The Navy, Science, and Professional History

Gary E. Weir, Ph.D.

This analysis will focus on the United States Navy as participant and patron in relation to the scientific community since the opening of the 20th century. The sciences that have played the greatest role in furthering the naval mission will take center stage, viz., physics, the earth sciences, and oceanography. This exploration will not venture into the literature relating to what the Office of Naval Research (ONR) would place outside the realm of federal basic research and exploratory development funding. Thus, the historical literature on the history of technology, and the applications that emerged from scientific work, will play no role here. That is a specialized field all its own. In the present essay, I shall critically touch on the more insightful works in the history of the Navy's interaction with the scientific community, seeking to reveal the direction and nature of current scholarly inquiry.

My work as a naval historian first touched the history of science when I began exploring the ocean environment as the submarine's natural habitat. I initiated work in the history of oceanography as soon as the ocean emerged in my research as a significant element in the design, construction, and operation of these remarkable vessels. From World War II onward, the environment below the ocean surface and its varying characteristics, especially temperature, pressure, and depth, defined the limits of submarine operations. These same attributes challenged engineers and designers as they developed successive generations of submarines. Of course the most important element in undersea warfare involves an understanding of ocean acoustics. As the submarine began to inhabit the ocean depths for longer periods with silence and detection

constant concerns, understanding the forces governing sound in the depths became essential. I needed to appreciate the scientific aspect of undersea warfare and how the Navy responded over the years to the challenges the ocean presented. Thus, more than a passing familiarity with the best literature on the Navy and the scientific community became necessary.

A review of the historiography in any field naturally leads a historian to look first at the broader treatments of the subject, which usually provide a foundation for further work. In this case a reader might expect that I would begin with comments on studies like James Phinney Baxter's *Scientists Against Time*, which reviewed in a penetrating manner the effort to mobilize science to defeat the Axis in World War II. In the process, Baxter won the Pulitzer Prize for history in 1947.[1] Moving to the Cold War, I might use David Allison's fine contribution on postwar naval research to Merritt Roe Smith's compendium, *Military Enterprise and Technological Change.*[2] For this presentation, however, I have decided to take quite another approach.

In my present position as chief historian at the National Geospatial-Intelligence Agency, I have learned to come to terms with the one perpetual feature of our history that emerges from every source, text, media, and oral history. I refer to the fault lines between the intelligence tradecrafts that came together to form our agency in 1996. The stovepipes that protect each tradecraft have, over the last 20 years, aggressively discouraged collaboration. In the late 1990s, our agency actually viewed the placement of cartographers and imagery analysts in the same office as a risky experiment. Thankfully, as a result of good leadership and some extraordinary developments in tradecraft, the divisions have softened considerably over the past decade.

As historians, we all realize that the cultural identity of professional communities and the traditions and practices that define them can strongly resist any effort at redefinition,

combination, or prolonged collaboration. Very often, even the importance of the mission cannot persuade a stovepiped community to understand that a combination of skills may prove stronger by an order of magnitude for mission success than any singular approach.

As historians, we can also become too comfortable in our splendid isolation. Are we historians of science or naval historians? I imagine that all historical professionals can stand up and define themselves given their training and special interest. As long as we remain within our special category we feel safe. As naval historians, we take comfort in and satisfaction from exploring and understanding a culture that has absorbed many of us for decades. We examine the nature of the naval experience, its ships, its internal structure, its leaders, and its role in the national defense. We carefully dissect the Navy's internal practices, its reward structure, its educational institutions, and its role in the national life.

What happens when we must invite a foreign element into our efforts to understand the Navy? What happened when a scholar like Frank Duncan had to wrestle with nuclear physics in his relentless effort, through multiple studies including a very fine biography, to understand his career-long, fascinating burden: Hyman George Rickover? Duncan touched the laboratory world of General Dynamics, General Electric, and Westinghouse. He had to explore the administrative interaction between the Navy and industry in the effort to create the nuclear Navy. He achieved an understanding of entities that were not naval in nature, which operated by different standards, and for the profit motive. He also offered some of the most important insights into the Navy's nuclear program produced by any historian. He managed this only because he reached outside his naval history experience, encountered the business, scientific, and engineering communities, and worked hard through research and human contact to understand those cultures. He revealed the product of their encounter with the Navy in his history of Rickover, the nuclear Navy, and in

his earlier work on the Atomic Energy Commission with Richard Hewlett. He taught us the discipline of technology as Rickover viewed it and as industry and science understood it.[3] He profitably crossed the cultural boundary through a concerted effort to understand all of the components that contributed to the understanding he wished to achieve and share.

