bomb] project arrived at the point where very large sums of money were evidently going to be necessary, I took the matter up with Secretary Stimson. The plans were made for setting up the Manhattan District. The decision to take the program up with the Army rather than the Navy was my own, and it was based on the general attitude of the Services in regard to relations with civilian research carried on in my own organization, and also based on the fact that I had enormous respect for and confidence in Secretary Stimson with whom I worked closely throughout the War.[38]

[36] *Ibid.*, Side 5.

[37] Harold G. Bowen, *Ships, Machinery, and Mossbacks* (Princeton: Princeton University Press, 1954), p. 178.

[38] Letter from Vannevar Bush to Dr. Vincent Davis, June 9, 1960, copy in papers of H.G. Bowen, Naval Historical Foundation, Washington Navy Yard, Washington, D.C.

Commenting about his early relations with the Navy, Bush said,

> I might...mention the fact that when the work was first started in my own organization on anti-submarine warfare, the Navy, in the person of Admiral Bowen, stated to us that the Navy needed no help along these lines. He even did so in writing. Fortunately, young naval officers and energetic civilians thought otherwise.[39]

The disagreement also figured in the creation of the Office of Naval Research in 1946. In several important ways, it would be the type of organization Bowen had wished to establish in 1941. Furthermore, he would figure prominently in its formation; he would ensure that NRL be put directly under its control, and he would become the first Chief of Naval Research. The Admiral, that is to say, would be deeply involved in bringing control of Navy research wholly back into Navy control once the war ended.[40] But that is another story.

THE NRL PROGRAM UNTIL PEARL HARBOR

Determining the relations between the Navy and NDRC was the concern of top policy makers. Admiral Bowen's actions had been taken at his own initiative, and little of what transpired except the final decisions filtered down to the working scientists and engineers at the NRL. There was, of course, also some jealousy on that level regarding NDRC and its Radiation Laboratory. Robert Page, for example, recently recalled his feelings about the new institution in this way:

> My first reaction was that they were raiding the chicken coop. They were taking away my baby. They were giving to somebody else a job that we had already partly done and could do much better because of our experience. That was my first reaction. That, of course, was a very parochial, narrow-minded, jealous approach. I felt that all through the war. Well, I shouldn't say that entirely, because I began to realize—I did begin to realize that the additional effort was necessary. That what they accomplished was far more than we could possibly have accomplished in the same time. Because their facilities, their resources, were so much greater. The people that they had in were very competent people and they learned fast. Having learned, with a large number of people—the large sums of money—they moved fast and accomplished things that we never could have done in the time scale, so I did begin to awaken to the fact that it was a wise and necessary move. And as years have gone on, I've come more and more to appreciate the wisdom and the value of having moved the way they did in setting up NDRC.[41]

[39] *Ibid.* The official history of the Manhattan Project, *The New World* (Washington: Atomic Energy Commission, 1972), by Richard G. Hewlett and Oscar Anderson states that Bush was following the wishes of President Roosevelt in selecting the Army. See p. 71.

[40] Bowen's personal involvement is not brought out in what has become the standard history of the formation of ONR: The Bird Dogs (authors' nickname), "The Evolution of the the Office of Naval Research," *Physics Today* **14** (Aug. 1961): 30-35. It is, however, discussed in Harvey Sapolsky, *ONR: Science and the Navy* (forthcoming). For an inside story, see the war diary of Rear Adm. Julius Furer, box 1, Furer papers, Library of Congress Manuscript Division, especially the entries of May 29, 1945 through July 31, 1945.

[41] Transcript of a tape-recorded interview with Dr. Robert M. Page, in the Historian's office, NRL, Washington, D.C.

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The initial jealousy was shared by many others. In contrast, some, like Guthrie, believed from the beginning that the Radiation Laboratory was necessary. But whatever the inner feelings of NRL reseachers, all recognized that there was a big and important job to do in radar and a definite need for complete cooperation. And all records indicate that, in technical matters, NRL and the Radiation Laboratory rapidly became partners, not rivals. Indeed the same was true of NRL's relations with the numerous private companies and other Government institutions that became involved. This was, unquestionably, a laudable attitude. Yet, at the same time, the good relations resulted not only from a genuine desire to join together for the national welfare but also from the nation's basic policy decision to follow virtually all paths simultaneously in its huge radar effort. This meant plenty of work to go around and little opportunity to become disgruntled.

General Order 150 put NRL back in essentially the same administrative position it had held in 1939.[42] And it implicitly mandated that the basic operating policy would remain unchanged. NRL would run partly on direct funds from Congress and partly on funds from the various Navy bureaus. Projects would be chosen and administered as they had been before. The Laboratory was not, however, the same as it had been 2 years earlier. Nor was the radar project.

While the policy debate had gone on, NRL had been reshaping its technical program in response to the disclosures by the British and in response to the increased awareness in the Navy of what could be done in the radar field. By October 11, 1940, it had reformulated its general plans, and on that date it sent to the Secretary of the Navy a "Proposed Program for Research and Development of Application of Radio Ranging Equipment."[43] The plan was a significant change from the comprehensive report Robert Page had made on NRL's radar program the previous February.

In the area of shipboard radar, the Laboratory still proposed to stress development of equipment in the range 400 to 500 megahertz—even though by recommending this course of action it differed from the British, who were planning to jump directly from around 200 megahertz to microwaves at around 3000 megahertz. In contrast to the earlier report, however, there was no longer a suggestion to drop development of new shipboard equipment using frequencies below 400 megahertz. In the spring, Admiral Bowen had proposed a submarine radar using pulses in the range 120 to 200 megahertz; the design of it was now well along. Moreover, British success in producing a variety of naval equipment along with increased pressure from the operational side of the U.S. Navy had led to a decision to build 200 megahertz sets for surface ships too small to carry the CXAM. Now more than ever it seemed unwise to await the results of the further research that would be needed to go to higher frequencies.

Of greater importance than the changes in shipborne radar was the development of airborne radar. This had not even been mentioned in Page's last report. Now it was suggested that the Navy move rapidly into airborne radar, drawing heavily on what the British had already achieved. Equal emphasis was placed on accelerating development of radio identification equipment to be used in conjunction with radar.

In fire-control radar, development was moving along well in the United States, thanks to the partnership between the Navy and the Bell Telephone Laboratories. However, British disclosures—especially of the multicavity magnetron—pointed to new possibilities. NRL's plan recommended active prosecution and immediate procurement of British equipment and tubes for study. Finally, the report advised more research on components, especially tubes. It requested, for example, that the tube-development group at NRL be increased immediately by six to ten highly trained personnel and that work on cathode ray displays and all other auxiliary equipment for radar also be stepped up.

[42]The main difference was that the Bureau of Engineering had been merged into the Bureau of Ships.

[43]Letter from NRL to the Secretary of the Navy, Oct. 11, 1940, in file S-S67-5 #3, box 4, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building.

Within several weeks NRL's proposal was endorsed, with only minor changes, by the Bureau of Ships and the Chief of Naval Operations.[44] The Laboratory had a blueprint for the next phase of its development of radar.

A principal difficulty in carrying out the NRL program was building the facilities and finding the trained personnel required. Few people had the requisite qualifications, and NRL faced increasing competition in recruitment from industry and the Radiation Laboratory. Being under the civil service system proved an additional handicap. On December 3, 1940, Hoyt Taylor poured out his frustrations about the situation in a memorandum to Admiral Bowen. The Radio Division, he said, was now 4 times as large as it had been only 5 years earlier. The men employed on the radar project alone, 31 by this time, now almost equaled what the entire staff of the division had been in 1934. And all indications were that the size of the group would continue its rapid increase. The growth had caused problems. Taylor commented,

> I wish that all of this expansion could have been done under Civil Service, but practically, it absolutely could not have been accomplished in that way. Money was not available for Civil Service positions and it was available for contract positions. The urgency of the work demanded immediate action and it had to be taken as best we could.[45]

Thus, many new employees were hired on contract. As rapidly as possible, the best of them were being converted to permanent civil service standing. But salaries were a problem. To get good contract men with the increased competition, financial offers had to increase. Unfortunately, civil service salaries were not going up at the same rate, and attempts to promote even the leading men were proving unsuccessful. Taylor described the situation he faced,

> If and when...we attempt to write a job sheet for a reclassification, we are supposed to describe in detail the work that the man is doing. But if we say that he is still head of [a]...Section [that he has not been promoted to a higher administrative level], in spite of the tremendous increase in quality and caliber of work that this section is doing, we don't seem to have much chance in getting him reclassified. Furthermore, since he is on a secret problem, we are forbidden to make any description whatever of his work.[46]

Yet, until top employees were promoted, those underneath them could not be. This was leading to the embarrasing situation of junior contract employees receiving higher salaries than regular civil service men of longer tenure. Taylor believed that no less than a general revision of personnel matters at the Laboratory was required to alleviate the situation. But even that could not possibly bring a rapid solution. Similar headaches were being encountered in getting new buildings—even temporary ones—constructed and into service. In short, increasing and improving the Navy's radar equipment required far more than solutions to technical problems. After the war, Taylor would look back on this period and comment,

> The Laboratory was beginning to be provided with much larger funds, and personnel, particularly in the radio field, was being expanded very

[44]Letter from the Bureau of Ships to NRL, Oct. 30, 1940, and a letter from the Chief of Naval Operations to the Secretary of the Navy, Nov. 1, 1940, both in file S-S67-5 #3 (note 43).
[45]Memorandum for the Director of NRL from A. Hoyt Taylor, Dec. 3, 1940, in file C-S67-5 #3, box 31, records of NRL, Confidential series (now Unclassified), record group 19, National Archives Building.
[46]*Ibid.*

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> rapidly, although not as rapidly as should have been the case. The fact is that none of us after being starved for years, could accustom ourselves to thinking in terms of millions rather than hundreds of dollars. We continually underestimated our immediate and future needs. The work that we were to be called upon to do from now on would have been done more quickly, with less cost and with better end results, if we had more liberal support during the years of peace.[47]

Whatever the problems in reaching the appropriate level of activity, the radar program at NRL was now moving as fast as it could. The Laboratory was a focus of the extensive effort to equip the Navy adequately with radar before the seemingly inevitable entry into war.

The last comprehensive report the Laboratory made on radar before Pearl Harbor came in May 1941.[48] The document provides both a clear view of the entire situation just a few months before the surprise attack and an opportunity to glimpse the future.

Page authored the report. There were now, he stated, 57 men working on radar, most of them full time. Their effort covered a wide range of activity from specific set development to broad general research. Work on the CXAM and CXAM-1 sets was now complete. Many of the 20 equipments ordered had already been installed; the remainder would be installed during the summer. Progress on the radar for submarines, now labeled the "SD," was good. An NRL design using pulses of 114 megahertz at 80 kilowatts of power was being built for testing. RCA had already been chosen as the principal producer, and deliveries were to start in the late summer. The radar used the submarine periscope as its antenna and, although not directional, was able to provide general warning against airplanes at distances up to 25 or 30 kilometers (15 or 20 miles). Equipment of this design would be used widely during World War II, until the Japanese discovered a way to intercept the signals and home in on their source. Over 400 of this model and its various modifications would eventually be procured.[49]

The effort to scale down the CXAM, so it might be used on destroyers and other warships of similar size, was proceeding apace at both NRL and in industry. NRL's model, the XAR, was scheduled to be ready for initial testing in midsummer. The equipment operated in the band 180 to 220 megahertz and had a pulse output power of 150 to 200 kilowatts. The antenna, much smaller than that of the CXAM, measured 2.6 meters (8-1/2 feet) wide by 2.1 meters (7 feet) high. The XAR would primarily be used for comparison to industry models so that methods of improving them could be discovered before large-scale production began.[50] RCA's design of this type of radar was named the SA; GE's, the SC. Both companies would learn much from testing their sets against the XAR and would adopt many of its features; both would eventually sell their equipments in large numbers to the Navy. These would become the first standard equipments for smaller warships.[51]

Building a 400-megahertz radar for shipboard use was now getting somewhat less attention than before. This was due partly to the increasing demands for other types of radar and partly to technical

[47]A. Hoyt Taylor, *Radio Reminiscences* (Washington: NRL, 2nd printing, 1960), p. 186.

