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ABSTRACT The present paper analyzes the transformation of seismology from a small academic discipline to a large academic–military–industrial enterprise during the 1960s. In the late 1950s scientists, diplomats, and policy-makers recognized that improved seismological knowledge was crucial for the detection and identification of Soviet underground nuclear-weapon tests. Consequently, the Eisenhower administration initiated a comprehensive research and development program in seismology, known as Project Vela Uniform. Vela Uniform, managed by the Department of Defense's Advanced Research Projects Agency, increased annual federal support for US seismology by more than a factor of 30. An analysis of the origins, mechanisms, and consequences of Department of Defense patronage for seismology is at the center of this paper. I emphasize the role of scientific advisory groups and mission agency program managers in negotiating the field's research directions. I argue that despite massive Department of Defense patronage, academic seismologists did not lose control over their field. They participated actively in the transformation of their discipline, realizing that arms control requirements offered a unique opportunity to modernize their field. This suggests that the case of seismology challenges some of the assumptions of the 'distortionist' theory, the dominant historiographical approach to science in the Cold War.

Keywords Cold War, knowledge production, military patronage, Project Vela Uniform, scientific discipline, seismology

The Politics of Seismology:

Nuclear Testing, Arms Control, and the Transformation of a Discipline

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Seismology, the 'science of earthquakes and related phenomena' (Richter, 1958: 3), was a small academic discipline throughout the 1950s (Barth, 2000: 25–64). Less than 10 years later, Cold War concerns had transformed the field. In the late 1950s, nuclear-arms control negotiations between the USA and the Soviet Union focused on how to monitor a possible nuclear test ban agreement. US and Soviet scientists agreed that underground explosions posed the major technical and political problem. They also agreed that seismic techniques offered the only promising approach to solve this problem. Scientists, national security experts, diplomats, and politicians found, however, that seismological knowledge was not sufficiently developed to detect, identify, and locate small nuclear underground explosions with sufficient accuracy.

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To improve seismic detection capabilities, President Eisenhower initiated a comprehensive research and development program in seismology in 1959, which became known as Project Vela Uniform.[1] Managed by the Department of Defense's (DOD) Advanced Research Projects Agency (ARPA), Vela Uniform supported almost every US seismologist and even a number of foreign scientists during the 1960s.[2] From 1959 to 1961, funding for seismology increased by a factor of 30 and remained at this high level for the better part of the 1960s. From the fiscal year 1960 to 1971, the US Government spent about $250 million for this improvement of seismic detection capabilities (US Congress, 1971: 19).

This enormous amount of money transformed the young, small, and underdeveloped academic field of seismology into a large academic-military-industrial endeavor.[3] Beginning in 1960, academic seismologists were awarded large multi-year contracts for seismological research. Equipped with hundreds of thousands, and sometimes millions, of research dollars, seismology programs around the country accelerated research activities.

An analysis of the origins, mechanisms, and consequences of DOD patronage is at the center of the present paper. What happens to a small academic discipline when it suddenly becomes important for national security and foreign policy? How did the field change as a consequence of the millions of dollars poured into its academic programs? What kind of new knowledge did US seismologists produce under the patronage of the DOD, and to what extent did this new knowledge evolve out of the research questions of the 1950s and the nuclear-test detection mission of the 1960s? And finally, to what extent does this case of seismology confirm or challenge the dominant historiographical view of science in the Cold War?

A detailed analysis of military patronage and seismology is possible, since most aspects of Vela Uniform were never classified.[4] The openness of the program was a consequence of policy decisions at the highest level: President Eisenhower and his Secretary of State Christian Herter regarded Vela as a foreign policy initiative to convince other nations that the USA was serious about finding a solution to the nuclear-test detection problem. Consequently, many Vela Uniform reports were widely distributed to national and international government agencies as well as to academic and industrial seismologists.[5]

Historiographical Context

In the past 20 years historians of science and technology have produced a rich literature on the interaction of science, technology, and the military. The majority of these works have focused on the development of the physical sciences in postwar USA. Authors such as Paul Forman, Stuart Leslie, Daniel Kevles, Robert W. Seidel, Silvan S. Schweber, Bruce Hevly, Peter Galison, and Michael A. Dennis have documented the mobilization of the physical sciences during World War II and the continued close relation between these sciences and the military during the Cold War.[6]

In contrast, the history of the geophysical sciences remains largely unexplored.[7] Recently, however, historians have begun to examine the development of geophysical research programs and disciplines during the Cold War.[8] Seismology, a geophysical discipline, has received only limited attention from historians of science,[9] and few authors focused on seismology in the Cold War.[10]

One unifying theme of these studies is how the Cold War has shaped scientific disciplines,and how in turn, these disciplines shaped the Cold War.[11] The most influential perspective, the so-called 'distortionist' theory, is based on seminal works by Paul Forman (1987) and Stuart Leslie (1993).[12]

In his paper, 'Behind Quantum Electronics', Forman (1987) documented the growth in government funding for physics in the period from 1945 to 1960, which was fueled nearly exclusively by the DOD and the Atomic Energy Commission (AEC). Concerned about the social consequences of a militarization of science, Forman asked what kind of science resulted from this military patronage. He argued that the patrons had little interest in the advancement of knowledge *per se* and pushed instead for mission-oriented applications and techniques, which aimed at addressing national security concerns. He found that the patron's emphasis on technical capabilities and large 'gadgets' was reflected in the physicists' research.[13] Forman (1987: 229) concluded that:

> [t]hough they have maintained the illusion of autonomy with pertinacity, the physicists had lost control of their discipline. They were now far more used *by* than using American society, far more exploited by than exploiting the new forms and terms of their social integration. (Emphasis in original)

Similarly, in his book *The Cold War and American Science*, Leslie (1993) detailed the postwar growth of electrical engineering, aeronautical engineering, physics, and materials science at two of the leading US universities, the Massachusetts Institute of Technology (MIT), and Stanford University. He showed how academic entrepreneurs and empire builders such as Frederic Terman, Stanford's Dean of Engineering, sought to transform their departments and schools into the world's leading research institutions. For these ambitious goals they needed large and sustained federal support and consequently cultivated close ties with their military patrons, who could provide the necessary funding. As in quantum electronics, military contracts fueled the transformation of these disciplines, leading Leslie to argue that the Cold War reoriented US science in character and content towards the needs of the national security state. In Leslie's view, the short-term benefits of rich contracts were outweighed by the long-term cost, the loss of independence.[14]

The following sections of the present paper document the transformation of seismology. The paper raises questions about patronage and knowledge production that are rooted in the tradition of Forman, Leslie, and others, who analyzed the consequences of military patronage for the development of scientific disciplines during the Cold War. I argue, however, that the case of seismology challenges some of the assumptions underlying the distortionist approach.

Structure of the Paper

The paper is divided into five sections and a conclusion: the first section sketches the development of seismology before Vela; the second shows how seismology became important for national security; the third discusses the origins of Vela Uniform; the fourth analyzes how scientists and program managers negotiated research directions for seismology in the 1960s; and finally, the last section shows how the Cold War context shaped seismology as a discipline in the 1960s. In the conclusion I discuss to what extent the case of seismology challenges the 'distortionist' theory.

Seismology before Vela Uniform

Seismology is a young discipline. Scientists developed instruments for the recording of seismic waves only in the late 19th century.[15] These seismographs produced outputs of wiggly lines on smoked paper, the so-called seismograms, which opened the way for a systematic mathematical analysis of seismic waves in the early 20th century. Early US seismology focused on Californian earthquakes, in particular after the catastrophic 1906 San Francisco earthquake, and was motivated by a longtime goal of predicting or even preventing future catastrophes (Rodda & Leviton, 1983).[16] In the 1930s leading seismologists moved beyond the earlier focus on local seismology to questions of global seismology and the study of the earth's deep interior (Goodstein, 1984). During the 1920s and 1930s the first generation of US seismologists, mostly trained in physics, developed a few academic seismology programs, primarily in departments of geology. This first generation included Beno Gutenberg, Charles Richter, and Hugo Benioff at the California Institute of Technology (Caltech); Perry Byerly at the University of California, Berkeley; Maurice Ewing at Columbia University's Lamont Geological Observatory; and Father James B. Macelwane at St Louis University.[17] These six individuals trained students receiving the majority of PhD degrees awarded in seismology until the 1950s, with an average of merely six PhD degrees per year.[18] Overall, the field included less than 50 active researchers in the US: faculty members at seismology programs, their graduate students, and a handful of researchers at the US Coast and Geodetic Survey, the research agencies of the armed forces, and the Jesuit Seismological Association.[19]

World War II disrupted seismological research activities. Some overseas stations were destroyed, and international communication, on which seismologists depended for exchanges of seismic data, was severely disrupted.[20] Unlike other physical sciences, seismology played only a minor role in World War II. Seismologists often abandoned their own research to contribute to projects directly related to the war effort. Beno Gutenberg, for example, the senior seismologist at Caltech and one of the world's leading geophysicists of his time, worked from 1944 to 1946 as a technical adviser for the US Navy to determine whether seismic methods could be used to track hurricanes and typhoons.[21] His Caltech colleague, instrumentation specialist Hugo Benioff, worked on radar and underwater listening devices (Press, 1973: 30).[22] In general, during World War II seismology went into hibernation; it emerged in 1945 with its international contacts shattered and its research stagnant.[23]

After the war, most seismologists continued to be concerned with the causes, effects, and mechanism of earthquakes as well as with the structure of the earth's interior. Others studied the geographical distribution of earthquakes, their size, and the frequency of their occurrence. Some worked to improve seismic instrumentation. Theoretical studies focused on the interpretation of seismograms and the propagation of seismic waves through the earth's interior and along its surface.[24]

Federal support for seismology was very limited, especially if compared with expenditures in physics.[25] In the mid-1950s, federal expenditures for seismology amounted to about $500,000 and remained well below $1 million during the late 1950s (Committee on Seismology, Division of Earth Sciences, 1969: 34–35). In addition to federal support, seismology programs depended on small contributions from sponsors, mainly insurance underwriters, and support through their departments.