William M. McBride's work on science and the Navy has accomplished much the same. In his work on the Navy's "alliance" with academia he demonstrated the nature of the conversation, born in the 19th century, between university-based science and naval officers. He also provided insights into the distinction between line officers and the engineering corps, that arm of the service constantly linked in many ways to scientific developments. The Navy of the Great War itself had a way to go before it realized how much it truly depended on science's growing knowledge of the natural world, especially in the area of undersea warfare. McBride permitted us to look closely at the opportunities World War I offered both to pure science and to naval officers motivated to enable science within the naval service.[4]

This need to reach beyond the boundaries of strictly defined naval history as suggested by Duncan and McBride also led Kathleen Broome Williams to explore the world of computer development in her biography of Rear Admiral Grace Hopper and the personal struggles of a number of important women in her volume on the improbable female warriors who managed to penetrate the world of science and technology.[5]

In one of her warrior portraits, Professor Williams examines the career of a scientist of my acquaintance, Mary Sears. To understand the life of Mary Sears and her accomplishments, any historian would have to set aside all naval history assumptions and intellectually embrace Sears in her context as a marine biologist at the Woods Hole Oceanographic Institution (WHOI). Sears served in the Navy as a WAVE (Women Accepted for Volunteer Emergency Service)

during World War II, but she became a vital part of the wartime role of ocean science and an important link with the Navy given her senior position at the Hydrographic Office. At one point in my research at Woods Hole, I uncovered a letter written by the institution director, Columbus Iselin, in which he accidentally referred to Sears with the male personal pronoun. She had become a natural part of the male-dominated ocean science community. Even as a WAVE she lived as a physical part of WHOI and a thread in the ocean science fabric.

Williams provides an excellent model for the type of historical analysis that can successfully and willingly cross between professional cultures to examine aspects of the naval experience that we can illuminate only in that way. She has successfully overcome the inclination to remain within the familiar cultural milieu of the Navy. Williams realized that in no other way could she reach Mary Sears and her fellow warriors.

Thus, this historian feels strongly that the best work examining the intersection between science and the U.S. Navy seeks to explore the relationship between two professional cultures and the extent to which that relationship affected the mission-related work of both.

In his work on nuclear testing and the broad ramifications of Project Vela Uniform, Kai-Heinrich Barth reveals a situation in which government funding increased by a factor of 30 in the 1950s and 1960s in an effort to monitor Soviet nuclear testing. All of the services and their efforts to develop nuclear capability depended in part on the knowledge thus gained. In this case, Barth looked at the most powerful of the scientific groups that touched the defense establishment during the Cold War, those working in weapons-related physics. His analysis demonstrates that scientists in pursuit of knowledge—that is, science for its own sake in the purest form—did not compromise their intent by taking defense dollars to work on military projects. Barth strongly rejects the suggestion that defense money and influence distorted the goals of science and

violated the intent of scientific inquiry. Both scientists and the United States government derived what they wanted from a mutual experience: an investment without compromise.

In Barth's view, the scientists involved saw the effort to monitor nuclear activity as an opportunity to advance their work while providing a byproduct that would satisfy the needs of those desiring to achieve an arms control treaty with the Soviet Union.

Many historians of science have energetically refused to agree. The Distortionist Theory as Barth describes it, pervades the history of science literature and appears in its clearest form in the writings of Paul Foreman, a historian of considerable influence, whose best works look into the early history of nuclear physics.[6] He and some others believe that defense funding, and the motives behind the goals of the Navy or any other service, divert pure science from its quest for knowledge into avenues that corrupt the practice or reduce science to a support function for military or political ends. This view presents the possibility that any naval involvement in scientific work would naturally pervert the scientific process. This perspective has influenced the way some historians of science look at the relationship between scientists and any arm of the government, including the Navy.[7]

