Introduction

"There's no such thing as tainted money, except 't ain't enough"—so a senior colleague told me many years ago, when I was first raising money for my own scientific research.[1] Like many jokes, this one contains a germ of truth and masks an anxiety: some money *is* tainted. And it suggests a serious question: what difference does it make who pays for science? Many scientists would say none at all. If scientists seek to discover fundamental truths about the world, and if they do so in an objective manner using well-established methods, then how could it matter who foots the bill? History, alas, suggests that it does matter. Few patrons have ever supported science for the love of knowledge alone; most have had orthogonal (or at least oblique) motivations, be they prestige, power, or the solution of practical problems, and the available evidence suggests that those motivations make a difference.[2] On the positive side, patrons can encourage scientists to attend to neglected questions, consider matters from new angles and perspectives, or try a new approach. In medical research, we have seen how patients have positively influenced researchers who previously neglected important questions.[3] Historians of technology have shown how the demands of industry and commerce can stimulate scientific innovation.[4]

On the negative side, however, the interests of patrons may cause scientists to focus on immediate answers to pressing problems at the expense of fundamental understanding (which, as we shall see in this book, many Cold War oceanographers feared would happen to their field). The pressure of external deadlines can cause scientists to take shortcuts, make mistakes, or miss important elements of a problem. The needs of funders may also introduce bias into the design of scientific studies, as when scientists funded by the chemical or plastics industries choose test animals known to be insensitive to the potential effects of concern, or into the data interpretation, as when scientists funded by the tobacco industry fail to find the adverse effects of smoking that their independent colleagues find.[5] Most worrisome, the demands of patrons may grossly distort science, as when, under the pressure of the Soviet government in the 1930s, Trofim Lysenko rejected advances in modern genetics in favor of an empirically inadequate theory of environmentally dominated inheritance, development, and growth, and used his political power to discredit colleagues who disagreed with him.[6]

Most scientists would like to think that these sorts of problems and pitfalls are rare and that scientists—both individually and collectively—are sufficiently smart and self-aware to recognize and avoid them. We like to think of Lysenko as the exception that proves the rule—a grotesque intellectual expression of the broader horrors of Stalinism.[7] Indeed, it is easy to compartmentalize problematic cases as anomalous, to assume that they are exceptions, or to think that they apply only in narrow domains where the interests of sponsors are overt or extreme. Sadly, these assumptions have been shown not to hold. There is empirical evidence to demonstrate that scientists have been overly optimistic about their ability to maintain their intellectual integrity, particularly in cases where the desiderata of their funders are obvious, as with tobacco or pharmaceutical research.

This is not to suggest that the interests of funders are necessarily at odds with those of the funded. In many cases scientists and their patrons have a shared interest in gaining knowledge, which enables them to work productively together. Under such circumstances, the positive impact of funding is obvious—scientists get to do work they want to do. Any negative impact, however, is subtle and harder to discern. Oceanography during the Cold War is a case in point.

The Transformation of American Ocean Science

Before World War II, no American earned a living as an oceanographer investigating the oceans beyond coastal waters. Two institutions encompassed American oceanography—the Woods Hole Oceanographic Institution in Massachusetts and the Scripps Institution of Oceanography in La Jolla, California—and both were small, young, and poorly funded.[8] In fact, neither was really "oceanographic," because most of their scientists did their work at the seashore or in small boats that plied coastal waters. Woods Hole had no year-round staff and no regular external funding.[9] Scripps had a scientific staff of eleven, kept barely afloat by the institution's modest endowment income and funds from the University of California that covered salaries and operating expenses but left little for research.[10] In 1940, Scripps launched its first expedition, to the Gulf of California, with a hard-won grant of $10,000 from the Geological Society of America. Money was so tight that the institution's director implored his scientists not to stay away even a day longer than planned, for "we shall find ourselves in a deep pit when you return. . . . Funds are pitifully low."[11]