[48]Letter from NRL to the Bureau of Ships, May 13, 1941, in file S-S67-5 #5, box 5, records of NRL, Secret series (now Unclassified), record group 19, National Archives Building.

[49]Louis A. Gebhard, *Evolution of Naval Radio-Electronics and Contributions of the Naval Research Laboratory* (Washington: NRL Report 8300, 1979), p. 186.

[50]Letter from NRL to the Bureau of Ships, Oct. 13, 1941, in file S-S67, 1941, records of the SecNav/CNO, Secret series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.

[51]Norman Friedman, "US Naval Radars: An Introduction" (unpublished Hudson Institute discussion paper, H1-2570-DP, 1977), pp. 160 and 161; Gebhard, *op. cit.* (note 49), pp. 183-186.

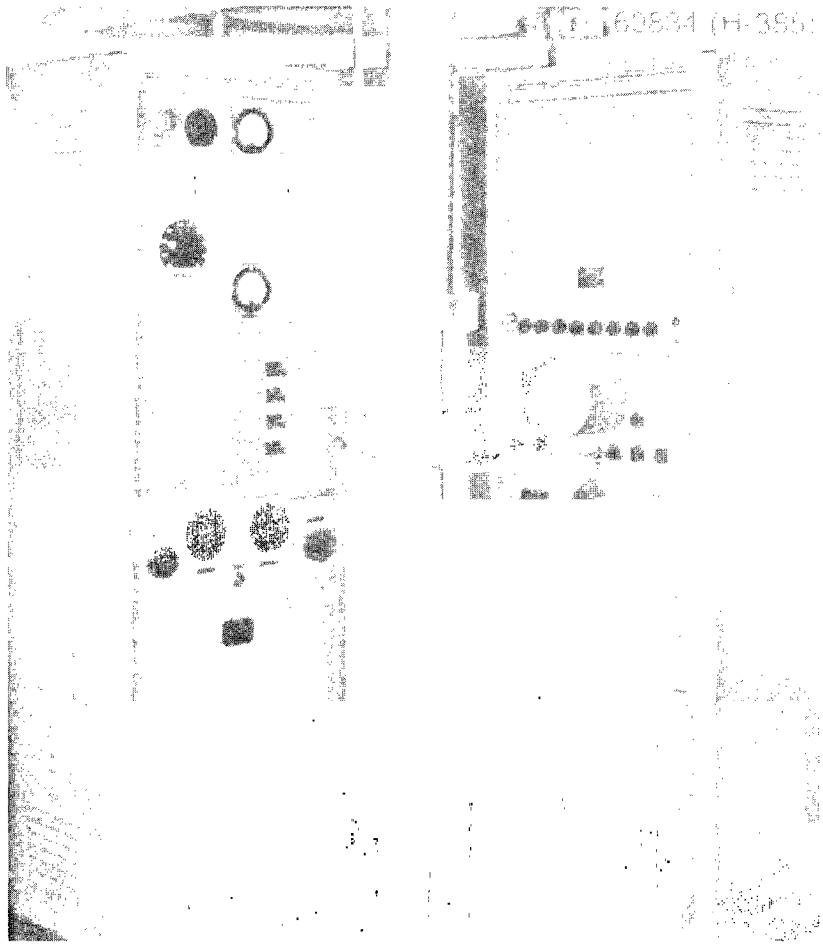

Fig. 21 — NRL's XAR radar was a scaled-down version of the XAF for use on destroyers and other medium-sized warships.

difficulties. Page reported that in tests of a 400-megahertz model, "performance on all targets was inferior to that of the CXAM."[52] Problems would continue to trouble the effort. Practical radar on 400 megahertz would eventually be developed, but it would never see operational use.[53] In the higher frequency fire-control radars, NRL continued to leave most work to the Bell Telephone Laboratories and simply test its products (the FA, FB, and FC radars). However, some work was being done on radar range finders, as well as on circuit and antenna designs.

The Laboratory was moving rapidly to catch up with the British in airborne radar. A British ASV had been procured and installed in a Navy plane for testing. Dr. E. G. Bowen, the English expert, supervised installation and many of the first trial runs.[54] Based on the results, the Navy decided tc procure, on a crash basis, a limited number of radars of the same design. More emphasis, however, was placed on changing a 500-megahertz pulse altimeter NRL had already built into a radar set. The use of this frequency would allow the equipment to be lighter and employ smaller antennas and thereby be

[52]Letter from NRL to the Bureau of Ships, May 13, 1941 (note 48).
[53]Gebhard, *op. cit.* (note 49), p. 186; A.A. Varela, T.H. Chambers, and H.W. Lance, "Development of XBF-1 L-Band Radar Equipment" (Washington: NRL Report 2559, 1945).
[54]Letter from NRL to the Chief of Naval Operations, Oct. 16, 1940, and letter from NRL to the Chief of Naval Operations, Nov. 5, 1940, both in file A8-3/EF 13, 1940, records of the SecNav/CNO, Secret series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.

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more effective. Essentially what was involved was reworking the transmitter and receiver of the altimeter so they could function with a British-style antenna system and indicator. This effort, just starting when Page wrote his report, would eventually lead to the XAT experimental radar and, in production form, the ASB. Over 26,000 devices of this type would be built for the Navy by RCA, Bendix, and Western Electric during World War II.[55] The ASB would become the "workhorse of carrier-based aviation during the critical phases of the war,"[56] and would be procured in larger numbers than any other model.

Fig. 22 — The ASB radar was NRL's first equipment designed for airborne use. While many design features were copied from the British, the basic circuitry was taken from a pulse altimeter that NRL had previously developed.

Page noted that much effort was being devoted to radar recognition equipment, identification friend or foe (IFF). As was stated earlier, NRL had been working on this subject since 1937. By early 1940, it had designed prototypes of devices for both ship-to-aircraft and ship-to-ship recognition. The General Electric Company had been designated as the principal contractor and participated in the design process.[57] In the summer of 1940, the NRL-GE plans were modified in conference with the Army so that the equipment would be suitable for joint service use. By mid-1941, experimental models were in production.

The cooperation between the British and the Americans that had started with the Tizard mission created a problem in this area of development. It was clear that both nations should use the same recognition devices so they would not misidentify each other's warships and planes. But which design

[55]Gebhard, *op. cit.* (note 49), p. 201.
[56]Lloyd V. Berkner, "Naval Airborne Radar," *Proceedings of the Institute of Radio Engineers* **34** (Sept. 1946): 671-706.
[57]Letter from NRL to the Bureau of Ships, Jan. 21, 1941, in file S67, 1940, records of the SecNav/CNO, Secret series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.

should be accepted—the British or American? When Page wrote his report, the question was still being discussed; he argued strongly for the American system. Ultimately the decision would go the other way. The existing British Mark II IFF would be modified, based on recommendations made by both countries, to become the Mark III. It would then be made the standard recognition device used by the allies throughout World War II. Nonetheless, the NRL-GE model, designated ABA for airborne use and BI for shipborne, would also be produced in fairly large quantities for use as a backup system.[58]

Thus, the bulk of NRL's effort in radar during the months preceeding Pearl Harbor was developing or improving equipment that could go into operation rapidly. Page reported, however, that NRL was continuing to do as much research on new ideas as it could. Studies of antenna theory had led to efficient, new designs; means had been worked out to allow lobe switching on a single antenna without loss of directivity. Tube research, conducted in conjunction with electronics companies, was resulting in increased power and receptivity in various set designs. The Laboratory was investigating such techniques as rapid scan, conical scan, and automatic tracking to improve performance. It was even keeping a hand in development of radars with wavelengths in the middle of the microwave region. Indeed, a complete radar system using a multicavity magnetron and 10-centimeter waves was under construction. Although NRL would never attempt to compete with the Radiation Laboratory or the Bell Telephone Laboratories in this type of work, it needed to acquire knowledge and expertise in the subject, for it would be responsible for testing many of the microwave sets developed elsewhere before they were accepted for service use. And during the war, the Laboratory's research would allow it to to make numerous contributions to electronics for microwave radar.[59]

Overall, even before American entry into the war, NRL's radar program had become a large, diversified effort that was moving forward quickly and would pay off handsomely. Entrance into the conflict would, of course, increase pressure for results, but the greatest changes had already occurred. The Tizard mission and the formation of the National Defense Research Committee had a far more pronounced effect on NRL's radar program than would the bombing of Pearl Harbor.

RADAR WHEN WAR BEGAN

The surprise air attack by the Japanese on December 7, 1941, was a great shock to everyone in the United States. Men who had been involved in the development of radar, however, felt a special sense of loss. Such an attack was precisely what they had hoped their work could prevent. As is well known, radar did have a chance to help. An Army model SCR-270 radar had picked up the incoming airplanes while they were still over 200 kilometers (130 miles) away. But since it was a Sunday morning, since the Army was not as yet accustomed to relying on radar information, and since a flight of B-17 aircraft was supposed to be in the same general vicinity, the warning from the young radar operators manning the set was ignored.[60] Even had it been heeded, it is unlikely that the disaster could have been prevented, although it would have been lessened.

In retrospect, this failure has often been considered a symbol of gross unpreparedness of the United States in radar.[61] There is an element of truth in such a conclusion, for, as this study has shown, much more could have been done in the 1920s and 1930s. However, as this study has also shown, it is an egregious mistake to oversimplify the situation and overlook the remarkable progress that had been made in radar by a nation that did not stand under the immediate threat of prolonged airborne bombing, a nation in which public sentiment was decidedly pacifist, a nation in which funding for defense research was niggardly.

[58]Gebhard, *op. cit.* (note 49), p. 255.
[59]*Ibid.*, pp. 187 and 188.
[60]George Raynor Thompson, Dixie R. Harris, Pauline M. Oakes, and Dulaney Terrett, *The Signal Corps: The Test* (Washington: Department of the Army, 1957), pp. 3-10.
[61]See, for example, Robert Watson-Watt, *The Pulse of Radar* (New York: Dial, 1959), pp. 11 and 12.

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NRL's radar research-and-development program on the eve of American entry into the war has been outlined. To close our study, it is appropriate to note what operational equipment the U.S. Navy had received by that time. This, after all, was the ultimate measure of how well the institution and those who directed it had done their jobs.

Records do not show the exact situation on December 7, 1941. However, a complete survey of what equipment had been installed and what delivered was made less than 2 weeks later. It was summarized in an internal memorandum of the Bureau of Ships written on December 20, 1941:[62]

> The situation with respect to the Radar program as of 18 December 1941...appears to be progressing very satisfactorily considering some of the early difficulties which had to be overcome....
>
> The following table shows the extent to which deliveries and installation of shipboard equipment have been made as of 18 December 1941.

Type	Sets Delivered	Sets Installed	Delivery Date
CXAM	6	6	Completed
CXAM-1	14	13	Completed
FA	10	9	Completed
FC	49	21	21 per week
SC	48	27	14 per week
SD	5	3	7 per week
	132	79	

Although NRL was not responsible for either the conception or design of all of these equipments, it had been heavily involved in producing each of them. It had played the leading role in developing naval radar in the United States, turning a questionable idea into the reality of powerful, electronic sensors ready for operation on American warships and into a massive technical-industrial program primed to produce more of them by the thousands.