In his groundbreaking work on the laboratories at the Massachusetts Institute of Technology and Stanford University, Stuart Leslie lamented the tendency of defense dollars to often determine scientific projects and the policies that guide engineering, engineering education, and the development of technology. If some of the best and most provocative work happens relative to naval needs and goals, does that not skew the larger world of scientific progress, professional education, and the choices we make as a society? Leslie's work puts the question in a more compelling form than does Foreman. This author must also take care in reading Leslie because I am from that generation that experienced the Vietnam War and its effect on American

society. The passion of the groups Leslie examines, from the student protesters on the one side to Charles Stark Draper on the other, brings me back to my days waiting for my number to come up in the draft lottery and I can feel that familiar social and political tension in his excellent narrative.[8]

However, while Leslie looks for science to divert attention from weaponry to more peaceful and constructive ends, he also sees the reasons for continued defense work at the laboratories, not all of which rested purely on the availability of funding. In my own effort to explore this same period, I discovered that underwater acoustics research not only offered the possibility of Soviet submarine detection, but also a much deeper understanding of the nature of the ocean, as well as ways of monitoring global warming, measuring tides, and determining water transport as well as the migration patterns of ocean mammals. Only by reaching across the boundaries between science and naval concerns can we attain this level of historical understanding.

We can see insights similar to those derived by Barth and some of Leslie's themes in Donald MacKenzie's historical sociology of nuclear missile guidance, *Inventing Accuracy.* MacKenzie very effectively illuminates the development of accurate missile targeting systems by Charles Stark Draper and others as both a product of a particular historical dynamic and as a social creation. He concludes that the achievement could only happen as a "complex process of conflict and collaboration." This author found MacKenzie's work very useful when working on *An Ocean in Common* and serving as head of contemporary history at the U.S. Naval History and Heritage Command. I followed MacKenzie's lead and, influenced by sociologist Clifford Geertz, closely examined the role of cultural translators in facilitating the kind of teamwork that both Barth and MacKenzie discovered as the primary factor enabling successful collaboration

between science and the military services. Cultural translators would use their knowledge of both communities, developed over time, through war, friendship, common interest, and training, to enable the communication necessary to permit both scientists and naval personnel to work together effectively. In that way they sought to satisfy both themselves and the goals of their patron.[9]

Very often those who define themselves as historians of science naturally have further questions to ask about the present subject relationship from their own cultural perspective. The question of classification frequently and legitimately arises. With the results of much naval-inspired research shrouded behind classification markings, how can shared knowledge advance humanity, inspire other inquiries, or encourage international collaboration? In reality, has the military truly taken control of the direction and nature of fundamental research? The debate on classification, although considered naive by many naval historians, has emerged over the years as a very legitimate concern. Michael Dennis, a historian of science whose work I respect, asked a question a few years ago in an article synthesizing the contributions of many scholars to a single volume of the journal *Social Studies of Science*. I paraphrase:

> What might have happened if the Navy declassified earlier than it did at the beginning of this century, the climate data that led many to eventually accept the reality of global warming?

This concern remains pressing and real. Might concerted action and policy come to the fore sooner?[10] This problem still confronts both historians of science and naval historians, often unnecessarily. Declassification takes place far too slowly and far too late.

The earth sciences offer yet another conundrum. In an article examining military influence on what he calls the environmental sciences, historian of science Ronald Doel looked

at the ways in which the Navy and other governmental groups influenced the selection of work considered essential, the ways in which scientists pursued these projects, and the selections of the questions they asked of the data. Can the process of scientific investigation and experimentation fall easily within the confines of an Office of Naval Research contract? ONR does not offer grants, but rather carefully composed contracts. In other words, can scientific inquiry have so clearly defined parameters that any scientist could project the beginning, middle, and end of an original research project in terms of a contract? What does this process do to the nature of scientific inquiry? Does this system encourage younger scientists to frame their work in a different way in an effort to achieve short-term results? In addition, if the system assists only those sciences the Navy values, does this amount to a setback for science in general? [11]

In addressing Doel's very legitimate concerns, the early years of the Naval Research Laboratory (NRL) provide an interesting and profitable set of observations. In his excellent dissertation on the development of radar at NRL, historian David Allison commented on the early years of research practice at the new laboratory created by the Naval Consulting Board and Congress. A large measure of the resources necessary to carry out research at NRL certainly came from the Navy bureaus, but the basic research money largely came from Congress. The internal administration of NRL initiated projects and dispensed the resources, set priorities, and managed projects according to its own judgment. [12] Only when direct naval application loomed on the horizon did the bureaus assume direction.