The same was true for marine geology and geophysics. The structure and composition of the ocean basins, pertinent to global tectonics, was of enormous interest to geologists, but a lack of access to the deep sea meant the topic was more speculation than investigation.[12] In 1935, Lehigh University geophysicist Maurice Ewing helped to invent the field of marine geophysics by applying seismic techniques developed for shallow oil exploration to study the Earth's deep crust. Ewing completed the first comprehensive geophysical study of the structure of a continental margin, but he hadn't a clue what to make of his results.[13] Conventional wisdom posited a sunken Paleozoic continent off the east coast of North America to account for thick Paleozoic sequences in the Appalachians, but the data revealed not a trace of it.[14] Ewing turned to his Lehigh colleague, geology professor Benjamin L. Miller, but he couldn't make sense of the results, either.[15] Miller supposed, tentatively (and in hindsight wrongly), that "somewhere in the Atlantic Ocean, there [must be] extensive Paleozoic strata."[16]

Within just a few years, matters would change dramatically. As war spread in Europe and a US entry appeared likely, American military planners recognized that this second world war would not be like the first. Throughout human history, warfare had taken place on two-dimensional battlefields—the surface of the land or the sea (or, in the case of U-boats, just barely beneath that surface). The impending war would be fought not only on those battlefields but also in the air above and the sea below them. After World War II, the newly formed US Air Force would look to the skies, the upper atmosphere, and even outer space as theaters of warfare; the Navy would look to the deep sea. The earth sciences—particularly physical oceanography and marine geophysics—would become crucial for antisubmarine warfare, weather and surf forecasting, undersea communications, navigation, air-sea rescue, vessel design and testing, submarine-based ballistic missile launching, the tracking of atomic bomb fallout, and a number of other operational ambitions and concerns. As Roger Revelle put it in 1947 in a report to Navy officials, during the war "a knowledge of oceanography was proved essential," and in the future an "increased emphasis on subsurface warfare in which a thorough knowledge of the medium is of prime importance" would lead to an even greater "requirement for oceanographic information." And there was scarcely an aspect of the ocean that was not operationally relevant: "The Navy, which operates on, under, and over the sea will be improved in effectiveness and striking power by precise knowledge concerning every aspect of the oceans."[17] Revelle was a scientist trying to make the case for his science, but relevant Navy officials and political leaders apparently agreed.

It is well known that the war economy provided unprecedented levels of research funding for American physics, particularly for the development of weapons systems. What is less known is how much was invested in understanding the environments in which those weapons systems would operate.[18] It is not that the US military suddenly discovered the value of earth science: meteorology, economic geology, geodesy, and cartography had long associations with terrestrial military campaigns, and navies had long recognized the operational value of various forms of oceanographic data.[19] Rather, it is that warfare itself was changing in a manner that required new kinds of scientific information, some of which could be obtained only with the help of innovative scientific research and nearly all of which increasingly seemed imperative.[20] The result, as historian Jacob Hamblin has put it, was that over the next half century, oceanographic science became "unsurpassed in its interconnections with the American military-industrial complex."[21]

With the expansion of submarine and antisubmarine warfare, questions of the internal configuration and conditions of the deep ocean would no longer be the domains of science fiction writers and their imaginations or scientists and their speculations; they would be domains of knowledge essential to military operations. During the Cold War interest in the deep sea would intensify as the submarine-launched ballistic missile became an arm of the nuclear triad, and as the US Navy built a global listening system to detect Soviet submarines carrying ballistic missiles of their own. It would not suffice simply to put sophisticated weapons on submarines. It would also be necessary to understand the environments through which those submarines would have to travel and from which those weapons would be launched.[22]

The result was an influx of money and logistical support that transformed American oceanography and marine geophysics and led to remarkable growth of scientific knowledge about the oceans, the seafloor, and the life associated with those domains. Scientists answered questions in which they had long been interested and also discovered some entirely new things. The Cold War was a not just a period of unprecedented growth in American oceanographic science; it was also a period of unprecedented growth in oceanographic knowledge.

