The Navy's "Sophisticated" Pursuit of Science

Undersea Warfare, the Limits of Internationalism, and the Utility of Basic Research, 1945–1956

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ABSTRACT

Although it often chafed at scientists' wishes to promote international cooperation, the U.S. Navy was a great supporter of such initiatives during the 1950s. This essay examines the impetus for the Navy's alliance with scientists to bolster its antisubmarine capabilities, the reasons for its acceptance of international cooperation as a means to ensure its technological capabilities during a general war, and the source of its clashes with scientists over the issue of classification. Using diverse sources, including the records of the Navy's Strategic Plans Division, the essay illustrates that, despite the confluence of interests between the Navy and oceanographers, there was a decisive difference in their views as to the ultimate utility of basic research. This difference stemmed largely from scientists' limited perception of science as the capital for new technology and the Navy's perception of science as the collection of operational data for existing technology. This essay, which takes the Navy's views more fully into account, brings to light an issue that should be an important component of any analysis of scientists working with the military.

IN LATE JANUARY 1958 THE JOINT CHIEFS OF STAFF met with officials in the Department of State to address several national security issues brought about by scientific cooperation during the International Geophysical Year (IGY) of 1957–1958. Although the

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launch of *Sputnik* during the IGY had received most of the press coverage, American leaders were also disturbed by the increased activities of Soviet ships in the waters surrounding the Antarctic continent as part of the international project. Not only did these ships promise an increased presence of the Soviet Union at sea; they portended a new zone of conflict in the southernmost continent. When one official at the meeting noted that many American scientists favored internationalization of the region as a solution, the Chief of Naval Operations (CNO), Admiral Arleigh Burke, responded that although the scientists always seemed to favor international cooperation, it was not always the wisest course.[1] Burke had reason to be concerned. Under his watch as Director of Strategic Plans and then as CNO, the Navy participated in some of the most ambitious oceanographic research projects in history, many of them as part of international cooperative initiatives. Burke felt that scientific research, even if it entailed cooperation with other countries, was an essential component of America's national defense strategy.[2] His caution, however, reflected a prevalent view among Navy leaders that, despite their increasing reliance on science, they should be wary of scientists' narrow appreciation of how science related to national security.

Despite Burke's reluctance to listen to scientists in 1958, the Navy had supported a vast amount of scientific research in oceanography throughout the 1950s. The relationship between the needs of the Navy and those of oceanographers is crucial in the history of the American armed services in an era dominated by the need to adjust to the requirements of constantly changing technology. Naval support was generous; not only was the Navy the leading patron of scientific research into defense technology after World War II, but it also provided logistical support for such symbols of peaceful international scientific cooperation as the IGY. Arguably, oceanography in the United States owed its very existence as a mature discipline to financial and logistical assistance from the Navy.[3] Historians

[1] John P. Glennon, ed., *Foreign Relations of the United States, 1958–1960*, Vol. 2: *United Nations and General International Matters* (Washington, D.C.: Department of State, 1992), p. 470. Many scientists after World War II championed international cooperation—especially freedom of publication and open collaboration across national lines—as a means to promote the growth of basic science. See, e.g., a series of articles in the *Proceedings of the American Philosophical Society*, 1947, *91*, promoting data collection networks, conferences, and societies as methods to collect and disseminate scientific work most efficiently. At the same time, some scientists adhered to an ideology of scientific internationalism out of moral obligation to both society and the scientific community. A useful history of the "scientists's movement" of social responsibility toward the international community immediately following World War II, particularly with regard to atomic weaponry, can be found in Alice Kimball Smith, *A Peril and a Hope: The Scientists Movement in America, 1945–1947* (Chicago: Univ. Chicago Press, 1965). More recently, Matt Price has argued that these internationalist attitudes, epitomized by the 1945 Franck report urging an international arms agreement, were less a product of international fraternity than the result of a culture of dissent among scientists caused by the strictures placed on them in the workplace by military supervisors. See Matt Price, "Roots of Dissent: The Chicago Met Lab and the Origins of the Franck Report," *Isis*, 1995, *86*:222–244. For a recent analysis of the pervasive belief in an obligation to international science throughout the 1950s see Joseph Manzione, "'Amusing and Amazing and Practical and Military': The Legacy of Scientific Internationalism in American Foreign Policy, 1945–1963," *Diplomatic History*, Winter 2000, *24*:21–55.

[2] Many scientists shared the defense-oriented view after World War II, especially because they felt that the imposition of communist ideas on scientific practice in the Soviet Union had made internationalism unrealistic. For an analysis of American efforts to gather intelligence on Soviet science during the late 1940s see Ronald E. Doel and Allan A. Needell, "Science, Scientists, and the CIA: Balancing International Ideals, National Needs, and Professional Opportunities," in *Eternal Vigilance? Fifty Years of the CIA*, ed. Rhodri Jeffreys-Jones and Christopher Andrew (London: Cass, 1997), pp. 59–81. For a treatment of many scientists' promilitary stance during the mobilization for war in Korea see Daniel J. Kevles, "K_1S_2: Korea, Science, and the State," in *Big Science: The Growth of Large-Scale Research*, ed. Peter Galison and Bruce Hevly (Stanford, Calif.: Stanford Univ. Press, 1992), pp. 312–333.

[3] Three invaluable accounts of the growth of oceanography at major American research institutions, including discussion of the Navy's role, are William Wertenbaker, *The Floor of the Sea: Maurice Ewing and the Search to Understand the Earth* (Boston: Little, Brown, 1974); Susan Schlee, *On Almost Any Wind: The Saga of the*

properly have emphasized the important role played by the Office of Naval Research (ONR) in supporting basic research, often without insisting that such research should have clear technological applications. ONR acted as a surrogate agency for the future National Science Foundation (NSF) between 1946 and 1950, and it continued to fund basic research long after NSF finally began to function. Throughout the 1950s, scientists routinely promoted basic research as the means for providing the building blocks for new technology, and ONR allowed this principle to guide its funding practices. The Navy's proscience policies elicited praise from civilian scientists such as the physicist Louis Ridenour, who noted that the branch's approach to funding scientific research made it a "much more sophisticated organization than the Army."[4]

Despite its importance for explaining the growth of American science during the Cold War, one cannot rely too heavily on the story of ONR to understand the relationship between science and the Navy. That tale can be misleading for at least two reasons. First, it obscures the unique role that science, particularly oceanography, occupied in shaping the strategic vision of the Navy's leaders, even outside of ONR. More important, it propagates among historians of science the restricted conception of the utility of basic research that motivated many scientists but frustrated many Navy leaders. One must also examine the Navy's pursuit of oceanographic research from the point of view of the Navy as a whole, not merely of ONR, and frame the Navy's decisions about science in a context more in line with its strategic goals.

In examining the Navy's motivations for funding research in oceanography and for pursuing international cooperation, an important contrast emerges between the Navy's and the scientists' perception of the utility of basic research. Heavy mutual reliance marked the relationship between oceanographers and the Navy, and they forged amicable ties that often were quite unlike the military relations with scientists in other disciplines. The oceanographers wished to conduct research, and the Navy wished to know everything there was to know about its own workplace—the sea environment. The Navy undeniably valued scientists' advice on long-term strategies such as the development of an alternative (submarine-based) nuclear deterrent and the investment in undersea warfare technology. The oceanographers, like other scientists, felt that basic research provided the

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Oceanographic Research Vessel Atlantis (Ithaca, N.Y.: Cornell Univ. Press, 1978); and H. W. Menard, *The Ocean of Truth: A Personal History of Global Tectonics* (Princeton, N.J.: Princeton Univ. Press, 1986). More recently, Ronald Rainger, analyzing the investigations of Bikini Atoll after atomic bomb testing, revealed how oceanographers sought to embed their own research within agendas designed to accomplish the Navy's objectives. See Ronald Rainger, "Science at the Crossroads: The Navy, Bikini Atoll, and American Oceanography in the 1940s," *Historical Studies in the Physical and Biological Sciences*, 2000, *30*:349–371.

[4] Louis N. Ridenour, "Military Support of American Science, a Danger?" *Bulletin of the Atomic Scientists*, Aug. 1947, *3*:222. On oceanography and the Navy see Harvey Sapolsky, *Science and the Navy: A History of the Office of Naval Research* (Princeton, N.J.: Princeton Univ. Press, 1990). It does not follow that the Navy as a whole, in the absence of ONR scientists, would have pursued only technology at the expense of basic research. This essay, which makes use of documents from the Navy's Strategic Plans Division, should make clear how critical scientific research became to the Navy—even outside of ONR. Chandra Mukerji already has suggested that the Navy and other funding sources came to value scientists not merely for the data they provided but also for the expertise that they honed over many years, providing a knowledge base (or, as Mukerji puts it, an "elite reserve labor force") for various military and policy needs. See Chandra Mukerji, *A Fragile Power: Scientists and the State* (Princeton, N.J.: Princeton Univ. Press, 1989). For accounts of the largely unsuccessful efforts to put government support for science more firmly into the hands of civilians see Nathan Reingold, "Vannevar Bush's New Deal for Research; or, The Triumph of the Old Order," *Hist. Stud. Phys. Biol. Sci.*, 1987, *17*:299–344; and Jessica Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation Debate Revisited," *ibid.*, 1995, *26*:139–166. For an overview of these events see J. Merton England, *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945–57* (Washington, D.C.: National Science Foundation, 1982).

building blocks on which technological applications could be based. They advocated international cooperation as a means to coordinate basic research efficiently on a large scale, a strategy that entailed openness and data sharing, and they assumed that the United States could transform such science into new technology the fastest. It was this strategy that, as historians correctly point out, was largely accepted within ONR. Yet Navy leaders learned that, if pursued in a limited fashion, research in oceanography was also a useful means to provide constant operational information for its existing defense systems. Ultimately the scientists' internationalist strategy failed to take the operational view adequately into account, leading to frustrating impasses in policies of classification. This operational outlook, largely ignored in the historical literature, was probably the most significant source of friction between oceanographers and the Navy.