A closer look at the Caltech's Seismological Laboratory emphasizes this point. Despite its prominent role in national and international seismological research, Caltech's Seismological Laboratory had to live off very limited funds. In 1955, Caltech's senior seismologist Beno Gutenberg complained that:

> We have tried for many years to get funds for enlargement of our buildings, which are by far too small, but thus far we are unable, and I know that practically all other seismological stations and institutions have to try very hard to keep their budget in line with the increase in expenses but have no possibility of adding new people to their staffs.[26]

In the academic year 1957–58 the Seismological Laboratory received a general budget from Caltech's Division of Geological Sciences of about $180,000. This budget covered the salaries of the four professors, Beno Gutenberg, Charles Richter, Hugo Benioff, and Frank Press; the 19 and a half non-academic positions, including engineers, technicians, machinists, and administrative aids; and finally, one graduate assistant and three summer assistants.[27] Nine sponsors, including a number of insurance companies, supported seismological research at Caltech with a total of less than $30,000.[28]

Similarly, government support remained comparatively small during the 1950s. Gutenberg and Benioff signed a research and development contract with the Army Air Forces in 1948 for an investigation of small vibratory motions of the earth, the so-called microseisms. This contract, which began in 1948 and ended in 1956, brought in about $200,000 over an 8-year period, or less than $24,000 per year, most of which paid for salaries.[29] In sum, Caltech's program, one of the world's foremost in earthquake seismology, received very limited financial support before Vela Uniform.

Even the International Geophysical Year (IGY; 1 July 1957–31 December 1958), arguably the most important international scientific effort in the earth sciences to the present day, did not transform seismology the way the Cold War did only a few years later.[30] During the IGY, seismology played a secondary role and received only about 2% of US IGY funds ($865,090), considerably less than programs in other geophysical sciences such as meteorology, ionospheric physics, or oceanography. The IGY supported US seismology primarily through the installation of new long-period seismographs around the world and the collection of seismic data from regions that were often inaccessible. Despite its considerable size and complexity, the IGY's seismology efforts were small compared with federal support for seismology in the 1960s.[31]

Seismology and Nuclear Explosions before Vela Uniform

Throughout the 1950s, seismology remained largely independent of Cold War concerns. A few important links, however, existed between the discipline and nuclear weapons testing before Vela Uniform. Most importantly, since the 19th century seismologists had used explosions as controlled seismic sources to explore geological structures.[32] With the advent of nuclear explosions in 1945, seismologists utilized seismic waves generated by these large atmospheric explosions to further their investigations of seismic travel times and the structure of the earth's interior.[33] While they had no influence on where and when a nuclear explosion would be tested, they benefited from nuclear explosions as artificial sources of seismic waves. Seismologists, however, realized that much more information could be gained if these large explosions were set off underground, where explosions would generate much larger seismic wave amplitudes compared with bombs exploded in the atmosphere, and in geological structures of interest to them.

Consequently, in 1955 a number of the world's leading seismologists proposed to the Governments of the USA, the UK, and the Soviet Union to set off four nuclear explosions purely for seismological research during the IGY.[34] While this proposal failed, it was the first major effort by seismologists to advocate the use of nuclear explosions for scientific purposes. Subsequently, in the 1960s, a number of nuclear explosions were set off as part of the Vela Uniform program specifically to test seismological theories.[35]

Another link between seismology and nuclear-weapons testing before Vela is the establishment in the late 1940s of the US top-secret nuclear explosion detection system, which relied primarily on the sampling of airborne radioactive particles, but also included seismic stations to detect seismic waves from nuclear explosions. Since this detection system was highly classified, it did not leave traces in the seismological research literature of the 1950s.[36] In short, seismologists had some experience with seismic measurements from nuclear explosions before Vela. In addition, some were involved in a number of defense-related projects before the 1960s, in which their expertise in the detection of seismic waves from explosions was sought.[37]

Research Directions in Seismology before Vela

Before turning to an explanation of how seismology became important for national security, I will summarize the major trends in seismological research before Vela Uniform. Then I will compare the trends of the 1950s with those of the 1960s. A content analysis of seismological research publications in the period from 1945 to 1959 highlights a number of research programs.[38]

Six major research trends stand out. First, Beno Gutenberg and his students at Caltech focused on the analysis of seismic waves, their magnitudes and attenuations over distance, and the understanding of the earth's deep interior. Second, Maurice Ewing and his students at the Lamont Geological Observatory began a concerted effort to utilize the largely ignored surface waves to study crustal structures. Third, seismologists from various institutions continued to work on the nature and causes of the continuous tremors of the earth, known as microseisms. Fourth, in the mid-1950s a number of seismologists began to analyze the mechanism of faulting, based on a method developed by Perry Byerly. Fifth, a few seismologists focused on the mathematical analysis of seismic wave propagation in complex geological structures. Finally, some seismologists continued to improve seismic instrumentation. In sum, academic seismologists aimed at a better understanding of the nature of earthquakes, their associated wave phenomena, and the internal structure of the earth.[39]

One research area, which had received considerable attention throughout the 1920s and 1930s, was conspicuously absent during the 1950s. Under the influence of leading seismologists such as Father Macelwane and Charles Richter, earthquake prediction was thoroughly discredited as a research topic. As Richter (1958: 385) put it in his influential textbook:

> At present there is no possibility of earthquake prediction in the popular sense; that is, no one can justifiably say that an earthquake of consequence will affect a named locality on a specified future date. It is uncertain whether any such prediction will be possible in the foreseeable future . . . .

Seismology Meets Arms Control

A number of political developments led to the involvement of seismologists in international arms-control negotiations. During the 1950s the US and the Soviet Union came to focus on a nuclear test-ban treaty as a possible first step towards more comprehensive arms-control measures.[40] Both sides disagreed, however, about the order of technical and political agreements. The Soviets consistently argued that the first step had to be an agreement among *politicians* to ban all nuclear tests, with no control system in place. In contrast, the US insisted that no political talks would be held, until *scientists* would agree about the details of a monitoring system. In an extended correspondence in early 1958, President Eisenhower suggested repeatedly to Soviet Premier Khrushchev that scientists from East and West should meet to solve some of the technical problems of nuclear-test detection before any diplomatic negotiations would begin. Although Khrushchev was not convinced that scientists could solve political problems, he finally agreed in April 1958 to send his experts (US Department of State, 1960, vol. 2: 1038).

Did the US have the technical capabilities to monitor a test-ban agreement? In other words, would the US be able to detect and identify all nuclear tests conducted anywhere on the globe, in all different environments, under all possible circumstances? Or were there loopholes that the Soviets could exploit for secret tests while the US was bound by a test-ban treaty? How efficient would a detection system have to be to deter the Soviets from risking a secret test? Would the US need a foolproof system detecting every single secret explosion, or was it sufficient to detect explosions with about a 50 % probability, just enough to deter the Soviets from trying? Was such a system technically feasible, and what would it look like?

On 1 July 1958, experts from East and West met in Geneva, Switzerland, for their first round of technical talks.[41] After 30 official and numerous informal meetings, both delegations agreed on 21 August 1958 to a final document, concluding that the control of nuclear testing was technically possible within certain limitations. The experts proposed a control system of 170 detection stations worldwide, augmented by ten stations located on ships to cover large areas of the world's oceans. They specified various means for the detection of nuclear explosions in the atmosphere, the oceans, on the earth's surface, and underground. As it turned out, detecting underground explosions became the most contentious issue (Jacobson & Stein, 1966: especially 34–81).

At Geneva, seismologists from East and West debated the strengths and limitations of seismic-detection methods and differed in their evaluations along Cold War lines.[42] While the Soviets emphasized the strengths of seismology, the US pointed towards the many uncertainties that remained. In this respect, the West's caution and the East's optimism coincided with their Government's attitudes towards a nuclear-monitoring system. Mistrusting the Soviets, the US Government insisted on reliable detection and identification of nuclear weapons tests, whereas the Soviets pushed for a test-ban agreement under all circumstances.[43]

From the US point of view, many areas of seismological research required further study to increase confidence in the capabilities of a seismic-monitoring system. Most important were further developments in the discrimination of earthquakes and explosions. Consequently, more knowledge was required about the physical processes close to earthquakes and explosions to gain a better understanding of how these different seismic sources generated seismic waves. Furthermore, the propagation of seismic waves through various geological structures, long at the center of seismological research, was insufficiently understood at that time to determine the size of an explosion from the recorded amplitudes. In addition, detecting and identifying an explosion among thousands of earthquakes of similar magnitude proved as difficult as finding a needle in a haystack.

The Origins of Vela Uniform

Following the agreement among scientists, diplomatic negotiations for a test ban began in Geneva on 31 October 1958. However, US underground tests conducted after the Geneva conference provided new seismic data. Seismic experts from government agencies and academic institutions concluded that the new seismic data challenged the technical agreement reached in Geneva only months earlier.[44] Suddenly a seismic-detection system appeared technically infeasible, a view that eroded domestic support for a comprehensive test ban. This new development severely displeased President Eisenhower, irritated Soviet diplomats who suspected sinister motives behind the new data and their interpretation, and alarmed arms-control supporters among US scientists and politicians (Jacobson & Stein, 1966: 147–66).[45]

The new data, however, did not pose the only technical challenge. In 1958, at the suggestion of nuclear physicist and prominent test-ban opponent Edward Teller, scientists at the RAND Corporation (Santa Monica, CA, US) developed a new theory about how underground explosions could be concealed. This 'decoupling theory' suggested that the amplitude of seismic signals from an underground nuclear explosion could be reduced by as much as a factor of 300, if the nuclear device would be exploded in a large underground cavity.[46] The implications for a test-ban treaty were obvious: an explosion 20 times the yield of the Hiroshima bomb (15 kilotons [15×10^6 kg]) would not, according to this theory, be detected by the monitoring system.

Together, the interpretation of the new seismic data and the new theoretical arguments for a possible concealment of nuclear explosions provided test-ban opponents with considerable ammunition. Part of the Eisenhower administration's response to these challenges was to improve seismic-detection capabilities through a large federally funded program. For Eisenhower, such a program was a 'matter of high priority'.[47] Secretary of State Christian A. Herter considered the technical work 'to be of the utmost importance from the standpoint of US political objectives'. He felt that 'a sizeable and vigorous program of research and development . . . [is] essential to the credibility before world opinion of the present US position on a nuclear test cessation'.[48] Both the President and his Secretary of State provided the political support for the successful initiation of a large government program on seismic detection.

A quickly organized panel of seismological experts, chaired by the experienced science administrator Lloyd V. Berkner, developed a grand research plan for seismology. The panel's report, which became known as the Berkner Report, shaped US seismology in the 1960s and beyond, and became arguably one of the most important documents in the history of seismology.[49] Its recommendations accelerated research and led to the development of new techniques, instruments, and advanced theories.

The Berkner Report's summary was written by leading seismologists Frank Press, Jack Oliver, and Carl Romney.[50] The three seismologists laid out a master plan for future research in seismology and suggested that 'major advances in [seismology] could be realized only if the level of research were significantly increased' (US Congress, 1960: 645). The suggested new level of research included studies relevant to the detection problem. To identify a seismic source as an earthquake or an explosion, more knowledge about the mechanism of earthquakes was required. A more detailed picture of the earth's crust and upper mantle was mandatory to understand the effects the medium had on the propagation on different seismic signals. Detection and identification of small seismic signals required significant improvement in the signal-to-noise ratio and a better understanding of the nature of the complex microseismic background. What were the sources and characteristics of this background, which 'swallowed' the smaller seismic signals? Was it possible to filter out or otherwise reduce this noise to enhance the identification of signals?