Not surprisingly, when oceanographers look back on the Cold War, they tend to see it as a golden age, a time when they had both funding and freedom and used them to great effect. In the spring of 2000, the US Office of Naval Research (ONR) sponsored a series of colloquia across the United States

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to celebrate that history. As part of the effort, the ONR commissioned oral history interviews with oceanographers and geophysicists whose research it had supported. Most of them sang the ONR's praises, largely because of its support for basic research. Douglas Inman, a senior researcher at Scripps, put it this way: "They *were* the basic science supporters in this country. If you look at who supported basic science . . . after World War II, it was ONR."[23]

The view that the US Navy, and particularly the ONR, freely supported scientific investigations without regard to military utility has long been widely held. In 1948, for example, in the aftermath of political attacks on physicist Edward Condon, a writer in *Fortune* alleged a crisis in American science caused by increasing restrictions on scientists' activities, decreasing intellectual freedom, and a lack of moral and financial support for basic research. The bright spot in this otherwise bleak landscape, the writer claimed, was the ONR, which "stepped into the breach created by the delay in establishing a civilian National Science Foundation, and was generously supporting pure research, with no strings attached, and a maximum of freedom for working scientists."[24] This early representation became the standard view, which over time developed into a prevailing narrative that Navy funding did not affect the science, except to make it possible. But was it true?

As is typically the case, historical attention suggests a more complex situation.[25] In his 1990 history, political scientist Harvey Sapolsky suggested that broad-ranging support of diverse research was characteristic only of the ONR's first few years, before the creation of the US National Science Foundation and at a time when few officials in the Navy were paying much attention to what the ONR program directors were doing.[26] Moreover, there is something peculiar about the claim that the ONR supported basic research without regard to salience—if this were true, it would stand at odds with its legal mandate to support research on behalf of the Navy mission. For ONR officials to have supported "pure research, with no strings attached," would have meant that they were not actually doing their job; they might even have been guilty of misappropriating federal funds. Wouldn't it make more sense to assume that they funded research that matched their goals, or that Navy funding involved various constraints, some innocuous but others perhaps not? After all, as biochemist Erwin Chargaff quipped in the 1970s, "If oratorios could kill, the Pentagon would long ago have supported musical research."[27]

Moreover, the ONR was one of several Navy bureaus that supported oceanographic and marine geophysical research during the Cold War. The other offices included the Bureau of Ships, the Bureau of Naval Weapons, the Hydrographic Office, and the Chief of Naval Operations.[28] It would have been inconsistent with the goals—indeed, the legal obligations—of these bureaus to spend resources on activities irrelevant to their mission. When we broaden our compass to examine Navy support for research in general, we find a far more complex and thought-provoking story than the one that has been told to date.

Why Oceanography?

Historians of physics have long been interested in the impact of military funding on their science in the Cold War, and their work provides us with some orientation and guidance. In the mid-1980s, the impact of military patronage was addressed head-on by two of the most influential historians of modern physics: Daniel J. Kevles and Paul Forman. Forman suggested that military funding had dramatically altered the character of physics, causing its practitioners to drift from an earlier goal of fundamental understanding of the laws of nature toward a science of "gadgeteering" that was preoccupied with technical prowess.[29] Kevles disagreed. He acknowledged the reality and significance of the military's pervasive patronage but insisted that American physicists had "retained control of their intellectual agenda."[30] He also insisted that there is little sense in arguing about what scientists might have done in a different world: History is the story of what has happened, not what might have happened. In any case there is no essential definition of what constitutes physics—physics is what physicists do. So Forman's claim that physics was "distorted" by Cold War concerns is not—and could not be—a historical claim.[31]

Kevles's argument had intuitive appeal to many historians of science, who reject essentialist notions of science and believe their job to be describing the world as it is, not as it might, should, or could have been.[32] "Science" as a category is both flexible and the subject of ongoing diminution and augmentation, so the observation that science changed during the Cold War—or during any particular historical period we might examine—is not by itself profound. Moreover, thanks to the work of Kevles and Forman, as well as that of many other historians since the 1980s, the idea that American science changed dramatically in the second half of the twentieth century is no longer novel, either.[33] What is of interest—and still not entirely resolved—is how it changed, why it changed in those particular ways, and how those changes were productive of our current states of knowledge and of ignorance. These are the questions I take up in this book.