"The Government," had said Thomas Edison, "should maintain a great research laboratory jointly under military and naval and civilian control. In this could be developed the continually increasing possibilities of great guns, the minutiae of new explosives, all the technique of military and naval progression without any vast expense."[63] This thought had been the beginning of NRL, the first modern research and development facility in the United States Navy. The story of radar is one example of what the institution has accomplished.

[62]Airborne radar was still in the testing stage at this point. The document cited is a Bureau of Ships memorandum from J.B. Dow to Capt. F.E. Beatty, Dec. 20, 1941, in file S67, 1941, records of the SecNav/CNO, Secret series (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C.
[63]Cited by note 2 in Chapter 3.

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11. CONCLUSION

The preceding chapters of this study presented a narrative account of the origin of radar at NRL, without explicit presentation of my own thoughts and opinions. In this chapter, I shall review my approach, analyze major themes, and give my interpretation of the significance of the story.

APPROACH

When choosing the subject for this inquiry, I decided to examine how NRL worked in one particular instance, that is, how a single major project progressed from initiation to accomplishment. Only in this way, I felt, could I pierce through generalizations about research and development; only in this way could I, in a sense, remove the housing and watch gears mesh. Yet, at the same time, I hoped to relate my subject to its broader context: at NRL, within the Navy, and even within the ongoing evolution of organized research and development as a whole. Hence, the case study I chose was a major project rather than one or even a group of "routine" ones. I knew that radar would *have* a context of interest, in the sense that it involved activity at the highest levels within NRL and interactions of the Laboratory with people and organizations outside of it and in the sense that records directly related to the subject would exist and would allow me to trace these interactions accurately.

Once my approach was chosen, my research and writing plan became simple. I first covered the historical background, then focused down to the men, ideas, and actions that led to the creation of radar, and finally broadened the view again as radar developed into a major technical program whose effect extended to international relations and the general organization of American science. The story of radar was the common element throughout, but the account as a whole was to depict, more broadly, a major national research-and-development facility in operation.

MAJOR THEMES

Historical Context

Chronologically, the story is situated in a single era in the history of mission-oriented research and development in America. To outline its context, that history may be divided into three major periods: the late 19th century to the start of World War I, World War I to the start of World War II, and World War II to the present.[1] The first period, which was summarized briefly in Chapter 2, was when pioneer research institutions were created in the United States, when their efficacy was demonstrated, and when they became firmly implanted in a small number of industrial firms and government departments.

The second period started when the forces of a major war began to act on the American technical community, which had by this time become as extensive as any in the world. The material demands of

[1]There are many other ways to divide this subject chronologically. W. David Lewis in "Industrial Research and Development," *Technology in Western Civilization*, vol. II, Melvin Kranzberg and Carrol W. Pursell, Jr., eds. (New York: Oxford University Press, 1967), pp. 615-634, argues that the effects of World Wars I and II should not be overemphasized: many trends accelerated by the wars, he says, existed before they began. His point is good but is much less true for military laboratories in the governnment than for private industry. Furthermore the period since World War II is obviously not all of a piece when viewed closely. One useful subdivision of this era is given by W. Henry Lambright in *Governing Science and Technology* (New York: Oxford University Press, 1967), pp. 15-26. I certainly realize that although the periodization I have followed is adequate for the purpose, it is by no means complete.

preparing for and engaging in a global ccnflict, however, required great increases in industrial production. The separation from Germany and its scientific output placed particularly high demands on indigenous technical capability. Consequently, the Government's needs for and investment in research and development increased rapidly. Dynamic growth of institutionalized research and development was the result; one case in point was creation of NRL. Another related event was formation of the National Research Council (NRC) within the National Academy of Sciences. When peace returned, the NRC became a tireless booster of industrial research and development and a major national force pushing for the creation of American research laboratories. Business was receptive to the idea, and by 1940, near the end of this second period, one analyst examined the situation and wrote,

> Since the First World War, industrial research has assumed the proportion of a major industry. Laboratories organized before the war have expanded their facilities and increased their staffs; new laboratories have been established by companies seeking to maintain or improve their position in the industrial order by using more efficient methods, by making better products, by developing new products, and by being better equipped to meet the challenges that come through science and technology.[2]

Surveys confirmed the view, showing that the number of industrial laboratories in America had jumped from around 300 to more than 2200 during the period. Growth within the Government was much more limited, but a few new facilities in the armed services, the Department of Agriculture, the Bureau of Mines, the new National Advisory Committee for Aeronautics, and other agencies were established.[3]

It was impossible to see in 1940 that the real period of expansion lay not just behind but just ahead. World War II affected American research and development far more profoundly than had anything before. The Federal Government, which had been overshadowed by business in the support of research and development in the 1920s and 1930s, would now assume the leading role in what was to be a far vaster enterprise. In his survey of science in America, historian Hunter Dupree summarized the change in this way:

> The year 1940 marked the beginning of a new era in the relations of the Federal Government and science. So far as the line can be drawn across the continuous path of history, this date separates the first century and a half of American experience in the field from what has come after. As the scale of operation changed completely, science moved dramatically to the center of the stage. By the time the bombs fell on Hiroshima and Nagasaki, the entire country was aware that science was a political, economic, and social force of the first magnitude.[4]

Financial statistics also demonstrated the marked alteration. In 1940, total national investment in research and development was $345,000,000, with government expending only 19%, industry 68%, and universities and others 13%. In 1945, the total was over $600,000,000 and the percentage quite different: government 83%, industry 13%, and universities and others 4%. The end of the war did not stop the increased funding. The total national expenditure rose in 1947 to over 1.1 billion dollars,[5] and

[2]Howard R. Bartlett, "The Development of Industrial Research in America," in National Resources Planning Board, *Research—A National Resource*, three vols. (Washington: GPO, 1938-1941), vol. II, p. 37.
[3]A. Hunter Dupree, *Science in the Federal Government* (Cambridge, Mass.: Harvard University Press, 1957), and Alex Roland, *A History of the National Advisory Committee for Aeronautics, 1915-1958* (Washington: NASA SP-4013, in press).
[4]Dupree, *op. cit.* (note 3), p. 369.
[5]John R. Steelman, *Science and Public Policy: A Report Made to the President* (Washington: GPO, 1947), vol. I, pp. 10-12.

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increases continued to outpace inflation up through the end of the Vietnam War.[6] Since then, funding, when inflation is considered, has tapered off somewhat, although in the last few years slow recovery has been emerging.[7] Throughout the postwar period, government has continued to be the major source of funding, trailed by industry, universities, and others, in that order. Thus the roles established during World War II have not altered.

The impact on the Navy of the changes during World War II was partly apparent in the account of the alterations made at NRL, described in Chapter 8. With the creation of the Office of Naval Research (ONR) in 1946—a clear indication of the increased importance the Navy had begun to ascribe to fundamental scientific study—NRL found a new home that was well suited to its operations. Ever since it has remained a constituent part of ONR, acting as its principal in-house laboratory. Navy-wide, the war pushed the number of principal research-and-development facilities to 31, half of which were new. Of those that had existed before 1940, most, as had NRL, underwent significant expansion—virtually all of which became permanent as the cold war set in. Unlike NRL, however, the other Navy research-and-development facilities, both new and old, have undergone numerous basic administrative changes since the war. They have been broken up, consolidated, reorganized, redirected, or moved around in the Navy hierarchy as means have been sought for more efficient and effective management.[8]

A summary understanding of the periodization of research and development in America, such as has been sketched, is crucial for situating the story of radar at NRL. General awareness of what has occurred in the period starting with World War II and how it differs from what went before is particularly important, because most notions about research and development in America derive from this present period. It is in this period only that, for example, one can rightly speak of a sizable military-industrial complex devoting large quantities of resources to building new weapons. The preceding period, in which radar was developed, was one when government involvement with science as a whole trailed behind that of industry and when military funding, particularly for scientific research, was low.[9] Also characteristic of the present period, especially the postwar portion, has been an abiding concern over administration of research and development. By far the greatest body of literature on research and development is management literature; 90% of it dates from after 1946, and virtually all of it focuses on management in the postwar setting. The studies have assisted the development of sophisticated new management tools, such as systematic long-range planning and budgeting, more effective personnel procedures, fuller awareness of the stages of research and development, and the means for moving projects from one stage to the next.

The radar story told in the preceding chapters is situated within the second historical period of the evolution of American research and development. It begins when the forces of World War I pushed the Navy into creating a new type of facility that had proven its effectiveness in the industrial sector and ends when the forces of war were, once again, beginning to affect the relations between the Navy and science. The development of radar, in fact, was one reason that the importance of investment in scientific research became so obvious as America headed toward its second global conflict. But the way the early work on radar was done was part of the period that was passing, not the period brought on by World War II.

The Beginning

Underlying the complex web of events that determined how and when NRL was formed, the events recounted in Chapter 3, were four principal factors: need, war, politics, and personalities. Need

[6]National Science Foundation, "National Patterns of R&D Resources, Funds and Manpower in the United States," 1953-1957 (Washington: NSF Report 75-307, 1975).

[7]Willis H. Shapley and Don I. Phillips, *Research and Development, AAAS Report IV* (Washington: American Association for the Advancement of Science, 1979).

[8]Booz-Allen and Hamilton, Inc., *Review of Navy R&D Management, 1946-1973* (Washington: Department of the Navy, 1976).

[9]George C. Reinhardt and William R. Kintner, *Haphazard Years: How America has Gone to War* (New York: Doubleday, 1960).

for means of keeping the Navy technologically up to date was, in 1915 and 1916, largely unquestioned by anyone with power to make decisions on the matter. There was even surprisingly little disagreement on the need of the Navy to upgrade its research establishment; quarrels began only when people started estimating how many millions this would cost. The European war, of course, was the most important silencer of potential critics; national recognition and respect for Thomas Edison, principal advocate of the idea, was also a muzzle. To recognize the importance of the timing and spokesman, one need only imagine the chilly reception that would have met, say, an Admiral Bradley Fiske if he had petitioned Congress in 1913 for 1.5 million dollars—or even half that much—for a sparkling new Navy laboratory. Yet, when Edison made the same request 2 years later, the House Appropriations Committee accorded him a standing ovation. Even at this time, the combination of champion and cause had to be just right. As Alex Roland relates in his forthcoming history of the National Advisory Committee for Aeronautics, the efforts to have Congress approve an aeronautical research laboratory during the same period were filled with frustrations and disappointments.[10]

After the initial approval, the details of the story of NRL's birth relate largely to political wrangling among the individuals involved over what the location and policy of NRL should be and who should have the power to decide them. Like most political arguments, this one turned partly on differences on issues and partly on differences in personalities. In the long run, however, more important than the particulars of the dispute was the clock ticking as the dispute played itself out. When, with the war over, Admiral Smith finally got approval from Secretary Daniels for construction in Washington, NRL was quite different from what it would have been if it had opened its doors in 1917. It was now more war orphan than war hero. With the Naval Consulting Board dispersed, uniformed officers were left holding the power to make the institution a reality. But there were no standing ovations from Congress for Navy laboratories in 1922, when NRL's director went to ask for operating funds. Even as workmen were finishing the wiring in laboratory buildings, legislators bickered over whether to allocate enough money to turn on the power.

In having a beginning so closely tied to the politics of war, NRL is hardly uncommon. If you trace the background of any Navy research-and-development facility, you are likely to find that it too had a wartime birth.[11] Equally common are the effects of political maneuvering and strong personalities. NRL's beginning is unique only in detail, not in form.