FRONTIERS FOR SCIENCE AND SURVIVAL FOR THE NAVY

During World War II the Navy underwent a complete turnaround in its perception of the value of oceanographic work for military purposes. Prior to the war, scientists felt that the Navy's appreciation of their usefulness came only in spurts and funding came only for specific projects.[5] Even as the war began, the Navy's knowledge and appreciation of science was limited, but it turned to civilians to help tackle some of the difficult problems it could not solve on its own. Under director Columbus O'Donnell Iselin, scientists at the Woods Hole Oceanographic Institution in Massachusetts did a great deal of work on undersea acoustics, using the physics of sound transmission to transform techniques of harbor mining and submarine detection. These scientists established a record of usefulness for national security by addressing the Navy's most acute difficulties in conducting undersea warfare. The wartime report by Woods Hole's W. Maurice Ewing, "Sound Transmission in Seawater," remained the standard text on the subject within the Navy for many years. By the end of the war, the Navy had committed to funding scientists at both Woods Hole and the Scripps Institution of Oceanography in California on a continuing basis for an indefinite period. As the oceanographer Roger Revelle later put it, the Navy gave these institutions tenure. Since the specific military applications were classified, usually even beyond the scientists' security clearance, many simply pursued their own ideas and Navy officials used the research as they saw fit.[6] It was a productive arrangement both for scientists and for the Navy.[7]

[5] This does not contradict the fact that the U.S. Navy had a long history of support for science, particularly in exploration and survey projects but also in basic science. See, e.g., Marc Rothenberg, "'In Behalf of the Science of the Country': The Smithsonian and the U.S. Navy in the North Pacific in the 1850s," *Pacific Science*, 1998, *52*:301–307. The Navy's willingness to fund research became especially clear during the interwar period, as described in William M. McBride, "The 'Greatest Patron of Science'? The Navy-Academia Alliance and U.S. Naval Research, 1896–1923," *Journal of Military History*, 1992, *56*:7–33; David K. van Keuren, "Science, Progressivism, and Military Preparedness: The Case of the Naval Research Laboratory, 1915–1923," *Technology and Culture*, 1992, *33*:710–736; and Naomi Oreskes, "Weighing the Earth from a Submarine: The Gravity Measuring Cruise of the U.S.S. S-21," in *The Earth, the Heavens, and the Carnegie Institution of Washington*, ed. Gregory A. Good (Washington, D.C.: American Geophysical Union, 1994).

[6] Wertenbaker, *Floor of the Sea* (cit. n. 3), pp. 42–44; and Roger Randall Dougan Revelle, "Observations on the Office of Naval Research and International Science, 1945–1960," interviews conducted by Sarah L. Sharp in 1984, Regional Oral History Office, Bancroft Library, University of California, Berkeley (hereafter cited as **Revelle oral history interviews**), pp. 6, 11. For reflections on Woods Hole's wartime experience with the Navy see also J. Lamar Worzel oral history interview, conducted by Ron Doel in January 1996, Lamont-Doherty Earth Observatory Oral History Project, American Institute of Physics, College Park, Maryland (hereafter cited as **Worzel oral history interview**), pp. 124–137.

[7] This was true not only for oceanography but also for other scientific disciplines in what became known as

Even given the mutually beneficial arrangement established in wartime, many in the immediate postwar years worried about the growing patronage of science by the military during a time of peace. Skeptics feared that the defense establishment might not accept the scientists' view that science should be free to pursue its own ends, regardless of technological applications. As military support for science expanded, no civilian funding agency experienced any comparable growth. The scientific community found itself divided between those researchers who relied on funding from the military and those who refused to do so and thus had to be content to work with older and cheaper techniques. In oceanography the problem was acute, since oceanographic expeditions were very expensive; those who refrained from making them could hardly be leaders in the field. Few scientists in any discipline could ignore the problem, and in 1947 a public debate over military funding of science erupted between several noted scientists and intellectuals in the pages of the *American Scholar* and the *Bulletin of the Atomic Scientists.* The opening salvo was launched by the Dean of the Graduate School at the University of Illinois, Louis Ridenour, who scoffed at the concept of "intrusion" by the military into the scientific realm. He felt that scientists should be glad to have the opportunity to do research and noted that they were in no way forced to take money from the military. Ridenour chided researchers, saying that the military might be guilty of seduction but that it was not guilty of rape.[8]

The responses to Ridenour reveal a pervasive recognition of the dangers inherent in a close relationship between science and the military. Several well-known American intellectuals and scientists rallied against the military, particularly the novelist Aldous Huxley, the mathematician Norbert Wiener, and the physicist Albert Einstein, who even likened American society to the militarized Germany under Kaiser Wilhelm II. None of these eminent authors trusted military dominance, and they hoped that the scientific community could take leadership over itself rather than being led astray by the money offered by the armed services. Moderates in the debate felt that such concerns could be addressed without taking the military out of the equation. Vannevar Bush, who had managed American military research efforts during the war, thought that most of the dangers could be avoided if scientists could persuade their patrons of the imperative of supporting "basic" research—work unconnected to any specific technological applications—as well as engineering research specific to military needs. If that particular imperative was met, then the danger posed by a military hand on the purse strings was negligible.[9]

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the era of "Big Science." This term was initially used by Derek Price to describe the exponential growth of science since the seventeenth century: see Derek J. de Solla Price, *Little Science, Big Science* (New York: Columbia Univ. Press, 1963). However, few can dispute that the definitive factor in the growth of American science during and after World War II—often termed "Big Science"—was government (largely military) involvement in supporting basic research. See A. Hunter Dupree, "The Great Instauration of 1940: The Organization of Scientific Research for War," in *The Twentieth Century Sciences: Studies in the Biography of Ideas*, ed. Gerald Holton (New York: Norton, 1972), pp. 443–467; and J. H. Capshew and K. A. Rader, "Big Science: Price to the Present," *Osiris*, 1992, N.S., *7*:3–25.

[8] Ridenour, "Military Support of American Science, a Danger?" (cit. n. 4). Ridenour was the assistant director of MIT's Radiation Laboratory during World War II. If Lorraine Daston is correct in writing that moral economies in science "can dignify some objects of study at the expense of a great many others," the Navy used money to work against the grain of Americans scientists' preferred choices of research programs—and usually succeeded. See Lorraine Daston, "The Moral Economy of Science," *Osiris*, 1995, N.S., *10*:3–24, on p. 23. Indeed, the dilemma of choosing between social responsibility and making a living marked the experience of scientists throughout the Cold War. See Alex Roland, "Science and War," *ibid.*, 1985, N.S., *1*:247–272.

[9] Aldous Huxley, "A Positive Program of Research for Peace," *Bull. Atom. Sci.*, Aug. 1947, *3*:225; Norbert Wiener, "The Armed Services Are Not Fit Almoners for Research," *ibid.*, p. 228; Albert Einstein, "The Military Mentality," *ibid.*, pp. 223–224; and Vannevar Bush, "Dangers to Research, If Recognized, Can Be Avoided," *ibid.*, p. 228.

Bush's view of military funding as a benefit with manageable risks was shared by many involved in oceanography, especially because so many leaders of this field had either served in the Navy or worked as civilians under the military during World War II (see Figure 1). Doubt about military control was offset by the sense of indebtedness to the Navy felt by many scientists, including Revelle, who became director of Scripps in 1950. Many oceanographers became involved in military work, including nuclear tests in the Pacific Ocean, out of a sense of obligation. Revelle recalled, "I'd been in the navy for eight years, and all of us had been involved with World War II. We felt that we ought to do what we could to help the United States government."[10] Next to this sense of obligation, many felt that the dangers of military patronage were comparatively minor.

Seeking to allay fears of a monstrous military, the Office of Naval Research also submitted a comment to the *Bulletin of the Atomic Scientists,* claiming that it had taken the lead in supporting basic research after the war. Such an integration of scientists' research ideals into general funding practices, ONR representatives claimed, was an expensive, difficult, and even pioneering task, but they felt that their office had a special responsibility to undertake it because no civilian agency at that time was equipped to do so. Roger Revelle later recalled that ONR took pride in supporting basic research when it might have chosen projects that seemed more practical. The office had learned that "the way to support research was to support the work that researchers wanted to do instead of dreaming up projects for them to do." Indeed, this line of reasoning entered into Ridenour's initial essay. Because freedom of inquiry was the hallmark of its attitude, he claimed that the Navy had a remarkably sophisticated approach to research.[11]

That the Navy, on the whole, was truly so scientifically sophisticated is doubtful. Those who championed the cause of basic research were a minority. A relatively small number of Navy leaders considered research a critical component of the branch's work. Rear Admiral Harold Bowen later wrote that he credited his stint within the Navy's research establishment to the fact that he had made so many enemies as a flag officer that the Navy effectively banished him to the Naval Research Laboratory, making him its director in 1939. Bowen was later instrumental in the establishment of ONR, but he was already a maverick within the Navy by that time. He even attempted to adopt the scientists' mantra about the value of basic research, but it came out sounding ridiculous. He recalled his testimony to a congressional appropriations committee, during which he tried to employ wit to explain that "if you knew what you were doing it wasn't research." Years later, Secretary of Defense Charles E. Wilson would turn the statement around, remarking disdainfully, "Basic research is when you don't know what you're doing." Basic research was among the first things that the Navy looked to cut when trying to trim costs as the war in Korea began to heat up in late 1950. Many Navy leaders chafed at attempts by science advocates such as Vannevar Bush to put scientists' priorities ahead of military ones. They saw Bush's attempts to form an expert elite that would ensure the pursuit of basic research as a move to implement a technocratic vision of American leadership, a bid for power and a bad omen that the military might lose control of the very projects it had funded. Military

[10] Revelle oral history interviews, p. 51. Ronald Rainger notes that oceanographers such as Revelle were instrumental in convincing the Navy that military patronage could be mutually advantageous but that they recognized the Navy's needs as paramount. According to Rainger, scientists embedded their own research within programs stemming primarily from the Navy's requirements. See Rainger, "Science at the Crossroads" (cit. n. 3).

[11] Alan T. Waterman and Robert D. Conrad, "Office of Naval Research Discusses Ridenour's Views," *Bull. Atom. Sci.*, Aug. 1947, *3*:230; Revelle oral history interviews, pp. 18, 4; and Ridenour, "Military Support of American Science, a Danger?" (cit. n. 4), p. 222.