Since the debates of the 1980s, historians of science have greatly broadened their outlook; there is now a robust literature on the history of the diverse sciences during the Cold War, both in the United States and elsewhere.[34]

Nearly all historians agree that American science took a dramatic turn during and after World War II, a turn that in myriad ways changed the priorities and perspectives of scientific communities. Toward the end of World War II and throughout the Cold War, the US government poured unprecedented amounts of money and levels of logistical support into American science. Scientists often focus their attention on the National Science Foundation and National Institutes of Health as their most important patrons, but during the Cold War in many domains the lion's share of the support came through the armed services. Much of this was to support areas of science, such as oceanography, that had been poorly funded or even had scarcely existed before. So this influx of funding mattered profoundly. One important way in which it mattered involves the question of the direction of science and who or what determines that direction.

Most oceanographers welcomed the wartime infusion of military funding, correctly anticipating the opportunities that it would create, the work it would enable. But, as the stories told in these pages will show, paths of inquiry were also shifted—if not entirely altered—and sometimes blocked. Moreover, many scientists were concerned that an expanded relationship with the US Navy would cause them to lose control of their science—the very thing that Forman argues happened in physics.[35] As World War II gave way to the Cold War and the US Navy became the principal patron of American oceanography, these concerns did not go away. Quite the contrary. They continued to express themselves in terms of anxieties—often privately felt, sometimes publicly expressed—and occasional open conflict over secrecy, the right to publish, and, above all, the question of who was setting the research agenda. The Cold War was a golden age of science in terms of the abundance of work that was made possible by generous financial and logistical support, but it was also a period of deep anxiety and profound conflict over the purpose and character of American science. These anxieties and conflicts—both personal and professional—animate the stories told here.

Throughout the Cold War and even after it was over, the oceanographers whose stories are told in this book insisted that they were doing "basic research," and it is true that for the most part they were not trying to solve specific operational problems. However, we will see in these pages that the Navy supported oceanographic work not *qua* basic research but because it was salient to specific problems the Navy was trying to solve. Even before the Cold War ended, scientists found that when those problems were solved—or resolved by other means—Navy support weakened and sometimes ended entirely. Research that was not salient was not funded, and therefore not done, unless other sources of support could be found. When the Cold War did end—and with it the geopolitical context that had justified the support they had been receiving and motivated much of the work they were doing—oceanographers found themselves scrambling to resituate themselves and their science, a project that did not always go well.

Forman and Kevles were right in what they affirmed but wrong in what they denied. Oceanographers, like physicists, were by and large grateful to have the abundant funding that made it possible to answer important and long-standing questions in earth science. In this sense, the Cold War was indeed a golden age for American oceanography. But many oceanographers were deeply concerned about matters of intellectual control and worried that they might lose—or were already losing—control of their science. Oceanographers worked to take advantage of the opportunities represented by military funding to improve their understanding of the natural world, but at the same time they struggled to preserve the degree of autonomy that most of them believed was essential to the pursuit of knowledge.[36] How did their goals intersect and interact with the aspirations of their funders, and how did they negotiate or adjust those goals when they needed to? The history presented here challenges the conventional dichotomies of autonomy and capture, intellectual integrity and corruption, pure and tainted. We will see that military support for oceanography and marine geophysics was both enabling and constricting. It resulted in the creation of important domains of knowledge, but it also created significant, lasting, and consequential domains of ignorance.