On 23 April 1959, Eisenhower's science adviser James R. Killian Jr met with Deputy Secretary of Defense Donald Quarles, and AEC Chairman John McCone. The three policy-makers agreed that the DOD should be the central governmental agency to implement the Berkner Panel recommendations. Guided by the Berkner Panel report, they proposed a three-step approach to increase seismic-detection capabilities: first, a centrally funded 'research program in seismology directed toward the fundamental problems involved in the detection and identification of underground tests'; second, a systems development program for new and improved instrumentation; and third, an experimental explosion program, including nuclear shots, to test the decoupling theory.[51] The DOD would implement research and systems development, while the AEC would be responsible for the test explosions, in close cooperation with the DOD and consulting seismologists. This agreement between top-level officials from the State Department, the DOD, and the AEC marked the beginning of Vela Uniform.

Negotiating Seismological Research

During the mid-1960s essentially every US seismologist received support through the Vela program. As one observer put it in 1965, 'Representatives of ARPA have made the statement that they bought the entire discipline of seismology when the issue of detection of underground nuclear tests came to their attention and it is hard to disagree'.[52] Vela's mission, as defined by ARPA, was to 'obtain at [the] earliest practicable date a reliable system for detecting underground nuclear explosions, both tamped and decoupled'.[53] Consequently, ARPA funded seismic research, system development, and a series of chemical and nuclear test explosions to improve seismic-detection capabilities.

What kinds of strings were attached to the large Vela research grants and contracts awarded to academic seismologists, and how did these strings change over time? Did seismologists find themselves working on research questions closely aligned to Vela's mission and far removed from their own former research interests?

We find a spectrum of scientists' responses to Vela funding: at one extreme, a scientist or a group of researchers would halt their projects and begin working instead on research closely related to the detection problem; at the other extreme, scientists would use Vela money for their own projects, even if these projects had little to do with Vela's mission. Not surprisingly, we find examples of both extremes and various approaches between them.[54] In addition, in many cases it was impossible to distinguish between seismic-detection studies and traditional seismic research, since the questions were often the same.

The major challenge is to determine the general trend of seismologists' responses to Vela patronage. To what extent did seismologists trade their scientific autonomy for the enormous funds Vela had to offer? Did Vela emphasize research areas that had not been major areas during the 1950s? In short, did seismologists lose control over their field by permitting the DOD to define the direction of academic research in seismology during the 1960s? How did DOD officials and scientists negotiate which research projects should receive Vela support?

We have to take at least three different groups into account if we want to understand how decisions about seismological research were made in the 1960s: DOD officials, university scientists, and Vela Uniform program managers, who mediated between the first two groups. The DOD's seismic-detection mission did not directly translate into action in university departments. Program managers at ARPA and its contracting agents served as mediators between the DOD and the academic scientists. These managers had considerable leeway in translating their agency's mission into contracts and grants for university scientists. As Arthur L. Norberg et al. (1996: 15) have argued in the case of developing new computing techniques, ARPA

> chose managers with foresight, experience, and ability, and gave them substantial freedom to design and manage their programs. Often the difference between programmatic success and failure came down to the person managing a program, and the way in which he or she managed it. The fact that IPTO [ARPA's Information Processing Techniques Office] recruited capable and technically able members of the research community and allowed these people, within the context of D[Defense]ARPA's mission, an unusual amount of freedom to promote research as *they* saw fit, seek advice as they felt the need, and manage the office as *they* thought

> the program required, was the deciding element in IPTO's success. (Emphasis in original.)

Here I make a similar argument for the importance of individual program managers in shaping seismological research funded by ARPA. I argue that program managers are crucial to our understanding of who determined the direction of research in the Cold War, because the direction of research was negotiated between program managers and scientists in the broad framework provided by policy-makers.

Vela Uniform as an Advanced Research Projects Agency Project

A closer look at Vela's organizational structure and institutional identity helps to understand Vela's opportunities and limitations in shaping seismology in the 1960s. On 2 September 1959, Secretary of Defense Neil McElroy formally assigned Project Vela Uniform to the DOD's ARPA (Richard J. Barber Associates, 1975: IV-28). The Eisenhower administration created ARPA in February 1958 as part of the Government's response to Sputnik (McDougall, 1985; Divine, 1993). When Vela was assigned to ARPA in the fall of 1959, the young agency was in turmoil, with no clear mission, demoted within the administration, and beleaguered by enough enemies within the Armed Services to make its very survival doubtful (Richard J. Barber Associates, 1975: chapter 3).

Under a new ARPA director, General A.W. Betts, the agency's situation stabilized in 1960. When Betts left in January 1961, the first scientist, Jack P. Ruina, became the Director of ARPA. Ruina, who had been Professor of Electrical Engineering at Brown University and the University of Illinois, came to ARPA from the Office of the Director of Defense Research and Engineering.[55] Ruina's style differed considerably from his predecessors. He pushed for scientific excellence and promoted a long-term, basic-research approach to military research and development projects. Ruina's successors as ARPA directors during the 1960s, Robert L. Sproull, Charles M. Herzfeld, Peter Franken, and Eberhardt Rechtin, were all either physicists or electrical engineers (Norberg et al., 1996: 8). While political constraints, in particular following the US increased involvement in the Vietnam War, led to limited ARPA budgets during the late 1960s, Vela survived and continued throughout the decade with few changes in its original tasks.[56]

Throughout the 1960s, ARPA managed a variety of military research and development projects, ranging from missile defense to psychological warfare, from materials science to seismological research (Richard J. Barber Associates, 1975: chapters 4–8). By far the largest ARPA project throughout the 1960s was research on ballistic missile defense, which consumed about one-half of the agency's annual budget of about $200 million. Vela remained the second largest ARPA project throughout the better part of the 1960s, supported by about one-quarter of ARPA's annual budget (Richard J. Barber Associates, 1975: chapters 4–8).[57] After the signing of the Limited Test Ban Treaty in August 1963, interest in a comprehensive test-ban treaty waned, and with it the focus on the detection of underground nuclear explosions. However, Vela continued, although under budgetary pressures following increased expenditures for the US involvement in Vietnam.

The US president, the Secretary of Defense, and military leaders participated in the debates about ARPA's mission, projects, and budgets, and they shaped the political climate in which Vela could flourish. They had, however, little, if any, influence on the negotiation of contracts and grants or the day-to-day monitoring of actual projects. This was left to lower levels of the hierarchy, in particular the ARPA director and his subordinates as well as to ARPA's contracting agents and university scientists.[58]

Vela Uniform Program Management

Following ARPA's general management philosophy, individual branch chiefs had considerable freedom designing a research project, once it had become their responsibility. Often neither the ARPA director nor any higher official would be involved in defining the details of the program, as long as no major problems would occur. Consequently, it fell to the chief of ARPA's Vela Uniform branch, Charles C. Bates, to design its research program.[59] Bates, trained in geophysics with a PhD in oceanography,[60] did not share the anti-test-ban sentiments of many DOD and AEC officials. Rather, he wanted Vela Uniform to be a resounding technical success, that is, 'a good detection system that could catch cheaters, using the BEST technology possible'.[61] He wanted to see improvements of seismic-detection capabilities by an order of magnitude in a period of a few years. Consequently, he tried to enlist every seismologist in the country as well as some scientists in Europe to contribute to the effort.[62]

In the period from 1960 to 1963 the Vela Uniform Program received funds amounting to $110.7 million. About 30% of this sum ($33.1 million) was assigned for 'basic and applied research' (Bates et al., 1982: table 6.2, 175). How did Bates distribute this wealth, and who were his advisors? In deciding which problem areas should be attacked, Bates relied on the Berkner Report and its subsequent modifications by two panels assembled by the Director of Defense Research and Engineering, Herbert York.[63] Both panels included prominent seismologists. Bates took care that every single suggestion of the Berkner Report became a research project under Vela.[64]

Equipped with virtually unlimited funds to develop a workable detection system as soon as possible, Bates funded essentially everyone with a good idea.[65] He was convinced that the fastest way to a workable detection system was a 'shot-gun' approach of supporting every peer-reviewed project related to the detection problem. By contrast, Carl Romney, the Air Force's leading seismic expert, supported research more closely related to his agency's mission. As one scientist put it:

> Carl's organization was judged to be the most appropriate source of such funds [for the scientist's project], but I understand from Carl that he would have difficulty justifying such a project unless he could receive more assurance than I was able to muster that this project would definitively result in improved methods of detection and identification.[66]

Bates coordinated the efforts of a small staff of five program managers.[67] His program managers worked closely with government agencies that acted as ARPA's agents in dealing with subcontractors in industry and academia.[68] Bates and his managers fulfilled a variety of policy functions. They decided which scientific and technical problems required attention, and then encouraged, evaluated, and funded corresponding research proposals. During the period of the actual contract they evaluated progress reports and periodically reviewed the contractor's performance.[69]

The Air Force and Research in Seismology

Two of ARPA's government agencies stand out in their influence on Vela's university-based research program: the Air Force Cambridge Research Laboratories (AFCRL) and the Air Force Office of Scientific Research (AFOSR).[70] During the 1960s, almost every US seismologist received Vela Uniform money through these two agencies.[71] How did these agencies decide who should get funded? In matters related to Vela Uniform research proposals, AFCRL managers relied on the judgment of AFCRL senior scientist, Harvard-trained seismologist Norman Haskell.[72] If Haskell regarded a proposal as unsatisfactory, it had no chance of funding.[73] A number of academic seismologists chose to submit their proposals directly to Haskell, because of his impeccable credentials as a highly respected theoretical seismologist. With ties to MIT and academic seismologists in general, Haskell was part of the seismological profession. He chose to support broadly conceived research projects in seismology, which were often independent of the detection problem. According to Frank Press, Haskell 'used his influence in a quiet way to see that research funds went to the most productive groups, often stretching the charter of his agency beyond its relevant mission'.[74] Major AFCRL university funds went, for example, to Lamont for 'Seismic Waves of Long and Intermediate Periods' with a total of $950,000 over a period of 4 years. Other universities received contracts of about $500,000 each for a 4–5-year period.[75] Written by seismologists, these proposals allowed them to continue existing research projects with the financial security of a long-term contract.