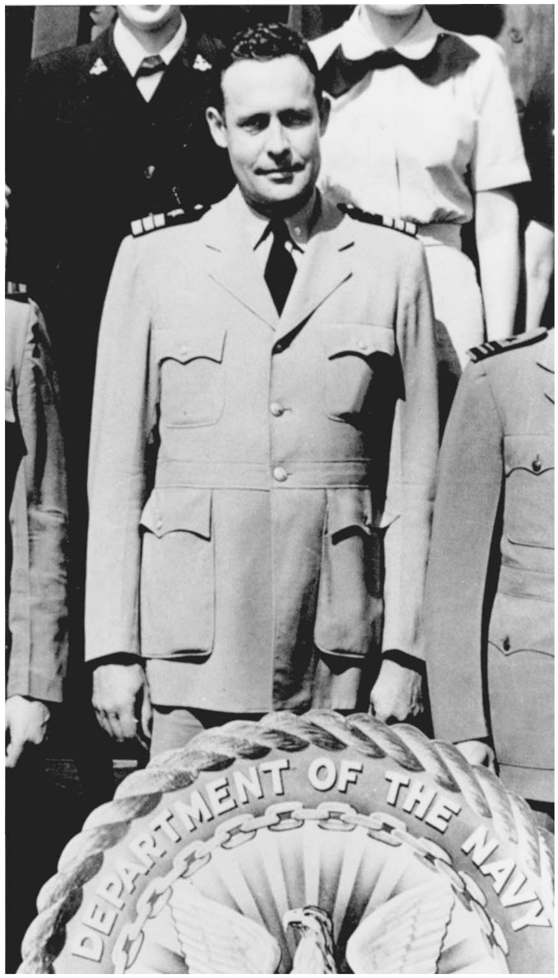

Figure 1. *Many oceanographers served in uniform during World War II. This 1945 detail of a U.S. Navy Bureau of Ships photograph shows Roger Revelle half a decade before he became director of the Scripps Institution of Oceanography. (Courtesy of Scripps Institution of Oceanography Archives.)*

leaders in each of the armed services, including the Navy, resented and resisted the new power of scientists in government.[12]

Relationships of power and stature within the defense establishment were highly unstable during the late 1940s, and the Navy was understandably hostile to any changes that might diminish its position. The atomic age was promising an era dominated by nuclear physics and strategic bombers, not naval task forces; the atomic bomb brought about changes in technology and strategy that made many of the hard-won lessons of conven-

[12] Harold G. Bowen, *Ships, Machinery, and Mossbacks: The Autobiography of a Naval Engineer* (Princeton, N.J.: Princeton Univ. Press, 1954), pp. 137, 354 (quotation); Daniel S. Greenberg, *The Politics of Pure Science* (New York: New American Library, 1967), p. 273 (quoting Wilson); Sapolsky, *Science and the Navy* (cit. n. 4), p. 60 (Korea-era cuts); and G. Pascal Zachary, *Endless Frontier: Vannevar Bush, Engineer of the American Century* (Cambridge, Mass.: MIT Press, 1999), pp. 340–341 (Navy resentment).

tional warfare in World War II seem irrelevant. The military services themselves entered a period of intense rivalry in which the Army and Navy resented the Air Force's presumptuous coup that placed so much of national security strategy in its hands, while the Air Force branded the others as redundant or even obsolete. In 1949 President Harry Truman appointed Louis Johnson as Secretary of Defense in order to end the rivalry through strong centralized leadership. Johnson's strategy was to eliminate programs that he considered wasteful duplications of effort. Although this was a fiscally sound strategy, his outlook was that the Navy itself was largely outmoded; its function, Johnson felt, had been usurped by the Air Force. The consequence of this attitude was that one of Johnson's first actions was to cancel—without notifying either Secretary of the Navy John Sullivan or Chief of Naval Operations Louis Denfield—the already-begun construction of the supercarrier USS *United States.* Sullivan resigned in disgust; adding insult to injury, Johnson announced a $353 million cut in Navy Department funds. Denfield was removed as CNO for his role in the subsequent "Revolt of the Admirals," during which Navy leaders struggled to show Congress that the next war could not be won by air power alone.[13] With new leaders more suited to the Secretary of Defense's outlook now in place, the struggle to assure Washington that the Navy occupied a crucial position in modern American military strategy appeared to require a new focus.

Viewed in this light, the Navy's burgeoning relationship with science had less to do with its "sophisticated" vision than with ensuring its survival as an institution. One Navy strategy was to emphasize conventional threats. Already intelligence reports had concluded that the Soviet Union possessed the capability to launch submarine-based rockets, built from designs stolen from the Germans at the end of the war, against major American ports. The submarines themselves were equipped with snorkel tubes, allowing them to remain submerged and undetectable by radar for long periods. Here was a research need that was decidedly different from the science of atomic bombs: the Navy needed classical physics to facilitate the manipulation of a water environment that was largely unknown. The new Chief of Naval Operations, Admiral Forrest Sherman, put scientists from the Massachusetts Institute of Technology under contract to study how to counter the threat. Project Hartwell (named for a restaurant at which those involved often dined) drew together scientists from elite institutions throughout the United States. These scientists recommended long-term planning to protect American oceanic shipping and to conduct antisubmarine warfare. The Navy was enthusiastic, praising the mutually beneficial relationship between science and the Navy and calling the report the "bible of undersea warfare."[14]

The Hartwell scientists had expanded the scope of their study to include the whole mission of the Navy. Although they did focus on some narrow problems, they also endeavored to put into practice a more system-oriented approach to national security. They recommended a number of changes, from the level of technical training for officers to the

[13] E. B. Potter, *Admiral Arleigh Burke* (New York: Random House, 1990), pp. 320–327. For a treatment of this episode, with particular regard to the role of carrier aircraft, see Jeffrey G. Barlow, *Revolt of the Admirals: The Fight for Naval Aviation, 1945–1950* (Washington, D.C.: Naval Historical Center, 1995). On the changes wrought by the nuclear age see Clark G. Reynolds, *History and the Sea: Essays on Maritime Strategies* (Columbia: Univ. South Carolina Press, 1989), p. 186.

[14] On the intelligence reports see Director, Naval Intelligence, to Director, Strategic Plans, 19 May 1952, Naval Historical Center, Washington, D.C., Strategic Plans Division Records, OP-30S/OP-60S Subject and Serial Files, Series XVI (hereafter cited as **NHC SPD**), box 274, folder "A16-8 Antisubmarine Warfare Operations." On the Project Hartwell recommendations see Jack S. Goldstein, *A Different Sort of Time: The Life of Jerrold R. Zacharias, Scientist, Engineer, Educator* (Cambridge, Mass.: MIT Press, 1992), pp. 97–103; the quotation is from p. 103.

development of tactical atomic weapons for use by American submarines. When war began in Korea in 1950, the Hartwell scientists felt a heightened sense of resolve in seeking to address exactly what it would take to fight a war halfway around the world in the 1950s and beyond. Rejecting the view that atomic weapons could only be land or air based, they urged Navy leaders to integrate them into their own battle plans.[15] Acting on these scientists' recommendations allowed the Navy leadership both to focus on a conventional threat—submarines—and to position the Navy within America's strategic vision for the nuclear age.

One Navy leader who took the Hartwell recommendations to heart was Admiral Arleigh Burke. Lucky still to have a career after participating in the "Revolt of the Admirals," he had been transferred to the Defense Research and Development Board for much the same reasons that Admiral Bowen earlier had been "banished" to the Naval Research Laboratory: he was unlikely to cause much trouble in a position of secondary importance, namely scientific research. But Burke was convinced that the Navy's future would be defined by its ability to accommodate and take advantage of new technology and, thus, that there must be parallel trajectories for science and for the Navy. He and a few others decided that the Navy could ensure its relevance by providing an alternative deterrent to bombers and land-based ballistic missiles. If they could develop submarine-launched missiles, Burke and his supporters reasoned, the U.S. nuclear deterrent would be hidden and mobile, obviating the need for a paranoid "use-it-or-lose-it" dynamic of nuclear confrontation. As Director of Strategic Plans and later as CNO for three terms in the 1950s, Burke became one of the forces behind the Navy's drive to develop a national strategy based largely on submarine and antisubmarine warfare.[16]

The scientists' recommendations shaped the Navy's operational requirements for the entire decade. As early as 1951 the Assistant CNO for Undersea Warfare, Frank Akers, began to implement the recommendations of the Hartwell scientists. Long-range detection of submarines proved a particularly enticing research program, and he backed Project Jezebel, the unclassified name for a secret project to develop fixed low frequency listening arrays (LOFAR) that spanned several years. By networking the listening stations together into a sound surveillance system (SOSUS), the Navy could provide a protective barrier against submarine infiltration over a huge area. Science and technology became, more than ever, fundamentals of naval strategy. Later the civilian scientists were active in helping the Navy coordinate information for the stations and even in setting them up, under Project Caesar, at their pilot locations in the Atlantic Ocean. Oceanographic surveys by civilians were needed both to find out whether arrays could be laid at particular locations and to ascertain the projected quality of the equipment's performance.[17] Shortly after the Hartwell scientists delivered their recommendations to the Navy, the Chief of Naval Research proposed a series of Undersea Warfare Symposia to keep civilian scientists abreast of naval developments and technological requirements. The Hartwell scientists' insistence that a submarine should not be perceived merely as an antishipping weapon was borne out when Naval Intelligence reported in 1952 that the Soviets were experimenting with launching guided missiles from submarines. The missiles, presumed to be similar to

[15] Kevles, "K_1S_2" (cit. n. 2), p. 327.

[16] Potter, *Admiral Arleigh Burke* (cit. n. 13), p. 330 (Burke's transfer to research); see also Chs. 22–24.

[17] Frank Akers, Assistant Chief of Naval Operations (Undersea Warfare), "Undersea Warfare Newsletter No. 2-51," 15 Nov. 1951, NHC SPD, box 264, folder "A16-6 Submarine Warfare Operations"; and Chief of Naval Operations Top Secret memorandum to Chief of Bureau of Ships, Chief of Naval Research, and Hydrographer, 6 June 1952, NHC SPD, box 272, folder "A1 Plans, Projects, and Development."

German-designed V-1 or V-2 rockets, could be fitted with atomic warheads. The Director of Naval Intelligence believed that these submarine-based missiles would be the most likely means of employing atomic weapons against the continental United States in wartime, adding that "it is believed that if the Soviets so desired V-1s could be launched against our coastal cities at any time."[18] Such developments brought the need to detect undersea threats sharply into focus.

Investigations into submarine detection drove a great deal of marine research. Some of this was applied research to improve the efficiency of the LOFAR equipment. For example, low frequencies gave a better chance of detection, so the Navy strove for the lowest frequencies possible in its equipment. But because low frequencies required large and expensive transducers, another area of research sought ways to provide higher quality at higher frequencies. Also, some of the more difficult problems with such listening networks were not in the actual detection of but in classifying noise. "[Long-range detection] is not worth a damn unless you have classification," insisted Captain J. S. McCain of the Navy's Antisubmarine Plans and Policies Group. Unless the detection mechanism could isolate and quantify momentum waves and natural resonance, it was difficult to differentiate attack submarines from natural phenomena such as whales or even large schools of fish. Even when what was clearly a submarine had been detected, discerning other sounds—shaft and blade speeds, engine explosion rates, gears and blowers, and cavitation speeds—could provide the "signature" of a submarine conducting particular operations.[19] Here were clear avenues of civilian research for both biological and physical oceanography, pure and applied. Through ONR, the Navy contracted such research to civilians in industrial laboratories such as Bell Laboratories and Hudson Laboratories and in oceanographic centers such as Woods Hole and Scripps.