When ONR emerged as the Office of Research and Inventions in 1947, it operated in much the same manner. Interviewing Gordon Lill, head of the geophysics branch of ONR and the premier source of oceanographic funding in the 1950s and 1960s, provided me with unique insights into ONR operation. Similar comments by Arthur Maxwell, Lill's deputy and later head

of the Institute for Geophysics at the University of Texas, confirmed those insights. They both recalled that rules did not exist; they wrote them as they went along. The system was absolutely flexible. Their personal relationships with various major scientists, including a number of institution directors, led them to provide ONR's funds to those directors, permitting their trusted associates to invest the money for the Navy's purposes while serving science at the same time. Lill and Maxwell assumed the latter, and knew these friends and World War II veterans would make sure they never lost sight of the Navy's purpose. Columbus Iselin, Woods Hole director, once commented that without ONR money he and his colleagues could do little more than explore Buzzards Bay in their smallest research vessel, *Asterias*. [13]

Did the system change over time? It did, but many historians of science assume that those who used and administered it in the 1950s and 1960s would recognize ONR practice in the 1980s and 90s.[14] No professional historian should make that assumption. This knowledge becomes analytically important when one realizes that one key to the nature of the naval-science relationship in the 20th century largely rests with organizations like the National Research Council, the Naval Consulting Board, NRL, and ONR. The last became far more influential and powerful than any of its government-inspired cousins. These issues and institutions need more professional historical attention.

In some cases, ONR's policies and the Navy's priorities did occasionally leave some avenues of scientific research out in the cold. Doel especially points this out. Remember, ONR represented the only source of government funding for science after World War II until the advent of the National Science Foundation (NSF) in 1950. For years after the arrival of NSF, ONR still acted as the premier investor of government funds in science. For the Navy's part, physical oceanography arose as one of the most important avenues of inquiry, especially in the

realm of antisubmarine warfare and pro-submarine matters. As a consequence, the Navy paid little attention to disciplines like marine biology, which suffered from a lack of funding in the years after World War II, when ONR emerged as the premier source of funding for the ocean and earth sciences.

To those historically examining Navy-related scientific work, it often seems that the service's perpetual presence and considerable resources truly placed the scientific community in an awkward position. As a result, a number of influential works have analyzed certain topics in a way that resonates very well with the Distortionist Theory mentioned by Barth. One case in particular introduced me to the distortionist perspective as I initiated my research into naval oceanography back in the early 1990s.

In the summer of 1993, I rode a bus down from Logan Airport to Woods Hole, returning from a research visit to the Scripps Institution of Oceanography during my work on *An Ocean in Common*. Leaving the bus at the Martha's Vineyard ferry terminal, I noticed the scientist Allyn Vine walking down the street from his home on Juniper Point, just a short distance away. My wife had told him when I was due to arrive and he came down with a rather intense look on his usually jovial face. He carried with him a book entitled *A Fragile Power: Scientists and the State* by Chandra Mukerji, a sociologist attached to the communications department at the University of California, San Diego. He asked me if I had seen the study and I had to admit that I had not, even though it had appeared roughly four years earlier. He gave me a questioning look and simply said that the book did not describe the world in which he matured as a scientist. He felt that the book's author had totally missed the mark.

In the days following that encounter I read the book and immediately understood the problem Vine had with it. Mukerji's thesis asserts that the Navy used the funds dispersed by

ONR to reduce a cadre of scientists to the status of an auxiliary workforce, suitable for fulfilling the operational scientific needs of the service given the threats presented by the Cold War.[15]

It is my habit in working major projects to dive deeply into the archival primary sources with my own preliminary questions before exploring any extant secondary literature. While this may go against the graduate school grain, I believe it is important to have a preliminary grasp of the source material before appreciating how other historians and academics analyzed those documents and offered the conclusions they have drawn.

In the Mukerji case, this process proved particularly valuable. Initially, I noticed that I could not track any of the interview quotations, because the author granted all subjects anonymous attribution. A serious historian would want to evaluate the information offered by these people in a larger professional context. This could not happen in this study. I have done more than 400 oral histories in my career and no subject has ever asked me for anonymity. The need to withhold names seemed a bit odd in this case. The most important discovery I made while reading Mukerji's book related to ONR: While that office obviously played a very important role in her analysis, she never used the ONR records in RG-298 at the Washington National Records Center at Suitland, Maryland.