Invention and Development

Several years ago, Professor Thomas Hughes, a leading historian of technology, wrote,

> Because the history of technology is a recently cultivated field of scholarly activity, not many of its critical research problems have been identified. As research and reflection continue, however, problems will emerge and in some cases will be identified as critical ones worthy of the attention of a number of scholars over a considerable period of time....It seems likely that the nature of technological change, a subject for study comparable to that of the scientific method [in the history of science], will be identified as a critical problem for the history of technology. Technological change will probably prove to be more complex and difficult to define than the scientific method. For this reason, in order to study it, it may prove advisable to break it down into subcategories. One set of categories already widely used in discussing this

[10]Roland, *op. cit.* (note 3).

[11]Booz-Allen and Hamilton, Inc., *op. cit.* (note 8); Albert B. Christman, *Sailors, Scientists, and Rockets* (Washington: Department of the Navy, 1971).

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> process includes invention, research, development, and innovation. Nevertheless, definitions of these still complex and difficult subcategories are numerous and differ considerably.
>
> Among these phases of technological change, development has received the least attention. Many popular books have been written about invention, because it is an activity that appeals to the imagination. Economic historians have cultivated the study of innovation, for they associate innovation with the bringing of new machines, devices, and processes onto the market. Development, in contradistinction, has been neglected as a research problem, probably because it lacks the presumed excitement of invention and seems to lack the general social and economic significance of innovation.
>
> Yet those of us in the history of technology who have studied the process of technological change have found that those populating the world of technology—inventors, engineers, appliers of science, and entrepreneurs—give much of their time and resources to an activity that they label development, even though they do not clearly define it.[12]

By covering the progression of the radar project from initial ideas to the production of standard Navy equipment, this study encompasses both its invention and development phases. Consistent with the point of view in Professor Hughes's comment, I have endeavored to explain not only how the ideas fundamental to radar were conceived at NRL but also how they were then elaborated, exploited, and shaped into a workable system. And, as Hughes indicated, the study has shown that development, no less than invention, is complex and difficult.

Specifying the difference between invention and development phases, as Hughes points out, is difficult. Precise definitions that will apply to every case will probably never be devised. Among the best working descriptions are those given by Jewkes, Sawers, and Stillerman in *The Sources of Invention*:

> A useful working distinction can be made between "invention" and "development." Just as a distinction is made between science and technology, so technology itself can be divided into these two parts. Invention is something which comes before development. The essence of invention is the first confidence that something should work, and the first rough tests that it will, in fact, work....
>
> Development is a term which is loosely used in general discussion to cover a wide range of activities and purposes, but all these activities seem to satisfy three conditions. One, development is the stage at which known technical methods are applied to a new problem, which, in wider or narrower terms, has been defined by the original invention. Of course, it may happen that in the course of development a blockage occurs, existing technology may provide no answers, and then, what is strictly another invention is called for to set the ball rolling once more. Two, and consequentially, development is the stage at which the task to be performed is more precisely defined, the aim more exactly set, the search more specific, the chances of final success more susceptible to

[12]Thomas P. Hughes, "The Development Phase of Technological Change," *Technology and Culture* 17 (July 1976), pp. 423 and 424.

> measurement than is true at the stage of invention. Invention is the stage at which the scent is first picked up, development the stage at which the hunt is in full cry....Three, development is the phase which in commercial considerations can be, and indeed must be, more systematically examined, the limits of feasibility imposed by the market are narrowed down. As one moves from invention to development the technical considerations give way gradually to the market considerations.[13]

In the case of military research and development, the "market considerations" mentioned here are largely replaced by "mission considerations." The principal concern, that is to say, is not whether a military device will be salable or profitable, but whether it will do the job for which it was designed reliably and effectively. Cost is obviously a factor in design and procurement, but a much less important one than for commercial products.

The invention phase of radar in the Navy, in my estimation, lasted from 1922 until June 1936, that is, until the end of Chapter 6 as the story is written here. The termination of the phase was neatly defined by the series of demonstrations of working radar equipment that were held at the time and also by the lengthy report Robert Page wrote on the subject, which is reproduced here as Appendix F. One might argue that the invention of radar was complete in 1934, when Page first assembled and tested his crude pulse equipment. This, however, would overstate what had been done by that time. Page was then able only to verify a concept, not the principles of a working device. Not until 1936 was there any real certainty equipment had been created that could measure and display clear radio echoes from distant objects.

In my account of the invention phase, I attempted to explain the various factors involved in the origin of the ideas basic to radar. Most important were the capabilities of the individuals involved: Robert Page, Leo Young, Robert Guthrie, and A. Hoyt Taylor. These depended on innate talent, training, and experience. The historian cannot actually specify what causes a mind to be inventive; he can only describe those factors in an individual's life that are related to his talent. In my depictions of these key individuals, this is all I attempted to do.

Other important factors in the invention of radar were the characteristics of the environment in which the work was done: the availability of high-quality equipment that could be borrowed or procured at no cost within NRL, the community experience at NRL with high-frequency radio components and research techniques, and the clear and continual goal of producing new technology for the benefit of the Navy—a goal which allowed little chance for researchers to strike out on interesting but irrelevant paths. The pattern of funding also had a significant effect on establishing the size and scope of the program. The importance of the boost of the direct Congressional appropriation that brought Page the assistance of Guthrie in late 1935, for example, is hard to overestimate.

The interconnection of technical developments and administrative and institutional factors became even more pronounced when radar development began. Jewkes and his coauthors wrote, "As one moves from invention to development, the technical considerations give way gradually to market [or mission] considerations." That is, nontechnical factors begin to predominate. The focus is no longer on whether something will work but, given that it will work, what should ensue. What kind of development should be made? Who should undertake it? How should the program be organized? In the radar project, these and related questions now came to the fore. Answering them meant that higher levels of administration in the Navy became heavily involved in making the decisions. The Bureau of Engineering, to take the principal example, changed from an uninvolved sponsor of the project to an

[13]John Jewkes, David Sawers, and Richard Stillerman, *The Sources of Invention*, 2nd ed. (New York: Norton, 1969), pp. 28 and 29.

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active manager. It ordered prototype development and then sea trials. Production contracts followed. Officials in the office of the Chief of Naval Operations and the office of the Secretary of the Navy began closely monitoring the development under the bureau and occasionally acted to modify the program or change its priority. Entering the development phase, then, meant not only that technical tasks changed from evaluating and testing a set of new ideas to building a prototype of a reliable piece of naval equipment, but also that the management of the project changed. The difference related both to the evolving technical character of the program and to its growing importance and visibility in the Navy.

One part of the development phase involved the focused effort required to produce the first prototype of operational equipment. A second part involved diversity—the evolution of radar into a field of investigation. As was emphasized in Chapter 8, radar was less a particular device than a technique that could lead to a spectrum of devices. Soon after the practicality of the radar principal was proven, a variety of projects that would yield different types of equipment were under way. As suggested in the quotation from Jewkes and his colleagues, what ensued was not a phase of routine engineering but rather a period in which development was mixed with continued invention. Basic principles had been discovered, but wholly new components had to be devised, new circuits created, and new antennas designed. A major invention is often like a stone hitting an undisturbed pond: the effect spreads out in all directions. The limits to how far it is propagated are usually external. They come more from restriction of resources or the guidance imposed by management than they do from limitations inherent in the laws of science.

Effect of Management

The central goal of this study was to assess the development of radar in its institutional and historical context. Careful attention to the record of management action was crucial, for I knew that the effect of contextual forces on the radar project should appear most clearly there. Consequently, when I conducted my research, I was particularly interested in examining how the administration of the project related to its progress.

If one approaches the development of radar from the present and looks for this effect, it is easy to expect too much. There was no system in the 1920s and 1930s comparable to the complex procedures of research administration and planning of recent years. Management was relatively informal—for two principal reasons. First, NRL was small. Employment numbered just over 200 in 1936 and just over 300 in 1940. Only during the war years did the employment jump to the thousands—increasing to over 4,000 in 1946. In the 1930s, managers could be personally acquainted with virtually every employee, and there was a less obvious need than there would be later for a formalized mode of operation.

The second reason for the informality is that no elaborate management schemes had been developed anywhere for the process of research administration. No major laboratory was run under the guidance of extensive, formalized management procedures. To be sure, planning, reporting, and financing all went on, but procedures were much less explicit and rationalized than they would be in later years. Therefore, one finds no long-range plan for the radar project in the 1930s that details what expectation managers had about its future. Nor were there 3-year or 5-year forecasts of budgetary requirements, personnel needs, or expected technical progress. As is shown by some of the memoranda I quoted, written plans were composed, but only irregularly. Moreover, budgetary decisions were related to technical progress only indirectly. That is, although both the military directors of NRL and the top civilian managers certainly had technical progress in mind as they projected costs or divided money between competing projects, their decisions depended on the unarticulated personal judgment of NRL division chiefs rather than on formal presentations and deliberations.

Viewed from the current era of carefully managed research and development, therefore, the period of the 1920s and 1930s seems immature in its methods. Yet when viewed chronologically and

historically, the perspective taken in this study, what is noteworthy is not how little management guidance there was but how much. This was, after all, an early, experimental period of mission-oriented research. In the development of radar, one sees not only the evolution of a new technology but also the evolution of institutional patterns for fostering innovation. In my account, therefore, I concentrated on trying to show the way these patterns appeared.

To introduce my analysis of how management operated, I related, in Chapter 4, the guiding policies of NRL in general. From that point on, I was concerned primarily with how the radar project was administered in particular. As I noted, the personal, internal judgment of Laboratory leaders proved significant. The role of Hoyt Taylor in determining the fate of the project was obvious many times, especially during the crucial early phases. Leo Young played a similar role on the next lower administrative level.

These two individuals remained important constantly, but the involvement of the principal sponsor, the Bureau of Engineering, fluctuated. A modicum of bureau support allowed the project to begin. It progressed through the invention phase largely under the influence of benign neglect. Once practical equipment proved feasible, however, the bureau quite actively pushed the project to fruition. The way limits were placed on NRL as it carried out this task had a determining effect on how the project progressed and, equally significant, on what possible applications were not considered.

Around 1940, intra-Navy influences on the radar project began to be supplemented by effects from international developments. Indeed, it was quite fortuitous, from a historical standpoint, that the Tizard mission came just as the Navy's first radar equipment was being produced, for the mission led to a series of events that give a fascinating perspective on the management of NRL's radar program. Here was a direct comparison, at a critical time, of two independent developments of similar technologies. Prior to the exchange, leaders of NRL had stated their expectations of what it would bring. The historian may compare these to records of the meetings and what actually took place. One important point that emerges from the documents is that there was as much concern with how the British accomplished what they did as there was with what they had done—as much concern with organization and management as with technical progress.

The disclosures of the Tizard mission alone would have led to significant change at NRL. But they coincided with the advent of the National Defense Research Committee and a growing national emphasis on expanding military capabilities. The confluence of events required a difficult sorting out of institutional roles. This was a particularly important episode in my story, because it was here that the two major themes I had developed throughout the text—the progress of the radar project and the continual debate over the role of NRL within the Navy—merged. Both had to be resolved together.

EVALUATING THE STORY

On a particular level, the story I told answered some discrete historical questions: How was NRL created? What was its basic research policy? When was the radar idea conceived and when did the Laboratory begin developing it? Why was the early equipment designed as it was? What was the response to it by leaders of the operational forces? How did the Navy radar program relate to other, independent developments? How did private industry become involved? What brought the transformation of the radar idea into a field of technology and then the massive production effort it became during World War II? These are important queries, the responses to which record and explain one important aspect of how the Department of the Navy met its responsibility to maintain national defense in the years between World War I and World War II.