Scope of This Book

The primary focus of this book is on three institutions—the Scripps Institution of Oceanography, the Woods Hole Oceanographic Institution, and the Lamont Geological Observatory—that were the leading centers for oceanography and marine geophysics in the United States at the time (and remain among the most important institutions of their kind in the world). Roger Revelle would overstate the case when he claimed after World War II that Woods Hole and Scripps were the "only two institutions that have contributed in any way to military oceanography," but they (and Lamont) did receive the greatest abundance of military funding, and a rich archival reservoir has made it possible to reconstruct what they were doing in (what I hope is) convincing detail.[37] My concern is not with Scripps, Woods Hole, or Lamont as institutions per se, but as sites where both seminal scientific breakthroughs and painful contestations occurred. I therefore also include a discussion of the work at Princeton University of Harry Hess, who made key contributions to the theory of plate tectonics but also played a major role in challenging Navy data classification.

My focus on these particular institutions should not be read as disparaging work done elsewhere. I have not, for example, looked at oceanographic research within the Navy, such as at the Hydrographic Office—although the Naval Research Laboratory makes several appearances. I discuss only in passing other academic institutions where oceanographic work was already being done in the 1930s, such as the University of Washington, or that became important centers for oceanography in the 1960s, such as Oregon State University and the University of Rhode Island. Centers of oceanographic work outside the United States play a role in the story only insofar as they highlight important points about American oceanography by comparison. In this sense, this book is not a history of oceanography; rather, it uses oceanography to address the question of the impact of funding on the subject, scope, and tenor of scientific work.

I have not attempted to explore the Navy "side" to my story in an equivalent manner as the scientific "side." Some readers may feel that I have given short shrift to the military perspective on the collaborations that drive this story, but that analysis has already been undertaken by Navy historian Gary Weir.[38] That said, I have endeavored not to make the Navy monolithic, stressing throughout that the military side of this story is not only—and perhaps even not even primarily—represented by the Office of Naval Research.[39] However, what mattered most for scientists was not what their military patrons really needed—if such a thing could be ascertained—but what scientists thought they needed and, more important, what scientists could persuade them to fund.[40]

Finally, I have not attempted to compare the impact of Navy funding with other patrons of earth science, such as industry. If I had, this book would have threatened (even more than it has) to spiral out of control. In any case, it would have been a different book. In the conclusion, I do offer some general conclusions about what made the Navy a good patron from the perspective of many earth scientists, and even, to a certain extent, from my own perspective.

Throughout this narrative, I have tried to understand how scientists took positive advantage of the opportunities presented to them in the Cold War and how they navigated the twin challenges of military expectations and military secrecy. Above all, I have tried to determine whether Navy patronage affected the content of the scientific work that was done and, if so, how. I want to show what scientists learned—and did not learn—on the Navy's dime, and what difference it made that it was the Navy, and not some other patron, who paid for this work. To the extent that I am concerned with balance, it is that I am equally interested in the production of knowledge and of ignorance.

The book also seeks to explain why, throughout this time period, oceanographers downplayed the impact of Navy funding. For, despite saying generally good things about the Navy as a patron, oceanographers have tended to de-emphasize the Navy role, as if it could just as well have been the Forest Service or the Post Office that funded them. During the ONR's anniversary, scientists rarely identified anything particular about the Navy that they considered salient—other than the claim that the Navy funded basic research.

The stories told here challenge that framework. I argue that the Navy role was highly salient. Most obviously, Navy priorities largely set the research agenda. The chapters that follow provide concrete examples of projects that were rejected because they did not fit "the mission profile," including some that in hindsight are of obvious societal importance and intellectual significance. We will see how Navy priorities focused attention on particular natural phenomena that in some cases inspired productive lines of thinking and investigation, but in other cases thwarted them. We will also see how Navy control of information created large domains of classified knowledge not available to scientists who did not have a "need to know." And we will see how fifty years of Navy sponsorship had cultural consequences that affected what scientists could do when the Cold War ended.