She explored the way the Navy and the oceanographic community interacted and never once used naval primary sources. Her analysis remained within the scientific stovepipe and followed carefully the observations of the eminent sociologist Robert Merton regarding the nature of the scientific community, its practices, expectations, and reward structure.[16] Mukerji's work offered no insights into the nature of the Navy in its interaction with ocean scientists. Quotations offered from the interviews she conducted confirmed her thesis, but a reader could

not evaluate the assertions or validate them in the context of the subject's work and career. As an informed historian in the field, I could only guess, which one simply cannot do.

Mukerji sees many scientists reduced to providing either sophisticated support to naval activity or the data necessary to operate naval weapons systems. As a scientist deeply involved in work with the Navy from the earliest days of World War II, Allyn Vine, the creator of the submersible *Alvin* (DSV-2) and the scientist who perfected the bathythermograph, did not recognize the world she described. That is because that world never existed.

Doubtless, all of it seemed perfectly reasonable to the author of *A Fragile Power* as seen from the perspective of a sociologist working with sources directly related only to the scientific community. However, she examined a *relationship,* which implied the presence of at least *two* entities. In this case, each had a strong cultural heritage, its own practices, professional habits, and technical language. The naval culture does not shine through in any of her narrative and naval intrusion into the scientific world via ONR appeared as an alien influence. Very little suggested either a fluency with things naval or an appreciation of ONR and its internal practices, the latter often based upon close friendships developed during World War II between senior scientists and those tasked with dispensing ONR resources to various scientific specialties.

Professor Mukerji also failed to explore the common use of summer studies like Projects Hartwell (1950) and Nobska (1956) which gave birth to systems like the undersea sound surveillance system (SOSUS) and the submarine-launched Polaris missile system. The summer study rationale brought together a critical mass of scientists and naval personnel to examine closely, precisely defined naval needs and issues over the course of a summer. The dynamic here would have revealed a great deal about the relationship Mukerji sought to examine, but her study gives no indication that she knew these things existed. Summer Studies still perform this

function, notably in the case of the on-call services of the *Jason*s, currently administratively supported by the Mitre Corporation.

The Mukerji study frequently appears in bibliographies of historical works on the Navy and science. Even when her views do not find universal acceptance, for some unknown reason they still help frame the debate and discussion over the Navy's relationship with science.

Any naval historian would find the results of historical analyses ignoring naval sources a bit questionable, both in a professional sense and as an indicator of simple research gone wrong. In an exceedingly long article, Naomi Oreskes explored naval research into oceanography and the discovery of the hydrothermal vents off the Washington and Oregon coasts.[17] While using the archives of the Woods Hole Oceanographic Institution and Scripps—save for a small number of primary sources related to the Naval Research Laboratory—this piece of work also ignores the Suitland collection of ONR records in RG-298. Orestes drew conclusions about the nature of the collaboration between science and the Navy that do harmonize with the better contributions to the literature. However, many of the links she makes in the course of her narrative contribute little to an accurate understanding of deep submergence, seemingly related projects, and the projects themselves.

Oreskes explores the origins of *Alvin* and *Aluminaut*, the deep-diving submersibles, linking them to the need to maintain the submarine acoustic warning nets in the Atlantic and Pacific, with secondary applications to basic science related to physical oceanography and ocean bottom geology. She never links the origins of *Alvin* to the 1963 loss of the nuclear-powered submarine *Thresher* (SSN-593). The inability to reach *Thresher* led the Navy to look more closely at this kind of vessel. Her assessment of Allyn Vine takes him at his word, that he saw himself as a Navy-related oceanographer. However, she never really tells us what that meant for

Vine, but uses this identity to demonstrate the close link between Navy goals and intentions and the origins of these submersibles. Her article offers no naval sources that might confirm or contest her assertions. She also links the submersibles to Project Artemis, an active acoustic detection system proposed for the Atlantic based upon an active, at-sea sound source with submerged Texas towers holding the acoustic reflection receivers. She uses sources prepared by eminent scientists Robert Frosch and Alan Berman but fails to inform us that the project never came to fruition owing to the size and unwieldy nature of the components. The passive acoustic SOSUS system presented more than sufficient warning of a Soviet submarine presence.