Yet, I also hope that this study went beyond such topics to touch on broader issues, issues not restricted chronologically. I hope that it captured some of the essential qualities of how people react to

the challenge and opportunities of scientific research and development—how their thoughts, wishes, and plans are generated and how these are shaped by the realities of their times. And finally, I hope that you, the reader, by experiencing vicariously the development of radar, have gained a deeper general understanding of the operation of mission-oriented research laboratories, institutions which are the major producers of technical innovation in modern America.

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Appendix A

This order established the initial operating policy of the Naval Research Laboratory.

GENERAL ORDER
No. 84

NAVY DEPARTMENT
Washington, D.C., 25 March 1922

SUBJ: REGULATIONS GOVERNING THE OPERATIONS OF THE EXPERIMENT AND RESEARCH LABORATORY

1. As provided in the Naval Appropriations Act approved 29 Aug. 1916, the Experiment and Research Laboratory is hereby established and placed under the Assistant Secretary of the Navy. The Laboratory shall be under the direction of a naval officer, not below the rank of captain, who will be designated "The Director of the Experiment and Research Laboratory" and be attached to the Office of the Assistant Secretary of the Navy.

2. There will be attached to the Laboratory staff an officer who will be designated as "Assistant Director" and who will have, under the Director, general charge of the work carried on in the Laboratory.

3. The Laboratory staff shall consist of such officers as may be detailed from time to time or assigned to work on special problems, civilian scientific assistants as provided for by law, and such technical assistants as may be employed.

4. Employees of the Experiment and Research Laboratory in any capacity shall, in addition to the regular oath, be required to take the following oath:

> I, _____, do solemnly swear (or affirm) that I will not by any means divulge nor disclose any information that I may obtain or acquire by reason of my connection with the Experiment and Research Laboratory unless authorized to do so in writing by the Assistant Secretary of the Navy or required to do so by Court of Justice in due course of Law.

5. All correspondence both to and from the Laboratory shall be sent through the Director.

6. Before beginning work on any problem (a) it shall be fully considered by a board, of which the Director shall be the senior member and consisting of such representatives of the staff and bureaus of office concerned as may be desirable, as members, (b) a preliminary estimate of cost prepared, and (c) when the estimated cost is $2,500 or more, approved by the Assistant Secretary of the Navy, or when the estimated cost is less than $2,500 approved by the Chief of the bureau for which the experimental or research work is to be performed.

EDWIN DENBY
Secretary of the Navy

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Appendix B

Navy General Order 223 of November 3, 1931, transferred the Naval Research Laboratory from the Office of the Secretary of the Navy to the Bureau of Engineering. This is a copy of that order, in the form of its verbatim reissue on May 13, 1935, as General Order 41.

GENERAL ORDER*
No. 41

NAVY DEPARTMENT
Washington, D.C., May 13, 1935

REGULATIONS GOVERNING THE OPERATION OF THE NAVAL RESEARCH LABORATORY

1. The administration of the Naval Research Laboratory is hereby placed under the cognizance of the Bureau of Engineering.

2. There shall be attached to the laboratory an officer who will be designated "director" and who will have, under the direction of the Chief of Bureau of Engineering, general charge of the work carried on in the Laboratory.

3. The laboratory staff shall consist of such officers and men as may be detailed from time to time or assigned to work on special problems, civilian scientific assistants as provided by law, and such technical and mechanical assistants as may be employed.

4. All persons employed or on duty, at the Naval Research Laboratory in any capacity shall, in addition to the regular oath, be required to take the following oath:

> I, _____, do solemnly swear (or affirm) that I will not by any means divulge nor disclose any information that I may obtain or acquire by reason of my connection with the Naval Research Laboratory unless authorized to do so in writing by the Secretary of the Navy.

5. All correspondence both to and from the laboratory shall be sent through the Bureau of Engineering.

6. The laboratory will undertake such research and development work as may be authorized and financed by various bureaus and officers of the Navy Department, and the director shall issue necessary orders for its execution. Work for other Government departments will be undertaken in accordance with existing instructions.

7. The laboratory staff and its facilities are available to assist officers and men to put into practical form ideas for improvement of naval material. They are urged to submit such ideas to the bureau or office having cognizance for consideration as to the desirability of further development.

/s/ CLAUDE A. SWANSON
Secretary of the Navy

*This is a re-issue (verbatim) of General Order #223, dated 3 Nov. 1931. (GO 223 placed NRL under BuEng)

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Appendix C

This order transferred the Naval Research Laboratory from the Bureau of Engineering back to the Office of the Secretary of the Navy, where it had been situated for the first years of its operation.

GENERAL ORDER
No. 124

NAVY DEPARTMENT
Washington, D.C., September 14, 1939

ADMINISTRATION OF NAVAL RESEARCH LABORATORY

1. General Order No. 41 is hereby canceled.

2. The Naval Research Laboratory, as now constituted, is established as an independent unit under the Secretary of the Navy.

/s/ CHARLES EDISON
Acting Secretary of the Navy

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Appendix D

This order laid the foundation for a plan to reorganize scientific research in the Navy under the leadership of the Director of the Naval Research Laboratory.

GENERAL OFFICE
No. 130

NAVY DEPARTMENT
Washington D.C., December 8, 1939

COORDINATION OF RESEARCH IN THE NAVY

1. In order to emphasize research in the Navy, the Secretary of the Navy has decided, after considerable investigation, to effect a higher degree of coordination than exists at the present time.

2. It is therefore directed that each of the material bureaus, viz., Ordnance, Aeronautics, and the bureaus of Engineering and Construction & Repair, considered as one unit, designate an officer in their respective bureaus who shall be the head of a section devoted to science and technology. The officer so designated shall be a liaison officer with the Naval Research Laboratory and shall be a member of the Navy Department Council for Research. The Director of the Naval Research Laboratory (Technical Aide to the Secretary of the Navy) shall be the Senior Member of this Council and is empowered to call meetings. The Executive Officer of the Naval Research Laboratory will be ex-officio the secretary of the Council. The Council will recommend to the Secretary of the Navy action in respect to research problems, their assignment and measures to be taken to finance them.

3. The Director of the Naval Research Laboratory (Technical Aide to the Secretary of the Navy) will keep the Secretary of the Navy informed of the progress of research problems. To enable him to discharge this function, each material bureau is directed to furnish the Senior Member of the Council quarterly a list showing the status of all scientific and technological problems being undertaken under the cognizance of the bureau.

4. Those duties of the Technical Division, Office of the Chief of Naval Operations, which are concerned with research and invention are hereby transferred to the Office of the Secretary of the Navy and placed under the Administration of the Director of the Naval Research Laboratory (Technical Aide to the Secretary of the Navy).

/s/ CHARLES EDISON
Acting Secretary of the Navy

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Appendix E

This order transferred the Naval Research Laboratory from the Office of the Secretary of the Navy to the Bureau of Ships, where it remained until the Office of Research and Inventions, the precursor of the present Office of Naval Research, was established in 1945.

GENERAL ORDER
No. 150

NAVY DEPARTMENT
Washington, D.C., July 12, 1941

COORDINATION OF RESEARCH AND DEVELOPMENT

1. General Orders Nos. 124 and 130 are hereby canceled.

2. The Naval Research Laboratory, as now constituted, is hereby placed under the cognizance of the Bureau of Ships.

3. In order to secure a more complete measure of cooperation and coordination in matters of research and development and to provide an agency for consideration of such matters, there is hereby established, in the Office of the Secretary of the Navy, the Naval Research and Development Board, with membership, functions, and procedure as follows:

(a) MEMBERSHIP—The Naval Research and Development Board shall consist of the Coordinator of Research and Development, as Chairman, with representatives of the Chief of Naval Operations, Bureau of Ships, Bureau of Ordnance, Bureau of Aeronautics, and Bureau of Yards and Docks.

(b) FUNCTIONS—The Board shall recommend to the Secretary of the Navy action in respect to research and development matters.

(c) PROCEDURE—The Coordinator of Research and Development will call the meetings of the Board, prepare its agenda, and transmit its findings and recommendations.

4. The Coordinator of Research and Development shall be a civilian scientist or a Naval Officer. The Assistant Coordinator shall be a Naval Officer who will assist the Coordinator and act as his deputy.

5. The duties of the Coordinator are as follows:

(a) Advise the Secretary of the Navy on matters of Naval research and development.

(b) Provide information to Bureaus and Offices of the Navy regarding research of outside agencies.

(c) Cooperate with all agencies of research and development with a view to coordination of effort.

(d) Arrange for suitable representation of the Navy on outside Boards, Committees, and Councils dealing with research.

(e) Provide a Progress Section and a Planning Section for formulation of coordinated programs of research.

(f) Supervise the handling of correspondence regarding suggestions and inventions brought by individuals to the Office of the Secretary of the Navy.

FRANK KNOX
Secretary of the Navy

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Appendix F

This letter was the first comprehensive report written on the invention of radar at the Naval Research Laboratory. It shows the level of development and understanding as of June 1936. The document is in File S-S67-5 #1, box 4, records of the Naval Research Laboratory, Secret series (now Unclassified), record group 19, National Archives Building.

NAVAL RESEARCH LABORATORY
ANACOSTIA STATION
Washington, D.C.
11 June 1936

From:	Director.
To:	Chief of the Bureau of Engineering.
Subject:	Radio - Use of Radio to Detect Enemy Vessels and Aircraft - Special Report on. (Bu. Eng. Prob. W5-2S)
Reference:	(a) Bu. Eng. 3rd end. C-A13-2(2-14-W9-4/18) of 19 March 1936 with enclosures to NRL
Enclosure:	(A) One block diagram [omitted in this copy]. (B) One table. (C) Plates 1 to 5 [omitted in this copy].

1. In accordance with paragraph 1 of reference (a), a special report on the use of radio to detect enemy vessels and aircraft is herewith submitted, together wth recommendations as to patent applications.

2. At the time the work was started at the Laboratory on the method of detection of enemy vessels and aircraft now under investigation, careful consideration was given to all the methods then known to give promise of yielding the desired results. As the work progressed and other methods were proposed, the same consideration was given each new method. In every case first consideration was given to the nature and reliability of the results promised by each method, if successful, and secondary consideration to the engineering difficulties presented by each method.

3. The various methods that have been considered are outlined in the enclosed Table 1, together with comments on each method relative to the nature of response, information obtainable if successful, major engineering difficulties, and remarks pertaining to the usefulness to be expected. All comments refer to the use of these methods to detect vessels, aircraft, or other objects. No consideration is given to their use for determining altitude of aircraft, in which application some may be better adapted. From this table it may be seen at once that concerning the nature and reliability of results promised, if successful, the discontinuous signal consisting of short pulses recurrent at a fixed frequency is overwhelmingly superior to all other methods. Indication is visual, continuous, fully automatic, direct reading, unaffected by signal variation, never false or ambiguous, shows all reflecting objects in its range

separately and simultaneously, relates each reflecting object unerringly to its own distance, and can give no indication of a reflecting object unless that object actually exists at the distance shown by the indication, while the accuracy of measurement is inherently greater than that of any other known method.

4. As to the engineering difficulties involved, this method appeared to be at a disadvantage, the known short time constants required being shorter by two to three orders than anything achieved at the time, and the reception of reflected energy from airplanes in the absence of a direct signal to produce an interference pattern as yet unestablished. However, the matter of isolation of transmitter and receiver as required by other methods is not as simple as may at first be apparent. The two signals to be matched differ in power level by 10^{12} to 10^{16} in the vicinity of the transmitter. This means that the voltage gradients due to the direct signal are one million to one hundred million times the voltage gradients due to the reflected signal. The balancing of these two signals in a receiver to obtain, for example, an indication of their relative phase requires a degree of isolation of transmitter and receiver so far achieved only by physical separation. Every attempt to decrease the physical separation of transmitter and receiver has resulted in reduction of range.