The second Air Force agency with significant involvement in Vela Uniform, the AFOSR, was established in 1955. By 1967, AFOSR's geophysics division was run by only two managers: the chief of the division, William J. Best, and project scientist Major Durward D. Young Jr.[76] Best became one of the key contract monitors of Vela Uniform and his role in the selection and funding of academic research will be discussed later. First, I look at the general funding policy of AFOSR as an Air Force organization.

Not surprisingly, AFOSR funded university research preferably in areas of direct relevance to Air Force needs. As William J. Price, AFOSR's Executive Director, unmistakably put it in July 1967:

> AFOSR conducts a grant and contract program of phenomena-oriented research in those areas of science related most directly to needs of the future operational Air Force. . . . Research is selected for support from unsolicited proposals on the basis of scientific quality and relevance to Air Force interests. (William J. Price, quoted in Air Force Office of Scientific Research, 1967: iii)

In Price's view, the major objective of AFOSR managers was to connect academic, phenomena-oriented research and military applications and to shorten the time between scientific discovery and military hardware:

> [S]taff members of AFOSR have the mission, experience and techniques required for identifying DOD problem areas and translating them into scientific research opportunities or in reverse, in translating scientific knowledge and understanding into results for DOD users. (ibid.: 4)

Price argued that funding selected projects in a research area of interest for the Air Force would allow the AFOSR, in his telling terminology, to 'colonize' this area. Such a 'colonized' area would attract other researchers, who then would provide scientific and technical results of interest for the Air Force. Besides identifying these critical areas, AFOSR grants and contracts would be used to establish connections between academic researchers and the Air Force. Ready and continuous access to the best scientific and technical workforce in academia was one of AFOSR's central goals, since AFOSR wanted to '[provide] the DOD with a quick reaction, program management and procurement organization for accomplishing special activities requiring the participation of the scientific community' (ibid.: 5). Vela Uniform was exactly such an emergency case, which required quick reaction and immediate access to academic scientists. Since academic scientists could hardly be ordered to work for the Air Force, AFOSR managers, who wanted to attract top-level scientists, had to offer as much money as possible with as few strings attached as politically defensible to Congress.

AFOSR's Geophysics Division chief, William Best, interpreted AFOSR's funding philosophy broadly. When seismologists honored him in 1992 with the American Geophysical Union Flinn Award, they praised him as one who:

> never let these [mission] goals and objectives get in the way of daring, innovative, or at times provocative research ideas. . . . [H]e not only shielded researchers – especially the younger ones – from the rapid fluctuations of national priorities and from the specific agency interests of the moment, but he also always seemed to succeed in finding a niche for good research ideas within these priorities . . . . (Anonymous, 1994)

Best funded academic research lavishly. A 1965 AFOSR report listed 28 contracts and 18 grants, for a total of about $14 million. Nearly 80% of these funds, or $11 million, supported research at US universities, most of which went to Caltech, Scripps, Michigan, Berkeley, and Lamont (Air Force Office of Scientific Research, 1965).

Best was responsible for final decisions regarding AFOSR grants and contracts for university-based research in seismology. In his evaluation of applications, however, he relied on recommendations by the AFOSR Advisory Committee for Geophysics, a small panel of academic and industrial seismologists that included Caltech's Hugo Benioff and Lamont's Jack Oliver.[77] This group advised Best also on research areas in seismology that required special attention to accelerate the Vela Uniform program. In this respect, academic seismologists were directly involved in the funding process that shaped their own projects in the 1960s.

Vela Uniform and the Negotiation of Research Directions

So far, the following picture emerged: at the Geneva Conference of Experts seismologists determined the technical problem areas, which required attention to improve seismic-detection capabilities. Leading academic seismologists authored the Berkner Report, which became the basis for the DOD's Program Vela Uniform. Academic seismologists submitted research proposals, which were then incorporated into the Vela program without significant changes. Consequently, Vela's research component (as distinguished from system development and research explosions) was predominantly defined by academic seismologists, who received substantial funds to continue their academic interests and at the same time to improve seismic-detection capabilities.

It is instructive to look in some detail at a typical contract and its resulting publications to determine to what extent Vela support resulted in an emphasis on detection research. Lamont received nearly $1 million for research on 'Seismic Waves of Long and Intermediate Periods'. The 4-year contract, monitored by the AFCRL for ARPA, specified the tasks to be completed.[78] These tasks included: compilation of phase and group-velocity data for surface waves at various periods; the development of high-speed computational techniques; the development of improved long-period seismographs; and the continuation of Lamont's IGY network of long-period instruments. In addition, the contract required the development of improved data-analysis techniques, such as 'tape recording systems, filtering, digital analysis and analogue computers for the purpose of improving detection techniques and methods of analysis of seismic records'. Finally, Lamont was to investigate crustal structures, establish an additional long-period seismograph station 'for basic studies of body and surface waves', and to analyze microseismic data. Although the contract pointed to the detection mission, it allowed a very broad range of research in seismology, covering Lamont's traditional emphasis on long-period surface waves and its IGY network. From 1961 to 1963 alone, Lamont seismologists published 13 reports and 32 articles based on research at least partially supported by this single large contract.[79] In fact, these 32 articles constitute nearly the entire body of publications by Lamont's earthquake seismologists during that period.[80] Most of the papers focused on various aspects of the dispersion of long-period surface waves, thereby continuing one of Lamont's traditional research foci of the 1950s. A number of papers, however, focused on investigations of the free oscillations of the earth, a research area that was neither mentioned in the contract nor closely related to the detection mission. This example suggests that Vela contracts allowed seismologists to continue their own research projects and to investigate research problems that were largely independent of Vela's mission. The same conclusion holds for other Vela contracts.[81]

The Consequences of Vela Uniform Patronage

This section analyzes to what extent Vela Uniform transformed seismology. First, I show how Vela shaped seismology by introducing large seismic measurement systems and data analysis approaches. Then I discuss the growth of the discipline in terms of scientists working in the field, papers published, and other easily quantifiable trends. Finally, I look at the research trends of the 1960s.

Vela Uniform and Seismic Instrumentation

Under Vela's guidance, contractors and university programs designed large seismic instrumentation systems that aimed at detecting small seismic signals. Three major Vela instrumentation efforts stand out: a global research network, which became known as the World-Wide Standard Seismograph Network (WWSSN); seismographs for the ocean floor; and large seismic arrays with centralized computer facilities. These systems highlighted the large scale, complexity, and cost of Vela instrumentation projects.[82]

The development, installation, and operation of the WWSSN, the largest seismic instrument built to that date, exemplifies the scale of Vela's seismic systems.[83] During the early 1960s, US Coast and Geodetic Survey employees installed the network's 120 standardized seismic stations in over 60 countries. Although about 500 seismic stations existed around the world before Vela, they constituted, as two seismologists later put it,

> [a] hodgepodge of instruments operating in different frequency ranges, measuring different components of ground motion, recording in a variety of forms, lacking calibration (or at least standard calibration), and [operating] sometimes without accurate timekeeping. (Oliver & Murphy, 1971: 255)

In contrast, the WWSSN allowed standardized, calibrated measurements, nearly global coverage, and a broad, basic data collection, which was available for non-classified scientific research.

While the network also recorded nuclear weapons tests, it was not particularly well suited for detection and identification of secret underground explosions. Many of the sites chosen for the WWSSN stations, for example, turned out to be very 'noisy', that is, considerable seismic background noise from microseisms drowned out small seismic signals (Powell & Fries, 1965).[84] Furthermore, seismograms were recorded on photographic film, which did not allow any timely digital signal-processing techniques so important in other Vela instrumentation systems. Finally, the WWSSN data collection was too slow for detection purposes: it often took months before all stations had submitted their seismograms for a particular day (Farrell, 1985: 487). While seismic networks for global earthquake recording had existed decades before the WWSSN, the new network marked a significant change in data quality and availability and in sheer costs.[85] Overall costs for deployment from 1960 to 1967 amounted to about $9.5 million, with annual operating costs of about $1 million (1960s dollars) (Peterson, 1992: 19). This shows that the network's operating costs alone exceeded annual federal support for seismology before Vela.

The network's significance for seismological research, not only in the US, can hardly be overestimated. The WWSSN recorded thousands of earthquakes each year, providing a wealth of high-quality data for a whole generation of scientists. Arguably the most significant scientific result based on WWSSN data was the work by Lamont seismologists Bryan Isacks, Jack Oliver, and Lynn Sykes, who utilized these data in support of the emerging plate-tectonics concept.[86] In addition, for many countries the WWSSN equipment offered the first opportunity to begin a seismological research program (Peterson, 1992: 17). Already in early 1962, Frank Press wrote to Vela Uniform manager Charles Bates that:

> the worldwide research net might well be, in the long run, the major contribution of VELA. So far as seismology is concerned, its effect will probably even be greater than the [IGY]. It represents a magnificent gesture on the part of the United States. (Press, 1962)

Although its instrumentation quickly became outdated, it remained the best available network for basic seismological data throughout the 1960s and much of the 1970s.[87]

While academic seismologists in the 1950s were primarily interested in seismic signals from sizable earthquakes, Vela aimed at detecting extremely small signals against a noisy background of naturally-occurring earth disturbances. Two instrumentation efforts to improve the signal-to-noise ratio stand out. First, the development of seismographs for the ocean bottom was motivated by the hope that the ocean floor would be a less noisy environment than the earth's surface. With lower background noise, the argument went, small seismic signals from nuclear explosions would be detectable. While the various attempts to install seismographs on the ocean floor were only a limited success, they provided new data on microseisms.[88]

The second approach focused on the construction of large seismic arrays, which included between 10 and 100 seismographs distributed in various geometrical patterns in large areas, ranging from a few to thousands of square miles. Arrays of sensors had been used successfully for signal enhancements in fields such as radar, sonar, and radio astronomy (Husebye & Ruud, 1989: 125). Furthermore, oil-prospecting crews had used seismic arrays effectively since the 1920s. Arrays offered the advantage that a simple summation of the outputs of all of the individual sensors added the signal amplitudes and canceled the uncorrelated noise, thereby increasing the signal-to-noise ratio compared with a single instrument.[89]

In contrast to the WWSSN, Vela designed seismic arrays primarily for the detection, identification, and location of secret underground nuclear explosions. Construction of the largest and most complex Vela array, the Large Aperture Seismic Array (LASA), began in 1964.[90] The size and complexity of LASA posed enormous technological challenges beyond the means of an academic program or even the world's seismological community: the 525 seismometers, grouped into 21 sub-arrays of 25 sensors each, spread over an area of 10,000 square miles (26×10^9 m^2) in a sparsely populated area near Billings, MT. The two major agencies responsible for LASA, the Air Force Technical Applications Center and MIT's Lincoln Laboratory, introduced sophisticated data-transmission equipment and general-purpose computers for signal processing and display. Only 5 years earlier Hugo Benioff and his colleagues at Caltech had evaluated individual seismic signals on photographic film or ink paper, whereas now digitized signals stored on magnetic tape allowed complex data-filtering processes on fast computers. The new data center resembled more a military command and control center than a seismological laboratory of the 1950s.[91]

In sum, Vela's legacy in seismic instrumentation is the large seismic system. Vela's multimillion-dollar systems could not have been developed, installed, or operated by academic seismologists alone. Only the industrial capacity of companies such as Geotech and Texas Instruments and the influx of technical expertise from other disciplines such as electrical engineering, mathematics, and physics made these systems possible.