Because some scientists might misread my intent, let me stress this: I am not saying that the oceanographers and geophysicists in this story were bad human beings or were necessarily ill-motivated. This book is not an exposé. Readers may conclude from chapter 8 that Charles Hollister broke the law when he used government funds to lobby for deep-sea disposal of nuclear waste, or from chapter 9 that scientists at Scripps made serious errors of judgment when they discounted both public opinion and the views of other scientific experts to push aggressively for a project that they wanted but that others found problematic. For the most part, however, the scientists in this history did not do anything wrong: no clinical subjects were exploited, no higher animals were sacrificed.[41] To the extent that the American people knew about this work at the time, they had little complaint with it. On the contrary, during the Cold War, the need for an expansive, sophisticated military presence in the global oceans was broadly accepted, and some scientists were proud to acknowledge the link between their scientific work and the geopolitical exigencies of the Cold War.[42] But many scientists downplayed—and some even lied about—the military linkages, insisting that the Navy was supporting them to do basic research even when that was manifestly not the case.

Why did these scientists feel the need to insist that they were doing basic science, to downplay the interconnections between their research and military matters, and, above all, to insist that they had not lost control of their intellectual agenda? I suggest that they were, in fact, quite worried that if they had not yet lost control of their science, they well might.

Chapter 1 begins by investigating a disturbing incident in the 1930s. Most people today are likely to assume that if anyone were to object to military funding of scientific research, it would be political liberals; certainly that was the case during the Vietnam War years. But in the late 1930s, a group of conservative faculty at Scripps objected to Navy funding on the grounds that it would threaten the autonomy of science. These men were part of the so-called freedom-in-science movement, which opposed government funding or direction of science as socialistic, a threat to scientists' intellectual autonomy, and a threatening impediment to scientific progress. The conservatives lost this debate and were proved wrong: government funding flowed, and it did not lead to the socialization of science. But the conflict did create a lasting schism between the conservative faculty and the institution's Norwegian director, Harald Sverdrup, who a few years later was accused by the same faculty of being a Nazi. This led to Sverdrup's being denied security clearance and prevented from working on wartime classified military projects, undermining his leadership position at the institution. It also raised troubling questions as to whether he would be able to direct its programs after the war, since many of those programs would be at least partly classified or would rely on classified data. As a result, Sverdrup left both the United States and the field of oceanography. He had welcomed military funding as good for his science, but it proved bad for him personally.

Chapter 2 dives into the question of whether scientific patronage can affect not just the questions asked but also the answers obtained, tracking the development of one of the most important theoretical advances of twentieth-century oceanography: the Stommel-Arons model of deep-ocean circulation. Before midcentury, it was a matter of considerable debate as to whether there were deep-ocean currents and, if so, what forces could drive them. The question was answered affirmatively by Henry Stommel's pathbreaking work with physicist Arnold Arons on deep-ocean circulation driven by density gradients and the rotation of the Earth. Many senior scientists point to this work as exemplary of basic scientific research supported by the US Navy. However, Stommel's work was not independent of military concerns. Not only was it linked to problems in sonar transmission, but the central insight on which it was based—the existence of the thermocline—was a direct outcome of operational work. If there is a relation to be discerned here between basic and applied science, it is the inverse of what is often asserted: basic science did not lead to application in this case; rather, an operational problem led to a fundamental scientific insight. Had Stommel not been paying particular attention to the thermocline—had he not been troubled by its existence in a way that few, if any, oceanographers before him had been—he would not have framed the problem in the way that he did. A specific operational problem led him to attend to something that others working on deep circulation had ignored, and this led to the insight that became the basis of his theoretical breakthrough.

Chapter 3 continues investigation of the issue of scientific autonomy through the story of the "Palace Revolt," a faculty mutiny at Woods Hole in the early 1960s. The revolt was triggered by the very developments that the apprehensive scientists at Scripps had feared in the 1930s: the role of the Navy in setting the research agenda and the prioritization of Navy needs over the interests of fundamental research. Feeling that their director was too responsive to Navy needs and too little invested in basic science, a group of Woods Hole faculty demanded that the trustees ask for his resignation. Their demand was rebuffed, the mutinous faculty driven out, and research at Woods Hole continued to be heavily directed toward science that, in the director's words, "fit the mission profile."