5. Because of its superiority, the pulse-echo method was decided upon and for the past two and a half years the Laboratory has worked on this method when time could be spared from other problems more immediately pressing. The major engineering difficulties have been pretty well disposed of, as will be seen in the description following, and airplanes have been located and "tracked" at distances up to 25 miles. The procedure consists in determining the distance to the plane by measuring the time required for a radio frequency signal of extremely short duration to travel from the transmitter to the airplane and back to the receiver, and getting the direction of the plane relative to the observing station by means of sharply beamed radiators and collectors. The transmitter and receiver are in all cases located at the same station.

6. The system is described with reference to Figure 1 [omitted]. The transmitter (1) is capable of radiating radio frequency energy in extremely short wave trains. These wave trains are regularly recurrent and their frequency of recurrence is controlled by energy taken from the audio oscillator (2), through buffer amplifier (3) and synchronizing amplifier (4). Energy from the same audio oscillator is taken through buffer amplifier (5) to synchronized sweep circuit (6), which sweep circuit provides a horizontal time axis on cathode ray oscilloscope (7). The receiver (8) picks up the directly radiated wave trains from the transmitter and delivers them in the form of rectified pulses to the vertically deflecting plates of the cathode ray oscilloscope , where they appear as one or more stationary vertical lines or peaks on the synchronized time axis and mark the position of zero distance. The receiver also picks up energy returned from various reflecting objects, which energy is amplified and likewise applied to the vertically deflecting plates of the oscilloscope. Since time is required for the transmitted wave train to travel from the transmitter to the reflecting object and back to the receiver, the vertical lines or peaks appearing on the oscilloscope and corresponding to the reflected energy will appear after the directly received wave train has ceased. During this time interval the electron tracing-stream of the oscilloscope will have moved vertically under the influence of the sweep circuit, so that the reflected pulse will appear horizontally displaced from the transmitted pulse. The magnitude of this horizontal displacement gives a measure of the distance to the reflecting object.

7. Directive antennae were placed on the transmitter only, and also on both transmitter and receiver. By means of the directivity of these antennae, bearings were obtained on the reflecting object.

8. The velocity of propagation of radio frequency energy in space is equal to the speed of light, which is 186,000 miles per second. The time intervals involved in this process are therefore extremely short, one mile in distance measurement corresponding to about one one-hundred-thousandth of a second time lapse. In the range of wave lengths being used for this purpose, airplanes do not reflect regularly as a mirror reflects light, since the largest dimension on the airplane is not more than a few wavelengths. Radio frequency energy re-radiated from the airplane is therefore not in one direction

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only, whose angle of reflection is equal to the angle of incidence, but in many directions determined by the radiating characteristics of the airplane considered as a secondary transmitter. While this makes it possible to get "echoes" from airplanes in almost any orientation, it reduces to a small fraction of the total re-radiated energy the amount of energy re-radiated in any one direction. As is well known, the fraction of the transmitted energy intercepted by an airplane is theoretically inversely proportional to the square of the distance from transmitter to airplane. In exact analogy, the fraction of the re-radiated energy picked up by the receiver is theoretically inversely proportional to the square of the distance from the re-radiating airplane to the receiver. Hence, when the transmitter and receiver are located at the same observing station, as in the tests conducted here, the fraction of the transmitted energy intercepted by the receiver by re-radiation from an airplane varies inversely as the fourth power of the distance from the observing station to the airplane.

9. In view of these considerations, the equipment here described must meet the following requirements:

(a) Both transmitter and receiver must have exceedingly short time constants throughout, preferably of the order of one-millionth of a second.

(b) The transmitter must radiate at a very high power level in order to produce a detectable signal at the receiver by re-radiation from a distant airplane. A peak power of around ten kilowatts is desirable.

(c) The receiver must have a very low threshold of sensitivity, preferably around a tenth of a microvolt.

(d) The receiver must have very high gain in order to build up faint "echo" signals to the level required for indication on a short time constant indicator, such as a cathode ray oscilloscope. This gain should be of the order of ten million in voltage, preferably all at radio frequency.

(e) The output stages of the receiver must be capable of delivering high voltages without saturation. A saturation level between 100 and 200 peak volts output is desirable.

(f) The indicating device must be capable of producing an identifiable indication of a desired signal in the presence of random interfering signals of several hundred times the desired signal voltage.

(g) The receiver must be immune to damage from the resonant signals of many volts due to the wave trains received directly from the nearby transmitter.

(h) The receiver must recover to full sensitivity very quickly after the saturating signal from the transmitter has ceased. This recovery should not require more than five or ten micro-seconds at most.

(i) Both transmitting and receiving antennae should be beamed, not only for indicating the direction to the reflecting object, but also for increasing the field strength at the airplane and the re-radiated signal at the receiver, and for reducing the amount of reflection from large nearby objects.

10. While the equipment used in recent tests does not meet these requirements as fully as is desirable and as now believed practicable, it is good enough to demonstrate at 25 miles what may be accomplished at greater distances with more refined equipment. The first transmitter tried was a master

oscillator power amplifier type with the amplifier grid keyed by means of a very high speed electronic key. When it was found that the continuous signal from the master oscillator would be far above the desired threshold signal for the receiver and therefore limit the range of the system, the same electronic keyer was applied to a highly over-biased transmitting oscillator. The length of wave train with this transmitter was reduced to about ten micro-seconds. The power output, however, was not sufficiently high. An attempt was made to shock excite an antenna by high level direct current pulses. This method was completely unsatisfactory. A self-interrupting transmitting oscillator was then tried with gratifying results. The wave train length finally obtained was about seven micro-seconds, the peak power about 3.5 kilowatts. The interruption frequency is also easily synchronized from a sine wave control voltage and may be synchronized on submultiples as well as on the fundamental of the control wave. Plate 1 is a photograph of this transmitter as used in tests at 28 megacycles. The transmitting antenna array is shown in Plate 2. [All photographs are omitted.]

11. The receiver time constant, under linear conditions, is of the order of one micro-second. It recovers from full saturation to full sensitivity in about four micro-seconds. The threshold of sensitivity is about 0.2 microvolts. The gain is not well known, but has been measured under varying conditions from five million to twenty-five million, the maximum stable gain depending on the degree of electrical isolation of input and output circuits. There is no audio or low-frequency amplification. The receiver saturates sharply at about 150 peak volts output. The receiver and oscilloscope together are capable of indicating the position of distant airplanes through severe static and other disturbances almost as clearly as without interference. The simultaneous fulfillment of all these conditions in one receiver represents an achievement new to the art of radio receiver design. This receiver, together with the auxiliary apparatus, is shown in Plate 3, the receiving antenna in Plate 4.

12. The other component parts of the apparatus constitute nothing new or unusual in themselves, with the exception of the sweep circuit. As this was an independent development and is being separately reported on in connection with the centimeter wave investigation, it will not be described here.

13. Photographs of the indication given by the cathode ray oscilloscope are shown in Plate 5. In (a), the receiver sensitivity is reduced so low that only the pulse received directly from the transmitter is shown. This is represented by the sharp vertical image that appears twice on the horizontal base line. In (b), the receiver gain is increased sufficiently to show only the nearby ground reflections. These are so numerous and close together as to merge into one continuous signal. In (c), the receiver gain is further increased so as to show more distant ground reflections, some of which are sufficiently isolated to stand out by themselves. The rest of the picture shows additional vertical images which represent "echo" signals from airplanes. For a given peak power radiation and receiver sensitivity, the size of these reflection images depends on the re-radiating characteristics of the airplane, the distance of the plane from the observing station, and the accuracy with which the airplane is centered in both transmitter and receiver beams. The distance of the plane from the observing station is indicated by the horizontal position of the reflection image relative to the two large peaks from direct radiation. The more distant the reflecting object, the further displaced to the right is the reflection image. Some of the reflection images shown in Plate 5 are from airplanes 15 miles away. In some cases, two or more airplanes are shown simultaneously at different distances from the observing station.

14. The development described in the foregoing paragraphs may have many applications. Some of these possible applications are suggested in the following outline:

A. Apparatus located on land may be used for

1. Area protection from aircraft.
2. Coast line or boundary protection from vessels and aircraft.
3. Apprehension of boats or airplanes crossing boundaries illegally.

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4. Range finding.
5. Tracking of moving objects for other than military purposes.
6. Long range surveying.

B. Apparatus located on shipboard may be used for

1. Protection against enemy vessels and aircraft for defense.
2. Location of enemy vessels and aircraft for offense.
3. Tracking own planes for information and guidance from base ship (substitute for homing device).
4. Maintaining fleet formation in fog or at night.
5. Location of icebergs, other surface craft, buoys, protruding rocks and shore lines in fog or darkness.

C. Apparatus located on aircraft may be used for

1. Determining height above ground.
2. Revealing mountains through fog, haze or darkness.
3. Determining ground speed.
4. Warning of approach of other aircraft.

15. Of particular interest in connection with A-5 above is the possibility of tracking meteorological balloons without the necessity of placing transmitters on these balloons. The re-radiating efficiency of the balloon may be enhanced by a resonant antenna on the balloon, and temperature and humidity indications on the balloon may be relayed to ground through timed momentary tuning or detuning of the re-radiating antenna.

16. Several patents have been issued covering systems somewhat analogous to that described in paragraph 5 above. The circuit diagrams and the assumptions naively made in the accompanying explanations in the patents indicate rather forcibly that these patents are purely "paper" patents on ideas the inventors had not reduced to practice. Reference is particularly to patents numbered 1,924,156, 1,924,174, 1,979,225, and 1,982,271. Since this Laboratory has developed the highly specialized apparatus necessary to successful operation of this system, which specialization has not been indicated in any existing patents and, further, since this Laboratory has applied said apparatus to the measurement of distance as described above and has demonstrated such measurement to certain individuals, it is requested that patent application be prepared in the names of L.C. Young and R.M. Page jointly covering the fundamental principle of operation on the basis of reduction of that principle to actual operation.

17. To assist in the preparation of such a patent application, the following sample claims are suggested:

(a) The method of determining the distance between an observing and a reflecting surface for distances less than 50 miles comprising radiation of regularly recurrent wave trains of radio frequency energy, the duration of each wave train being very short and preferably less than ten micro-seconds, reception of these wave trains as reflected from said reflecting surface, and measurement of the time lapse between radiation of each wave train and reception of reflected energy of the same wave train whereby said distance may become known.

(b) The method of determining the distance between an observing station and a reflecting surface for distances less than 50 miles comprising radiation of regularly recurrent wave trains of radio frequency energy, the duration of each wave train being very short and preferably less than ten micro-seconds, reception of these wave trains as reflected from said

reflecting surface, synchronization of a time-function comparator device with the recurrent periodicity of the transmitted wave trains, and application of both direct and reflected wave trains to said comparator device whereby the time lapse between radiation of each wave train may be indicated and said distance therefore known.

(c) The method of determining the distance between an observing station and a reflecting surface for distances less than 50 miles comprising radiation of regularly recurrent wave trains of radio frequency energy, the duration of each wave train being very short and preferably less than ten micro-seconds, reception of these wave trains as reflected from said reflecting surface, synchronization of the time axis circuit of a cathode ray osciloscope with the recurrent periodicity of the transmitted wave trains, and orthogonal projection of both direct and reflected wave trains on said synchronized time axis of said oscilloscope whereby the time lapse between radiation of each wave train and reception of reflected energy of the same wave train may be indicated and said distance therefore known.