Vela Uniform and the Growth of Seismology

How did Vela shape academic research programs? Support for academic research increased to about $7 million per year in 1961 and began to decrease in 1964, when Vela's mission began to shift towards more applied aspects, following congressional pressures.[92] By 1970, the heyday of ARPA's support for academic seismology was over, and seismology programs found it difficult to generate sufficient support from other federal agencies. Subsequently, the National Science Foundation became a major funding source for academic seismologists.[93]

Not surprisingly, Vela's support led to a dramatic expansion of seismology. Academic departments were able to admit and support more graduate students than ever before. At both Lamont and Caltech, for example, about ten graduate students contributed to the research effort during the early 1960s. Many of these students became leaders in the profession and continue to shape seismology today. In general, PhD degree production in seismology at US universities doubled, from an average of six per year during the 1950s to an average of 13 per year during the 1960s, reaching a peak of 39 in 1972.[94] This small number of PhD degrees did not suffice to fill Vela's manpower demands. Consequently, researchers from fields such as mathematics, physics, oceanography, geology, and engineering, attracted by Vela funds and at times by the political significance of detection research, moved towards seismology. Overall, the number of active researchers in seismology increased by about a factor of five since the late 1950s (Committee on Seismology, Division of Earth Sciences, 1969, Part I: 31). Scientific output increased accordingly. In 1970 the *Bulletin of the Seismological Society of America* contained more than three times as many pages as in 1960. Most of the *Bulletin of the Seismological Society of America*'s papers by US researchers acknowledged at least partial support through Vela. In general, almost all US seismologists received funding through Vela during the early and mid-1960s.

Vela Uniform and Research Trends in Seismology in the 1960s

The growth of the profession and its extended scientific production is easy to document. It is much more difficult to evaluate to what extent the DOD's patronage shaped not only the quantity, but also the research direction and even the content of the scientific knowledge produced during this time. I argue that research trends of the 1960s did not show a significant bent towards detection problems, despite massive DOD patronage. Rather, major new trends such as earthquake prediction, free oscillations of the earth, and the reinterpretation of earthquake occurrences in the light of the new plate tectonics originated essentially outside of Vela's mission.[95] However, Vela shaped these emerging research areas with dramatically improved instrumentation and the subsequent availability of better and more data.

The reasons for Vela's limited influence on changing research directions in seismology are manifold. Most importantly, Vela's research emphasis paralleled traditional seismological research questions. Often, seismic detection required improved knowledge about seismic propagation phenomena and the structure of the earth, areas of central interest to seismologists at least since the late 19th century. Furthermore, Air Force program managers and scientific advisory boards regarded the modernization of seismology and the increase of the number of seismologists as the prime responsibility of Vela Uniform. Pressures to produce results more narrowly focused on the detection problem appeared in the late 1960s, but at that time the National Science Foundation offered seismologists other funding opportunities for projects they wanted to pursue.

In the middle and late 1960s many seismologists turned away from the detection problem and focused their attention on other large projects, including international collaborations such as the Upper Mantle Project,[96] earthquake prediction,[97] and questions of seismicity and deep earthquakes related to debates about sea-floor spreading and continental drift.[98] In addition, in the wake of the Apollo program, some leading seismologists focused on the development of seismic instrumentation for the lunar environment.[99] Most importantly, some of the largest earthquakes ever recorded occurred during the 1960s and shaped seismological research directions quite independently of Vela Uniform. The Chilean earthquake of 1960, for example, accelerated research on the earth's free oscillations,[100] and the devastating 1964 Alaskan earthquake led to a political climate that called for research on the possibilities of earthquake prediction, mitigation, and even control (Geschwind, 2001: 140–64). While both research areas received funding through Vela Uniform, they originated and developed largely outside Vela's detection mission.

Program builders, concerned about the maintenance of their considerably enlarged programs and burned by Vela's sudden withdrawal of support for the WWSSN, sought new avenues of federal support. With the decline of Vela funding in the mid-1960s, earthquake prediction became a new rationale for seismologists to seek continuous federal support. Berkeley's Perry Byerly, one of the discipline's early program builders, wrote in 1970 that:

> Too much emphasis is now being placed, in my opinion, on predicting when the big earthquake will occur. We have large staffs of seismologists, mainly from physics, who were hired in the heyday of government spending on seismology as a tool for detecting nuclear explosions. That heyday is passed, and heads of these large groups are desperate for funds. It is they who hold out veiled hopes of predicting the time, as if it would prevent damage. They invariably state, 'No one can predict when the next great earthquake will occur. We need more money for research'. This is a *non sequitur*, in my opinion.[101]

Program builders moved on to the next patron to continue their own research interests.

Conclusion

Historians of science and technology disagree about the general influence of patronage, or, in this case, of military funding, on the choice of research topics, scientific practice, and the very results of scientific inquiry. Who determined the research direction of seismology in the 1960s? Were scientists used, or even corrupted, by DOD funding? I suggested that the direction of research in seismology in the 1960s grew out of a negotiation between scientists and decision-makers in ARPA and the Air Force research agencies. As mediators between the academic scientists and the Air Force agencies, program managers were in a unique position to direct research. They had considerable leeway in their decision-making. At one extreme a program manager would rely on the advice of seismologists in the evaluation of research proposals. At the other extreme he would support primarily mission-oriented research.

I have argued that seismologists did not lose control of their discipline, despite massive DOD interests. Leading university seismologists participated in every advisory panel concerned with funding decisions for seismological research. They made certain that seismological research was supported broadly and not limited to narrowly defined agency goals. Individuals such as Frank Press understood that seismologists could contribute to the improvement of detection capabilities and at the same time foster the growth of their own discipline. Seismologists, who felt that seismology was underfunded and underdeveloped, realized their chance to modernize their science.

Without doubt, Vela Uniform transformed US seismology during the 1960s in terms of increased funding, numbers of graduate students, scientific output, and other quantifiable factors. Vela Uniform shaped seismology through the installation of the standardized network, the introduction of sophisticated data-acquisition and analysis methods, and the support of a generation of graduate students. However, I emphasized that the major research topics of the 1960s were continuations and extensions of the research areas of the 1950s, despite Vela's massive funding and dominant patronage of seismology during the 1960s.

Seismology and the Distortionist Perspective

Forman and Leslie argued that the Cold War reorientation of the physical sciences distorted research towards military applications and away from traditional pursuits of fundamental knowledge. They were concerned that scientists lost their scholarly independence by accepting large federal grants and contracts that came with sometimes subtle, sometimes obvious strings attached. The evidence provided by both authors is compelling. However, their conclusion depended on the assumption that scientific developments follow a certain inner logic, a certain trajectory, which was then distorted by the patron's interests. In other words, their critique is based on assumptions about how a discipline would have developed without a military patron.

This speculative framework is problematic. Leslie acknowledged this implicitly on the final page of his book, stating that:

> [N]o one now can go back to the beginning of the Cold War and follow those paths not taken. No one can assert with any confidence exactly where a science and engineering driven by other assumptions and priorities would have taken us.[102]

The historiographical danger lies here in postulating a 'natural path' of science. Would scientists have chosen other research questions in a world without DOD patronage? This question is intriguing, although counterfactual and therefore of limited value for a historical analysis. I argued that the most fruitful approach to analyze the interaction of patron and knowledge production in the case of seismology is to compare research trends of the 1950s with those developed in the 1960s under DOD patronage. I argued that these trends did not change substantially as a consequence of the nuclear test detection mission. Rather, the new trends were only loosely, if at all, coupled to the DOD's mission.

While few will doubt the interpenetration of academia, industry, and military agencies in Cold War USA, some scholars have taken issue with Forman and Leslie's 'distortionist' theory and the underlying distinction between the realm of pure science, developed from within the discipline, and the realm of military applications pressed onto the discipline by the patron. Daniel J. Kevles, for example, has pointed out that 'it would seem arbitrary to say that one type of investigation is truly basic physics, while the other is not' (Kevles, 1989: 24).[103] Others have emphasized that it is not self-evident that the military's patronage of scientific disciplines corrupted or otherwise damaged these fields.[104] Furthermore, Seidel (1994) has argued that scientists were often adept in using the patron's money for continuing their own projects.

An important shortcoming of both Forman's and Leslie's analysis is that they treated military agencies as homogeneous entities. I argued that a closer look at the role of mission-agency program managers and their scientific advisory groups is warranted to understand who shaped research directions of scientific disciplines in the Cold War.[105] In my narrative I emphasized the role of these program managers, who mediated between mission agencies and academic scientists.

Ultimately, Forman and Leslie's argument expresses a political or normative position: science *should not* depend on military patronage, because this patronage is corrosive to the values of science. While I sympathize with their *political* view, I find its inherent bias (assuming that military patronage is necessarily bad for science) too limiting for *historical* analysis.

I argued that the case of seismology is an extreme example of military patronage and is therefore well suited to revisit some of the assumptions of the distortionist theory. According to this theory we would expect that massive military patronage would lead to a significant distortion of seismology's research direction. While the patronage led to a significant acceleration of research activities and substantial growth in scientific output, we do not find that major shifts in seismological research directions can be attributed to the interests of the patron.

Critics of my argument, however, might point out that seismology is not a good example to challenge aspects of the distortionist perspective.[106] Some have suggested that seismology did only become a discipline because of the massive military patronage under Vela Uniform. As Michael Dennis (2003) puts it in his 'Postscript' for the present special issue of *Social Studies of Science*. Michael Dennis (2003), for example, argued in his 'Postscript' for the present special issue of *Social Studies of Science* that seismology was not a field in itself before the Cold War, since it served only as a tool for the exploitation of mineral resources and earthquake prediction efforts. Dennis (2003) concludes that 'Hence, what would "distortion" even mean with respect to seismology? By its very nature, the field existed to serve other, far more potent, masters. [. . .] Seismology was always inseparable from practical technical problems – there was no field to distort'.