Oceanographers welcomed the Navy's support because it enabled them to move to the forefront of research by conducting expeditions. Initially the Navy wished to prioritize the North Atlantic, because it conducted its most active peacetime forward deployment missions there. Soviet deep-water ports were most accessible from the North Atlantic, and thus Navy leaders assumed that the most significant threat to American sea-lines of communication in wartime would come from Soviet submarines in this region. Consequently, Woods Hole enjoyed ongoing support from the Navy to continue oceanic research in the Atlantic. In addition, scientists at the fledgling Lamont Geological Observatory (founded in New York in 1949) had been active in setting up a pilot SOSUS in Bermuda, and the Navy recognized their efforts by giving them their own ship. Use of this ship, the R/V *Vema,* enabled some of the most intensive oceanographic surveys in the Atlantic, organized by Lamont's director, the tireless W. Maurice Ewing.[20] The Navy needed these expeditions as badly as the scientists did, and numerous observations and experiments were conducted—some primarily for the Navy and some mainly for the scientists—on the same cruises. One oceanic phenomenon that kept ships at sea in the Atlantic was the thermocline

[18] Chief of Naval Research memorandum to various, 9 Mar. 1951, NHC SPD, box 264, folder "A16-6 Submarine Warfare Operations"; and Director of Naval Intelligence Top Secret letter to Director, Strategic Plans, 19 May 1952, NHC SPD, box 274, folder "A16-8 Antisubmarine Warfare Operations."

[19] Minutes of Meeting of Anti-Submarine Plans and Policies Group, 5 Feb. 1952, pp. 1–3, 6, NHC SPD, box 271, folder "A16-8 Antisubmarine Warfare Operations."

[20] For the expectation of action by Soviet submarines in the North Atlantic see David Alan Rosenberg, "American Naval Strategy in the Era of the Third World War: An Inquiry into the Structure and Process of General War at Sea, 1945–90," in *Naval Power in the Twentieth Century*, ed. N. A. M. Rodger (Annapolis, Md.: Naval Institute Press, 1996), pp. 242–254, on p. 245. On work at Lamont see Worzel oral history interview, p. 294. For a discussion of Ewing's work and the cruises of the *Vema* see Wertenbaker, *Floor of the Sea* (cit. n. 3).

(or temperature gradient). This phenomenon, discovered before World War II, obstructed the effectiveness of sonar. Because of great variations in water temperature in certain parts of the ocean, the boundaries between temperature layers would act as walls to the active sonar pings from surface ships. A submarine could hide beneath such a thermocline and escape detection. This phenomenon provided the ultimate research agenda for both the scientists and the military. Using an instrument called the bathythermograph (BT) (see Figure 2), scientists could map the locations of thermoclines for the Navy's operational use while simultaneously conducting whatever other research they prioritized for themselves.[21]

Scientists based on America's Pacific Coast also took advantage of the Navy's pressing needs. Very soon after taking the helm at Scripps in 1950, Roger Revelle planned the institution's first major oceanographic expedition, Mid-pac. The expedition was an eye-opener both for science and for undersea warfare. Scientists found that heat flow through the sea floor was at least as high as that on land, a fact that, Revelle noted in a letter to *Nature,* could be explained by a convection (rather than conduction) hypothesis of heat flow. Because convection had been envisioned by scientists such as Arthur Holmes as the mechanism for the controversial concept of continental drift, the heat flow findings on Mid-pac were particularly striking. Mid-pac scientists also dredged very young corals—Upper Cretaceous, only about eighty million years old—from the peaks of undersea mountains. This meant that somehow these mountains had transformed from islands to deep mountains in just eighty million years, whereas scientists had assumed that they had sunk over a period of two or three billion years. In addition, seismic research showed that the sediments of the ocean floor, which scientists had expected to be thousands of meters thick, were in fact only a couple of hundred meters thick. Revelle recalled years later that "everything turned out to be different than anybody had thought it would be before."[22] These findings were early signs of the youth and mobility of the ocean floor, concepts that would soon transform how scientists viewed the earth.

The Navy, in turn, drew on the work of these scientists to improve the effectiveness of naval technology. All of the Mid-pac studies were science based, and Revelle insisted that they did not benefit the Navy directly. "They were fundamental discoveries about the ocean," he said, "but they didn't tell you very much about how a submarine could behave." This was not altogether true. Although the scientists may have come away from Mid-pac with puzzling new insights about oceanic processes, the Navy learned something far more basic. H. William Menard, then a scientist at the Naval Electronics Laboratory (NEL), recalled that as he compiled the data from Mid-pac "it became obvious that almost all of

[21] Cord-Christian Troebst, *Conquest of the Sea* (New York: Harper & Row, 1962), p. 66; and Worzel oral history interview, p. 128. For a discussion of the development and use of the bathythermograph during this era see Gary E. Weir, *Forged in War: The Naval-Industrial Complex and American Submarine Construction, 1940–1961* (Washington, D.C.: Naval Historical Center, 1993). Work on BT slides was a necessary, if time-consuming, task that was probably valued more by the Navy for its operational purposes than by ambitious scientists seeking novel data. Consequently, as Naomi Oreskes has pointed out, such work was accomplished largely by women, who were excluded from other scientific work. See Naomi Oreskes, "*Laissez-tomber*: Military Patronage and Women's Work in Mid-Twentieth-Century Oceanography," *Hist. Stud. Phys. Biol. Sci.*, 2000, *30*:373–392.

[22] Roger Revelle and Arthur E. Maxwell, "Heat Flow through the Floor of the Eastern North Pacific Ocean," *Nature*, Aug. 1952, *170*:199–200; and Revelle oral history interviews, p. 41 (quotation). For discussions of the impact of heat flow studies on the theory of continental drift see Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York: Oxford Univ. Press, 1999); H. E. LeGrand, *Drifting Continents and Shifting Theories: The Modern Revolution in Geology and Scientific Change* (Cambridge: Cambridge Univ. Press, 1988); and A. Hallam, *A Revolution in the Earth Sciences: From Continental Drift to Plate Tectonics* (Oxford: Clarendon, 1973).

Figure 2. *Scientists used the bathythermograph, or BT, shown here on deck, to measure temperature and to identify the depth of any sharp changes that might obstruct the efficacy of sonar. (Courtesy of Woods Hole Oceanographic Institution.)*

the sea floor was an endless expanse of hills."[23] Although somewhat interested in the peculiar scientific results of the Mid-pac expedition, the Navy was far more concerned with the severe challenge that this hilly sea floor might pose to the effectiveness of submarine detection. Its interest in expeditions to map the contours of the ocean basin was ignited anew during the efforts to enhance antisubmarine capabilities. Over the next several years, scientists on both coasts organized expeditions that provided the Navy with a constant flow of new operational information while adding considerably to scientific knowledge (see Frontispiece).

The Navy recognized that its technology required constant attention and that scientists could provide the means to ensure such attention. Research on expeditions was perceived largely as operational, even if it had nothing to do with the improvement of the LOFAR devices themselves. New knowledge of the ocean floor improved the effectiveness of the technology. For the oceanographers, of course, this was the most fundamental of research; the development of so many new findings and ideas about the ocean floor only reified that perception. Yet to the Navy it seemed closer to operations research than to basic research as defined by Vannevar Bush in his 1945 report *Science, the Endless Frontier,* more like troubleshooting existing technology than providing what Bush had called scientific "capital" for the development of new technology. The Navy appreciated this latter form of research as well; yet many, particularly civilian scientists, failed to acknowledge any dis-

[23] Revelle oral history interviews, p. 41; and Menard, *Ocean of Truth* (cit. n. 3), p. 52 (see Ch. 4 for accounts of the expectations and findings of these expeditions).

tinction.[24] The Navy and the scientists each appreciated basic research as a component of both scientific advance and national security, and that convergence of mission served both groups well during the 1950s. Nevertheless, their conflicting perceptions of the underlying utility of research would cause difficulties later.

THE LURE OF INTERNATIONAL RESEARCH

If the Navy was reasonably quick to recognize the importance of science as a whole, it was slower to understand the need for international cooperation. The most obvious reason for international cooperation in oceanography, beyond the search for basic knowledge, was coordinating data with other nations to help predict environmental conditions. The two fields most intensely explored during World War II were marine acoustics (because of the submarine threat) and ocean wave research (for protecting coasts and planning amphibious warfare). MIT's Athelstan Spilhaus, who constructed the bathythermograph used in the late 1930s to detect thermoclines, leaked the design of his instrument to the British to help them fight German submarines even before the United States was involved in the war. He later recalled that he "could have been shot for a spy," but his actions anticipated more intensive wartime cooperation with the British in oceanography. American and British scientists working for the British Admiralty's Swell Forecasting Section, led by the British oceanographer G. E. R. Deacon, made weather forecasts based on the coordination of meteorological data gathered in both countries. Using principles developed largely by the Norwegian Harald Sverdrup and the Austrian Walter Munk (both at Scripps), the section set up wave reporting stations on the southern coast of England that were used to make forecasts for the invasion of Normandy in June 1944. Later the section was moved to the Pacific to help prevent repetitions of the disastrous 1943 Tarawa landing, during which ignorance of local tide conditions contributed to the death and wounding of over three thousand American Marines.[25]

At war's end, oceanographers saw mixed portents for international cooperation. One positive sign was the establishment of a division of oceanography within the Navy's Hydrographic Office. The wartime director of Scripps, Harald Sverdrup, saw it as a great stride forward for international cooperation, anticipating that the Navy might pursue even larger cooperative information networks than that of the weather forecasting system it had helped develop during the war. Granted, the division would be charged only with the responsibility of carrying out research based on the interests of the Navy and the Merchant Marine. But for Sverdrup, the military's new special interest in oceanography was no burden upon science. He had been concerned about his discipline, "by far the youngest member in the family of sciences." Previously, its greatest problem had been recruitment;

[24] Ronald Kline has shown persuasively that, even prior to World War II, it was characteristic of scientists and engineers to draw rhetorical boundaries around their disciplines, calling one research problem "pure" science and another "applied" science. This concept was perpetuated in Vannevar Bush's authoritative report *Science, the Endless Frontier* (Washington, D.C.: Government Printing Office, 1945), which emphasized the need to create "scientific capital" for new technology. Kline argues that this "'assembly-line' blueprint for producing technological change (put science in and get technology out)" was not seriously questioned during the 1940s or 1950s. See Ronald Kline, "Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880–1945," *Isis*, 1995, *86*:194–221, on p. 220.