(d) The method of determining the location in space of a reflecting object comprising directive radiation of regularly recurrent wave trains of radio frequency energy, reception of these wave trains as reflected from said reflecting object, and measurement of the time lapse between radiation of each wave train and reception of reflected energy of the same wave train, whereby both direction and distance of said reflecting object relative to the observing station may become known.

(e) The method of determining the location in space of a reflecting object comprising radiation of regularly recurrent wave trains of radio frequency energy, directive reception of these wave trains as reflected from said reflecting object, and measurement of the time lapse between radiation of each wave train and reception of reflected energy of the same wave train whereby both direction and distance of said reflecting object relative to the observing station may become known.

(f) The method of determining the location in space of a reflecting object comprising directive radiation of regularly recurrent wave trains of radio frequency energy, directive reception of these wave trains as reflected from said reflecting object, and measurements of the time lapse between radiation of each wave train and both direction and distance of said reflecting object relative to the observing station may become known.

18. In view of the bearing of the subject system of enemy vessel and aircraft detection and long distance range finding on national defense, it is recommended that all possible measures be taken to protect the Government in the use of said system compatible with the secret status of the problem.

19. It is hereby certified that the originator considers it to be impracticable to phrase this document in such a manner as will permit a classification *other than* secret.

H.M. Cooley

Table I

No.	Type of Signal	Nature of Response	Information obtainable (if successful)	Major Engineering Difficulties	Remarks
I.	A. Continuous radiation 1. Unmodulated	"Beats" Frequency to be measured.	Detection of presence of moving objects. Rate of change of range.	Isolation of transmitter and receiver.	Determination of direction of reflecting object made difficult by necessity of balancing direct and reflected signals.
II.	2. Modulated a. Fixed modulation frequency	Two continuous low frequency signals synchronous but not in phase. Phase difference to be measured.	Distance and approximate direction.	Same as above.	Same as above. Presence of two or more reflections simultaneously would render distance determination impossible. Beats would destroy indication.
III.	b. Variable modulated frequency 1. Amplitude modulated	Two continuous low frequency signals, synchronously to be adjusted to 180°.	Same as above.	Same as above.	Same as above. Indication not fully automated.
IV.	2. Frequency modulated	Beat note between two modulation frequencies. Frequency of beat note to be measured.	Same as above.	Same as above.	Same as I above. Does not require cathode ray oscilloscope.
V.	B. Discontinuous radiation 1. Short interruptions	Two apparently continuous signals each marked with timing knotches. Separation of timing knotches to be observed.	Same as above.	Same as above.	Same as I above.
VI.	2. Short pulses a. Variable pulse frequency b. Constant pulse frequency (1) Reflected pulse delayed relative to direct pulse.	Two separate pulses to be brought into synchronism by manual adjustment.	Same as above.	Same as above.	Same as I. Indication not fully automatic. Reflections may be present without being detected.
VII.	(2) Reflected and direct pulses not synchronized.	Two separate pulses, separation to be observed.	Distance and direction, both accurately.	Short time constants. High power, high sensitivity, quick receiver recovery from saturation.	Indication visual, continuous, fully automatic, direct reading, unaffected by signal variation, never false or ambiguous. Receiver and transmitter may be operated in close proximity, possibly on common antenna. Requires cathode ray oscilloscope.

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Appendix G

This is an extract from a letter from NRL to the Secretary of the Navy, October 4, 1940. The document is in file A8-3/EF 13, 1940, records of the Secretary of the Navy/Chief of Naval Operations, Secret section (now Unclassified), Operational Archives Branch, Naval History Division, Washington, D.C. The letter gives a general appraisal of the comparison of British and American radar at this time, a comparison summarized in the chart reproduced below.

S-A8-3(1) 10-4-40
Ser. No. 168

The following table lists the types of British RDF radio-ranging applications now in use or under intense development; and any existing American counterpart.

British RDF Applications			U. S. Navy Counterparts		
RDF Designation	General Characteristics	Approx. No. in use :	Designation	General Characteristics	Approx. No. in Use
CH System	Shore chain of RDF devices to detect approaching enemy aircraft; about 20-30 miles apart; range from 40 to 120 miles, depending on altitude of plane. New RDF is under development for this use.	26; 10 under construction for West Coast of England.	None*		
GL-1	Shore use. Gun-laying and searchlight director. Range 20-60 miles		None**		
GL-2	Improved GL-1		None		
GL-3	Shore use. Under development. For searchlight director only.				

British RDF Applications			U. S. Navy Counterparts		
RDF Designation	General Characteristics	Approx. No. in use	Designation	General Characteristics	Approx. No. in Use
279	Naval use. Long range warning and AA range-finder. Range as high as 200 miles.	75-100	CXAM	Shipboard use. Performance comparable to that of British 279.	6 now being installed in designated BB, CV, and CA's. From 5 to 15 others under order.
281	Naval use. Will replace 279.		None		
282	Naval use. Under development as pom-pom range-finder.		None		
284	Naval use. Main battery range finder.		None		
285	Naval use. Under development as high-angle AA range-finder.		None		
ASV-1	Aircraft use primarily, but some shipboard. Detection of convoys, enemy submarines, etc.		Radio pulse altimeter.	Aircraft use primarily, but may be used as range-finder.	1 now under development at NRL.
ASV-2	Improvement of ASV-1.		"		
ASV-3	Improvement of ASV-2.		"		

UNCLASSIFIED

S-A8-3(1)
Ser. No. 168

10-4-40

British RDF Applications			U. S. Navy Counterparts		
RDF Designation	General Characteristics	Approx. No. in use	Designation	General Characteristics	Approx. No. in Use
AI-1	Aircraft use. Air interception of other aircraft, primarily.		None		
AI-2	Improvement of AI-1.		"		
AI-3	Improvement of AI-2.		"		
IFF-1	Aircraft use. Identification of aircraft in air.		None		
IFF-2	Improvement of IFF-1.		None		

*Model CXAM equipments could be used on shore as is the RDF in Great Britain. The Army has approximately 120 radio ranging devices (some portable) on order for aircraft detection purposes. In general, it is better to employ specially designed radio ranging equipments for particular purposes.

**The Army has approximately 400 of counterpart equipments under procurement.

NOTE: The ranges of the various RDF applications vary widely with conditions. Range data are not complete because of the general newness of this phase of the radio art.

CH System (Shore). The British have a highly developed shore system (called CH) of RDF equipments and associated antenna arrays, which are arranged to look out to sea and thus to cover completely the eastern and southern coastlines of England and Scotland, and, to a much more limited extent, the western side of the island. Even at this early stage of development, the efficiency of this system on shore is so high that the British can quickly and accurately determine the range and bearing of enemy airplanes as they make approaches for raids; and, by a complete communication system, can bring into action the appropriate groups of defending fighter aircraft, batteries of searchlights, anti-aircraft gun batteries, or any combination of these.

GL (Gun-Laying and Searchlight Director). This application is used on shore in connection with the control of anti-aircraft guns and searchlights. By its use the range, bearing and elevation of an air target are obtained. The range attainable by this equipment is estimated to be approximately 20 to 60 miles.

ASV (Anti-Surface Vessel). This remarkable application of the RDF is primarily for use in aircraft, but is now being installed in all destroyers. Using the RDF principle of pulsed radio transmissions of perhaps one-half of a microsecond, the ASV is generally employed in crew-carrying aircraft as a type of altimeter for determining the presence at sea of enemy submarines, convoys, or men-of-war, or for locating coastlines or other well-defined geographical points. The range of this equipment varies greatly with conditions, being capable of effective employment at approximately 30 miles when a 30,000-ton ship is the target, and at 5 miles when a submarine is being tracked. Although quite complicated for use in an already crowded airplane, it has performed so creditably that many British pilots have grown to depend upon it under the many precarious conditions of flight.

AI (Air Interception). This device is used in British aircraft to determine the location of enemy airplanes, or even to find friendly units in the air. Many difficulties have been experienced in its development, but the British have already installed it in a large number of their two-seater airplanes, and are now planning installation in remaining aircraft which are capable of carrying two or more persons. The apparatus is designed to locate an object (aircraft) in the air not only in respect to direction and range, but also in respect to altitude. The range attainable by use of the AI is limited by the height above ground of the containing airplane; and varies between the probable values of 450 feet (the minimum) and 20,000 yards, depending in great part on the position of the target relative to the airplane. An observer is required for reading the cathode ray tube screen and sending the interpretation to the pilot. The potentialities of this device are great, especially under low visibility conditions of flight.

IFF (Identification of Friend or Foe). This application is still under intense development in Great Britain, but is nevertheless being supplied in large numbers as rapidly as the equipments can be built. This recognition device, designed for eventual employment in all aircraft, is essentially a transmitter-receiver which accepts, augments in distinctive fashion, and repeats back a signal which has been transmitted by another aircraft for purposes of identification. It is reported that some IFF equipments can be used to reply to signals transmitted by CH or GL apparatus on shore. As in all RDF applications, a cathode ray tube screen is employed, and thus the services of an observer would seem to be required, although it is conceivable that future developments will permit the IFF to be effectively used in a one-seater airplane.

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ARCHIVAL MATERIALS

NRL Records

The principal source on which this document relies are the archival records of the Naval Research Laboratory. Most of the records dated prior to 1942 are in record group 19 in the National Archives Building, Washington, D.C. Many of these files were previously classified, and the series is still organized in three parts: Secret, Confidential, and Unclassified. However, all these materials have been declassified and are open for general public use. The remainder of the records of the Laboratory, some dating from its origin in 1923, but most after 1942, are still under NRL's control and are stored either at the institution itself or, more frequently, at the Washington National Records Center in Suitland, Maryland. All of the materials at Suitland are in record group 181. Special permission from the Laboratory is required for access, but almost all documents through World War II have been declassified. As with most institutions, some significant historical records never became part of the official files of the Laboratory. A number of these have been collected and preserved by the NRL Historian and are located in his office.

Other Institutional Records (in alphabetical order)

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UNCLASSIFIED

Index

Adams, Charles F., 76
Admiralty Signal School, 147
Almy, E.D., 52, 63, 165
American Telephone and Telegraph Co., 6, 9, 11 111, 119, 121-126, 128, 140, 151, 154-155, 159, 175, 178-180
Antisubmarine warfare, 166-169
Appleton, E.V., 57
Armstrong, Edwin, 39
Austin, L.W., 14
Barnett, M.A.F., 57
Bawdsey Manor, 145-147
Bell Telephone, *see* American Telephone and Telegraph Co.
Bell Telephone Laboratories, *see* American Telephone and Telegraph Co.
Bendix Corp., 179
Bennett, E.L., 36, 47-49
Blackett, P.M.S., 142
Blair, William, 137-139
Boot, H.A.H., 148
Bowen, E.G., 146, 148, 150-151, 159, 178
Bowen, Harold G., 77, 91, 95, 129-135, 137, 150-151, 153, 155-156, 158, 161-165, 167, 169-176
Breit, Gregory, 57
Briscoe, R.P., 133, 151
Bullard, W.H.G., 42
Bureau of Aeronautics, *see* Navy, U.S., Bureau of Aeronautics
Bureau of Construction and Repair, *see* Navy, U.S., Bureau of Construction and Repair
Bureau of Engineering, *see* Navy, U.S., Bureau of Engineering
Bureau of Ordnance, *see* Navy, U.S., Bureau of Ordnance
Bureau of Ships, *see* Navy, U.S., Bureau of Ships
Bureau of Steam Engineering, *see* Navy, U.S., Bureau of Engineering
Bush, Vannevar, 155-157, 161, 168, 171, 173-174
Chalkey, Lyman, 131
Chief of Naval Operations, *see* Navy, U.S., Office of Chief of Naval Operations
Churchill, Winston, 149
Civil Service System, 176
Cockcroft, J.D., 150-151
Colpitts, E.H., 166-169
Committee of Imperial Defense, 145
Compton, Karl, 155-156, 158
Conant, James, 155-157
Congress, U.S., Naval Appropriations Subcommittee 78, 90-91, 119, 134, 164, 186
Cooley, H.M., 78, 91, 95, 97
Cox, Oscar, 155-156
Crawford, Arthur B., 66
Curts, M.E., 104
CXAM radar, 111, 124-125, 152, 175, 177-178
CXAS radar, 124
CXZ radar, 107, 109-110
Dahlgren, John A., 12
Daniels, Josephus, 19-21, 27, 29, 31-34, 186
Davis, Vincent, 132
Denby, Edwin, 34-35
Direction finder, high frequency, 61
Dorchester, 39
Dow, Jennings B., 95, 121
Dreller, Louis, 155
Dubose, W.G., 129
DuBridge, Lee, 173
Duplexer, radar, 100-101, 152
Dupree, Hunter, ii, 184
Eastman Kodak Co., 6, 9
Eccles, W.H., 56
Edison, Charles, 130-133
Edison, Thomas A., 9, 11, 74-76
  education, 7
  involvement of with Naval Consulting Board and Naval Research Laboratory, 17-19, 21-22, 25-29, 32-37, 46, 181, 186
  own scientific laboratories, 7-9
  Inc., 11, 31
Eitel-McCullough Company, 102, 111
Eliot, Charles, 6
Engineering Experiment Station, *see* Navy, U.S., Engineering Experiment Station
English, R.H., 162
England, Carl L., 66
FA radar, 124, 178
FB radar, 178
FC radar, 178