Chapters 4 and 5 address the question of secrecy. It has long been an article of faith among many scientists that research must be free and open to operate or, at least, to operate optimally. Yet historically, a great deal of science has been done in secret. This was particularly true in the Cold War. Did it matter? In chapter 4, we follow Harry Hess, the Princeton professor credited with developing the concept of seafloor spreading, the crucial idea that laid the foundations for the theory of plate tectonics. Hess felt that secrecy was stymying his science, and he worked his Navy contacts to try to get key data declassified, but without success. In chapter 5, I follow marine geologists Bruce Heezen and Bill Menard, who were both thwarted by security restrictions in trying to interpret key data from the seafloor.

Hess feared that military secrecy would be bad both for military officers who would not know what useful information existed and for scientists who did not have access to information that could advance their field. But he went further than arguing that secrecy was problematic in principle: he claimed that science was being impeded in practice, that advances were not being made because of military-imposed secrecy. Of course, it is impossible to prove that an advance was not made that under other circumstances could or would have been made, but in these two chapters, I argue that Hess was right: Navy secrecy stood in the way of the emergence of modern global tectonic theory.

The impact of secrecy also helps to explain comments that appear otherwise astonishing. In 1966, on the eve of the plate tectonics revolution, George Woolard, president of the American Geophysical Union who was also working closely with the US Air Force on gravity measurements related to missile guidance, complained that earth science was "in a bad way." Writing to National Aeronautics and Space Administration (NASA) director James Webb, Woolard griped that scientists needed an "earth program" comparable to the space program. He bemoaned that geoscientists had a bad history of "mixing fact with fiction in studying the earth, [and placing] too much significance ... on limited data."[43] Woolard lodged this complaint at the very moment that data collection in earth science had reached a historical zenith. There was no dearth of data, but, for the reasons that Hess bemoaned, there was a startling dearth of knowledge about and access to that data, even among leaders of geophysical science.[44] That dearth, I argue, had both social and epistemic consequences.

Chapter 6 and 7 continue the argument that Navy patronage impeded scientific investigations that were not seen as pertinent to Navy needs. In chapter 6, I explore the history of the deep submersible research vessel *Alvin*, which has long been touted for its role in "basic science," particularly the discovery of deep-sea hydrothermal vents and the remarkable biotic communities they sustain. But *Alvin* was not developed as a research vessel. It was developed to satisfy the demand for deep-submergence capacity to assist salvage operations and to develop a long-range active listening system to detect Soviet submarines. Although it may be hard to believe in hindsight, during the planning stages few Woods Hole scientists could imagine much scientific use for a deep-submergence vessel. But after *Alvin* became a research vessel, its early history and role in classified projects were whitewashed. Thus, I argue, contra earlier accounts, that scientists did not "paint their projects blue," taking basic science and pretending it had military relevance. Rather, they "painted their projects white," cloaking military projects under the cover of basic research. Chapter 7 explores how, even once *Alvin* became a research vessel, its agenda was still largely set by military demands, and projects that did not fit the mission profile were rejected even when they were of profound scientific interest.

Chapters 8 and 9 consider what happened to oceanographers as military funding began to wane and they needed to find new patrons and a new context of motivation for their work. Chapter 8 follows the work of Charles

Hollister, a marine geologist and dean of graduate studies at Woods Hole. When funding dried up for his research on deep-sea sedimentology, Hollister turned to the problem of radioactive waste disposal. For some years the US Department of Energy funded him to study the deep sea as a potential nuclear waste repository, but when the US government decided to focus instead on land-based disposal, he refused to accept that decision. Hollister became an outspoken political advocate for deep-sea disposal and an opponent of Yucca Mountain (the land-based alternative designated by the US Congress), writing opinion pieces, lobbying members of Congress, and appearing on television with the conservative pundit William F. Buckley to make his case.