[25] Athelstan Spilhaus oral history interview, conducted by Ron Doel in November 1989, American Institute of Physics, p. 96; and G. E. R. Deacon, "Ocean Waves and Swell," *Occasional Papers of the Challenger Society*, Apr. 1946, *1*:1–13, rpt. in Margaret B. Deacon, *Oceanography: Concepts and History* (Stroudsburg, Pa.: Dowden, Hutchinson & Ross, 1978), pp. 196–197. See also Susan Schlee, *The Edge of an Unfamiliar World: A History of Oceanography* (New York: Dutton, 1973), pp. 306–311 (Ch. 8: "Oceanography and World War II").

the war, however, had revealed myriad uses for knowledge of the sea and created a sudden demand for oceanographers. Now he expected more men to work not only on military problems but also on fisheries research, geology, navigation, erosion studies, and even civilian construction problems. That expansion, Sverdrup felt, should be encouraged not only in the United States but also abroad, so that all could benefit from the oceanographic work done in different parts of the world. It could lead to an unprecedented level of peacetime international cooperation, making possible truly global scientific portraits of the earth's oceans.[26]

Anticipating the possibility of cooperation, the National Academy of Sciences in 1946 had organized a symposium on "Problems of International Cooperation in Science." Young sciences such as oceanography seemed especially promising areas in which to promote cooperation. Here Sverdrup and other renowned scientists highlighted what they perceived as some of the best ways to go about it. Through international scientific unions, research within specific disciplines could be coordinated in order to prevent duplication of effort and to ensure that all work would be of value. International scientific congresses similarly allowed contacts across geopolitical lines; because they were voluntary congregations of individuals within a given discipline, they should be free from "government control, propaganda, national prestige, or perfunctory representation by officials." Many participants in the symposium, including the California Institute of Technology's Robert Millikan, felt that activities such as international exchanges of scholars were important not merely for science but also for the maintenance of world peace.[27] All of these proposed activities were predicated on the principle of sharing on an international scale. Sharing served national necessity by promoting efficient scientific growth that might lead to new technology, and it served hope through the possibility of furthering world peace.

Despite high expectations, little was accomplished to make oceanography more international immediately after the war. When collaboration occurred, it was between very close allies—as in the case of Operation Cabot, which employed six research vessels from the United States and Canada to take bathythermograph readings and current measurements of the Gulf Stream. This expedition was coordinated between the two national navies for the purpose of their common defense and was only barely international. Sverdrup ultimately was dissatisfied with the ability of leading American scientists to make international cooperation a reality. When he departed Scripps in 1948 to return to his native Norway, he reasoned that he could be more influential in the international arena if he operated from a smaller country. When Columbus Iselin stepped down temporarily as director of Woods Hole in 1950, his replacement by Rear Admiral Edward "Iceberg" Smith did not seem to mark a new commitment to international cooperation. Instead, the trustees' choice of a military man symbolized a break with the informality of oceanographic science and an embrace of efficiency and professionalism. Under Smith, the trustees hoped that Woods Hole would be able to attract and handle increasing amounts of money from military sponsors.[28] On the whole, the course of oceanographic research fell into the Navy's defense-oriented hands.

[26] Harald Ulrik Sverdrup, "New International Aspects of Oceanography," *Proc. Amer. Phil. Soc.*, 1947, *91*:75–78, on p. 75.

[27] John A. Fleming, "The International Scientific Unions," *Proc. Amer. Phil. Soc.*, 1947, *91*:123; Jerome C. Hunsaker, "International Scientific Congresses," *ibid.*, p. 126; and Robert A. Millikan, "The Interchange of Men of Science," *ibid.*, p. 132.

[28] Schlee, *On Almost Any Wind* (cit. n. 3), pp. 196–197 (Operation Cabot), 193–195 (Woods Hole). On Sverdrup see William A. Nierenberg, "Harald Ulrik Sverdrup," *Biographical Memoirs of the National Academy of Sciences*, 1996, *69*:357. Indeed, one of Sverdrup's first acts as director of the Norwegian Polar Institute in Oslo was to organize the 1949–1952 Norwegian-British-Swedish expedition to Antarctica.

Whether the Navy establishment could properly support international activity was still open to question. It was receptive to some activities, such as Operation Cabot and a 1951 information exchange with Norway's Defense Research Establishment. Harald Sverdrup no doubt felt vindicated by this program of oceanographic data sharing. But to some scientists' irritation, the Navy interfered with international scientific ventures that did not explicitly address security issues. Scientists at Scripps hoped in 1953 to use a Navy-sponsored expedition across the Pacific as a means to transport scientists to the Pacific Science Congress held in the Philippines that year. They also hoped to show support, with the presence of an American research vessel, for science in East Asia generally, making stops in Tokyo, Manila, and Nhatrang (in Vietnam). The Navy instead insisted that the visits be limited to Japan, where the scientists might concentrate on the militarily crucial North Pacific and tease scientific information about the region from their Japanese counterparts. The National Academy of Sciences broached the idea of letting Japanese scientists come aboard the research vessel on the 1953 expedition. Although eventually the scientists were allowed on board for a brief leg of the expedition (named Trans-pac), the Navy initially resisted the idea because all the information on board the ship was highly classified. Although scientists could claim a free hand to conduct their own research on military expeditions, red tape impeded the expansion of research to include partners who had not proven themselves reliable allies.[29]

Eventually the Navy did come to support limited cooperation during the 1950s, despite being uncomfortable with the idea of absolute scientific internationalism. The need to subdue the ocean environment, to make it a manageable and even advantageous battleground, drove the Navy to seek to increase its knowledge of the world's oceans. One way to do this was to appropriate that knowledge from others. Leading scientists were highly critical of scientific appropriation as something that the *other* side did, not just because it was unethical but because it was inefficient; compared to the expansion of research and the sharing of information, stealing information appeared to be an unsustainable way to make science grow. For example, Vannevar Bush looked disdainfully on the Soviet Union's faster-than-expected development of the atomic bomb, which he felt was due largely to the appropriation of resources and technology when it took over Czechoslovakia after the war. Such a strategy, he felt, could work only in the short term.[30] The Navy did come to believe that continuous data sharing was preferable to short-term appropriation as a strategy to ensure the steady growth of oceanographic knowledge, but that belief stemmed less from scientists' evangelizing than from its own operational concerns.

To the Navy, one of the most disconcerting facts, evident by the early 1950s, was that the most northerly regions of the globe could no longer be considered a natural defense barrier; indeed, Navy planners felt these would be the most likely battleground in a general war with the Soviet Union. The task of addressing this situation fell to the Navy's Strategic Plans Division, whose duty was to determine how the Navy would fight the next war and to ensure that its plans were sound. Research in oceanography appeared to be one way to

[29] On the information exchange with Norway see Assistant Chief of Naval Operations (Undersea Warfare) to Director of Naval Intelligence, 1 Mar. 1951, NHC SPD, box 261, folder "A1(2) Navy Research (Agenda Items, Minutes of Meeting)." Regarding Trans-pac see Harold J. Coolidge to Claude ZoBell, 12 Jan. 1953; and C. N. G. Hendrix to ZoBell, 25 Feb. 1953, Scripps Institution of Oceanography Archives, La Jolla, California, Subject Files, AC 6, box 7, folder 29. See this folder for other correspondence regarding the Trans-pac expedition.

[30] Vannevar Bush oral history interview, conducted by Eric Hodgins in 1964, Massachusetts Institute of Technology Archives and Special Collections, Cambridge, Massachusetts, MC 143 (hereafter cited as **Bush oral history interview**), p. 272.

address the problem of attacking or defending strategic geographical areas throughout the world. In April 1953 the Director of Strategic Plans, Admiral Arleigh Burke, complained to the Deputy Chief of Naval Operations (for Operations) that the Navy could not expect to conduct successful operations in the critical Norwegian Sea–Barents Sea region. Contrary to "an optimistic trend—unsupported by factual data or experience," he wrote, American fast carrier task groups had little or no knowledge of the ice conditions, sea state, and temperature of the region, information that would be required to conduct antisubmarine and antiaircraft activities there in time of war. Burke claimed that the entire area east of Greenland and north of 70° N was relatively devoid of marine observations and that the few bathythermograph readings available could not ensure accurate prediction of sonar conditions. He suggested that the Hydrographic Office undertake a comprehensive oceanographic program with the assistance of Woods Hole or Scripps.[31]

The Navy immediately stepped up the oceanographic and meteorological research program of its submarine expedition to the area while it brainstormed possible sources to exploit for more data. Even if the information could be obtained easily by using an American oceanographic vessel, the Navy reasoned, "the purpose would be too obvious, and clandestine operations practically impossible." Perhaps instead a British ship "could traverse the area throughout the year collecting needed information under the guise of a routine research expedition." This plan, one of the Navy's top secret sham operations, became known as Project Ice Pick. Its explicit purpose was to enlist a civilian scientific vessel that would pretend to conduct routine research while actually collecting data of paramount operational importance to any action against the Soviet Union in a time of war. Burke, long a believer in the centrality of science and technology to the Navy's mission, saw the project as critical. In February 1954 he wrote to the Director of Fleet Operations that current plans envisaged fast carrier task groups conducting operations in the Norwegian and Barents Seas in the early phase of a general war. But he was "still concerned that insufficient steps are being taken to correct the complete lack of representative weather and hydrographic information upon which plans for these naval operations should be based." He cited a disastrous 1951 supply run in Arctic waters, when inaccurate advance information and inexperience with the weather had resulted in crews forcing ships through the ice, causing millions of dollars in damage. Thus Burke insisted that a full year's worth of oceanographic observations should be obtained to provide operational knowledge of the Norwegian and Barents Seas.[32]

The Navy had some difficulty finding a vessel to do the work. It was impossible to send the Navy oceanographic survey unit into the area: it was already booked for the next few years putting the SOSUS into place under Project Caesar (the implementation stage of the long-range detection research conducted under Project Jezebel). The next option was to find a foreign research vessel to do the job discreetly. Conveniently, the two leading countries in oceanography in the northern region had institutes headed by old friends of the United States Navy: G. E. R. Deacon of Britain's National Institute of Oceanography and

[31] Director, Strategic Plans, to Deputy Chief of Naval Operations (Operations), 24 Apr. 1953, NHC SPD, box 279, folder "A1 Plans, Projects, and Development." On the probability that the northern regions would be a battleground see Op-03D3 Top Secret letter to Chief of Naval Operations, 12 May 1953, NHC SPD, box 289, folder "EF-61 Russia." See also Potter, *Admiral Arleigh Burke* (cit. n. 13), p. 369.

[32] Deputy Chief of Naval Operations (Air) to Deputy Chief of Naval Operations, 22 July 1953, NHC SPD, box 279, folder "A: Plans, Projects, and Development" ("too obvious," suggestion about a British ship); and Director, Strategic Plans, Top Secret letter to Director, Fleet Operations, 25 Feb. 1954, NHC SPD, box 302, folder "Hydrography" ("insufficient steps," year's worth of observations).

Harald Sverdrup of the Norwegian Polar Institute. Even more conveniently, it seemed that they might have ships—the *Discovery II* and the *Albatross II,* respectively—available for charter. At about this time the Navy was finishing construction of a new oceanographic center at Woods Hole. The purpose of the facility was to allow the Navy better control and coordination of oceanographic research, while simultaneously expanding the scope of research and thus the creative freedom of scientists. Scientists from several countries, including Sverdrup, attended the dedication ceremonies in June 1954; it was there that the subject of international collaboration in the Barents and Norwegian Seas was broached. Because it was necessary to conduct the research under the aegis of a civilian scientific institution, the foreign scientists were advised that it would be a cooperative expedition with Woods Hole. But the planning of the project dragged for several months. Burke was replaced as Director of Strategic Plans and sent to sea.[33] No expedition materialized.