Not having explored naval sources and oral histories, she also failed to discover the dual nature of the word "Artemis," which presented some confusion at the time. While certainly the code name for the active system Oreskes described, it had another significant meaning. If she had read other studies or oral histories, especially those held by the Naval History and Heritage Command, she would have realized that Artemis, the goddess of the hunt, also stood, in many quarters within the scientific and submarine community, for a scientist actually named Hunt. Frederick V. Hunt of the Harvard Underwater Sound Laboratory issued the so-called bombshell report in May 1950, proclaiming the possibility of a passive system like SOSUS. Highly classified at the time, the very idea of a long-range, shore-based, passive acoustic detection system able to sweep an entire ocean in one hour, remained "behind the green door," and needed a convenient name to permit even classified discussion. "Artemis" performed that function before "Jezebel," "Michael," and "Project Caesar" took its place. Without that knowledge, much of Oreskes's narrative on Artemis becomes a bit confusing. Naval sources would have enabled a more coherent analysis.

Oreskes also briefly treats the summer studies used by the Navy and the scientific community to solve problems and set priorities. Her article gives these gatherings only passing attention in spite of the remarkable products and ideas that emerged from them. As I mentioned earlier, these studies would provide an excellent subject for an in-depth study of the dynamic that ruled the relationship under examination here.

The affliction affecting Oreskes and Mukerji in different ways has reached even further in the history of science literature. It appears in the works published more recently by Jacob Hamblin.[18] He produced a study examining international programs in oceanography during the Cold War. I was asked by the journal *Technology and Culture* to review the book, but when Professor Hamblin referred to civilian Assistant Secretary of the Navy James Wakelin as an admiral, I knew the study might present a few problems. Like Oreskes and Mukerji, Hamblin never used ONR records even though virtually every project he examined in the book doubtless drew on Navy funds during this period. He did use the strategic program files at the old Naval Historical Center's Operational Archives, but ignored the files on the Hartwell Summer Study, Project Nobska, and the Low Report housed in the same place. He understood the need to penetrate analytically down to the individual level, factoring in the personal relationships, but without the context that the ONR records would have provided. In an article derived from his book and published by the journal *Isis,* Hamblin concluded that

> [D]espite 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.[19]

The conclusion relative to "operational data" conforms exactly to the analysis presented by Mukerji. Hamblin consulted strategic program files, which would naturally review scientific

inquiries underway that should provide immediate help addressing the perceived threats that any strategy must confront. Those who composed these records would take pains to make sure the weapons systems would perform. However, Hamblin never consulted the ONR files that would directly reveal the nature of the relationship between the Navy and the scientific community that would appear in the ONR records. Only then would the files from WHOI, Scripps, and other institutions actually make sense. Since ONR enabled many of the summer studies, the reasoning behind them and the relationships that made them so productive would also reside there. Hanblin's work in the records of the National Academy of Sciences National Research Council files should have provided an additional naval perspective because of ONR involvement and the maritime emphasis of a good number of the projects he examined. Both in his book and the article on the Navy and science he published in *Isis* in 2002, naval culture does not emerge. He never took the time to explore the nature of the records sufficiently to understand the culture of the scientific community's partner in these cases. He remained within the stovepipe with all of his conclusions.

Many historians of science also feel that the ONR records reside under a cloak of classification. Actually, the bulk of the documents most useful in cases related to the environmental sciences and oceanography no longer carry restrictions or actually offer perfect cases for Freedom of Information Act (FOIA) actions. Furthermore, any exploration of these records should happen in conjunction with oral histories left by the major players. Professor Hamblin knew of the interviews resident at Scripps and Woods Hole, but never looked at those housed at the U.S. Navy's Operational Archive, done with many of the seminal players in these matters. Only through use of those interviews would the actual nature of the Navy-science relationship begin to emerge.

Unfortunately, no comprehensive history of ONR has yet appeared. The history sponsored by the Sloan Foundation and ONR written by Harvey Sapolsky in 1990 never achieved the scope envisioned by the original project. Professor Sapolsky felt obliged to turn his attentions elsewhere and the history became six rather brief chapters that did not deeply probe the nature of the organization or sufficiently explore its significance. The first three chapters provide a much needed exploration of ONR's origins, but the last chapters do not offer the detailed insight that the literature needs. That history still awaits composition. However. Professor Sapolsky did explore the changes in ONR, owing to close congressional oversight, that altered the habits of the first two postwar decades into the stricter contract system many of the history of science community currently identify with ONR practice. [20]

As an avenue of naval-scientific advancement, albeit of a very different sort, the NRL commissioned science writer Ivan Amato in 1998 to prepare a 75th-anniversary narrative. Amato provided an excellent foundation for an appreciation of the laboratory's origins and achievement. This work provides a sound basis for appreciating the many very specific reports about NRL's scientific activity that do not historically reach outside the realm of naval culture.