This criticism neglects significant differences between earthquake seismologists, the focus of the present paper, and professional groups such as earthquake engineers and exploration geophysicists.[107] In the 1950s earthquake seismology was a small, but well-developed academic discipline, based on a rich tradition of mathematical analysis of seismic waves and investigations into the nature of earthquakes and the earth's deep interior. By contrast, neither earthquake engineers nor exploration geophysicists were primarily concerned with the nature of earthquakes or the earth's deep interior.[108] In general, earthquake seismologists differed in their academic training, research interests, instruments, and professional identity from earthquake engineers and exploration geophysicists.[109] It is this small group of academics and the knowledge they produced that I am concerned with here. These academics published most of the research papers and built the discipline of seismology in the period from the 1930s to the 1960s, steering their field through the period of intense government interest.

In conclusion, I have shown how the Cold War and Project Vela Uniform transformed the small discipline into a large military–academic–industrial enterprise. Clearly, without Cold War concerns about nuclear-test monitoring, seismology would look very different today. Consequently, this case study appears to confirm the distortionist theory. I argued, however, that a close look at the cognitive development of seismology does not reflect a massive reorientation of seismological research questions towards the patron's interests. My analysis of the negotiation of research contracts between academics and program managers of defense agencies suggests that seismologists did not lose control of their discipline.

Notes

1. Charles C. Bates, Branch Chief of Vela Uniform from 1960 to 1964, pointed out that the term 'Vela' referred to the stellar constellation Vela (The Sail). According to Greek mythology, the Vela constellation commemorated the adventures of Jason and his Argonauts, who sailed off to capture the Golden Fleece. I was unable to determine who was the individual who gave the seismic research and development program its name and to what extent this person intended a reference to the story of the Argonauts. As Bates has pointed out, the pronunciation of 'Vela' was not standardized: some pronounced it like 'Vayla', others like 'Veela' (Bates et al., 1982: 172 n.). Project Vela included three branches: Vela Uniform covered the detection of underground explosions, Vela Sierra focused on the detection of high-altitude explosions using ground-based detectors, and Vela Hotel developed satellite-based detectors for high-altitude test. Here I am only concerned with Vela Uniform. For a summary of the management, technical, and funding aspects of Vela Uniform, Vela Sierra, and Vela Hotel see 'Statement of C.M. Beyer, Deputy Assistant Director, Nuclear Test Detection Office, Advanced Research Projects Agency' (US Congress, 1962: 37–66).
2. For Vela Uniform support for seismology see VESIAC [Vela Seismic Information Analysis Center] Staff (1962), and Air Force Office of Scientific Research (1965).
3. To limit my investigation, I focus on academic earthquake seismologists. Earthquake engineers and exploration seismologists are not the focus of this paper. In the 1950s

and 1960s, earthquake seismologists primarily worked in university departments of geology or geophysics and in some government agencies such as the US Coast and Geodetic Survey and the US Geological Survey. These seismologists were concerned primarily with the nature of earthquakes, their geographical and temporal distribution, and the propagation of seismic waves through a multi-layered earth. This group included university professors and others with PhD degrees who focused on the academic study of earthquakes.

4. A.W. Betts, Advanced Research Projects Agency, Director, 29 April 1960, 'Classification Guide, Project Vela', folder 'Panel-Disarm-NT-Vela-UK/US Jt. Working Group', box 53, Records of the Executive Office of the President, Office of Science and Technology, Subject Files, 1957–1962, Record Group 359, National Archives, College Park, MD. Hereafter cited as RG 359. With the exception of some details related to Vela's test series of nuclear explosions, the 'Classification Guide' does not give any evidence for secret documents or classified Vela Uniform sub-programs.
5. Vela managers established a facility at the University of Michigan's Willow Run Laboratories that focused on 'information collection, analysis, and dissemination'. This Vela Seismic Information Analysis Center (VESIAC), which operated from December 1960 to June 1971, became a clearing-house for information related to Vela Uniform. VESIAC published comprehensive bibliographies, state-of-the-art reports, proceedings from Vela-funded conferences, a newsletter, and a contract compendium, to keep Government agencies, industrial contractors, and academic seismologists informed about the status of the project. See for example VESIAC Staff, 1965; for issues of the VESIAC newsletter (The Vela Uniform Periodic Information Digest), see folder 'Disarmament-N/T Vela 1961', box 80, RG 359. The first such newsletter appeared on 1 September 1961.
6. Relevant collections, in chronological order, include: Smith (1985); Seidel (1987), in particular Forman (1987); Mendelsohn et al. (1988); de Maria et al. (1989); Galison & Hevly (1992); Thackray (1992), in particular Geiger (1992); and Forman & Sánchez-Ron (1996). One of the most influential works is Leslie (1993). See also Kevles (1987, 1990), Galison (1997: 239–311), and Dennis (1994).
7. As Ronald E. Doel (1998: 383), a leading historian of geophysics, recently put it, 'few historical studies have examined academic research programs in space science, climatic studies, seismology, hydrology, or volcanology; fewer still have addressed such issues as methodology, instrumentation, or the emergence of research schools in geophysics within the university community'.
8. See the papers in the present special issue of *Social Studies of Science*. For a concise overview of the history of geophysics, see Doel (1997). For a careful study of oceanography in the Cold War, see Mukerji (1989).
9. Among historical works, see for example Geschwind (1996, 1998, 2001), Goodstein (1984), and Brush (1980). In addition, some seismologists have written about intellectual developments in their field. See for example Howell (1986, 1990), and Ben-Menahem (1995). Most active in this area is Bruce A. Bolt, the former director of the Berkeley Seismological Stations and one of the most prominent popularizers of seismology. For example, see Bolt (1985, 1987, 1993, 1999).
10. See Siever (1997). Bolt (1976) is a very readable introduction to seismology and nuclear explosions from the 1950s to the early 70s. Like most accounts written by practitioners, however, his book is primarily based on published scientific papers and personal recollections. In addition, two former Vela managers, Carl F. Romney and Charles C. Bates, have written about the early years of Vela Uniform (Romney, 1985; Bates et al., 1982: 172–205; Lawyer et al., 2001: 119–45). Both accounts are based on private collections of documents and personal recollections.
11. Historians of science have debated what constitutes a 'discipline' and how it can be distinguished from 'profession', 'research school', 'research area', or 'scientific field'. See for example Lemaine et al. (1976), Rosenberg (1979), Farber (1982), Barrow (1998: 4–6), and Servos (1993: 3–15). Similarly, historians disagree about how the emergence of a new discipline is related to the specialization, institutionalization, and

professionalization of scientific practices. Some have argued that 'discipline' has at least two meanings: on the one hand it refers to specialized knowledge or skills, on the other hand it refers to an institutional structure in which scientists operate. For a recent article on discipline formation and change, see Good (2000). In the present paper I use the term 'discipline' synonymous with 'scientific research field', or just 'field'. A discipline is constituted by various research schools, each of which is constituted by small groups of researchers with specialized knowledge (Geison & Holmes, 1993). For an analysis of historians' use of 'research school' see Servos (1993) and Geison (1993). For a now classic discussion of research schools see Morrell (1972). These researchers interact to investigate specialized research problems and often share methodologies and practices. In this regard, seismology can be described as an emerging discipline, based on the convergence of a number of research programs on questions of the nature of earthquakes and seismic waves phenomena.

12. Hounshell (2001) shows that Forman (1987) and Leslie (1993) still inform the debate about science and military patronage in the Cold War.
13. Forman (1996) used the history of the maser to further document the physicists' reorientation towards techniques and 'gadgets'.
14. Leslie was also concerned with the economic consequences of this reorientation. He argued that the focus on military applications led to a decline in US economic competitiveness on the world market. Here I am not concerned with the economic, but with the conceptual consequences of the interaction between a scientific discipline and its Cold War context.
15. For a review of early seismograph developments see Louderback (1942), Dewey & Byerly (1969), and Bolt (1989).
16. For early works on seismology see Dutton (1904) and Hobbs (1907). On the importance of the 1906 San Francisco earthquake for seismology, see Dean (1993).
17. For detailed biographical references and an analysis of the research programs of these program builders, see Barth (2000: 25–64).
18. Total number of PhD degrees compiled from National Academy of Sciences, National Research Council, Committee on Seismology (1977: 76). The author has compiled a list of PhD students in seismology during the 1950s. Seismology produced fewer PhD degrees than small fields such as archaeology or paleontology (National Research Council, Office of Scientific Personnel, 1963: 2 and 10).
19. For a more detailed analysis of the number of active researchers, see Barth (2000: 57–58).
20. See Richter (1948). Beno Gutenberg to Captain O.W. Swainson, Chief, Division of Geomagnetism and Seismology, US Coast and Geodetic Survey, 19 May 1943, folder 3.14, Beno Gutenberg Papers, California Institute of Technology Archives, Pasadena, CA. Hereafter cited as Gutenberg Papers.
21. Beno Gutenberg, curriculum vitae [no date, probably 1953], folder 19.6, Gutenberg Papers; Beno Gutenberg to 'Lieber Herr Kollege' [Dr Schlechtweg], September 5 1946, folder 3.12, Gutenberg Papers. According to seismologist Leon Knopoff, 'Gutenberg was a valuable World War II consultant to the Navy, applying his knowledge of the structure of the upper atmosphere to the problems of ballistics. He also worked on applications of the observations of microseisms to locate hurricanes in the Caribbean and the western Pacific' (Knopoff, 1999: 140). For the scientific results, see Gutenberg (1947).
22. Benioff did not publish a single paper in 1942–47 (Press, 1973: 36).
23. This is based on a close reading of the leading seismological journal, the *Bulletin of the Seismological Society of America* in 1930–55. No significant difference in seismological research topics can be discerned from 1942 to 1947.
24. Based on an analysis of research publications in the *Bulletin of the Seismological Society of America* in 1945–50.