Though the expedition never happened, it was not for lack of trying; both the Navy and civilian scientists expended a great deal of effort in looking for a ship. Neither the British nor the Norwegian research vessels were available for the amount of time required. So in August ONR had its liaison officer in London—at that time the marine geologist Robert S. Dietz—gather information about ship availability. He investigated three Danish ships: the *Kista Dan* would be available only for short periods, the *Dana*'s use by the United States Navy would have to be approved by the Danish Parliament, and the *Galathea* would require a complete—and expensive—overhaul of machinery. At last they were left with the Canadian *Arctic Sealer,* whose small size promised little in the way of scientific dividends. After some preliminary negotiations, the Ice Pick Planning Group decided that because of its high cost, engaging the *Arctic Sealer* simply was not worth the trouble of dealing with security classification, installing research equipment, and potentially provoking international discord. Project Ice Pick, in its original incarnation, was cancelled.[34]

The project, although it was cancelled, showed how much the Navy could gain from the free flow of information from foreign sources. The only reliable information previously possessed by the United States was in the logs of ships that had made "the Murmansk run" during World War II. After Ice Pick was initiated, several American scientists and naval officers began to gather meteorological and oceanographic data from their foreign colleagues. The logs of all the ships engaged in those runs between 1941 and 1943 were examined and the relevant weather observations extracted. Still, the information was not only incomplete but also inadequate, as it said little about the extent of ice at any given time of year. Soon more information was obtained from the British, including data captured from the Germans during the war—although Burke suspected that the British held back a great deal.[35]

[33] Director, Fleet Operations, to Deputy Chief of Naval Operations (Operations), 16 Mar. 1954, NHC SPD, box 302, folder "Hydrography" (Project Caesar); Op-33 memorandum to Op-03, 13 Apr. 1954, NHC SPD, box 302, folder "Hydrography" (British and Norwegian ships, cooperative venture); Gordon Lill, "Office of Naval Research Laboratory of Oceanography and Hydraulics Laboratory, Woods Hole, Massachusetts," *Nature*, 29 May 1954, *173*:1017–1019 (new oceanographic center); and Potter, *Admiral Arleigh Burke* (cit. n. 13), p. 377 (Burke sent to sea).

[34] Robert S. Dietz Secret letter to Commanding Officer, Office of Naval Research, 11 Aug. 1954, NHC SPD, box 302, folder "Hydrography" (ship availability); and Planning Group, for Employment of a Chartered Research Vessel, to Director, Strategic Plans, 16 Nov. 1954, NHC SPD, box 302, folder "H1 Hydrography" (decision on the *Arctic Sealer*).

[35] Planning Group, for Employment of a Chartered Research Vessel, to Director, Strategic Plans, 16 Nov. 1954, Enclosure (1), "Brief of Background Correspondence," NHC SPD, box 302, folder "H1 Hydrography" (data from "Murmansk run"); and Director, Strategic Plans, to Deputy Chief of Naval Operations (Operations), 3 Mar. 1954, NHC SPD, box 302, folder "Hydrography" (data from British).

The Navy continued to seek information from foreign sources; to its surprise, the amount of new data it managed to collect was immense. One Navy captain estimated in October 1954 that a particular haul of information from "a foreign source" yielded 150,000 additional meteorological observations, the equivalent of ten ships taking four observations per day continuously for over ten years. Hefty lodes of data followed, including some synoptic observations and microfilmed raw data. The most significant new sources were British, Norwegian, and German. The British information included meteorological observations conducted at Lowestoft and Harrow as well as oceanographic observations made by the vessel *Ernest Holt,* a new ice atlas of the Arctic compiled at Cambridge, and other information gathered by scientists and the Royal Navy in Liverpool and London. The British Meteorological Office exchanged data with the U.S. Weather Bureau. Information from Norwegian oceanographic ships conveniently flowed to the United States through this same agreement because of an established British–Norwegian exchange. From the Federal Republic of Germany, the Navy acquired about 200,000 observations and worked to establish continuous exchange.[36] In short, Ice Pick yielded successes of an entirely unexpected sort.

To the Navy's surprise, the information so obtained was remarkably useful and filled most of the existing gaps. Efforts at collecting data yielded such a volume of information that the Ice Pick Planning Group estimated that scientists would need to spend several months reducing it to a usable form for operations. The Navy realized that, because of the efforts of non-Americans, a major expedition to the region was not an immediate necessity after all. The Ice Pick experience generated an increased interest in and an effort to begin seeking data from all sources, including foreign ones, for operational use.[37] Ice Pick began as a lie, a plan for pretended engagement in cooperative research; but it ended as a truth, a major lesson regarding the potential value of international cooperation to the Navy.

In the course of these efforts, ONR's Gordon Lill made an observation that illustrated the difference between the cooperation required to mount an expedition and that involved in sharing data over an extended period. Probably oceanographic surveys were necessary both to enhance scientific knowledge and for providing general information for the Navy, but in fact the most crucial data, essential for conducting operations, could be acquired only through ongoing research: "Securing 6 months more survey type data in the ICEPICK area will add practically nothing to the aerologists [*sic*] ability to forecast the weather or oceanographic conditions. Current data is absolutely essential for successful prediction."[38] Lill's incisive comment was not so much a criticism of the value of expeditions as a recognition of the need to coordinate research and data exchanges on a continuing basis. Oceanic conditions were not static; knowledge of the environment—its processes, its patterns, and its anomalies—required continuous observation and continuous cooperation. It could not be achieved through short-term appropriation of others' work. Oceanographic research, particularly of the operational kind required by the Navy, could not be reduced to a tangible commodity, a secret formula that could be held in a secure vault, hidden from the enemy. Instead, that research required the kind of international cooperation that Harald Sverdrup had advocated while still director of Scripps in 1947: a mutually beneficial information network.

[36] Op-605 Secret memorandum to Op-533, 5 Oct. 1954, NHC SPD, box 302, folder "Hydrography" (information from "a foreign source"); and Op-533 Secret memorandum to Op-33, 1 Nov. 1954, NHC SPD, box 302, folder "Hydrography" (British, Norwegian, and German data).

[37] Planning Group, for Employment of a Chartered Research Vessel, to Director, Strategic Plans, 16 Nov. 1954, NHC SPD, box 302, folder "H1 Hydrography."

[38] Gordon G. Lill Secret memorandum to Commander E. B. Rankin, Op-605D1, 16 Sept. 1954, NHC SPD, box 302, folder "Hydrography."

Lill's dilemma posed the question, What was the ultimate utility of oceanographic research, either to scientists or to the Navy? The conditions of specific interest during Project Ice Pick—the weather and the state of the sea in particular—were data primarily of an operational nature. Operational data had an immediate value distinct from its utility as the foundation upon which new technology would be based. Yet that foundational utility was the chief justification that scientists gave when advocating the funding of "basic" research. In most instances, this difference was trivial. The scientists received money to pursue their work; the Navy received the data it wanted. But the different outlooks on the utility of research did have ramifications, particularly when the Navy insisted on strict control of the information it had "bought."

THE BATTLE FOR OPENNESS

The Navy came to accept that the free flow of information, at an international level, was a good thing both for the growth of science and for the pursuit of U.S. technological superiority. It supported international expeditions throughout the 1950s, and during the International Geophysical Year of 1957–1958, recognizing that cooperation was a two-way street, it even allowed the dissemination of all the data for use by enemy as well as ally. Apart from the IGY, however, the Navy and scientists clashed over the issue of total internationalism in scientific research throughout the 1950s. It may have been beneficial to share data among allies, but consistent openness to the entire international community seemed a different matter. As the historian Bruce Hevly has argued, the Navy's focus was on designing or supporting institutions that conducted research in pursuit of national power. Not only did the Navy's influence constrain research in general through the technological commitments inherent in that pursuit, as Hevly makes clear; it also constrained the ways in which international cooperation could be pursued because of the need for secrecy.[39] Public disclosure of research results was the most significant site of friction between the Navy and oceanographers.

Many scientists felt that concerns about security, particularly the fear of spies, were highly exaggerated. Vannevar Bush decried America's fears: "We're really a bunch of nuts in this country; the Soviets don't really need a spy system to find out all they need to know about us." Open societies such as the United States required that foreign spies simply peruse engineering and technical magazines and study the advertisements. The military's attitude—better safe than sorry—actually hindered rather than helped national security by retarding technological development. Scientists had dedicated a whole section of the Hartwell report to criticizing the idea that secrecy equaled security. The criticism hearkened back to the days of the Manhattan Project, when General Leslie Groves had tried to keep each scientist on a "need-to-know" basis. The scientists believed that such attitudes inhibited the more effective system-oriented approaches to problems. Overspecialization had blinded Navy leaders to the problems within the entire organization identified by the Hartwell scientists, who believed that the scientific benefits of openness far outweighed the dangers of revealing sensitive data to the Soviets.[40]

But this was a period when concerns about secrecy dominated American life. For all its

[39] Bruce Hevly, "The Tools of Science: Radio, Rockets, and the Science of Naval Warfare," in *National Military Establishments and the Advancement of Science and Technology*, ed. Paul Forman and José M. Sánchez-Ron (Dordrecht: Kluwer, 1996), pp. 215–232, on p. 226.

[40] Bush oral history interview, p. 279 (quotation); and Goldstein, *Different Sort of Time* (cit. n. 14), p. 102.

positive effects on government support for research, the new role of science as the centerpiece of national security also made scientists vulnerable to the unsavory mix of vigilance and paranoia that permeated American politics. Worries about secrecy had brought prominent scientists, most notably the atomic physicists E. U. Condon and J. Robert Oppenheimer, under suspicion for their alleged "anti-American" ties. Eventually both were removed from the government's corridors of power by the revocation of their security clearances. The attack on Condon focused on his internationalism; his belief that the United States should cooperate more directly with the Soviet Union struck the House Committee on Un-American Activities as naïve and even subversive. His opposition to secrecy and his support for civilian control of atomic energy only exacerbated doubts about him. The same aims made Oppenheimer a target; his opposition to pursuing the hydrogen bomb struck many politicians as a sign of bad judgment and thus he seemed a security risk. Oceanographers also came under suspicion. Harald Sverdrup was denied security clearance during World War II because of his possible sympathies toward fascism. This proved embarrassing when he became director of Scripps; the limitations on Sverdrup's ability to participate in classified projects even in the postwar years may have contributed to his decision to return to Norway in 1948. In general, scientists during the first decade after World War II found themselves both respected and suspected. Those with connections to communism or even the liberal left were routinely denied security clearances and government funding. Travel to scientific conferences abroad was limited and controlled through legislation and the active intervention of consular officials and Department of State bureaucrats.[41] All these strictures infringed on scientific activity.