To understand NRL's role in naval science, one has to appreciate the laboratory not as a commissioning agent on the ONR model, but as a practitioner. In this role some of the best historical work comes from historians like David Allison, mentioned earlier, and the works of the late NRL historian David van Keuren on progressivism, science, and the military as exemplified by the NRL in its early years, as well as Hoyt Taylor's 25-year review of NRL's history penned in 1948.[21] The functioning and nature of the laboratory emerged in studies like historian Bruce Hevley's examination of NRL's work in ultra-violet and X-ray astronomy.

NRL deserves a great deal more attention, especially for its work appreciating various geospatial aspects of the Earth. NGA has taken particular note of the NRL's activities in this sphere. A bureaucratic blunder caused the National Archives and Records Administration to dispose of the records documenting NRL's role in the Vanguard program. In spite of this tragedy, the 1969 history of that effort published as a NASA report took the naval effort out of the laboratory's realm and moved it into the larger initiative that drove the early space flight competition with the Soviet Union. This examination of the Vanguard missile and satellite program by Constance McLaughlin Green and Milton Lomask provided an indication of the Navy's role, exploring NRL's contribution as well as the support provided by ONR. These historians made a beginning that other documentation and oral histories might analytically resolve in the future.

The sources feeding the history of the Navy's relationship with science clearly demonstrate a work-in-progress led and facilitated by individuals who could appreciate the cultural divergence between the two communities and yet still recognize the possibilities. In writing his book *An Ocean in Common,* this author discovered this truth emerging repeatedly from all manner of primary sources. In the aftermath of World War I, the relationship and its productivity rested with personal ties between scientists and ranking naval officers. Two of the most important ocean scientists of the century, Roger Revelle and Richard Fleming, went to sea on their first major cruise because Thomas Wayland Vaughan, director of the Scripps Institution, knew the Hydrographer of the Navy who needed his help. Arrangements between professional colleagues counted for much of the opportunity for training and advancement. Allyn Vine, creator of the *Alvin* submersible, and J. Lamar Worzel, co-discoverer of the deep sound channel and its amazing acoustic transmission properties, found themselves at Woods Hole as World War

II began. They came with Maurice Ewing, an emerging leader in the field of geophysics and at the invitation of Columbus Iselin, the director of the WHOI. Their ability to understand the Navy's needs and communicate the value of their work saved countless lives. Ewing taught them, as Karl Heinrich Barth demonstrated in his history, that they could do groundbreaking science while satisfying the needs of their naval patron. The two were not mutually exclusive.

Friendships forged by the war did not end in 1945 with the conflict. Much of what ONR became rested on the personal trust established between naval officers and scientists, between ship-drivers and seekers, made firm by cultural translators who understood both communities and could facilitate between them. Part of the future in this field must determine how and why that changed. The relationships touched by the Navy had a flexibility and rested on a personal basis, much of which has eroded. We need to understand that process. A lack of understanding or just poor research and analysis has resulted in the distorsionist view discussed earlier. The Navy's relationship with science and scientists became much more professionally intimate than many historians of science realize.

In conclusion, let me share with you a mistake that I made, one that I hope other naval historians will not repeat. When Allyn Vine gave me Chandra Mukerji's book, I read it, suspected the problems that it had, but I never sought to review it for the larger naval history community. I should have. A conversation about her approach needed to begin at that point. We now need to do these things. Naval historians need to enter this intellectual debate aggressively. We need to explore the history of science and the Navy in a way that respects Duncan, Barth, Williams, McKenzie, Leslie, and others. We need to challenge analyses that fall terribly short and we need to fill gaps in the literature. We need to address the interaction between the Navy

and science in a professional way; in a way that our graduate mentors prescribed all those years ago when we learned how to do history. In the coming days, if a young naval historian decides to look at the summer study phenomenon alone, so much about the relationship will emerge. Naval historians need to challenge those who have virtually ignored ONR and RG-298. You cannot validly examine the relationship in question here without the ONR records. It is simply not possible.