25. Daniel J. Kevles (1987: 369), for example, has pointed out that 'in 1952 the physical sciences at nonprofit institutions received almost $250,000,000, including more than $17,000,000 for unclassified basic research in physics'.
26. Beno Gutenberg to Albert Polumb, 9 August 1955, folder 3.9, Gutenberg Papers.
27. The exact amount of the General Budget for 1957–58 for the Seismological Laboratory was $177,605. 'Seismological Laboratory, Division of Geological Sciences, Record of Growth' [no date, probably 1964], folder 12.2, Lee DuBridge Papers, California Institute of Technology Archives, Pasadena, CA. Hereafter cited as DuBridge Papers. Twenty non-academic staff members worked in the Seismological Laboratory in the late 1950s, in addition to the four professors, Benioff, Gutenberg, Press, and Richter. For statistics of staff at the Seismological Laboratory between 1956 and 1963, see 'Seismological Laboratory, Division of Geological Sciences, Record of Growth' [no date], folder 12.2, DuBridge Papers. For a staff list of the Seismological Laboratory of February 1967, see 'Local Bulletin of Earthquakes in the Southern California Region, 1 January 1963 to 31 December 1966', second page [no page numbers], folder A 10.2, box 10, Caltech Historical Files, California Institute of Technology Archives, Pasadena, CA.
28. 'Sponsors of Seismological Research', folder 11.6, DuBridge Papers.
29. Contract No. W 28-099ac-426 ('Seismology #1'). The contract began on May 15, 1948, and was terminated on 31 March 1951, with a total of $80,890. Contract AF 19(122)-436 ('Seismology no. 2') began in 1 March 1952, and ended 29 February 1956 with a total of $110,346. For 'Seismology #1', see folder supplement 2, Office of Sponsored Research Papers, California Institute of Technology Archives, Pasadena, CA. Hereafter cited as Caltech Sponsored Research Papers. For 'Seismology £2', see folder 2.12, Caltech Sponsored Research Papers.
30. For contemporary accounts of the IGY's history and organization see the *Annals of the International Geophysical Year*, in particular Jones (1959), and Gerson (1958). Popular accounts of the IGY include Wilson (1961) and Sullivan (1961).
31. US National Committee, International Geophysical Year, 'Program and Budget for Seismology' [no date, very likely 1958], folder 381, box 11, Frank Press Papers, MIT Archives, Cambridge, MA. Hereafter cited as Press Papers.
32. The use of explosions as tools in seismology can be traced back at least to the mid-19th century, when Irish engineer and early seismologist Robert Mallet (1848: 97) pointed out that explosions of gunpowder could produce seismic waves, or as he put it, 'give us the power of producing, in fact, an artificial earthquake at pleasure'. For a history of explosion seismology before Vela, see Barth (2000: 65–99).
33. For seismic measurements during the first nuclear explosion, Trinity, on 16 July 1945, see Robert E. Reinke & Kenneth H. Olsen, 'Trinity Revisited: The First Nuclear Explosion Seismograms', unpublished manuscript, 1985; Kenneth H. Olsen & Robert E. Reinke, 'Trinity 1945: Seismograms of the First Nuclear Explosion', poster paper presented at the 1992 Annual Meeting of the Seismological Society of America. I thank Ken Olsen for making these two papers available to me. For the first seismological paper on waves from a nuclear explosion see Leet (1946). For other early seismological papers on nuclear explosions see Gutenberg (1946), Gutenberg & Richter (1946), Bullen (1948; 1958: 35), and Burke-Gaffney & Bullen (1957: 135).
34. Kai-Henrik Barth, 'Shots for Science: Seismology, Nuclear Weapons Tests, and the Interior of the Earth, 1955–1960' (paper presented at the History of Science Society meeting, Atlanta, 14 October 1996). In 1955 the Australian seismologist Keith Bullen proposed that 'for seismological and other experimental purposes, one or more atom bombs [should] be exploded during the International Geophysical Year' (Bullen, 1955: 550). Bullen, a professor of geophysics at the University of Sydney, Australia, was an accomplished mathematical seismologist, trained at the University of Cambridge under Harold Jeffreys. At the time of his proposal, Bullen was the President of the International Association of Seismology and Physics of the Interior of the Earth, the major international professional organization for seismologists. In the USA, Beno Gutenberg, Father Macelwane, Perry Byerly, and Maurice Ewing supported Bullen's

proposal. Gutenberg to Bullen, 19 September 1955, folder 1, series 4, The Personal Papers of Keith Edward Bullen (1906–1976), University of Sydney, Australia. I thank Mary Bullen and Kenneth Smith for making it possible for me to use Bullen's correspondence. For a detailed discussion of Bullen's proposal, see Barth (2000: 73–85).

35. By 1960 nuclear explosions had become a standard tool for seismological research. In the 1960s and early 1970s, the Atomic Energy Commission set off seven underground nuclear explosions for seismological purposes as part of the Vela Uniform program, thereby fulfilling Bullen's hopes, at least in part. The seven nuclear explosions related to the Vela Program were: Shoal, 26 October 1963, Fallon, NV; Salmon, 22 October 1964, Hattiesburg, MS; Long Shot, 29 October 1965, Amchitka, AK; Sterling, 3 December 1966, Hattiesburg, MS; Scroll, 23 April 1968, Nevada Test Site (NTS); Diamond Dust, 12 May 1970, NTS; and Diamond Mine, 1 July 1971, NTS. Some of these explosions played significant roles for seismological research. For a discussion see Bates et al. (1982: 199–205).
36. For the history of the early US test detection system, see Ziegler & Jacobson (1995), and Air Force Technical Applications Center (1997). Even a close reading of the major seismological journal of the time, the *Bulletin of the Seismological Society of America*, does not reveal any hint or suggestion that this early nuclear detection system changed seismological concepts, theories, instruments, or practices.
37. The classified Air Force program for seismic research during the period from 1948–60 was shaped by a panel of seismologists, chaired by the industrial geophysicist Roland F. Beers, co-owner of the instrumentation company Beers and Heroy. Roland F. Beers to Perry Byerly, 11 August 1948, folder 'Restricted, second folder', Carton 3, Perry Byerly Papers, Archives, University of California, Berkeley, CA. The panel included all leading US academic seismic experts: Perry Byerly of the University of California at Berkeley, Maurice Ewing of Columbia University, Father Macelwane from St Louis University, Beno Gutenberg, Charles Richter, and Hugo Benioff from the California Institute of Technology, and Louis B. Slichter from the University of California at Los Angeles. In addition, we find the leading government seismologists Norman Haskell from the Air Force Cambridge Research Center, Frank Neumann and Dean Carder from the US Coast and Geodetic Survey, and John Adkins from the Office of Naval Research ('Highlights of Department of Defense Seismic Research Program, 1948–1960', in US Congress [1960, part 2: 431–33]).
38. The author has developed a database of research publications in seismology based on entries in the GeoRef database. This content analysis focuses on the leading academic journal, the *Bulletin of the Seismological Society of America*. The *Bulletin of the Seismological Society of America* published 346 research articles during the period 1945–59. The author has analyzed these articles for their contributions to academic questions in seismology. While many articles cannot be classified easily into a single subject category, general trends emerge nevertheless. For more detail, see Barth (2000: 25–64).
39. See also Richter (1958) and Howell (1990).
40. For a detailed history of post-World War II arms-control negotiations written by a participant, see Bechhoefer (1961).
41. Still the best source is Jacobson & Stein (1966), in particular 34–81. For an anti-test ban position and some of the technical content, see Voss (1963), in particular 136–241. For aspects of the seismic debate see ibid., 194–201. For an analysis of the Geneva Conference from the US perspective, see Zoppo (1962). For a critical evaluation of the scientists as policy-makers see Gilpin (1962); for a more recent discussion, see Evangelista (1999).
42. For a more detailed discussion, see Barth (2000: 131–61), which is primarily based on the conference's verbatim records, Record Group 326 (Atomic Energy Commission, records of the AEC [Atomic Energy Commission] Office of Special Projects relating to the Geneva Conference on the Discontinuance of Nuclear Weapons Tests), National Archives II, College Park, MD. In addition, I interviewed

two US participants: Jack E. Oliver, interview by author, Cornell University, New York, 8 June 1998. Tape recording and summary available at the American Institute of Physics, Center for History of Physics, Niels Bohr Library, College Park, MD; hereafter cited as AIP-Center for History of Physics. Carl F. Romney, interview by author, Arlington, VA, 20 January and 28 January 1998. Tape recording and transcript available at AIP-Center for History of Physics.

43. For a detailed discussion of controversies between seismologists from East and West in early test-ban treaty negotiations, see Barth (1998).
44. For a detailed analysis of the origin of these new seismic data see Barth (2000: 162–68). 'US Working Paper on New Seismic Data, January 5 1959', Document DNT/25, reproduced in (Department of State, United States Disarmament Administration, 1961: 331–34). The new seismic data were later published by its main author (Romney, 1959).
45. For Eisenhower's reaction see Jacobson & Stein (1966: 155), and also for the Soviet reaction (162–66).
46. The decoupling theory was originally published by Latter et al. (1959); reproduced in US Congress (1960: 851–64). This theoretical work was undertaken at Edward Teller's suggestion and he apparently had given Latter's group 'important technical advice' (ibid., 853).
47. G.B. Kistiakowsky to John A. McCone, 24 February 1960, folder 'Disarmament – N/T Vela (High Alt. Seismic)', box 53, RG 359.
48. Christian Herter to John A. McCone, 21 February 1960, folder 'Disarmament – N/T Vela (High Alt. Seismic)', box 53, RG 359.
49. The Berkner Report, including its appendices, is reproduced in US Congress (1960: 643–838).
50. The summary report originally did not list any authors, but in a publication later in 1959 Frank Press pointed out that he, Jack Oliver and Carl Romney prepared the summary report (see Press, 1959). The summary report, with minor changes, was made accessible (ibid., 213–21).
51. 'Memorandum of Conversation: Subject: Recommendations of the Panels on High Altitude Detection and Seismic Improvement', 23 April 1959, folder 'Disarmament – N/T Vela (High Alt. Seismic)', box 53, RG 359, 2.
52. David B. Beard, A Research Program in Basic Geophysics, Report to the Assistant Director for Physics and Mathematics Programs, Division of Research, US Atomic Energy Commission, August 1965, 10; folder 963, box 31, Press Papers.
53. ARPA's Vela Uniform objectives were spelled out in Advanced Research Projects Agency (1961: 13). I thank Carl F. Romney for providing me with this document.
54. Charles B. Archambeau, interview by author, Boulder, CO, 18 June 1998. John M. De Noyer, interview by author, Herndon, VA, 22 January 1998. Jack F. Evernden, interview by author, Golden, CO, 16 June 1998. Robert A. Frosch, interview by author, Cambridge, MA, 28 May 1998. Carl Kisslinger, interview by author, Boulder, CO, 17 June 1998. Jack P. Ruina, interview by author, MIT, Cambridge, MA, 29 May 1998. Tape recordings and summaries of these interviews available at the AIP-Center for History of Physics.
55. Ruina, interview by author, 29 May 1998.
56. For ARPA budgets in 'Then-Year' dollars and constant dollars see Atta et al. (1991); in particular II-3–6.
57. For the early 1960s see ARPA, 'Status of ARPA Projects as of April 30 1962', 30 April, 1962, folder 8, box 1, Press Papers.
58. Ruina, interview by author, 29 May 1998.
59. Charles C. Bates, interview by author, Green Valley, AZ, 22 October 1997. For biographical information about Bates, see US Congress (1962: 67). Bates is co-author of Bates et al. (1982) and Lawyer et al. (2001).
60. Bates, interview by author, 22 October 1997.
61. Emphasis by Bates. Bates to author, 29 August 1995.