Critics of these policies argued that the strictures necessitated by data classification were more damaging than visa denials, passport restrictions, or even the blacklisting of specific scientists from government work, all of which prevented scientists from pursuing their research. Government scientists, because of classification policies, often were unable to discuss, publish, or circulate their work to other interested researchers. Dissemination of information was a significant problem. The scientists' argument was illustrated, Vannevar Bush later recalled, by the publication of the Smyth report shortly after World War II. Manhattan Project physicist Henry Smyth wrote about atomic energy under specific instructions that he should not reveal anything that the Soviets did not already know; the resulting report told no more than what the Soviets knew but a great deal more than what most American scientists knew about the project as a whole. Neither scientific discovery nor engineering development could flourish, Louis Ridenour argued after the publication of the Smyth report, "except under conditions which allow all competent men to be fully informed, and thus able to contribute to progress."[42] Bush, Ridenour, and many others felt that excessive secrecy meant compartmentalization of knowledge, which might weaken national security.

[41] On Condon see Jessica Wang, "Science, Security, and the Cold War: The Case of E. U. Condon," *Isis*, 1992, *83*:248–267. The case of Harald Sverdrup is discussed in detail in Naomi Oreskes and Ronald Rainger, "Science and Security before the Atomic Bomb: The Loyalty Case of Harald U. Sverdrup," *Studies in the History and Philosophy of Modern Physics*, 2000, *31*:309–369. For an analysis of the effect of anticommunism on the academic community see Wang, *American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War* (Chapel Hill: Univ. North Carolina Press, 1999); and Ellen W. Schrecker, *No Ivory Tower: McCarthyism and the Universities* (New York: Oxford Univ. Press, 1986).

[42] Bush oral history interview, p. 269 (on the Smyth report); and Louis N. Ridenour, "Secrecy in Science," *Bull. Atom. Sci.*, Mar. 1946, *1*:8. On the restrictions on government scientists more generally see Edward A. Shils, *The Torment of Secrecy: The Background and Consequences of American Security Policies* (Glencoe, Ill: Free Press, 1956), p. 178.

When the Navy began to fund civilian expeditions, it strictly controlled access to the oceanic charts produced by scientists. Publication in any publicly accessible journal was impossible unless approved by the Navy, which maintained strict proprietorship over ocean floor soundings and other data. Ocean soundings data came from measurements of depth using an instrument called the echo sounder (see Figure 3), and they were the basic components of any bathymetric map (like a topographic map, only showing depth contours rather than elevation contours). When the Navy approved publication of data, it did so very slowly. H. William Menard, who compiled bathymetric charts for NEL during the early 1950s, later estimated that a few scientists with the proper security clearances had access to oceanographic data about five years before anyone else did. To make matters worse, the Navy typically refused to disclose data to other scientists who asked for information. When J. D. H. Wiseman of the British Museum asked him to provide corrections to a British-made bathymetric chart of the Pacific basins and trenches, Menard responded only that many details were wrong; he could be no more specific. Menard retained his security clearance when he moved to take a position at Scripps in 1956, and his colleagues there benefited from his access to the best bathymetric maps of the Pacific Ocean. The same was true of Lamont director W. Maurice Ewing and others with respect to data on the Atlantic Ocean. Pockets of scientific knowledge emerged, informally connected and isolated not only from the public but from other scientists as well. Menard likened the fortunate ones to an elite "invisible college."[43]

Laboring within this closed community, many scientists attempted to strike a reasonable balance between secrecy and scientific freedom. In September 1951 the Princeton geologist Harry Hess wrote to the Hydrographic Office about the problems of classification. He acknowledged that in some strategic areas (particularly the mid-Atlantic ridge, the Alaskan coast, and the Aleutian Islands) the oceanographic data should be kept classified. But in other areas, such as the continental shelf and slope of both U.S. coasts and parts of the Caribbean, a great deal of information already was publicly available; here the Navy's release of information would constitute no breach of national security. Revelle supported this view and opposed "blanket" classifications of whole regions, such as the one he claimed was in effect for everything north of 20° S. He felt that the Navy should make an effort to classify only areas of specific strategic significance; all other classifications were excessive and even counterproductive.[44]

The scientists often felt that their wishes fell on deaf ears. Hess complained to Revelle in April 1952 that while he had attempted before and would attempt again to present his argument in terms that were readily understandable to the Navy, as yet he had failed to make any progress. Despite his strong feelings about the issue, Hess cared less about winning the argument on principle than about seeing a more reasonable policy put into place, even if it meant that a great deal of data would remain classified. In July 1953 he insisted to the Navy Hydrographer, "There is only one issue as I see it and that is what policy would most benefit the United States and the Navy." Hess pulled out the old ar-

[43] J. D. H. Wiseman to Menard, 5 Nov. 1953, and Menard to Wiseman, 11 Jan. 1954, H. W. Menard Papers, Scripps Institution of Oceanography, box 2, folder 24 "Correspondence 1954"; and Menard, *Ocean of Truth* (cit. n. 3), p. 111. For an analysis of how classification, and Cold War politics generally, shaped the reception of ideas of plate tectonics see Jacob Darwin Hamblin, "Science in Isolation: American Marine Geophysics Research, 1950–1968," *Physics in Perspective*, 2000, *2*:293–312.

[44] Harry Hess to Captain Hobbs, Hydrographic Office, 14 Sept. 1951; and Roger Revelle to Hess, 19 Apr. 1952, Harry Hammond Hess Papers, Firestone Library, Princeton University, Princeton, N.J. (hereafter cited as **Hess Papers**), box 5, folder unlabeled.

Figure 3. *The echo sounder, seen here being examined by scientists below deck, was the basic instrument for mapping the depth of the sea floor. (Courtesy of the Woods Hole Oceanographic Institution.)*

guments: without the free flow of information, new principles that might otherwise have been discovered could go unnoticed, delaying new weapons technology. The damage was done, he said, not because the country's top oceanographers did not get the data but because "it never comes to the cognizance of scientists working in other fields."[45]

There were numerous reasons for this seemingly excessive control of data. Most significant were Navy officials' lack of attention to policy and their tendency to err on the safe side. As Earl Droessler pointed out to Revelle in 1952, the very idea of "blanket" classification was erroneous; the Navy at that time had no explicit policy at all. But that was precisely the problem: because the various policies of the Hydrographic Office were not compiled or disseminated, it was even more likely that junior officers would classify

[45] Hess to Revelle, 24 Apr. 1952; and Hess to Captain Joseph Cochrane, USN, Hydrographer, 13 July 1953, Hess Papers, box 5, folder unlabeled.

data as Restricted. Lloyd Berkner, who led the international committee responsible for organizing the International Geophysical Year, complained that the judgment often rested with a military officer who might be censured (or worse) if his superiors suspected that he had released information improperly. That same officer "is under no penalty for improperly keeping information from the public that might have the most beneficial influence on the future of our affairs."[46]

Another reason for the continued classification was that, although in theory the scientists' idea of openness was sound, in practice it appeared suicidal to the Navy. Years of Cold War had convinced many naval officers that the scientists' attitudes toward the Soviets were misguided and that openness was an invitation to trouble. Rear Admiral Felix Johnson, Director of Naval Intelligence, unambiguously labeled all Soviet peace propaganda as an attempt to exploit American ethical standards. He wrote in 1950 that "the soul-searching and expressions of fear which arose in the U.S. after the President's decision to proceed with the attempt to produce an H-bomb were interpreted by the Kremlin as signs of decay and weakness in the U.S. to be exploited at an opportune moment." This sense of distrust permeated the Navy, whose leaders had to plan for the possibility of war with the Soviet Union. In 1952 the Chief of Naval Research recognized that blanket classifications might stifle the growth of the embryonic science of oceanography, which in turn, in the long run, could possibly reduce the Navy's military readiness. He even stated that it was important that the policy on the dissemination of oceanic soundings be improved. In effect, he agreed with the scientists. He could appreciate, from the lessons of the Hartwell report, that one must think at a broad level and follow policies that benefited scientific and technological growth as a whole. Yet he also cautioned that "the classification at any given time depends on several technical and military considerations."[47] By 1953, strategic planners had made a conscious decision to withhold soundings of the ocean floor.

The Navy's primary reason for withholding soundings data was this: its operational requirements prohibited it from pursuing the scientists' ideal. Navy leaders could not consent to handing such valuable information to the Soviets. Disclosure of ocean floor soundings would provide the Soviet Union with detailed knowledge of the strengths and weaknesses of any LOFAR network the United States might install. This was the sort of "technical" consideration to which the Chief of Naval Research referred. It was impossible for the Navy always to think of long-term benefits when in the short term the effectiveness of existing technology depended on the Navy's control of the data. The Soviets would need only to locate the stations, through intelligence operations, in order to know precisely what the Americans could and could not see inside the oceans. Conscientiously to supply the Soviet Union with such information in the name of scientific cooperation would be a service to an enemy in time of war. Burke acknowledged that "although the Soviet Union could obtain this information if sufficient effort were expended, she does not now have it nor does it appear that she can expend the necessary effort to obtain it."[48] The scientists viewed soundings as basic research; the Navy viewed them as operational information that could be just as useful to Soviet submarines as to American ones.

[46] Earl Droessler to Revelle, 26 Mar. 1952, Hess Papers, box 5, folder unlabeled; and Lloyd V. Berkner, "Is Secrecy Effective?" *Bull. Atom. Sci.*, Feb. 1955, *11*:68.

[47] Rear Admiral Felix Johnson, Memorandum of Information, 27 Feb. 1950, NHC SPD, box 255, folder "Intelligence"; and Chief of Naval Research to Chief of Naval Operations, 8 Dec. 1952, NHC SPD, box 269, folder "Al Plans, Projects, and Developments."

[48] Director, Strategic Plans, to Director, New Developments and Operational Evaluation, 12 Nov. 1952, NHC SPD, box 269, folder "Al Plans, Projects, and Developments."

A number of scientists continued to voice opposition to overclassification. Hess, for example, tried to influence the Navy by arguing that the Russians could collect the data for themselves easily enough by assigning twelve ships to the task for three years. The Navy's attitude was to let the Soviets expend the time and resources to do so, not give them the information for free. In 1954 the National Academy of Sciences asked Hess to speak on the subject of geology and geophysics in oceanography to an international audience at the opening of the new Navy oceanographic laboratory at Woods Hole. He agreed but expressed some agitation, noting that it would be difficult to do a proper job because "the high classification placed on much of this field in recent months makes it extremely difficult to deal with the subject as a whole." Not only would it be difficult for Hess himself to deal with the subject; it would also be difficult to tell what he did know to an international audience. Because of military classification, scientists such as Hess and Revelle felt they might be compromising the international backbone of science. Their lament was nicely captured by the social scientist Edward Shils in his 1956 book *The Torment of Secrecy:* "No professionals have been asked to sacrifice so much of their own tradition as the scientists. . . . They have suffered from misunderstanding, often well-intentioned, but sometimes suspicious, by their military supervisors and their security officers, who have not always understood their nature and their needs."[49] Although Shils was no oceanographer, he understood the complaints of scientists struggling against the ignorance of military patrons who tried to keep science a secret.