The final empirical chapter, chapter 9, considers the problem of the legacies of Cold War funding through the lens of a sad saga that took place at the end of the Cold War. In the early 1990s, a group of oceanographers proposed a clever but controversial project: Acoustic tomography of ocean climate. It was clever because the scientists realized that military underwater listening systems developed in the Cold War could be used to determine whether the world ocean—and by implication, the entire globe—was warming and thereby prove the reality of climate change. It was controversial because they failed to attend to the fact that the sound transmissions they proposed using had the potential to disrupt a number of forms of marine life, including several species of endangered whales. When cetacean biologists and whale aficionados raised this concern, the oceanographers responded in an arrogant and dismissive manner. The result was a painful, prolonged, and expensive conflict in which the oceanographers—trying to shift their attention from military to civilian projects—found themselves distrusted by civilians who questioned their motivations and doubted that the military tigers had changed their stripes. I argue here that motivations matter in framing not only what scientists decide to do but also in how they are viewed by others, and therefore whether they are seen as trustworthy.

In the conclusion, I return to the motivating question of this book: What difference does it make who pays for science? The short answer is: a lot.

The Production of Knowledge and Ignorance

In my prior work on the history of debates over continental drift, plate tectonics, and anthropogenic climate change, I have been primarily interested in the production of scientific knowledge. I have queried how scientists decide when they have enough evidence of sufficient quality to say that a scientific question has been answered, as well as how they judge what constitutes "evidence" and "quality."[45] Here, I am interested in how military funding affected which questions scientists believed needed answering in the first place and how they went about answering them. I argue that, while Navy funding produced a great deal of scientific knowledge, it was also productive of considerable ignorance, not only by bringing some questions to the fore and pushing others aside, but also by structuring how scientists thought about the ocean and what they even thought the ocean was. The military context of motivation led oceanographers to view the ocean primarily as a medium through which sound was transmitted and men and machines would travel, and not as an abode of life. This, I argue, had significant, lasting consequences. Thus, I offer this work as a contribution both to the history of science—the study of the production of knowledge—and to agnotology, the study of the production of ignorance.

To write this book has required delving into the history of these scientists' work in considerable detail. As my longtime colleague Steven Shapin has put it (in a different context and with no pun intended), "The very possibility that history can contribute to the general understanding of human behavior arises from the depth of detail it can dredge up from the past."[46] This book has involved much dredging. Each chapter offers a detailed account of one particular discovery, research program, conflict, or failure in order to understand why the work was undertaken or the conflict erupted, and how and why it succeeded or failed.

I have attempted to tell these scientists' stories in their rich complexity in order to recover the texture of their lives *qua* scientists trying to understand complex phenomena that are vexingly difficult to access without expensive equipment and instrumentation, which in turn makes their work impossible without deep-pocketed patrons. This book is not one story, but a set of intercalated stories about a group of men and women (though mostly men) who worked to satisfy these patrons and sustain support for their research while maintaining their focus on the natural world and their own vision of what it meant to be a scientist. That challenge was not a trivial one: sometimes they succeeded, sometimes they did not. Sometimes they were happy; sometimes they suffered.

Every history is a history of choices made and not made, paths taken and forgone. Every history of science is a history both of knowledge produced and of ignorance sustained. The world as we know it is one of many possible worlds; it is the task of the historian to understand how and why it came to be this particular world.[47] Although the chapters in this book may be read individually, they are intended to be read collectively as narrative history, with each chapter illuminating one or more aspects of Cold War oceanography. Some chapters focus on a particular individual, and others on a particular development, discovery, or problem. Collectively, they attempt to paint a landscape of scientific work in a particular time and place. But while the time is the Cold War and the place is America, it is, more broadly, illustrative of the landscapes in which scientists strive to make durable discoveries about the planet we live on in a social and political world that is at least as complex and difficult to understand as the natural one.[48]