62. Bates published a variety of articles that introduced Vela Uniform to a broad audience of seismologists, geologists, electrical engineers, and other scientists who, he hoped, would contribute to Vela. See Bates (1961a, 1961b; 1962).
63. For more details about these panels see Barth (2000: chapter 7).
64. To be absolutely certain, Bates asked Lamont's Jack Oliver to confirm that no suggestion had been overlooked. Oliver, interview by author, 8 June 1998. Bates designed Vela Uniform so that every item in the Berkner Report corresponded directly to at least one Vela research project. ARPA, 'Supplementary Information on Project Vela: Prepared in response to a request by Congressman Chet Holifield, Chairman of the Special Subcommittee on Radiation of the Joint Atomic Energy Committee, during a public hearing at the Capital [sic] on 22 April 1960', 25 April 1960, folder 11, box 1, Press Papers. ARPA, 'Cross Reference Index from Berkner Report to Vela-Uniform Project', 25 April 1960, ibid.
65. Bates to author, 29 August 1995.
66. David T. Griggs to Lloyd Berkner, 13 March 1959, folder 843, box 28, Press Papers.
67. Bates himself dealt with domestic and foreign agencies involved in the program; Donald Clements handled the interactions with the US Coast and Geodetic Survey and some academic programs; Theodore (Ted) A. George monitored ARPA's cooperation with the Defense Atomic Support Agency and the Atomic Energy Commission in the test explosion program as well as research related to on-site inspections; Robert Harris and Rudy Black mainly covered the seismic system development in cooperation with the Air Force Technical Applications Center; Joseph Berg, an academic geophysicist himself, handled Vela Uniform's interaction with universities. Bates, interview by author; Bates et al. (1982: 175–76); Bates to author, 22 October 1997.
68. For a detailed description of Vela's organizational structure see Advanced Research Projects Agency (1961: 15–17), and similar organization charts and the accompanying statement by Carlton Beyer, Deputy Assistant Director, Nuclear Test Detection Office, ARPA, in US Congress (1962: 56–67).
69. For Vela's management approach see Advanced Research Projects Agency (1961: 14–18).
70. For AFCRL's history see Liebowitz (1985, 1995, 1997). I thank Ruth Liebowitz for making these reports available to me. For a short history of the AFOSR and its predecessors see 'The History of AFOSR' (in Air Force Office of Scientific Research, 1967: 12–14). Still the major work on the early history and organization of AFOSR is Komons (1966); see also Sigethy (1980).
71. When I asked Robert Frosch, director of ARPA's Nuclear Test Detection Office from 1963 to 1965, whether any seismologist had refused to accept money from the Air Force research agencies, he remembered the case of two researchers as a curiosity. Robert Frosch, personal communication, 15 September 1995.
72. About Haskell, see Ben-Menahem (1990).
73. Former AFCRL Program Manager Karl E. Seifert, phone interview by author, 23 February 1998. Former AFCRL Program Manager Ker C. Thomson, phone interview by author, 3 March 1998.
74. Frank Press to Ari Ben-Menahem (in Ben-Menahem, 1990: xiii).
75. Charles Bates, memorandum 'Background Information on Seismological Research within the USA', 7 December 1963, folder 8, box 1, Press Papers.
76. 'AFOSR Scientific Staff', in Air Force Office of Scientific Research (1967: 10).
77. Advisory Committee for Geophysics, 'Recommendations on Research in Seismology for the Vela-Uniform Program of the Advanced Research Projects Agency', Air Force Office of Scientific Research, 22 April 1963, folder 19, box 1, Press Papers. William Stauder, phone interview by author, 12 April 2000. Oliver, interview by author, 8 June 1998. According to Charles C. Bates, the AFOSR Advisory Committee for Geophysics 'was handpicked by Best and myself and Haskell'. Bates to author, 22 October 1997.

78. Contract AF 19(604)-7376; see VESIAC [Vela Seismic Information Analysis Center] Staff (1962: 6–6a).
79. These numbers are compiled from the *VESIAC* [Vela Seismic Information Analysis Center] *Monthly Bulletin of VELA Uniform Research Publications*, Bulletins 1–18, 15 February 1963 to 15 July 1964.
80. Based on a list of Lamont publications (Lamont Geological Observatory, 1964, 1965).
81. See contract descriptions in VESIAC [Vela Seismic Information Analysis Center] Staff (1962); resulting publications can be traced through *VESIAC Monthly Bulletin of VELA Uniform Research Publications.*
82. For Vela instrumentation projects see Farrell (1985) and Bates et al. (1982: 172–94). For a more detailed discussion of the Vela system, see Barth (2000: chapter 8).
83. On the history and deployment of the WWSSN see Oliver & Murphy (1971), Peterson (1977, 1992), and Powell & Fries (1965).
84. This handbook described each WWSSN station in detail, including levels of microseismic background and the maximum possible amplification of seismic signals in presence of this background noise.
85. Already in the late 1890s, for example, British seismologist John Milne helped to establish a network of about 27 seismic stations throughout the British Empire with support from the British Association for the Advancement of Science. For Milne's efforts see Stoneley (1970), Herbert-Gustar & Nott (1980: 122–29), and Adams (1989).
86. For a participant's view of the role of seismology (and data from the WWSSN) in the confirmation of plate tectonics, see Oliver & Murphy (1971), and Oliver (1996). Oliver's book (1996) includes the pivotal paper by Bryan Isacks, Jack Oliver, and Lynn R. Sykes, 'Seismology and the New Global Tectonics' (Isacks et al., 1968).
87. The robustness of the network's design paid off: in December 1994, 53 of the 121 installed stations still functioned. In 1996, the WWSSN stations were replaced by a network of much improved stations run by the Incorporated Research Institutions for Seismology, a consortium of US universities with research programs in seismology.
88. Vela funded Texas Instruments' ocean-bottom seismograph efforts with at least $4 million over a 7-year period from 1961 to 1968. See Charles C. Bates, 'Memorandum for Distribution List: Background Information on Seismological Research within the USA', 7 December 1963, folder 8, box 1, Press Paper.
89. For an overview of the development of seismic arrays, see Carpenter (1965).
90. About LASA see the articles in the special Nuclear Test Detection Issue of the *Proceedings of the IEEE* (1965, 53 [December]), in particular, Green et al. (1965). On the origin of LASA, see my interview with Robert A. Frosch, Director of ARPA's Nuclear Test Detection Office (1963–65) and later ARPA Deputy Director (1965–66); Frosch, interview by author, 28 May 1998. See also my interview with the contracting manager for LASA: Harry Sonnemann, interview by author, Tyson's Corner, VA, 22 January 1998. Tape recording and transcript available at AIP-Center for History of Physics.
91. See the photos of LASA's data center in Wood et al. (1965: 1850).
92. For a table of Vela Uniform funding from fiscal years 1960 to 1971, see Bates et al. (1982: 175).
93. In total, National Science Foundation (NSF) funding for seismological research projects from 1960 to 1968 was about $8.6 million. These numbers give only a general indication of NSF funding for seismology. In a number of cases it is not obvious whether a research project focuses primarily on earthquake seismology or on marine geophysics, structural geology or other related fields. The NSF funded large interdisciplinary projects such as Caltech's study on the San Andreas Fault, which received a 2-year $242,000 grant in 1964. In addition, other agencies such as the US Geological Survey and the Office of Naval Research offered grants and contracts for seismological research. For fiscal year 1963, for example, Vela Uniform supported

basic and applied research in seismology with $10.4 million, compared with NSF grants of about $1 million.

94. Information derived from National Academy of Sciences, Office of Scientific Personnel (1977: 76). I am aware of only one case in which Vela support led to a new faculty position in seismology. Frank Press at Caltech succeeded in winning AFOSR support for an 'AFOSR Professorship in Geophysics'. See California Institute of Technology, Seismological Laboratory, 'Ten-Year Grant for Establishing and Air Force OSR Professorship in Geophysics', grant proposal to Air Force Office of Scientific Research, 2 October 1961, folder 19, box 1, Press Papers.
95. For trends in seismology of the 1960s see US National IUGG Committee (1963). The report includes sections, among others, by Ari Ben-Menahem (1963), G.H. Sutton (1963), Eugene Herrin (1963), J.T. Wilson & T.W. Caless (1963), S.W. Smith (1963), and Leon Knopoff (1963). Equally informative are the later US National Reports: US National IUGG Committee (1967), in particular pp. 389–426, which cover research in seismology and physics of the earth's interior. Finally, see US National IUGG Committee (1971), in particular pp. IUGG 157 – IUGG 194.
96. For the Upper Mantle Project, see Beloussov (1964), Oliver (1966b), and International Council of Scientific Unions, Upper Mantle Committee (1972).
97. See for example Oliver (1966a, 1970).
98. See Barth (2000: 267–312) for a detailed discussion. For a summary of seismicity studies in the mid-1960s, see Lynn R. Sykes, 'Seismicity', in US National IUGG Committee (1967: 389–95: especially 391); see also Oliver & Isacks (1967).
99. Gary V. Latham, 'Lunar Seismology', in US National IUGG Committee (1971: IUGG 162 – IUGG 165).
100. See for example Benioff et al. (1961). For an introduction to free oscillations see Howell (1990: 95–96).
101. Emphasis in original. Perry Byerly, talk prepared for meeting at the Earthquake Engineering Research Institute, 14 February 1970, folder 'Correspondence 1965–1969', carton 3, Perry Byerly Papers, Archives, University of California, Berkeley, CA.
102. Leslie (1993: 256).
103. See also his slightly different version (Kevles, 1990).
104. See for example Skolnikoff (1995). For critical reviews of Leslie's book see, for example, Geiger (1994) and Sapolsky (1994).
105. The call for more emphasis on program managers and scientific advisory groups is not new. See Seidel (1994: 44).
106. I thank Michael Aaron Dennis and an anonymous reviewer for their critical comments on this point.
107. See Barth (2000: 6–8) for a distinction between earthquake seismologists, earthquake engineers, and exploration geophysicists.
108. For background on earthquake engineering, see Geschwind (1996, 2001) and Cutcliffe (1996). For a history of exploration geophysics see the relevant chapters in Bates et al. (1982). For a sketch of the history of seismic exploration see Sheriff & Geldart (1995: 3–32). For an early comparison of the methods of earthquake seismology and exploration seismology see Mintrop (1947).
109. The boundaries between these groups were permeable. Many academic earthquake seismologists had some professional experience in oil exploration and often worked with earthquake engineers to investigate how structures responded to violent ground motion generated by earthquakes. Nevertheless, earthquake seismologists predominantly sought to investigate the nature of earthquakes and seismic wave phenomena.

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