Fiske, Bradley A., 11, 14
Froude, William, 13
Gebhard, Louis A., 45, 58, 85, 104, 111
General Electric Company, 6, 9, 11, 25, 29, 119, 124, 126, 128, 135, 177, 179
Griffin, Robert, 27
Guerlac, Henry, 58, 80, 137
Gunn, Ross, 118-119, 151
Guthrie, Robert C., 45, 92-95, 99, 101, 119, 125, 139, 145, 172-173, 175, 188
Halligan, John, 70
Hayes, Harvey C., 82, 90, 95
Heaviside, Oliver, 56
Hershberger, William D., 138-140
Hertz, Heinrich, 53
High-frequency radio, *see* radio, high-frequency
Hill, A.R., 142, 148-149
Hooper, Stanford C., 36, 42, 50, 68, 70, 72, 77
Hopkins, Harry, 155
Hughes, Thomas, 186-187
Hulburt, E.O., 57
Hulsmeyer, Christian, 54
Hunsaker, Jerome C., 170-172
Hutchinson, Miller Reese, 21-22, 31, 73-76
Hyland, Lawrence, 61
Identification Friend or Foe equipment, 126, 179-180
IFF, *see* Identification Friend or Foe equipment
Incandescent lighting system, Edison's invention of, 8
Industrial research, *see* mission oriented research and development
Ionosphere, 57, 80-82
Isherwood, Benjamin, 11
Institutionalized research and development, *see* mission-oriented research and development
Jewett, Frank, 155-156, 167-168
Jewkes, John, 187-188
Johnson, A.W., 105, 108, 110
Kelly, Mervin J. , 121-122
Kennelly, Arthur E., 56
King, Ernest J., 112, 117
Knox, Frank, 168, 172-173
Kodak, *see* Eastman Kodak Co.
Lack, F.K., 123
Leary, H.F., 163
Long, John D., 72
Lusitania, 17
Mahan, Alfred T., 10
Marconi, Guglielmo, 11, 53-54, 56
Marshall, Edwin, 17, 19
Mauborgne, Joseph O., 123, 158
Maxwell, James Clerk, 53
McVay, Charles B., 35
Melville, George, 13
Menlo Park, New Jersey, Edison's laboratory at, 7
Michelson, A.A., 138
Mission-oriented research and development, comparison of Edison's laboratories with later organizations, 8-9
Mission-oriented research and development, in America, 5-7, 183, 185
Mission-oriented research and development, number of American laboratories in 1919, 9, 184
MIT Radiation Laboratory, *see* Radiation Laboratory (Massachusetts Institute of Technology)
Morrill Land Grant Act, 6
Multicavity magnetron tube, 151, 155, 158-159, 175
Mumford, William W., 66
National Advisory Committee for Aeronautics, 13, 155-156, 171, 184, 186
National Bureau of Standards, 99, 103, 139, 156
National Defense Research Committee, 155-158, 161, 163-166, 168, 172-174, 180, 190
National Research Council, 21, 32, 184
Naval Academy, 14, 28
Naval Appropriations Subcommittee, *see* Congress, U.S., Naval Appropriations Subcommittee
Naval Research Advisory Committee (National Academy of Sciences), 166-167
Navy, U.S., Bureau of Aeronautics, 117, 128
Navy, U.S., Bureau of Construction and Repair, 12-13, 129-130
Navy, U.S., Bureau of Engineering, 12, 27, 32, 36, 76, 129-130
  funding of NRL by, 51-53, 63, 67, 89, 91
  interaction of with private industry, 70-71, 105, 107, 111, 120-122, 124
  management of NRL by, 68-69, 77, 88, 91, 95-96, 103-105, 100, 127, 128, 188
  response of to NRL technical ideas, 40, 56, 62-63, 79
  principal responsibility of, 50
Navy, U.S., Bureau of Ordnance, 12, 35, 113-117, 128
Navy, U.S., Bureau of Ships, 129-130
  funding of NRL by,
  management of NRL by, 153-154, 161-162, 165, 176, 181
Navy, U.S., Bureau of Steam Engineering, *see* Navy, U.S., Bureau of Engineering
Navy, U.S., bureaus system, 12, 25, 76

UNCLASSIFIED

Navy, U.S., communication, 11
Navy, U.S., Engineering Experiment Station, 13, 28
Navy, U.S., General Board of, 72, 164-166, 168, 169, 170
Navy, U.S., modernization, 10
Navy, U.S., Naval Consulting Board,
  activities of after World War I, 37-38
  contributions to Navy, 24, 131
  control of research Laboratory, 14, 29, 186
  name, 24
  origin, 19-22
Navy, U.S., Naval Radio Telegraphic Laboratory, 14
Navy, U.S., Naval Research Laboratory
  administrative position of, 35, 47
  appropriations for, 27-28, 36, 47, 50-51, 134-135
  change of name of, 49
  Congressional approval for, 25-28
  commissioning of, 46
  original idea for, 17
  original organization of, 37
  place in history of U.S. Navy research and development, 11
  Radio Division,
    establishment of, 37
    policies of, 49, 56, 72, 77, 172, 176
Navy, U.S., Naval Torpedo Station, 12
Navy, U.S., Navy Research Council, 166
Navy, U.S., Naval Aircraft Radio Laboratory, 39
Navy, U.S., Office of Naval Research, 174, 185
Navy, U.S., Office of the Chief of Naval Operations, management of radar project by, 110, 127, 53, 164, 166, 170, 176, 189
Normandie, radar equipment on, 93, 119
Noyes, Leigh, 163
Noyes, Robert H., 139
Oberlin, Edgar G., 48, 67, 70-78, 114, 132, 164
Oliphant, M.L., 148
Oliver, William, 73-75
Page, Irving, 125
Page, Robert M.
  biographical sketch of, 85-88
  involvement of in radar project, 1-2, 78-80, 82, 88-90, 92-94, 97, 99, 10-101, 103-104, 108-110, 119, 123-126, 139, 144-145, 174-175, 117-178, 180, 188
  views of concerning A. Hoyt Taylor, 46
Parsons, William S., 113-118
Patents
  policy of NRL on, 87
  radar-related, 54, 67
  relation to rise of mission-oriented laboratories, 6
Pearl Harbor, attack on, 180
Philpott, La Verne, 93
Plan Position Indicator, 125
PPI, *see* Plan Position Indicator
Radar,
  development of by Great Britain, 137, 141-148, 152, 154
  development of by U.S. Army, 65-66, 106-107, 111, 126, 137-141, 154
  first ideas related to, 39-40, 80-82
  frequencies of, 53, 78
  initial test of equipment, 1
  management of development of, 3, 50, 63, 78, 91, 95-96, 103, 127, 135, 151, 172, 174-175, 177, 180-181, 189-190
  relation of to ionospheric research, 58, 80
Radiation Laboratory (Massachusetts Institute of Technology), 158-159, 161-162, 172, 174-176, 180
Radio Corporation of America, 50, 105-107, 109, 111, 113, 120, 124, 126, 128, 139, 154, 177, 179
Radio
  detection, *see* Radar
  high-frequency, 37, 40-41, 54, 56-57, 61, 80, 83
Randall, J.T., 148
Reinartz, John L., 57
Ridenour, Louis, 173
Ring-tube oscillator, radar, 101, 103
Robins, Thomas, 22, 28, 76
Robinson, J.K., 36
Robinson, Samuel M., 69, 72, 75-77, 130, 143-144
Roland, Alex, 186
Roosevelt, Franklin, 149-150, 156
Roosevelt, Theodore Jr., 46
Rowe, A.P., 141-142, 144-145
Ruble, Wilbur J., 95, 103, 121, 123
Rysgaard, Jens M., 87
SA radar, 177
Saltzman, Charles, 138
Saunders, William, 22, 35
Sawers, David, 187-188
SC radar, 177
Schuyler, G.L., 165
SCR-268 radar, 140
SCR-270 radar, 141, 180
SCR-271 radar, 140
Scrugham, James, 90, 133, 135, 143-144
SD radar, 135
Senate, U.S., Naval Affairs Committee, 130
Signal Corps Laboratories, *see* radar,

development of by U.S. Army
Smith, William S., 24, 33-37, 130, 186
Sperry, Elmer, 11
Stark, H.R., 155
Stillerman, Richard, 187-188
Taylor, A. Hoyt
analysis by of early NRL governing policy, 48, 50
biographical sketch of, 41-43
early ideas of for radio detection, 39, 41, 54, 62
management of radio division and radar project by, 64, 77, 79, 82, 87, 90-92, 94-95, 100-101, 104, 119, 135, 138-139, 176, 188
work of on high-frequency radio, 57
Taylor, David W., 13
Tizard, Henry T., 142-143, 146, 148-151, 179-180, 190
Torpedoes, naval, 12
Tuve, Merle A., 57
Upton, Francis, 8
USS *Leary*, radar tests on, 101, 103, 120
USS *New York*, radar tests on, 105, 110
USS *Texas*, radar tests on, 107, 109
Van Keuren, Arthur H., 130
Varela, Arthur A., 99-101
Vinson, Carl, 130
War Committee of Technical Societies, 21
Washington Navy Yard, 12-13
Watson-Watt, Robert, 58, 142-147, 149
Western Electric, *see* American Telephone and Telegraph Company
Westinghouse Corp., 140
West Orange, New Jersey, Edison's laboratory of, 8, 26, 30
Whitney, William, 11
Whitney, Willis R., 9, 25-27, 29
Wilkins, A.F., 142-144
Wilson, Woodrow, 17, 19
Wimperis, H.E., 142-143
Wolff, Irving, 105-107, 139
Woodring, Harry, 140
XAE identification device, 126
XAF radar, 105, 107-110, 124-126, 146
XAR radar, 177-178
XAT radar, 179
Young, Leo C., 58
biographical sketch of, 43-45
early ideas of for radio detection, 39, 41, 54, 61, 80
management of radar project by, 66, 78-79, 82, 89, 100, 151, 188