Find the excellent literature, not all of which I could address in my time here, learn from it, and build upon it. Because of some of the flaws I have mentioned, one can do a great deal in this field, but only by taking care to explore and understand the part of the relationship that remains external to the naval experience. Bring yourself to the point at which fluid intellectual movement between cultures becomes possible. Know the sources, interview as many of the players as you can. Your analysis will broaden and your insights deepen. That exploration will prove fascinating. In the process you will illuminate both the Navy and its most significant partner.

Dr. Weir is chief historian of the National Geospatial-Intelligence Agency.

---

[1] James Phinney Baxter, *Scientists Against Time* (Cambridge, MA: MIT Press, 1968).
[2] David Allison, "U.S. Navy Research and Development since World War II," Merritt Roe Smith, *Military Enterprise and Technological Change* (Cambridge, MA: MIT Press, 1985), 289–328.
[3] Francis Duncan, *Rickover and the Nuclear Navy: The Discipline of Technology* (Annapolis, MD: Naval Institute Press [NIP], 1990); *Rickover* (Annapolis, MD: NIP, 2001).
[4] William M. McBride, "The 'Greatest Patron of Science': The Navy Academia Alliance and U.S. Naval Research, 1896–1923," *The Journal of Military History* 57: 7–33.
[5] Kathleen Broome Williams, *Grace Hopper: Admiral of the Cyber Sea* (Annapolis, MD: NIP, 2004); *Improbable Warriors: Women Scientists and the U.S. Navy in World War II* (Annapolis, MD: NIP, 2001).
[6] Paul Foreman, "Behind Quantum Electronics: National Security as a Basis for Physical Research in the United States, 1940–1960," *Historical Studies in the Physical and Biological Sciences* 18 (1987): 149–229.
[7] Kai-Heinrich Barth, "The Politics of Seismology: Nuclear Testing, Arms Control, and the Transformation of a Discipline," *Social Studies of Science* 33, no. 5 (October 2003): 743–81.

[8] Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993).
[9] Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge, MA: MIT Press, 1993); Clifford Geertz, *Local Knowledge: Further Essays in Interpretative Anthropology* (New York: Basic Books, 1983).
[10] Michael Aaron Dennis, "Earthly Matters: On The Cold War and the Earth Sciences," *Social Studies of Science* 33, no. 5 (October 2003): 809–19.
[11] Ronald E. Doel, "Constituting the Postwar Earth Sciences: The Military's Influence on Environmental Sciences in the USA After 1945," *Social Studies of Science* 33, no. 5 (October 2003): 635–66.

[12] David Allison, *New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory*, NRL Report 8466 (Washington, DC: NRL, 29 September 1981), 52–53ff.
[13] Gary E. Weir, *An Ocean In Common: American Naval Officers, Scientists, and the Ocean Environment* (College Station, TX: Texas A&M University Press, 2001).
[14] Harvey Sapolsky, *Science and the Navy: A History of the Office of Naval Research* (Princeton, NJ: Princeton University Press, 1990), ch. 1–3.
[15] Chandra Mukerji, *A Fragile Power: Scientists and the State* (Princeton, NJ: Princeton University Press, 1990).
[16] Robert K. Merton, *On Social Structure and Science* (Chicago: University of Chicago Press, 1996).
[17] Naomi Oreskes, "A Context of Motivation: U.S. Navy Oceanographic Research and the Discovery of Sea-Floor Hydrothermal Vents," *Social Studies of Science* 33, no. 5 (October 2003): 697–742.
[18] Jacob D. Hamblin, *Oceanographers and the Cold War: Disciples of Marine Science* (Seattle: University of Washington Press, 2005).
[19] ———, "The Navy's 'Sophisticated' Pursuit of Science: Undersea Warfare, the Limits of Internationalism, and the Utility of Basic Research," *Isis* 93: 1–27.
[20] Harvey Sapolsky, *Science and the Navy: A History of the Office of Naval Research* (Princeton, NJ: Princeton University Press, 1990).
[21] David van Keuren, "Science, Progressivism, and Military Preparedness: The Case of the Naval Research Laboratory, 1915–1923," *Technology and Culture* (1992); A. Hoyt Taylor, *The First Twenty-Five Years of NRL* (Washington, DC: Navy Department, 1948).