The Navy might have issued similar complaints about misunderstanding and suspicion against the scientists. Its most deeply felt concerns seemed to enter the scientists' proclamations on the issue of classification only as an afterthought. Respected spokespersons for the scientific community such as Vannevar Bush reasoned that, given the amount of scientific exchange going on in the form of research cooperation and joint publication, advances discovered in one country would soon seep into others, regardless of secrecy provisions. In such a context, withholding information made no sense. But in an oral history interview recounting his memories of the early 1950s, Bush added that secrecy with regard to military plans was far more sacred, since "the Russians would like to know what we would do in certain eventualities." Lloyd Berkner similarly acknowledged to an audience at Dartmouth College in 1954 that it would obviously be foolhardy completely to abandon technological secrecy in military matters such as the design of specific weapons, as long as withholding such information yielded advantages greater than the corresponding disadvantages. "In the long view," he said, "the public interest requires the freedom of substantially all scientific and technological information as well as the potential nature and implications of its application." Both Bush and Berkner acknowledged the logic of secrecy in isolated, extreme cases, but they focused on the positive long-term effects of openness on both science and technological growth.[50] One is struck by the simplicity of the scientists'

[49] Hess to Cochrane, USN, Hydrographer, 13 July 1953, Hess Papers, box 5, folder unlabeled (arguing that the Russians could collect the data themselves); Detlev Bronk to Hess, 3 Mar. 1954, and Hess to Bronk, 10 Mar. 1954, Hess Papers, box 22, folder "National Academy of Sciences" (regarding the Woods Hole speech); and Shils, *Torment of Secrecy* (cit. n. 42), p. 186.

[50] Bush oral history interview, p. 329; and Berkner, "Is Secrecy Effective?" (cit. n. 46), p. 68. By focusing on long-term benefits, Bush and Berkner were by no means breaking new ground in scientific rhetoric in the United States. As Marlana Portolano has recently demonstrated with regard to John Quincy Adams and nineteenth-century American astronomy, rhetoric in support of science has many effects, some related to immediate policy decisions and others of a less tangible nature, such as establishing dialogue on issues or simply encouraging the public to value science beyond its immediate uses. See Marlana Portolano, "John Quincy Adams's Rhetorical Crusade for Astronomy," *Isis*, 2000, *91*:480–503.

argument as to how science related to national security; they saw a linear progression from basic science to applied technology. Scientists were trying to dispel the myth that there were any real secrets in basic science, insisting that only the applications of it should be kept secret. Yet oceanographic data was a clear case in which basic science itself was a commodity of extreme importance to the Navy's operations.

Few in the Eisenhower administration were sympathetic to the Navy's needs, despite its explicit and straightforward explanation of the danger of releasing bathymetric data. Likely the Navy was perceived as a stubborn adversary not only to scientists but also to the interests of the nation, as the administration sought to improve efficiency and cut costs. In 1953, proving that scientists often had more friends in Washington than did the Navy, Eisenhower abolished the whole Restricted category under which the Navy had classified oceanic soundings. Having experienced firsthand the tendency to overclassify military documents, Eisenhower felt that the time had come to free this particular facet of science from its Navy-inflicted restraints. But faced with the knowledge that the Navy was about to be forced to give a massive quantity of largely unreviewed information to the enemy, the Chief of Naval Operations irked scientists and the administration by promptly upgrading all soundings data to the Confidential category. Not long after, funds for the Navy's planned independent expeditions in Antarctica, to take place while scientists participated in the International Geophysical Year, were cut completely out of the budget by Eisenhower. The Navy had seen these expeditions as the coat of sugar on the bitter pill of supporting extensive unclassifiable scientific research during the IGY. But when Eisenhower quizzed NSF director Alan Waterman, formerly chief scientist for ONR, about whether the Navy's own expeditions were worth the money, Waterman (never one to invite controversy) took no stand for the Navy, and the projects were immediately canceled. Such efforts to undermine their plans severely demoralized Navy leaders, including the famous naval aviator Admiral Richard E. Byrd, who had hoped that these expeditions would help America establish strategic footholds in areas such as Antarctica. This episode was symptomatic of a period in which the Navy, in its attempts to place itself at the forefront of national strategy, worked at cross-purposes with an administration whose goals often kept it on the periphery.[51]

The debate over classification was a conflict between strategic outlooks: one focused on the efficient growth of science, the other based on the need to protect the operational effectiveness of existing technology. It was not an irreconcilable conflict. Virtually all scientists recognized that the Navy should control some of the material, especially that related to certain regions of critical importance to projected wartime submarine activities. After all, scientists too were in the business of national security. The Navy, for its part, made an effort to declassify soundings on a case-by-case basis if a scientist needed them for publication. It realized the necessity of supporting oceanography, and of promoting

[51] On the upgrading to "Confidential" see Athelstan Spilhaus to Honorable Donald A. Quarles, Assistant Secretary of Defense for Research and Development, 28 Dec. 1953, Hess Papers, box 5, folder unlabeled. For some scientists' perception of Eisenhower's attitude toward classification see Menard, *Ocean of Truth* (cit. n. 3), pp. 63–64. On Byrd's hopes for the Antarctic expeditions see Paul Siple, *90° South: The Story of the American South Pole Conquest* (New York: Putnam, 1959), pp. 126–127. On the Navy's view of its role in national defense strategies see Rosenberg, "American Naval Strategy in the Era of the Third World War" (cit. n. 20), p. 247. Under Defense Secretary Charles E. Wilson many Navy programs were cut back in an effort to implement Eisenhower's New Look strategy, which sought to reduce the cost of defending American interests throughout the world by relying less on conventional weapons and more on the threat of nuclear retaliation. See Bruce E. Geelhoed, *Charles E. Wilson and Controversy at the Pentagon, 1953 to 1957* (Detroit: Wayne State Univ. Press, 1979).

international cooperation, for the health of the discipline and the probable rewards of future technology. By way of compromise, it declassified a great deal of low-precision information for wide distribution. Still, even at the end of the 1950s, only about twenty civilian scientists had access to all of the information; scientists would have preferred to extend that number into the thousands.[52] This was a deplorable situation, and most leaders of the scientific community spoke out against it to some degree. Scientists such as Hess and Revelle, by advocating only a limited dissemination of data, nevertheless tacitly acknowledged that the ideal of scientific growth as a means to ensure national supremacy perhaps was overly simplistic. In the short term, the Navy had to bow to operational needs. The Navy acknowledged scientific internationalism as a valid avenue for the pursuit of only one of its ends, new technology. In guarding its more immediate purpose—preserving the effectiveness of existing technology—it fought an unpopular battle with the scientific community.

CONCLUSION

If historians accept the rhetoric of scientists themselves, the story of scientists working with the defense establishment during the 1950s may seem straightforward. In Vannevar Bush's *Science, the Endless Frontier,* we read an appeal to government and to responsible citizens of a democracy to support basic science as the means to ensure American strength. No one made this appeal more clearly than Bush did; he gave it an American flavor by adopting the metaphor of capitalism: basic science was the capital, engineering the product.[53] Scientists after the war applauded ONR for attempting to put that aspect of Bush's vision into place: the Navy supported research without trying to interfere in it by making scientists address specific problems. Scientists would get their funding for basic research, and the Navy eventually would get its technology. Even after ONR could no longer afford such support, during the budget slashing just prior to the Korean War, the view of basic research as the capital for new technology was akin to a religious principle for researchers wishing to acquire funds from the military. Debates centered on whether particular research problems were really basic science, whether too much applied science was funded at the expense of basic science, or—as the science writer Daniel Greenberg later noted—whether basic science really translated into new technology at all.[54] These were all valid questions in the context of the linear perception of science and technology, but they did not begin to address the Navy's position that much basic research was itself inherently valuable to national security. This position is particularly clear in the case of oceanography, in which operational data and basic research results were often the same thing.

Granting that the Navy and oceanographers rarely antagonized each other deliberately, they certainly came into conflict over the issue of classification. An expanded understanding of basic research helps to make sense of their disagreement. ONR could boast of its successes in putting the scientists' conception of basic research into practice, in striking a compromise between freedom of inquiry and military necessity. Yet despite its role in brokering a close relationship between scientists (not just oceanographers) and the Navy,

[52] Hess to Senator Styles Bridges, 10 Feb. 1960, Hess Papers, box 22, folder "NAS-NRC Committee on Oceanography Ocean-Wide Survey Panel, January–June 1962."

[53] See Bush, *Science, the Endless Frontier* (cit. n. 24). See also Kline's analysis of the role of Bush's report in perpetuating the "assembly line" model of basic research turning into technological applications: Kline, "Construing 'Technology' as 'Applied Science'" (cit. n. 24), esp. pp. 218–221.

[54] Sapolsky, *Science and the Navy* (cit. n. 4); and Greenberg, *Politics of Pure Science* (cit. n. 12).

ONR fell short of developing a more sophisticated definition of basic research that would take its operational nature into account. Oceanographers routinely requested that the Navy declassify soundings, reasoning that existing practice only stunted the development of new technology. This technological rationale had historical precedent, to be sure. Woods Hole's work on the physics of sound transmission in the sea, for example, led directly to the development of antisubmarine surveillance stations guarding American coastlines. But oceanography was also an environmental science. Its basic research was valuable not merely for ascertaining how the laws of physics functioned in the sea, but also for determining the ocean's basic topographic features and the oceanic and meteorological conditions at any given time. These facets of oceanography were specific to time and place, and the Navy understandably went to great lengths to learn the details and, to the scientists' chagrin, to keep some of them secret. One might claim that oceanography has always been international, because information must be coordinated throughout the whole world. At the same time, oceanography inherently challenged the concept that science was international because its basic science—knowledge of environmental conditions at any given time—was itself a precious commodity for nations at war. The Navy's occasional sympathies for international cooperation stemmed from its own belief in the value of gathering operational information on a sustained basis from all parts of the world. For those who accepted the notion of basic research as defined by Vannevar Bush and by ONR, the Navy's insistence on classifying oceanographic data, against the wishes of many oceanographers and other scientists, must have appeared short-sighted or ignorant. Yet to evaluate such actions fairly, historians ought to develop a view that takes into account both the Navy's strategic needs and its different perception of the utility of basic research. The Navy's emphasis on operational data, which informed so many of its strategic decisions, was a fundamental aspect of its pursuit of science and a critical source of its conflict with scientists during the postwar era.